

PROBLEMS IN INVESTIGATING PSYCHOKINESIS IN SPECIAL SUBJECTS

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Qualification:

This thesis is submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy of the Council for National Academic Awards.

Sponsoring Establishment: Polytechnic of North London

Date of submission:

PROBLEMS IN INVESTIGATING PSYCHOKINESIS IN SPECIAL SUBJECTS

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of thesis plus photocopies of charts

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Gregory, A. 'Testing by means of random number generators', <u>Parapsychology Review</u> , 8 (1977) 15-16.	151	B
Gregory, A. Review of Playfair, G.L. <u>This House is Haunted, An Investigation of the Enfield Poltergeist in JSPR</u> , 50 (1980) 538-541.	157	C
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Declarations concerning registration and assistance

During my registration period with the CNAA I have not been registered for any other award of the CNAA or of a University, nor has the material been used in any other submission for an academic award.

Mr. D. Chapman (then Technical Assistant in the Department of Electrical and Electronic Engineering, City University) and Dr. B. Millar (then post-graduate student in the Department of Psychology, University of Edinburgh) examined and reported on the random event generators described in Chapter 3.

Ms. F.M. Branch (formerly Principal Psychiatric Social Worker at Guy's Hospital, London), acted in a capacity of clinical or social work supervisor as described in connection with the Enfield case (Chapter 4). Ms. E. O'Keeffe (Secretary of the Society for Psychical Research) and Ms. K.M. Wilson (Research Assistant in the Department of Teaching Studies, Polytechnic of North London) kindly checked some of the references in the Enfield chapter against the relevant documents in the archives of the Society for Psychical Research and confirm their accuracy.

The City University investigation of Matthew Manning contained in Chapter 5 was convened by me and the infra-red experiments were devised and planned by myself. However, the apparatus was constructed for me by the late Mr. C. Brookes-Smith, an instrumentation engineer, and installed and maintained by Mr. D. Chapman who also revised my section on technical equipment (section 5.6). Prof. A.J. Ellison and Mr. Roger Chapman (Senior Lecturer in the Department of Electrical and Electronic Engineering, City University) further examined the equipment and advised me concerning it. Prof. Ellison revised the circuit diagram (Figure 7) which was re-drawn by Mr. A. White, Technician at the Polytechnic of North London. Miss Ruth West gave extensive administrative and secretarial help in the course of the investigation. Ms. K.M. Wilson, who did not herself participate in the experiments, transcribed and as far as possible timed the audio record and assisted with the preparation, description and discussion of the relevant tables (34, 36, 37 & 38).

I am indebted to Prof. A.J. Ellison who acted as my Director of Studies, and Dr. J.R. Beloff who was my additional Supervisor, for advice and encouragement throughout, and help with the revision of the final typescript.

The thesis was typed by Ms. Wilson to whom I would like to express special gratitude for this voluminous task.

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ABSTRACT

PROBLEMS IN INVESTIGATING PSYCHOKINESIS IN SPECIAL SUBJECTS

Anita Gregory

An attempt is made to establish the authenticity of a number of 'psychokinetic' (PK) effects claimed to have been obtained with Special Subjects and in the process to elucidate the question why in over a hundred years these phenomena remain controversial. Four cases are examined in detail.

The first, that of Rudi Schneider, is a well documented case history and archival and other records are subjected to qualitative and quantitative examination, which suggests a prima facie case for authenticity of some of the claims, both of earlier gross effects and later vestigial irregularities detected by means of infra-red equipment. Scrutiny of the evidence also highlights the complex social and psychological factors entering into controversies in this area.

In the second case an experimental claim to have established PK by means of a random number generator is shown to have been dubious if not spurious, and the circumstances, social and psychological in which it arose, are discussed.

The third case considered is a recent set of supposed poltergeist occurrences, the Enfield case, in which the writer participated and witnesses were interviewed and documents examined. It was concluded that the case was spurious and the pressures on all involved are discussed.

The fourth case concerns a modern active psychic, Matthew Manning, whose earlier phenomena are considered, and with whom laboratory experiments were conducted in an attempt to replicate the infra-red disturbances found in the case of Schneider. These were ostensibly successful, and the records obtained were subjected to detailed scrutiny.

The major problems facing researchers in this area are summarised. It is suggested that PK effects arise from group configurations of persons and are particularly connected with competition or personal ascendancy: however, there is no reason to suppose that the psychological circumstances surrounding ostensibly genuine PK are any different from normal and abnormal ones. An inter-relation between the ability actively to dominate and delude, and that to facilitate genuine phenomena, is suggested as a reason, in addition to numerous social and practical ones, why greater certainty has not been achieved to date. Future research is suggested.

INTRODUCTION

This thesis is addressed to the problems of investigating some of the so-called physical phenomena of psychical research.

Attempts were made to establish authenticity or otherwise of a number of different effects and, in the process, to elucidate the question why, after more than a hundred years of effort often by persons of competence and integrity, these phenomena remain fundamentally controversial.

The methods and approaches are various: historical scrutiny of primary and secondary sources, experimental investigation inside and outside the laboratory and field observation, were used in different proportions, as well as some theoretical and philosophical discussion.

The first chapter, 'Psychokinesis: contexts and perspectives', attempts to define, isolate and order some of the problems under review and to explore the settings within which the phenomena are apt to feature. The analysis points to a context of the phenomena at the interface between science on the one hand, and philosophy, religion, psychological and social factors and ethical issues on the other. It is suggested that the unresolved and often unacknowledged problems and conflicts involved are likely to play a part, tacit or explicit, in any investigation of psychokinesis, and militate against a resolution of the problems. This raises a number of issues analogous to musical themes in terms of which problems arising out of the investigation in subsequent chapters are considered.

The case history of Rudi Schneider, Chapter 2, was selected as an instance of one of the best documented physical mediumships on record, because of the sheer extent of testimony available, because it took place within living memory and straddles the transition from traditional to modern research, and also because I had in my possession some of the important relevant primary source material. This latter provided an opportunity to extract some quantitative data from qualitative

material. In addition, the primary source material available at the Harry Price Library, University of London, at the Institut Métapsychique, Paris, in the archives of the Society for Psychical Research, London, and that put at my disposal by Professor H. Bender from the archives of the Institut für Grenzgebiete der Psychologie und Psychohygiene, University of Freiburg, threw important light, not only on aspects of the mediumship itself, but also on the controversies surrounding it.

Among the conclusions that emerged from this study were:

- (a) that there was a sufficiently impressive prima facie case for the authenticity of some of the phenomena to support some earlier historical claims and to make worthwhile future work;
- (b) that there was also good evidence for instrumentally recordable vestigial effects in the form of partial occultation of an infra-red beam after the macro-physical effects had virtually ceased, and that hence it was worth investigating the working hypothesis that such vestigial effects might be present in Subjects not manifesting major physical phenomena;
- (c) that since there seemed to be reasonable evidence of far more vigorous gross paranormal physical activity at the outset and in a home setting, prior to more systematic instrumental and laboratory investigation, it seemed advisable to attempt to participate in domestic settings as early on as possible in cases where there were at least some grounds for hoping for physical phenomena;
- (d) that controversies surrounding mediumship were often virtually independent of the merits or otherwise of claims, but could be usefully analysed in terms of social, personal and economic considerations.

In view of the working hypothesis arising out of the Schneider material that there might be minor, merely instrumentally detectable psychokinetic abilities or characteristics present in persons not manifesting any classical physical phenomena, I at first considered using ordinary Subjects and random number generators. Initial tests had suggested promising results might be obtained with a particular Subject who had never before demonstrated any psychic abilities whatever. However, it turned out that these findings were neither valid nor reliable. Chapter 3 provides a brief description of this attempt and some of the

implications are discussed.

After reviewing the situation, I decided to abandon random number generators as being too problematic as well as possibly unlikely to be related to the phenomena I was attempting to explore, and also to confine further empirically based work to Special Subjects, i.e. persons who had given some reason to suppose that they at one time had shown psychokinetic properties. My aims here were two-fold: to replicate the Schneider vestigial infra-red phenomena in the case of an ex-physical medium or former poltergeist focus, and I had equipment constructed for me with this in mind; and also to examine some of the problems that arise in the process of conducting such investigations.

Chapter 4, the Enfield case, is an essay in attempted participant field observation on my part in a domestically based poltergeist case which had received widespread publicity, and in which I attempted to preserve the roles of detached observer whilst remaining sensitive to the needs of the family and to some extent acting in a counselling capacity. It is concluded that the conflictful role of field investigator in such situations is an inherent unavoidable problem to be faced. A more detailed discussion is provided concerning the social and subjective involvement of the investigator. The main conclusion as regards authenticity in this case was that the effects, at any rate those observed by myself, were wholly spurious and/or pathological, and no further experiments on infra-red lines with the children involved seemed promising, practical or desirable. The case and its aftermath also demonstrate how little fundamental controversy about standards of evidence has changed in 100 years, and the problems this presents for dispassionate enquiry and especially for publication. Despite the negligible value of the evidence for authenticity the case illustrates vividly the social, psychological, ethical and religious issues outlined in Chapter 1.

As regards Matthew Manning, the Subject of Chapter 5, I had had much earlier though indirect contact with him when he was still at school. Nothing had come of this for reasons of a kind relevant to the problems considered in this thesis. Matthew Manning later approached me and asked me to arrange

for him to be investigated.

In the course of the ensuing experiments an attempt was made to replicate, in a modern setting, with a well-known and much publicised psychic who had been an alleged focus for much physical paranormality in childhood, the vestigial phenomena in the form of infra-red occultations observed with Rudi Schneider. Chart recordings of such occultations were ostensibly obtained, but although there are reasonable grounds for accepting the paranormality of the instrumental malfunctioning, it is not entirely certain whether these were due to an actual occultation of the infra-red radiation for reasons discussed. The observations however lend support to the hypothesis that it is worthwhile searching for vestigial and only instrumentally recordable psychokinetic manifestations in Subjects who have at one point been foci of poltergeist activity.

An attempt was made to analyse the coincidence between vocal (audio) and instrumental (chart) records in order to examine the circumstances in which positive results were apparently obtained, and to examine the degree, if any, of voluntary and conscious control by the psychic over the instrumental anomalies. Results suggest a more obscure and complex interaction than conscious and voluntary control on the part of any one person. It is suggested that the psychic notices normally small fluctuations in the apparatus (which may or may not be of paranormal origin) and, possibly paranormally, enhances these. The term 'bandwagoning' is proposed for this effect.

This case also provides at least some information, though little hard evidence, concerning the earlier childhood phenomena, and suggests some developmental and psychological avenues to be explored. The case as a whole serves to highlight some of the major difficulties encountered in investigating Special Subjects at the present time, and the roots of these difficulties.

In the final chapter an attempt is made to summarise, in the light of the findings contained in the thesis, the major problems facing researchers into PK in Special Subjects, to highlight some of the key issues in the psychology (in the broadest sense) of physical mediumship, and to suggest future

avenues of research in the area.

A distinction is frequently made between micro and macro psychokinetic phenomena, micro phenomena being anomalies which are not observable by the naked eye but which nevertheless have observable causal consequences. This distinction is not a sharp and absolute one. Some of the effects to be discussed, such as the earlier Schneider phenomena, clearly fall into the macro category. The RNG experiments were concerned with typical micro PK. Some of the effects, such as the infra-red records of both Schneider and Manning, might be regarded as being at an uncertain intersection of the micro-macro PK spectrum, the effects being fairly gross and no anti-chance calculus being necessary for their detection whilst requiring apparatus for their capture.

The term 'psychokinesis' and the abbreviation PK (used as a noun or adjectivally for 'psychokinetic'), refer to the entire range of alleged paranormal traditional physical phenomena of psychical research as well as to laboratory based effects in a neutral manner without implying any ultimate theoretical explanation as advocated by Beloff in the preface to Thalbourne's glossary (1).

Words such as 'alleged', 'so-called' and 'ostensible' and inverted commas designed to indicate suspension of belief are normally omitted after the manner customary in the literature published by the Society for Psychical Research (for which the abbreviation SPR will normally be used), and their omission does not indicate acceptance of authenticity. Inverted commas around the names of secondary persons such as 'Olga' in the case of Rudi Schneider and pronouns referring to 'her', are, however, retained for clarity to convey the hypothetical status of the persons.

Numerous letters, documents etc. relating particularly to Mr. Harry Price's investigation of Rudi Schneider are preserved in the Harry Price Library, Senate House, University of London. They are not kept in any one file, and relevant documents etc. are referred to by HPL in both text and reference notes.

Some of the research was conducted at the Institut Métapsychique International, 1, Place Wagram, 75 Paris 17e.

As in the case of the Harry Price Library, the papers were not organised in any particular way and had to be extracted from the general correspondence which in the case of the Institut was in very roughly chronological order. These documents will be referred to by the letters IMI.

Documents contained in the archives of the SPR will be referred to as SPR, followed by the relevant file reference, e.g. SPR-S1, or SPR G/P 6 to indicate authorship plus page number. These and other conventions used are explained as appropriate in the text and/or reference section.

In view of the personal nature of some of the interactions between colleagues, Subjects and myself, and to avoid artificiality, cumbersome circumlocutions and confusion, I have referred to myself in the first person singular throughout.

CHAPTER I, CONTEXTS AND PERSPECTIVES

1.1 Focussing on PK

In attempting to examine the problems in the investigation of psychokinesis, it is clearly necessary to provide a background against which such investigations arise. In a more orthodox scientific subject where there is widespread agreement, and where a given problem can be relatively easily isolated and specified, a straight forward chronological historical account referring to the relevant literature normally suffices to provide such a context. However, the problems for anyone wishing to consider the physical phenomena of psychical research arise from so many and such diverse directions that it was found more satisfactory to subdivide the perspectives into topics most of which embody a historical element.

1.2 Problems of definition

Before engaging in further attempts to trace the origins of problems concerning PK, it seems appropriate to offer a definition of what is being considered.

1.21 Working definition

Psychokinesis or PK operates if there are changes in physical objects or processes due to personal influence but without the intermediary of normal effectors or their normal extensions.

The use of the phrase 'due to personal influence' is intended to avoid question begging as deplored for example implicitly by Gauld (1) and directly by Randall (2) when the latter, rightly in my view, queries the neutrality of the definition offered in the Journal of Parapsychology, 'a direct mental influence exerted by the Subject on an external physical process'. 'Personal influence' is intended as neutral and impartial to cover individual humans, groups, part-persons, and intelligent agents of any kind whatever.

In the present state of knowledge, the need for neutral or

theory-free terminology is particularly acute, as was formally recognised by Thouless and Wiesner (3). However, the term 'psychokinesis' will serve, provided its formal character is recognised, and 'kinesis' is interpreted in the conventional parapsychological context, rather than as implying mechanical movement or force. In effect, as embodied in 'psychokinesis', it resembles not so much our modern notion of movement as Aristotle's concept of kinesis (4), meaning any kind of change whether of quality, size or place: for him the fall of an apple and its ripening were both subsumed under the same term.

Perhaps the most important of all the words which embody an attempt to represent neutrality as regards interpretation is 'phenomenon', as in 'physical phenomena', a concept derived from philosophical tradition dating back to Kant to the extent to which he was influenced by Hume and since developed in different ways by various schools of philosophy. This was intended to convey agnosticism as to the reality underlying subjective experience. It is frequently overlooked, however, that in parapsychology the term is employed in a specialised manner to indicate suspension of belief or judgment in a particular instance, and not to indicate that there is a realm of inferred but unknowable noumina, or in any attempt to reduce the totality of our knowledge of the world to subjective experiences. On the contrary, the word 'phenomenon' in parapsychology indicates merely a provisional suspension of belief and usually, if not implies at least invites, eventual ontological or at least practical resolution. Thus we may speak of a spurious or a genuine phenomenon; this is perhaps a bad linguistic habit, and when we decide on the authentic status of a phenomenon (if we ever do) then we should call it something else, such as a genuine or spurious paranormal event. Meanwhile it is above all necessary to remember exactly how the term 'phenomenon' is used. The limited practical scepticism of the psychical researcher is that of the founders of the SPR (quoted in 1.22 below), and refers to open-mindedness rather than to any more radical variety of scepticism.

The definition offered is like all parapsychological definitions, negative, or at any rate differential, and involves

the requirement positively to establish a residual category.

1.22 Logical and pseudo-logical aspects

Most of the problems encountered in attempting to define psychokinesis or the physical phenomena of psychical research are shared by all so-called paranormal phenomena. Every issue of the Journal or Proceedings of the Society for Psychical Research (SPR) contains a statement to the effect that the Society exists for the purpose of examining 'without prejudice or prepossession, and in a scientific spirit, those faculties of man, real or supposed, which appear inexplicable on any generally recognised hypothesis'. Virtually every other word or phrase in this definition is open to query. Nevertheless the intent and care of those who framed the definition are plain: the first part contains ethical and psychological conditions for the satisfactory pursuit of knowledge, the second, not altogether successfully, attempts the demarcation of the field.

The subject matter of psychical research has come to be grouped together under the general term 'paranormal', para meaning besides, over and above. The term Parapsychologie was coined by Max Dessoir at the end of the 19th century (5) and did not come into wider use until the 1930s (see also Grattan-Guinness 1982 (6)). 'Para', like 'appear to be inexplicable', implies a negative definition or set of definitions, as does the prefix 'extra' in extra-sensory perception.

More modern approaches, as e.g. by Beloff (7), Schmeidler (8), and Isaacs (9), do not escape negative definition: in dealing with any paranormal phenomena we are in principle always confronted with data not subsumed under standard categories or explanations. We are therefore dealing with a shifting set of definitions: either the data are explained in some standard manner in which case they cease to be classed as 'paranormal'; or they are not so explained, in which case they await either novel constructs for explaining them, or else still greater ingenuity for explaining them away. The paper by Zinchenko et al (10) is a particularly clear instance of writers correctly pinpointing this problem, whilst failing to do any kind of

justice to it.

Now it is precisely here, in the logical realm, that one encounters the first major factor militating against a resolution of the disputed issues: given any set of puzzling empirical or historical data, there are no absolute criteria which enable us to decide whether it is more correct to attribute them to some form of error, or else to invent new explanatory constructs: systematic doubt is always a logical option.

In everyday life we tend (it is to be hoped) more or less satisfactorily to decide on appropriate explanations, opting for complexity or simplicity as required, wielding Occam's questionable and blunt razor at our discretion; when we come to the very margins of human experience presented by the psychical phenomena, the merely formal problem that no particular set of explanatory constructs is logically coercive becomes acute in practice: these matters are by definition out of the ordinary, not normal, and hence normal rules of thumb do not apply. Since there are no conventionally time-honoured criteria for choosing between the two residual categories of constructs, i.e. some variant of error versus some type of novel explanation, choices will be made on a wide variety of grounds, by no means all of them, as will be argued, either rational or even consciously acknowledged. The result may be fiercely contended sets of pseudo-empirical and crypto a prioristic assertions.

These logical hazards are, as has been said, shared by all the paranormal phenomena, including psychokinesis.

1.23 Characterisation by differentiation

The two first categories of characterisation when we are presented with a paranormal phenomenon, viz some variant of error versus some new unknown explanation, have both been increasingly refined in the past 100 years. Any psychology textbook will bear witness to such enhanced sophistication as regards perceptual judgment and memory, and even though arguments concerning depth psychological factors as affecting perception remain unsettled (11) there is now a far greater alertness, at least among psychologists, towards complex potentially motivational

sources of error. Such errors, which might be thought of as unconsciously tendentious, ranging from wishful thinking via self-deception to occasional manipulation, might to some extent be thought of as bridging the gap between simple error and outright fraud.

In parapsychology there is, from the nature of the case, considerable emphasis on potential error, and this is particularly acute in the case of PK where actual physical changes are claimed to be involved. In experimental work, various forms of automatic recording have been introduced, either to extend or to corroborate human testimony. Nevertheless, testimony and judgment cannot be wholly excluded even if excellent automated records exist, if only because the instrumental arrangements need to be vouched for. However, in field work it may be far more difficult to obtain instrumental recordings of actual events, and testimony, plus quasi-clinical judgement of that testimony, may be the main bases of assessment.

Deliberate fraud has, of course, been an enduring source of concern from the very inception of the subject, and particularly as applied to the physical phenomena (12, 13), although there would not appear to be evidence of enhanced sophistication in this respect in modern days (14). It should however be mentioned that, as Gauld observes (15), in only 41 out of 500 cases of poltergeist reports was fraud shown, and trickery could frequently not account for all the observations.

The greatest advance in understanding of complexity of possible sources of error is probably in the realm of the assessment of chance coincidence, as against causal factors. In micro-PK experiments sophisticated statistical techniques to assess this possible source of error are clearly basic. However, in the grosser physical phenomena of either the seance room or of poltergeist manifestations, what Bender has dubbed 'Duell mit dem Zufall' (16) (duel with chance) is virtually superfluous.

It seems fair to state that the other side of the divide, 'novel explanations', is far less developed than the 'error' side. As Charl says (17), we have no comprehensive theory to account for all the reported physical phenomena. The fact that no such

acceptable theory exists, constitutes no logical argument against their occurrence: it is however a factor militating against their consideration by those who for whatever reason wish to exclude them from serious debate.

At any rate the very definition of paranormal phenomena, to the extent to which it is a negative or differential one, means that any attempt to establish authenticity involves the exclusion of normal sources of error as far as humanly possible and, like any human enterprise, this cannot in principle be perfect. Such an undertaking inevitably involves great practical difficulties.

1.24 Descriptive subdivisions

In addition to general attempts to define PK, such as that offered in 1.21, it is also possible to list a number of topics which have traditionally been considered under this heading, such as 'levitation', 'materialisation', 'dice-throwing' etc. Such headings are of great practical usefulness, and may be followed up in any suitable work of reference such as Fodor (18), or the section on PK in Wolman's Handbook (19). Such headings may be treated as useful sub-divisions of observations or classification, which may eventually lead to acceptable theory, provided it is recognised that, in themselves, such labels have no explanatory value. The whole issue of taxonomy and theory in psychokinesis has been admirably set out by Braude (20) who shows that neither the dichotomies between for example 'spontaneous' and 'experimental', nor those between 'micro' and 'macro' PK are as clear or as illuminating as is often taken for granted. They are however of limited practical and terminological utility: Certainly there is some practical need for distinguishing between naturally occurring or spontaneous PK, and experimentally evoked PK, although here too there may be no abrupt transition.

1.3 Brief chronological survey

Historical accounts can, from the nature of the case, only be brief selections, and sub-divisions or orderings made can always be challenged. For simplicity and conciseness, only two

very rough divisions will be offered.

1.31 PK until the later 19th century

It is difficult to obtain any reliable accounts of PK in earlier times although some such phenomena have been claimed, so far as we can tell, from time immemorial. Table-turning would seem to go back at least to ancient Roman times (21) and there is disagreement among experts as to when the earliest poltergeist story was recorded. Dodds (22) believed them to be unknown in classical antiquity, but Inglis (23) traces accounts as far back as 218-201 BC.

Although poltergeist cases are often quite reasonably clearly described in numerous older accounts, e.g. Aubrey (24) and Glanvil (25), it is extremely difficult to disentangle in most early writings what was actually said to have occurred by way of paranormal physical activity, since stories are apt to be embedded in tales of omens, portents and marvels of so questionable a nature, or so totally unattested, that it is difficult to know quite what to do about them. The softest modern researcher would find it difficult to entertain the, at the time, widespread belief that in 1314 Margarita of Holland gave birth to 364 infants (26). Aubrey (27) in 1784 juxtaposes the assertion that it was 'common knowledge that the head of King Charles I's staff did fall off at his trial', under the heading of 'Apparitions', with a very reasonable and circumstantial account of a stone throwing episode. The book illustrates the difficulty that may be encountered in attempting to establish any kind of estimate as to what did and did not happen, being largely concerned with omens, portents, magical invocations, magical medicines, second sight and what might today be called 'coincidences' under the heading of 'Day Fatality'.

Far more rigorously organised and argued is Glanvil's Sadducismus Triumphatus (28) containing the splendid account of the 'Demon Drummer of Tedworth' referred to in (25).

Gauld (29) points out that there was a 'serious and almost systematic attempt to collect and sift authenticated accounts of all sorts of psychic phenomena' in the second half of the 17th century by what he calls 'Cambridge centred parapsychological

endeavour'. (He brushes aside the fact that Glanvil was in fact an Oxford man on the grounds of his Cambridge style intellectual affinities.)

In spite of these earlier attempts it is so far not easy to assess the incidence of claims for paranormality in days ante-dating the late 19th century. One reason has already been suggested: the lack of any taxonomy by which such effects might be ordered: marvels were a catch-all category, a pitfall difficult enough to avoid even in modern times in view of the difficulties of definition!

Another reason was the embedding of all such matters in a religious context (see below 1.4), and in particular in beliefs about witchcraft. The testimony for alleged paranormal events contained in accusations and confessions of witchcraft is so tainted by being extracted under torture and by being set down in response to leading questions, the replies being recorded in the tendentious language of prosecutors and inquisitors, that it is not, any any rate in the present state of knowledge, possible to disentangle truth from falsehood.

It has always seemed to me that the philosophers of the Enlightenment were actuated in their total rejection of what we now call the paranormal, not only by what seemed to them the obvious good sense of the new science, but also by decent humanitarian sentiment, a desire to put a stop to what must surely rank among Europe's all-time records for collective atrocity.

However, from the point of view of the 20th century psychological researcher the possibility remains open that there were in fact substantial numbers of paranormal occurrences underlying the witch craze, even if 'Sabbath', 'Pact' and 'Satanic Conspiracy' are relegated to the realm of paranoid fiction.

So far as I am aware, no scholar of any repute any longer accepts the theory championed by Dr. Margaret Murray (30) that there in fact existed an underground and organised witch cult that was persecuted in the late Middle Ages and modern times, and Cohn (31) is quite right to tax her with falsely contriving to convey the impression that there existed any reliable evidence for the Witches' Sabbath. As he showed, she did so by

omitting passages in which bizarre events were reported. 'Stories', he writes, 'which have manifestly impossible features (my italics) are not to be trusted in any particular as evidence of what physically happened.' (32) However, for anyone willing at least to envisage physical paranormality or even collective hallucination or delusion the matter is not so simply dealt with (33).

Both Inglis (34) and Randall (35) provide modern popular but well researched historical accounts of psychokinetic phenomena which attempt to provide some historical continuity. For somewhat older accounts see Tischner (36), or Sudre (37). The latter, like most writers in the continental tradition, places greater emphasis on the Mesmeric and 'fluidic' traditions; see also Poortman (38), Barrett and Besterman (39); for a brief review of these aspects see Gregory (40).

1.32 PK since the 1870s

The reason for starting a decade or so before the foundation of the Society for Psychical Research is that 1871 saw the publication of the Report of the Dialectical Society (41) largely on the physical phenomena. The latter society held 50 seances and unambiguously supported the genuineness of paranormal sounds, movements of heavy bodies often to order and spelling out answers. Interestingly enough, it was noted that, whereas the presence of no given individual ensured the phenomena, the presence of some seemed necessary, the presence of others hindered phenomena, but this was not dependent on the belief or scepticism of the persons concerned. The Committee was split on other claims such as apparitions of faces and apports.

The results of this enquiry brought Sir William Crookes into the psychical research arena and his work with D.D. Home and other mediums is well documented (42). Relatively modern attacks on Crookes on the grounds of his collusion with one of his subjects, Florence Cook, made by Hall (43) on the testimony of Anderson can in my view be safely dismissed (44).

The foundation of the Society for Psychical Research and events leading up to it is admirably documented by Gauld (45). The early luminaries of the SPR were undoubtedly primarily preoccupied with the so-called mental phenomena and problems of

survival of death. They tended to find the physical phenomena distasteful both because of their frequent and undoubted association with fraud, and also because of what was felt to be their trivial and unserious nature. It seems to me likely that other factors may have played a part, such as that those who came to predominate in the SPR were humanists, philosophers and classical scholars rather than natural scientists, and there can be little doubt that rather different skills and interests are conducive to monitoring whether a physical object moves, or whether a message should be interpreted as indicating paranormal transmission of information. Still further factors may well have contributed towards the strong anti-PK stance traditionally taken up by the SPR, see for example Fodor (46).

On the continent, Richet (47) and Schrenck-Notzing (48) were among the most prominent researchers and they, on the whole, supported belief in the authenticity of physical phenomena, both of telekinetic movement and of materialisation without, however, giving such phenomena any spiritual or religious significance: they were inclined to regard them as extensions of normal physics and biology.

Between 1870 and the 1930s the study of PK was largely conducted in terms of investigations of 'the great mediums' which were then in abundant supply: D.D. Home (49), Florence Cook (50), Eva C. (51), Eusapia Palladino (52), Franek Kluski (53), Stella C. (54), Kathleen Goligher (55), Anna Rasmussen (56), Stanislaw Tomczyk (57) and the brothers Willi and Rudi Schneider (58, 59) to enumerate only some of the more outstanding names. Moser (60) provides a fairly comprehensive account of most of these, and Nicol (61) gives a useful summary which he concludes by saying that, 'when allowance is made for the possibilities of human error, it does appear from the histories of Home, Palladino (at her best), Rudi Schneider, Stella C and perhaps a few others that a surprisingly good case can be made out for their genuineness and therefore for the reality of the phenomena'.

Attempts to conduct quantitative statistical experiments in PK analogous to those in ESP date back, so far as I am aware, to the mid 1930s, when experiments to influence the fall of dice were begun at Duke, followed by similar experiments elsewhere,

and by so called placement effects. An account is given by Pratt (62).

Nature's most elementary source of randomness is provided by quantum processes which are in principle random and unpredictable. The particular interest of quantum processes for parapsychology was first pointed out by Beloff and Evans (63) in connection with PK experimentation. Electronic technology has indeed changed the entire complexion of much PK research by making possible compact random event generators in which the Subject is expected to influence the random events generated. Stanford (64) reviews the subject for Wolman's Handbook.

There has been a certain revival in the investigation of star Subjects and major phenomena partly in the wake of Uri Geller (65) and partly because of continued and even renewed interest in poltergeist activity (66, 67, 68), often under the would-be soothing acronym RSPK (Recurrent Spontaneous Psychokinesis), and partly because of the disappointing replicability of laboratory based statistical PK experiments with ordinary Subjects.

1.4 Psychokinesis in relation to science

It is widely believed that somehow the pursuit of psychical research is contrary to science. This feeling is probably even stronger as regards PK than any variant of ESP, an attitude more due to sentiment than to any reasoned point of view since the problem of a channel for information transfer exists just as much for paranormal reception of information as for action. However, it seems reasonable to suppose that the strong feeling against the physical phenomena springs largely from the fact that these would seem to infringe on the territory of the physical sciences which are most securely anchored in public and learned esteem, which cannot be said of the social sciences. The physical sciences are widely, though in my view inaccurately, thought to have solved problems that are in fact particularly acutely raised by a consideration of psychokinesis.

1.41 Are PK phenomena contrary to science as such?

What is, and what is not, properly a part of science has been a matter for debate for centuries, and is currently usually referred to as the 'demarcation' debate, the best known names amongst protagonists being Popper (69) and Kuhn (70). It is beyond the scope of the present thesis to enter into any detail in this debate beyond making a few comments which, it is hoped, will be pertinent.

The Popperian demand for an open-textured or 'falsifiable' set of hypotheses (regarded not as a positive description of what science actually is, but as an ethical demand for a set of necessary conditions) would certainly seem to be in accord with the pursuit of PK. Indeed, it is the dogmatic denial of the very possibility which ought to incur Sir Karl's wrath. Popperian problems might well occur at a later stage when theory construction begins, but at the rough and ready outset, when attempts are made to rule out 'normal causes' there should be no strictures on this count since the propositions set up are well and truly falsifiable: if a normal cause, such as a reaching rod, an optical illusion or instrumental defect, is established, the hypothesis of paranormality is refuted. The problem is, what counts as 'established'? In this respect parapsychology is theoretically no worse off than any other science.

Parapsychologists have at times been attracted towards the Kuhnian view that there are 'normal' and 'revolutionary' phases in science, and that a new 'paradigm' is required to accommodate the new subject. There are considerable and complex issues involved here, particularly as regards the concept of 'paradigm' and the inter-translatability of paradigms (71,72). The demarcation dispute remains unsettled and seems likely to remain so for a long time. The chief dangers of a premature capitulation to the difficulties in finding firm criteria seem to me to arise from two major sources: either an acceptance of a limitless relativism, which leads to a position where the pursuit of PK would be a pointless activity, as exemplified by Collins and Pinch (73) and discussed by Gregory (74); or else to an adoption of the stance advocated by Feyerabend to the effect

that 'anything goes' (75), which would let in precisely all the ill-considered and careless welter of undifferentiated superstition that any would-be scientific parapsychologist is particularly anxious to exclude.

It seems to me important to emphasise the quasi-ethical half of the definition of the aims and objects of the SPR (see 1.22). There are sets of ethical cognitive attitudes which form the substratum of scholarship in general, including science; and here 'anything' emphatically does not 'go': care, precision and accuracy as appropriate, detachment, simple truthfulness, are part of this enterprise.

However, methods, though disciplined and rule-governed, are multiple and need to be adjusted to the subject matter in question. It is as regards content that parapsychology needs a philosophy of 'anything goes' - as indeed advocated as early as Francis Bacon (76).

It is sobering that despite the efforts of Gregory and Kohsen (77) with respect to criteria for credibility we have hardly progressed so very far beyond Mairhofer's statement in 1581 (78) that 'when anything is accepted as certain or probable cause by prudent and circumspect men who are not misled by their emotions or by devotion to the suspect arts, but have knowledge and experience in natural science, the same should be accepted by less prudent and expert persons'. Among the many troubles with this robust set of views are that people are not at all likely to regard themselves as any less prudent and expert than others, that anyone considering PK seriously is likely to become ipso facto suspect and that even very prudent persons disagree among themselves, not to mention the problems raised by the word 'cause'. These are among the reasons why such disputes may be exceedingly difficult to resolve in practice, as later parts of this thesis will, it is hoped, serve to illustrate. There is, however, nothing in the enterprise of science as such that would prevent the investigation, and possible authentication, of psychokinesis.

1.42 Are PK phenomena in conflict with specific presuppositions of science?

The term 'cause' has been mentioned as problematic. What counts as a proper cause, and hence as an explanation for scientific purposes, remains one of the major contentious issues in philosophy. Hume (79) argued that no conclusion of fact can be demonstrated with complete certainty since the contradictory of any factual proposition is always possible. There is in Humean philosophy, as Flew (80) points out, 'no a priori limitation on what sort of cause is required by what. There are contingent conjunctions only: necessity is not to be found in the universe around us.'

Despite the considerable influence of Hume and empiricist philosophy in general, a very restricted notion of causality in fact pervades learned and scientific thought when there is question of providing explanations. For example, in discussing the objections to Aristotle's views of causality, O'Connor (81) writes

To a modern philosopher the paradigm cases of causality are simple mechanical instances like one billiard ball hitting another... To Aristotle the paradigms were those cases where the influences of a final cause seemed most clearly at work - human artifacts and biological processes. To us of course these are far too complex to be suitable objects for causal analysis. It seems not unfair to attribute Aristotle's liking for explanation in terms of final causes, and his reading of purpose into nature as an unexercised residue of the magical animistic thinking with which Greek philosophy was infected.

The point of quoting this in extenso is that it brings out particularly clearly the unresolved disputes over millenia concerning causation. Mechanical causation is singled out as 'paradigmatic' because it is simpler and less troublesome for us as philosophers, and Aristotle's other suggested types, among the most important of which are final causes, are discarded as being 'animistic' and 'magical'.

The embargo on final causes was most forcibly expressed by Hobbes (82), embarked as he was on a systematic materialistic metaphysic. This embargo of course affects what are going to be viewed as permissible causes of physical phenomena. As might be anticipated, Hobbes subscribed to the conjuring or imposture

interpretation of 'miracles' (83). It seems plain that some type of teleological construct, animistic and magical or not, will be indispensable for parapsychology, as suggested for example by Beloff (84). It seems to me proper to go further and to suggest that the language of intending, wishing, willing, planning, hoping, meaning, trying, wanting, striving, etc. cannot be translated without loss of central substance into the language of, ultimately, pushes, pulls, forces, movements, etc. etc. The issue in theoretical psychology remains unresolved. Even if Schopenhauer's teaching that all force including gravitation be regarded as due to cosmic acts of will (85) is regarded as fanciful, there remains a basic core of problems centred around the consciousness of self-initiated action to which, since Freud, must be added relative degrees of unconsciousness.

The matter is far more complicated still and involves the whole concatenation of questions concerning 'action at a distance' and 'occult qualities', discussed by Whittaker (86). The doctrine that there can be no action at a distance, that all influence must be due to pressure and impact goes back to the ancient Greek atomists, and was stoutly defended by Aristotle, Aquinas and even Newton, who wrote (87) 'that one body may act upon another at a distance through a vacuum without the mediation of anything else...is to me so great an absurdity that I believe that no man who has in matters philosophical a competent faculty of thinking can ever fall into it.'

This of course raises the spectre of 'nature abhorring a vacuum' and the entire issue of 'occult qualities' which science is presumed to have settled. Descartes (88) postulated that the aether had complex mechanical properties, putting forward the vision of the cosmos as a vast machine. But the scientific demise of the aether has not led to the universal abandonment of the picture of the cosmos of just such a mechanism.

In 1730 Voltaire (89) wrote that 'A Frenchman who arrives in London...will have left the world a plenum and now finds it a vacuum'. He was referring to the reluctance on the part of enlightened Frenchmen to adopt Newton's system which, they probably quite correctly sensed did introduce 'action at a

distance' - 'the vacuum', despite Newton's own disclaimers. Voltaire ascribes the slowness of the French to adopt Newton's physics to his language, especially the word 'attraction', and in Germany, Leibnitz (90) described Newton's formula as a return to the discredited scholastic concept of 'occult qualities'.

It seems to me that science has in fact not resolved any of the ancient problems of action at a distance, the nature of personal influence and purpose, the problem of occult qualities: it has simply side-stepped them by inventing ever more sophisticated parables called models, formalisms or mathematical constructs to describe systematic physical correlations and interactions. This seems a very practical and sensible as well as sophisticated way of dealing with apparently intractable conundra. However, the tacit assumption that they have been resolved, which they have not, creates considerable difficulties for the discussion of concepts such as psychokinesis, with its semantic overtones of 'action at a distance' and 'occult qualities'. The problems are not really scientific but in the most extended sense ideological. As Burttt observes (91)

It ought to be fairly obvious from the facts of modern physics that the world around us is, among other things, a world of masses moving according to mathematically stable laws in time and space. But when...men seek out of the temporal and spatial realm all non-mathematical characteristics and concentrate them in a lobe of the brain, and pronounce them the semi-real effects of atomic motions outside, they have performed a rather radical piece of cosmic surgery which deserves to be carefully examined.

It is the trauma attendant on this cosmic surgery that is, I believe, uncomfortably disturbed when psychokinetic phenomena are taken seriously.

Another presupposition of science that should be mentioned, and which is widely felt to be threatened particularly by psychokinetic claims, is the need to assume for experimental intents and purposes that portions of the universe can be satisfactorily isolated and varied, other facts being kept constant, the 'ceteris paribus clause' as discussed

e.g. by Koestler (92). Science has achieved what it has precisely by making the assumption that, given sufficient care and attention to detail, factors affecting any particular physical system can be either controlled or at least accounted for. Many classical demonstrations, such as that life is not spontaneously generated, and many other scientific and medical experiments, are entirely dependent on experimental punctiliousness. An attitude of mind that is forever lightly invoking physical paranormality when physical irregularities are found, would indeed be undermining to science as an enterprise. As I hope to indicate, such an attitude does exist, but hardly among serious parapsychologists. Provided 'normal' explanations (whatever such may be in a given state of knowledge) are exhausted first, the danger is containable. It is the ethical-cognitive rather than the substantive conditions of the subject that are crucial here.

Moreover it seems that in normal science, both physical and social, instances may be given where the very fact that observations are made affects the results. In the case of PK such interference may be felt to be more insidious, 'occult', since it is suggested that 'mere mental factors' affect physical systems. However, it seems to me that just as physiologists and medical scientists have to accept the fact that mere information effects physiological and even anatomical changes, so the learned world may have to come to terms with the possibility that mental factors of uncertain status and origin may affect physical systems.

On the hypothetical supposition that there is some PK, it would seem desirable to form some idea how much or how little PK might be expected to occur. The empirical indications from the infrequency, transitoriness, instability and relatively slight nature of the phenomena claimed suggests that the total amount may be slight, although of course their significance and potential impact and application might be great. Further speculation along such lines are outside the scope of this thesis.

1.5 Religious aspects

It is all too often not acknowledged that a consideration of paranormal phenomena is, whether rightly or wrongly, deeply involved with long-standing and fundamental religious issues and questions. Thus controversy surrounding parapsychology frequently becomes the unacknowledged arena for religious (and anti-religious) battles, a fact hardly likely to be conducive to their objective and dispassionate resolution.

1.51 'Miracles'.

Paranormal events, particularly physical ones (and prophesy with which I am not here concerned) are inevitably linked with the religious idea of a miracle, and dogmatic sceptics such as G.R. Price (93) have done so in an attempt to invoke of all people David Hume as their patron saint to guard the world against parapsychology. As long ago as 1956 Kohsen (94) pointed out the incongruity of using Humean philosophy, with its repudiation of any a prioristic knowledge of the world, to rule out, a priori, testimony in favour of paranormal phenomena. For Hume there could be no inherently probable or improbable or indeed miraculous events, only more or less frequent 'impressions'. Price quoted Hume to the effect that 'No testimony is sufficient to establish a miracle unless the testimony is of such a kind that its falsehood would be more miraculous than the fact which it endeavours to establish'. Kohsen at the time, rather misleadingly (I now believe) suggested that this quotation should merely be regarded as a precautionary requirement on Hume's part before accepting the apparently miraculous. What Hume in fact goes on to say (95) is that 'we may establish it as a maxim that no human testimony can have such force as to prove a miracle... so as to make it a just foundation for any system of religion...' He is in fact ready to admit that 'there may possibly be miracles or violations of the usual course of nature of such a kind as to admit of proof of human testimony and, if such a thing were to be shown to have happened, philosophers ought to search for the causes whence it might be derived'. It would be hard to find a plainer logical brief for a scientific parapsychology, although

Hume himself would probably not have been too enthusiastic about this in practice.

However, Hume's qualification (and possibly Kohsen's original failure to see its force, and above all G.R. Price's failure to cite it) is of the utmost significance for understanding, not so much the logical or even the empirical problems facing the would-be PK investigator, as the passionate ideological issues that are - rightly or wrongly - felt to hang on the occurrence or non-occurrence of such phenomena. In Hume's day it was taken for granted, for example by Butler (96), that it was 'an acknowledged historical fact that Christianity offered itself to the world and demanded to be received on the allegation, i.e. as unbelievers would speak upon the pretence, of miracles publicly wrought to attest the truth of it'. Butler took the line that miracles should impress rational men as 'direct and fundamental proofs'.

Whereas as recently as 1870 Vatican I (97) declared to be anathema anyone who doubted that miracles could be known for certain, or that the Christian religion could be duly proved (probari) thereby, on the whole the more traditional Christian institutions have since then moved away from such a position. However, the newer religions, such as Christian Science and above all Spiritualism relied on the new 'miracles' to prove their truth. The clearest proponent of such a view in the psychical research arena was Wallace who in 'Miracles of Modern Spiritualism' (98) took on Hume directly and ably. There is here clearly not space to discuss in extenso either the history or the logic of religious controversies as regards the miraculous beyond making three specific points: 1. in view of the fierce emotions and personal commitments likely to be aroused, it will be difficult for people to view dispassionately any alleged event felt to have a bearing on their religion (interpreted as their overall attitude towards the universe), and this may be quite insidious in that such connections may not be made explicitly or even consciously; 2. that whether something be regarded as miraculous has really nothing much to do with modern science or 'natural law', since science can always be stretched (or at least cannot be shown to be unstretchable) to cover the

phenomena: people appealed to 'miracles' as extraordinary, or else as unworthy of belief, long before the days of modern science; 3. that in the case of any miraculous event, its investigation will inevitably involve a historical component: if 'miracles' are believed to have ceased 2000 years ago or so (as e.g. by James I (99)), to the extent to which critical thinking is envisaged at all, the critical thinking will take the forms of historical investigation which may or may not include a forensic scientific element; however, even if miracles are believed to continue, to the extent to which they are specific events rather than experimentally reproducible processes, there is bound to be a strong historical component in their evaluation. Such seemingly abstract considerations will strongly emerge as practical problems in PK research.

The paranormal then has to be differentiated from the miraculous in that the former is intended to be theoretically neutral. On the other hand, there are 'family resemblances' in that at least the so-called spontaneous macro-PK events are extremely unusual and therefore surprising, and also that they are historically if not unique, at least discrete.

Nevertheless, it still seems difficult to disagree with Hume that the 'miraculous' cannot be used to prove a system of religion.

1.52 Natural selection.

There can however be no question but that the founders of psychical research deeply hoped and indeed believed that the pursuit of the paranormal would throw some light on religion in an extended way (100). The intellectual and emotional 'culture shock' administered by the victory of the Darwinian theory of natural selection can hardly be over-estimated. Ideologically, the doctrine of natural selection deprived religion of the natural theological argument from design. Although it may well be that there was less clash between science and religion over evolution than is at times supposed, and the issues are subtle and complex (101), nevertheless, there can be no doubt that not so much natural descent as natural selection raises extremely grave problems for traditional religion. Perhaps the four most

fundamental issues touched upon are: 1. the general question of meaning; 2. the status of purpose or intention; 3. the origins and status of moral and other values; and 4. the significance of suffering for the individual. All that can be done here is merely to indicate that all four are deeply if sometimes obscurely involved in questions concerning phenomena, in which semantic purpose is supposed to be exercised on inanimate matter in a non-mechanical and not directly biological manner.

Wallace (102) was particularly concerned with the supposed inadequacy of natural selection to account for specifically human moral and cultural development, and invoked whatever underlies the physical phenomena of psychical research to fill some of this gap. Members of the Sidgwick group which obtained and retained the leadership in official psychical research, noting the on the whole unsaintly and trivial content of much PK phenomenology, were inclined, as has been mentioned, to look elsewhere for the fulfilment, if such were possible, of religious aspirations. The question of the mechanism of differentiation of species is by no means fully resolved even today (103, 104). I believe that a reluctance to reconsider fundamental issues in the area of human origins to be at the root of much unreflective opposition in principle to psychokinesis. Whatever the eventual outcome of debates concerning the mechanism of evolution, the serious consideration of PK phenomena in principle touches a nerve concerning the modus operandi of phylogenetic development which renders its detached consideration extremely difficult.

1.6 Relation with psychology

The term 'parapsychology' suggests that the field is a branch or would-be branch of psychology. Its adoption was probably at least partly an academic-political decision to bring the subject into universities in the 1930s, as described by Mauskopf and McVaugh (105). It can certainly be argued that the phenomena of psychical research at least largely fall within the general penumbra of psychology, even if the 'human faculties' in the definitions of the aims and objects of the SPR are now obsolete.

1.61 Intra-disciplinary problems.

As Beloff points out (106) psychology itself should be viewed as a pluralistic activity, being composed of frequently near-distinct branches or fields not fully inter-translatable in terms of general approach, methods or subject matter focussed upon. He may perhaps be a little sanguine as to the peacefulness of the nature of this academic co-existence: whilst it may be that sheer complexity has forced specialisation upon psychology, there do remain fundamental tensions within the field particularly as between what Beloff, following Boring, calls 'behaviouristics', and what is often called the humanistic perspective and the different psychoanalytic schools, and more recently also the social perspectives.

The study of parapsychological phenomena is, in addition to its own special problems, subject to all the different difficulties that beset psychology as such, including demarcation disputes, and at least as many approaches and perspectives are required. Parapsychology is, in a way, in the same dilemma as a would-be science as ordinary psychology: it can either confine itself to tidy experimental laboratory-style investigations, which might be said to amount almost to Kuhn-style 'puzzle-solving' on a behaviouristic paradigm; or else it can attempt to cover the whole gamut of cases reported, from the untidiest spontaneous cases, via some of the more regular mediumistic instances and field work, to regular laboratory work.

It is naturally one of the objectives of any would-be science to attempt to reproduce and demonstrate the subject matter studied under controlled conditions. This may not be easy for a variety of reasons: for example, behaviour may be difficult or impossible to elicit in artificial and simulated conditions; a sufficiently sophisticated theoretical framework may not yet exist for isolating the characteristics to be studied; the phenomena may be too tenuous and unreliable to be elicited on demand. All these operate in parapsychology, in addition to the fact that we are almost invariably involved in issues of human deviance, to put it at its most neutral. It therefore seems important to continue studying the phenomenology of the subject as it presents itself, trying out whatever methods

and approaches seem promising, and ordering, refining and systematising them as much as possible.

In the present state of knowledge, or rather ignorance, this somewhat pioneering approach is still appropriate and will, it is to be hoped, eventually lead to a more theoretically coherent discipline. These methods in the making, as will be seen, are still in a considerable state of imperfection, but unless the problems are at least recognised, improvement is hardly likely to take place.

1.62 Inter-disciplinary problems.

In PK research the psychologist as such is faced with the additional difficulty that the target of observations as well as the instruments and methods of their detection, belong to the realm of physics, and are therefore not strictly within the psychological sphere of competence. (The present thesis is virtually not concerned with the type of PK where the target is biological.) The psychologist therefore normally has to rely on expert collaboration, which may present considerable problems for both sides, which are often not even admitted to exist.

1.7 Social and ethical issues

The social problems facing the would-be investigator of the paranormal and particularly of PK as the popularly less acceptable type of phenomenon, are only just beginning to be considered, see Collins and Pinch (107, 108) and Gregory (109, 110). For some of the ethical issues see Gregory (111). Social and ethical issues are usually so intertwined that they will be dealt with jointly in this brief section. Many difficulties arise from the fact that the subject of parapsychology is not, by and large, officially recognised.

1.71 Danger of narrowing subject for acceptability.

There naturally exists the temptation in a non-established subject to confine the field of activities to a narrow base with an acceptable methodology and technical terminology, in the hope

of gaining acceptance by the academic community. This has, I believe, happened in parapsychology generally, with its emphasis on the 'mental' phenomena and subsequent narrowing to laboratory based statistical experiments, and the rejection of the gross physical phenomena for which, nevertheless, there is often evidence at least as acceptable as for some laboratory type results.

1.72 A prioristic pressure in favour of negative interpretations.

There is no logical coercion to select a 'normal' or 'paranormal' interpretation in any given case (see 1.22). However, there will be very strong social pressure to select the normal interpretation, which may mean that quite inadequate dismissals of paranormality may be accepted, and this may introduce an important non-rational pressure into the selection of interpretations.

1.73 Problems of collaboration and expertise.

In any non-validated subject the distinction between amateur and professional, expert and layman, will be uncertain and at times blurred. Difficulties and friction are inevitable, not only as is normal in established fields between peers, but between persons of very different levels of competence, education, sophistication, background, long-term aims and ethical commitment. Disputes often display many of the most painful characteristics of religious or political controversy. It is by no means always entirely plain who is the best and most expert interpreter of evidence. This fact is still further complicated, particularly in PK research, in that physical scientists, often with adequate or even high standing in their own fields, may be complete novices in parapsychology and indeed in psychology (see 1.62). The status of such physical scientists who enter the field of psychical research has, from the earliest days of the foundation of the SPR been problematic and subject to conflicting interpretations. At the other extreme, collaboration with enthusiasts who may be neither necessarily primarily committed to, nor in any sense socialised into, the dispassionate pursuit of knowledge may present well-nigh

insuperable obstacles for would-be scientific and scholarly investigators. In addition, as is well-known though hardly ever referred to in print, threats of litigation are by no means uncommon and may actually hamper the exchange of views, and certainly of their publication.

1.74 Publication, media and popularisation.

Psychical research, and particularly the PK phenomena, have considerable sensationalist and commercial value if exploited in a certain way. Serious researchers are vulnerable on a number of counts, such as distortion by the media of their claims and findings, and exploitation by popular writers who have not themselves made any contribution, yet who may pose as experts simply by association (see 1.73). The attention of the media focussed on an investigation or a topic may, in practice, have deleterious consequences.

1.75 Working with individual Subjects.

Since there is no large-scale incidence of PK in the human population as a whole, at any rate as measured by currently available techniques, the option of investigating such characteristics in individuals over the often limited period whilst they exhibit them to any marked degree, is an important one. This entails a number of problems for the investigator. The individual will be difficult to keep anonymous, and may indeed be reluctant to remain so: this renders difficult candid and full discussion of case histories on the clinical model. The individual in question will, ex hypothesi, be so to speak, a rare commodity, and there will therefore be likely to be competition for his services and possible jealousy as regards these. The individual may, furthermore, either be a professional psychic, or become a professional partly at least as a result of the publicity he derives from having been investigated. He will now have, in addition to psychological incentives to achieve 'positive' results (e.g. attention), solid economic ones, and a vested interest in maximum publicity. The pitfalls and dangers here are obvious.

At the same time, the Subject of an experiment is entitled to proper ethical treatment: such a Subject is at risk from a

hostile press and establishment, and investigators may be tempted to ascribe fraud to him so as to maintain their own reputation for care and caution. Furthermore, young subjects are obviously particularly vulnerable and need protection and a safeguarding of their interests. A professional investigator may well have different ideas as to what constitutes these interests from those of say, parents. A researcher may also well be in a state of conflict between serving the interests of a child or family, or else finding out more about the phenomena, or indeed possible psychopathology leading up to it. He may also find himself in the crossfire between the media, sensational claims made by less cautious collaborators, a family's desire for help and support, as well as for notoriety.

1.76 Resources.

Of all the social difficulties encountered by the would-be investigator, one of the most all-pervading and least frequently mentioned is economic. Lack of money for funding for suitable apparatus, qualified technical personnel and running expenses are serious enough; the absence of settled career structures in which individuals are able to give their best energies and the bulk of their most productive time to the enterprise of investigation places a virtually insurmountable handicap in the path of the scientific researcher, especially into PK, with its need for technical assistance, expertise and equipment.

CHAPTER 2, RUDI SCHNEIDER

2.1 Biographical outline

Rudi Schneider was born in Braunau, Austria (Adolf Hitler's birthplace) in 1909. His father Josef (subsequently called 'Vater Schneider'), a typesetter with the firm Stampfl & Co., and his mother, Elise, were hard hit by the fact that out of nine children only six boys survived, Rudi being the youngest of these. Until he was 4 or 5, his parents called him 'Rudolfine' and his mother dressed him in girls' clothes and curled his hair (1). Like Rudi, his two brothers Karl and Willy were also mediums.

It was Willy who first attracted the attention of the scientific world as a consequence of the physical phenomena said to occur in his presence. He was so to speak discovered by Kapitän F. Kogelnik (2), and subsequently widely examined in Germany by Dr. A. von Schrenck-Notzing (3) (subsequently called 'Schrenck') and also by Dr. E.J. Dingwall (4,5).

In the first instance Willy was in a waking state whilst phenomena were reported and communication took place via a small tripod, called a 'psychograph', one leg of which was a pencil, said to have moved, at least at times, whilst no one was touching it. The personage claiming to be the author of the phenomena called 'herself' 'Olga' who, on being asked the curious question by a participant, 'What sort of an Olga are you then?' ('Ja, was für eine Olga bist Du denn?') replied to the effect that really she had been Lola Montez, the mistress of King Ludwig of Bavaria (6, 7, 8). 'Olga' asked for masses to be said for the repose of her soul and, after Vater Schneider had complied with this request, 'she' thanked him and promised to make them famous. At first sittings were held in full light, and then red light was substituted. Eventually Willy insisted on total darkness.

As is usual in cases of physical mediumship, Willy's phenomena grew less intense and frequent. One evening,

probably in 1923, when in spite of all efforts and promises, nothing had happened, 'Olga' announced 'she' required Willy's younger brother, Rudi, as her medium. Despite the parents' protests that Rudi was too young, 'Olga' insisted and it would appear that the boy entered in a somnabulistic trance, went into the same state characteristic of Willy as 'Olga' which combined rapid breathing, extreme muscular tension and hyperactivity, together with a breathless whisper purporting to be the voice of 'Olga'. The previously blank seance became replete with phenomena.

Willy now developed two further secondary personalities, one 'Mina' and another 'Otto', and never again manifested 'Olga'. His mediumship disappeared altogether at around the age of 23. His own account was given to the late Mr. John Cohen in 1964 in an interview on tape which he kindly gave me. Willy's account varies slightly from the usually accepted one, in that he claims that 'Olga' used Rudi as a medium because he, Willy, was not at home, and that they subsequently floated about under the ceiling together - a version I am inclined to discount.

Karl, the other Schneider brother to manifest physical phenomena, seems to have started giving his own seances as a result of sitting with Rudi. Unfortunately the account by Jaschke (9) is not such as to provide much that might be called evidence. Karl's control personality was called 'Helga' who seems to have manifested herself just like 'Olga' and to have been somewhat jealous of the latter.

In addition to the numerous sittings he gave at his own home and in that of many others, Rudi was investigated over a number of years under increasingly sophisticated control conditions by Schrenck in Munich in the latter's purpose-built private laboratory. He was the subject of three investigations in London under the aegis of Mr. Harry Price's National Laboratory for Psychical Research (10,11). W.F. Prince conducted a series of experiments in Stuttgart at the home of Dr. R. Lambert (12). Rudi was investigated at length by Dr. E. Osty and his son Marcel, an engineer, at the Institut Métapsychique in Paris (13). Lords Charles Hope and Rayleigh conducted a series of systematic experiments under the auspices of the

SPR in London (14). Mr. Theodore Besterman and Mr. Oliver Gatty undertook another (largely negative) series in London (15). Experiments were conducted in Prague by Prof. Oskar Fischer and Dr. Karel Kychynka (16). Finally there was a major series of experiments by Prof. G. Schwaiger in Vienna (17).

Table 1 shows the principal investigations.

1924-1928	Schrenck-Notzing in Munich (ca. 88 sittings). Other sittings in Central Europe. Various accusations, see 2.2.
1929	Price First Series in London (5 sittings).
1929-1930	Price Second Series in London (21 sittings).
1930-1931	Osty sittings in Paris (90 sittings over 15 months), infra-red apparatus introduced.
1932	Price Third Series, London (27 sittings), February-May, infra-red apparatus.
1932-1933	Hope-Rayleigh investigation (22 sittings), infra-red apparatus.
1933	Osty, Paris, February/March (17 sittings), infra-red apparatus.
1933-1934	Besterman and Gatty, London (55 sittings), infra-red apparatus.
1935-1936	Schwaiger, Vienna (78 sittings), infra-red apparatus.

Table 1. Principal investigations of Rudi Schneider

Vater Schneider kept a regular record, contained in two thick notebooks, of seances that took place when he was present, which was usual in the early days. These jealously guarded books known on the Continent as 'Vater Schneider's Geisterbücher' (ghost books) were given to C.C.L. Gregory and myself by the late Mrs. Mitzi Schneider in the summer of 1963, and are in my possession. Vater Schneider was clearly an orderly and intelligent man. He noted what was experienced, when, where and in whose presence in an impressively systematic manner. Sitters were expected to read and usually sign seance accounts afterwards. These books will be referred to as Schneider Journals or Journals.

By the early '30s, when the Ostys' investigated Rudi in Paris, the phenomena had weakened considerably, negative

sittings were frequent, and sitters had to wait many hours for a small effect such as the slight levitation of a handkerchief. It was at that unpromising point in the mediumship that the Ostys employed as part of an anti-fraud control system an infra-red network surrounding the target, such a system then being a novelty as a burglar alarm device. Any solid object approaching the target handkerchief would, in crossing the beam, trigger a system of bells and/or photographic apparatus. The alarm system was indeed set in motion, but by no visible agency: the recording cameras photographed nothing. At first it was supposed that the triggering off of the alarm system was due to instrumental failure, but eventually it was found that such interruptions - or rather partial interruptions - or ostensible occultations of the radiation, coincided with 'Olga's' declared intention to move the target object.

'Olga', who had at first complained that the flash set off by the alarm system had disturbed and upset 'her' and had prevented 'her' from picking up the handkerchief, was pacified and eventually persuaded by Dr. Osty that 'her' ability to 'go into the ray' was a considerable paranormal achievement. After this, experimenters concentrated more and more on getting 'Olga' to affect the infra-red beam, registered by means of galvanometric apparatus and automatically recorded.

The interruption of the beam, whatever its nature, was never complete, usually only something of the order of 15% of the beam was absorbed or occulted. (For a further discussion of the interpretation of such interference, see below, 6.2.) Observers in Paris noted that at times the beam oscillated at a period of twice that of the medium's abnormally fast respiratory cycle. Superimposed on this oscillation there were marked deflections of the galvanometer coinciding with 'Olga's' announcement that 'she' would 'go into the beam', or that 'she' would 'try and lift up the handkerchief'. Infra-red observations of this type were made independently in London (18,19) and in Vienna (20).

It would seem that after the Schwaiger experiments' (1935-1936) Rudi's phenomena virtually ceased, at least so far as scientific experimenters were concerned. He settled down in

Weyer in Upper Austria with his wife Mitzi, where he founded a driving school, and built a home for himself and his wife with his own hands (21). He seems to have been happy, healthy and contented, but died at the age of 49 of a cerebral haemorrhage in 1957 (22).

2.2 Accusations of fraud*

Whenever there are claims that psychokinetic manifestations have been observed, one of the first difficulties encountered is that there are liable to be accusations of fraud which themselves raise numerous and complex issues, and which need to be disentangled for a satisfactory picture to emerge. Such accusations may be well or ill-founded, and may or may not account, in detail, for the phenomena claimed.

In the case of Rudi's mediumship, certainly its earlier 'macrophysical' phases, there can hardly be question of mere error on the part of sitters if a normal explanation is to be offered: there would have to be gross fraud on the part of the medium as well as, normally, his entourage. There were inevitably a number of major accusations of fraud.

2.21 The Meyer and Przibram accusations

The first serious attacks on Rudi's mediumship came from two physicists at the Vienna Radium Institute, Professor S. Meyer and his assistant Dr. K. Przibram (23). Both attended a few seances mainly at the home of a Major Barauski. In February 1924 various newspapers published accounts of Rudi's 'unmasking' by Meyer and Przibram.

What had happened was that these gentlemen had, at a party given by themselves, simulated a total levitation of a 'medium's' body to the full satisfaction of their guests. The 'medium' was in fact Przibram, who made two important claims: (a) he was able to free a hand for manipulating objects in such a manner as to persuade the audience that the objects

*Some of the episodes in this section, 2.2, especially 2.27, have been discussed and illustrated in a paper appended to this thesis, Gregory, A., 'Anatomy of a fraud', Annals of Science, 34(1977) 449-549. (Label A) In this section numbers in square brackets and Figure numbers, e.g. [459-460 Fig. 2] refer to page and Figure numbers in that paper.

were floating; (b) that he was able to support himself on one leg whilst raising the other horizontally in the air (having slipped one foot out of the tie that bound the luminous cardboard to his feet). Both tricks of course demanded the full complicity of the controller, i.e. the person in charge of the medium's body (Meyer). During seances both trouser legs of the medium were marked with a row of luminous pins, one row of which would presumably have had to be transferred.

Meyer and Przibram claimed that at the seance of 26.1.24 the medium freed his right hand from control, and slipped it back into the controller's hand before the end of the sitting. As regards levitation of the medium's body, they claimed that the only possible explanation was that the medium freed a leg, stepped on his chair and pushed one leg forwards whilst bending his torso backwards. Przibram asserted that Meyer had noticed that the medium's right hand had been freed because it was noticeably cooler than the left. Protests came naturally enough from persons who had themselves controlled Rudi: a mere simulation was not comparable to what actually happened. Schrenck-Notzing answered in some detail (24). The physicists issued a press statement to the effect that they had admittedly not exactly 'unmasked' (entlarvt) Rudi, but had found ways of producing the phenomena in an entirely natural manner.

There is from the point of view of Meyer and Przibram, one very awkward fact: the seance of 26 January 1924 is recorded in the Schneider Journal, and there, under the account of seance events, is the statement, 'Die Kontrolle war einwandfrei' (the control was perfect, literally 'free from objections'), signed 'Professor Dr. Stefan Meyer' [459-460 Fig. 2]. Why did Meyer testify in writing to the excellence of control conditions if he and Przibram considered these so poor as to be useless and, above all, if he claimed to have detected fraudulent manipulation?

It did not take long before the news of the 'exposure' reached England, the relevant correspondence being preserved in the archives of the SPR (25), as Everard Feilding, then Research Officer of the Society corresponded with Kapt. F. Kogelnik. Kogelnik was clearly a trusted correspondent, but

his letters are two-edged: on the one hand he did not agree that the Meyer/Przibram affair constituted any kind of exposure and sent Feilding an (unsigned) article defending Rudi; on the other hand he had come to dislike and distrust Vater Schneider who, he said had become so vain as to fancy he could 'play football with people'. As Feilding persisted, Kogelnik wrote that he attended one seance with Rudi on 14th March at the home of an acquaintance. He had been asked to lead the sitting 'I noticed that all the phenomena were faked'.

The sitting to which he was presumably referring is dated in the Schneider Journals as having taken place on 13 March 1924 in the flat of a Judge Raschofer. Phenomena, including levitation of the medium's body were noted. Kogelnik's signature is not appended, although he is listed (mis-spelt) among the sitters. The episode is puzzling. Why did not Kogelnik, as 'leader', protest at the time and call off what he later described as a fraudulent farce? One would have thought that, so far from sending an article defending Rudi to Feilding, he would have added his voice to that of Meyer and Przibram. There can be no doubt that Kogelnik subsequently (26, 27) expressed total belief in the genuineness of Rudi's phenomena.

In March/April 1927 Harry Price in his house journal, The British Journal of Psychical Research, published an article by Prof. Hans Thirring (28) dismissing the Meyer/Przibram incident, since at least one of the controllers would have to be in on the plot. The accusations were revived by Price in 1933 as though he had never heard of them before (see below 2.27).

2.22 The Vinton accusations

The next heavily publicised attack was made by Mr. Warren J. Vinton (29). The article is such that, had Vinton offered corroboration of the mediumship, it would hardly have been taken seriously, dealing exhaustively with his and his collaborator's, Miss Helen Augur's, feelings, and generalities, rather than providing precise accounts. However, one particular claim is of interest, in that it was subsequently widely accepted as eye-witness testimony of fraud, e.g. by T.R. Tietze (30): Vinton

claimed that during a seance on 9 August 1926 he saw a figure crouched in the cabinet.

In the ensuing controversy, Schrenck-Notzing again played a prominent part (31), and Kogelnik also defended the Schneiders (32). Most of this is of psychological, sociological, or perhaps journalistic interest rather than historical and scientific relevance, and concerns issues such as whether or not Helen Augur was Vinton's wedded wife (Schrenck having ascertained that they had occupied the same hotel room), whether steins of beer and boxes of chocolate had been pressed on the embarrassed Schneider family or greedily seized, whether or not Vinton had passed himself off as having built the Ford works in New York, whether or not Vater Schneider had a shifty look in his eyes, Mother Schneider was 'degradingly obsequious and son Karl a nasty mixture of the two, and so on.

It is, however, interesting to examine the Schneider Journals for the relevant dates [461-464, Figs. 3, 4, 5, 6]. At the first sitting, 30 July 1926, there were present among others, Dr. and Mrs. E.J. Dingwall, and Mr. Vinton. A special paragraph in Vinton's handwriting states, 'I controlled Rudi throughout the sitting except a preliminary period of a few moments. Very interesting and striking phenomena were produced. I am certain they were in no wise produced by the normal body of Rudi'. For the next entry, 2 August 1926, there is a comment by Vinton in German, 'particularly good sitting'. The next entry, for the seance of 9 August 1926 is peculiar, for in addition to members of the Schneider home circle and the Vintons, there were signatures by Americans, a Mr. and Mrs. Cannon from New York and a Mr. Gubish from Meriden, Connecticut. There is also testimony to the effect 'Controlled 1st and 2nd periods of the seance - perfect conditions.', signed 'Mrs. W.J. Vinton'. Squeezed in, between the signatures of participants and Mrs. Vinton's testimony, there are, in Vinton's handwriting, the words 'see note by me written after sitting of Aug. 19 1926, Warren J. Vinton'. At the bottom of the page there are the words in Vinton's hand 'also seen' crossed out.

A note inserted after the 19th August, headed 'Note to sitting of Aug 9 1926 states 'I was told to go in cabinet and pick up tambourine. While doing so I felt a large crouched

form in corner behind medium. I reported this to Vater Schneider next morning, who explained it was collected teleplasm.'

The comment was plainly inserted later, on 19 August, and is in English. Why did he not draw attention to the incident at once? Why did he not subsequently search the cabinet? Who told him to check the cabinet? Clearly no definitive answers are possible. Presumably the other American participants would have made him test his allegations had he voiced them. Was the 'observation' some after-thought, an elaboration of what 'must' have happened? He ends his entry for 19 August by thanking 'the whole Schneider family for their invairable [sic] kindness, courtesy and friendliness'. It is hardly surprising that when the denunciatory article appeared, those whose hospitality he had enjoyed and whom he had assured of his conviction of the authenticity of the mediumship were severely displeased.

The accusations seem flimsy (or would do in any other context). What remain of interest are the categorical assertions that whatever produced the phenomena, it was not Rudi's 'natural body', but had to be due to confederacy: Meyer and Przibram on the other hand had satisfied themselves that they had explained the phenomena in terms of what Vinton called 'Rudi's natural body'. If both parties were correct in believing in total fraud, then phenomena had to be produced in different ways on different occasions.

Whereas Schrenck had a relatively easy task in demonstrating that neither Rudi nor his family could have manipulated phenomena in his own laboratory in Munich, it was far more difficult to show that Braunau seances were not a collective Schneider family effort. In fact it was precisely in order to fend off this accusation that Schrenck proceeded to conduct sittings in Munich, whilst increasingly excluding anyone who could reasonably be thought of as an accomplice of the medium. Since he was successful in this, he himself inevitably fell under suspicion of being in the plot. According to Mr. M. Bird (33), Vinton's Braunau expedition as well as his confederacy theory were inspired in advance by Dr. E.J. Dingwall rather than by Vinton's subsequent deliberations.

2.23 The W.F. Prince accusations

The next denunciation came from a far more expert researcher, the Rev. W.F. Prince (34), who participated in a long series of sittings with Rudi. Careful scrutiny shows that there is no actual evidence being offered that Prince detected any cheating during the 13 seances, although there is a good deal of pre-supposition and inference. His main suspicion was that somehow or another Rudi used his mouth to produce the phenomena, either by blowing at the curtains, or by extracting from some hiding place about his person a secret reaching rod and conveying this to his mouth, possibly by Vater Schneider's help. Yet when, on one occasion, Prince having apparently forgotten his suspicions, a sitter cried out, 'Dr. Prince, Dr. Prince, his [Rudi's] face is in your hands', Prince complains that the sitter was distracting his attention from the curtain movements.

The nearest that Prince came to an actual claim that something suspicious was actually observed was a rather unsatisfactory story to the effect that a sitter, 'X', wrote to Prince afterwards claiming to have seen a small luminous narrow oval shape surrounding a black disk hovering about near Rudi's head, which then withdrew towards the keyhole. Prince speculates that this might have been the end piece of a slender reaching rod extended through the keyhole by Vater Schneider, who had been excluded from this seance.

The supposition that Vater Schneider could, in a strange house (and an upper middle class house with servants), have wandered about and extended reaching rods through keyholes, and whether such a manoeuvre would have been capable of producing the effects described during that seance, were neither examined nor tested for feasibility. And why was not 'X's' identity revealed? It seems surprising to me that a person should refuse to claim responsibility for what amounts to an accusation of fraud. The late Dr. G. Walther told me she thought 'X' was the celebrated German surgeon Sauerbruch, and Mrs. Laura M. Dale believes it was Mrs. Lydia Allison. Either way, it hardly amounts to evidence.

Mr. T.R. Tietze has recently severely taken me to task for dismissing Dr. Prince's report (35), but I can, as I made clear in my rejoinder (36), see no reason for revising my opinion.

It should also be borne in mind that, as Mr. Tietze admits, Prince was so prejudiced that he would hardly have seen any phenomena had there been any.

2.24 The Bird accusations

This latter consideration does not apply to Malcolm Bird, another American researcher who next threw doubt on Rudi's mediumship (37). The paper is very full indeed, written in a manner that inspires somewhat more confidence than Vinton's sensationalistic brashness or Prince's tetchy irritability. Moreover, as a major champion of 'Margery', Bird was not prejudiced against the macrophysical phenomena as such. On the other hand, he had only a single sitting, 11 October 1927, in Braunau and, despite being urged to stay on, he refused to remain any longer to satisfy himself. His interpretation of events was that, whilst he 'deliberately' allowed his attention to be deflected, someone slipped into the room at 11 p.m. who then worked matters as from the 'cabinet' (a corner of the room separated off by a black woollen curtain). This hypothetical accomplice could have been let out again by Major Kalifius, one of the principal sitters, under pretext of letting in Franz Schneider, one of the brothers.

Yet Bird himself was entrusted with the key. Why did he part with it, and why did he not go to the door himself when told that Franz needed admission? Bird makes much of the fact that, whereas to begin with he was constantly asked to inspect the cabinet, such requests ceased once the time had passed when he subsequently thought an accomplice could have been slipped in. But there is no suggestion that anyone stopped him from looking into the cabinet: why should the Schneiders have persisted? Indeed, why did he not have a look during the interval when the 'accomplice' had to hide there, and when no possible objection could have been made against such inspection?

If the Journal is consulted, it is found that there is one minor discrepancy between it and Bird's report. Both agree that the first part of the sitting was blank. However, according to Schneider's account feeble phenomena began at 11.20, whereas according to Bird they started at 11.00 p.m. Had the Schneiders wished to gloss over the fact that an accomplice was smuggled in

at 11.00, one would expect them to err on the side of giving too early rather than too late a time of the onset of phenomena, which would have been easy since Bird signed the book before rather than after the seance account.

Bird, then, based his confident assertion of large-scale conspiracy on a single seance during which he took no precautions worthy of the name, or checked up on a single suspicion.

2.25 The Hoppe-Moser accusations

Dr. F. Hoppe-Moser (subsequently called 'Moser'), a major figure in Central European psychical research, attended one seance at Schrenck's (his serial No. 13) at 9 pm on 22 November 1924. In her huge two volume work, Okkultismus, published in 1935, she devoted a special section to this seance and 'its aftermath' (38). According to this account, she experienced what she unhesitatingly accepted as genuine phenomena, notably the forcible snatching of a handkerchief from her own hand, and the movements of a violin after she had assured herself that it had no attachments.

Nevertheless, the tone of the passage is such as to lead one to suppose that she had been highly critical. About 3 years later October 1927, she had asked Schrenck to be allowed to attend another sitting, but he had replied it was not possible at the time. In March 1928 she wrote to Vater Schneider in Braunau, who pointed out that Rudi was in Munich. She wrote to Rudi, but received no reply, 'Now I knew!' she wrote ('Nun wusste ich Bescheid!').

On these supposed or real snubs or evasions, together with her disagreement with Schrenck's seance report as circulated to participants, she bases a somewhat obscure but very hostile attack. Fortunately, it is possible to reconstruct what, roughly, happened at the time, from letters and seance reports that have been preserved in the archives of the University of Freiburg Institut für Grenzgebiete der Psychologie und Psychohygiene kindly lent to me by Prof. Hans Bender.

She wrote three letters to Schrenck, dated 29 Nov., and 10 and 20 Dec. 1924. These are in her handwriting and she almost certainly did not remember them when she launched her attack over 10 years later. Schrenck's replies are not

extant but some of their content may be inferred. Her first letter acknowledges the unexceptionable conduct of the medium, and admits having without any doubt experienced genuine phenomena. In the second she thanks Schrenck for a copy of the seance report, objects to a mis-spelling of her name, and apologises for having been a disturbing element during the seance: she only regretted that he did not tell her off at the time; however, she considered that her 'lack of passivity' may after all have been a good thing since it meant that her unauthorised actions provided her with 'absolute certainty' that the phenomena were genuine and fraud impossible. She expresses the intention to attend further seances in Braunau. (There are no further entries in the Journal with her name.)

From the tenor of Moser's last letter I suspect that Schrenck had been tactless enough to send her a copy of some highly uncomplimentary remarks about herself made by Professor K. Gruber, a fellow participant during her seance. In this letter, dated 20 Dec., Moser refuses to sign Schrenck's report. She gives no specific reason apart from her lack of experience and expertise, and stresses that 'this refusal in no wise affects my conviction concerning the genuineness of certain phenomena...' She complains that her name has been mis-spelt again.

As a matter of fact, it is a pity that, instead of refusing to sign, Moser did not give her reasons. The report of the sitting in question, though not printed in full in Schrenck's posthumously edited papers is preserved in typescript in the Freiburg archives, and substantially tallies with the extracts from Schrenck's report that Moser quotes in her 1935 book. There are indeed two important differences, but how could Schrenck acknowledge these if Moser did not tell him what they were? These differences concern conflicting interpretations of the phenomena of the greatest possible interest.

Schrenck wrote that 'one' gained the impression that an organ-like member is active in manipulating the violin, and that Moser could convince herself that the violin was held 'as from the curtain by an invisible organ'. Moser wrote that whoever was the 'one' under this impression, it was not she. On the contrary, she felt all around the violin and felt nothing, and it was her impression that the violin was swinging freely in

the air as though on an invisible and intangible elastic support. She also repudiates the assertion that 'one' received the impression that 'a full-sized figure was standing in the cabinet pushing out the curtain', and gives it as her impression that the curtain was being blown outward by something more like an air current.

It seems clear that Moser felt affronted and offended by Schrenck's and Gruber's treatment of her as a tiresome and pushing novice, and she came in the course of the next ten years to forget the precise origin of her grievance whilst retaining considerable hostility towards Schrenck and indirectly towards Rudi. It is clear that Rudi was ill with bronchitis when she wrote and asked for a sitting with him in 1927; his failure to answer her letter in March 1928 is hardly sinister, and does not justify any dark suspicions. In any case, so far as he was concerned, she had, on the face of it, accepted his phenomena as genuine, and any possibly academic differences in interpretation would not concern him. Moser also bitterly complained at the extent to which Schrenck allowed himself to be dictated to by 'Olga'.

2.26 The negative sittings with Woolley et al

Schrenck himself increasingly found himself in open conflict with 'Olga' (39). By 6 June 1928 the time Schrenck had to wait before the onset of phenomena at a given seance had risen to as much as 131 minutes and he told the (waking) Rudi before the seance that in future he would not wait beyond half an hour. When 'Olga' put in an appearance, 'she' said she knew everything, and had come to say goodbye. Schrenck did not object: he felt that even at the risk of losing Rudi's physical mediumship, his own health was being seriously undermined by the long undignified waits in the dark. He hoped that perhaps a new trance personality would emerge.

The next sitting, 12.6.1928, was entirely different from all others to date: the medium's usual tremors, shallow fast breathing and muscle contractions were absent, instead of which he displayed slow, deep respiration. He did not talk, but indicated by gesture that he wanted to write. Schrenck asked the (very leading) question, 'Is "Anton" there?' and the medium

confirmed this in writing with his left hand.

This 'Anton' had been a personification who had occasionally put in an appearance: his alleged sphere of activity was to produce sensations of touch, and these were the only phenomena reported. However, to Schrenck's disappointment, 'Olga' once again announced 'her' presence on 19 June 1928. However, 'she' refused to produce any phenomena until Schrenck had left the room. A few feeble phenomena were then observed. Schrenck was allowed back towards the end, 'she' forgave him, and gave him Rudi's hand in token of reconciliation. However, during the next seance, 28.6.1928, war broke out again between 'her' and the baron: 'she' demanded 'her' music - cheerful military marches and brass band stuff, and he insisted on more classical music. 'Olga' said in that case 'she' might as well go, and did. The next 9 Rudi sittings recorded by Schrenck were totally negative.

Unfortunately for him this wholly negative phase coincided with a visit of a party to Munich from England, led by the SPR's Research Officer, Mr. V.J. Woolley, and comprising his secretary, Mrs. E. Brackenbury, Miss May Walker and Prof. E.R. Dodds (40). Theodore Besterman joined the party for two of the wholly negative set of sittings. These were some feebly positive sittings with Willy.

Why should Schrenck, in the circumstances, have tried to demonstrate Rudi to the British delegation, whom he must have been keen to impress? It is impossible to tell for certain. Probably the fact that he had just 'unmasked' another psychic, O.S., as fraudulent left Schrenck without other mediums to demonstrate.

Rudi's own sittings at home during that summer were blank as regards physical phenomena, they were 'talking seances' (Sprechsitzungen) at which 'Olga' expressed her views on things in general (41). The transcripts are long, ca. 17 single-space typed foolscap pages, and contain passionate shrewd and very detailed denunciations of O.S. and precise explanations of how he had produced his fake phenomena. 'She' also foretold that Rudi would die quite soon, earlier than in 15 or 16 years' time, because of his preoccupation with

aeroplanes, motor cars, and football, all of which she disapproved. As for Schrenck-Notzing, 'she' would certainly get the better of him!

The latter did not admit to the British team that he had quarrelled with 'Olga', rather he suggested three other explanations (42): anti-British prejudice, 'psychic shock' at the unmasking of O.S., and 'bodily causes' - Rudi was said to have impaired his psychic abilities by making love. The first two may be dismissed, since Rudi was quite soon to be very much at home in England, and 'Olga' had been fulminating for months against O.S., and must have positively exalted at his downfall. As regards the third, Dr. Gerda Walther told me she thought it unlikely that Rudi's 'love-making' was more than some rather mild petting; but of course Rudi himself may have believed that this would impair his psychic powers. Indeed, for all we know, this too may have incurred 'Olga's' severe displeasure with him.

Schrenck noted (43) that after the seance of 26 June 1928 sperm staining was found on the bathing trunks Rudi was given to wear during the seance for the first time, whereas previously he had worn his own underwear. It does not seem impossible that the embarrassment this may have occasioned to Rudi, who must have known about this, could have been a contributory factor towards further hostility on 'Olga's' part toward Schrenck and possibly the fact that the ensuing sittings were negative.

Be that as it may, the fact that these seances for the British team were totally blank, coming as they did after the Vinton, Prince and Bird accusations, marked the nadir of Rudi's international reputation and lent support to British and American opinion that his mediumship had been fraudulent.

2.27 The Harry Price accusations

The denunciation of Rudi by Mr. H. Price has been documented and discussed at length (44)*. The accusation was launched on

*In the ensuing sections, HPL refers to Harry Price Library and IMI to Institut Métapsychique International, where the documents referred to were kept.

5 March 1933 in a newspaper, the Sunday Dispatch, and referred to a seance on 28 April 1932. The incident is of some importance not so much because of any light it sheds on the authenticity of the mediumship as on the circumstances surrounding investigations of this type, and the problems encountered in attempts to investigate psychokinetic phenomena.

On 12 February 1929 Schrenck died unexpectedly. In March Price 'found himself in Munich' (45) or, as he later wrote, made a dash in order to secure Rudi's services (46) with the avowed aim of 'once and for all' settling the present status of the mediumship and 'to inform the press'. He certainly felt he had triumphantly succeeded as regards both aims (47). Until Price brought him to international notice, Rudi had been from the point of view of most British, French and American researchers a questionable and not particularly interesting physical medium. Schrenck's papers on Rudi (48) were only published posthumously in 1933 and then not completely. Without Price Rudi might never have been heard of again outside Austria.

Price was and remains an ambiguous and controversial figure. He was without doubt a fearless if at times somewhat uncritical propagandist, see for example (49) [471], and a tireless worker, a genuine enthusiast for psychical research. Although he emerges from my own researches as capable of treachery, lying and double-dealing of the most unpleasant and ruthless kind I cannot subscribe to the wholesale dismissal of him by Hall (50).

Price's establishment in London was the National Laboratory of Psychical Research, one of the numerous organisations in the psychical research field competing for dominance. Of special interest is an article by Price (51) in his house journal, the British Journal of Psychical Research January/February 1929, presenting a sociogram of the almost entirely hostile mutual relations of the various organisations and major figures, and recommending mergers between them all as the desirable resolution of conflict. As will be seen, this recommendation was no mere idle sentiment. Virtually the only researchers exempted from Price's 1929 anathema on fellow

investigators were Lord Charles Hope in London and Dr. Eugène Osty in Paris, both of whom were highly commended.

2.271 Price's three Series of experiments

When he arrived in London for the first time, Rudi was accompanied by Dr. Amereller, an engineer who brought with him Schrenck's blueprints for the electrical control of the medium and who, to save time, had made up the switchboard before his arrival in London. The device consisted of low voltage warning lights controlling hands and feet of the medium and controller by four different circuits. If the medium was properly controlled, four bulbs would be lit. Price added two circuits for the combined hands and feet of all sitters. This device, by which Price set great store and for which he claimed credit, was itself highly controversial in England, where it was widely felt, especially among experienced members of the SPR, that it merely added complexity, without being truly fraud-proof.

Price's seance protocols were similar to those of Schrenck; they were however not circulated to sitters before publication for their endorsement or dissent, and thus were effectively his accounts.

The seance room was on the fourth floor of 16, Queensberry Place, London SW7, now occupied by the College of Psychic Studies.

The first set of six sittings terminated to everyone's satisfaction. 'Olga' demonstrated her usual repertoire. Price issued a £1000 challenge to magicians to duplicate the phenomena, which was not taken up [474 - 475] since the conjurors could not undertake to replicate Price's conditions of experimentation. Price's Second Series ran from October 1929 to January 1930 and contained 21 sittings, which were highly successful.

In Price's First and Second Series altogether 110 different persons were present during sittings, of whom 21 acted as controllers, including Lord Charles Hope, Dr. William Brown (said to have been selected for this office because of his 'obviously sympathetic nature and charming personality'), Prof. A.F.C. Pollard, and Professor Fraser-Harris, 'on account of his experience with psychics, medical knowledge and genial personality'. Price himself controlled most of the

time.

Lord Charles Hope provided most of the finance for the enterprise, but he was manifestly not in charge of proceedings; Price was. The scientists and researchers involved, notably Pollard, Hope, Brown, Rayleigh and Fraser-Harris were sufficiently interested to think it worth while participating in further investigation. Price himself was entirely satisfied and published his book Rudi Schneider in 1930 (52), in which he stated categorically that subsequent 'exposure' of Rudi would never invalidate the present judgment of paranormality, that Rudi was the most tractable medium he had ever encountered, that Rudi never demanded any payment for his services other than expenses though there was no reason why he should not.

By no means everyone shared Mr. Price's exalted opinion of his Rudi research, although dissent hardly ever found its way into print. Notably Eleanor Sidgwick in a letter to Mr. W.H. Salter dated 24 September 1930 and marked 'private' (53) expressed a wholly unflattering opinion of Price's work with Rudi. It is a pity, though understandable in view of Price's vindictiveness towards those who crossed him, that she did not permit her detailed views to be published at the time. Her word, of course, however privately uttered, carried great weight.

It was her suggestion that Rudi might have employed 'a sharp boy or girl', perhaps some servant, to perform tricks from within the cabinet. She actually supposed that some such confederate could have escaped the notice of both Professor Pollard and Lord Charles Hope both of whom were sitting in the 'cabinet' (an arc of curtains 7'3" wide, 8' high, screening off a corner of the room from angle of wall to curtains of 4'10" each), with the explicit purpose of catching such an accomplice. I confess that, on the supposition that both men were sane, this hypothesis over-taxes my own credulity. Nor does there seem to me to exist a vestige of evidence that Rudi could employ (let alone that he did so) any 'sharp boy or girl'. His English was virtually non-existent, he had no contacts in England and little money, and so far as one can tell, he was accompanied everywhere by his hosts. Furthermore the fact

cited by Mrs. Sidgwick that an intruder could be and on one occasion was, introduced into the cabinet without the medium's knowledge is of course by no means the same as the supposition that this could have been done without the experimenter's knowledge.

Now quite a number of people were not at all averse to suspecting Price, but this would have been exceedingly dangerous to utter in writing. There is some correspondence in Paris in which Conan Doyle explicitly, in a letter to Osty dated 6 September 1928 (IMI) had, in another connection, accused Price of fraud; and Mrs. Sidgwick in general made no secret of her low opinion of him.

There followed Osty's investigation in Paris with the successful employment of the infra-red equipment concerning which Besterman, one of the most critical and sceptical of all Rudi's investigators, declared himself satisfied (54).

Rudi returned to London on 3 February 1932 for the Third Series of Price experiments accompanied by his fiancée, Mitzi Mangl, who had also been with him in Paris. Ominously, several London papers announced the arrival of the world's highest paid medium. (Rudi was in fact being paid £10 per week plus his and Mitzi's expenses.)

Rudi gave in all 27 seances from 9 February to 5 May. Of these 8 were negative and many nearly so, others were sufficiently impressive, as will be seen, to convince eminent scientists of paranormality. The electrical control was not employed, but Price introduced an interesting device, a little box table on four legs with a loose top balanced on knife edges. If the target object shifted its weight, this would trigger off a flash and camera, taking photographs of the critical area (55) [483 - 485, Figs. 9 & 10]. An infra-red ray installation, similar in principle to Osty's, was also introduced (56) [485 - 487, Fig. 11]. This was neither made by nor under the control of Price, and was used only occasionally. It was constructed at the University of London Observatory by C.C.L. Gregory, then Director of the Observatory and Head of the Department of Astronomy and Wilson Observer, together with C.V.C. Herbert (now Earl of Powis) then a research student at the Observatory.

The phenomena observed were similar to those noted previously. Photographs were obtained purporting to demonstrate the displacement of objects, and plates are still in existence (HPL). The infra-red also worked, as he always assured me, much to C.C.L. Gregory's surprise.

Rudi left again for Austria on 6 May 1932. On 5 March 1933 Price published an article in the Sunday Dispatch denouncing Rudi as fraudulent, followed by Bulletin IV of the National Laboratory of Psychical Research, 'A minute by minute account of 27 seances' (57) purporting to show that on one occasion, namely 28 April 1932, when Price himself had acted as controller, Rudi had freed an arm at the same time as the displacement of a handkerchief on the special little table was taking place. Price added a good deal of disparaging comment about Rudi's mediumship and threw in for good measure a photograph sent him by Capt. Kogelnik eight years previous (HPL) [491 Fig. 17] showing supposed trickery on Willy's part.

Price's case, stated baldly, was that since his own 1929/30 experiments Rudi's powers had waned to such an extent that he had ever since resorted to fraud, and that consequently the intervening Osty and Hope-Rayleigh investigations were worthless: so was his own Third Series, but then he, Price, had been astute enough to catch Rudi, whereas they had been fooled.

2.272 Overall background of Price's denunciation

In order to understand what turned Price's uniquely high regard for Osty and Hope and his admiration and affection for Brown, Fraser-Harris and their colleagues to virulent hatred, one must look behind the scenes and examine contemporary records, many of them unpublished. To make it easier to follow the sequence of events a time-table of some of the relevant key happenings and documents has been compiled and is reproduced in Table 2.

In order to make the involved and often secret events and their consequences reasonably clear, Price's various relations and transactions will be divided into sections, although it will become apparent that to some extent these overlap.

2.273 Price's relations with French researchers and the Institut Métapsychique International

It has been mentioned above that Price's remedy for the

February 1929	Schrenck-Notzing dies
March 1929	Price in Munich, signs up Rudi for London sittings
May-October 1929	Price negotiates merger with <u>Institut Métapsychique</u>
July 1929	Hope: suspicions of Price fraud during Borley visits
November 1929	<u>Institut Métapsychique</u> turns down merger
April 1929- January 1930	Price's First and Second London Series
November 1930	Price attempts merger with SPR, London
February 1931	SPR AGM turns down merger with Price
March 1931	Osty refuses to publish Price's 'teleplasm' paper
July 1931	Rudi and Mitzi invited to London
August 1931	Price disinvites Mitzi, Rudi refuses to go to London
November 1931	Scathing review of 'teleplasm' paper <u>Revue Métapsychique</u>
January 1932	Osty warns Price against Mitzi
February 1932	Rudi arrives in London for Third Series
March 1932	Infra-red apparatus successfully used at Price sitting
12 April 1932	First written reference to Hope-Price quarrel
18 April 1932	German Brocken 'experiment' organised in London
26 April 1932	Major row at National Laboratory Council meeting
<u>28 April 1932</u>	<u>Seance No. 25 at which 'double exposure' subsequently claimed to have been taken</u>
3 May 1932	Sitting particularly impressive to W. Brown
4 May 1932	Letter Price to Fraser-Harris supporting Osty and extolling Brown
7 May 1932	Letter Brown to <u>Times</u> supporting phenomena
8 May 1932	Rudi leaves London
9 May 1932	Letter Fraser-Harris to <u>Times</u> supporting phenomena
10 May 1932	Letter Price to Ach, claiming 'new epoch' due to Brown
10 May 1932	Letter Brown to Price, asking for cut-down in publicity
12 May 1932	Price expels Hofsten from National Laboratory
14 May 1932	Letter Brown to <u>Times</u> , qualifying original letter
14 May 1932	Letter Price to Brown, protesting that Rudi has never dictated conditions, and supporting Osty's results
25 May 1932	Hope asks Osty to give Myers Memorial Lecture for SPR

27 May 1932	Letter Price to Rudi, warning him against cooperating with SPR
17 June 1932	Night on Brocken Mountain
6 July 1932	Article on Brocken 'experiment' in <u>Listener</u>
13 July 1932	Letter Price to Fraser-Harris, 'not a shred of evidence that Rudi ever cheated.'
27 July 1932	Letter Price to Editor of <u>Nature</u> , upholding Osty's work, vouching for Rudi's authenticity
1 September 1932	Letter Price to Rudi, referring to suspicious photograph
September 1932	Culmination of Price - Brown row
October to December 1932	Rudi in London, Hope-Rayleigh experiments, infra-red successfully used
5 March 1933	Price article <u>Sunday Dispatch</u> denouncing Rudi
6 March 1933	Letter Price to Editor of <u>Nature</u> Rudi 'fraudulent' and insisting on accomplice.

Table 2. Key events leading up to Price's denunciation of Rudi.

ills of psychical research in the 1920s and 30s was the advocacy of amalgamation of the different organisations. In effect he made a number of bids for amalgamating his own 'National Laboratory of Psychical Research' (NLPR) with various different organisations, the first of which, kept strictly secret and confidential in England, was with the Institut International Métaphysique (IMI) in Paris. In a long letter dated 25 October 1929 (IMI) Price set out conditions of the proposed amalgamation. Contents of his laboratory, an itemised account of which was provided, and Price's library, were to be transferred to Paris at the cost of the IMI. Address and title were to remain in London for purposes of correspondence. The property was valued at £7000. Price was to be given a seat on the Council or other Committee and to have access to the work of the IMI. He was to make the first announcement of the merger. He agreed that his own Council (who were kept in the dark as regards these negotiations) would have to confirm the arrangements, but he thought this would be a mere formality.

Now I did not realise when I first published my accounts in Annals of Science [491 - 495] that there had been earlier extensive negotiations going back at least to May 1929, that Richet had written to Price that the Comité de l'Institut Métaphysique at a meeting of 1 June 1929 gratefully accepted Price's generous offer in his [Price's] letter of 28 May 1929, and that this constituted an official letter of acceptance. The existence of this letter, discovered in Paris at the IMI by Prof. S. Mauskopf, was drawn to my attention by Prof. M. McVaugh (58). In the circumstances Price did have reasonable grounds for resentment when on 4 November 1929 Osty wrote to Price, thanking him in the name of the Comité for his generous offer but firmly declining on grounds of lack of space (IMI). He adds that he, Osty, personally had hoped for a different outcome. Price replied curtly 8 November 1929 (IMI), saying that this was quite different from what Prof. Richet had said. There can be no doubt that, whether rightly or wrongly, Price took the French rejection as a personal affront originating from Osty. Relations cooled considerably. When on 17 February 1930 Price suggested that his own secretary, Miss Kaye, should accompany Rudi to Paris, Osty declined firmly but politely (IMI) despite

much further pressurising from Price.

On 9 April 1930 Dr. Gerda Walther wrote a letter to Price (HPL) warning that the Paris investigators were likely to experiment with Rudi without anyone's controlling him. She reminds Price of Rudi's violent and involuntary movements in trance. Yet 'if this movement was photographed, his adversaries would be sure to jump on it and say it was proof of fraud'. Could not Price impress on Osty the importance of adequate control? 'You know how it was in Budapest when Mr. Besterman controlled the medium Layos Papp. He let go Papp's hand and then said he tried to make the phenomena with his free hand...' Price does not seem to have passed on the warning but, at any rate in my view, he remembered it well enough. On 13 July 1930 Hope wrote to Osty (IMI), though only 'in his private capacity, and not as a member of the National Laboratory of Psychical Research' that he would be willing to subscribe to further investigation of Rudi by Osty.

In March 1931 a correspondence ensued which must have sealed the breach between Osty and Price. On 9 March 1931 Price sent Osty for publication in the Revue Métapsychique an article by himself on the subject of the microscopic analysis of 'a teleplasm', said to have been carried out by 'the most eminent analytical chemist in England' (HPL). Osty responded guardedly. Did not Price think it would be of 'vital importance' to be 'absolutely certain that he was dealing with an actual teleplasm?' He advised against publication. Price replied that he was convinced of its genuineness (HPL). Several portions were taken and others had received specimens. 'Owing to the jealousy of another Society I am unable to give full particulars as to how I got it and where it came from. But someone will eventually write a paper on it and I want to be the first'.

Undeterred by Osty's warnings, Price published his article in Luce e Ombra in May 1931. As Osty was to write later Price, having failed to obtain an invitation to the SPR sitting of the medium in question, Mrs. Helen Duncan, secretly arranged for these specimens to be taken. He then secretly, with the medium's husband's help, organised sittings without the knowledge of the

investigating group (59). Price then performed a total somersault: he published the first of the Bulletins of his National Laboratory purporting to show that Mrs. Duncan was a fraud, who worked by regurgitation (60).

The Revue Métapsychique thereupon, in November 1931, carried a most ironical review by C. Quartier of Price's own work (61). Price's reaction was that the 'amusing' article by Quartier read as if he, Price, had actually believed in the genuineness of the 'teleplasm'. 'If Luce e Ombra have elected to say more than I did, that is their affair. The reason why I so much desired to publish my article on the teleplasm was to give the Duncans confidence....'

It is hard to see how Osty could be expected to take Price seriously after this. Nevertheless, he was incautious enough to write him a letter 15 January 1932 (HPL and IMI) in which he advised him not to allow Mitzi to be present at Price's London seances. 'When there are no phenomena she tries to produce some and does so.' On 18 January 1932 Price replied (HPL and IMI) that he was 'very grateful' for the information, asserted that Mitzi would be specially controlled if he were compelled to have her at sittings, and insinuating that Rudi had now turned his mediumship 'into a business', by implication blaming Osty for spoiling Rudi financially. Osty replied (HPL) that Rudi himself had never occasioned the slightest suspicion.

In the summer of 1932, the whispering against Osty began. C.W. Olliver, a French researcher of some note, wrote a letter to Richet (undated, IMI) saying that when in London he had heard rumours against Osty and his work with Rudi, and the presence of a possible 'accomplice' not mentioned in Osty's own report. Richet forwarded the letter to Osty, who wrote a long and detailed reply on 23 September 1932 (IMI). The salient point of this is that, at a single seance, which was entirely negative and the last in its series, a secretary found her knee touched by Mitzi's hand. Osty was not absolutely certain that Mitzi had tried to simulate phenomena and in any case the target objects and infra-red installation were elsewhere and quite inaccessible to Mitzi. He goes on to say that he warned Price that Mitzi 'tries to produce phenomena' and then, Osty complains,

Price went on and played about with his, Osty's, confidential and friendly warnings, to get people to believe that Mitzi had been present at all sittings, and 'that she had produced the very phenomena we were investigating!'. He continues about Price's 'satanic' behaviour and determination to bring about his, Osty's, moral ruin.

Price had now performed another complete volte face. Earlier he had treated Osty's work with Rudi as corroborating his own, and rendering superfluous any further investigation, above all by the SPR, as he had insisted over and over again throughout May, June and July 1932. From August 1932 onwards he took the line that Osty's work was worthless because he had suppressed the presence at seances of an accomplice. The sequence of letters makes it clear that he decided on this change of tack once he had come to accept that Osty could not be persuaded to have nothing further to do with the SPR.

Osty did indeed have reasons for being displeased with Price, whose conduct in failing to pass on Osty's warnings to his own fellow investigators, and in having Mitzi present at all sittings during the Third Series, is impossible to defend. On the other hand, one might ask, what right had Osty to blacken Mitzi's character in so uncompromising a manner on virtually no evidence, and to denounce her to Price in writing, and that at a time when he no longer entertained any regard for the latter's judgment or integrity in view of the 'teleplasm' affair? Osty was to pay dearly for his rash accusation of Mitzi.

Osty continued to be held in high regard by the SPR, and in 1933 delivered the Myers Memorial Lecture (62) on his infra-red investigations with Rudi. In that year he also published a biting and ironical as well as thoroughly accurate article in the Revue Métapsychique, 1933, entitled 'L'étrange conduite de M. Harry Price' (The strange behaviour of Mr. Harry Price)(63) which it might have been hazardous to publish in this country during the latter's lifetime on account of the British laws of libel.

2.274 Price's relations with the SPR

Having failed in his attempted secret merger with the IMI,

Price about a year later launched an all out offensive to amalgamate with the SPR. On 12 November 1930 (HPL) he addressed to all members of that Society a circular and printed letter marked 'Private and Confidential' proposing the merger. His health, he wrote, was poor; he was averse to joining forces with a spiritualistic society; and he was reluctant to let his organisation 'fall into foreign hands'. In return he would 'of course expect to take a part in all major investigations brought about by his agency' and 'generally to cooperate with the SPR'. On 19 December 1930 (HPL) there appeared a second printed letter expressing regret that the SPR had turned down his offer. He had, he wrote, 'failed to instil into the London SPR some new and active blood'. As usual, however, he persisted and privately circulated a census asking members of the SPR to 'vote' in favour of the merger. Mrs. K.M. Goldney in fact proposed such an amalgamation at the Annual General Meeting of the SPR on 26 February 1931 (64). It is perhaps a pity that the Minutes of that meeting are terse, merely stating that Lord Balfour, Mr. deBrath and Mr. Bousfield took part in the discussion. The motion was withdrawn.

Besterman of the SPR asked Price to be admitted to Rudi's sittings, Price replied that Besterman was welcome to Thursday sittings (for fee-paying persons of lesser rank), but that since Rudi had taken a dislike to Besterman the application had better be addressed to Rudi. Rudi's supposed objections to Besterman were almost certainly a fabrication: Rudi subsequently sat for Besterman without any demur, and Besterman made no secret of his endorsement of the Osty results.

In Price's eyes Besterman's sins were (at least) twofold: he had established excellent relations with Osty, intimating that he took Osty's, as opposed to Price's, work with Rudi sufficiently seriously to provide evidence for parapsychical phenomena, for example letter to Osty, dated 13 April 1932 (IMI). He had also stated in print that Price's extension of Schrenck's electrical control was 'very defective' and deprived Price's seances of much value. Price set particular store, not only on the electrical control, but on his own (fictitious) status as its inventor.

On 22 March 1932 C.C.L. Gregory and C.V.C. Herbert took

their infra-red apparatus to a sitting during Price's Third Series without any warning, and the equipment registered irregularities, having been in good order beforehand and never having been out of their possession or control (65).

Hope, as we now know, had been trying to have Rudi investigated independently of Price as far back as 1930. When he realised that as a result of Osty's infra-red experiments and their replication by Gregory and Herbert these effects stood a chance of being taken seriously by scientists in Britain, he decided that an independent investigation was now essential. If Hope imagined that this could be achieved without too much unpleasantness, and that Price would simply allow Rudi to be investigated in England by another Committee not under Price's direction, he was badly mistaken. Price was livid with rage, as emerges clearly from correspondence during April 1932 preserved at the HPL [501 - 502]. Price did not consider his work to be in need of replication: he talked of 'conspiring' against himself, and that it was 'like the host at a dinner party having his throat cut by his guests' (66).

The attitude of the Sidgwick group, that pillar of the SPR establishment, towards Price as well as Rudi, has already been mentioned. To them Price was a thoroughly distasteful upstart and vulgarian.

2.275 Price's relations with members of the National Laboratory of Psychical Research

Members of the Council of Price's National Laboratory of Psychical Research (subsequently abbreviated NLPR) were almost invariably also members of the SPR, and the sub-division here is therefore somewhat artificial. However, it was precisely their dual allegiance that largely occasioned Price's wrath, and the convoluted involvements can be made somewhat clearer by making the distinction.

One of the crucial events was a major quarrel at a Council meeting on 26 April 1932 (67). Price insulted Hope, who resigned from the NLPR. A furious correspondence ensued between Price and Fraser-Harris, in the course of which Price came close to calling the latter a liar over the matter of arrangements with Rudi, and in which he returned to the theme of 'conspiracy' to take Rudi away from him. On 3 May 1932

Harris wrote (HPL) in the most frigid tones that he could not permit terms such as 'conspiring'. Price's reply on 4 May 1932 (HPL) to Fraser-Harris is of considerable importance, since it is full of jubilation at the brilliance of Rudi's sittings and supports Osty's findings, whereas he was subsequently to claim that Rudi had been guilty of fraud on 28 April 1932 and that Osty's sittings were totally worthless. Price was particularly triumphant that Rudi's seance had impressed Dr. William Brown 'whose opinion is worth a dozen of those of Lord Charles Hope and his scientific friends'.

There is no doubt whatever that Brown was indeed deeply impressed, to the point of writing a letter to the Times, which appeared on 7 May 1932 [504] describing the phenomena at Rudi's seance, expressing belief in their genuineness and stating that they were 'worthy of the closest scientific investigation'. Prof. Fraser-Harris wrote a letter to the Times published May 1932 [505], supporting Brown's views, and adding testimony of his own to the effect that he had seen 'a so-called "pseudopod" or "phantom"'.

It would be an understatement to suggest that Price was pleased. As Mrs. Goldney wrote in a contemporary note: he was 'absolutely DELIGHTED' (68), and he embodied his jubilation at Brown's support in various letters, e.g. on 10 May to Prof. Ach in Göttingen (HPL) and indeed to Vater Schneider, also dated 10 May 1932 (HPL).

Unfortunately for Price, Brown had second thoughts and expressed these in a second letter to the Times on 14 May 1932 [507 - 508]. He implied in this that his 'feeling of conviction at the time' that the phenomena were paranormal had not lasted. He mentions at some length Osty's infra-red work, and generally suggests that all sorts of considerations about the circumstances surrounding sittings were needed to help one come to a final verdict. He goes on to say 'I am still unable to dispute the genuineness of the phenomena...on the other hand extensive lacunae in my knowledge of facts present me from going sponsor for the phenomena...intellectual conviction can only come, if at all, after much more stringent scientific investigation, carried out in a university laboratory or in the seance room of the SPR with trained scientists or psychical researchers as

sitters. In saying all this, I am not deprecating the very important preliminary work that has already been done here in England and on the Continent, but I am emphasising the need for confirmatory evidence and of systematic verification... Whether genuine or spurious, Rudi Schneider's trance manifestations are worthy of the closest scientific study...'

Dr. Brown's demi-recantation is one of the major and most far-reaching instances of the celebrated phenomenon of fading conviction, so graphically described by William James (69) and called 'retrocognitive dissonance' by Brian Inglis (70). There can be no doubt that Brown had been totally convinced and had written an enthusiastic letter to the Times on 4th or 5th May 1932, which he substantially toned down and qualified in a letter dated 11 May 1932. It is, from contemporary correspondence and documentation, quite clear that Brown did not merely undergo an internal psychological shift in the sceptical direction, although such factors may have played their part.

Things had not gone smoothly at Oxford for Brown since his first letter to the Times: by associating himself with Price he had laid himself open to more than he bargained for. There had, in the first six months of 1932, been numerous (often unflattering) references in newspapers less staid than the Times about a characteristic and sensational coup Price was cooking up for Walpurgisnacht celebrations on the Brocken in Germany (71, 72) [506 - 507]. The aim of the 'experiments' was described as being to test whether ancient magical ritual would succeed in turning a goat (led by 'a maiden pure in heart') into a 'fair youth of surpassing beauty'. As reported in The Listener for 6 July 1932 by Price himself, festivities eventually took place on the Brocken on 17 June 1932, where the author described how, accompanied by 42 press photographers, 73 pressmen and a cinema team, he demonstrated to absolutely everyone's satisfaction that despite the scrupulous observance of the venerable ritual, despite the appearance of a pure maiden in the person of Miss Ueta Bohn and the use of a kid specially selected at birth, the goat remained a goat. 'The scoffers', Price concludes, 'will tell us that because we had no faith the experiment was not conclusive; in other words that the magical formula will not work automatically. That is all very well, but what state do we

have to induce in order that the magical metamorphosis shall take place?'

Now this was of course some weeks later than Brown's first letter to the Times. But we do know that there was a good deal of uproarious advance publicity in the German press, much of it preserved in cuttings at the HPL. And this may well have reached the eyes of Einstein and Lindemann (later Lord Cherwell) in Oxford, both of whose mother tongue was German. Whether or not they did, we do know from a letter of Hope to C.C.L. Gregory dated 13 May 1932 that 'Dr. Brown has been having rather a thin time of it at Oxford University being laughed at by Einstein and Lindemann among others' (73) [506]. We also know from correspondence between Hope and Osty July 1932 (IMI) that repercussions of the Brocken affair did damage to Price in serious circles.

It must have been clearly impressed upon Brown, judging by the tenor of his second letter to the Times, that he could only afford to 'go sponsor' for the phenomena if he had rather more reputable antecedents and follow-ups than provided by Price: and it was precisely this implication of his letter that was, most violently resented by the latter. Brown may have thought that, by expressing a sort of personal conviction and 'not deprecating' Price's investigations he had to some extent covered himself against the wrath to come. In this he was woefully mistaken: the storm of hatred that Price unleashed against him in letters to and about Brown remains enshrined in the correspondence of the Harry Price Library, most of it baiting the clearly conventional and somewhat stuffy doctor with having 'solicited' publicity, and some of it attempting to stir up trouble between, for example, Osty and Brown.

The only letter that has any direct bearing on the question of Price's subsequent accusations against Rudi is his first to Brown, dated 14 May 1932 (HPL). In this he launches a furious attack on Brown for having given comfort to his enemies, 'Salter, Lord Charles Hope and the rest of the SPR people (mad with jealousy at the success of the Laboratory)'. The 'sting' of Brown's letter was the implication that 'an SPR investigation would be persuasive, whereas those at his Laboratory were not. He hoped, he wrote, that Osty would not see Brown's letter (a

copy of which he immediately sent Osty adding the assertion that Brown had belittled Osty's work thereby). Price further wrote (HPL):

...I do not propose to publish a further report of our sittings with Rudi...the Third Series...Never once has Rudi dictated or even suggested...he has never questioned any control or experiment and has cheerfully obeyed every order. Osty mentioned the same compliance on the part of Rudi in a letter to me a few weeks ago...

This letter to Brown reads oddly as having been written by a man who was to claim a year later that he had by that time found conclusive photographic evidence of Rudi's fraud in late April or early May, and who was persuaded of fraud and of the worthlessness of Osty's control conditions.

Ironically, on 13 May 1932 (two days before the second Brown Times letter, Price had unceremoniously expelled Prof. Nils Hofsten from the roster of Foreign Correspondents of the NLPR on account of the latter's 'damnable' conduct. His misdemeanour had been to give every indication of belief in Rudi at his 1929 seances, and then launching an attack on him in 1932 (HPL) [516 - 517] .

Price's 'incriminating' Rudi photographs were kept as a closely guarded secret from most of the Council Members of the NLPR, and indeed members of the SPR. The exception was Mrs. K.M. Goldney, then joint Hon. Treasurer of the NLPR (and subsequently Organising Secretary of the SPR). At some date which she can no longer recall, but later than 28 April 1932 she told me that she found Price in a state of 'pleasurable excitement': he had a secret to tell her which she had to promise 'word of honour' not to pass on. Price then called in his secretary, Ethel Beenham, to witness that Mrs. Goldney had sworn not to tell. He then said 'We've caught him' and showed her the double exposure photograph. He told her he would publish this when it suited him and that it would serve those investigators right who were trying to make an independent investigation of Rudi. Mrs. Goldney felt bound by her promise to which Price kept her when she later begged to be released.

As regards Miss Beenham, after Price sprang his accusation Hope, in a letter to W.H. Salter (75) toyed with the idea of

obtaining a statement from her without much hope that this would be feasible. Apparently Miss Beenham had 'let it out' that Price had told Rudi at the time that 'the matter of the photographs' was 'of no importance' - and Rudi said the same thing. Years later, after Price's death in 1948, Miss Beenham did make a statement (76). According to this she says she arrived at the office one day (she could of course not remember the date) and was called in by Price who was developing photographs in the darkroom. He showed her the freed-arm photographs, expressing himself speechless with amazement. Almost immediately Rudi and Mitzi arrived and were shown the photographs. No definite accusation was made; Price afterwards absolutely forbade her to mention the matter to anyone. She was quite sure that he was 'terribly spiteful' against Lord Charles and he was waiting for a chance to hit back at him'. If this account is roughly true, then Price must have had cognisance of the photographs no later than 6 May 1932, since that is when Rudi and Mitzi left London. There is also a questionnaire filled in by Rudi (77) according to which he and Mitzi were shown some puzzling photographs and bound to silence.

There does seem to have been some talk in London about photographs in connection with Rudi, since there is a letter dated 12 July 1932 from Fraser-Harris to Price (HPL), although there is no indication of any suspicion that Price might have turned hostile to Rudi: 'Don't you think it would be a good thing if some of us met and discussed the photographs taken during recent sittings? I for one have never studied them, and only glanced at them in artificial light casually before one or two sittings. Certain appearances are indeed in need of interpretation.' Price replied curtly and by return, 13 July 1932 (HPL): 'Yes, do come along and examine the Rudi photographs. There are only a few rough prints, but they are quite clear...There has not been a shred of evidence published that Rudi ever cheated'. This was hardly the kind of letter that might be expected from one in possession of incontestable evidence of fraud. Coupled with the statements made to Brown that he was not likely to publish anything about the Third Series and the Hofsten episode, this must have thrown all his fellow members of the NLPR completely off the scent.

It is indeed quite clear from all the correspondence and contemporary testimony that Price regarded the NLPR as his show, his fellow Council members as his guests and himself as their host, that he permitted them not the slightest autonomy or even responsibility (if he could prevent it) as regards the conduct of the Laboratory's affairs or the Rudi sittings. He clearly resented the fact that the infra-red apparatus was entirely in the hands of 'Lord Charles Hope's scientific friends'. He had assured Osty that amalgamation with the IMI was a mere formality, he plainly did not consult the members of his Council before publishing Bulletin IV as is clear from a letter that H.G. Bois, V. Cochrane-Baillie, D. Fraser-Harris, A. Dribbel, K.M. Goldney and C. Richards addressed to Rudi Schneider on 16 March 1933 (78) [522] dissociating themselves from the allegations contained in the Bulletin.

There were numerous resignations, and the National Laboratory seems to have disintegrated as a going concern, although the books and instruments and, of course, the psychical research activities of Mr. Price remained. It does not seem that Price believed that his actions would destroy his own record as Rudi's investigator, nor even the Rudi mediumship itself. Indeed on 24 January 1934 the Senate of the University of London resolved, largely on the strength of the Rudi Schneider experiments, 'that Psychical Research is a fit subject of University Study and Research' (79) [548] while continuing to refuse the offer of Price's equipment and Library on his own terms.

His search for mergers did not cease. There were numerous offers (HPL) to the College of Psychic Science, then an explicitly spiritualist organisation, and eventually in 1937 an overture to the University of Bonn, after personal negotiations between Price and Adolf Hitler (HPL) who referred the matter to the Reichs- und Preussisches Ministerium für Wissenschaft, Erziehung und Volksbildung, the Innenministerium and the Auswärtiges Amt. It was then passed on to the University of Bonn, on behalf of which Dr. Hans Bender wrote to Price (HPL) on 20 March 1937 that his offer was in principle acceptable, and that the German government would confer on him the Red Cross Medal 1st class. Nothing ever came of this deal either. The

correspondence is preserved in the HPL at the University of London, which finally inherited the Library after Price's death on its own terms.

2.276 Price's denunciation and its immediate repercussions

In the autumn of 1932 Rudi, despite all Price's strenuous efforts, came to London for the Hope-Rayleigh SPR experiments, and remained until early in the next year. Price published his Sunday Dispatch 'exposure' on 5 March 1933, followed on the next Monday by Bulletin IV purporting to prove fraud, in that Rudi had freed a hand whilst phenomena were in process. Hope was in France when the scandal broke, and in a letter dated 10 March to Price (HPL) declared himself 'interested' to know who were the 'we' who had caught Rudi in fraud, and why Price had not seen fit to inform members of his Council of his 'discovery'. He also asked to know how Price had come to solicit himself, Hope, among others for financial donations towards publication of the Report and subsequent Rudi investigation, without ever mentioning either photograph or suspicions.

The letter that most clearly conveys the stance that Price was now to take up was addressed to the Editor of Nature 6 May 1933 (HPL) in which Price stated that Rudi had refused point-blank to sit without his 'accomplice':

....It had been arranged to hold a series of sittings during the summer of 1931, but negotiations broke down because we refused to allow the 'confederate' (a close friend of Rudi's) to accompany him. When we found that Rudi would not visit the laboratory without his friend, we consented but - after Dr. Osty's experiences - special precautions were taken to keep her under close observation during seances.

Although Dr. Osty now admits that he detected Rudi's friend producing the 'phenomena' herself, his report contains no mention of this and the fact that she was present at any experiment is not recorded in the account prepared for the public.

Stress has been laid on the fact that our cameras caught Mr. Schneider evading control, but so long ago as 1924 Professors Meyer and Przibram detected Rudi producing telekinetic movements of objects by means of a freed arm. That he can free an arm from the usual tactual control (and without the controller's being aware of the fact) has now been proved by our photographic evidence.

Mr. Schneider has promised to visit us during the coming autumn.

Unlike Price's earlier defence of Rudi, in which he had vouched for Osty and which the Editor had returned to him, this letter was duly published in Nature (80). It is of course a tissue of suggestio falsii. Osty did not 'now' admit that there had been a 'confederate': Osty had warned Price before the Third Series that Mitzi might cheat, a warning that had been in Price's possession for six months when he had defended Rudi and Osty to the Editor of Nature in July 1932 - which was long after Price's alleged discovery of fraud in April. Four months after Osty's warning Price had in writing (14 May 1932) assured Brown that Rudi had never dictated conditions, and on 13 July 1932 Price had assured Fraser-Harris that none of the earlier exposures amounted to anything. Price had, as a matter of fact, known about the Meyer-Przibram affair at least as early as 1927 (see above). Rudi had indeed refused to come to London without his fiancée after she had been invited: by spelling his 'laboratory' with a small 'l' Price conveyed the false impression that Rudi had refused to give seances without Mitzi. Every other researcher, before or after Price, friendly or hostile, agreed that Rudi never refused any seance conditions. As for 'stress has been laid...' who but Price himself had stressed the freed arm photograph? The idea that this could be done without the full complicity of the controller rested simply on Price's assertion.

Price now maintained that he had not kept the photographs particularly secret except from those members of his Council who had been 'disloyal' to him, and had 'plotted' to 'get Rudi away' from him. In a long letter dated 3 July 1933 to Feilding (HPL) he wrote that when he discovered the photographs, 'I was not on speaking terms with Hope; Dr. Brown had played me a dirty trick in publishing that second letter to the Times... Fraser-Harris and Cochrane-Baillie and Evelyn were so hand-in-glove with Hope that I dared not show the photographs to them if I did not want Hope to know about them...'

Price must have made his 'discovery' no later than 6 May 1932 when Rudi left London, since there is no doubt that he showed him and Mitzi something by way of 'suspicious-looking photographs'; if Price himself is to be believed, he made the 'discovery' the very next day, i.e. 29 April 1932. By that

time he was indeed no longer on speaking terms with Hope, but Brown's first enthusiastic Times letter appeared on 7 May, whereupon Price wrote large numbers of letters to say that Brown's testimony had inaugurated a new era. Brown's second qualifying Times letter did not appear until 14 May, and since Mrs. Goldney took him to task for letting Price down on 23 May 1932 (81) [537] she could hardly have been aware of the incriminating photographs at that time.

The question Feilding might reasonably have asked was why, in view of Price's alleged discovery of fraud, should he ever have condoned Brown's first letter to the Times, and why should he have treated his second, more cautious, letter as 'a dirty trick'? Had Price indeed been in possession of evidence of fraud when Brown wrote his second Times letter, Price's proper course of action would of course have been to congratulate him on his insightful addendum!

2.277 Interpreting the evidence of Price's accusations

On the face of it, at some point during seance No. 25 at Price's Third Series, Rudi's hand was not held by that of his controller, Price. There had been, claimed Price, an accidental double flash,

The first flash caught Rudi's arm as it was held out straight behind him, the second flash ignited when the medium got into position again with his head and body turned towards the screen reflector...
(see below Fig. 1)

In other words, Rudi shifted the handkerchief from the counterpoise table, thus igniting the flash that released the picture, and then quickly got his arm back into position in time - about half a second later - when the second flash occurred. Price says in his report that after experimenting in the week following 28 April 1932, he himself succeeded in snatching an object off the little table and getting back into position before the bulbs ignited. Presumably Rudi did not have access to Price's seance room there to practice snatching handkerchiefs off the table; nor did Price discuss how he himself as controller could possibly have failed to notice that Rudi's hand had been free, beyond saying that he was 'unfit to control' because he had a toothache. He did not volunteer why, in that case, he did control, nor why he dismantled the photographic apparatus

immediately afterwards: one might have thought that now it would come into its own!

In his denunciation of Rudi, Price relied on a simple psychological device: he first gave his interpretation of the photographs as established fact; he then proceeded to build upon that 'fact' speculations such as 'The question now arises whether any of the phenomena we saw at the seance of April 28th were genuine. The fact that Rudi can evade control has set us wondering...' By 4 April 1934 in a letter to Sir Richard Gregory (HPL) Price wrote that he thought the entire Third Series to have been fraudulent.

To the best of my knowledge, most of Price's co-investigators (or co-participants) placed a different interpretation upon the double exposure, namely that the first flash led him to give a violent jerk freeing his hand, the second exposure showing the freed arm. This was the view of Fraser-Harris (82, 83) and C.V.C. Herbert (Lord Powis) who consulted a photographic expert (84) [525] who gave it as his opinion that the image with the free hand was the second of the two as it was darker: apparently exposed emulsion is more sensitive than virgin emulsion. Hope wrote a more detailed account (85), pointing to the absurdity of supposing that Price could have been in ignorance on the numerous other occasions during that very seance immediately preceding and immediately following the alleged episode at 10.50 pm when phenomena had occurred, and sitters had been enjoined to 'hold tight':

...Are we to believe that 20 times during that evening alone Rudi freed his hand from Mr. Price's hand without Mr. Price knowing it and that 20 times he succeeded in getting it back into Mr. Price's hand also without Mr. Price knowing it? Or that Mr. Price for minutes or even hours together was holding nothing in his right hand and making no effort to find Rudi's left wrist? I find either of these suppositions incredible in a man of anything like Mr. Price's experience.

Hope also pointed out that the sheer distances involved would have made it impossible for Rudi actually to reach the handkerchief with his hand. His legs were controlled, which is clear from the photograph.

How could Rudi with his left hand move an object of at

least 2 ft 7½ inches from the right side of his chair? It is easy to reconstruct the scene, and it is not possible to fault Hope's reasoning, both with respect to the controller of the medium, and also as regards the distances involved. There is further independent testimony from a medical man, a Dr. Hutton Chisholm, whose only sitting with Rudi was the famous No. 25 of 28 April. In a letter to Fraser-Harris Chisholm (86) wrote that during the sitting Price claimed that 'Olga' had cured him of toothache, that Price had gone out of his way to insist on the excellence of the control conditions and Chisholm believed fraud was out of the question. Miss Irene Manby (87), another sitter at this seance, also stated that Price virtually bullied participants in his determination to demonstrate the perfection of his control. Indeed, in the Report itself there is reference to 'Olga' stroking 'Onkel Harry's' cheek to cure his neuralgia. Why should Chisholm, a doctor whose only sitting this was, misremember the claim that 'Olga' had cured Price's toothache?

The majority of participants then were inclined to take the line that there had been an accidental double flash and that Price in his virtually maniacal hatred of his 'enemies' used this in the most nefarious manner, correctly anticipating that any accusation, however silly, was bound to stick and be taken seriously well beyond its deserts, especially if supported by just the sort of campaign of innuendo and character assassination Price proceeded to engage in.

Hope and Osty were somewhat more sceptical concerning the accidental nature of the double flash. There were altogether too many synchronous accidents: the timing of the incident after the NLPR Council row; the precise split-second coincidence of a malfunctioning bulb with Rudi's freeing a hand; the malfunctioning of the ceiling camera by 'fogging' on this precise occasion; that Price himself happened to be controlling and happened to have a toothache; and the fact that the photographic apparatus was dismantled the very next day. Osty whilst not dotting 'i's and crossing 't's plainly let it be inferred that he thought the photograph was no accident, especially when viewed against the background of Price's behaviour before, during and after the crucial seance (88).

If the double exposure photograph was no accident, then Price must have contrived it. Price could have arranged beforehand for a double flash so that he, as controller, could have let go the medium's arm, thus obtaining a double exposure of a held and a free arm. The 'neuralgia' could have served as an alibi for poor controlling. Price himself had installed the photographic equipment and dismantled it immediately for reasons unstated. Arranging for a double flash would have been particularly easy because he used two bulbs in his Vaku-Blitz flash apparatus after an alleged failure during an earlier seance (No. 7, 25 February 1932) (89) [528] on the part of a 'faulty detonator'.

It is a curious feature of the extremely bitter controversy that ensued that Price's opponents hardly seem to have bothered to examine with any degree of care the pictures he printed. There is in my view at least a major objection to both the 'Price-letting-go-Rudi's-hand on purpose' theory and particularly to the 'Rudi-accidentally-wrenching-his-hand free' theory: that is the actual appearance of Rudi's back on the photograph. If Price deliberately released Rudi's wrist, possibly giving it a push, one would have expected his left arm to fly past the left (far) side of the chair back; or if he twisted Rudi round or Rudi twisted himself round, so that his left hand shot past the right (near) side of the chair back, one would have expected considerable rucking and twisting of the striped pyjama jacket. However, the picture shows Rudi's back in ostensible placid repose. The appearance seems to me difficult to reconcile with the theory that Rudi's left hand flew past the right edge of the back of the chair.

It was when I asked the late Mr. Colin Brookes-Smith for his expert opinion as an instrumentation engineer and photographer of many years' standing, whether it would have been possible to rig up a double flash in advance, and he agreed that it was, that he first suggested the idea that the entire photograph on which the accusation was based might be a fake. The negative plate is No. 530 in the Harry Price collection, from which Figure 1 [490, Fig. 16] and Price's Fig. XX (p. 176 of the Bulletin IV) were prepared. With the permission of the Collection's Curator, Mr. A.H. Wesenecraft, the brown paper covering most of this plate but for a window was

removed. My Figure 1 is a reproduction of the picture after the removal of the brown paper.

Mr. Brookes-Smith found that two diagonal lines from corner to corner crossed at precisely the image of the extended arm, which he found it difficult to attribute to chance: such centering would have greatly facilitated superimposition of one or more plates some of a photograph of a dummy. In his view there were originally three plates: an A plate showing an extended arm and back; a B plate taken at the actual seance; a C plate resulting from a re-photographing of a retouched composite print made by superimposing A and B: Price on this view destroyed A and B and largely covered C with brown paper.

In my paper in Annals of Science (90) [528 - 533] I gave the case in favour of the fake photograph theory, the main arguments in favour of which are:

1. The placid appearance of the back, both exposures of which are parallel to the camera. Had Rudi twisted round, the pyjama stripes would have shown this. For his left hand to fly past the right side of the chair back he had to twist: yet there are two 'Rudi back views', both suggesting that Rudi was bending forwards (Fig. 2).
2. The detail of the feet, when the obscuring paper had been removed suggests, at least to me, that they are at right angles to the back [532, Fig. 21] (Fig. 3).
3. The fact that the stereogram shows evidence of not being of the same incident as that in the main photograph [530, Fig. 18].

Dr. Alan Gauld (91) has criticised my original interpretation of the photographic evidence (in which he was supported by Dr. V. Harrison (92)) on a number of grounds, some of which I accept, but some, and indeed most of which, seem to me very doubtful.

I originally thought the brown paper tape, which obscured the detail of the feet and legs, was necessarily suspicious, the more so since I could not find any other glass plate that had been so treated in the Harry Price collection. I accept Gauld's statement (93) that this could have been ordinary procedure normal at the time, and that it might have an innocent explanation.

Dr. Gauld was probably right in taking me to task for over-stressing discrepancies between Price's enlargement of the left hand stereogram and the original left-hand stereogram, and I accept that these might be due to trimming and poor printing and re-touching. He may also have been right in suggesting that what had presented itself to me (and others) as 'a strange white vertical object' in the left-hand stereogram could be the black covering of the lamp, appearing as white because of the light reflection of the flash.

On the other hand, I remain unconvinced by Gauld's assertion that I am mistaken in supposing the feet to be at an angle of 90° to the camera. Moreover, I cannot believe that the stripes of the pyjama jacket could at all easily, as Gauld claims they could, have presented the placid appearance nearly square to the viewer facing the camera (and at about right angles to the feet) if Rudi was in fact twisting, or being twisted between flashes, i.e. in about half a second. Actually Mr. Brookes-Smith tried it out, with the help of a young friend wearing a similar old-fashioned loose pyjama jacket, and he could not replicate a placid appearance. In any case, there are two back views, neither of them twisted.

Dr. Gauld claimed that I was mistaken in believing that appearances suggested the sleeve to be empty (which would of course at least tend to support the 'fake photo with dummy' theory). He says he could detect the fingers of a lightly clenched hand in my [p. 490, Fig. 16]. I cannot. We seem here to be in Rorschach-blot land. The fact cited by Gauld that a print of the picture published by Price years later should show clearer evidence of a faint hand is, in the circumstances, hardly a germane argument!

Gauld might perhaps be right in speculating that Price may have hung up the bolster to protect the glass case, and not as I suggested to act as a photographic backcloth: I do not see how this could ever be decided, and arguments could be put forward on both sides. Gauld claims that the fact that the bolster was striped makes it a poor backcloth, whereas to me it seems ideal: the stripes blend perfectly with the pyjama stripes enhancing the whole appearance of the sleeve. But why position Rudi so near the glass fronted case if this was in



Figure 1. Seance of 28 April 1932 during which Rudi allegedly freed his hand from the control of Price as controller. Photograph prepared from the original glass plate at the Harry Price Library, University of London, after masking tape had been removed.



Figure 2. Enlargement of detail of Figure 1, showing back. Note stripes of bolster, looking like pyjama sleeve stripes, and dual back view, one slightly forwards.

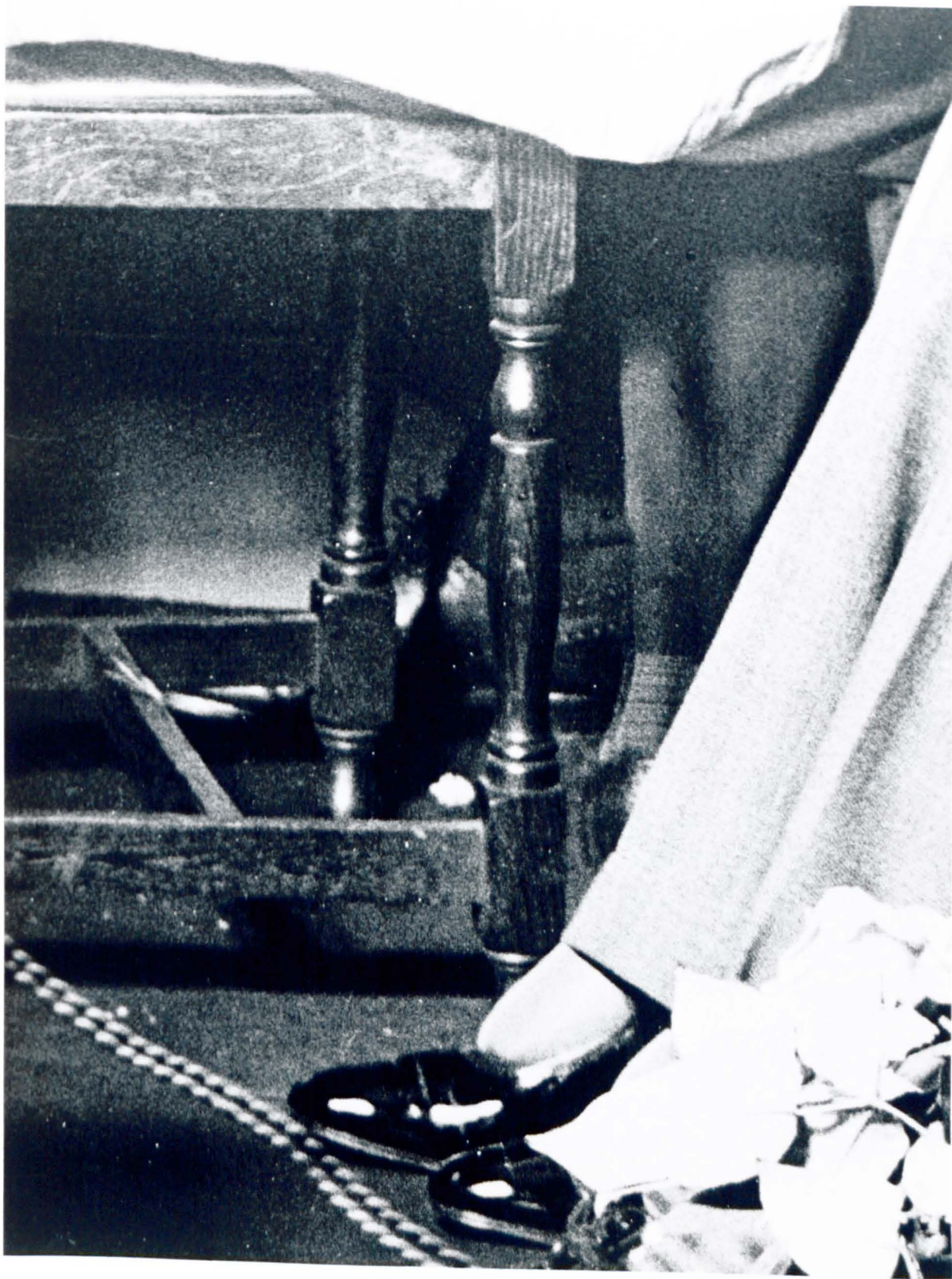


Figure 3. Enlargement of detail of Figure 1, showing Price's right foot on far side of chair legs, and Mrs. de. Guernon's feet in the foreground. Rudi's legs are shown in part and clearly identifiable. Trouser fold on right knee and part of right leg shows no movement, left leg, sock and shoe in profile show double margin due presumably to slight movement during double flash.

danger from him, as Gauld supposes? And why wait until seance No. 25 to protect the glass (and then immediately dismantle the whole photographic apparatus)?

It should also not be lost sight of that Price published rather poor prints of his (excellent) plates. The fact that unfortunately his illustrations, referring to his figures, are called 'plates' seems to have created some confusion. He did not permit anyone access to his plates in his lifetime, only to such prints as he published, and these are thoroughly unsatisfactory.

Perhaps the suggestion made by Gauld with which I take issue most strongly is that, had Price really faked the photographic plate, he would have destroyed it. All the evidence adduced by me in connection with this incident shows plainly and unambiguously that that is precisely what Price might well not have done. Not only was (on the fake photo hypothesis) the final plate a photographic and political pièce de résistance, he was manifestly given to preserving incriminating evidence of double-dealing in his files. Gauld says it would have been 'sensible' to destroy it. But, as I pointed out in my rejoinder (94), good sense would have dictated that Price should destroy most of his outrageously double-dealing correspondence. Hall's carefully, if unlovingly, documented Search for Harry Price, even if some of the condemnations are exaggerated, bears eloquent testimony to Price's reckless audacity and apparent belief that he could get away with anything, and there seems to be no doubt that in numerous other areas of his life Price engaged in quite blatant falsification without bothering to cover his tracks much. 'The circumstances of Price's pose as an archaeologist differed from his blatant plagiarism as a numismatic journalist in at least one important particular. In the latter activity he was not exposed, while in the former he was.' (95).

In the SPR Journal following Gauld's review, Grattan-Guinness (96) stated it as his opinion that the original plate could hardly have been a double exposure since the peripheral parts of the photograph are completely sharp and show normal illumination rather than over-exposure, and that the view in the main picture is not compatible with the allegedly simultaneously taken stereogram (97). Brookes-Smith also wrote in support of the possible fake-photograph interpretation, his principal points being: 1. that since enlargements

of the original showed that several sitters had their eyes wide open and would have seen Rudi turning half round and freeing his hand; 2. that enlargements of the leg detail showed that there is no doubt that Rudi's foot was pointing to the right, i.e. at right angles to the camera, and that he hardly moved more than an inch; 3. that the empty appearance of the sleeve and the failure of close examination of the highlights of the book case to reveal any hand suggested a dummy; 4. that examination of the stripes of the pyjama jacket strongly suggested this view to be incompatible with the foot position.

Both Grattan-Guinness and Brookes-Smith agree that there is no proof that the actual photograph was the result of super-imposed plates, and that we shall probably never sort the problem out for certain; nor had I at any point claimed that we could or that I had. My overall conclusion had been that 'to me the fake photograph theory presents the fewest difficulties, but others may judge differently' [538] .

The trouble is that there are objections to all interpretations, at least all that I have been able to suggest. I will briefly list these here and then equally briefly discuss them.

1. Rudi freed his hand from Price's control without Price noticing this and stretched out his hand towards the counterpoise table. He then rapidly re-inserted his hand into Price's. On this, Price's, interpretation the first flash shows Rudi's freed arm, the second shows him controlled.
2. As a result of the first flash, Rudi gave a convulsive jerk, Price let go Rudi's hand, and there followed the second flash. Price may or may not have noticed at the time, but eventually made use of the accident to discredit Rudi and his 'enemies'.
3. Price controlled and contrived a double flash, with the intention of possibly later discrediting his 'enemies'. He released Rudi's arm after the first flash so the first picture shows Rudi held, the second freed.
4. Price controlled and contrived a double flash, even set it off tactually during the seance. Immediately beforehand, he swivelled Rudi round by 90 degrees, so that

his back would be to the camera, and his left arm could more readily fly past the right chair back. He then set off a flash and immediately let go Rudi's hand, which, since Rudi was in constant convulsive tension, flew back.

5. Similar to the above, but Price first released the arm, and then set off the flash, quickly again seizing the hand.

6. The photographic picture is a cleverly contrived fake, the result of superimposing a hypothetical plate of a back and an outstretched sleeve of a model or dummy upon the double flash picture taken at seance No. 25, during which Rudi's hand was in fact controlled at all times.

7. The cameras at seance No. 25 photographed a materialised arm.

On none of these could Rudi in fact have produced the phenomena with his hand, neither at this nor the other seances.

The main objections to 1 and 2 are: the number of 'coincidences' (double flash coinciding precisely with freed arm; ceiling camera also malfunctioning; Price controlling; Price's 'toothache'; dismantling of apparatus next day; quiet appearance of back; direction and quiet appearance of feet; left-hand stereo apparently incompatible with control picture; difficulty as to how Price could have failed to notice free hand, especially after flash. In the case of alternative 2, there is the additional objection of the time factor: how could the medium inside half a second having his left hand released get it back past the right chairback and into the controller's hand?

Hypotheses 3, 4 and 5 at least avoid the concatenation of 'coincidences'. However, other objections remain.

On 3 the placid back remains an objection, since Rudi had to twist for the left arm to fly back the right chair back, and the direction and placidity of the feet is difficult to explain.

On 4 and 5 at least the direction of the back towards the camera is explained on the supposition that Price swivelled Rudi round before both flashes, thus showing the back placid and slightly bending forwards between flashes. However, the quiet feet and legs (on my reading) are at variance with the back, pointing at right or near right angles to the back.

True, the brown paper obscuring them originally could have been innocent, but why produce a picture which obscures the very important legs and feet at all? Price himself is shown during the picture sitting well back during both flashes. He could certainly have previously swivelled the medium round, but it is not easy to see how he could have done the guiding of Rudi's left arm past the right chair back that would have been necessary, especially on hypothesis 5, on which he had to release and recapture the arm in half a second. Hypothesis 5 is also vulnerable to the time factor considerations.

3, 4, and 5 then all fail to cope with the direction of the feet originally obscured by tape, and with the empty appearance of the sleeve (on my interpretation).

As regards 6, the fake photo hypothesis, the very crudity of some of the printing and re-touching might perhaps argue innocence rather than subtle faking. If Dr. Gauld's view is accepted that the back and feet might have appeared so placed, and that the feet are in accord with the back, then the staging hypotheses are somewhat easier to consider seriously. The complexities required by any 'conspiracy-type' theory lead one to incline against it, particularly in that several essential conditions for such a fraud would have had to have been, on the face of it, created some long time before Price quarrelled with Hope, Brown and Co. (the substitution of two flashbulbs for one on 25 February (98), the substitution of an armless chair on 10 March (99), the lowering of the lampshade on 7 April (100)). This latter objection is weakened by the consideration that, since Price did not circulate his accounts at the time but published them a year later, he could subsequently, once he had decided on a bogus 'exposure', have ante-dated these changes, precisely in order to disarm such suspicions.

There is an even harsher variant of 6, namely that Price all along intended to 'frame' Rudi. Anyone wishing to champion this interpretation would stress the questions: why did Price in the Third Series omit the electrical control by which he set such almost fanatical store? Why did he arrange such unfortunate advance publicity for Rudi when the latter arrived in England? Why did he keep Osty's warning about Mitzi secret from his co-investigators? Worse still, why did he have Mitzi

present at all sittings - outside the circle of sitters next to his secretary? Why indeed did he, on his own testimony, early on in the Third Series, make the various seance room arrangements (double bulb, chair etc.) without which his 'exposure' could not have been made? Why did he hang a bolster between the medium and the book case only as late as seance No. 25? Might not the rather shabby Duncan episode, the first half of which was hardly known in England, be thought of as a trial run?

Objections against 7 need perhaps not be rehearsed, even if Price's own argument when Rudi supposedly suggested it, namely that the 'spirit arm' wore a pyjama jacket is hardly a knock-down argument. For all we know of such matters, Rudi's whole body might have become paranormally plastic and elongated, in which case one could cease to worry about foot direction, pyjama stripes and speeds of movement. However, on such occasions when alleged materialisations were reported, e.g. of 'Olga's' hand (see for example the oblique reference by Fraser-Harris in his Times letter of 9 May 1932 [505]): it was always a dainty little 'hand' or a vague 'pseudopod' at the end of a somewhat amorphous nebulous elongated shape, and not any kind of Rudi 'double'.

It seems clear to me that it is simply not possible to decide firmly in favour of any of the hypotheses. The episode neatly (if that is quite the right epithet) illustrates a major problem encountered in investigating PK in special subjects; the great difficulty of reaching conclusions about specific controversial historical episodes.

So far as I am concerned, I would rule out hypotheses 1, 2, 3 and 5, as being wholly implausible. The fake photograph interpretation, 6, remains a strong contender in that it avoids virtually all the objections. True, there is no direct evidence for it, but then it is hard to see how there could be. If 6, the fake photo view, is rejected, then hypothesis 4 is a barely conceivable candidate, on the supposition that Price took a chance on Rudi's arm flying past the chair back, and it did; Price then packed up his photographic apparatus and kept the picture as a rod in pickle, a contingency measure should his enemies 'capture' Rudi: he may well have hoped he would never 'have to' use it, hence his

almost insane rage against Brown. Feet and bolster remain difficult to explain. Neither faking of the photograph, nor the faking of fraudulent exposure, would be out of character.

I very much doubt if we shall ever know for certain. On no interpretation can Price's conduct be vindicated. Even on the most charitable and immediately implausible interpretation that he was fully persuaded that the photograph constituted unambiguous proof of Rudi's guilt, he lied verbally and in writing to a large number of people in vouching for the medium's alleged discovery, and supported, even demanded, publication of this view; and if he indeed had what he deemed adequate proof of guilt, his co-investigators, several of whom had in fact been canvassed by him to finance the venture and who (so he claimed) were fully jointly responsible with himself for the investigation, were plainly entitled to know that he held such proof. Of one thing there can be no doubt: either Price put an accident to most nefarious use, or else he staged a bogus accident: there is no escape from this dilemma.

However, whatever interpretation be adopted, the 'freed arm' did not and could not explain the phenomena observed at that or for that matter, Price's other seances. Despite the huge amount of documentation, publication and publicity surrounding the incident, it has as little bearing on the authenticity of Rudi Schneider's phenomena as those of Meyer and Przibram, Vinton, Prince or Bird. It does however, highlight important problems for the would-be researcher into PK, which will be discussed below.

2.28 The negative Besterman-Gatty experiments

It is clear that, for whatever reasons, after the Hope-Rayleigh series, Rudi's phenomena dwindled considerably. Sittings with Osty in Paris in February and March 1933 were so unsuccessful that he offered to return the money contributed by the SPR and others (IMI). Inevitably, Price's denunciation was creating a good deal of stir behind the scenes. Dr. W.H. Prince, for example (see above 2.23) considered Price's 'exposure' to be ludicrous (101), and went so far as to write to that effect to Dr. Gerda Walther who had, at the time, defended Rudi against Prince himself (102). Walther on 14 May 1933 (103) responded in a most ironic vein, admonishing him

to dry his tears since the falling from grace of Price was more than compensated for by the fact that Rudi had enlisted the sympathy of Theodore Besterman.

Besterman now corresponded with Walther on the subject of Rudi fairly extensively. As regards Price, the items discussed merely served to demonstrate further Price's bad faith in the matter of the 'exposure'. It is however of considerable interest with respect to the status of 'Olga' especially as Besterman and Gatty were to attempt to explore this facet of Rudi's mediumship, and since the question of the psychic's state of mind and personality are clearly among the very important issues presenting themselves to the investigators into PK.

Rudi, so far as we know, only ever gave sittings in trance, and he then manifested as 'Olga' who, unlike Rudi, had at least occasional pretensions to clairvoyance. 'She' often knew, for example, where some object was to be found in the pitch dark or whether two sitters had let go of each other's hands. Yet when a first rate attack was being mounted on Rudi she was apparently as oblivious of this as anyone else. In a letter to Besterman of 10 July 1933 Walther wrote (104) that Kalifius had had one short sitting with Rudi, a so-called Sprechsitzung (talking seance). On 26 July 1933 she wrote again, 'Concerning the photo "Olga" said that Price had made a photo with a kind of doll with the arm stretched out behind. But I certainly don't think this explanation sounds very probable...'

It would appear that 'Olga' endorsed the fake-photograph view by means of a dummy (independently subsequently put forward by Brookes-Smith). If 'she' 'knew' at least that Price by that time meant Rudi and 'her' no good, why did she not protest at the time in April 1932? At the fateful sitting, if Price's record is to be believed, 'she' was affectionate and stroked Price's cheek to cure his neuralgia. Unfortunately we shall never know what 'Olga' may or may not have known before during or after the event.

Dr. Walther told me that it was never possible to rely on relevant information from 'Olga'. On one occasion Schrenck was anxious to discover whether a potential co-investigator had a criminal record, and 'Olga' was quite unable to help. Yet

'she' knew about S.O.'s tricks before anyone else. Osty also stresses (105) that 'Olga' was hardly better informed than anyone else about prospective success or otherwise even of seances.

Rudi married Mitzi on 27 July 1933. During the wedding Vater Schneider had a stroke from which he never recovered.

In September 1933 an SPR party including Hope and Besterman went to visit Rudi in his new home in Weyer, Austria, and observed phenomena, though under 'non-evidential' conditions, and Rudi was invited to London once more. However, in the course of the ensuing Besterman-Gatty investigation 'no phenomena clearly of a paranormal kind were obtained' (106). There may or may not have been some curtain movements due to PK, but the experimenters were either unable or unwilling to provide adequate conditions. New infra-red apparatus failed to provide results, and no paranormal variations in temperature were obtained. Desultory attempts were made to test whether 'Olga' could see in the dark, and abandoned as unpromising.

Besterman and Gatty concerned themselves with the question of Rudi's trance, and his extremely high rate of breathing came under scrutiny. Both Besterman and Gatty claimed to be able to breathe at the same fast rate as the medium. Dr. C.G. Douglas, a colleague of Gatty's, assisted them in collecting and analysing samples of the medium's breathing. His figures suggested that Rudi's breathing grew shallower as it grew faster. Unfortunately, they failed to discuss the report on Rudi's breathing by Osty, who measured both the frequency and also the amplitude of the muscular movements involved. According to Osty (107) whereas the frequency had increased by 10 to 20 times the normal rate, the amplitude of muscular movements sometimes less and sometimes greater than normal, was not reduced on average by more than a quarter. Now it could of course be the case that, though the extent of muscular movement was that claimed by Osty, the actual volume was that described by Besterman and Gatty. On the other hand, it also seems quite possible that on the single occasion when Rudi was taken to Douglas in Oxford, his breathing was different from that when giving a seance in Osty's laboratory. Again it is possible that Rudi's breathing had changed since Osty's day.

For all we know, Rudi's failure to put out enough of the necessary muscular effort was in some sense responsible for or related to the negative results of the Besterman and Gatty sittings.

Besterman and Gatty also collaborated with Whateley Carington (108) on word association tests, comparing Rudi's and 'Olga's' responses. Psycho-galvanic reflex responses were also tested. Despite the fact that a large proportion of the readings were spoiled, Carington concluded that there was significant similarity between trance and waking personality, arising both from psycho-galvanic and verbal responses. Carington emphasised that Rudi and 'Olga' reacted 'similarly' to certain special words inserted by Besterman in an attempt to see what their attitudes were to fraud. The stimulus words singled out were aufpassen (pay attention), Gesetz (law), streiten (fight, quarrel), falsch (false or wrong), Angst (fear), Geist (spirit), drohen (threaten), Schwindel (swindle, fibbing, giddiness, vertigo), Vorsicht (care, attention, caution), vertrauen (trust). Carington's conclusion was that this group of words alone accounted for about one third of the total similarity between the two personalities, although he states that the remainder of the words were also significantly similar as regards responses elicited. The inference he drew was that Rudi and 'Olga' could be said to have quite a 'complex' in common connected with fraud, suspicion and trust. This could hardly come as much of a surprise to anyone familiar with the recent Price double-exposure episode, of which Carington shows no awareness!

Moreover Carington, whilst admitting his imperfect acquaintance with the German language, suggested that there was something suspicious about the combination of short response time, irrelevance of association and apparent borrowing of association from near-by words. In effect, Carington's criteria for 'irrelevance' were conspicuous by their absence, and it is difficult to see how he could have inferred anything from 'short' response times. Besterman and Gatty in their Report at least give a list of words that occurred most frequently in 'Olga's' responses, and it is abundantly clear to anyone with the most rudimentary acquaintance with Rudi's history and with German that what they judged 'totally irrelevant' (and therefore

suspicious) may well not have been anything of the kind.

'Touch' and 'head', for instance, would seem quite a reasonable association for anyone who knew the often reported experience of sitters having their head touched; 'car' and 'end' could be meaningfully related to 'Olga's' (erroneous) insistence that cars would be the end of Rudi; 'rot' and 'Brot' rhyme; 'lamp' and 'red' were obviously related in both Rudi's and 'Olga's' experience of seance illumination; the same applies to the fact that 'needle' and 'bright' elicited 'stripe' - hardly surprising in view of the fact that bright luminous stripes and rows of pins were fastened to the medium's seance pyjamas as an anti-fraud precaution by Schrenck. The word 'Nadel' in German stands for both 'pin' and 'needle'.

Besterman and Gatty wrote that Carington's findings strongly suggested that Rudi was afraid that free responses might betray something he wanted to keep to himself, and that a clue to the nature of that something might be found in the stimulus response 'Schwindel - schön' which they translated as 'fraud-good'.

It is difficult to see how any weight whatever could have been attached to this association. There is to begin with the clear alliteration of the initial 'sch' (sh) - a very normal associative response. For another thing, 'fraud' and 'good' are hardly adequate translations. 'Schwindel' can just about mean fraud, but it is usually a much milder word, denoting anything from 'fib' or 'white lie' to 'swindle'. 'Schön' does not really mean 'good', but 'beautiful' or 'fine', and it is used in many contexts rather like 'nice' in English. What could one infer if an English speaking subject reacted to the word 'fib' by 'fine'? Or, perhaps more remotely, 'swindle' - 'sweet'. Not much, I submit. However, the linguistic issue is ever more blurred.

The proper German word for fraud is Betrug, which has the additional advantage of being unambiguous; for not only is the word 'Schwindel' usually a mild and permissive term for a childish trick or minor deception, the verb 'schwindeln' being the near-permissive term for (white) lying especially in children, as opposed to the harsher lügen; it is also ambiguous as it carries the meaning of 'vertigo'. (I tried

out a little word association test on a Subject whose native tongue was German, and her response to 'Schwindel' was 'which, fib or giddiness?') If Besterman and Gatty were correct in asserting that Rudi's and 'Olga's' reactions to mountains were pleasurable, why should not the response to 'vertigo' have been something non-committally pleasurable like 'schön' meaning 'nice'?

To give a sample of Besterman and Gatty's interpretation of 'similarity' it is instructive to examine how they treated 'Olga's' and Rudi's responses to 'mountain'. (They say that by an oversight they included both Berg (mountain) and Gebirge (mountain ranges). 'Olga's' reactions to Berg and Gebirge were: Himmel (sky or heaven), hell (light, bright), frei (free), laut (loud) besser (better), Höhe (height). Rudi's reactions were Garten (garden), Kopf (head), Himmel (sky or heaven), grausam (cruel), schön (beautiful, fine, nice), Glaube (faith, belief), Bild (picture), Baum (tree). Besterman and Gatty say that these responses are about equally similar and dissimilar to 'Olga's', whatever that may mean. How anyone could claim them to be systematically irrelevant, justifying suspicion of bad faith, is entirely beyond my comprehension. They particularly considered that the dual responses of Himmel were 'notable'. No translation into English of most of the German words is given in Besterman and Gatty's paper. Conceivably if the word Himmel could only mean 'heaven' as they may have imagined, it might have been a little bit 'notable'; but a conjunction of 'mountain' and 'sky' is hardly remarkable or significant and suspiciously farfetched.

It is on such flimsy evidence that Besterman and Gatty stated that it was with regret that they were obliged to draw attention to the 'fact' that there was 'nothing in the nature of genuine responses' in Rudi's replies, 'for it is impossible to praise too highly Rudi's straightforward behaviour throughout the ordinary sittings and his willingness to submit to every test' (109).

It seems entirely clear that nothing whatever published by Carington or Besterman or Gatty substantiates any supposition, let alone as 'fact', that Rudi's responses were not 'genuine'. In any case, Besterman and Gatty believed that Rudi was unconscious of 'Olga' and that the trance was genuine. However,

they considered it probable that 'Olga' was conscious of Rudi's thoughts and doings.

Fortunately we do not have to base this judgment on the psychological acumen of Besterman and Gatty: all we know about 'her' bears out her status as a typical 'co-conscious' personality which usually does know at least much of what is happening to the principal person who, on the face of it, is totally oblivious of the secondary person (110, 111).

Walther describes an entertaining and relevant incident in the course of which, during a tea party, 'Olga' manifested after Rudi had boasted that now he was his own master and she could not dispose of him as usual (112):

As he was saying this, he was about to take a bite of cake, but just before it touched his lips, he suddenly fell back and went into a trance, and his hand with the piece of cake laid itself on the table. 'Olga' announced her presence as usual in her rapid whisper: the medium had been altogether too impertinent - she would show him who was boss!

'She' only released Rudi after being begged by all present to do so, and to all appearances he woke up feeling sheepish and non-plussed.

The Besterman and Gatty investigation left a vague feeling of inconclusive resentment. Some desultory correspondence ensued. Hope in a Note (113) protested against the general impression created by Besterman and Gatty that they had disproved the infra-red experiments and the movement of objects, and Gatty, who seems to have been the moving spirit as far as disparagement of the medium was concerned, apologised if the Report had led to such misunderstandings. Besterman (114) retained some belief in the authenticity of at least some of Rudi's phenomena until the latter's death.

The entire episode shows how very amateurish, and biased may be the 'psychology' on which accusations of bad faith may be levelled in this area, and the ease with which careful work can be buried under a tissue of ill-founded innuendo.

It also highlights a problem which is far from solved: the status of consciousness and of personality in general, and in relation to PK in particular. On the supposition that 'Olga's' vocabulary was on the whole confined to that of Rudi, and was perhaps even more limited, what are we to infer from

this? The secondary personalities of mediums such as 'guides' generally claim that they have to make use of the medium's pool of words, just as they have to use his vocal organs for talking!

It is regrettable that, in the case of a medium as compliant as Rudi, far greater efforts were not made by researchers to establish contact with 'Olga' and get to know 'her' for the purpose of exploring 'her' psychological make-up and account of herself. This seems to have been entirely left to Kalifius in Braunau. To do him justice, Price at least touched on the issue of 'Olga's' status in his first book about Rudi (115) where he reprinted two letters from Major de Montmorency, both dated 21.11.29 [477 - 478]. Montmorency had private and personal information concerning Lola Montez, the supposed real person underlying 'Olga'. When 'Olga' was asked about this, she said she would 'tell the sitters next time' and 'she' would 'go to find out'. But 'she' never so far as we know did give any evidence of knowing anything at all about Lola.

Lola spoke English perfectly, 'Olga' knew no English at all, although the late Mrs. Mitzi Schneider (who did not know any English either) assured me that 'Olga' spoke perfect English.

The whole issue of 'ownership' of PK has remained an open question, Batchelder (116) having made a particular issue of it. Certainly the fact that in a classical case such as Rudi's PK is associated with dissociation of personality, a state normally thought of as pathological, and as in some sense related to self-deception however unconscious, creates additional problems for the investigator. If the only sign of mental abnormality is the manifestation of a secondary personality such as 'Olga', it would seem to amount to psychopathology. In the case of Rudi Schneider, there is no evidence whatever to suggest that in any other respect he was not entirely normal and well-adjusted.

2.3 Partial quantification of earlier records

The status of qualitative data in psychical research is one of the major problems facing the would-be investigator since quantification is at least one of the ultimate aims of

scientific investigation. It may be argued that much of psychical research (like, for that matter, much of psychopathology) does not lend itself to such quantification, and that too much is lost in the translation from word to number for this always to be an appropriate mode of assessment. On the other hand, it seems to me that where there is any relatively constant and oft-repeated situation, as well as characteristics capable of being abstracted, it is worth while attempting some quantitative analysis with the aims of systematic representation of findings or claims, isolation and comparison of specific characteristics, facilitating the interpretation of results in a systematic manner, and for suggesting improved classifications and working hypotheses for further reference. It also seemed worth making the attempt of subjecting the data to quantitative analysis or at least representation in view of the high quality of the testimony in favour of Rudi's genuineness and the lamentable quality of the accusations levelled against him.

It seemed to me that Vater Schneider's Journals were suitable for just such an attempt, since they constitute a fairly stereotyped record containing descriptions of similar events over a considerable number of incidents, complete with names of sitters. It is possible, from time to time, to compare the Schneider Journals record with what is described by other participants, notably Schrenck-Notzing, occasionally more hostile investigators such as Bird or Prince, as well as others, and there is reasonably good coincidence between the Journals, and these and other sources.

The Schneider Journals are two shabby dog-eared books whose binding is fragile and breaking up, and which must now be handled with great care. The writing is beginning to fade in places, but Vater Schneider's beautiful handwriting provides no problem, provided that one is able to read and translate his now archaic script, Sütterlinschrift. Some of the seance records are written by Franz Ramspacher, whose handwriting is also clear. Comments by participants are frequently less legible, but they too can, with a little perseverance, eventually be deciphered.

Questions that presented themselves were: were there differences between the seances in the Schneider home as

contrasted with sittings elsewhere? Was there evidence to suggest that some sitters were 'psi-conducive' or the reverse? What different types of phenomena can be isolated? What was the distribution over a period of time of the different types of phenomena? Does an over-all phenomenological profile emerge? In the event of another psychic becoming available, are working hypotheses and recommendations for recording of data suggested by the analysis?

The Journals contain altogether reports of 269 sittings with Rudi between 8 December 1923 and 1 January 1932. Each report is headed by the date, followed by the place where the sitting in question took place. Then follow the signatures of the sitters, after which there is an account of what took place (or seemed to take place, or was reported to have taken place). Often after this there are further entries, containing some testimonial to the effect that the control of the medium was satisfactory, or that the sitter who adds his or her name was persuaded of the genuineness of the phenomena.

Tabulating such data has, of course, no direct bearing on the genuineness or otherwise of the events reported. All that one can count is how many times and when and what type and combination of events were reported to have taken place where and in whose presence. Having done this one can examine the various interpretations that suggest themselves. The tables have naturally been compiled with certain questions in mind, and occasionally percentages have been calculated so as to render sets of sittings, containing different numbers of sittings, comparable with regard to certain characteristics. Thus, if one wants to compare how many sittings were negative at Schrenck-Notzing's laboratory, one does not want to compare the absolute number of the two, but rather the percentage of sittings that were blank under the two sets of circumstances in question.

Table 3 provides a comprehensive list of all the sittings reported in the Schneider Journals. The serial numbers of sittings are mine. One of the first questions one is inclined to ask is: how many of these sittings were held in the Schneider home in Braunau, and how many took place elsewhere? The serial numbers of those sittings which took

Table 3. Sitzings in Schneider Journal.

1	2	3	4	5	6	7
Serial No. (A.G.'s) Schneider Journal Underlined in Schneider home	Date	Schrenck's Serial No.	Reported movement of objects	Visible materialisations	Levitations of medium	Experience of being touched
1	8.12.23		X	O	X	O
2	9.12.23		X	X	X	O
3	10. 1.24		O	O	X	O
4	12. 1.24		O	X	O	X
5	17. 1.24		X	O	O	O
6	21. 1.24		O	X	O	X
7	26. 1.24		O	O	X	O
8	27. 1.24		O	O	X	O
9	31. 1.24		X	X	X	O
10	3. 2.24		X	O	O	O
11	6. 2.24		X	O	O	O
12	10. 2.24		X	O	O	O
13	11. 2.24		X	O	X	O
14	17. 2.24		O	O	O	O
15	18. 2.24		X	O	X	O
16	29. 2.24		O	O	O	O
17	1. 3.24		O	O	O	O
18	5. 3.24		X	O	X	O
19	10. 3.24		X	X	X	O
20	13. 3.24		X	O	X	O
21	17. 3.24		X	O	X	O
22	22. 3.24		X	O	X	O
23	23. 3.24		O	O	O	O
24	27. 3.24		X	O	X	O
25	31. 3.24		X	X	X	O
26	7. 4.24		O	O	X	O
27	8. 4.24		X	O	O	O
28	10. 4.24		X	O	O	O
29	21. 4.24		X	O	X	O
30	23. 4.24		X	X	O	O
31	24. 4.24		X	X	O	O
32	26. 4.24		X	X	X	O
33	2. 5.24		O	O	O	O
34	5. 5.24		X	O	O	O
35	8. 5.24	[1]	X	X	X	X

1	2	3	4	5	6	7
<u>36</u>	12. 5.24		X	0	0	0
<u>37</u>	14. 5.24		X	X	0	X
<u>38</u>	16. 5.24		0	0	0	0
<u>39</u>	17. 5.24		X	0	X	0
<u>40</u>	18. 5.24		X	0	X	0
<u>41</u>	20. 5.24		X	0	X	0
<u>42</u>	23. 5.24		X	0	0	0
<u>43</u>	26. 5.24		X	0	X	0
<u>44</u>	29. 5.24	[2]	X	X	X	0
<u>45</u>	31. 5.24		X	X	X	X
<u>46</u>	5. 6.24	[3]	X	X	X	X
<u>47</u>	8. 6.24		X	0	X	0
<u>48</u>	12. 6.24		X	X	X	0
<u>49</u>	19. 6.24		X	0	0	X
<u>50</u>	21. 6.24	[4]	X	X	X	X
<u>51</u>	23. 6.24	[5]	X	X	X	X
<u>52</u>	25. 6.24		X	X	X	X
<u>53</u>	28. 6.24		X	X	X	X
<u>54</u>	4. 7.24		X	X	X	X
<u>55</u>	8. 7.24		X	X	0	X
<u>56</u>	10. 7.24		X	0	X	0
<u>57</u>	13. 7.24		0	0	0	0
<u>58</u>	15. 7.24		X	X	0	X
<u>59</u>	21. 7.24		X	0	X	X
<u>60</u>	24. 7.24		X	X	0	X
<u>61</u>	28. 7.24		X	X	X	X
<u>62</u>	27. 7.24		X	X	0	X
<u>63</u>	29. 7.24		X	X	0	0
<u>64</u>	2. 8.24		0	0	0	0
<u>65</u>	4. 8.24		X	0	0	0
<u>66</u>	7. 8.24		X	0	X	0
<u>67</u>	9. 8.24		X	0	X	0
<u>68</u>	10. 8.24		X	0	0	X
<u>69</u>	11. 8.24		X	0	0	0
<u>70</u>	16. 8.24		X	0	X	0
<u>71</u>	18. 8.24		X	0	X	X
<u>72</u>	19. 8.24		X	X	0	X
<u>73</u>	26. 8.24		0	0	0	0
<u>74</u>	1. 9.24		X	X	0	X
<u>75</u>	4. 9.24		X	X	0	X
<u>76</u>	6. 9.24	[6]	X	X	0	0
<u>77</u>	8. 9.24		X	0	0	0
<u>78</u>	15. 9.24		X	X	X	X
<u>79</u>	18. 9.24		X	X	0	X
<u>80</u>	20. 9.24	[7]	0	0	0	0
<u>81</u>	21. 9.24	[8]	X	X	0	X
<u>82</u>	23. 9.24		X	0	0	X
<u>83</u>	27. 9.24		X	0	0	X
<u>84</u>	29. 9.24		X	0	X	0
<u>85</u>	2.10.24		X	0	0	0
<u>86</u>	5.10.24	[9]	X	0	0	0
<u>87</u>	9.10.24		X	X	0	X
<u>88</u>	11.10.24		X	0	0	X
<u>89</u>	15.10.24		X	X	0	0
<u>90</u>	18.10.24	[10]	0	0	0	0
<u>91</u>	19.10.24	[11]	X	X	0	0
<u>92</u>	23.10.24		X	X	0	X

1	2	3	4	5	6	7
<u>93</u>	25.10.24		X	0	X	0
<u>94</u>	28.10.24		0	0	0	0
<u>95</u>	1.11.24	[12]	X	X	0	0
<u>96</u>	2.11.24		X	X	0	0
<u>97</u>	8.11.24		X	0	0	0
<u>98</u>	10.11.24		X	X	0	X
<u>99</u>	12.11.24		X	0	0	0
<u>100</u>	13.11.24		X	0	0	0
<u>101</u>	19.11.24		X	X	X	0
<u>102</u>	22.11.24	[13]	X	0	0	0
<u>103</u>	26.11.24		X	X	0	X
<u>104</u>	29.11.24	[14]	X	0	0	0
<u>105</u>	4.12.24		X	0	0	0
<u>106</u>	7.12.24	[15]	X	X	X	0
<u>107</u>	10.12.24		X	0	0	0
<u>108</u>	12.12.24		X	0	X	0
<u>109</u>	12.12.24		X	0	X	0
<u>110</u>	19.12.24		X	X	0	0
<u>111</u>	22.12.24		X	X	X	0
<u>112</u>	27.12.24	[16]	0	0	0	0
<u>113</u>	1. 1.25		X	0	0	0
<u>114</u>	5. 1.25	[17]	X	0	0	0
<u>115</u>	14. 1.25		X	X	X	X
<u>116</u>	17. 1.25	[18]	X	0	0	0
<u>117</u>	21. 1.25		X	0	0	0
<u>118</u>	24. 1.25	[19]	X	X	0	0
<u>119</u>	31. 1.25		X	0	0	0
<u>120</u>	5. 2.25		X	0	0	0
<u>121</u>	14. 2.25	[20]	X	0	0	0
<u>122</u>	18. 2.25		X	X	0	0
<u>123</u>	25. 2.25		-	-	-	-
<u>124</u>	14. 3.25		X	0	0	0
<u>125</u>	30. 3.25		0	0	X	0
<u>126</u>	3. 4.25		X	0	0	0
<u>127</u>	4. 4.25		X	0	0	0
<u>128</u>	5. 4.25		X	0	0	0
<u>129</u>	18. 4.25	[21]	0	0	0	0
<u>130</u>	25. 4.25		X	X	0	0
<u>131</u>	30. 4.25		X	0	0	0
<u>132</u>	6. 5.25		X	0	0	0
<u>133</u>	7. 5.25		X	X	X	X
<u>134</u>	9. 5.25		X	0	X	X
<u>135</u>	16. 5.25		X	0	0	X
<u>136</u>	20. 5.25		X	0	0	X
<u>137</u>	28. 5.25		X	X	0	0
<u>138</u>	31. 5.25	[26]	X	0	0	0
<u>139</u>	1. 6.25	[27]	X	X	0	0
<u>140</u>	10. 6.25	[28]	0	0	0	0
<u>141</u>	10. 6.25		X	0	X	X
<u>142</u>	21. 6.25	[31]	X	X	0	0
<u>143</u>	4. 7.25		0	0	0	0
<u>144</u>	8. 8.25		X	X	0	0
<u>145</u>	18. 8.25		X	0	0	0
<u>146</u>	20. 8.25		X	0	0	0
<u>147</u>	28. 8.25		X	X	0	X
<u>148</u>	1. 9.25		X	X	X	0

1	2	3	4	5	6	7
<u>149</u>	9. 9.25		X	X	0	X
<u>150</u>	15. 9.25		X	X	0	0
<u>151</u>	17. 9.25		X	X	0	X
<u>152</u>	21. 9.25		X	X	0	0
<u>153</u>	25. 9.25		X	X	0	X
<u>154</u>	30. 9.25		X	X	0	X
<u>155</u>	5.10.25		X	0	0	0
<u>156</u>	9.10.25		X	X	0	0
<u>157</u>	14.10.25		-	-	-	-
<u>158</u>	23.11.25		0	0	0	0
<u>159</u>	25.11.25		X	0	0	0
<u>160</u>	1.12.25		X	X	0	X
<u>161</u>	5.12.25		X	0	0	0
<u>162</u>	9.12.25		X	X	0	0
<u>163</u>	12.12.25	[35]	X	X	0	X
<u>164</u>	16.12.25		X	X	X	0
<u>165</u>	19.12.25		X	X	0	0
<u>166</u>	28.12.25		X	X	0	0
<u>167</u>	2. 1.26	[36]	0	0	0	0
<u>168</u>	24. 1.26		X	X	0	0
<u>169</u>	15. 2.26		X	0	0	0
<u>170</u>	20. 2.26		X	0	0	0
<u>171</u>	21. 2.26		X	X	0	0
<u>172</u>	23. 2.26		X	X	0	0
<u>173</u>	24. 2.26		0	0	0	0
<u>174</u>	26. 2.26		X	X	0	0
<u>175</u>	5. 3.26		X	X	0	0
<u>176</u>	13. 3.26	[38]	0	0	0	0
<u>177</u>	19. 3.26		X	X	0	0
<u>178</u>	9. 4.26		X	X	0	0
<u>179</u>	12. 4.26		X	X	X	X
<u>180</u>	14. 4.26		X	X	0	0
<u>181</u>	17. 4.26		X	X	0	0
<u>182</u>	24. 4.26		X	0	0	0
<u>183</u>	28. 4.26		X	X	0	0
<u>184</u>	29. 4.26		X	X	0	0
<u>185</u>	7. 5.26		X	X	0	0
<u>186</u>	10. 5.26		X	X	0	0
<u>187</u>	26. 5.26		X	0	X	0
<u>188</u>	31. 5.26		0	0	0	0
<u>189</u>	16. 6.26		X	0	X	0
<u>190</u>	17. 6.26		X	X	0	0
<u>191</u>	2. 7.26		X	X	0	0
<u>192</u>	10. 7.26		X	0	0	0
<u>193</u>	20. 7.26		X	0	0	0
<u>194</u>	26. 7.26		X	X	0	0
<u>195</u>	28. 7.26		X	X	0	0
<u>196</u>	30. 7.26		X	X	0	0
<u>197</u>	2. 8.26		X	X	0	0
<u>198</u>	9. 8.26		X	0	0	0
<u>199</u>	12. 8.26		X	0	0	0
<u>200</u>	16. 8.26		X	0	0	0
<u>201</u>	19. 8.26		0	0	0	0
<u>202</u>	30. 8.26		-	-	-	-
<u>203</u>	11. 9.26		X	0	0	0
<u>204</u>	15. 9.26		-	-	-	-

1	2	3	4	5	6	7
<u>205</u>	18. 9.26	[41]	X	X	0	0
<u>206</u>	21. 9.26		X	0	X	0
<u>207</u>	25. 9.26		X	0	0	X
<u>208</u>	29. 9.26		X	0	0	0
<u>209</u>	16.10.26		X	X	0	0
<u>210</u>	23.10.26		X	X	0	0
<u>211</u>	31.10.26		X	0	0	0
<u>212</u>	2.11.26		X	0	0	0
<u>213</u>	3.11.26		X	X	0	0
<u>214</u>	4.11.26		X	0	0	0
<u>215</u>	6.11.26		X	0	0	0
<u>216</u>	7.11.26		-	-	-	-
<u>217</u>	22.11.26		X	X	0	0
<u>218</u>	29.11.26		X	0	0	0
<u>219</u>	6.12.26		X	X	0	0
<u>220</u>	15.12.26		X	X	0	0
<u>221</u>	4. 1.27		X	X	0	0
<u>222</u>	12. 1.27		X	X	0	X
<u>223</u>	15. 1.27		X	X	0	0
<u>224</u>	22. 1.27		X	X	0	0
<u>225</u>	2. 2.27		X	0	0	0
<u>226</u>	9. 2.27		X	X	0	0
<u>227</u>	12. 2.27		X	X	0	0
<u>228</u>	16. 2.27		X	X	0	0
<u>229</u>	5. 3.27		X	0	0	0
<u>230</u>	26. 3.27		X	0	0	0
<u>231</u>	2. 4.27		X	0	0	0
<u>232</u>	9. 4.27		X	0	0	0
<u>233</u>	20. 4.27		X	X	0	0
<u>234</u>	27. 4.27		X	X	0	X
<u>235</u>	17. 5.27		X	X	0	X
<u>236</u>	18. 6.27		X	X	0	0
<u>237</u>	25. 6.27		X	X	0	0
<u>238</u>	9. 7.27		X	0	0	0
<u>239</u>	25. 7.27		X	X	0	0
<u>240</u>	12. 8.27		0	0	0	0
<u>241</u>	no date		X	0	0	0
<u>242</u>	no date		0	0	0	0
<u>243</u>	no date		-	-	-	-
<u>244</u>	no date		X	0	0	0
<u>245</u>	no date		0	0	0	0
<u>246</u>	no date		0	0	0	0
<u>247</u>	no date		X	0	0	0
<u>248</u>	no date		X	0	0	0
<u>249</u>	no date		0	0	0	0
<u>250</u>	? date		X	0	0	0
<u>251</u>	? date		X	0	0	0
<u>252</u>	14. 9.27		0	0	0	0
<u>253</u>	15. 9.27		X	0	0	0
<u>254</u>	17. 9.27		X	0	0	0
<u>255</u>	28. 9.27		X	0	0	0
<u>256</u>	1.10.27		X	0	0	0
<u>257</u>	2.10.27		-	-	-	-
<u>258</u>	3.10.27		X	0	0	0
<u>259</u>	6.10.27		X	0	0	0
<u>260</u>	9.10.27		0	0	0	0
<u>261</u>	11.10.27		X	X	0	0
<u>262</u>	16.10.27		X	0	0	0

1	2	3	4	5	6	7
<u>263</u>	31.10.27		X	0	0	0
<u>264</u>	2.11.27		X	X	0	X
<u>265</u>	26.12.27		X	X	0	0
<u>266</u>	29. 4.28		X	0	0	X
<u>267</u>	21. 5.28		-	-	-	-
<u>268</u>	21. 1.29		X	X	0	X
<u>269</u>	1. 1.32		X	.0	0	X

Table 3.

place at the Schneider flat in Braunau are underlined. Occasionally no location is indicated, in which case I have assumed that the sitting in question took place at home. The other seances were in a number of different places, some in Schrenck's laboratory, others in Vienna, Prague, Zürich, some in the house of friends of the family, others in that of individual scientists who had taken an interest in the matter.

Column 2 in Table 3 gives the date on which the sitting in question was held.

Column 3, that is figures in square brackets, indicates sittings at which Dr. v. Schrenck-Notzing was present. Figures in square brackets are his serial numbers as published in his posthumous book Die Phänomene des Mediums Rudi Schneider in 1933, which lists all his sittings (117). Fuller transcripts of these reports are preserved in the archives of the Freiburg Institute für Grenzgebiete der Psychologie und Psychohygiene, put at my disposal, but for reasons of economy extensive details of only some of them were published in Schrenck's book. Not all the Schrenck sittings are listed in the Schneider Journals, since not all were written up by Vater Schneider, and from Schrenck's sitting No. [37] onwards, on 6 February 1926, Rudi's relatives hardly ever attended at Schrenck's laboratory in Munich.

In compiling the tables I was naturally faced with the problem of how to classify the different seances. I rejected after a good deal of hesitation any idea of categorising sittings according to whether the reporter, whoever he was, considered it was a 'brilliant', 'good', 'average', 'poor' or 'negative' sitting. Whereas in many cases such an ordering would have been quite possible, in an even larger number it would have been arbitrary guesswork since relevant information was just not available. Although in the Schneider Journals the phenomena observed are usually listed, they are not counted.

After having transcribed a resumé of every report, I looked for regularities which might suggest some sort of classification, and finally categorisation in terms of four alleged occurrences suggested itself: 1. movements of physical objects; 2. visible materialisations; 3. levitations

of the body of the medium; 4. sitters reporting that they felt themselves touched by an invisible agency. I then decided to use the symbol X for the reported occurrence of such phenomena at a given seance, and the symbol 0 for the non-occurrence of such phenomena, the order of my noughts and crosses indicating to which of the above four types of phenomena they referred. This is reflected in columns 4, 5, 6 and 7 of Table 3. Column 4 indicates movements of objects, column 5 visible materialisation, column 6 levitation of the body of the medium, and column 7 the experience of being touched.

Thus if a sitting is labelled X X X X this means that during this seance objects were reported to move; materialisations were seen; levitations of the medium were claimed; and sitters felt themselves touched. 0 0 0 0 refers to totally negative sittings. - - - - means that no information at all is given as to what happened during the sitting. X 0 0 0 means that the only type of phenomenon observed was the movement of objects ostensibly by no human agency; X X 0 X refers to seances at which objects moved, materialisations were supposed to be seen and sitters felt themselves touched, but no levitations of Rudi's body occurred and so on.

Such categorisation is by no means wholly satisfactory. For example, a phenomenon that was reported with great frequency, the knotting of a handkerchief by an invisible agency, had to be classified under the first category (column 4), namely movement of objects, and would therefore come under the same heading as the floating of a violin, the playing of a broken down musical box, the throwing about of a handbell, the upsetting of a table or a bowl of water, or the breaking of a window pane. Often, quite a number of these phenomena would occur during a seance and one might be faced with a sitting at which all this remarkable activity was witnessed and call it X 0 0 0; at another seance nothing but curtain movements believed to be of 'paranormal' origin would be observed, and this too would be called X 0 0 0. On the other hand, in the early years of Rudi's mediumship, such sittings at which curtain movements only were observed were rare, and would tend to be described as negative sittings by their reporters.

Despite these difficulties this method seemed a better way of proceeding than to attempt a subtler sub-division

which might perhaps suggest a precision that is just not to be found in the material, and over-complicate a picture that can, after all, be only an impressionistic sketch however carefully one proceeds.

'Visible materialisations', my second category (column 5), range from vaguely defined grey mists and stumps to full figures; but the most frequently claimed observation was that of a hand or hands, usually 'small' and 'white', partial or whole. If a sitter felt his own hand clasped by an invisible hand or his beard pulled by an invisible agent, I have classed this event under the 4th category (column 7), i.e. experience of touch; if the hand felt by a sitter was visible, I have classed it under the 2nd (materialisation, column 5) as well as the 4th (touch, column 7). If a sitter's shoe was removed by mysterious means or a cigarette 'appeared' in his mouth, I have classed this under the 1st heading, movement of objects, but if in addition it is specially mentioned that he felt the touch of a hand, I have added an X in the 4th category (column 7).

From the point of view of categorisation the third (column 6), levitation of the medium's body, is the easiest. This either happened, or was reported to have happened, or it did not; and so an X in the third place (column 6) is easy to interpret although this does not show whether there were several alleged levitations during one seance, nor whether they lasted long and were supposed to be high.

The fourth category (column 7), the subjective experience of being touched, is also fairly straight forward. These touches might be gentle or violent, frequent or isolated, but if they were reported at all during a seance, there is an X in the 4th place.

The tables show what types of phenomena were reported at each of the seances in the Schneider Journals and the Schrenck reports. By following the tables one can trace what type of combination of phenomena occurred at different times, under different circumstances, in different places, and in the absence or presence of certain persons.

As I have already indicated, the Journals do not contain all the sittings given by Rudi that occurred between 8 December

1923 and the 1st January 1932, and towards the end of the period they tend to become more and more sketchy. As will be recalled (see Table 1), Rudi spent many months abroad from 1929 onwards so that there are long blank periods in the Journals as regards dates. Also, as Rudi increasingly passed into the sole charge of psychical researchers, Vater Schneider may have attached less and less importance to an account in his own books of every seance. For example, we know that Mrs. K.M. Goldney, Dr. Gerda Walther, Miss Ethel Beenham and Mr. Harry Price jointly held three sittings in Braunau on July 28th, 29th and 31st, 1931, after which several of the participants took exhaustive notes (118); but there are no corresponding entries in the Schneider Journals.

An obvious question one might wish to ask is: roughly what proportion of the different types of sittings are described in the Schneider Journals? And then: did this proportion change over the time covered by the Journals? Table 4 summarises relevant data: in the penultimate column, under Total (Column 11), one can see at a glance how many of each type of sittings were recorded. The last column, 12, shows the number of sittings of each type expressed as a percentage: in other words, approximately 10.4% of all sittings were negative; at 5.2% of all sittings were all four types of phenomena reported; at 1.5% were there movements of objects, levitations of the medium and touches, but no visible materialisations, and so on.

In the intervening columns the number of the types of sittings are given in successive groups of 50, column 10 summarising the type of 19 sittings.

From this it emerges that the proportion of totally negative sittings did not rise over the period covered by the Schneider Journals. However, the number of sittings at which all types of phenomena were reported, as symbolised by X X X X, grew perceptibly less.

Sittings at which visible materialisations and movements of objects were experienced, X X O O, constituted 22.3% of sittings. This type of sitting seems to have been most frequent in the period between September 1925 and September 1927.

The proportion of sittings at which the only phenomena reported were of movement of objects, X O O O sittings, was

Table 4

Frequency of different types of sitting, Schneider Journal.

X and O denote presence and absence of phenomena (- - - - signifies seance recorded but no phenomena reported).

Under type of sitting (only combinations reported):

1st column: movement of objects

2nd column: visible materialisations

3rd column: levitation of medium

4th column: sitters report experience of being touched

1 2 3 4	5	6	7	8	9	10	11	12
Type of sitting	Nos. 1-50	Nos. 51-100	Nos. 101-150	Nos. 151-200	Nos. 201-250	Nos. 251-269	Total	%
O O O O	6	6	4	5	5	2	28	10.4
X X X X	5	6	2	1	0	0	14	5.2
X O X X	0	2	2	0	0	0	4	1.5
X X O X	1	12	3	5	3	2	26	9.7
X X X O	6	0	4	1	0	0	11	4.1
O O X O	4	0	1	0	0	0	5	1.9
X O X O	14	6	2	2	1	0	25	9.3
X O O X	1	4	2	0	1	2	10	3.7
X X O O	2	6	9	24	17	2	60	22.3
X O O O	9	8	20	11	19	9	76	28.2
O X O X	2	0	0	0	0	0	2	0.8
- - - -	0	0	1	1	4	2	8	2.9

28.2%, the most frequent of all types of sittings, more than a quarter. It is most interesting that this type shows a tendency towards proportionate increase over the period of time covered, and there is a proportionate decrease of the more varied type of sittings, such as those at which paranormal movement, paranormal appearances and paranormal touches (X X O X) was described.

It is very striking that whereas movements of objects only are reported for 28.2% of sittings, there is not a single sitting at which it is claimed that there were materialisations only, in fact there are hardly any positive sittings at which movement of objects did not occur. The only exceptions are the O O X O column and the O X O X column. The occurrence of these combinations of types of phenomena was obviously very rare, in fact I strongly suspect that they may have been non-existent. It may well be that the O O X O sittings, only six in all, each in the early days, refer to sittings at which no attention was paid to anything except the levitation of the medium, as this was regarded as 'the royal phenomenon of mediumship'. As regards the O X O X types of sittings, these were very early, my serial numbers 4 and 6. Both were inadequately written up and there may well have been movements of objects that were neglected because other events seemed more interesting, namely a rather heated debate that ensued in the course of the earlier sitting No. 4, and full figures that repeatedly showed themselves during sitting No. 6. A special note was also made in the case of this particular sitting that no curtain was used on that occasion.

The basic phenomenon so to speak would seem to be the movement of an object, and this phenomenon persisted longest and frequently occurred without any of the others. It seems certainly surprising and, so far as I was concerned emerged only from the figures that, whereas paranormal movement of objects would occur coupled with any other type of phenomenon, other types of phenomena would scarcely ever appear without movement of objects. It might be suggested that this is the easiest type of phenomenon to produce fraudulently, but that is a somewhat dubious assertion. It would seem at least as easy, if not easier, to counterfeit the touch of a hand.

If a charade had been played at the Schneiders, why should there never have been, say, hands and figures only, but no movements? And why was the phenomenon of touch not reported more frequently? If one wishes to make out a case for fraud on the strength of these figures, one would have to suppose that an accomplice was normally hidden in the cabinet and that consequently there would always be at least certain movement provided anything happened at all; that this would precede the manipulation of some sort of visible appearance, thus giving an X X O O seance; but that whether the accomplice ventured out of the cabinet and touched any of the sitters, i.e. an X X O X sitting, would depend on circumstances and might be more difficult, hence the proportion of only 9.7%.

One could thus interpret the figures and I have no doubt that a great many people will so interpret them. They would, however, have to meet the difficulty that the movements reported were very largely movements of the little bell, swinging of the violin, the floating about of 'August' (a toy puppet), the distribution of cigarettes, the removal of people's shoes, the knotting of a handkerchief - in other words, acts which on the face of it require the agency of a body outside the cabinet, and whether such activity could quite so persistently have escaped detection by such large numbers of people, many of them highly critical, must seem problematic.

Next we might well ask: which types of phenomena tended to occur more at home, and which tended to happen more when Rudi was outside the family circle? Table 5 has been compiled to give some of the data that will go some way towards answering this question. Columns 17 and 18 show the number of types of sittings, divided up into 'home' and 'away', whilst columns 19 and 20 express this information in the form of percentages. Thus, for example, 6.4% of home sittings were negative against 18.7 away; but the percentage of sittings at which all types of phenomena occurred is much the same, 5.3% and 5.1%. Whereas of the X X O O sittings (that is seances at which there were both paranormal movements and visible materialisations) 28.3% took place at home and 11.4% away, of X O O O sittings (movements only) there were actually

Table 5

Comparison of frequency of different types of sitting in Schneider flat in Braunau ('home') and elsewhere ('away'), from Schneider Journals. Numbers 1-50 etc. below column numbers 5 to 16 refer to serial nos. of sittings, types of sitting as before.

1 2 3 4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Type of sitting	1 - 50		51 - 100		101 - 150		151 - 200		201 - 250		251 - 269		total home	total away	% home	% away
	home	away	home	away	home	away	home	away	home	away	home	away	home	away	home	away
0 0 0 0	2	4	4	2	2	2	2	3	0	5	1	1	11	17	6.4	18.7
X X X X	3	2	4	2	1	1	1	0	0	0	0	0	9	5	5.3	5.1
X 0 X X	0	0	2	0	1	1	0	0	0	0	0	0	3	1	1.7	1.0
X X 0 X	1	0	10	2	3	0	4	1	8	0	2	0	23	3	13.3	3.0
X X X 0	4	2	0	0	3	1	1	0	0	0	0	0	8	3	4.6	3.0
0 0 X 0	1	3	0	0	1	0	0	0	0	0	0	0	2	3	1.2	3.0
X 0 X 0	8	6	4	2	1	1	2	0	1	0	0	0	16	9	9.2	9.3
X 0 0 X	1	0	2	2	2	0	0	0	0	1	2	0	7	3	4.0	3.0
X X 0 0	2	0	3	3	6	3	21	3	15	2	2	0	49	11	28.3	11.4
X 0 0 0	4	5	3	5	8	12	9	2	9	10	6	3	39	37	22.5	38.5
0 X 0 X	1	1	0	0	0	0	0	0	0	0	0	0	1	1	0.6	1.0
- - - -	0	0	0	0	1	0	1	0	2	2	1	1	<u>5</u>	<u>3</u>	2.9	3.0
													173	96		

more 'away' than 'home': 38.5% away against 22.5% home. As will be seen, there is not much difference as regards the others except the X X O X type of sitting, i.e. paranormal movement, materialisation and touches where 13.3% were at home and 3% away.

It will be seen at once that whereas there was a tendency for sittings to be more varied, in terms of our categories, during 'home' sittings, and there were certainly more blanks 'away', nevertheless not a single type of combination occurred at home that did not also occur away. Thus there was no indispensable ingredient in the Schneider home setting, whether fraudulent or paranormal, that was essential for phenomena to occur.

One can of course again interpret the tables in accordance with whatever hypotheses one is willing to entertain; if one glances through them in order to find evidence of fraud, one will emphasise the relative frequency of phenomena at home. One will then have to meet the difficulty that, even if there was greater (though not much greater) frequency of certain types of sittings at home than away, still: all the types of phenomena did occur at sittings away from home. And in particular one would have to meet the difficulty that there were actually more sittings with movement only away than there were at home.

On any hypothesis this suggests that the relatively more meagre sittings were easier to accomplish away from home, or were all that it was possible on many occasions to accomplish away from home; that somehow or another the alien setting was not as good for the further stage of materialisation (or fraud) represented by visible appearances.

If we now examine columns 5 to 18, we can see that there were few striking discrepancies between what happened at home and away over any given period. There was a rather higher incidence of negative sittings during sittings 201 to 250 away than at home, there were a striking number of X X O X sittings at home as compared to away during sittings Nos. 51 to 100, and there were very many more X X O O sittings at home than away in the course of sittings 151 and 251. It is clear that there was a gradual concentration on X X O O and X O O O sittings as time goes on.

There is nothing in the figures to suggest that certain

phenomena never happened away from the Schneider home in Braunau, and indeed reading the accounts it is clear that qualitatively they appeared much the same to spectators.

Table 6 shows the sittings recorded by Schrenck-Notzing, but leaving out those that took place in the Schneider flat in Braunau. The total number of these is 84. There are two sittings numbered [20], one is [20], the other [20a]. This second one took place in Schrenck's absence in the flat of a physician, Dr. Kurt Lange, Thierschstr. 41, Munich. In charge was Professor Gruber and Schrenck's secretary, Miss Maria Baader, acted as note taker.

Table 7 represents an attempt to compare what happened at seances under Schrenck-Notzing's control (excepting those which took place in the Schneider home in Braunau) with sittings that took place without him, both in the Schneider home and away. Those columns which refer to Schneider Journals away sittings, 8 & 9, are sub-divided into those at which Schrenck was not present, column 8; and all Schneider away sittings, column 9, which of course include some at which Schrenck was present.

It is immediately clear that the largest single percentage of totally negative sittings took place in Schrenck's laboratory. This proportion, 25%, is of course swelled by the long stretch of time, from September 1928 onwards, when 'Olga' was busy 'getting the better of' Schrenck because he among other things refused to play 'her' kind of music. If we compare this with column 8, Schneider away sittings without Schrenck, we see that this is 14.1%, certainly higher than the 6.4% at home sittings.

It is interesting to examine which types of combination never occurred under Schrenck's direction when he had Rudi in his own laboratory. These are X O X X , O O X O , X O X O , X O O X , O X O X and - - - - .

The combinations then which never occurred at Schrenck's were paranormal movement, levitation of the medium and touches: the table shows at once that this was a very rare combination in any case; O O X O meant levitation only: at the sittings in Schrenck's laboratory at which levitations were reported, other phenomena were noted as well, not however levitations and

Table 6

Sittings recorded by Schrenck-Notzing, excepting those that took place in the Schneider flat in Braunau. (Except in this Table, Schrenck's serial numbers are in brackets.)

Serial No. (Schrenck's)	Date	1	2	3	4	Serial No. (Schrenck's)	Date	1	2	3	4
4	21. 6.24	X	X	X	X	48	11.11.27	X	0	0	0
5	23. 6.24	X	X	X	X	49	17.11.27	0	0	0	0
6	6. 9.24	X	X	0	0	50	24.11.27	X	X	0	0
7	20. 9.24	0	0	0	0	51	1.12.27	X	0	0	0
8	21. 9.24	X	X	0	X	52	6.12.27	X	0	0	0
9	5.10.24	X	0	0	0	53	13.12.27	X	X	0	0
10	18.10.24	0	0	0	0	54	19.12.27	X	X	0	0
11	19.10.24	X	X	0	0	55	2. 1.28	X	0	0	0
12	2.11.24	X	0	0	0	56	10. 1.28	X	X	0	0
13	22.11.24	X	0	0	0	57	17. 1.28	X	X	0	0
14	29.11.24	X	0	0	0	58	24. 1.28	X	X	0	0
15	7.12.24	X	X	X	0	59	31. 1.28	X	0	0	0
16	27.12.24	0	0	0	0	60	7. 2.28	X	X	0	0
17	5. 1.25	X	0	0	0	61	14. 2.28	X	X	0	0
18	17. 1.25	X	0	0	0	62	22. 2.28	X	0	0	0
19	24. 1.25	X	X	0	0	63	28. 2.28	X	X	0	0
20	27. 1.25	0	0	0	0	64	2. 3.28	X	X	0	0
20a	14. 2.25	X	0	0	0	65	7. 3.28	X	0	0	0
21	18. 4.25	0	0	0	0	66	5. 5.28	X	0	0	0
22	22. 4.25	X	0	0	0	67	9. 5.28	X	0	0	0
23	2. 5.25	X	0	0	0	68	15. 5.28	X	X	0	0
24	23. 5.25	X	0	0	0	69	29. 5.28	X	X	0	0
25	30. 5.25	0	0	0	0	70	1. 6.28	X	X	0	0
26	31. 5.25	X	0	0	0	71	6. 6.28	X	0	0	0
27	1. 6.25	X	X	0	0	72	12. 6.28	0	0	0	X
29	13. 6.25	X	0	0	0	73	19. 6.28	X	0	0	0
30	20. 6.25	0	0	0	0	74	26. 6.28	X	X	0	0
31	21. 6.25	X	X	0	0	75	10. 9.28	0	0	0	0
32	22. 6.25	0	0	0	0	76	13. 9.28	0	0	0	0
33	23. 6.25	X	X	0	0	77	15. 9.28	0	0	0	0
34	15.10.25	X	0	0	0	78	17. 9.28	0	0	0	0
35	12.12.25	X	X	0	0	79	19. 9.28	0	0	0	0
36	2. 1.26	X	0	0	0	80	26. 9.28	0	0	0	0
37	6. 2.26	X	X	0	0	81	1.10.28	0	0	0	0
38	13. 3.26	X	0	0	0	82	22.10.28	0	0	0	0
39	14. 3.26	X	0	0	0	84	26.11.28	0	0	0	0
40	12. 6.26	X	0	0	0	85	11. 1.29	X	X	0	0
41	18. 9.26	X	X	0	0	86	17. 1.29	X	X	0	0
42	4.12.26	0	0	0	0	87	24. 1.29	X	X	0	0
43	19. 2.27	X	0	0	0	88	1. 2.29	X	X	0	0
44	20. 2.27	X	0	0	0						
45	18.10.27	0	0	0	0						
46	24.10.27	0	0	0	0						
47	7. 9.27	X	0	0	0						

Table 7

Comparison of sittings reported by Schrenck-Notzing (excluding those that took place in the Schneider home) with sittings recorded in Schneider Journal (a) home; (b) away but excluding those supervised by Schrenck-Notzing; (c) away all recorded in Schneider notes.

1 2 3 4	5	6	7%	8%	9 %
Type of sitting	Schrenck n	Schrenck %	Schneider (a) home	Schneider (b) away without Schrenck	Schneider (c) away all recorded
0 0 0 0	21	25	6.4	14.1	18.7
X X X X	2	2.4	5.3	4.2	5.1
X 0 X X	0	0	1.7	1.4	1.0
X X 0 X	1	1.2	13.3	1.4	3.0
X X X 0	1	1.2	4.6	2.8	3.0
0 0 X 0	0	0	1.2	4.2	3.0
X 0 X 0	0	0	9.2	12.7	9.3
X 0 0 X	0	0	4.0	4.2	3.0
X X 0 0	27	32.1	28.3	7.1	11.4
X 0 0 0	31	36.9	22.5	42.3	38.5
0 X 0 X	0	0	0.6	1.4	1.0
- - - -	0	0	2.9	4.2	3.0
0 0 0 X	1	1.2	0	0	0
	<hr/> 84		(n = 173)	(n = 71)	(n = 96)

movements of objects only, X O X O. It so happened that when Schrenck witnessed the levitation of the medium, there were materialisations and in two cases materialisations as well as touches.

It is certainly interesting that the proportion of Schrenck sittings at which there were movements of objects and materialisations, X X O O, was fairly substantial: 32.1% as against 7.1% at Schneider away sittings at which Schrenck was not present. Schrenck seances at which there were movements of objects only, X O O O sittings, were only slightly more frequent at 36.9%; Schneider Journal away seances from which Schrenck was absent of the type X O O O were 42.3% of sittings.

The only seance on record at which touches only occurred, O O O X, was a Schrenck sitting: this was the occasion when only 'Anton' was in evidence and this was apparently the only phenomenon in his repertoire. Materialisations and touches only, O X O X, never took place at Schrenck's, but this combination was also extremely infrequent in the other contexts, indeed as has been suggested, such seances may in fact never have occurred and be an artefact of incomplete reporting.

Thus, although there were appreciably more negative sittings under Schrenck's direction than in the home circle, all the major types of phenomena, levitation, movement of objects and materialisations did take place under Schrenck's exclusive control, though certain combinations did not take place. On the other hand, one gets the impression from Table 7 that though Schrenck may have exerted an inhibiting influence on the phenomena (on whatever interpretation) in general, he did not depress the tendency towards visible materialisations.

Table 8 isolates the types of sittings, all combinations, at which objects were reported to move. It will be seen that at 83.9% of all sittings in the Schneider home circle did objects move, at 76.1% of all sittings away from the Schneider home and without Schrenck's presence were they reported to move, as compared with 73.8% of sittings under Schrenck's control and in his laboratory. The differences do not seem appreciable. The figures suggest that whereas the home milieu was slightly more conducive to encouraging the paranormal movement of objects, Schrenck's presence or absence made little difference to this

Table 8

Comparison of sittings at which objects were reported to move: (5) Schneider Journal, home; (6) Schneider Journal, away but excluding sittings supervised by Schrenck; (7) Schneider Journal, away, all sittings; (8) Schrenck excluding those in Schneider home in Braunau.
(S.J. = Schneider Journal)

1 2 3 4	5	6	7	8
Type of sitting	(a) S.J. home	(b) S.J. away without Schrenck	(c) S.J. away all : recorded	(d) Schrenck minus Braunau
	%	%	%	%
X X X X	5.3	4.2	5.1	2.4
X 0 X X	1.7	1.4	1.0	0
X X 0 X	13.3	1.4	3.0	1.2
X X X 0	4.6	2.8	3.0	1.2
X 0 X 0	9.2	12.7	9.3	0
X 0 0 X	4.0	4.2	3.0	0
X X 0 0	28.3	7.1	11.4	32.1
X 0 0 0	22.5	42.3	38.5	36.9
	<hr/>	<hr/>	<hr/>	<hr/>
	88.9	76.1	74.3	73.8

phenomenon.

Table 9 isolates the types of sittings at which visible materialisations were reported. Here it will be seen that the home circle was definitely more favourable, with a total percentage of 52.1% of sittings at which there were materialisations. The comparatively high percentage in column 8 showing that at Schrenck controlled sittings there were altogether 36.9% at which there were materialisations confirms the impression that Schrenck in some way furthered this type of phenomenon, when this figure is compared with materialisation at away sittings at which he was not present, 16.9%

Table 10 isolates the sittings at which total levitations of Rudi's body were said to have occurred. Here, for the first time, we have a substantial difference between the incidence as observed under Schrenck's control away from Braunau at 3.6%, with the percentage at home 22.0%, and away sittings without Schrenck 25.3%. It is clear that this phenomenon took place outside the Schneider home at least as well as in Braunau. This certainly confirms the evidence we have to the effect that no elaborate and heavy installation could have been involved. It would seem that either Schrenck in some way exerted an inhibiting influence over the phenomenon of levitation of the medium, or else that it was waning when Schrenck did his most concentrated experimenting.

A comparison of Tables 3 and 6 bears out this latter supposition. Schrenck's last report of a levitation was his sitting No. [15], which is Vater Schneider's 106 (my serial number). Before this sitting, out of 105 seances, there were 43 at which levitations were observed; in the subsequent 190 sittings there were only 14 levitation seances, and these grow even sparser. Therefore at his first 15 sittings at his laboratory Schrenck observed three levitations, one fifth, which may be compared with the over-all percentages observed over the whole course of the Schneider Journals.

Levitation of the body of the medium of all the phenomena observed with Rudi was the only one never reproduced outside Central Europe. It disappeared relatively early on in the course of the mediumship, the last recorded instance being 21st September 1926. Indeed, from 5th March 1924 onwards

Table 9

Comparison of sittings at which visible materialisations were reported: (5) Schneider Journal, home; (6) Schneider Journal, away but excluding sittings supervised by Schrenck; (7) Schneider Journal away all sittings; (8) Schrenck sittings excluding those in the Schneider home in Braunau.
(S.J. = Schneider Journal).

1 2 3 4	5	6	7	8
Type of sitting	S.J. home	S.J. away without Schrenck	Away, all recorded sittings	Schrenck, minus Braunau sittings
	%	%	%	%
X X 0 X	5.3	4.2	5.1	2.4
X X X 0	4.6	2.8	3.0	1.2
X X 0 X	13.3	1.4	3.0	1.2
X X 0 0	28.3	7.1	11.4	32.1
0 X 0 X	0.6	1.4	1.0	0
	<hr/> 52.1	<hr/> 16.9	<hr/> 23.5	<hr/> 36.9

Table 10

Comparison of sittings at which levitations of the medium were reported (explanation as for Table 9).

1 2 3 4	5	6	7	8
Type of sitting	S.J. home	S.J. away without Schrenck	S.J. away all recorded sittings	Schrenck, minus Braunau sittings
	%	%	%	%
X X X X	5.3	4.2	5.1	2.4
X 0 X X	1.7	1.4	1.0	0
X X X 0	4.6	2.8	3.0	1.2
0 0 X 0	1.2	4.2	3.0	0
X 0 X 0	9.2	13.7	9.3	0
	<hr/> 22.0	<hr/> 25.3	<hr/> 21.5	<hr/> 3.6

there were recorded altogether about 50 seances, many of which claimed several instances of levitation, including those observed and independently witnessed at Schrenck's laboratory.

One naturally asks why were the levitation phenomena so much more transient than the rest? It appears plain that from the evidence (including that of Meyer and Przibram) that heavy apparatus could not have been involved, and that if it was a trick of some sort, it must have been some gymnastic feat that could be performed as easily in his own home as at the place of complete strangers. It would be tempting to dismiss levitation as some sort of trick if the evidence and the testimony for this phenomenon had not been quite substantial despite the fact that it did not maintain itself and was never observed in Rudi's case outside Central Europe.

The only remotely 'non-paranormal' interpretation that still seems feasible is a suggestion by Professor R. Hecker (which he himself did not believe) that someone was pushing Rudi upwards, but neither Hecker nor anyone else detected the slightest evidence for such a strong-arm accomplice (119). And even this explanation fails if it is indeed the case that reputable and sane witnesses cited by Schrenck-Notzing passed their hands underneath Rudi's floating body.

Table 11 isolates those sittings at which people reported the subjective sensation of being touched. Considering that this is probably the most easily faked phenomenon of all, the very low percentage of occasions on which it was reported is rather remarkable: only at about a quarter of the home sittings is there any report of people feeling touches of an invisible agent, and at sittings under Schrenck's control away from Braunau the percentage is low at 7.7%.

These figures can be explained in several ways. This phenomenon did not fade away completely as did the levitations, though it became somewhat sparser in the course of time. Unlike levitation, there were fewer reported touch-sensation seances away than at home. It could be the case that this phenomenon was less faithfully reported than others. We are dependent, in this case, upon an individual sitter's telling the others that he was experiencing what, when all is said and done, could be described as an hallucination, an unverifiable experience that could not be shared by other participants. It could be that

Table 11

Comparison of sittings at which persons reported experience of being touched: (5) Schneider Journal, home; (6) Schneider Journal away but excluding sittings supervised by Schrenck; (7) Schneider Journal away all sittings; (8) Schrenck sittings excluding those in the Schneider home in Braunau. (S.J. = Schneider Journal).

1 2 3 4	5	6	7	8
Type of sitting	S.J. home	S.J. away without Schrenck	S.J. away, all recorded sittings	Schrenck, minus Braunau sittings.
	%	%	%	%
X X X X	5.3	4.2	5.1	2.4
X 0 X X	1.7	1.4	1.0	1.7
X X 0 X	13.3	1.4	3.0	1.2
X 0 0 X	4.0	4.2	3.0	1.2
0 X 0 X	0.6	1.4	1.0	0
0 0 0 X	0	0	0	1.2
	<hr/> 24.9	<hr/> 12.6	<hr/> 13.1	<hr/> 7.7

Table 12

Comparison of entirely negative sittings; explanation as in Table 11.

1 2 3 4	5	6	7	8
Type of Sitting	S.J. home	S.J. away without Schrenck	S.J. away, all recorded sittings	Schrenck, minus Braunau sittings
	%	%	%	%
0 0 0 0	6.4	14.1	18.7	25

the more strangers were present, and the more educated and sophisticated the sitters, the more reluctant they were to state that they had had such an experience.

If one takes the line that all phenomena must have been fraudulent one might be inclined to interpret the figures as meaning that control at Schrenck's was much better. However, such an interpretation would have to meet the objection that his conditions were not that much better as regards the prevention of movements of objects or materialisation (see discussion of Tables 8 and 9).

If one accepts the possibility that the phenomena may have been genuine, one might speculate that in order to produce tangible impressions a greater degree of materialisation is required than to produce paranormal movements of objects. At the subsequent Hope-Rayleigh investigation (120) where the actual number of occurrences was counted (phenomena being by that time, Autumn 1932, far less frequent), there were altogether 15 instances of sitters reporting being touched as against 84 reports of objects moving paranormally and 83 sounds being heard which included indications of paranormal movement; and this (8.9%) seems quite a reasonably similar relationship to that between 7.7% of all sittings of Schrenck's containing reports of sitters feeling touched. On the face of it, the touch phenomenon was a good deal less frequent than the movement of objects.

If we now compare the distribution of entirely negative sittings in Table 12, we find that the lowest incidence, 6.4%, was at the Schneider home in Braunau, as against the highest, 25%, at Schrenck-Notzing's laboratory in Munich.

One obvious question that arises as one examines the Schneider Journals is whether the phenomena tended to occur only in the presence or absence of certain persons: this is of interest and importance on any hypothesis, whether fraudulent or paranormal. Obviously, the information to be gleaned from figures again could at most be suggestive and could not possibly be conclusive. On the other hand we have a good deal of evidence to suggest that on the whole Vater Schneider's records of those present were adequate, and also a good deal of testimony to the effect that if the phenomena were indeed being

produced by trickery, the culprit must be a member of the circle. It must be stressed that nothing is claimed for the following tables other than that they give a picture of who was, at any rate officially, present at Schneider sittings as compared with the types of phenomena reported. If one wishes to entertain the hypothesis that the phenomena were genuine, then it is of very great interest to know whether the presence or absence of certain sitters and conditions promoted or hindered their occurrence as first incorporated in a table by C.C.L. Gregory (121) with respect to the Hope-Rayleigh series.

In order to enable one to form a picture of the number of sitters listed in the Schneider Journals and how many times each one attended, it was essential to decipher all the names of all the sitters. This, as can be imagined, was a considerable labour. It cannot be guaranteed that my count was perfectly accurate - I counted 796 names, and allotted each one a record card. However, some signatures simply defied interpretation and I was forced to leave them out, it is quite safe to assume that these names belonged to persons who attended only once or twice. People who went more often tended to be mentioned in the quite legible main body of the report which usually enabled me eventually to decipher who was meant by the scrawl at the head of the report. Also, slight variations in oft-repeated signatures made it easier to hazard a guess as to what name was intended. Even in the case of names that occurred only once, mention in the main text of the report was a frequent source of enlightenment as to who was the owner of the otherwise puzzling signature. Occasionally I may have counted the same person as two people: one cannot be absolutely certain whether 'Maria Kraus' is the same as on some later date 'Mitzi Kraus' or as the better half of 'Herr und Frau Kraus'. Incidentally, where the ladies have signed their own names their signatures are incomparably easier to read than the men's, and it is often plain what a gentleman's name must be after reading his wife's straight forward signature.

In spite of these reservations I have no doubt that my figures are of the right order, and it is safe to assume that the number of different signatures in the Schneider Journals is of the order of 800.

Table 13 shows the number of seances attended per person, or more pedantically, the number of reports signed per person.

Out of 796 people who attended seances or signed their names, 557 attended only once; in other words, just under three quarters of the sitters were casual spectators. 174 participated at between 2 and 4 sittings. The remaining 65 sitters attended appreciably more often.

I have analysed the attendance records of those who were present 15 or more times, and these are listed in Table 14. From this it is plain that four attended very much more regularly than anyone else, namely Franz and Cilli Ramspacher, and Karl and Rosa Schneider, two married couples. Franz Ramspacher acted as leader of the Braunau circle. He was a colleague of Vater Schneider, also a type-setter. Karl was as has been mentioned Rudi's older brother who was regarded as an auxiliary medium, and he often went into trance along with Rudi. On the other hand, as I have already stated, he also quite often acted as controller.

I have ignored Vater Schneider's signatures. He was manifestly nearly always there in Braunau, though he did not always bother to sign, and if one is determined to regard him as the stage manager, little can be done by examining the data as presented in the Journals, since he is so largely their author and compiler. It should however be noted that much of the same phenomena occurred in Schrenck's laboratory when neither Vater Schneider nor Karl nor any other member of the Schneider clan or entourage was present.

Table 15 summarises the type of sittings that occurred if one progressively excludes Karl and Rosa Schneider, Franz and Cilli Ramspacher, and Sergeant Traunmüller, Wastl Riebel and Major Rudolf Kalifius.

Column 5 shows what happened when Karl and Rosa were present, and column 9 expresses this as a percentage of the 120 sittings at which between them they participated according to the records. Column 6 shows what happened at the 149 sittings from which Karl and Rosa were absent and column 11 expresses these as percentages. Column 7 shows what happened at the seances from which Karl, Rosa, Cilli and Franz Ramspacher were absent and column 12 converts this into percentages.

Table 13

Frequency of attendance per person at seances as recorded
in Schneider Journals.

2	1
Number of reports signed or sittings attended	Number of persons
1	557
2 - 4	174
5 - 20	51
21 - 64	10
(65 - 99)	0
100 or more	4
	<hr/>
	796

Table 14

Names of persons who signed 15 or more seances:

Number of reports
signed

15	Gusti Morauke
16	Otto Meixner
21	Prof. Karl Gruber, Mrs. Hedwig Flick
22	Major Flick
23	Lorenz Kraus
24	Hans Morauke
28	Poldi Ofenmüller
41	Dr. v. Schrenck-Notzing
49	Major Kalifius
51	F. Traunmüller
64	Wastl Riebel
102	Cilli Ramspacher
108	Franz Ramspacher
105	Rosa Schneider
107	Karl Schneider

Table 15

Comparison of types of phenomena reported in the presence of those members of the Braunau circle who had the highest record of attendance (Schneider Journals).

1 2 3 4	5	6	7	8	9	10	11	12	13
Type of sitting	Karl & Rosa S. present (no. of sittings)	Karl & Rosa S. absent (no. of sittings)	Karl & Rosa S., C. & F. Ramspacher absent (no. of sittings)	Karl & Rosa S., C. & F. Ramspacher, Traunmüller, Riebel, Kalifius absent (no. of sittings)	Karl & Rosa S. present	Karl & Rosa S., F. & C. Ramspacher, Traunmüller, Riebel, Kalifius absent	Karl & Rosa S. absent	Karl & Rosa S., F. & C. Ramspacher absent	All Schneider Journal sittings
0 0 0 0	8	20	17	17	% 6.6	% 16.5	% 13.5	% 13.2	% 10.4
X X X X	10	4	3	2	8.3	1.9	2.5	2.4	5.2
X 0 X X	4	0	0	0	3.3	0	0	0	1.5
X X 0 X	15	11	9	5	12.6	5.8	7.3	7.0	9.7
X X X 0	10	1	1	1	8.4	0.9	0.6	0.8	4.1
0 0 X 0	0	5	4	4	0	3.9	3.4	3.0	1.9
X 0 X 0	12	13	11	10	10.1	9.8	8.7	8.4	9.3
X 0 0 X	5	5	5	4	4.2	3.9	3.4	3.9	3.7
X X 0 0	29	31	26	12	24.1	11.7	20.8	20.2	22.3
X 0 0 0	23	53	49	43	19.1	41.8	36.0	38.1	28.2
0 X 0 X	0	2	0	0	0	0	1.3	0	0.8
- - - -	4	4	4	4	3.3	3.8	2.5	3.0	2.9
	<hr/> 120	<hr/> 149	<hr/> 129	<hr/> 102					

Column 8 analyses those 102 sittings from which Karl, Rosa, Cilli and Franz Ramspacher, Wastl Riebel, Sergeant Traunmüller and Major Kalifius were absent and this information is expressed in terms of percentages in column 10.

It appears that there were certainly fewer totally negative 0 0 0 0 sittings in Karl and Rosa's presence than in their absence: 6.6% when they were there, 13.5% when they were not. The Ramspacher's presence does not seem to have coincided with any difference of negative sittings and the remaining habitués very little. Totally blank sittings did occur when all these regular sitters were present, and quite frequently such blank sittings were attended only by inner circle members.

8.3% of X X X X (all types of phenomena) sittings were noted when Karl and Rosa were present, 2.5% when they were not; and when all the most frequent sitters were absent the proportion of these X X X X sittings fell as low as 1.9%.

It is very striking that (with the exception of the 0 0 X 0 type whose number is extremely small) Karl Schneider's presence always coincided with a higher percentage of a given type of sitting except in the case of the X 0 0 0 (paranormal movement only) type. As regards these X 0 0 0 sittings, not only were there nearly twice as many instances recorded when the Karl Schneiders were absent than when they were present, there is also a small rise in the incidence of this type of seance when the habitual sitters were absent.

This was so far as I was concerned an unexpected result, and it emerged only when the tables had been compiled. If we compare the data in the X 0 0 0 line, we will find that there were 19.1% of such seances with Karl and Rosa present, 28.2% in the entire series of sittings, 36.0% when they were absent, 38.1% when the Ramspacher's also were absent and even more, 41% when the remaining habitual sitters were not there.

It is plain from the figures that the seances were richer or more varied in the presence of the habitual sitters, but that the X 0 0 0 (movement only) seances were actually more frequent in their absence. Like all figures, these may again be interpreted in a number of ways. One could for example suppose that there was some accomplice who came into operation when they were not there and who specialised on movement of objects

or who could produce nothing else, whereas they specialised in producing the other types of phenomena. On the other hand, there is the entry in Table 6 which shows that the highest percentage of X O O O sittings took place at seances away from the Schneider home and from which Schrenck was also absent, 42.3% (38.5% all away sittings). Did Rudi bring his unnamed and unrecorded and unnoticed accomplice or accomplices along with him? Did he manage to do so in the 36.9% of sittings at Schrenck-Notzing's laboratory? This is unthinkable without the latter's complicity plus a considerable simple-mindedness on the part of his numerous academic, medical and professional sitters. Is this feasible? The figures of course will not tell us.

If, on the other hand, we are willing to entertain the hypothesis that these phenomena were such as to require a 'paranormal' explanation, then it would seem that movement of objects was somehow a simpler type of phenomenon, one that persisted longer, maintained itself in the presence of strangers more easily than the other phenomena: both as time went on, and also as one excludes certain sitters, and as conditions become more unfamiliar to the medium, so do these X O O O sittings increase in relative frequency.

It would certainly be very interesting to compare this process with what happened in other careers of physical mediumship, although we do not to the best of my knowledge have the same wealth of precise data in any other case.

What emerges from an analysis of this type is that quantification is one tool among others, and a potentially useful one, for promoting understanding of recurrent episodes of this kind, for examining characteristics of such situations, and for enabling us to discriminate at least between prima facie hypotheses. Numbers as such are no more decisive than are words: they too have to be interpreted and evaluated in terms of concepts logically superordinated to themselves.

Such an explanation highlights, among other things, the importance of collecting sufficient qualitative data in the first place to make possible subsequent quantitative analysis. It may of course become apparent only with the benefit of hindsight just what types of data it would have been desirable to have. For example, in the case of the Schneider records a systematic account of the intensity and frequency of

phenomena at any given sitting would have been more than useful. However, such cavilling should not blind one to the, so far as I know, unprecedented achievement of Vater Schneider's records. He must have been a most careful, patient, tidy, persistent and methodical individual.

Collecting data in this manner is highly suitable for subsequent computer analysis, which may reveal other features. Such an analysis is in preparation (122).

Without implying that the figures prove the genuineness of the phenomena, the tables may reasonably be interpreted to suggest the following:

1. There is evidence for a long-term decline effect both as regards variety and frequency of the phenomena reported, and as regards their occurrence.
2. Levitation of the whole body of the medium was a phenomenon belonging to the earliest and most vigorous phase and requires the greatest amount of paranormal physical energy, whatever that may be.
3. Visible materialisation also belongs to an earlier and more active phase, although it persists longer than levitation.
4. The movement of material objects (until the subsequent affecting of infra-red radiation) was the least difficult effect to produce and that which persisted longest.
5. The whereabouts of the seance affected the phenomena: the home environment was on the whole the most favourable, other homes being second, and Schrenck-Notzing's laboratory the least favourable. However, no particular place was essential for the production of any phenomena.
6. At least some sitters affected the incidence of phenomena. Schrenck's influence seems to have been somewhat negative overall, although it is interesting to note that he did not depress the proportion of visible materialisations (which were subject to an overall decline effect). Schrenck was of course a strong believer in materialisation. On the other hand, the habitual home circle sitters seem to have enhanced the effects; in particular Karl Schneider, who had himself some claims to being a physical medium.

5 and 6 are not entirely independent. However, the figures suggest that both sitters and familiarity of the environment contribute to the occurrence or otherwise of phenomena.

7. No particular named participant other than the medium himself was essential for the production of any of the phenomena.

2.4 Overall considerations

The case of Rudi Schneider is in certain respects not characteristic of physical mediumship: it is one of the 'best' cases, and it was chosen exactly because there exists such reasonable testimony and ample documentation. This latter is no doubt in large part due to the fact that the medium was 'discovered' very early on in his career by researchers who were able to persuade Vater Schneider of the paramount importance of scientific considerations. The Schneiders were Catholics though obviously not particularly orthodox: sufficiently devout to pay for Masses for Lola's soul, not compliant enough to refrain from having seances. They were artisan-class working people: Capt. Kogelnik's fastidious disdain is all too clear from his initial reactions to the cramped Schneider flat (123), and Mother Schneider's appeals after her husband's stroke for money for medicines (124) are testimonies to poverty. Clearly Schrenck's aristocratic, financial and professional status carried great weight with all the Schneiders, and he persuaded them that it was all-important for Rudi to become a 'scientific medium'. ('Olga', who saw 'herself' as an equal of kings and barons, was far less deferential, and might be said to have engaged in a bit of personal class struggle of 'her' own, for which Rudi could not be held responsible!)

The fact that there is virtually - indeed no - evidence of fraud worthy of the name, again is not wholly characteristic. In most cases there is some evidence at some stage, although as in the case of Palladino this may not account for the observations at other times (125). Rudi was, by all accounts, a simple straight forward upright person, interested in football, cars, aeroplanes and mechanical matters generally. Besterman (126) suggests that he was rather stupid, but I doubt that: he was compliant, deferential, cooperative, relatively uneducated and spoke no English. He also seems to have been attractive to the opposite sex and given to flirting in his younger days. However, he turned down the advances of a wealthy young woman,

Miss M. I., whose father would have welcomed him as a partner in his business (127), and married Mitzi Mängl who was as poor as he. There is nothing in his life to suggest that money or power or notoriety were major goals. ('Olga' is inevitably another matter!)

Some of the problems encountered in investigating special Subjects are thus avoided: documentation is good from the start, there are no religious or cult complications, his personality and conduct were as good as one could hope to find.

On balance and especially in view of the excellence of some of the records, testimony and reports, and in view of the poor quality of the attempts at exposure, it seems to me that the case presents an excellent prima facie argument in favour of genuineness of major physical phenomena and for interference with infra-red radiation, and for further work on similar lines.

On the other hand, most of the other difficulties outlined in Chapter 1 may be seen exemplified in the Rudi case study. For clarity and conciseness these will be grouped under four headings.

2.41 Problems arising from lack of theory and definition

When dealing with historical records, obtaining neutral descriptions of occurrences may be peculiarly difficult. The dispute between Schrenck and Moser may be taken as an interesting example of a genuine difference in perceptual interpretation, masquerading as some sort of non-specific accusation of fraud. Were the objects, so to speak, 'swung about' as if by an invisible hand, as described by Schrenck, or did they move more impersonally as though on an 'elastic support' as perceived by Moser? Did the curtains bulge as though pushed by a person, or fly about as though subject to some current of air? These are potentially interesting and important theoretical issues and require a careful reconsideration of what is meant by 'physical causes' and 'movement': we really are in the realms of analysing, or needing to analyse, Aristotle's kinesis. The phenomenology of observation is of course a thorny issue.

Difficulty was encountered in attempting to extract quantitative data, in that these had to be based on historical accounts (unavoidably post hoc), and that the descriptive accounts were not designed for the purpose, consequently

important information, though probably available to the writer of the Journals (e.g. some indication of intensity and frequency per hour of phenomena) was lost.

2.42 Problems arising from orthodoxy in science

The case clearly illustrates the problems arising for open discussion and consideration of claims deemed impossible a priori by the scientific community. The provision of evidence in favour of the phenomena becomes a virtually hopeless task since any evidence against them, however ludicrously inadequate, will be acceptable to the educated public. The publication of Price's letter denouncing Rudi in Nature is a sorry and instructive prize exemplification of this reflection, but the other 'exposures' illustrate milder degrees of the same principle.

Furthermore, this failure to accord at least a reasonably equitable hearing to (a) serious investigation and (b) the possibility of genuine positive results, means that it is very difficult for any organisations or institutions to embody and effectively promote scholarly and academic standards, since from the point of view of the 'Establishment' it was not the quality of investigations (or investigators) that counted, but their negative outcome. Thus the Society for Psychical Research, and the Institut Métapsychique International, both in principle fully committed to scientific standards, were quite lightly brushed aside in favour of the National Laboratory of Psychical Research - virtually a front for a one-man public relations effort.

2.43 Social and ethical problems

The issues mentioned under 2.42 also have social and ethical implications in that they spring from a systematic biasing of purely logical considerations in a particular direction and have a bearing on fairness and the pursuit of truth. There are a number of other social issues that are highlighted by a consideration of the investigations. Most of the interactions inevitably have ethical implications or at least overtones, since norms of conduct between individuals and groups are involved, as well as choices between values.

Mutual confidence is dependent on shared standards, and this is particularly important where phenomena are not readily repeatable. Without such confidence collaboration and progress

in a field are difficult if not impossible. In addition, even individuals subscribing to similar standards may themselves have different a priori prejudices. For example, the Sidgwicks who were, deservedly in most respects, immensely influential in the field of psychical research, tended to throw the full weight of their prestige behind a repudiation of PK so that, despite Mrs. Sidgwick's total contempt for Price, the latter could count on the unilateral scepticism as regards physical phenomena supported by her to lend maximum impetus to his denunciation of Rudi. There is good reason to suppose that Price had no intention whatever of undermining the physical, or even most of Rudi's own phenomena. In his very last letter, 27 March 1948 (128) he ridicules Dingwall for suggesting that an accomplice might have been introduced into Rudi's sittings, and for casting any doubt on the physical phenomena of Willi or of Stella C. Yet a consequence of his action was almost certainly to finish the topic for a generation of researchers and potentially benevolent outsiders.

Supporters such as Dr. William Brown were wholly alienated. The correspondence in the Harry Price Library clearly shows that it was not only the element of personal treachery on Price's part, but the questions of respectable affiliation and of publicity that were deciding factors in Brown's case. His contribution to the Hope-Rayleigh papers (129) is distinctly non-committal, and it is not until after his retirement that he once again openly and publicly re-affirmed his belief in the genuineness of Rudi's phenomena at a packed meeting of the University of Oxford Psychological Society sometime in the early 1950s. (I regretfully report that in the case of at least one member of the audience, the present writer, Brown was simply dismissed as deranged in consequence.) It is easy to see that Brown, especially as a member of the medical profession, and as one seeking to establish ordinary psychology as an academic subject at Oxford, could hardly afford contact with a field that was not only even more heterodox than psychology itself then seemed to many academics, but in addition rendered ludicrous and repugnant by Price's conduct.

The whole question of publicity and publication is highlighted by the case history: both claims and denunciations are

eminently newsworthy, which may be a considerable hindrance to dispassionate scrutiny. According to Dr. Gerda Walther (130) a Dr. Edmund Holub, who had vouched for the authenticity of Willi Schneider was recovering from an operation when some imprudent person gave him a newspaper containing the Meyer-Przibram denunciation of Rudi, whereupon Holub had an immediate relapse and actually died. The publicity surrounding Price, and both his advocacy and subsequent denunciation, could hardly have been more sensational even without the somewhat embarrassing Blocksberg episode. The problems arising from the notoriety of the subject matter are all too plainly illustrated.

However, even less sensationalistic publication presents special problems in an unorthodox field. When it comes to joint publication by persons, especially of widely disparate standards and loyalties, problems may, and did, become explosive. It has been seen how invidious was, for example, Price's sole control over the notes taken at sittings; hardly anyone at the time, however hostile to him, seems to have even questioned the accuracy of his actual seance reports that appeared about a year later.

Vater Schneider had understood the importance of instant recording and signing for his son's reputation, and Hope and Rayleigh circulated all seance reports as soon as the official note-taker's manuscript had been typed, within a day or so. (A set is in my possession (131).) It is the very high standards in this respect that secures for the Hope Report its importance in the field.

Osty's research was more original, adventurous and in a sense also more professional. Indeed, the Hope sittings might be seen, as a mere replication of Osty, although of course, in this field, replication is of the utmost importance. However, Osty's policy with regard to publication was explicitly not to enumerate and involve individual participants and provide detailed records; he wished to get away from the whole personalised cycle of accusation and counter-accusation. His wishes in this respect are of course understandable, and have been shared by numerous Parapsychologists since but, as may be seen from his brush with Price over Mitzi's presence, his policy rendered him vulnerable to attack from Price.

Osty's encounter with Price over the Duncan 'teleplasm' article illustrates the problems of priority of discovery and publication as they may present themselves in a psychical research context, and the deplorable consequences of the absence of agreed minimum standards.

Who, in the case of a joint venture, should determine format and timing of publication? To whom should credit (or the reverse) be apportioned? Which should have priority: a cautious, careful scientific account or the stirring popular claim to new marvels? What, with respect to recording and responsibility for publication, are the relative rôles, rights and duties of the organisers of an investigation, the experimenters, the experts consulted, and those who have made financial contributions? What the whole Schneider investigation makes abundantly clear is the need for explicit agreement and conventions that are made plain to all right from the beginning of a research project. Matters of confidentiality, publicity, records and publication should not be left unformulated and at the mercy of the most enterprising and perhaps least scrupulous of participants. At the same time, it is of obvious importance to preserve enthusiasm and enterprise, and not to discourage participants by too many quasi-bureaucratic restrictions and too much galling anonymity, and an unrealistic denial to them of more worldly incentives. The Rudi-Price episode at any rate illustrates what may happen if such a balance is not even considered beforehand.

Financial problems played an important part in nearly all phases of Rudi's investigations. Even in the case of the Schrenck sittings, these could not be published in full after his death because the Baroness refused to spend the money: in all Paris and London investigations shortage of funds was a major problem. Some of the friction between Price and Hope, and Price and Osty, undoubtedly arose because there was an all round scarcity of cash. Price for obvious reasons did not wish to admit his indebtedness to Hope and others in this respect, and Hope in his turn was too gentlemanly to mention his own contributions beyond restrained remonstrance at Price's claims to having financed Rudi's NLPR visits. However, the extensive correspondence both at HPL and IMI bears testimony to the financial worries underlying investigations.

Even more serious than the personal friction engendered was the extent to which experimentation itself was hampered, as expressed unambiguously by C.V.C. Herbert in his contribution to the Hope Report (132). This economic factor tends to be glossed over, but it deserves emphasis if only because of the extent to which parapsychology finds itself in a 'poverty trap', where it cannot achieve respectability without adequate funds, and cannot obtain adequate funds without respectability.

Another factor that comes under this heading is the treatment of the Subject of experiments, an issue that has not been confined to psychical research. It is however particularly acute in this context, in view of the rarity of such Subjects, and also in view of their ambiguous status in the community at large. They are subject both to adulation, and also automatic suspicion and vilification. Inevitably also there are apt to be special temptations for parapsychologists to establish a reputation for critical hard-headedness among sceptical professional colleagues at the expense of their Subjects (and of fellow researchers who have 'come out' in favour of the phenomena).

Rudi's case certainly illustrates the extreme vulnerability of a Subject as well as his associates, notably his then fiancée, to the risks of a disregard of his interests amounting to serious injustice. Even an upright man like Osty, after realising how little Price was to be trusted, clearly continued to regard Price as more of an 'important other' than the medium. Rudi's manifest failure for a long time even to grasp 'Onkel Harry's' treachery again exemplifies problems arising out of the ambiguous definition of relative rôles. It goes without saying that the Rudi Schneider mediumship has to be considered in the context of social rôles and regulations as they were in Europe and Britain in the 1920s and early 1930s, and that these have since to some extent changed.

2.44 Special parapsychological and psychological problems

Just like logical issues, parapsychological and psychological considerations cannot be fully separated from social and ethical ones, since problems of heterodoxy and outsiderdom have repercussions here also.

The most obvious feature of psychical phenomena is their elusiveness. They can neither be produced to order, nor can

circumstances be specified in which they might reasonably be expected. In the case of a medium such as Rudi, phenomena were reported over a number of years, and up to a point experiments could be conducted with him. But even a relatively successful experimenter such as Schrenck-Notzing in the early days found the waiting around in the dark or near-dark for manifestations hard to endure. The fact that phenomena are tied to a particular named person presents certain problems ideally illustrated by the Rudi Schneider case: had he not been such a 'valuable property', Hope and Osty would have been able to afford to take the line that Price could keep Rudi, they would continue to experiment with someone else.

The fact that phenomena decline over a period of time creates its own difficulties. Not only do seances become more and more blank and irritating, but inevitably doubt, reasonable or otherwise, is apt to arise over previously reported successes. Now it may be the case, either that earlier experimenters are more credulous and less careful, or else that the phenomena themselves become systematically attenuated. There is naturally a general prejudice in favour of the former supposition. However, in the case of the Schneider phenomena it seems to me somewhat arbitrary to make the automatic assumption that the earlier investigators were any less careful or honest than later ones. Moreover, apart from the levitation of the medium's whole body, there is overlap of all phenomena, even if these were weaker and less frequent in later sittings in France and England. Furthermore, the quantitative analysis of early sittings suggests that Schrenck had, if anything, a negative influence on effects.

Since the 'decline effect' is so ubiquitous in parapsychological experimentation, there is at least reason to suppose that this is yet another instance. This creates obvious problems: phenomena are apt, on the face of it, to be most vigorous, investigable and least unreliable earlier on, when researchers are least prepared and organised for their exploitation. (There is also a human tendency to believe that somehow, this time, they will continue unabated.) At any rate, such a sliding slope presents obvious experimental difficulties, whatever its interpretation.

In addition, psychological conditions required, either by the medium, or the controlling 'person', are also liable to

create further difficulties. In Rudi's case, he seems to have been willing to accept any conditions anyone thought up as regards security arrangements, timing and cooperation generally. Besterman and Gatty's reservations in connection with their word association tests illustrate their bias rather than Rudi's conduct. 'Olga' too seems, from all we know, to have been compliant as regards control conditions, and to have tried 'her' best for investigators, unless 'she' happened to be currently at war with them. On the other hand, 'she' insisted if not on darkness, at any rate on red light only, and demanded music of a type which drove at least one experimenter, Schrenck, to desperation; and on a degree of activity, noise and chatter, reciting and singing from participants which most of the rather dignified sitters found embarrassing and tiresome. Now these conditions of relative darkness, noise and general uninhibited racket are presumably equally conducive to a lowering of critical attention, and also to genuine phenomena. There is a good deal of general corroboration for the supposition that a rumbustious atmosphere is 'psi-conducive', and the Schneider mediumship may be taken as corroborating this supposition. It does not, however, make an investigator's life any easier, since the frame of mind required for careful monitoring and light-hearted and uncritical jollity may be hard to combine. This points to the unavoidable necessity for almost complete instrumentation and automation of recording equipment, and the extreme desirability of phenomena of an instrumental type which cannot be readily faked. The psychological atmosphere of spontaneity required is also at variance with the need for carefully pre-planned trial and control periods.

At least instrumentation has improved since the early 1930s although their cost has escalated correspondingly. The nature of secondary personalities in mediums and others however remains problematic, and means of investigating them have hardly progressed. A researcher in the field is confronted head-on with all the psychological problems of states of mind, introspection, self-assessment, consciousness - normal and 'altered' - which have posed near-intractable difficulties for normal and abnormal psychology.

In most mediumships issues regarding the psychology of

fraud and trickery arise, which would not appear to be the case as regards Rudi. The emergence of 'Olga' can hardly be used as evidence of dishonesty in any ordinary sense. 'Olga' herself was, by all accounts, frivolous, bossy, helpful in an imperious sort of way, sure of 'herself' irrespective of whether 'she' turned out to be right or not. 'She' continuously announced forthcoming splendid phenomena when nothing whatever in effect happened (133). Such total assurance combined with total fallibility may well be one of the psychological features that have to be taken into account in cases of physical (and possibly other) mediumship. It should be stressed in the case of Rudi that, if there was sufficient 'force' to produce phenomena at all, these gave every appearance of being under 'Olga's' control as regards onset, duration and location (134).

The issue of honesty does arise as regards Price, whatever construction is placed on his double exposure. Scientific fraud is not, of course, confined to parapsychology (135), and its elucidation has received little attention. In a more recent case (136) pressure to produce positive results and financial considerations seems to have been paramount, but such motivation could hardly have played much of a direct part in Price's case. Price was clearly caught in a web of reasonably straight forward desires: to be first in the field with any discovery, to be more powerful than any other investigator, to have monopolistic control over 'his' Subject, to manipulate fellow investigators into supporting his claims, to have maximal exposure and personal glory in the media and to punish, and if necessary destroy the reputation of, anyone who crossed him in any of these respects. On the other he also wanted scientific and academic respectability, and recognition of his status as a bona fide psychical investigator and acceptance and esteem as a colleague. These two sets of desires clashed head-on, and the first won out. What the episode illustrates is the extent to which an individual not perhaps in many respects so very different as regards motivation from many genuine scientific workers (and others) is liable to be destructive and ultimately self-destructive if that individual is totally unsocialised into the academic, scientific and scholastic ethos which forms the normal psychological backcloth of most researchers, and which moderates at least some of our more

piratical and entrepreneurial inclinations! It is in this respect also that research and researchers are particularly vulnerable in non-validated and heterodox areas: 'tacit 'social contracts' are not always in operation.

CHAPTER 3, SOME RANDOM NUMBER GENERATOR TRIALS

3.1 Preliminary experiments

On 22 and 23 September 1975 some informal experiments were conducted at my home, 43 Rosslyn Hill, London NW3, which took place partly (Table 16, Serial No. 1) in the kitchen and partly (Nos. 2 to 6) in my sitting room. My fellow experimenter was Mr. Scott Hill (SH) whom I had met at a session of the Parascience Conference 1975 during the preceding two days, where he had exhibited some apparatus for parapsychological experimentation which, I understood, had been devised and built by himself at the University of Copenhagen under the supervision of the late Dr. R. Mattuck. The main Subject in the experiments was my daughter, CMG, then 20 years old, and a very few tests were also performed with my other daughter, HRG. However, we concentrated on CMG, since she seemed to be obtaining systematically high scores.

The instrument used was the 'PK meter' which SH had exhibited and which he carried with him: a small, light instrument (for face see Fig. 4) an electronic random number generator based on semi-conductor noise with digital read-out and auditory feed-back.

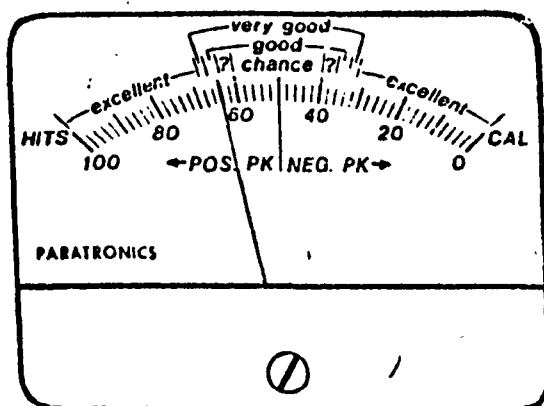


Figure 4. PK meter, 'face'.

Session Serial No.	Date	N	Hits	Deviation from MCE	Automatic Recording	Present in addition to CMG
(1)	22.9.75	4000	2098	98	Not taped	SH, AG
(2)	22.9.75	2000	1071	71	Taped	SH, AG
(3)	22.9.75	1000	513	13	Taped	SH, AG
(4)	23.9.75	1800	959	59	Not taped	- - -
(5)	23.9.75	3600	1852	52	Taped	- - -
(6)	23.9.75	3000	1560	60	? Taped	SH, AG
		15400	8053	353		

$$\sigma = \sqrt{Npq} = \sqrt{15400 \times \frac{1}{2} \times \frac{1}{2}} = \sqrt{3850} = 62.04$$

$$z = \frac{d}{\sigma} = \frac{353}{62.04} = 5.68 \quad \text{For CR} = 5.69, \\ P < 1.9 \times 10^{-9}$$

Table 16. Preliminary experiments with CMG
Analysis of individual sessions is contained
in Table 17.

$$(1) \sigma = \sqrt{\frac{4000}{4}} = \sqrt{1000} = 31.62, \quad z = \frac{98}{31.62} = 3.09 \\ \text{for CR } 3.10, P < 0.001$$

$$(2) \sigma = \sqrt{\frac{2000}{4}} = 22.36, \quad z = \frac{71}{22.36} = 3.17 \\ \text{for CR } 3.18, P < 0.001$$

$$(3) \sigma = \sqrt{\frac{1000}{4}} = 15.81, \quad z = \frac{13}{15.81} = 0.82 \\ \text{for CR } 0.82, P < 0.18, \text{ not significant}$$

$$(4) \sigma = \sqrt{\frac{1800}{4}} = 21.21, \quad z = \frac{59}{21.21} = 2.78 \\ \text{for CR } 2.78, P < 0.005$$

$$(5) \sigma = \sqrt{\frac{3600}{4}} = 30, \quad z = \frac{52}{30} = 1.73 \\ \text{for CR } 1.73, P < 0.05, \text{ not significant}$$

$$(6) \sigma = \sqrt{\frac{3000}{4}} = 27.38, \quad z = \frac{60}{27.38} = 2.19 \\ \text{for CR } 2.19, P < 0.01$$

Table 17. Analysis of individual sessions with CMG

The trials were automatically generated, 100 at the pressing and release of a button, at the rate of ca 3 per second, each 'hit' being fed back by a small flashing light, an audio tone and a (cumulative) advance of the pointer, each 'miss' being marked by the absence of light, tone and advance. Chance expectation being 50, the pointer would be expected to stay around mid-point at the end of a bout of 100. In the case of some of the trials the auditory output was recorded on cassettes for subsequent analysis. The experiments were conducted in a number of sessions, see Tables 16 and 17.

SH supplied AG with data for controls carried out by himself as follows, see Table 18.

N	Hits	Deviation from MCE
38700	19531	181

$$\sigma = \sqrt{\frac{38700}{4}} = 98.36 \quad z = \frac{181}{98.36} = 1.84$$

For CR = 1.84, P > 0.05, not significant.

Table 18. Control trials said to be carried out by SH on the same instrument as that used for SMG in Tables 16 and 17 above.

Discussion of results

CMG had never before given the slightest indication of paranormal accomplishments and, as the instrument was portable and convenient to use in any environment, it was thought that further experimentation with her and other unselected subjects might yield interesting results. Strict anonymity for CMG was insisted upon.

The data given above, described as 'pilot experiments', were added as Appendix 5 to an application of the Council of National Academic Awards (CNAA) on 26.1.1976 in an application to register for the Council's Research Degree. The concluding paragraph of Appendix 5 was:

SH's instrument might be thought to display a small positive bias: this is however debatable. AG would not have accepted any Critical Ratio of less than 2 in an experimental series. In any case the difference in CR between CMG (5.69) and SH (1.84) is considerable.

3.2 Following events and tests of equipment

There ensued a lengthy exchange of letters, papers etc. The number of documents involved is of the order of 100, some long and detailed. Perhaps an impression may be gained from the weight of my 'SH' file, which (admittedly including some duplicates) weighs $3\frac{1}{2}$ lb. It would obviously be inappropriate as well as intolerably tedious to detail all these, and what follows is therefore a discussion of a selection. Throughout I sent copies to Prof. Ellison and Dr. Beloff, and generally kept them informed, not always, I suspect, to their unalloyed delight. However, as it is the purpose of this thesis not merely to document actual attempts to obtain PK effects, but to study the difficulties encountered in the process, it is appropriate to give an account of the problems that arose in the wake of the preliminary experiments described in 3.1. Table 19 provides an outline of the main events briefly described below.

Even the most cursory glance at Table 19 will show that nothing came of the high hopes raised by the preliminary experiments. I had met SH for the first time at the Para-science Conference, and had been intrigued by his apparatus. I had definitely gained the impression that SH was a member of staff at the Physics Department at the University of Copenhagen, and constructed the apparatus there. Indeed correspondence and documentation were headed 'Fysisk Laboratorium 1, H.C., Ørsted Institutet Universitetsparken 5, 2100 København Ø, and my bank duly remitted to SH to that destination in response to an invoice from that address. I was certainly amazed eventually to receive the apparatus from a company called 'Paratronics' located in California and also to learn that SH was in effect a sales representative of that American firm, and a research student at Copenhagen University.

When I calculated the critical ratios and anti-chance values for the informal experiments conducted in September 1975, and found these to be substantial, I informed Dr. Beloff and Prof. Ellison, and sought and obtained funds to purchase further equipment from the SPR Research Grants Committee. I completed an application to the CNAA for registration for a higher degree, part of which was concerned with this type of experimentation.

22/23 September 1975	Experiments with CMG.
24 September 1975	Results deposited with SPR.
6 December 1975	AG orders PK-1 and ESP-1 equipment.
26 January 1976	Date of AG's application to CNAAC
26 January 1976	SH to AG, 'Sessions only informal as regards psychological atmosphere, controlled as regards fraud and error.'
February 1976	Article in <u>Psychic</u> re 'Do it Yourself' training with Paratronic equipment.
11 April 1976	AG to SH, sends report of bias of ESP-1 and errors in manual. Asks for instrumental recording of SH's randomising tests on CMG's PK apparatus.
Some time unknown	SH submits joint paper by himself to PA without AG's knowledge.
20 April 1976	SH to AG, informing her of her errors, 'why not let circuitry do job for you?'...
1 May 1976	AG to SH, rejecting arguments in letter 20.4.76.
20 May 1976	SH to AG, 'Let me assure you our ESP machine was thoroughly tested...shows no bias...Neither Dr. Mattuck nor I could find any errors in your methodology'. Accepts AG's withdrawal from paper.
4 June 1976	Report by David Chapman on PK-1 and ESP-1 - total rejection of both.
23 June 1976	SH to AG complaining of her withdrawal from joint paper.
29 June 1976	Beloff passes ESP-1 and PK-1 to Brian Millar.
July 1976	Report by Millar, ESP grossly biased and manual confusing, but PK-1 in principle suitable.
July 1976	ESP-1 returned to Ira Spector of Paratronics in U.S.
6 August 1976	Ira Spector to AG confirming bias of ESP-1 and that manual systematically cloaks defect, offers to send alternative machine ESP-2.
11 August 1976	AG to Editor of <u>Psychic</u> re his article.
10 September 1976	Editor of <u>Psychic</u> to AG, to effect that Spector has agreed to publishing correction to manual and circulating ESP-1 owners.
September 1976	Letter from Spector to ESP-1 owners.
September 1976	AG's comments on ESP-2.
2 October 1976	SH offers to sell AG further equipment.
July/August 1977	AG's paper, <u>Parapsychology Review</u>

Table 19. Brief selection of events and documents concerning random number generators supplied by SH to AG.

Two pieces of apparatus were ordered from SH in Copenhagen, one PK-1, similar to the one used with CMG and another ESP-1, said to be an all purpose ESP and PK machine.

When I sent SH a draft for Appendix 5 (see above 3.1) I received the first slightly worrying letter from him. On 26 January 1976 he wrote

Paratronics is an international concern interested in research and development in parapsysics. Our factory facility for the time being is in California...we would prefer to keep purely manufacturing enterprises separate from research since it is hard to do both simultaneously... Thanks very much for your write-up. Remember that the sessions were informal only as regards the psychological atmosphere. As regards the possibility of fraud or experimenter error etc. the sessions were quite controlled... As you become more familiar with PK-1 you will appreciate this.

There had been a side-stepping of my request for some declaration of how he was involved with Paratronics, and the repudiation of informality caused some concern. The proceedings had been totally informal, for example we had not laid down any prior protocol whether CMG was to try and make the pointer go above or below 50: the very fact that above 50 was labelled 'hitting' and below 'missing' meant that 'above' or 'hitting' was what CMG tried to do. If (as already noted in my Appendix 5) the instrument did have a positive bias, the results could be spurious. I relied on SH's figures for the randomness base line.

Meanwhile the apparatus PK-1 and ESP-1 arrived and I set about familiarising myself with ESP-1, a slightly more complex instrument than PK-1. In the process, I discovered that the apparatus was very seriously biassed indeed, and that this bias was systematically cloaked by the instructions contained in the accompanying manual. This manual was, furthermore, so badly formulated that it was impossible to follow the reasoning as to how PK experiments could be conducted with ESP-1. I conducted a large number of tests of increasing rigour, and wrote a report on ESP-1. The following is a somewhat abbreviated and tidier version, numbers, Tables and essentials being identical.

Report on ESP-1

(Sent to SH 11 April 1976)

Objective: To test ESP-1, a piece of apparatus supplied by 'Paratronics' as an ESP and PK testing machine, for suitability as a PK detector.

Apparatus: ESP-1 is a supposed random number generator such that there are four equally probable target lights (manual p. 10). There are four selector buttons, 1a, 2a, 3a and 4a and four corresponding lights, 1, 2, 3 and 4 and an on/off switch. This switch, apart from turning on the apparatus, causes a light to light up. There is also a display panel which lights up after every 10 trials to show how many 'hits' (correspondence between selector button and target light) have been scored. There is a fifth, red, 'feedback button', which gives the score in figures on the display panel at any time, if depressed.

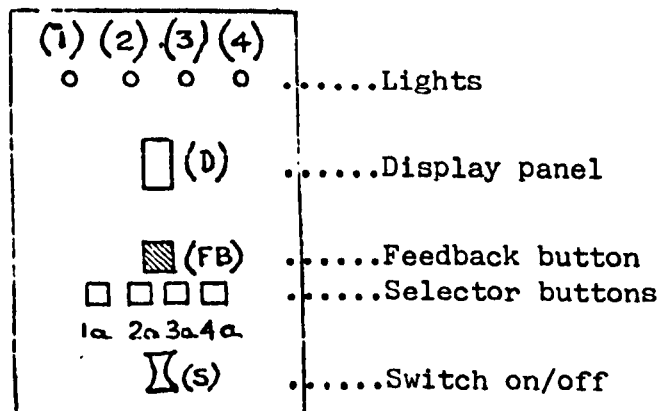


Figure 5.

According to the manual p. 11, the electronic portion of the apparatus is activated

when one of the target select buttons is pressed. It includes two randomly-phased oscillators which are used to 'mix' the targets at an unpredictable rate.

The delay after the button is pressed is a function of the random bounce associated with closing the switch. This is the mechanical element of the randomising process.

The length of time that the targets continue to mix depends on how long the operator holds the button down. This feature adds an unknown human influence over the target selective process.

Account of procedure

It will be seen that, to begin with, I seemed to obtain quite high ostensible results using the instructions of the manual (p. 14), leaving it, however, to the switch to 'decide' on which number to concentrate, i.e. if the switch led to no. 4, I would then depress button 4 and 'will' light 4 to light up.

I ran two sets of control tests for randomness as instructed by the manual, p. 15, and obtained such random results on these (CR 1.85 and CR 1.07 respectively) that I nearly began to entertain the possibility that I was obtaining positive results when 'willing' a particular light to appear. I noticed at the time, when conducting 'PK trials', that I felt as though I were actually active somehow in turning on the red light, though I felt that I was somehow 'cheating' in some unknown way. In the event, I did not get as far as conducting PK trials, over and above the first systematic attempt recorded in Table 20, since my time was fully occupied testing the randomness of the apparatus, and it soon became apparent that it was seriously deficient.

(1)	5	
(2)	4	
(3)	4	N = 100
(4)	7	MCE = 25 (1 in 4)
(5)	5	X = 42
(6)	2	D = 42 - 25 = 17
(7)	5	$\sigma = 4.33 \sqrt{100 \times \frac{1}{4} \times \frac{3}{4}}$
(8)	4	$\underline{D} = \underline{17} = \underline{3.93}$
(9)	4	$\sigma = 4.33$
(10)	<u>2</u>	CR = 3.93 (significant)
	42	

Table 20. High results obtained with ESP-1 in 1000 trials, Subject A.G.

It seemed to me that the switch constituted a problem since, in order for the score on the display diode to be out of 10, 'switching on' has to be counted as the first trial of PK.. According to the manual, p. 14, the user was instructed to 'visualise that your target is illuminated as you turn the

power on'. But how could an electronic gadget 'count' this visualisation, unless the switching on itself constituted a first trial? Furthermore, the display panel 'counts' coincidence between the button pushing time t and the corresponding light at the same time t, and not between the button pushing at time t and the target that lights up at time t + 1.

Initial trials suggested that the switch was indeed itself seriously biassed. In the course of depressing the switch just over 300 times (303), the lamps lit up as follows:

Light no. 1	Light no. 2	Light no. 3	Light no. 4
60	100	48	95

Table 21. Light no. illuminated in response to ca 300 pressings of switch.

It seemed clear that only very orderly and systematic manual recording would meet the case, and I worked out the schema displayed in Tables 22 and 23. Ignoring the display panel, I systematically recorded in writing which light was lit by depressing the switch and by then pushing each of the buttons 9 times, recording in the appropriate section which light had lit up. Each 'bout' is thus 10 trials.

	switch	push no.1	push no.2	push no.3	push no.4	Totals	
Bouts		15	15	15	15	push	push + swi.
Light no.1	9	25	28	19	22	94	103
Light no.2	24	36	40	49	48	173	197
Light no.3	8	26	32	27	27	112	120
Light no.4	19	48	35	40	38	161	180
Total	60	135	135	135	135	540	600

N = 600, MCE = 150, $\sqrt{}$ of 600 = 10.61

$D_1 = 150-103 = 47$ (below), $CR_1 = \frac{47}{10.61} = 4.43$ (neg)

$D_2 = 150-197 = -47$ (above), $CR_2 = \frac{-47}{10.61} = -4.43$ (pos)

$D_3 = 150-120 = 30$ (below), $CR_3 = \frac{30}{10.61} = 2.83$ (neg)

$D_4 = 150-180 = -30$ (above), $CR_4 = \frac{-30}{10.61} = -2.83$ (pos)

Table 22. Testing ESP-1 for randomness, results obtained in 600 button pushings and switch depressions (60 'bouts'), with manual recording, ignoring display panel.

	switch	push no.1	push no.2	push no.3	push no.4	Totals	
Bouts		10	10	10	10	push	push + swi.
Light no.1	4	13	13	15	18	59	63
Light no.2	10	30	29	23	22	104	114
Light no.3	6	28	18	22	13	81	87
Light no.4	20	19	30	30	37	116	136
Total	40	90	90	90	90	360	400

$N = 400$, $MCE = 100$, σ of 400 = 8.66

$D_1 = 100 - 63 = 37$ (below), $CR_1 = \frac{37}{8.66} = 4.27$ (neg)

$D_2 = 100 - 114 = 14$ (above), $CR_2 = \frac{14}{8.66} = 1.62$ (pos)

$D_3 = 100 - 87 = 13$, $CR_3 = \frac{13}{8.66} = 1.5$ (neg)

$D_4 = 100 - 136 = 36$, $CR_4 = \frac{36}{8.66} = 4.16$ (pos)

Table 23. Further randomness tests on ESP-1 (40 'bouts'), manual recording, ignoring display panel.

Amalgamating the results of Tables 22 and 23, in terms of lights lit up, the following frequencies are obtained out of $N = 1000$ trials:

Light no.1	Light no.2	Light no.3	Light no.4
103	197	120	180
<u>63</u>	<u>114</u>	<u>87</u>	<u>136</u>
166	311	207	316

$N = 1000$, $MCE = 250$, $\sigma = 13.9$

$D_1 = -84$, $CR_1 = 6.04$

$D_2 = +61$, $CR_2 = 4.39$

$D_3 = -43$, $CR_3 = 3.09$

$D_4 = +66$, $CR_4 = 4.75$

Table 24. Amalgamated results of Tables 22 & 23 in terms of Lights lit up.

At a later date I tried again to see if the same results would be obtained, and these are shown in the following Table 25.

		switch	push no.1	push no.2	push no.3	push no.4	Totals	
Bouts		10	10	10	10	10	push	push + swi.
Light no.1	11	13	18	18	18	18	67	78
Light no.2	10	34	33	34	31	31	132	142
Light no.3	6	17	14	18	19	19	68	74
Light no.4	13	26	25	20	22	22	93	106
Total	40	90	90	90	90	90	360	400

$N = 400$, $MCE = 100$, $\sqrt{V} = 8.66$

$$D_1 = -22, CR_1 = \frac{-22}{8.66} = 2.54 \text{ (neg)}$$

$$D_2 = 42, CR_2 = \frac{42}{8.66} = 4.85 \text{ (pos)}$$

$$D_3 = -26, CR_3 = \frac{-26}{8.66} = 3.00 \text{ (neg)}$$

$$D_4 = 6, CR_4 = \frac{6}{8.66} = 0.69 \text{ (pos)}$$

Table 25. Testing ESP-1 for randomness, further 40 'bouts' as in Tables 23 and 24.

It will be seen that in all these tables (22, 23, 24 & 25), $D_1 + D_2 + D_3 + D_4 = 0$. I then did a number of runs, depressing switches only. Results are shown in table 26:

Light no. 1	79
Light no. 2	164
Light no. 3	88
Light no. 4	<u>169</u>
	500

$N = 500$, $MCE = 125$

$$D_1 = -46, \frac{-46}{9.68} = -4.75$$

$$D_2 = 39, \frac{39}{9.68} = +4.03$$

$$D_3 = -37, \frac{-37}{9.68} = -3.82$$

$$D_4 = 44, \frac{44}{9.68} = +4.55$$

$$D_1 + D_3 = -83$$

$$D_2 + D_4 = +83$$

Table 26. No. of lights lit up in response to despressing of switch.

Switches, then, deviate from randomness in the same manner as do the button-light systems, and again deviations from MCE balance one another in systems, such that $(D_1 + D_2) + (D_3 + D_4) = 0$, D_1 and D_3 and D_2 and D_4 balancing one another precisely.

It would seem, therefore, that switches 1 and 3, and 2 and 4, form joint systems deviating equally and with opposing sign from chance.

However, since they precisely cancel one another out, an overall test of randomness, using the display diode which lumps together 'hits' on all 4 lights, as instructed by the manual (p. 15), is worse than useless, suggesting as it does to the user that the apparatus is random whereas in fact the individual buttons differ widely as regards their deviation from randomness. According to the manual:

A simple and convenient way to test the machine for chance operation when ESP is not involved is to push the push buttons in a non-random sequence, 1, 2, 3, 4, 1, 2, 3, 4...etc...At the end of each run of 10 trials, record the displayed score. Repeat the procedure...

That the machine is random overall, i.e. when all 4 lights are lumped together by taking readings from display diode, in the terms suggested by the manual, is indicated by the results recorded in table 27, the procedure used being as per instructions in the manual, i.e. using display diode, which summarises each 'bout' of 10.

I also checked on the accuracy in adding of the display panel, and there seems no reason to doubt the accuracy of the visual display feedback.

Conclusions

Overall, then, this machine is unsuitable as it stands for any parapsychological experimentation, ESP or PK, since the two systems, (1,3) and (2,4) are not equally random. However, it might be possible to correct this, presumably by suitable adjustment of the electronic system.

End of Report on ESP-1.

Randomness check as suggested by p. 15 of the manual

(1)	4	(11)	3	(21)	1	(31)	4	(41)	1
(2)	2	(12)	4	(22)	2	(32)	2	(42)	4
(3)	4	(13)	1	(23)	4	(33)	1	(43)	2
(4)	3	(14)	3	(24)	5	(34)	0	(44)	3
(5)	3	(15)	2	(25)	1	(35)	2	(45)	4
(6)	3	(16)	2	(26)	2	(36)	2	(46)	3
(7)	2	(17)	4	(27)	4	(37)	4	(47)	2
(8)	3	(18)	2	(28)	1	(38)	3	(48)	4
(9)	5	(19)	4	(29)	3	(39)	3	(49)	5
(10)	0	(20)	1	(30)	2	(40)	4	(50)	1
<hr/>		<hr/>		<hr/>		<hr/>		<hr/>	
29		26		25		25		29	

N = 500

MCE = 125

D = 134-125 = 9

$\sigma = 9.68$

$CR = \frac{9}{9.68} = 0.93$ not significant

Table 27. Results obtained when testing ESP-1 for randomness in accordance with instructions from manual, using digital diode.

SH sent me a copy of a projected 'joint' paper by himself and me, about the CMG experiments, to be submitted to the next Parapsychology Association Conference in Utrecht, August 1976. Unfortunately without further consultation he also, at some time not exactly known, sent this paper to Prof. Martin.

Johnson for the conference. It contained (among others) the somewhat surprising passage that 'AG is now investigating her own scoring on another PK-meter. To satisfy Walker's criteria... to guarantee no experimenter PK effect, neither the subject, nor the experimenters may ever be notified of the score! (also after the experiment!).'

Meanwhile I had completed the above Report on ESP-1. I sent this, together with photocopies of my handwritten notes to SH on 11 April 1976, thanking him for his paper but

Before commenting on this further, I feel I ought to send you the Report on ESP-1 since in my view the status of the instrumentation used should be absolutely clear before we proceed...As you will see, my view is that (a) the manual (b) the claim that ESP-1 can be used as a PK machine as it stands, should be withdrawn; (c) a new manual giving i.a. proper instructions for randomising should be issued and suitable publicity given to the inadequacy of the old instructions (d) if the defect in randomising is inherent in the machine (as opposed to being an accidental defect easily remediable...) then the machine itself should be withdrawn.

Have you got some sort of instrumental recording of your randomising tests with the PK machine we used for CMG? And to what extent are you responsible for the design and manufacture of the apparatus? (I am only raising the sort of queries which others, less friendly to our subject than I, are bound to raise, especially if fault can be found with the manual, the apparatus and the sales campaign, e.g. the article in the current Psychic by the Editor, [who is] basing his success at PK enhancement on having followed the manual.)'

SH's response to this was a letter dated 20 April 1976, accusing me 'primarily of two errors'; of not reading the manual properly, and mis-counting the number of hits per trial. He expressed bafflement why I should have 'hand-counted' each light separately.

Why not let the circuitry do the counting for you automatically, which will decrease the possibility of making a recording error?...We suggest to our customers who have the statistical training to carry out a serial test of randomness....However since this requires an on-line computer we do not mention this in the manual [sic] ...Please don't be discouraged! Statistics is a tricky business, and electronics even more so...

I responded on 1 May 1976, thanking him for his 'somewhat

astonishing' letter, 'I do not feel you have met any of my points about the bias of ESP-1', explaining that my notes only showed how I came across the bias, and only then how I tested for it, and that the instructions in the manual systematically cloaked, and were bound to cloak, the very bias I had unquestionably discovered. I told him that the apparatus would be examined at City University under Prof. Ellison's supervision, and then at Edinburgh under that of Dr. Beloff. '...we will have to leave it for the time being. Nor can we in the circumstances write a joint paper...'

On 7 May 1976 SH rang me up claiming not to have received my letter. The conversation was exceedingly disagreeable (my account was sent within a few hours to Prof. Ellison and Dr. Beloff). SH simply refused to take my points that ESP-1 was biased and that the procedure described for testing randomness described in the manual systematically disguised the very bias I had found. He kept reiterating 'but our machines are hardly ever biased', 'but you have no business to criticise the machine if you didn't apply the proper testing procedure as laid down in the manual', 'but it is a simple matter of mathematics and electronics, and you know nothing of either'. There were eight or nine variations on this theme, and he demanded that I should refrain from showing my Report to anyone. He then told me that he had sent a joint paper in his and my name to Prof. Johnson in Utrecht for the forthcoming conference on the grounds that there had been no time to consult me. I informed him that I did not now wish to write any joint paper with him. He expressed pained amazement: 'You aren't taking all this seriously, are you?' I assured him I was: and that the machine, manual and Report would be examined at City and Edinburgh Universities.

On 20 May 1976 I had a very different letter indeed from SH, saying that he had now shown my 'notes' to Dr. R.D. Mattuck, and he apologised for the 'tactless phrasing' of his last letter which had pointed out 'some errors in the first part' of my Report. He had not intended to be insulting. He now agreed that I had quite rightly pointed out that a test of the frequency of each lamp should be

carried out, and that this was 'an oversight which will be corrected...Let me assure you that our ESP machine was thoroughly tested, but if an electronic component had failed' the machine would be replaced. He hoped I would help with the wording of the next manual. 'We are always interested in constructive feedback and look forward to receiving any suggestion you might have...' He had, as requested, withdrawn my name from the 'joint' paper.

Mr. David Chapman, then Research Assistant to Professor Ellison, completed his report on ESP-1 on 4 June. In his view ESP-1 was 'a party-toy not deserving serious consideration. I am also inclined to believe that it is calculated to mislead users into a belief that they have extrasensory ability'. He pointed out that I (AG) seemed to be a 'little confused about the switching arrangements'; I did not appear to realise that 'ten selections were made, although only nine pressings of the white buttons in a complete run initiated the selection. This is really because the manual doesn't make it abundantly clear that the on/off switch, as well as the white [selector] buttons, has a dual function'. He agreed that the machine was unsuitable as a PK machine.

Brian Millar's report confirmed the gross bias of ESP-1. He too agreed that the machine did not disentangle PK from other possibilities. However, he also thought my reasoning about switching and button pressing arrangements was faulty, although his grounds and views were different from those of Chapman (who had assumed, as had I, that switching-on counted as the first trial). According to Dr. Millar my error was due to the fact that the manual had omitted to mention that the white selector button should be held down before the power was switched on! I was (and am) inclined to accept Dr. Millar's suggestion here, although the lack of clarity in the manual did not mislead just me, but evidently David Chapman also. This does not however make the slightest difference to my assessment of the randomness of the instrument, or the rest of my reasoning as regards its suitability as a PK detector.

Eventually SH's paper about the CMG experiments was accepted as a Research Brief omitting as requested all reference

to CMG's or my names. In an Appendix to the Brief he provided a Table (his Table 2, my Table 28), giving details of randomness trials alleged to have been conducted on the same machine (a PK-1) as that used with CMG. This, even if taken at face value, would appear to suggest a less than ideal distribution for providing a random base line.

Date	n, trials	Hits	Deviation from MCE	Exp.	Recording
13.10.75	7000	3463	-37	SH	meter/mag tape
25.10.75	3000	1506	+6	SH	meter/X-Y chart
25.10.75	3600	1878	+78	SH	meter/X-Y chart
25.10.75	1500	763	+13	SH	meter/X-Y chart
25.10.75	3000	1516	+16	SH	meter/X-Y
25.10.75	4000	2008	+8	RØ SH	meter/X-Y
22.10.75	2600	1346	+46	RØ SH	meter/X-Y
22.10.75	2600	1306	+6	SH	meter/X-Y
17.10.75	3600	1841	+41	SH	meter/X-Y
17.10.75	3800	1930	+30	SH	meter/X-Y
Subtotal	34,700	17,557	mean % = 50.5965 (i.e. after table 1 exp.)		
15.2.75	2000	980	-20	IS	meter/X-Y
15.2.75	2000	994	-6	IS	meter/X-Y

Subtotal before exp. in table 1

4000

Grand total: 38,700 19,531 mean % = 50.46

Table 28. 'Randomness trials with the same machine' as with CMG, Table 2 in Appendix to SH's Research Brief. His Table 1 contained results obtained with CMG.

Paratronics agreed to replace ESP-1 and (after prolonged correspondence with the Editor of Psychic, who declined to publish a letter from myself on the topic) to inform all purchasers of ESP-1 in September 1976:

It has come to our attention that the randomness tests in the ESP-1 Testing Machine Owner's Manual is not an exhaustive test, particularly when using the instrument in formal research. For example, if a failure were to occur in the random number generator such that one or more buttons were 'biased' in one direction, while

the remaining buttons were biased in the opposite direction by an equal amount, the user would not be able to detect this occurrence using the existing randomness testing procedure...

An additional randomness test is then described involving pressing the same button 10 times, which seemed satisfactory provided the summing-up circuitry worked. It is another matter whether users would think it at all probable that buttons were liable to be biased by precisely the same amount in different directions; or for that matter whether the new test is any less important in non-'formal' research, in the course of which people (such as the Editor of Psychic) might perhaps be supposed to be more predisposed to being misled into believing in positive results!

As regards the other PK instrument supplied, a PK-1 (similar to that used with CMG), Chapman and Millar disagreed; Chapman thought it unsuitable on engineering grounds, but Millar considered it acceptable as it appeared to be random. I am inclined to agree with Millar, in that the machine did seem random to an acceptable extent.

I asked for another piece of apparatus from Paratronics to replace ESP-1, as offered by Ira Spector of that firm, a rather simpler instrument called ESP-2, said to be suitable for formal and informal ESP and PK work, and Mr. Spector wrote congratulating me on my selection. The following is my Report on ESP-2.

Report on ESP-2

ESP-2 consists essentially of the following:

2 target lights (1 and 2), 2 buttons (1 and 2), on/off switch, various sockets and movable screen.

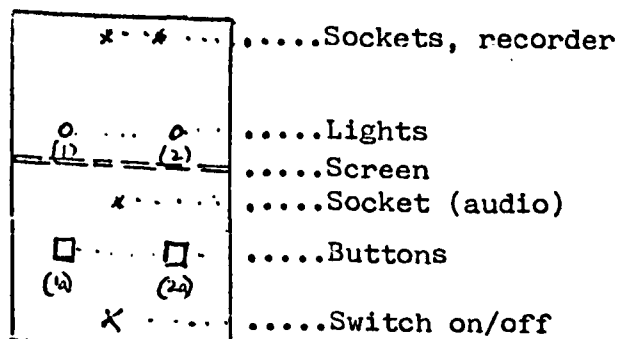


Figure 6.

Operation is as follows: In the case of ESP experimentation, the subject tried to guess which light is on, while screen is interposed. If light 1 is on pressing button 1 will be a hit, pressing button 2 will be a miss; when light 2 is on, pressing button 1 will be a miss, button 2 will yield a hit. By means of the bleeper attachment, audio-feedback trials (the only really satisfactory ones for this type of experiment if conducted on one's own) the subject can get immediate information when he has pressed the correct button: 2 bleeps for 'hit', 1 bleep for 'miss'. This can be recorded, and this would appear to be quite satisfactory for clairvoyance and telepathy tests (in principle, that is, provided the instrument is random, sensory cues have been excluded etc). In any case it seems to me quite adequate as a trainer and informal apparatus.

ESP-2 is supposed to be designed equally for PK experimentation, and here problems arise when the apparatus is used with audio-feedback and with recording. The reasons for this are as follows.

The signalling 'hit' and 'miss' arrangement has already been described. When light 1 is on and button 1 is pressed, the signal given is 'hit', when light 2 is on and button 1 is pushed, the signal is a 'miss' etc.

Now suppose our subject is trying to get light 1 to light up for the next n trials. Suppose the switch has indeed lit up light 1:

Subject presses button 1, light 2 comes on, but the signal is 'hit'. [Because the machine is organised so as to signal a hit if the button pushed is the same as the light now on.] The subject has, in fact, missed, but is given the signal 'hit'.

Subject continues to press button 1, wanting light 1 to come on. He is now doomed to receive a 'miss' signal, even if light 1 lights up, since the machine is organised to signal miss if a different button is pushed from that of the present light, irrespective of what happens with the next light.

Thus, subject has (a) 'missed' but been given the signal 'hit' and (b) 'hit' but been given the signal 'miss'. This is

bound systematically to confuse the Subject.

An example of a run of 10 showing how confusing this would be is given below in Table 29. At the beginning, Light No. 1 is on. The Subject now presses button 1 10 times.

Trial no.	Button no.	Light no.	Signal*	How transcribed into 'hit' and 'miss' from magnetic tape
1	1	2	d	'hit'
2	1	2	s	'miss'
3	1	2	s	'miss'
4	1	1	s	'miss'
5	1	2	d	'hit'
6	1	2	s	'miss'
7	1	1	s	'miss'
8	1	1	d	'hit'
9	1	2	d	'hit'
10	1	2	s	'miss'
3 hits				4 hits out of 10
7 misses				so far as record
i.e. 3 out of 10				goes!
or 4 out of 11				

* s = single bleep, d = double bleep.

Table 29. A run of 10 PK trials with ESP-2, showing how Subject would be confused by auditory feedback.

The auditory feedback signal always refers to 'past', i.e. immediately preceding hits and misses, i.e. to the (nth-1) trial: but of course psychologically the Subject needs feedback about how he has done on the current trial. Since in a PK test the Subject sees the display, he already knows whether he has 'hit' or 'missed' from visual feedback: hence audio signal is redundant as a signal of past success and misleading about ongoing effort: auditory and visual feedback conflict in the most confusing manner possible.

As regards the magnetic record, although the above considerations make an appreciable difference in the short run, out of 10, this would make relatively little difference in a longer run taken globally, e.g. out of 100. One could cope with this by, for example, adding 1 to the n trial numbers for

calculation purposes.

However, since it is clearly undesirable to feed back the auditory information, however softly, a magnetic recording is not advisable. In my view, therefore, although the apparatus could be suitable for informal PK testing with visual feedback only, where no record is desired, and for self-training purposes, it is not useful for formal PK experimentation where an instrumental record is essential.

General comments on ESP-2:

The machine would appear to be random over 500 to 600 trials. It seems suitable for informal clairvoyance or telepathy tests, and for training, though somewhat cumbersome, provided it is random in the long run and test design etc. are adequate. (However, the screen is not adequate for precluding visual clues in more formal experimentation.)

It is, again if suitably random, possible to use it as an informal PK tester with visual feedback only, largely for self-training. It is quite unsuitable for use in PK work with auditory feedback, and for any formal work where a permanent magnetic record is essential.

End Report on ESP-2.

These comments were completed in September 1976 and sent to SH who, on 2 October, wrote to me, offering to sell me yet further equipment and attachments to ESP-2, which were declined with thanks.

Eventually I published a rather brief paper in the Parapsychology Review (1)*. The article contained, in addition to the principal points made in more detail in this chapter, the argument that

The suitability of a device such as ESP-1 for testing PK might in any case be questioned since, even if it were random, [according to the manual] 'The length of time the targets continue to mix depends on how long the operator holds the button down'. I would therefore be inclined to regard it as, at best, a primarily cognitive ESP-type task, 'knowing when to let go' rather than 'influencing the apparatus'.

* Appended to this thesis (Label B)

I concluded with the paragraph:

Attractive, light and reliable equipment for the purpose of ESP and PK testing and training are highly desirable. It would, however, be a regrettable error to suppose that the availability of commercial equipment will automatically lead to widespread and successful amateur parapsychology. Amateur activity in this field is, in my view, very much to be welcomed. However, where difficult technical questions such as randomness are concerned, very high standards for assessing of evidence are needed for the experimenter's results to be of any value.

3.3 Concluding observations

The episode highlights a number of problems: the desperate need for and lack of, suitable, reliable, light and fairly cheap equipment that is readily available commercially; difficulties in collaborating between persons of different backgrounds and interests; the ways in which frankly commercial considerations may affect promotion and presentation of results and equipment.

It is impossible to assert definitively whether or not positive results were originally obtained with CMG. My own strongly held view is that they were not, and that the instrument exhibited by SH and used with CMG was seriously biased. There could - given SH's cavalier attitude to questions of randomising - easily have been errors concerning his tests of the (allegedly) same machine about which, too, there might have been error. His attitude towards tests of randomness became clear as a result of queries concerning another machine (ESP-1), but it would have been very inadvisable, once this attitude had become apparent, to accept without further ado his claims that the PK-1 we had used was indeed adequately random. The fact that CMG never again obtained any positive results on the new instrument presents a feature that is more part and parcel of the investigation of the paranormal in general, the problem of non-replicability: in this instance it seems to me to support a reasonable belief that there had been nothing to replicate, and reinforces an important point made by West (2) concerning the relatively greater danger of the promulgation of 'bogus findings' in parapsychology than in other subjects.

Now the fact that SH was in effect a sales representative for an international firm itself does not disqualify him as an experimenter, but it constitutes a factor that at least has a bearing when issues such as the reliability and suitability of machines sold are involved. His determination to clear ESP-1 in the face of the plainest evidence until confronted, first by Mattuck, and then Chapman and Millar in addition to myself, suggests a certain unwillingness to face the academically unpalatable. The entire correspondence reflects attitudes more like those of a consumer's protest in the face of a supplier's claims and evasions, rather than a scientific interchange. The issue of Psychic containing the Editor's article enthusing over his own PK enhancement with the help of Paratronic's equipment also had, on the back page, a large advertisement for the products of the same firm.

It is true that Paratronics eventually offered to replace the machine and revise the manual. But meanwhile a quite inordinate amount of time and energy had been consumed in the matter, and research time is a very precious and scarce commodity in a field where hardly anyone works full-time.

I believe this issue also illustrates one of the important and deleterious consequences of the fact that parapsychology is not established scientifically, and that those engaged in it may have to make their living in all sorts of possibly unusual ways, and this may result in rival and incompatible socialisation. This in its turn will militate against the establishment of the subject.

A problem that might have arisen (though it did not) is that of the effect on the experimental Subject. As it happens, CMG is an unusually well balanced and robust young woman, who never quite believed in her own success, whilst happily basking for three or four days in the role of 'Psychic Superstar'; also she had grown up in a setting where a dispassionate, provisional, somewhat light-hearted and (I hope) balanced attitude towards any claims to paranormality is part of the atmosphere. Yet it seems to me I can detect to this day a certain understandable echo of disappointed if jokey irritation at my refusal eventually to accept the results as positive. It takes very little imagination to envisage

the damage, temporary or permanent, that might have been caused in an unprepared Subject from an unsophisticated milieu, starting possibly with quite acute sibling rivalry, continuing with conflict, culminating in a crisis of self-esteem.

At any rate, I decided to abandon attempts to search for special Subjects in the population at large, and instead to try to find special Subjects who had already given some signs of PK ability, and then to try to capture larger-scale phenomena than those reflected in Random Event Generators.

CHAPTER 4, THE ENFIELD CASE

4.1 Sources of information and procedures

As it became known in psychical research circles that I was looking for Subjects, either physical mediums or 'foci' of poltergeist activity who might be investigated for PK effects, especially by means of infra-red equipment, various cases were brought to my notice including one at Enfield. In addition to personal communications which are referenced accordingly, the present chapter is based on the following sources of information.

4.11 Case notes

I wrote notes on all my visits except unfortunately the first, a social introduction. I also made notes on various incidents, conversations and reactions on my part.

One purpose of keeping such contemporary notes was to capture as quickly as possible impressions and observations, so as not to succumb to too much subsequent secondary elaboration and possible 'fading of conviction' and denial of what I had experienced at the time, since this is believed by many to be a hazard of testimony in this area. There is also a widespread belief among those engaged in psychical investigation, and there is some slight evidence to support it (see for instance Chapter 2), that the observers' or experimenters' or participants' moods, attitudes or beliefs may significantly affect the course of events.

It seemed to me that, as a first step, however imperfect, explicit attention should be paid to at least my own subjective reactions as objectively as possible. I therefore decided to obtain the type of support, counselling and guidance usual in the supervision of social work cases, in psychotherapeutic training situations, or, although less analogous, in teaching practice placements where, however, full and systematic notes are deemed of special importance for self-evaluation. In the present context it was hoped that, by creating for me a documented, quasi-clinical and supervised situation, this would help to improve the quality of my observation and testimony,

clarify my own reactions towards issues and participants, throw some light on the stresses encountered by field investigators which are in general largely disregarded or treated in terms of stereotyped generalities, and to form some assessment of what in fact was, or seemed to be, my own personal contribution to the situation.

Mrs. F.M. Branch agreed to be my 'case supervisor' for this purpose. She is a practising psychotherapist and counsellor, formerly Principal Psychiatric Social Worker at Guy's Hospital. She has extensive experience of teamwork in training, treatment of clients, and in the supervision of psychiatric social workers, and has a specialist knowledge of the problems of gifted and exceptional children, having been a president of the National Association for Gifted Children. She has been particularly concerned with the problems encountered by actors, artists, musicians and writers. She has no experience of psychical research, but is open-minded though rather critical in her approach to the field. Although she is a member of the Society for Analytical Psychology and her general theoretical inclination is towards Jungian psychology, her approach is non-doctrinaire, eclectic and commonsensical. I was also fortunate to be able to draw on her extensive practical experience with disturbed children and family situations.

My notes on the case, approximately 60 typed pages consecutively numbered, were written at three levels. The first was fairly impersonal and descriptive, merely giving an account of what happened; the second, not always wholly distinct from the first, included my own subjective feelings and reactions so as to give subsequent readers, as well as myself, an opportunity to make allowances for my own reactions to the situation, provided as honestly as I could. The third concerned only more personal, quasi-self analytic material which might have a bearing on Enfield issues, and my reactions to them. Notes at level 1 and level 2 were regularly circulated to Mrs. Branch, Dr. Beloff and Prof. Ellison, level 3 notes to Mrs. Branch only. They were written, typed and circulated as fast as possible, normally within 24 hours. The objective in such rapid circulation was to make it possible at later dates to establish what I had claimed at the time, should I subsequently be accused

of fabricating or falsifying evidence at a later stage. This turned out to be a wise precaution.

4.12 Dossier of the SPR Enfield Committee

The Council of the Society for Psychical Research on 25 May 1978 voted in favour of setting up a Committee - Enfield Poltergeist Investigation Committee (EPIC) - to assess the happenings in this case. The result was a report and a dossier, some of it confidential. Various persons approached gave their views in writing, and the Committee also conducted some enquiries of its own in addition to accounts of visits paid previously by individual members of the Committee. This dossier has been used as a source of information and cross-checking of data, appropriate reference being made. The dossier also contains a joint paper by G.L. Playfair and M. Grosse on their views and experiences in the Enfield case up to 10 December 1977, and this has occasionally been used as a source of relatively neutral information, especially where this could be cross-checked.

4.13 Published sources

G.L. Playfair wrote a book on the Enfield case called This House is Haunted (1) which I reviewed in the Journal of the Society for Psychical Research (2)*, a review that was the subject of further correspondence in the Journal (3, 4, 5)*. After a lecture I delivered to the Parapsychology Foundation in New York in November 1981 in which the Enfield case was mentioned and which was published in Parapsychology Review (6)*, there was a further exchange of published correspondences (7, 8, 9, 10). These controversies are relevant to an assessment of the case. Playfair's book, however, as pointed out in my review is confusingly written, and has not been used as a source of information concerning facts.

Pseudonyms are used for surnames of family relatives and neighbours.

* Attached to this thesis (Labels C, D, E, F & G respectively)

4.2 Outline of events

On 31 August 1977 raps and other disturbing phenomena were experienced in the council house of the Harper family, composed of the mother, Peggy, two daughters, Margaret age 13, Janet 11, and two sons, Johnny 10 and Billy 7 (11). The police were eventually called in by a neighbour, Mr. Vic Norton, as no source of the noises and footsteps was discovered (12). The police arrived including Woman Police Constable Heeps, who according to a statement quoted by Grosse and Playfair witnessed ostensibly uncaused movements of objects (13). The police left, apparently having been unable to explain what was happening. \ So far from dying down, disturbances continued and Mrs. Peggy Norton on 5 September 1977 called up the Daily Mirror (14, 15). At least two reporters were present on 19 September 1977, whilst apparently paranormal movement of objects was occurring (16, 17). At some point fairly soon, Mr. George Fallows from the Daily Mirror rang up the SPR, and the Secretary, Miss O'Keeffe, suggested to Mr. Maurice Grosse, a member who lived not too far away from Enfield, and who had been keen to become involved in practical investigation, that he should look into the case (18, 19). Mr. Grosse went to Enfield and soon afterwards at a meeting of the Society, appealed for help as he was inexperienced and felt unable to handle the situation by himself (20). Mr. Guy Playfair, another member, a journalist experienced in following up and reporting spontaneous cases, responded to the appeal.

Grosse and Playfair took over the case and spent many evenings with the family. They enumerated the phenomena claimed at Enfield as follows (21): percussive sounds, throwing of small objects, movement of furniture, opening and closing of doors, interference with bedclothes, appearance of liquid and solid substances, apparitions, levitations of persons, physical assaults, matter through matter, psychological disturbances, automatic writing and drawing, automatic speech, disembodied voices, equipment failure, and outbreaks of fire.

Grosse in particular became friend, confident and protector

of the fatherless household, and his kindness, patience and practical generosity have never been in doubt. He and Playfair became convinced, not only of the genuineness of the case, but of its epoch-making importance in the annals of psychical research. Numerous persons were invited to the household, normally in the evenings, to witness what was going on. There is no full register of persons who called, although the SPR dossier contains the names and some accounts by those willing to reply. I know of several who were there but refused to provide any testimony as they wished to avoid the animosity likely to be aroused by adverse comment.

My own first visit took place on 18 September 1977, at the suggestion of Guy Playfair (22). Unfortunately I made, or else kept, no notes of the occasion. According to my subsequent recollections, as I entered the house there was a loud slap followed by louder howling on the part of a small boy: Mrs. Harper had cuffed Billy's face for breaking something or other. I was introduced to those present: Margaret, Janet and Mrs. Barnes, sister-in-law of Mrs. Harper. The TV was on at high volume throughout, and once Billy had calmed down I was made welcome and chatted freely to all including Billy, who had so serious a speech defect that I found him hard to understand. My only specific recollection was a description by Mrs. Barnes of how a book rose straight in the air, travelled along horizontally, and plummeted down vertically, and that 'Leggo' bricks and marbles had flown about without being thrown by human hand.

Two features were uncharacteristic about this visit: on no other occasion did I ever see Mrs. Harper even quite mildly self-assertive, let alone punitively aggressive; and I never saw Mrs. Barnes again.

According to Playfair paranormal events began to spread and to escalate, taking place not only at the Harper's, but at the Norton's and Barnes' also, in general however, centering on the Harper family, and normally (if that is the right expression) on the Harper house. As early as 1 October Playfair said he saw no reason why the case should not now be 'stopped' (23). But despite the introduction of a medium on 3 October (24), the

case showed no sign of dying down.

On 6 October there was a meeting at Janet's school, because it had become clear that her attendance, work and welfare were suffering from the strain. Among those present were the headmaster, officials from the local authority education and welfare departments, a child psychiatrist, Dr. Alan S. Clark, and Mr. Maurice Grosse. Dr. Clark, who had previously been a member of the SPR, rang up the Society afterwards and asked if it would not be possible to withdraw Grosse and Playfair from the case. He was deeply concerned both about the impossibility of helping the family in the climate they created, and also for the sake of the SPR's reputation (25, 26, 27). This was of course not within the powers of the SPR.

Playfair wrote that on 15 October he witnessed a series of very major episodes (28) and on 16 October Mr. L.E. Berger (29) still found the case 'active'. On 24 October, in the middle of the night, a heavy metal fireplace was ripped out (30) which not unnaturally caused some alarm. The family were sent by Enfield Council for a holiday in Clacton over half-term from 24-29 October.

After their return Grosse and Playfair initiated a new phase, adopting a strategy allegedly advocated by Prof. J.B. Hasted by personally addressing 'the entity' responsible for the events, so as to lessen the energetic input by enhancing communication (31).

10 November was Janet's birthday and, as I found out much later (32), the day on which her father remarried. Two days later a visit took place by three senior and experienced psychical researchers, Dr. Bernard Carr, Mr. A.D. Cornell, and Dr. A.O. Gauld (33, 34, 35, 36). These gentlemen's statements suggest that they were not persuaded that anything they had seen required a paranormal explanation, but they seem to have parted from the family on amicable terms, the usual exertions being made to keep them there as long as possible. Nevertheless, according to Playfair, Mrs. Harper bitterly complained of their conduct afterwards, and demanded that they be excluded from her house in future.

On 15 November Mrs. Harper had reached such a state of

exhaustion that the children were taken into care (37). On their return, Janet and Margaret both went into violent trances, screaming loudly and their convulsions were so alarming that on 28 November Mrs. Norton actually called an ambulance to take them to Chase Farm Hospital (38). They were however not kept in, and Playfair introduced a young Brazilian 'spirit painter', Luis Gasparato, and his mentor, Mrs. Elsie Dubugras, a spiritual healer (39). The lady seems to have exercised a calming influence on the girls. But by 1 December Janet and Margaret were said to have 'joint dreams' supposed to have been 'witnessed' by Berger and J. Friedman (40).

On 10 December 1977 Guy Playfair, John Beloff and I went to visit the Harper household (41, 42, 43, 44). The following is an extract from my contemporary notes:

...no phenomenon ever happened in John's [Beloff's] or my actual presence. However, Guy and Maurice were satisfied that there had been plenty of phenomena, and that clearly John had not lived up to his reputation as a universal inhibitor.

...Billy was quite boisterous and seemed happier [than previously] ...The two girls recognised me and were friendly and forthcoming.

The atmosphere was easy and informal, the TV going... nothing happened apart from the whistling. We were told by G and M and also the [mother's] brother (Barnes) that things always happened at 25 to the hour. I paid special attention but nothing happened at 25 to the hour at any time.

Eventually the children went to bed, all three in the same room, Billy sharing bed with Margaret, leaving one empty bed...M, J, G and I hopping about outside bedroom...we were told nothing ever happened when anyone was looking, so Maurice would pop into bedroom, would pop out, nearly shut door, having addressed p[olter] g[eist] (pg).

Every now and then there would be a thump, and Janet would be sitting on the floor giggling, claiming to have been thrown out of bed. Whenever I tried to look in through a crack in the (ajar) door, I could see Janet, usually looking straight at me. M and G both assured us she didn't see us, but the poltergeist did. Janet was invited by Maurice to jump out of bed, she said she couldn't, just couldn't - no idea what he meant. When [encouraged she] would give a much feebler leap than what landed her on the floor in our absence - allegedly paranormally. My personal impression is and was that she jumped out of bed at all times in a perfectly normal manner, perhaps making the loud thump afterwards.

I had a long talk with Mrs. H's brother. He said

his sister Peggy hates her husband, who used to treat her abominably. She put up with everything...now divorced... He still comes to the house every Saturday to bring money... Billy's speech defect attributed to being shouted at by father...

I suggested to Maurice that instead of one unsatisfactory father the family now had three virtually resident males dancing attendance. He said he saw the 'Freudian' drift of my comments. I said they weren't particularly Freudian. I also said that, in my view, on any interpretation whatever, it was undesirable for children to stay up in the early hours of the morning, night after night, whilst grown ups danced attendance...

G and M both protest that they wish to end the phenomena; and yet everything in their behaviour other than some thin dialogue with the poltergeist contradicts this. Guy goes into the children's room, reads the pg the riot act informing him that he is dead and telling him to go away...Maurice loudly recites Hebrew prayers for the dead...It is plain to me that...neither want 'him' to 'go away' and the pg knows it. (In my honest opinion the pg is a pretty deliberate creation of Janet's, a sort of pretend-person, at least on the evening when we were there.)

This evening for the first time the pg talked in a low, hoarse voiceless whisper. When asked questions like 'What is your occupation?' he replied 'f... off'... He was also quite engagingly rude to me, answering 'yes' to the question whether he was enjoying himself!...

...M and G are both in my view so uncritical as positively to invite every possible kind of nonsense and deception. Neither of them seems to appreciate that to ask the children to imitate the feats they may be thought to have brought about by normal means is hardly an adequate control!...[to the children this must seem a] simple-minded prank which is quite incomprehensibly taken seriously by adults.

M and G both base their faith in the reality of the phenomena on the supposed impossibility of a normal imitation of the 'feats' and this they re-iterate over and over again. Although there was not [that evening] ...anything remotely resembling evidence for genuine phenomena, both talked as though only the most hardened sceptic could possibly wriggle out of a belief in the reality of the phenomena of that evening. This unfortunately to my mind casts doubt on their reliability as witnesses on other occasions when better phenomena are said to have occurred.

M said he had told the children that if he caught them faking just once he would never come back. But Joe Friedman...told me he understood the children had quite definitely been caught several times...

It is in my view quite impossible to come to any definite conclusion about the case other than that the whole episode seems unwholesome - with the possible exception of its effect on Billy...

(a) there may have been remarkable psychokinetic effects on other occasions; but the extremely uncritical attitude of MG and GP seems to me to cast doubt on their reliability

as interpreters of experiences.

(b) the children are enjoying themselves; and the mother is ostensibly suffering a great deal;

(c) the attitudes of M and G make me, at any rate, far more tolerant of the stance of the total sceptic...an evening at the H's could complete one's total disbelief, and contempt for all who dabbled in such matters;

(d) there is a very noticeable unanimity...among the two resident researchers and the brother. All keep insisting on the 'impossibility' of the children's simulating the phenomena. Whether this is generalised from what was [on another occasion] genuinely inexplicable to what is plainly all too easily explained, or a general credulity at all times, is not clear...GP strikes me more as a 'believer-in-any-case', MG and brother as possibly conforming to the other hypothesis.

The passage from my notes (what I have called '2nd level' type notes) is given at such length because it is fairly representative of my experiences in the course of this case, and lays the foundations for some of the reflections concerning testimony and authenticity to be discussed below. Also, another and highly respected psychical researcher was present, whose impressions closely resembled my own. What was most striking was that the supposed 'phenomena' were never actually observed by outsiders, including Grosse and Playfair, and yet both behaved as though plainly paranormal marvels were going on.

At Playfair's insistent demand, John Beloff and I wrote a joint statement (45), much less detailed than the above notes (46) and more formally worded, but to much the same effect '...at no time had we been in a position to vouch for the paranormality of any of the incidents that took place during our visit'.

The following is part of a copy of a letter to Beloff from myself, dated 16 December 1977:

...Just now had a 'phone call from Guy Playfair who is distinctly displeased with our report...this is a more or less immediate reconstruction.

GP demanded that I should modify the statement in the light of his and MG's subsequent experiences; naturally I told him this was quite out of the question, indeed a very wrong demand of him to make. I said that if I had witnessed something more impressive, I would not hesitate to say so: meanwhile, I had not seen a single thing that impressed me as genuine.

He then claimed that on the following day they had obtained quite conclusive evidence and had taped up Janet's

mouth whilst the voice was heard. Would that satisfy me? I asked 'What about Margaret?' Ah well, no, it couldn't possibly be her! Would I (AG) not look a fool when the most eminent laryngologist in England gave it as his testimony that the voice could not possibly have been that of Janet? G suggested he had such evidence. However, when I asked for names only those of Dr. Ian Fletcher and Prof. John Hasted were mentioned; but when I asked point blank for the name of the expert and asked to see the evidence which would make me look such a fool, Guy more or less accused me of bullying him to produce evidence at break-neck speed when the phenomenon of the voice had only just started...

GP also took me to task for talking for hours in the other bedroom rather than investigating 'the phenomena'. I showed a certain amount of temper and said I was not going to be dictated to by him how I was going to conduct an investigation. I was presumably there as having some sort of professional competence, and in my view getting the background from another member of the family, especially such an apparently responsible witness as John B...was at least as important as dashing into a room, addressing a 'poltergeist', dashing out again, and dashing in once more when there was a thump or a hoarse barking. GP said he could do the scientist as well as anyone... 'Don't you do an SPR on me!'...

The telephone conversation lasted about 1½ hours, I could not get GP off the 'phone - a phenomenon you experienced yourself. A friend* [reported my] ... phone out of order...

My next visit took place ten days later, and was no more memorable as regards genuine phenomena (47, 48). Dr. Hugh Pincott was present, as was Johnny, the eldest brother home from boarding school.

The family were delighted to see me, especially Janet, who made me a present of one of her duplicate stamps. She quizzed me about my religious beliefs. Did I believe in God? Sometimes yes, sometimes no. She said she always did. Maurice, she told me, was a rabbi. What did I do? I was a teacher. Did I shout at my class? I said no, I used to, but I learned one didn't usually have to do that. Anyway, my pupils now were older, one never had to shout. How old? 20, 21 or so. Our teachers shout at us all the time! Janet said. I told her I could shout very loud, and we all took to howling at the tops of our pretty considerable joint voices. We created a fine racket, Margaret, Janet,

* Mrs. Branch

Billy, Johnny, all quite happily making all the poltergeist noises and demonic barkings that were supposed to be PK phenomena. Janet said she liked chocolate. Did I like ice cream? She was a vegetarian except for beefburgers. She did not like 'that sole minoo' Guy had ordered for her when he had taken her out.

According to Pincott he had, during a recent visit, been put in a somewhat unique position of explaining the facts of life to a departed spirit who had apparently led a somewhat sheltered existence during his lifetime. Pincott seemed to imagine that he would have to plead for me to be admitted to the children's bedroom - the reverse turned out to be the case. I was admitted to the sanctum, now occupied by the mother, the two girls and Billy, provided I hid my head under the quilted dressing gown hanging on the door, with my back to the children in their beds. Pincott was eventually allowed to remain after pleading by me, crouching with his face to the corner of the room. By this time the 'poltergeist voice' was a feature. 'He' called himself first Bill, then Adam. The dialogue was roughly as follows:

'pg' : I know your name

AG : What is it?

'pg' : Anita

AG : What's my second name?

'pg' : Gregory

AG : What is my job?

'pg' : Teach

AG : Teach whom?

'pg' : Children

AG : No

'pg' : Older pupils of 21...I like ice cream, and jelly and chocolates...you've got your hair different!

AG : How different? [I had had a perm, which I felt to have been a minor disaster.]

'pg' : All curly

AG : Isn't it dreadful?

'pg' : No, different!

Whenever I swivelled round and emerged from the quilted robe (which I did quite frequently and as fast as I could) Janet

always had the sheet over her mouth.

Various alleged 'physical phenomena' happened. The curtain over Janet's bed was dragged off in stages. Slippers were supposed to jump off Janet's bed, and were shied at my head, as was a cushion - nothing that could have hurt. I was never quick enough to catch the thrower - it was very like a game of 'grandmother's footsteps', and I felt quite convinced the children all took part, and we all laughed a great deal. The only thing I did manage to catch out of the corner of my eye was a moving curtain, but Janet's hand was lying nearby, though it appeared placid.

It all seemed more like horse-play than anything else. Billy needed the loo and departed, quite openly doing his poltergeist bark. When I announced I was going home one 'Adam' started to announce his presence. No, he didn't throw things, that was Bill. I left with what was to become my theme song: all children and poltergeists should be asleep by 11 o'clock. This drew some fairly good natured obscenity from the poltergeist, which failed to move me. I said firmly that this wasn't at all a nice way to talk, I was off now, good night. As I drove away, Janet waved to me from the window.

Pincott seems to have entertained the possibility, at any rate by the time he came to write up the night's events nine months later, that there might have been paranormal events that evening. So far as I am concerned, this cannot be contemplated with any vestige of rationality.

I returned again two days later (49) with some small Christmas presents. I decided not to stay for any 'phenomena' unless specifically asked, so that the family should not feel they had to put on a performance for me. The girls welcomed me with much warmth and showed me their records. Janet told me she wanted to be a pop star. Playfair was there and seemed to have forgotten all about our past disagreements.

The children went to bed, Playfair accompanied them, but Barnes and I stayed downstairs. He said how tired he was - and he looked it. I decided to take a risk, and reiterate my perennial refrain: 'Entities' - whatever they are, really should be persuaded to confine their antics to before 11 o'clock.

As Playfair had impressed upon me how ardent a believer Barnes was

I made a special point of not casting any doubt on their [the spirits'] independent existence but said it didn't matter who they were, they were just like the rest of us, open to persuasion and argument: and that in this case pressure should be brought to bear. After all, 'they' wouldn't gain anything at all from all and sundry getting worn out and tired. When did he, J.B., have to be at work? At 6 many mornings. And how long was he kept up at nights? Till after three quite often; I said this was quite absurd: and what happened to the children's schooling? Ah well, they persuaded their mother to let them stay at home. I said I thought all this really was very bad - the family had a life to live. I felt very strongly that J.B. was dying to talk frankly to someone. So I said it was all very well for these young men whose time was their own, and who didn't know what it was like having children and what children were like.

So it all came tumbling out. J.B. said he thought Janet had taught herself the trick of speaking from deep inside her, and that she enjoyed keeping all the strangers hopping about; that the mother didn't have the strength, and never had had, to say what was to happen in her own house. Just as she had knuckled under to the father all along the line, [she] now just gave in to all the visitors. She should occasionally say 'no, not tonight - go home!' but she never did and never would: she enjoyed all the attention far too much. 'This family has never been anything, and now every night is like Christmas.' The fact was, she couldn't bear to be in the house without a man. 'What do you think will happen the very first evening there isn't anyone there? She'll be at my door!' 'Mr. Playfair and Mr. Grosse say they are trying to get rid of it all, and they may even think that's what they are doing, but really, they'd be heartbroken if it all stopped. It's all just a show for them. And young David just eggs them on; a young man like that, no idea of how to deal with children, just all of them larking about in the dark, what do you expect?'

To cut a long story short, John B. has a pretty solid and shrewd idea what's going on, and also why: he feels helpless in the face of his sister's supineness and motivation to continue on the one hand, and the 'researchers' greater worldliness and education on the other...

He seemed...relieved to have someone to talk to who saw it in such commonsense terms and he shook me by the hand! He is a very worried man. I cannot tell whether he believes the phenomena or not - but he certainly does not believe in the spirits. He thinks Janet is making them up, the mother allows it for reasons of her own, and the researchers are egging them on for reasons of their own, and the crash is bound to come...I did not in any way prompt J.B. I had believed him to be a complete

believer and staunch supporter of it all, which is what I had been told by GP, so I was most surprised to get this little lot!

At this point GP came back...and volunteered that David Robertson had hypnotised the poltergeist! I nearly jumped out of my skin and asked had the lad hypnotised Janet?! No, no, he had made history by hypnotising the poltergeist! I nobly restrained myself. I had better meet this David myself. I couldn't think of anything peaceful and polite to say, so shut up.

When I went to say good night to the children, I was addressed this time by 'George', whom I duly told to go to sleep by 11. He was inclined to bargain: no, four o'clock. I told him that was far too late. I was allowed to look at Janet during this performance. Her face was as usual half-covered by her bed-clothes. I wished them all a happy Christmas, and Janet again waved good-bye from the window as I drove away.

On New Year's Eve 1977 I saw Playfair during a demonstration of 'psychic painting' by Luis Gaspareto (50). He told me he had now caught the girls cheating; he had used a video camera which clearly showed them larking about.

He seemed tremendously impressed with himself and Maurice for this feat of detection. He seemed quite unabashed as regards me since I had made it abundantly plain that I never thought anything else, without benefit of video cameras.

Amazingly in the circumstances, he again attacked John Beloff for having said in his accompanying letter that he thought the girls were cheating.

On 3 January 1978 Luis Gaspareto was giving another performance of 'spirit painting' for a TV film in 'Acacia House', West London, and Mrs. Branch accompanied me there (51). Playfair proudly told her that he had now caught the girls cheating, and Grosse told her and me separately that he now thought the voices 'came from the girls'. Mrs. Branch 'vividly' recalls the incident (52) and thought the two of them seemed very pleased with their research acumen in having caught out the girls. I passed on her suggestion that an experienced speech therapist might, with advantage, be involved. Playfair told me he would present Janet with an

ultimatum: she must let herself hypnotised. He was clearly under the impression that since she would be incapable of lying in a state of hypnosis, he would then find out what really happened! I expressed the strongest possible disapproval of this plan, especially if the hypnotising was to be done by Playfair or Robertson. If there was any question of hypnosis, a responsible medical man like Dr. Ian Fletcher should be asked.

On 11 January 1978 I went to see Prof. Hasted at his invitation, and he played for me a videotape which showed Janet bending cutlery, trying to bend a stout metal bar and jumping up and down on her bed all in a perfectly normal manner. Most unfortunately, although the date is clear from my diary, my scrappy pencil notes are not dated: but since I do not seem to have a later entry for a visit to Hasted, since I definitely saw it at Birkbeck, and since it came so close on Playfair's admission that there had been some catching out by means of a video camera, I am pretty certain this must be the occasion when I saw the tape. Hasted arranged that I should transport David Robertson, complete with video equipment from Birkbeck to Enfield on 15 January. He particularly asked me to try to give some protection to David Robertson, a young physics student, who was intermitting for an indefinite period after one year at King's College, London.

On 15 January (53) Robertson first came to my house where he met David Line, an electronics engineer, who had agreed to come to Enfield with me that evening. Line was helping me to get into working order the infra-red equipment made for me by (the late) C. Brookes-Smith, with whom he had discussed the construction of the apparatus. Had there been any promise of physical paranormality, Line would have managed the infra-red equipment for me.

When we arrived Mrs. Harper greeted us all cordially and immediately volunteered that things had gone too far: 'It now writes S-H-I-T on the toilet walls!'

Robertson set up his video apparatus, with some help from Line who thought he displayed adequate competence. The most marked new feature that day was that Margaret, two years older than Janet, now seemed much more in evidence. As I

wrote at the time:

She seems quite blatantly to practise speaking in a deep vowel-less pg voice whilst sitting about with all and sundry...in the general undirected TV enveloped atmosphere with much rushing about, screaming, coming and going, it seems perhaps less incongruous to hear her utter these weird noises...than in a more peaceful setting.

When she lapsed into this voice - and the notion that a throat mike is needed to trace whence it comes is quite ludicrous - I looked at her, grinned, and she grinned back. Occasionally I would put on a similar deep tone (nothing to it!) and she would grin again: it was really a pretty open game.

Mrs. H. talked to me at length about her troubles, more or less from the second I came in. How her ex-husband...brings the children's money on Saturdays, and how they all hate it...the voices told him to shove off, f... off, p... off etc, [and] although Mrs. H. says she very much disapproves of this sort of language...it was quite useful in getting Mr. H. to go away quickly.

She then told me a great deal more about her disastrous married life. She seemed to need some legal advice, and I suggested she might consult Miss Barrington who, it turned out, had been there the previous evening, together with John Stiles and Peter Hallson (54, 55, 56). All three are experienced and senior psychical researchers, and did not witness anything they felt was in need of a paranormal explanation.

David Line established good rapport with Janet and Margaret. The 'voices' kept sending out poor Dave Robertson, despite dogged protest on his part, and demanded 'that other David': however, in spite of this flattering attention, Line too could see no need to invoke any paranormality, nor did his all too patent scepticism inhibit 'phenomena' (57).

The poltergeist voice, now clearly emanating from Margaret, that evening called itself 'Tom', and greeted me with the words 'God bless you' and announced he was religious. When I finally said good-bye, 'Tom' told me to 'shove off' - which I did.

I then had quite a lengthy talk with the neighbour Mrs. Norton (58)

Mrs. Norton thinks that what is going on now [Jan 1978] is pure nonsense, (as I do); and that it is entirely kept going by the investigators, notably Maurice...But she says

how marvellous and kind he was when things were bad, and she is in no doubt that there were genuine happenings earlier on.

She described in particular the throwing about of marbles. These were very hot to the touch, immediately after moving about, and did not fly about as though thrown in the ordinary way, but dropped straight down and stayed put, not rolling further. Her husband had been hit by a piece of 'Leggo' with such force that he had a swollen bruise on his arm for days...

Mrs. N. said that unfortunately the happenings spilt over into her house, which is semi-detached with that of the H's. They have heard raps and noises. She has had her hair stroked and her bottom pinched... the noise often prevents them from sleeping.

Further people are on record as visiting the family.

Francis Huxley (59) seemed unimpressed, and Eleanor O'Keefe, the SPR's permanent Secretary, in a modest but exemplary statement (60) actually describes how she saw Billy kicking over a chair, and gently laughs at the 'phenomenon' of a roll of toilet paper 'appearing' in the lavatory pan after Billy's visit there.

My own next visit took place on 17 February 1978, together with Dr. Ivor and Mrs. Enid Grattan-Guinness (61, 62, 63, 64). Mrs. Harper immediately told us that beds had taken to making themselves, and that 'they' now also spoke 'through' Billy and Johnny. Maurice played us some tapes with a voice saying 'no' and 'look at Tom': he said he was there when the tape was made, and there was nothing to explain them. I. Grattan-Guinness observed that he was more impressed with the accounts provided by relatives present than by anything he himself had witnessed, there was no evidence either way: 'If there were phenomena they must have been submerged under the publicity and general anxiety in the family'. E. Grattan-Guinness also was impressed with the candour of Barnes, but thought the voices were 'a possible hysterical development and were being very well imitated by the youngest members of the family...feeling of a show being put on for the benefit of visitors. One wonders what would have happened to them had they been left alone for two or three weeks?'

The next visitors on record were Dr. Carl Sargent and Peter Dear, 27 February 1978 (65, 66). Sargent wrote he would submit various reports later, but I could not find these.

He does, however, state that he has 'a statement on tape from the various next-door neighbours of the Harper's (Vic and Peggy) to the effect that they feel that nothing genuine (i.e. paranormal) has occurred since the turn of the year in their opinion'. Dear, in a long and detailed statement, considered that if there ever had been anything paranormal, it was entirely finished by December 1977.

The next incident described in the dossier occurred when a roundsman, Mr. Rainbow, saw a cushion appearing on the roof of the Harper house (67). It is difficult to know quite what to make of this event, which John Stiles seemed to have had little problem in replicating. The original cushion was said to be no longer available (68).

Prof. Hasted paid one visit to Enfield on 19 April 1978, said in a statement dated 20 July 1978 to be his only visit to date, and he states that 'My assessment is that there have been physical events of a "poltergeist" character', but that reports differ widely in their reliability. He thinks that the 'voice is a very interesting case of automatism, originating in the false vocal chords'. In his notes he too says he was transporting video equipment.

The adults played 'the levitation game' with Janet. Then some bangs were heard (one by Hasted, two by Grosse), and apparently a light bulb went. Hasted later examined the 60 watt bulb in his laboratory and found that it was of Woolworth origin and that a radial mounting support rather than the filament was broken, which he considered surprising. Like numerous others (e.g. 70, 71, 72) he noted that Billy's articulation was much better when he was being 'the voice' than when speaking as himself. Hasted also thought that Billy was 'not really stupid, he is observant'.

My own last visit was on 21 May 1978 (73), and Mrs. Harper, Janet, Margaret and Billy all seemed pleased to see me. Janet was busy with a little garden plot and immediately planted some Livingstone Daisies that I had brought. I talked to Mrs. Harper for about two hours. I tried to sort out some of the dates to fit into a chronological sequence of events, and learned for the first time that, according to Mrs. Harper,

the first poltergeist disturbances had actually taken place at the Nortons' the previous June. When I subsequently went next door and asked her, Mrs. Norton categorically denied this: disturbances had started at the Harper's. However, according to her son Gary, whom I was not able to see although he was said to be in the house, he had had the bedclothes pulled off him 'some weeks before the manifestations started at the Harpers' (74) and came in to his parents' bedroom with mattress and bedding, refusing to sleep alone. He was 21 at the time.

I also, whilst talking to Mrs. Harper, probed for possible stressful events prior to the onset of the case, and she told me how Janet had shortly beforehand been beaten up on her way from school by some fellow pupils - an event Mrs. Harper and I both felt to be of major emotional significance. What she did not tell me, and I found this out only much later from Mr. Fallows of the Daily Mirror (75), was that before the onset of phenomena there was considerable upset and distress over Johnny, who was at that point about to be sent to a special boarding school. Faeces appeared in the Nortons' garden, and inevitably Johnny was suspected. Not unnaturally a veil was drawn by all over these unpleasant incidents.

Table 30 shows a chronological sequence of events in the Enfield case.

<u>Date</u>	<u>Event</u>	<u>Source</u>
<u>1976</u>		
14 June	Mr. Harper finally leaves home	(AG 46)
<u>1977</u>		
1 June	Margaret first menstrual period	(AG 46)
? June/July	Gary Norton incident	(SPR Barrington 25.5.1979 (AG 46)
end July	Janet beaten up by gang of girls	(AG 46)
August	Norton garden incidents	(pc Fallows to Gregory, 25.11.82)
31 August	Onset phenomena at Harper's	
1 September	(early hours) Police, including WPC Heeps	(Heeps to AG) & (GLP 4)
5 September	Mrs. Norton phones <u>Daily Mirror</u>	(SPR G/P 6) & (AG 24)
5 September	Grosse first visits Enfield	
10 September	<u>Daily Mirror</u> reporters visit	(SPR Fallows, 30.8.78) & (SPR Bence, 30.8.78)

12 September	Playfair first visits	(SPR G/P 15)
18 September	Gregory first visits	(SPR G/P 17)
24 September	'Teapot' witnessed Grosse	
3 October	Visit by medium	(SPR G/P 31)
6 October	Meeting at Janet's school, including Dr. A. Clark	(pc Clark to Gregory) (SPR G/P 52)
16 October	Berger visits	(SPR Berger 18.7.78)
24 October	Metal fireplace ripped out	(SPR G/P 39-40)
29 October -		
5 November	Clacton holiday	(GLP 46/7)
10 November	Janet's birthday; Harper remarries	(AG 46-48)
12 November	Carr, Cornell & Gauld visit	(SPR Gauld 14.11.77), (SPR Carr 8.10.78) (SPR Cornell, confid. 2.78, various) & (SPR G/P 53) (SPR G/P 55)
15 November	Children into care	
26 November	Children return from care, violent trances, screaming	(SPR G/P 58)
28 November	Mrs. Norton calls ambulance, as violent trances and convulsions continue	(SPR G/P 59)
29 November	Gaspareto and Dubugras	(SPR G/P 61)
10 December	Beloff and Gregory visit	(SPR Beloff & Gregory 11.12.77), (SPR Beloff confid. 7.78) & (SPR G/P 70) (SPR Rainbow 9.4.78) & (SPR Robertson 4.5.78) (AG 46)
15 December	Rainbow incident	
16 December	Onset of Janet's first period	
'Near Christmas'	Visit by R. Grosse	(SPR R. Grosse, 24.10.78)
20 December	Gregory visit	(AG 3 to 6) & (SPR Pincott 2.9.78) (AG 7)
22 December	Gregory visit	
31 December	Playfair tells Gregory Janet caught cheating by video	(AG 16)
<u>1978</u>		
3 January	Playfair repeats allegations to Branch, Acacia Hall	(AG 18)
11 January	Gregory visit to Hasted, video	(AG notes & Diary)
13 January	Barrington, Hallson and Stiles visit	(SPR Stiles 16.1.78), (SPR Barrington 13.3.78), (SPR Hallson 20.6.78 from cont.notes)
14 January	Gregory visit with Robertson & Line	(AG 21-25) (Line statement 14.1.78)
20 January	Huxley visit; O'Keeffe visit	(SPR Huxley 21.2.78) & (SPR O'Keeffe 27.10.78)
17 February	I. & E. Grattan-Guinness & Gregory visit	(SPR I. & E. Grattan- Guinness 4&7.7.78) & (AG 39-40), (SPR I. & E. Grattan- Guinness, confid. 5.7.78)
27 February	Sargent and Dear visit	(SPR Dear 9.8.78) & (SPR Sargent 10.7.78)

19 March	Sargent and Dear visit	Ref. same as above
27-30 March	SPR Conference, Cambridge, informal session on Enfield	
19 April	Hasted first visit	(SPR Hasted 20.7.78 & notes), only visit to that date (AG 46)
21 May	Gregory visit	
25 May	SPR Council sets up Enfield Committee (EPIC)	

Table 30. Chronological sequence of events in Enfield case. Letters and figures in brackets denoting source, e.g. (AG 46) refers to page number of my own contemporary notes described in section 4.11).

e.g. (SPR Gauld 14.11.77) refers to a document signed by Dr. A.O. Gauld, deposited in the SPR dossier (see section 4.12).

e.g. (SPR G/P 2) refers to the page number of Grosse and Playfair's joint account, also deposited in the SPR dossier.

e.g. (SPR Cornell confid. 2.78) refers to documents by Mr. A.D. Cornell, .Feb., deposited in the SPR dossier, confidential section, but which I have been permitted to cite.

e.g. (GLP 4) refers to page number of G.L. Playfair's book This House is Haunted.

4.3 Report by the Enfield Poltergeist Investigation Committee

The Enfield Poltergeist Investigation Committee (EPIC) was set up by the Council of the SPR on 25 May 1978 (76) largely on the initiative of Mr. A.D. Cornell, who was anxious to have the matter properly sorted out and to prevent 'another Borley'. It was composed of Miss M.R. Barrington, Mr. J. Stiles, Dr. P. Hallson and Dr. H. Pincott.

The Committee took a great deal of trouble in contacting potential witnesses, attempting to persuade them to write reports, and answer a structured set of questions. They visited the Harper household and such relatives and neighbours as could be contacted. They presented general conclusions concerning the witnesses, investigators and phenomena, and added personal statements by individual members of the Committee. The report includes a 'confidential' section.

Reports submitted as a result of letters sent out by the Committee in order of date were by the following (see Table 31).

Mr. J. Stiles	-
Mr. F. Huxley	-
Miss M.R. Barrington	-
Dr. A.O. Gauld	-
Dr. I. Grattan-Guinness	-
Mrs. E. Grattan-Guinness	-
[Dr. C. Sargent - report promised but not on file]	
Mr. L. Berger	?
Prof. J. B. Hasted	?
Mr. Peter Dear	-
Mrs. A. Gregory	-
Mr. G. Fallows	-
Mr. D. Bence	?
[Mr. J. Fuller - interviewed Mrs. Harper only]	
Dr. B. Carr	-
Miss E. O'Keeffe	-
Mr. M. Grosse	+
Dr. P. Hallson	-

Table 31. Reports submitted as a result of letters sent out by the Enfield Poltergeist Investigation Committee in order of date.

- = witnessed nothing paranormal
 ? = witnessed something conceivably paranormal but without commitment
 + = witnessed what was firmly deemed to be paranormal
 For amplification, see text.

Of the 16 persons listed(excluding Sargent and Fuller), 12 are marked '-' to indicate that they did not experience anything they were willing to describe as paranormal; three were willing to consider paranormality for something that happened at Enfield without, however, firmly committing themselves: these I have marked '?'. One witness was a convinced believer, and was accordingly marked '+'. It must be borne in mind that those answering EPIC's enquiry composed letters and statements of varying lengths, quality and reservation.

Sargent (77) told me that his estimate agreed with the negative evaluation of Dear, although he had obtained initial promising results on a random number generator. According to Cornell (78) this random number generator was never genuinely random, and there is in effect no further report by Sargent. Pincott, though not in this section of the dossier, would probably have been marked with a '?' or possibly '?/+', had he been listed.

This analysis certainly lends no support to claims that numerous experienced psychical researchers were eye-witnesses

to what they believed to be genuine paranormality.

The dossier further contains a statement by Mr. J. Rainbow, already alluded to, about a cushion on the roof. There is also a statement by D. Robertson, presumably referring to the same incident, since he refers to his visit of 15 December 1977 (79). In effect, Robertson states that he witnessed nothing paranormal, having been sent out of the room by 'the deep voice', but thinks he would have heard the window opening (presumably, one is led to infer, for Janet to push the cushion outside in a normal manner). There is a further article by a Mr. Charles Moses, intended as a report for the American parapsychologist W.G. Roll, which seems largely based on what Grosse told him, and takes virtually all the claimed events at face value whilst adding some psychological observations of his own about the family (80).

Confidential reports are added by A.D. Cornell (81), J.R. Beloff and A. Gregory (82), J.R. Beloff (83), I. and E. Grattan-Guinness (84), L.E. Berger (85) and A. Gregory (86). Cornell, whose report is in my view the most detailed and professional of all the accounts, providing separate floor plans for each of 13 incidents discussed, has given me permission to make what use of it I wish. I hardly break any confidences if I state that all these confidential statements contain reservations rather than endorsements. The tendency for witnesses to testify confidentially illustrates one of the major problems of investigative parapsychology: people are naturally extremely reluctant to publish views likely to hurt, give offence or cause acrimony and possibly litigation; indeed the whole subject is such that whatever a witness says is virtually bound to given offence to someone.

The four members of EPIC, after detailing some evidence taken from witnesses about certain specific events, presents an eight page over all composite report (87) which seems to me to represent a scrupulous attempt at fairness and impartiality, and primarily addresses itself to two questions:

- (a) Do we think the witnesses are credible, and
- (b) Do we think that any paranormal events took place at Enfield?

The writers continue by summarising to the effect that two

investigators, who were early on the scene and devoted an 'almost unheard of amount of time' to their task, claim to have witnessed poltergeist activity on as massive a scale as the family, relatives and a 'few other citizens of Enfield'. A fair number of SPR members who paid occasional visits either observed nothing unequivocally paranormal, or failed so to report.

On closer scrutiny, they continue, of the two pro-Enfield investigators, Playfair actually claims himself to have observed relatively little, although he attributes to poltergeist activity the sort of events that have failed wholly to convince even others inclined to be sympathetic to Enfield paranormality (Berger, Robertson, Pincott). They consider it

speaks a lot for GLP's accuracy and restraint in the detailing of facts that third parties are able to distinguish description from opinion...When allowance has been made for GLP being comparatively ready to attribute paranormal interpretations to both what he experiences, and what others report to him, it seems that only MG, of all the investigators, really witnessed events that cannot be explained away...

The members of EPIC have no hesitation in accepting both Grosse and Playfair as 'witnesses of truth. We do not doubt their sincerity and they have been scrupulous in reporting only what they believe themselves to have seen or heard'. According to the report 'one of the clearest examples of evidently paranormal movement described by MG is the rocking of a teapot on the draining board, and another is the sliding backward and forward of the kitchen cupboard doors'. The Committee discounts 'most of the wilder scenes' and the paranormal origin of the 'voices' - and expresses the view that Playfair's and Grosse's concentration on these voices and their implausible spirit origin is likely to lead to the (presumably unreasonable) belief that 'nothing of interest happened at Enfield'.

Whilst disclaiming any corporate right to pronounce, the Committee expresses the view that 'some of the dramatic events have lost rather than gained credibility after attempts were made by us to substantiate reports'. For example:

MG relates how a sofa in the living room flew into the air, spun backwards on its axis and landed on its

back four feet pointing upwards. He was standing immediately in front of it and John Barnes was in front of it at the corner at the other end. No one was near it. This seems to us a manifestation of extraordinary magnitude and we were astonished to find that John Barnes was not able to recall this event to mind.

With, in my view, somewhat over-scrupulous fairness, verging on the patronising, the report continues by saying that, whilst it seems 'surprising' that Barnes should have failed to remember so major a phenomenon, to him this might have seemed not so much as a flouting of the law of gravity as 'just another nuisance' (because Mrs. Harper tended to insist on moving into the garden pieces of furniture that had 'offended by moving around unasked'). In other words, what might seem epoch-making to a researcher, might seem irrelevant to the family.

They then discuss the photographs, supposed to be proofs of paranormal events, but could find no reason to suppose that they constituted evidence for such events (although one member discerned an extra digit on Margaret's hand in one of the pictures). These photographs are also, and in my view devastatingly, criticised in a contribution by Harris (88).

The Committee felt impressed by the statement by Woman Police Constable Heeps, also by the honesty of Barnes and Mrs. Norton, 'both of whom related extraordinary incidents that could not reasonably be ascribed to malobservation'. They single out as example Barnes' statement that he saw drawers moving in and out of his daughter's bedroom dressing table, and one where Peggy Norton saw a beanbag ashtray levitating to the ceiling.

We think it reasonable to conclude that if any of the manifestations were genuine, then some of the phenomena reported only by the Harpers may also have been genuine. This is entirely speculative but not unreasonable.

The report pays tribute to the time and trouble put in by Grosse and Playfair, and their kindness and helpfulness towards the family. There follow four personal reports by individual members of the Committee, which in effect show up some differences of opinion.

M.R. Barrington says she personally felt satisfied that paranormal events did take place at Enfield, and in view of this

she felt free to speculate about some incidents that, in principle, could have been caused by the children. She singles out the forcible removal of the fireplace.

Dr. Hallson is inclined to stress 'possible mischief by the children' - 'However, assuming the good faith of the adult witnesses and their freedom from mal-observation and false inference it could be that in the early stages of the disturbances events of a poltergeist nature may have occurred. This is however by no means certain, and the evidence does not enable me to come to a definite conclusion'.

Pincott, states that 'there can be little doubt that in the opening stages at least the case appeared to show paranormal aspects...I share the view that in the later stages of the investigation the children were quite clearly and openly shamming in order to attract attention...' Pincott, somewhat strangely one might have thought, considers the case 'one of the most important ones for many years', apparently because of the mere volume of time spent on them. He half dismisses and half endorses the photographs.

Stiles singles out as being impressive the Heeps chair incident, partially confirmed by Vic Norton, a 'dancing teapot' incident reported by Grosse, the drawer movements and a 'dress gathered in at the waist' by invisible hands, reported by Barnes. In Stiles' view any incident that might be attributed to the children needs to be ruled out and should not be considered paranormal. He dismisses the photographic evidence, and does not commit himself about the rest - 'it depends how many people you want to disbelieve'.

It is thus clear that despite some protestations, the joint report is something of a compromise, between two judges inclined towards belief, and two tending towards disbelief.

4.4 Assessment of evidence for genuine physical paranormality

As will be clear from 4.2 and 4.3, first-hand reporting of physical phenomena in the Enfield case rests on the word of the immediate Harper family, their close relatives (Barnes) and neighbours (Norton), of Grosse and a very few outsiders,

notably Woman Police Constable Carolyn Heeps (89). Playfair, as EPIC described, whilst being rather easily inclined to attribute paranormality to events that seemed normal to others, himself claimed to have personally witnessed relatively little by way of paranormality by the time he was interviewed by the Committee. If this is correct then, ironically enough, Playfair seems to be the only witness who suffered from fading conviction!

4.41 Instrumental recordings

Inevitably any so-called 'spontaneous case' is chaotic, and instrumental corroboration is hard to come by. Still photographs are unsatisfactory evidence since, as the EPIC report pointed out, they cannot show how an object or person got to where they are shown. In this case, matters are hardly helped by the fact that photographs, e.g. of Janet draped over the TV set watched by a startled-looking Barnes, were reproduced in the sensational press with changed captions (90). Harris challenged Playfair about this, but Playfair shrugged off the matter (91). The Enfield photographs are also peculiarly unsatisfactory since, as Harris showed, the camera was triggered by actions performed by the children themselves when it suited them. As he pointed out, video evidence would have been particularly important since this would have shown the entire action, and could not have been interfered with by the children.

As first mentioned in my review of Playfair's book (92) he strongly conveyed the impression that the idea of using video recording only occurred to 'the investigators' in June 1978, that the equipment could be installed without the children's knowledge, that the children had never before been videotaped whilst phenomena were supposedly going on, and that there was something quite remarkable about the way the children, or the poltergeist, and particularly Janet, frustrated all attempts at videotaping.

Now in point of fact, as has been shown, video cameras had been at Enfield off and on for at least six months by June 1978: for Janet to have been caught cheating by 31 December 1977 there had to be video equipment there in December at the latest. I personally transported video plus Robertson there

in mid-January 1978, Hasted moved video there plus Robertson and Vic Robbie of the National Enquirer in April 1978 (93), and I saw video apparatus hanging about Enfield in May 1978. Indeed, according to a BBC broadcast, some engineers from Pye had been on the scene in September 1977, and found their instruments malfunctioning in the most bizarre manner (94). The video film Hasted showed me 11 January 1978 certainly displayed Janet merrily cheating away at metal bending and levitating (on my interpretation).

My query of the Playfair version concerning video recordings in June 1978 remained unchallenged until two years later, when Grosse and Playfair discovered that I had alluded to the matter again (95). Grosse thereupon wrote to the SPR Journal (96)* and Playfair to the Parapsychology Review (97), taking rather different lines. Grosse now admitted that there had been earlier videotapes of the girls 'indulging in tomfoolery in their bedroom', and he implied none too subtly that only a stupid and pompous pedant such as myself could possibly have taken them seriously but, since there were such fools about, it had been deemed wiser to keep them in the dark. There is some equivocation in his letter about just which videotape he was referring to - it was pretty clearly not the one Hasted showed me, but some other tape that Grosse and Robertson, for reasons best known to themselves, took of the girls larking about. What he entirely fails to meet as pointed out in my rejoinder (98)* is my original point that the misleading impression conveyed that video cameras were a bright and novel idea in June 1978, and that all omission of any reference to the incident when he and Playfair had claimed to have caught Janet in late December 1977 and early January 1978 clashes with the claim that the girls had never been unknowingly caught. Also, why had Playfair told Harris, shortly after my review appeared, that he, Playfair, knew nothing of any video recording?

Playfair took an even more magisterial line: why should he select for publication one particular incident, when he could not possibly, within the compass of a book of reasonable size, chronicle all that had happened? He was, he stated, just not interested in 'academic debate'. He too, as I pointed out (99), failed to answer my central queries: it was not a question of

*attached to thesis (Labels H & I respectively)

omitting some odd incident I happened to have fastened upon, but failing to mention one that is highly germane to, and casts doubt upon, actual statements made in his book. Furthermore, as Playfair was anxious to invoke in a somewhat ambiguous manner the authority of Hasted and Fenwick, complete with description of their titles and seats of learning, and also that of Robertson, presumably because of their academic affiliations, he was hardly entitled to ignore criticism on the grounds of a lack of interest in academic debate. (Harris also, and he is not an academic, has not had adequate answers.)

It would appear that it is not so much 'academic debate' that Playfair disliked as any kind of querying or criticism of his views and presentation.

The video episodes, as it seems to me, certainly do not constitute any evidence about the authenticity of the Enfield events one way or the other. My view has been at all times, even when Playfair seemed really persuaded and incensed that he had at last 'caught' the girls cheating, that 'it doesn't take a video camera' - the girls were so very obviously playing about. However, it does point to serious shortcomings in the accounts given of the case by Playfair. It would seem that both Grosse and Playfair had forgotten that they had actually told, not only me but also Mrs. Branch, that Janet had been caught cheating by means of video; and also the lengths of time for which video equipment was installed at Enfield.

The video debacle illustrates two further important issues that arise in investigative psychical research. There is a general, almost mythological tradition that there is something paranormal about the failures of investigators to capture paranormality by means of equipment, that there is 'paranormal instrument failure' and other evasive tactics engaged in by the ostensibly paranormal agencies. For all anyone knows, there may have been something strange about the instrument failure experienced by the engineers from Pye (100); they have not published particulars of their findings. However, as regards the rest of the videotape saga, there is clearly evidence for nothing here but the most normal and cumulative misfortunes and failings of human beings. (And why should the paranormal agency have conspired against video but not against photographs?)

The problems of instrumental recording are serious enough without adding to them a smokescreen of mystification bathed in a general aura of a paranormal conspiracy theory.

The other important point that emerges clearly is that instrumental recording in itself does not and could not amount to evidence: it is merely contributory, on the model of scientific forensic evidence in a court of law: it has to be evaluated in terms of human judgment and such evaluation is bound to be a somewhat dry, tedious, technical and semi-technical 'academic' matter. This is a far from trivial point when the question is posed just what would count as good or conclusive evidence one way or the other in such a case. It is the quality of the evidence, and the security with which it is anchored in coherent and reliable, cross-validated human testimony that, in the present state of knowledge, assures a degree of belief-worthiness, not the mere fact that there is some supposedly 'hard' record.

As for the audio tapes frequently cited by Grosse and Playfair as irrefutable evidence, it is difficult to see how these could be thought of as constituting anything other than, at best, the raw material upon which some sort of argument in favour of paranormality could have been based, if the Enfield champions had been willing and able to take the requisite amount of trouble and bring the necessary amount of expertise to bear, in transcribing and analysing them. Grosse seems to imagine that the often barely audible and very noisy recordings of which he plays snatches at meetings constitute the sort of 'facts' that others have to disprove if they are to be morally entitled to challenge paranormality at Enfield.

It is typical, for example, that Grosse claims that 'the voices' spoke continuously for long periods in such a manner that, in the opinion of 'an expert', the vocal cords would have been damaged had 'the voices' been produced 'normally'. When I pointed out that in my experience 'the voices' spoke in brief snatches and that an appropriate time-analysis of the audio record would be necessary to substantiate even mere continuity (let alone anything else), Grosse simply failed to reply, confining himself to the comment that he did not wish to continue 'point scoring' (see below 4.45).

Now there can be no doubt that accurate transcription and timing, and the argumentation and tabulation based upon them, are peculiarly time-consuming as well as difficult and controversial matters, as will be clear from the analysis of the Manning records (see Chapter 5). What is of considerable importance in the present context is that what 'researchers' like Grosse and Playfair demand is that others (unspecified) are under some sort of moral obligation to take the trouble to analyse raw data obtained under conditions not under their control, if these others are to be entitled to challenge Grosse's and Playfair's views. What they demand is in effect some sort of 'expert', (such for example as Prof. Hasted), willing to underwrite their own claims, and they do not themselves wish to be involved in the labour, trouble and expertise of doing the work, of creating and even merely following and defending the arguments in appropriate detail - that would be trifling 'academic debate'.

Yet if rationality is to prevail, any appeal to 'authority' must in the last resort be based on the quality of the arguments used by the person whose authority is invoked, and not upon the conclusions he reaches, or the social or academic position he holds. It is precisely on this painstaking process of hard work, intricate reasoning and dispassionate analysis, as opposed to appeals to authority as such, that ultimately the whole fragile edifice of rationality in general, and science in particular, depends. 'Researchers' such as Grosse and Playfair feel entitled to expert status without the more onerous and demanding obligations entailed by a description.

4.42 Testimony of outside witnesses: WPC Heeps and Mr. M. Grosse

The only ostensibly impressive testimony for macro PK in the Enfield case emanating from an outside source that I have been able to find comes from Woman Police Constable Carolyn Heeps, which is perhaps the most important statement made in the case, as Grosse ostensibly implies (101). Indeed I confess that I myself had felt that this certainly argued in favour of some measure of paranormality at the very outset of the case. Members of the SPR's EPIC had found (102) that despite (or perhaps because of) written application to her superiors, they were unable to obtain an interview with WPC Heeps. I decided on the simpler

expedient of ringing up Enfield police station and asking for WPC Heeps at a time when Ms. K.M. Wilson was with me, and able to listen in on the conversation from an extension telephone. The conversation on the following page ensued.

It would thus appear as if 'Dr. Grosse of the Psychical Research Society' coached Mr. Playfair's star witness to testify in the desired direction. Although I had certainly not expected anything like this; on re-reading my own notes, and reflecting on Mr. Grosse's conduct of the case in general, it should not have come as any surprise. Mr. Grosse habitually, apparently without suspecting any incongruity, emphatically suggests to anyone discussing any matter relating to the Enfield case with him, his own usually intensely paranormal interpretation of events; if he is contradicted, even gently or by implication, he becomes, in my experience, exceedingly distressed and agitated and, if the offender persists, Grosse loudly shouts at him for quite long periods and becomes increasingly angry and abusive. Miss O'Keeffe, Ms. Wilson, Mr. Cornell, Mr. Harris and others as well as myself have certainly experienced this sequence of ostensibly entirely unconscious and unsophisticated attempts to exert pressure on others.

Mr. Grosse joined the SPR on 9 December 1976. Previously, on 10 September 1976, he had sent to the Society for its archives an account of the tragic death from head injuries in a motor cycle accident of his daughter Janet at the age of 22, on 5 August 1976 (103). The account is repeated in Playfair's book (104), and it is difficult to imagine any affectionate parent not being deeply upset, not only by the tragedy, but by the fact that her brother Richard had received from her on the very day of the accident a greetings card depicting a figure with a bandaged head, with the printed caption 'I was going to send you a bottle of toilet water but the lid fell on my head' with a hand-written addendum 'And there won't be much of that left soon either!' The word 'And' is connected by a hand-sketched arrow to the general direction of the statement 'But the lid fell on my head'.

Mr. Grosse went on to discuss what he believed to be this and other paranormal coincidences in connection with his daughter's death with senior psychical researchers (105). One

Telephone conversation

5.11.82

(a.m. , CA 10.30 - 11.45)

I rang WPC Heeps and she immediately remembered the incident, date and time of the Enfield case episode. She said the room was dark and the illumination was from a street lamp outside the house. She immediately said, without any prompting, "at the time I thought the children or their friends had done it all, but later Dr. Grosse of the Psychical Research Society told me that he and various media people proved scientifically that the children couldn't have done it." She said at the time she was quite sure that all the throwing about of marbles and leggo bricks was done by the children but the movement of the chair was a bit more difficult to explain. It rocked up and down about 3 inches; I said 'didn't it move along?' and she said 'Yes, by about 3 feet'.

I asked her what she had thought at the time and she repeated that at the time she thought the children had done it but later on, after talking to Dr. Grosse and on more mature reflection, she thought the H[] children just couldn't have done it. It was such a heavy chair that three little children could hardly move it though there were eight people in the room. So how could this chair have moved by itself?

She repeated that she thought this after Dr. Grosse had explained to her how he had seen leggo bricks fly about in such a way that they could not be thrown. She did not think there were wires or threads attached to the chair. She thought Mrs. H[] threw the chair out afterwards which she could ill-afford. She thought a priest had told her to do this.

She volunteered that the children would disappear into the kitchen and return to the room and a few minutes later Mrs. H[] would go to the kitchen and say a fire had started. She said it certainly was a strange experience.

I thanked Officer Heeps.

Kathy Wilson was on the extension line at the time from the time that I had dialed the number. She confirms that this is what took place and that WPC Heeps didn't require prompting and was not pressurised in any way.

signed



Anita Gregory



Kathleen Wilson

of these was the late Rosalind Heywood, a Vice-President of the SPR herself and, unusually in one of its dignitaries, subject to psychic experiences, accounts of which she had published (106). He was however, as he told Miss O'Keeffe, disappointed: Mrs. Heywood had not 'understood'. This seems to me extremely doubtful: what she probably did was to caution him against publishing as evidential what he so deeply felt to be significant. He also on Miss O'Keeffe's recommendation consulted the late Brian Nisbet, one of the Society's most experienced investigators of spontaneous cases - like Mrs. Heywood a person of the greatest kindness, patience and integrity. Again Grosse came away disappointed: Nisbet also 'had not understood'.

Mr. Grosse constantly pressurised Miss O'Keeffe to send him out on some case brought to the Society's notice, and when she finally gave him the Enfield address he seems to have felt that here was yet another remarkable coincidence. Furthermore the name of one of the Harper children was the same as that of his daughter. He appears to have increasingly come to link his faith in the authenticity of Enfield with evidence for personal survival, and so far as I know everyone, myself of course included for obvious reasons, tends to avoid any direct allusion to the real topic of his original preoccupation. Indeed it took a great deal of heart-searching for me even to mention it in this thesis. Yet it cannot be omitted.

Playfair only too correctly states that Grosse has 'little patience for academic debate' (107) - in fact he has none. The whole notion of dispassionate appraisal, of making allowance for one's own bias or shortcomings, the painstaking weighing of pros and cons characteristic, I would have thought, not only of academic but of high grade journalistic investigation or legal enquiry, is utterly foreign to him. His special dislike and contempt is reserved for psychology.

He sees no incongruity in publicly expressing the view, or rather stating as fact, that psychologists (such as Beloff, Gauld or myself) who have been in the field for literally decades longer than he thus risking the permanent disapproval and contempt of our colleagues, are cowardly, prejudiced and incompetent. He refers to 'the depths to which researchers can

sink' with clear and special reference to Beloff and myself (108), who had had the audacity to suggest that 'the kids were just playing tricks on you'.

To give a proper picture of what had actually happened it is necessary to quote in extenso John Beloff's letter to Guy Playfair, dated 13 December 1977.

Dear Guy,

Here is the statement I promised to send you. I hope you and Maurice will consider it a fair account of what transpired on Saturday evening although, of course, we are not claiming that we have mentioned every detail that could be relevant.

To speak frankly, as I feel morally obliged to do for the sake of everyone concerned, both Anita and I consider that the girls are playing games with you. With the help of your instrumentation you may prove us wrong but my guess is that your flashlight photos will reveal nothing evidential nor will the double tape-recording. I beg you therefore to exercise the greatest caution before further committing yourself in public. I realise you may bitterly resent this advice, after all the time, effort and money you have spent on the case this would be entirely natural. But remember that I am not asking you to deny the paranormality of events which you or Maurice may have witnessed with your own eyes, merely to reflect that those girls are perfectly capable of faking phenomena and, for the sake of keeping the affair going, which they are obviously enjoying enormously - it must be much the most exciting thing that has ever happened in their young lives - they would not scruple to do so.

Your friend and well-wisher,
[signed] John Beloff

To describe this cautious, gentle and humane piece of not particularly academic advice as 'depths to which researchers can sink' seems to me to reveal a degree of emotive bias and blindness, and a failure to be able to 'decentre' so extreme that rational - let alone academic - discussion seems hardly practicable. At the time, as has been mentioned, Grosse believed there to be no element of trickery at all, cautioning the girls that he would go away and never return if he ever once caught them cheating, the very attitude that prompted the tenor of Beloff's letter; when Grosse was, later on, compelled to admit that there was indeed some trickery, he shifted his position, attributing to Beloff and myself the crude and simplistic

view (probably originally entertained by himself) that any play-acting by the children would exclude paranormality: this had the multiple advantages of keeping himself in the right, discrediting Beloff and myself as 'experts', and showing what (pernicious) fools psychologists were apt to be.

Normally Grosse is an amiable person, indeed he is capable of warm spontaneous kindness, empathy and consideration as I have myself experienced; unfortunately where Enfield is concerned, and now more and more with respect to numerous other experiences of his own that he believes to be paranormal, his spontaneity takes the form of increasing degrees of unconscious manipulative coercion.

This no doubt falls on somewhat stony ground in the case of researchers such as Beloff and myself, but I have no doubt that, to Carolyn Heeps, Grosse must have seemed impressive and formidable, an authoritative expert who communicated to her the very depths of his own conviction. Grosse is also a solidly kindly and paternalistic man, and there is evidence to suggest that whatever Carolyn Heeps thought, she felt frightened, as described by Vic Norton (109). She may well have felt that Grosse's weighty and considered judgment absolved her for having been afraid. Now the fact that she felt fear, if it is a fact, is not evidence for anything paranormal: it must be thoroughly unnerving to arrive at a house in the early hours of the morning, and be expected to take charge of a situation where eight to ten people are crammed into a small room in a state of acute terror, switching off the lights at intervals, whereupon amid shrieks, objects fly about, chairs move and fires are started!

It is perhaps of some interest that Carolyn Heeps described the piece of furniture that moved as a very heavy arm chair, whereas Vic Norton referred to it as 'an ordinary kitchen chair'. In the statement she gave Grosse in September 1977 she stated that the arm chair 'wobbled' and did not leave the ground, and slid for three or four feet. When she spoke to me, November 1982, she thought it was so heavy that 'three little children' could hardly have moved it. However heavy the (no longer extant) chair, this is surely an under-estimate of the combined might of a trio of children aged 7, 11 and 13! (Actually, there

were at least 4 children there, since Johnny was still present.)

It is clear that the statement made by Heeps was made no less than 10 days later (10 September 1977 according to Grosse and Playfair's account), before Playfair according to himself appeared on the scene on 12 September (110). Again this underlines the importance of obtaining statements as soon as possible and before witnesses have been subjected to various forms of pressure and indoctrination. It also emphasises the need for such statements to be obtained by persons of some self-discipline, impartiality and sophistication, and circulated to responsible outsiders as rapidly as possible.

4.43 The testimony of Mrs. Norton and Mr. J. Barnes

Members of EPIC and others, as well as I myself, as may be seen from such notes as I quoted above, regarded Peggy Norton and John Barnes, especially the latter, as reliable witnesses and truthful informants. None of us, as I now see it, myself included, made sufficient allowance for the heavy and continuous dogmatic pressure exerted by Playfair and especially Grosse on everyone involved in the case, and the extent to which testimony was obtained by leading questions. Once the Nortons and Barnes had been persuaded that 'the experts from the Psychical Research Society' knew what they were talking about and thought it all paranormal, once they had been told authoritatively what to report and what to expect, this may well have coloured and then fixed their recollection of earlier incidents, just as it did in the case of WPC Heeps. The latter, being somewhat more sophisticated about evidence, and also only having had the experience of the Enfield circus during a single night, was able to make a clear distinction between her earlier and later interpretations of the situation. (She may also, and this is purely speculative though quite reasonable, have had to contend both with Sgt. Hyams, the colleague who was with her, who was apparently unmoved, and with her own original case notes for the events of the early hours of 1 September 1977.)

Barnes and Mrs. Norton, on the other hand, were exposed for months to Grosse's and Playfair's, especially the former's, doctrinaire certitudes night after night. It is plain from

my notes quoted above (pp. 167, 170-71) that by 22 December 1977 Barnes and by 14 January 1978 Peggy Norton felt disenchanted with them, whilst still acknowledging past kindness. Mrs. Norton persisted in the view that at any rate her early experiences had been authentic; but in Barnes' case, as may be seen from my contemporary notes (p. 167 above) there is even some doubt as regards his overall assessment of authenticity by Christmas 1977.

Mrs. Norton, being a neighbour, was a somewhat freer agent than Barnes, more able to withdraw from the situation if she chose and she clearly also confided her current doubts to Sargent in February 1978 (111). As already mentioned, I tried to find out from her in May 1978 where the phenomena had actually started: whilst remaining cordial and polite, she made it plain to me that I was not about to disturb her husband and son at dinner. I accepted this with a diffidence quite appropriate in the case of a social caller, although I might be criticised for this as a researcher (and was by Mrs. Branch). Had I been ill-mannered enough to persist, I would probably have found out that despite Mrs. Norton's categorical denials to me on that occasion, Mrs. Harper was quite right in insisting that phenomena had indeed ostensibly started at the Nortons, and not at the Harpers: her son Gary later told a member of EPIC that he had indeed had an experience he felt to be uncanny some weeks earlier (see above p.173) (112). I expect she too, like Mrs. Harper, was not anxious to have Johnny's possible delinquent exploits disinterred (see above p. 173) - which they might have been had I probed persistently enough.

Paradoxically, the Gary Norton incident might be deemed by some to indicate a measure of initial paranormality after all, but centering on the Nortons, and then spreading to the Harpers, whereupon Mr. Norton called the police and Mrs. Norton the Daily Mirror. Mr. Norton, as has been seen, might have had other reasons for ringing the police and it would appear that Mrs. Norton was not wholly unsophisticated in the ways of poltergeists, since she seems to have been in possession of and discussed an article of 13 November 1976 in Woman magazine by Mrs. Manning (the mother of Matthew Manning, see below

Chapter 5), 'Why my son must live in the past' (113).

Some phenomena were reported at the home of the Nortons, and also at the Barnes' some houses further down the street. John Barnes was, by Christmas 1977, exhausted with lack of sleep, tension and departure from normal routines. He also became increasingly socially isolated as a result of Enfield events. He confided to me that he was in trouble with his wife for spending too much time with his sister and her family: indeed Mrs. Barnes' absence from the scene was notable. I would not be totally surprised if Barnes, often kept awake until 3 or 4 in the morning, frequently having to be on duty at 6 a.m., was at times compliant to suggestion and succumbed to the general atmosphere of noisy hysteria, to the point of hallucinating. By Christmas 1977 he clearly felt desperate, trapped by people more sophisticated, authoritative, knowledgeable, well-off and well-rested than himself, and to whom he and his sister's family were under a deep obligation.

His original willingness not only to accept but to re-iterate Grosse's 'proofs' of paranormality seems to me to corroborate the view that Barnes was extremely vulnerable to suggestion. It will be recalled that Janet was again and again discovered after a shriek and a thump sitting on the floor some distance from her bed. Grosse and Playfair, also again and again, insisted that she could not possibly have jumped that far and, in order to 'prove' this, repeatedly challenged her in front of witnesses to do so. Naturally, she gave a feeble little leap, landing at a small distance from the bed. Not only could she, in my estimation, most emphatically have jumped far enough to land where she was found when viewers were allowed in her bedroom (it was a tiny room, I have taught junior age children, and the bedroom was plastered with certificates testifying to Janet's special athletic prowess); but there was not the slightest need for her to jump at all, since the 'phenomena' always took place behind closed doors: she could perfectly well have walked to the spot, thumped the floor and shrieked. Now Barnes repeated, to begin with, over and over again, just like Grosse and Playfair, that the fact that Janet 'could not' jump that far meant that it must be some extraordinary

effect, and he also concurred with similar 'reasoning' as regards the 'voices' (though as has been mentioned, he later went back on this). Barnes was a practical man and had children of similar ages, and in my view his conviction of such 'proof' must surely have stemmed from the same source that persuaded WPC Heeps that 'three little children' could not have rocked and shifted an arm chair, however heavy, by three or four feet, and suggests a degree of doctrinal compliance on the part of Barnes, at least when over-awed and exhausted. I entirely concur with, for example Carr (114), that Barnes was honest and trustworthy though suggestible.

Another matter that needs to be considered is the possible part played in the drama by the Barnes children, particularly Paul, a pleasant, intelligent and alert boy of about 13 or 14 at the time, and who at one stage, as Grosse told me, acted as the latter's helper and young fellow researcher. EPIC members also formed a favourable impression of Paul (115). Now according to George Fallows of the Daily Mirror, within a very short space of time of his entering the house at the very outset of the case (i.e. presumably no earlier than 5 and no later than 10 September 1977), he had mentioned the word 'poltergeist' (116). According to Fallows the Barnes children had right away made for the public library and consulted books on poltergeists etc.: he believes that within the space of less than a day the children were the best informed persons in the house concerning such matters, himself and Grosse included. (Playfair, it will be recalled, did not come on the scene until 12 September.)

Paul was thus in a position not only to share his knowledge of poltergeist lore etc. with all involved in the case from a very early point in time. The whole matter can hardly fail to have been a major topic of conversation (and later one of tension) in the Barnes home, in the course of which no doubt what might be expected next, and how to interpret what had been observed, would feature quite prominently.

4.44 The Harper family as witnesses

Few would, I think, accept Grosse and Playfair's opinion that Mrs. Harper should be viewed as 'the chief investigator' of the case (117). They may have thought that they were thus

bestowing upon her credit and credibility, incidentally justifying their own reliance on her alleged testimony, but I cannot share this view. It seems to me to land Mrs. Harper with a responsibility that the poor lady hardly needed to add to her other troubles. In my experience, and also during the broadcast interview she gave (118) she made virtually no claims and rather passively and miserably treated each new manifestation with mildly unhappy resignation, as yet another misfortune such as her ill-health (it seems she was or had been an epileptic), and her ex-husband's disgraceful treatment of her. Although I too found her an intelligent and ostensibly accurate informant when she provided factual information (e.g. about dates of her own life, her daughters' periods, the fact that phenomena had started at the Nortons') my estimate of her role coincides roughly with that of her brother which I have cited above (p. 193) (119). She was far too inert, passive and generally unwell to be thought of as an 'investigator'.

The Harper children cannot of course be thought of as witnesses. As I have stressed repeatedly, they hardly even pretended, certainly not to me, to have done anything other than play-act most of the time. When Fallows took his leave of them in September 1977, saying he would not be seeing them again, he reassured them that all would now calm down since events were in some obscure way connected with Johnny, and Johnny had now gone away. Within an hour or so the girls had rung up his house trying to summon him back: it had all started up again (120).

Much has been made by Grosse and Playfair of the family's lack of education, sophistication and knowledge of matters occult or paranormal. Playfair makes a special point of emphasising how if he himself wished to fake a poltergeist, he would have no difficulty, having studied the field for years and experienced other cases: but the Harper family were entirely ignorant of such matters and relatively uneducated, the only adult book in the house being the Bible, in addition to some magazines and children's books (121).

Yet, as has been mentioned, Mrs. Norton's interest in the

Manning article (122) does not seem to have been a secret, and the Barnes children's instant research activities have been mentioned above.

There is a minor clash of testimony between the account given in Playfair's book and Fallows' recollection concerning the first mention of the word 'poltergeist'. Fallows told me he said it when he - mistakenly - thought the children were out of earshot. Playfair describes a little exchange where Fallows says 'poltergeist' in the presence of Janet, who queries 'polka-dice'? and Fallows spells it out for her (123). Since in the BBC programme it is Grosse who mentions her 'polka dice' comment, perhaps Janet overheard Fallows use the word, and then asked Grosse who, among his many other kindnesses, helped her with her schoolwork. It hardly matters: someone spelt it out for her, and very early on in the case indeed, and the Barnes children took themselves to the library. As a matter of fact Grosse also, from the moment he took on the Enfield case, read whatever he could lay his hands on in the SPR library concerning poltergeists.

Quite apart from special visits to the library, there are films, the ever present TV and of course, as has been mentioned, the leading questions and unconscious but persistent coaching that would provide enough information for the - in my view pretty thin - performances put up by the children, such as the demands that Janet should demonstrate that she could not, under her own steam, jump or talk as she was supposed to be compelled to do under poltergeist influence.

No major intellectual feats on the part of any of the children need be presupposed: the ability to look up in a library, possibly with the help of a librarian, some unfamiliar term and find out more about it, is part and parcel of ordinary primary education; and few children fail to pick up, without benefit of formal education, how to get round, please, trick and score off adults, part of what is known as 'the hidden curriculum'. If the children actually absorbed something of the poltergeist literature, they could have, possibly innocently, 'coached' the adults around them as regards how to interpret what had happened and what to expect next. This would tend to coincide with the interpretations and expectations of their investigators,

thus reinforcing the latter's beliefs. It seems to me that Grosse and Playfair quite consistently underestimated the resource, enterprise and abilities of the children in the case.

It should also be obvious at least to anyone not deeply emotionally involved, that with quite so many children on the scene, and with the suspicion of being poltergeist focus falling on the Harper children in turns, this gives considerable scope for enterprise, information gathering and dissemination and for evading detection.

4.45 The 'voices'

The emergence of 'the voices', uttered by the children in turns, has been described and examples given of the quality of their communications (see p. 165 above). Hasted (124) described them as 'automatisms' in his letter to EPIC, although he was more cautious in a radio interview (125) in which he quite correctly pointed out the obvious fact that the laryngographs he apparently obtained could not in principle show whether the girls were conscious of producing the voices or not: such records merely showed that they did issue from the girls. Grosse seems particularly attached to the view that their paranormal origin is proved by their continuity of utterance without ill-effect on the children's vocal apparatus (126). Not only has neither Grosse nor Playfair presented any evidence for such continuity in spite of having it pointed out to them that this would be a pre-requisite, in the absence of prolonged and systematic scientific examination by appropriate experts it is hardly possible to analyse exactly what the children did with their vocal apparatus, let alone whether there is anything particularly remarkable about it (127). E. Grattan-Guinness, a trained singer and herself in charge of several choirs, thought there was nothing in the least paranormal about the voices (128). I have certainly heard others produce quite similar splendid imitations of the 'deep voices'.

In my view the Harper children were, at least sometimes, perfectly conscious of being 'the voices'. The little exchange given on p. 165 has been quoted to show how ordinary and unremarkable and schoolgirl-like was their content when they talked

to me: note for example the reference to my hairstyle which Janet might have felt too much of a 'personal comment' to make face to face. I have no doubt that the emotional tone of the conversations was at least in part set by whoever was talking to the children, and I of course deliberately kept it very calm and cool.

Grosse and Playfair actually insisted with some vehemence that the standard four-letter vocabulary employed by 'the voices' proved their spirit origin, a testimony, as I see it, to the sheltered existence the two investigators must have led to date, sheltered, that is, from modern children, schools and working class families generally.

Billy's 'demon bark' was done so openly and guilelessly that I find it hard to credit there was anything remotely unconscious about it, apart from the fact that it became somewhat standard and automatic. Normally his speech was virtually incomprehensible. The fact that when he was being a spirit his articulation was much better is not surprising. It is a well-known technique in infant teaching to let children with speech difficulties talk via puppets or imaginary characters: this sometimes greatly improves their ability to express themselves. Unlike Grosse, I find no difficulty at all in believing Dr. Bernard Carr when the latter reported hearing Billy in his room one morning referring to the investigators as 'silly idiots' (129). This may well not have been Billy's own considered opinion: indeed he did seem far too immature and limited to verbalise a sophisticated judgment, although he may well, as Hasted noted, be 'not stupid...observant' (130). It seems more probable that Billy was happily parrotting an older child, or possibly the incautiously expressed view of one of the adult Enfield observers to another, just as Janet when being 'the voice' relayed to me information I had myself provided earlier on the same evening.

The attempts to tape up Janet's mouth to show that the 'voice' did not emanate from her, even had they been efficient would have been beside the point, since no one watched the other children, notably Margaret who as has been shown above, took over from Janet as chief executant of 'the voice'. Since

hardly anyone seems ever to have paid any attention to Billy, the appearance of demonic barks on tapes etc. are no great mystery either. When Johnny came home for Christmas he too, as was shown above (p. 165), joined in.

It seems entirely clear that 'the voices' are not a paranormal issue.

4.5 Problems of role

Role has been defined as 'a pattern of behaviour that is characteristic or expected of an individual occupying a particular position within a social system' (131). Without considering the intricacies and controversies of role theory, it may be seen from the foregoing that issues of role conflict are particularly acute in the present context. Such clashes are far from abstract, involving as they do claims to competence and expertise, standards of integrity, duties as regards the education and welfare of persons especially children, freedom of research and publication, the education and entertainment of the public - a list far from exhaustive. In each respect different patterns are 'characteristic' or at least 'expected' from different individuals within the 'social system', and a state of affairs such as that at Enfield illustrates an almost Hobbesian state of war of each against all.

Before focussing further on the Enfield case and the roles occupied by various persons, it is important to consider the role and status of the Society for Psychical Research, however briefly. The nature of the Society's aims and objects has been described under 1.22, and the nature of some of the general problems to be discussed below foreshadowed under 1.73, 1.74 and 1.75.

The SPR was founded by scientists and scholars for the scientific and scholarly investigation of alleged paranormal happenings. However, it is an amateur organisation, admission to membership of which is by Council vote, based on the provision of two names as referees, and contingent on the payment of a subscription. The only persons explicitly debarred from membership are professional mediums. The Society in fact

comprises the most heterogeneous set of people imaginable, ranging from academics with years of experience in the field to members of the public who, for varying reasons and motives, are sufficiently interested in the subject to pay a subscription. The very fact that it is an organisation with academic-style aims but a non-selected membership makes not only for continuous conflict, only palely reflected in the minutes of 100 years of Annual General Meetings (see for example p. 54 above), it also renders particularly acute the chronic and ever-increasing shortage of money. Its aims and objects debar it from making the sort of lucrative and emotive appeals to the public which might relieve its ever-increasing excess of expenditure over income; at the same time, since there are virtually no permanent professional posts in the field, certainly not in this country, the number of people personally committed is small, and always in danger of being replaced by persons of very different concerns and convictions.

This very threat of having the scholarly aims submerged in the quasi-political struggles of keeping populists at bay, means that the meagre time available for research to those involved academically shrinks even more drastically. Nothing could more vividly illustrate this extreme practical point, namely the huge expenditure of time and energy necessary for preserving the character of the Society, than the events of 1978 to 1980, the tip of whose iceberg is preserved in the Report of the Working Party October 1980 in the Society's archives (132).

It is not possible to give further space to this matter here, beyond mentioning its relevance: it seemed a deeply regrettable necessity to members such as myself, in order to preserve the scholarly nature of the SPR, to relegate to second place personal research for well over a year, so as to put the Society's affairs in order. In an ordinary academic subject one can normally choose whether to engage in politics or research: in psychical research the subject as an organised scholarly activity would undoubtedly go under in this country if the handful of persons committed to its pursuit all took this line. The events alluded to pinpointed the acute role

conflict between those who see it as a primary objective to investigate and to establish authenticity and elucidate the status of the phenomena dispassionately on the one hand, and those particularly concerned to popularise, evangelise and write about them on the other. Who should count as an 'acknowledged expert'? Mr. Grosse's co-optation to this Working Party, to which he contributed effectively, was due to the demand made by Mr. Cornell, the Society's Hon. Treasurer, that another businessman in addition to himself, and a vocal representative of the ordinary members, should have access to the financial accounts and documents. Yet it would be difficult to imagine anyone more deeply out of sympathy with the stated aim of the Society 'to investigate dispassionately and in a scientific spirit'(see above p. 3) than Mr. Grosse.

To return to Enfield, at the very outset when Gary Norton experienced his night terrors (which must have been acute for a 21 year old working man to seek refuge in his parents' bedroom) no outside help was invoked (so far as we know).

However, once the Harpers were alarmed by noises of unknown origin, the question arose who was the competent authority to deal with whatever was going on? Vic Norton called the police as presumably the appropriate agency to deal with intruders and irregularities. The British police are notoriously unwilling to become involved in domestic cases, and that for good and sufficient reasons, and after their initial and ambiguous brush with the Enfield ensemble seem to have decided that it was not their business to deal with minor domestic vandalism by hands unseen.

The next agency invoked, this time by Peggy Norton, was the press, in the first instance the Daily Mirror. Interesting enough, according to Playfair (133, 134) the Mirror had an unwritten law tabooing 'ghost stories' dating back to the 1930s, when they are supposed to have hired Harry Price (see Chapter 2) as a salaried 'ghost chaser' and had found him a liability. There would appear to have been considerable trouble centering around the Mirror visits to Enfield, although I understand from George Fallows that his own relations with the family and with Grosse were harmonious.

On receiving Fallows' telephone call, the permanent Secretary of the SPR, Miss O'Keeffe, sent along Mr. Grosse, who had been wanting to go out on a case for some time, and who lives in North London within reasonable driving distance. As will be apparent from what has been said, the fact that the Secretary gives a name and address where PK phenomena are alleged to be happening may look like some sort of professional referral system, it is in fact nothing of the kind. Mr. Fallows had little option but to turn to the only source of help and advice available in this country, the SPR, and Grosse, the person sent, was on his own admission and that of Playfair, a total novice in the area of psychical research. Yet, to WPC Heeps, within under a week from Grosse's initial involvement in the Enfield case, he seemed an august and authoritative figure, 'Dr. Grosse of the Psychical Research Society', and indeed, he now feels himself to be, as he has frequently expressed, one of the foremost experts on poltergeist phenomena in the country, largely on the strength of his experiences at Enfield.

Thus Grosse and Playfair became 'the investigators', with some sort of prior claim on 'The Enfield Poltergeist', and indeed they spent a great deal of time at Enfield 'on a virtually full-time basis' according to Playfair's letter to the Parapsychology Review (135) in which he demands that 'She [Gregory] might also like to explain why none of her colleagues was able to undertake such a task at the time'. This is the one point in Playfair's attack on myself I decided not to deal with in my rejoinder (136), resisting the temptation to point out that my colleagues, being neither free-lance journalists nor company directors, cannot afford such a commitment in time. Although this is an element, and a very important one, the answer is far more complex and the problems even more intractable.

The two major sets of issues perhaps briefly labelled 'welfare' and 'research' might be illustrated by two sets of clashes that occurred at Enfield: those between Dr. A.E. Clark, Consultant at the Enfield Child Guidance Clinic, and Grosse and Playfair; and that between these two latter gentlemen and Dr. Carr, Mr. Cornell and Dr. Gauld already alluded to.

4.51 Example of role conflict concerning welfare

When the meeting at Janet's school took place on 6.10.1977, Grosse was also present, and he seems to have felt no incongruity in his re-iterated demands that Clark should comply with his, Grosse's, wishes and demands. Dr. Clark for his part (137, 138, 139) felt he could not be responsible for the family so long as Grosse and Playfair were a quasi-permanent fixture in the Harper household, and he rang up Miss O'Keeffe asking that the SPR should withdraw them from the case. Now this (apart from being impossible) was naturally enough bitterly resented by Grosse and Playfair, who seemed to have felt that between them they were not only authorised experts as regards authentication, but also that they had jointly taken charge of the family's welfare: they clearly felt Clark's demand to be not only insulting to themselves but also heartless towards the family. Clark, clearly a kind, concerned and competent professional not unnaturally resented the unloving description of himself thinly disguised in Playfair's book, but eventually decided he had better things to do than sue for libel.

The sensitive role relationships between doctors and patients, social workers and clients, teachers and pupils are of course the subjects of much conflict and controversy - they are at least also the subject of some regulation and a good deal of sophistication, awareness and often agonised self-examination and heart-searching on the part of the professional side of these relationships. This cannot be more fully explored here, it is merely pointed out in order to emphasise the contrast with a field which is not integrated into the accepted body of knowledge and convention. If people, wholly untrained and, from a professional point of view, undisciplined and on the face of it entirely unused to questioning their own motivations, enter into a disturbed social situation, particularly one involving minors, savage conflict is almost inevitable. From the point of view of Grosse himself his near permanent presence, his espousal of all Harper causes, his constant prodding of educational, welfare and medical authorities, his presents and benefactions, his constant advice, all might appear purely as generous kindness and benefit, freely

bestowed upon an indisputably deprived and distressed family. To an outsider, especially a professional, this plethora of activity on his part might present an altogether different and more mixed complexion.

At least one non-professional visitor to the Enfield household hints at reservations an ordinary person might have: E. Grattan-Guinness (140) says 'one wonders' what would happen if the family were left alone for a few weeks. Barnes and Mrs. Norton, whatever they may have said to Grosse in 1981, expressed much the same sentiment December 1977/ January 1978 to me (see above pp. 167, 170-71): they then felt certain that 'the investigators' were, by that time, keeping things going at Enfield. Even Mr. Fallows, although he retains a high regard for Grosse's personal kindness, tells me that since he has done some ambulance work since his Enfield experiences, he has come to question the wisdom of giving too much affectionate attention to persons displaying all the signs of hysteria (141). However, it must also be said from Grosse and Playfair's point of view that once they had, for good or ill, made themselves indispensable to this sad family in need, abrupt withdrawal was scarcely humanely feasible.

4.52 Example of role conflict concerning research

The other type of clash to which allusion has been made is research, rather than welfare, oriented. Carr, Cornell and Gauld visited the household and did not themselves experience anything they were inclined to consider paranormal, while leaving open the possibility that strange things might well have happened at other times. There is a minor ambiguity of testimony between Gauld and Playfair, in that the latter says at one point that (142) Gauld told him he heard someone going to the toilet upstairs, whereas Gauld merely stated that he had heard footsteps upstairs which he dismissed at the time as probably being those of someone going to the toilet (143). Since the house is a flimsy semi-detached structure it may simply have been the Nortons next door wandering about. The matter merely illustrates the germ of a myth.

There was clearly instant tension between the researchers,

Grosse apparently ridiculing attempts by the new visitors to take precautions and use instruments, failing to understand their point, and occasionally interfering with equipment (144, 145). Accounts here clearly diverge. According to Cornell, he, Gauld and Carr parted from the Harpers on the best of terms, the family trying to keep these new visitors in the house as long as possible; according to Playfair the visitors behaved so badly that Mrs. Harper took such a dislike to them she forbade them to return to the house (146). Although I am inclined to suspect that one of the three visitors may well have incautiously - quietly but not quietly enough - made the comment parrotted by Billy (see above p.198), and possibly overheard also by one of the permanent team, I myself decline to believe that an experienced senior trio of veteran researchers behaved in the clumsy and ridiculous fashion ascribed to them by Playfair.

One of the points illustrated is that it was not at all easy for researchers other than Grosse and Playfair to do any investigating. One was given to understand quite plainly that one was there on their terms, even by their invitation, and there can be no question but that intensive pressure was brought to bear to see and report things their way. My exchange with Playfair over the telephone has been described above (see pp. 163-64): he expressed undisguised contempt because I had concentrated on talking to Barnes rather than keeping vigil outside the girls' bedroom; he also expressed considerable anger that I would not retroactively change my account concerning my visit with Beloff. It may well be that Grosse and/or Playfair felt that I constituted less of a threat than Carr, Gauld and Cornell, or else my relations with the Harper family were just too cordial for Mrs. Harper to be at all likely to order me out, whatever might be said about me.

There are several sides to this. If a new investigator, who has taken no time or trouble, simply breezes in and out and pronounces ex cathedra that there is nothing in a case, this may rightly be resented as impertinence, and doubtless this may happen. However, I have no reason to suppose that any serious psychical researcher behaved in this manner with regard to Enfield. It is, however, the picture sketched by

Playfair in his book, not only of Cornell, Carr and Gauld, but also of, for example, Beloff and myself, and this in my view constitutes a serious distortion of our attitude and conduct, and a slur both on our competence and our concern. It is, however, a risk encountered by anyone who queries a claim to paranormality seriously championed by a journalistic writer, and one to which I have also been subjected on another occasion (147, 148).

Again, as shown in a past case (see Chapter 2) it may be seen, from a contemporary case in this instance, how hazardous and problematic an issue access to PK cases may become in practice: an 'invitation' by a researcher-in-possession to others to come and have a look is not at all the same thing as an invitation to engage in genuine collaboration, although it may easily be so mis-represented; and a refusal to acknowledge paranormality leaves one open to attacks on one's competence, integrity and humanity against which one may have little effective defence.

The problem of media exposure is also ever-present. Mr. Playfair would be less than human if he were not delighted that, as he told a Psychic News reporter (149), 'his book on the Enfield poltergeist received about "50 plugs" on the air'. Unfortunately by the same token, those concerned with establishing psychical research as a serious pursuit, who are likely to share my unflattering assessment of this book as expressed in my review (150), are bound to deplore such media promotion. There is ineradicable conflict between what Grosse and Playfair stand for on the one hand, and that represented by what they despise and resent under the heading of 'academic debate' - i.e. detailed, dispassionate and critical appraisal.

Thus researchers, but particularly professional persons involved in the investigation of spontaneous PK with its media and journalistic concomitants, may find themselves in double professional jeopardy, especially if their testimony is at all coveted and respected. Since psychical research is not officially accepted as a valid research activity, a declaration in favour of paranormality in a given case, indeed even the activity of investigating itself, may render a researcher

suspect to his own colleagues and 'the establishment'. Rejection or even querying of such a set of claims, on the other hand, renders the researcher vulnerable to pretty unpleasant attacks from those dedicated to defending their version of its paranormality. It is really not surprising that problems of investigating PK have attracted ever fewer competent researchers since the most elementary worldly wisdom would seem to dictate other areas of research endeavour.

4.6 Some conclusions

It is not possible to summarise adequately all the issues touched upon in this chapter; it seems useful, however, to list a number of emerging areas of concern.

4.61 Situational halo effect

In this case, the evidence for paranormality likely to impress any even moderately critical person, let alone a systematic sceptic, is virtually non-existent. It is conceivable that there may have been some PK irregularities which sufficiently upset the witnesses in the manner observed by McHarg (151) to lead to near-pathological consequences. It is also possible that the general penumbra of hysterical credulity acted in the manner of a Batchelder 'sitter group' and led to isolated real PK disturbances, such as conceivably Grosse's teapot and Barnes' dressing table drawer and some of the noises.

Whether or not such occurrences took place has now become unascertainable. However there is ample evidence, I would submit, for the creation of what might be called an extended, or social, or situational 'halo effect'. The halo effect is, in its more usual psychological usage, defined as 'the tendency to be influenced by a particular trait or over-all impression of a person when rating another trait of that person' (152), a tendency towards stereotyping against which teachers in particular are warned in their assessment of individual children's performance. Once a case such as the present one receives media and journalistic publicity, and individuals convince themselves and one another of the tremendous paranormality and

psychic significance of the situation, then there seems to come into being a tendency, especially by those closely involved and not sufficiently emotionally detached to ascribe paranormality to features of that situation irrespective of any objective justification. The case illustrates and underlines not only almost all of the cognitive pitfalls and problems indicated in West's 'Thoughts on the testimony to the paranormal' (153) but points to still further sources of possible error resulting from social, emotional and moral pressure brought to bear upon witnesses.

4.62 Role conflicts

In this case, as often happens, problems of welfare and problems of research may become confounded, and it is therefore desirable that these issues be considered separately as well as dispassionately. The problems of the roles of the psychical researcher and of members of various professions, of popular writers and representatives of the media and questions of relative and respective competence and expertise, need consideration. Clearly some degree of detachment and accountability is desirable. Quite apart from humane considerations, without some regularisation in this respect, problems of physical phenomena in 'spontaneous' contexts are likely to remain intractable.

This case shows with peculiarly vivid clarity the problems encountered in attempting to reconcile the roles of professionals and amateurs, both in respect of welfare and of research conclusions. In a subject without a recognised professional hierarchy or well-established standards of excellence, and where criteria of success are apt to be who can make himself heard most widely and enlist the support of the most prestigious champions, reason is an easy casualty.

4.63 Recording and supervision

The self-imposed disciplines of instant writing up and circulating notes, plus that of accounting to a critical but friendly and experienced 'supervisor', were of great value in preserving some subjective equanimity and objective perspective whilst making due allowances for my own personal feelings and shortcomings in what is, overall, a stressful and confused

situation. For example, the stress consequent upon fierce moral pressure to interpret situations as paranormal which I firmly considered to be no such thing was experienced as quite considerable. My own normal reaction to such stress would be to remove myself from the situation altogether, and to shrug off the whole issue; yet there were and are factors of various sorts prohibiting such an escape reaction. Support and understanding from an independent and experienced counsellor were certainly felt to be of great value. A system such as that usual in social work, or in teaching practice supervision, might be considered in the future administration of such cases. It is however very time-consuming and at times onerous.

4.64 Historical precedents

The case shares important features with witchcraft and possession cases in the none-too-distant past in European history (not to mention such incidence in other parts of the world) (154). It was on the strength of testimony such as that described in the Enfield case, and on the pronouncement of just such 'voices', that large numbers of innocent persons, men, women and children, have been tortured, imprisoned and brutally executed. Given a different cultural ethos and belief system, there can be little doubt that persons who, for example, did not fully subscribe to the spirit origin of such a case or had in some other way fallen foul of the main participants of the Enfield saga, might find themselves in desperate trouble after the manner of events in Salem (155) or of Loudun (156). A psychical researcher therefore bears the very serious responsibility of being engaged in undermining our modern Western belief-system that simply rules out 'superstition' a priori, and thus guarantees the non-occurrence of consequent atrocities and injustices. It is only by participating fairly fully in a case such as this, and becoming aware of the savage emotional forces unleashed, that it is possible to gain a measure of insight both into some of the psychological factors in witchcraft trials, and also tolerance towards the a priori sceptics of the Enlightenment (see Chapter 1) who, even if they were not always totally consistent, yet displayed to a high degree the civilised virtues

of balanced commonsense and compassionate humanity. Beyond issues of witchcraft trials there are here, all too clearly, echoes of more general problems in the sociology and psychology of religious belief.

4.65 Emotional intensity and social complexity

It will have become clear that not only are the emotional involvements intense, and problems of deepest pathology (physiological and social as well as psychological) apt to be highlighted and exacerbated, but that the sheer intricate complexity of the inter-relationship of people and events, often carefully hidden, is a time-consuming and also often quite distasteful, even hazardous matter. It is also a thankless task. Yet without such painstaking untangling of an intricate web of interactions it is impossible even to approximate to any kind of balanced outline picture, and without this there can be no worthwhile investigative psychical research of spontaneous cases.

CHAPTER 5, MATTHEW MANNING

5.1 Introductory contacts

My original contact with Matthew Manning (subsequently called Matthew) was indirect and took place in 1973 when he was 17 years old. I was approached by a firm of publishers, Messrs. Colin Smythe Ltd., whose directors asked me to 'take under my wing' a brilliant new young psychic they had discovered. I received the impression that they wished him to be both protected and investigated prior to a book by or about him, to be published by their firm. I understood that his main talent lay in producing very rapidly remarkable drawings in the style of numerous well-known dead artists, and writing messages in a dissociated state purporting to come from other illustrious deceased persons, often in obscure foreign scripts and languages. It was thought that I would be a suitable mentor for him, particularly because of my contacts in the world of education with whose help I might be in a position to explore the degree of accomplishment and competence normally to be expected from a secondary school boy of a given age, automatic drawings having started two or so years previously. Indeed, I took the first steps of asking in principle for the assistance of my Head of Department, Mr. S. Jones, and that of the Head of the Art Division of this Department, Mrs. M. Clarke.

However, nothing came of these preliminary overtures which turned out to be my first practical introductions to the problems of investigating a young supposedly psychic star subject. In the process of discussions with the publishers I clarified what, as I saw it, were the necessary pre-conditions for my working with Matthew, and these turned out to be entirely impractical from their point of view.

My first request was that before I should sign anything I would need to meet both Matthew and at least one of his parents. I had been told that he intended to study a social science subject, probably psychology, and I considered that he should be put fully in the picture as to what might await him

if he attempted to gain a university place, especially in psychology, if his name had been promoted as that of a psychic star. I wished to put to him the desirability of keeping anonymity under a nom-de-plume; I further felt that he and his family needed to consider his long-term future, and to weigh up very carefully the possibly conflicting demands of academic and professional education against the life of a psychic, especially a professional one. The age of 17 seemed (and seems) a particularly crucial one in this context. Moreover, as a researcher I stressed the importance of conducting experiments and investigations before the publication of the projected book which would otherwise have no research underpinning and consist merely of a number of unsubstantiated claims.

These pre-requisites on my part turned out to be entirely unacceptable to the publishers, understandably enough: my insistence on bringing to Matthew's attention the advantages of anonymity and amateur status clashed head-on with their need for publicity and promotion of their new young author; and my demand of prior investigation was clearly an intolerable interference with their time-scales and deadlines: they viewed Matthew as their contact and their property, and expected me to accept their terms and sign their contract before ever having met Matthew. I knew neither his address nor the terms offered to him nor his father's first name. I declined to sign my contract (1, 2).

Until 1 March 1978, when Matthew wrote to me, I had in fact never met him in person. I had seen his book The Link (3), a copy of which he later presented to me with the inscription 'The book you nearly wrote with me'. I had also reviewed his second book In the Minds of Millions (4),(5)*. His 1978 letter, so far as I was concerned, came out of the blue. He told me he no longer had any contact with his erstwhile publishers, that perhaps not all he had heard about me was entirely accurate, and suggested that I should arrange for experiments to be conducted with him. I organised an investigation for later that summer, by kind invitation of

*attached to this thesis (Label J)

Prof. Ellison, at City University.

5.2 Sources of biographical information

It was not until I came to write up the City University experiments that I pieced together Matthew's earlier biography. My reconstruction is partly based on conversations, most of them on tape, with Matthew (6), and with his parents (7), and also on Matthew's books (8, 9, 10). Since the completion of the relevant section of the SPR Proceedings containing an account of the City University experiments (11)*, Prof. D.J. West has kindly lent me copies of letters and notes concerning Matthew by himself, by Dr. A.R.G. Owen, and by Mr. Derek Manning, Matthew's father. Mr. A.D. Cornell who also had earlier contact with Matthew and the Manning family than I, has kindly given me his recollections. Appropriate indication of sources is given in the reference section (12).

5.3 Biographical outline

Matthew was born on 17 August 1955, and is the oldest of three children, his sister Rosalind being three and his brother Andrew five years younger than he. His mother said on 12 November 1978 that she had, whilst carrying Matthew three months before his birth, suffered an electrical shock so severe that the doctor was angry with her for the risk she had taken to the life of the child. Yet strangely enough in the article in Woman alluded to in the Enfield chapter (13) Mrs. Manning had particularly drawn attention to the normality and uneventfulness of her pregnancy. Matthew's own earliest recollection is sitting at the age of 18 months in a high chair by some french windows (which details apparently fix his age) 'refusing to eat chicken because it was cruel to eat birds' (14). (Matthew is not a vegetarian.)

According to his mother, he day-dreamed a good deal as a young child, a view confirmed by the head teacher of his infant school, who said that 'Matthew always had his head in the clouds, he's miles away' (15).

*attached to this thesis (Label K)

Matthew is generally held not to have displayed any talent for drawing at school, although there seems some doubt about this (see below p.221). The only religious feeling he could recall was 'anti-religion' and he says he never had any religious faith. The family are not religious except in a very generally conventional way. Matthew considered being confirmed a waste of time (16).

So far as any member of the family could recall, his childhood was on the whole unremarkable and uneventful, and only three episodes occurred which might possibly be viewed as traumatic.

When Matthew was seven or eight he was cut off by the tide during a seaside holiday: more than anything else, he recalled 'being surrounded by fishes' which frightened him. He said he had had recurrent nightmares of being cut off by water with fishes swimming in it ever since (17).

When he was ten or eleven, a boy died at his prep school, which he feels affected him deeply. Interestingly his parents cannot recall his ever having told them about this, and it seemed news to them on 12 November 1978 when the conversation took place (18). Matthew dates some more night visitations to this episode (19):

I'd suddenly wake up in the middle of the night and I'd suddenly see somewhere, probably within three feet of me, though it's difficult to gauge, the distance in the dark, I'd see a disembodied face somewhere close to me and I knew that I was awake because if I close my eyes the face disappears but I get a kind of direct communication between whatever it is straight into my head, and I can communicate with it. Some of the faces I recognise, some I don't.

The third incident that took place when Matthew was ten or eleven was contributed by Mr. Manning, Matthew's father, an architect. Andrew, Matthew's younger brother, had done 'something' that particularly incensed their father - it is not clear what the crime was. Mr. Manning eventually recalled that a tray had been thrown downstairs with such violence that it broke a plate glass window. Matthew had totally forgotten the incident, which is interesting. However, when his father recalled the smashed window, Matthew immediately

thought he remembered that Andrew had merely slipped. Mr. Manning was certain it was deliberate (Mrs. Manning contributed that it was 'just stupidity'). Be that as it may, Mr. Manning says (20)

I belted [Andrew] for all I was worth. I think I really did lose my temper and perhaps lost control of him and myself as well, but Andrew was remarkably resilient and it went over him, and he came up smiling and he's beaten me at the end of it. But Matthew nevertheless I remember was extremely distressed as if it had all happened to him and he's experienced it personally, and I have never seen anybody so distressed as Matthew at the fact that his brother was given a hiding.

Mr. Manning has evidently continued to feel over the years that somehow this incident caused Matthew quite traumatic distress. Be that as it may, the onset of the first lot of poltergeist phenomena must have roughly coincided with this incident. The date for the onset is given by Matthew as 18 February 1967 (21), when he was 11, his brother 6 and his sister 8. A favourite tankard of Matthew's father's was displaced from a shelf, and a vase of flowers on the table moved in front of the mother's place setting - no normal causes were discovered. Phenomena increased in frequency and intensity. Knocks and taps, creaks and a 'batlike pinging' were heard in the modern detached house, doors would open and shut and objects of varying size flew about. At about Eastertime 1967 a mislaid india rubber that slowly rose in the air and gently floated down beside its owner, Matthew's sister, seems to have been the only incident to inspire terror during this phase: it also marked the end of manifestations for the time being.

According to Dr. A.R.G. Owen, in a letter written in 1971 (22, 23), he confirmed that he had been of the opinion that the original phenomena at Shelford had been genuine,

and not caused by trickery, that the house was not haunted. It was not possible at that time to ascertain which member of the family was the source of the force. Matthew was the most likely on the grounds of age, though there was no other factor specially to indicate this...At one time I wondered if Mrs. Manning was the unconscious involuntary source of the force...but this was only because she was the

one who seemed outwardly to be the most tense. However, in these things it is obviously hard to separate cause and effect.

After the cessation of phenomena in 1968 the family moved from Shelford to a beautiful house in Linton near Cambridge, built and extended between 1550 and 1730, where there were no consistent paranormal incidents for some time although it is not entirely clear that there were no disturbances at all. (In his third book Matthew wrote that the family had become accustomed to 'odd happenings' ever since they moved house (24).) Also in the winter term of 1969, there was a 'craze' for seances at Matthew's public school, Oakhampton in Rutland (25), which he had moved to in 1968 after taking Common Entrance examinations. He was under the impression that phenomena were more prolific when he was a participant (26), but the seances seem to have been abandoned when, as often happens in schools, all concerned became scared.

However, disturbances started up at the Linton home in July 1970 and gradually increased in intensity. There were footsteps and raps, opening of cupboard doors, books were thrown about, cushions took on a life of their own, and so forth. This time, the manifestations clearly centred around Matthew, but he says that his parents for a time either were not interested or declined to believe him (27). However, according to his father (28) Matthew had not told them - or himself at any rate.

Matthew who is normally at School had apparently been 'uneasy' about his room during holiday time and had endured odd noises etc. for several months without reporting this to us.

He now says he took a deep dislike to an 18th c. wardrobe which we bought last summer for his room. His objection quite irrational, was that it was in two pieces. It also developed a habit of opening its doors spontaneously. On one occasion he watched this happen, and the departure of his slippers from the wardrobe. These floated around the room and he managed to photograph one which, he says, was in flight.

These occurrences had been intensifying and at Eastertime my wife reported that Matthew was distressed by the situation. I had a long talk with him and tried to tell him not to worry, that no one could help these 'alleged' happenings and no one was to blame.

Unfortunately my attempt to help proved disastrous -

perhaps because I had made the situation 'respectable' for Matthew.

The following morning, when we had all come down to breakfast, the living room was in complete disarray...From that moment the entire house was subjected to the most violent movement of furniture. Valerie was going round the house hourly setting the rooms in order.

According to Matthew, he went to his parents' bedroom during Easter 1971 (29) and refused to sleep in his room any longer 'until something was done about it'. The next day the dining room looked 'as though a bomb had hit it' (30).

Mr. Manning wrote to Dr. Owen (31) that there seemed to be a pattern:

...Matthew remarked 'It hasn't done any wall writing'. The day following curious scribbles began appearing based upon figure eights and circles...Matthew says he actually saw one as it was being produced. He could hear the scratching sound but there was no pencil visible!

Shortly after that episode, Matthew again remarked 'We haven't had water through the ceiling yet'. By now you can guess the result. Pools of water appeared in the hall but with never a tell-tale trail...

It became apparent too that Matthew had a partial 'control' over these events. At lunch one Sunday he said, 'I am going to tell it to move Rosalind's bed and your bed, and Rosalind's chest of drawers'. He wanted us to leave the table at once to confirm this, but we did not do so...Eventually after the meal we proceeded upstairs and the 3 articles which he had mentioned were indeed moved.

It does not seem to have occurred to Mr. Manning (or for that matter to Dr. Owen) to invoke normal explanations for these predictions. Indeed Mr. Manning wrote (32) that he personally seemed to sense (but no one else did) energy waves being emitted by Matthew as he passed from one room to another.

In the summer term of 1971 the disturbances followed Matthew to school, where there was genuine concern that the upheavals in the dormitory could endanger fellow pupils' O level studies. As Mr. Manning wrote (33) to Dr. Owen (who had left his fellowship at Trinity College to set up a foundation in Canada, the New Horizons Research Foundation, for the investigation of psychic phenomena (34)):

During the remainder of the week the Headmaster was in daily communication with us, reporting the bizarre happenings and the terrorisations of the dormitory...

[after consulting a Mr. George Bowlen, 'a medium and mystic of some calibre' who was employed in some not specified capacity at a mental hospital and tried to help and advise Matthew, and who claimed to have the most remarkable powers including the ability to write in Chinese without having been taught this, and warned Matthew that a complete mastery of the situation might take six months, matters intensified.]

At 2 a.m. the boys in the dormitory were awakened by an intense light about three feet high, circular in shape with a cross at the centre. The wall upon which this shone became warm and the room extremely cold. One boy who beheld this fell - or was pushed - out of bed and was in such a state of extreme fear that he was unable to stand. Half an hour later he was found in the corridor kneeling in prayer outside the Matron's door...

Matthew was going to be told to leave the following day. We were chatting to Matron when the phone rang. It was Bowlen with a message for Matthew. The Headmaster had changed his mind and Matthew would not have to go.

This indeed proved to be the case, and in the days which followed some return to normality commenced. About 150 nails appeared mysteriously and also 9 old-fashioned knives, also of unknown origin were thrown in the dormitory...

In the three weeks which have ensued, Matthew has 'developed' with amazing rapidity. He claims to be completely telepathic (in contact with Bowlen) capable of mind reading, able to control the behaviour of others and 'turn on' an amazing range of phenomena....

In the same letter (35) Mr. Manning told Dr. Owen that for the last six months (my italics) Matthew had been working for his history O-level on a project on the Webbe family which had occupied the Manning home from the late sixteenth century for 200 years, as evidenced by various inscriptions. Matthew's research

...was based upon records in the Cambridge Records Office and the work was considered worthy of a prize - Matthew's first which pleased him.

With his newly acquired gift he has been anxious to test the veracity of his work...He had been in trance the previous night and talked with some of the previous occupants as far back as 1601.

He has gleaned a number of fresh facts and confirmed others...from the hall he looked into the dining room at...the furniture in position - furniture of a style which Matthew identified in a book as late 17th century or early eighteenth century.

He also told us that the week previous he had decided

to 'go to Crete' as he knew nothing of the island. On his return he wrote a description of what he had seen, and which another boy corroborated from books...

In May 1971 Matthew had his first experience of 'hearing' a spirit voice - one 'Henrietta Webbe' - and in June 1971 he first found himself writing automatically in a handwriting quite different from his own (36). This habit developed and the writing purported to be from numerous deceased persons. These were originally unknown, although 'they' signed their name. However, Mr. Manning (37) suggested his son should try to 'contact'

a famous person and suggested Churchill. Imagine our surprise when he wrote out a message about tanks coming, get Eden, and the Luftwaffe invading! In the middle of this, strangely, was inserted a very domestic comment about having a nap and not to be disturbed for 30 minutes. Churchill I believe had this capacity to sleep at will.

I enclose copies of some automatic writing by Samuel Pepys, Cromwell and Queen Anne...

As so much effort has been put into retrospection, I thought it would be interesting to look ahead. At my suggestion, therefore, Matthew 'contacted' my grandfather who was a great racing man. He asked for the names of the winners at Ascot on 23rd July. No one in our home had looked at the racing page in the newspaper, and three hours before the first race Matthew was 'told to try' 6 horses. Each name given was that of an actual horse and produced: 1 winner, 2 second place, 2 third place, 1 unplaced. I do not know whether Matthew should engage in such activities but the results are fascinating. If only the effort could be channelled and controlled, the potential seems immense!

It is particularly interesting that apparently by means of deliberately developing his automatic writing Matthew was able to attenuate and eventually abolish the poltergeist manifestations, whatever they might have been.

On 2 November 1971 whilst Matthew was at home for half-term, his mother suggested he should try his hand at automatic drawing (38) and call for inspiration on the spirit of Sir Alfred Munnings: she thought Matthew's limited skill as an artist would ensure that by his own talents he could not draw a good horse. He did produce a horse of apparently no great artistic merit, but still beyond his own abilities. This started a new phase, Matthew's automatic drawings in the style

characteristic of large numbers of deceased artists among them Dürer, Picasso, Beardsley, Keble-Martin, Rowlandsön, Leonardo, Beatrix Potter, Goya and Klee. The drawings certainly look accomplished and were apparently done at some speed. These continued until 1975, and have since ceased.

Later in November 1971, Matthew had his first 'visual' encounter with a member of the Webb or Webbe family (39). According to Matthew, the vision was different from how he had described an eighteenth century gentleman in his school project, and this convinced him that what he had seen was 'no mere hallucination'.

On 25 September 1971 Owen wrote to Mr. Manning and suggested that Dr. (now Professor) D.J. West might be of help (40). The following letter from West to Owen is self-explanatory (41):

Following my meeting with Matthew last term, I wrote to him suggesting some distance ESP making use of automatic writing. He did not reply until 31 December when he wrote that he was 'not prepared to do [that] at the moment'.

Meantime, as I was to be abroad for Xmas, I arranged for Prof. Rushton and Prof. Graham Hough (English) to see Matthew during the Christmas vacation. He was entertained at Trinity and produced some reports of new phenomena, the chief one being automatic drawings in the shape of reproductions of well-known woodcuts. Rushton was superficially impressed, but Hough who went into the question further, noting Matthew's normal drawing abilities, was totally sceptical...it doesn't seem at all promising. Matthew seems to be a great romancer, and quite elusive when you try to pin him down to anything concrete...

It would also appear that around the same time the BBC attempted to contact Dr. A. Gauld and Mr. A.D. Cornell concerning Matthew. Cornell tells me that he made a preliminary visit by himself at a time when, as it turned out, the parents were away. He was entertained by Matthew, Andrew and Rosalind who seemed to be impressed with the 'impossibility' of transporting a particular table from its normal location to the cellar where it was 'discovered'. Cornell says it took him less than a minute to demonstrate how it could be done quite easily and very normally.

Cornell, like myself, was deeply impressed with the exquisite nature of the house, its contents and superb antique

accoutrements. He was struck by the readiness with which Mr. Manning senior accepted as paranormal Matthew's supposed psychic feats in tracing the origins of antique furniture and porcelain, when the house contained reference books where this information could easily be gleaned. He also made a number of other observations, suggesting that there might have been nothing so very remarkable about the appearance in the house of assorted pages of an old book, if the book had been in the house in the first place.

Cornell lent Matthew a tape recorder for 'Raudive experiments' (42). He also tried to 'contact' Sir Oliver Lodge via Matthew's automatic writing, but the script made no sense at all, especially as 'Lodge' wrote he did not know Cornell, whereas Cornell was involved in some experimental seances elsewhere at which 'Lodge' was purporting to be in attendance. According to Cornell Gauld, who after some time, also met Matthew, was particularly unfavourably impressed. It would appear that Matthew at that point proposed to study psychology in Dr. Gauld's department at Nottingham University.

Mr. Rodney Tibbs of the Cambridge Evening News discovered some strange historical errors in Matthew's supposedly ancient scripts, the fact that Matthew had won a school prize for art, and also that he had written a Webb project for his O-level. Now as pointed out earlier, the dating of this project was known to Dr. Owen, to Matthew's school and family. Tibbs told me (43) that he was able to trace numerous signatures found on Matthew's walls to the documents in the Cambridge Records Office - if and when, that is, he himself could gain access to these, since Matthew was monopolising the same documents for much of the time! Tibbs was also told by a curator at the Fitzwilliam Museum that the celebrated artistic drawings by dead artists bore all the hallmarks of the classical fake. Be that as it may, Tibbs suggested that Matthew should work with the Cambridge University SPR, which is directed by Cornell, a recommendation which Matthew dismissed with the utmost contempt.

At about the same time Mr. Manning senior made a special appointment to see Cornell alone (44), and expressed anxious doubts about the authenticity of the phenomena and Matthew's

part in them.

As already mentioned, early in 1973 Matthew made contact with Colin Smythe Ltd. and decided not to apply to any university or engage in any professional training, and instead became in the first instance, a writer about his own experiences. His first book called The Link (its working title appears to have been Open to Suggestion) contains an Introduction by his father, beginning with the sentence (45)

Shortly after the war I read Harry Price's book Poltergeist Over England...The narrative which follows has an added immediacy because the victim (and I believe any poltergeist child is an unwitting victim) is in this case my son. I am grateful for the information I obtained and remembered from Price's book: without it the phenomenon [sic] which began to uncoil might have remained unrecognised far longer and the anxiety it provoked have been more serious for my family.

From January 1974, in the wake of Uri Geller's impact, Matthew added metal bending to his psychic repertoire. As described largely in his second book (46) he became a sort of migratory psychic star subject, stop-whistled across the globe, at odds with most of the researchers who tried to work with him.

Meanwhile it would seem that the experiences had not left Mr. Manning senior unscathed. On 17 December 1974 he had a most remarkable set of alarming hallucinations (47), in which he felt himself, whilst apparently awake in bed, to be inside the body of Robert Webbe, looking out across the latter's teeth, and of his own body being plastic and varying in size. He threatened the vision: 'Go away, or I shall tell Matthew!' and repeated the latter's name over and over again, whereupon at last 'Webbe left the bed'. The way this is recounted by Matthew, events of this sort seem to have become quite a frequent occurrence in the Manning household by that time.

In 1977, whilst in the Himalayas he had an experience which deeply impressed him (48), whilst looking at the mountains and watching the sun rise.

I realised how completely unimportant I was physically, how transient human life is...I just felt a tremendous feeling of harmony and unity. And I just...felt some Presence while I was up there that told me what I should do and what I shouldn't do. In fact, I should do what I wanted to do and not what other people told me to do.

In the course of the City University investigation in the summer of 1978 I asked Matthew what he now proposed to do with his life (49). Matthew said he did not yet know but that Harry Edwards had just died, so there was now room for a great healer. Indeed, this is what he set out to become. By summer 1979, when I referred, in the course of a class at the Polytechnic where I teach, to my experiments with Matthew Manning, a student asked 'Do you mean the healer?'. Just over two years later he was sufficiently established in that role for the Sunday Express Magazine (50) to carry an article 'A day with healer Matthew Manning' - 'As a boy he was linked with poltergeists and uncanny writings by the dead. At 26 he uses his amazing powers for healing. Anthea Courtney describes him at work.'

Matthew married a friend of his sister's, Christine, in the summer of 1982, and now lives in a farm house in Suffolk. He works as a healer, lecturer and general promoter of a spiritual way of life, and is widely revered as quite a cult figure. He has become a very accomplished speaker and runs a business jointly with his brother Andrew, selling tapes made by himself, intended to promote healing, Matthew Manning Cassettes Ltd. Indeed, he seems to be the moving spirit in a prospering family business marketing himself and his products and as he told me, 'goes to the office' when he is tired of healing.

He now makes few, if any claims to paranormality, and says 'I often tell those that I am working with that what I am doing for them, they could just as easily do for themselves' (51).

In a post-script to 'London experiments with Matthew Manning' (52) I referred to these cassettes and made the (doubtless tactless) remarks:

If it is really the case that, as he now says, he does nothing more for people than they can do for themselves, he has presumably no special cachet as a psychic in promoting health. He has adopted the roles of teacher, counsellor, and therapist, and is in competition with countless other practitioners, orthodox and alternative, physiological, psychological and spiritual, all of them purporting to help suffering humanity.

I suggested (53)

...it seems to me just about conceivable and worth

exploring that if he (or any other healer at least of his style) is therapeutically effective over and above normal suggestion and chance factors, this could be due to a two-stage process: that he could have a disruptive effect on an organism, probing its weaknesses and disabilities...and that, by then suggesting soothing as well as likely positive images, the patient is occasionally able to cure himself, having been (paranormally?) shaken out of prior pathological adjustment.

Within less than a fortnight of the appearance in print of the over-80-page paper devoted to himself (54) at a meeting chaired by myself, on 9 December 1982 (55), Matthew told the audience that perhaps the fact that people often experience a temporary set-back after initial attempts at healing but before recovery has commenced, could be explained in terms of the view I had suggested in that paper. This is mentioned here to give some indication of his alertness and intelligence. At the same time he disarms objections to his views on reasoned grounds by saying that he cannot answer that: 'I am not an intellectual'.

After the meeting Matthew was friendly and forthcoming, and talked with evident enjoyment about his various activities, expressing particular satisfaction with the fact that he had never gone on to psychology which, as he said, he was now practising anyway without benefit of university training.

I asked after his parents, and he said he no longer saw much of them: he himself no longer experienced any poltergeist disturbances but they still reported odd happenings at their home.

Table 32 presents a sketchy chronological record of some relevant events in Matthew's life up to the City University experiments.

5.4 Overall setting of City University experiments

The investigation at City University was arranged for the period of 24 July to 4 August 1978.

Prof. A.J. Ellison offered us his cooperation and that of his technical staff, as well as his hospitality at the Bio-Electricity Laboratory in the Department of Electrical and Electronic Engineering at City University, London .

Date	Event	Source
Pre-natal	Mother's electric shock during pregnancy	T1 (56)
17 August 1955	MM's birth	Pers comm
Summer 1961/62	At 7 or 8, MM cut off by tide	T1
1965 or 1966	Age 10 or 11 boy died prep school	T1
1966/67	Age 11 or 12 father belted Andrew	T1
18 February 1967	Onset phenomena; A.R.G. Owen called in; date not quite certain, MM 11, Rosalind 8, Andrew 6	L15 corr (57)
Easter 1967	Rubber episode ends this bout of phenomena	L24
1968	MM takes Common Entrance; family moves home; MM to public school	L26 corr (58)
Late 1969	Craze for seances at school	L26
July 1970	Onset recurrence of home phenomena	L28 corr (59)
Christmas 1970	Increase home phenomena	L33 corr (60)
Winter/early Spring 1970	MM compiling Webbe family project for 0-level project	L83 corr (61)
Easter 1971	Violent phenomena; MM's bed moves, room 'as if bomb hit it'	L34/36 corr (62)
Summer term 1971	Disturbances start at school; violent disruptions	L41 corr (63)
May 1971	Weekend at home; first communication with 'spirit' entities, 'Henrietta Webbe' voice	L59 corr (64)
June 1971	Automatic handwriting, different from own; Virtual fading of poltergeist phenomena	L62
June 1971	Ostensibly successful ESP	L66 corr (65)
June/July 1971	Takes 0-levels	
June 1971	Automatic writing characteristic of 'Robert Webbe'	S16 corr (66)
June/July 1971	Father suggests contacting Churchill	(67)
31 July 1971	Names on wall begin	S20 corr (68)
2 November 1971	Half term; automatic drawing suggested by mother	L92
November 1971	Vision of 'Webbe' on stairs	S11 corr (69)
Some months after	MM discovers old school project with	S15
November 1971	different description of 18th C gentleman	
1972	Various phenomena, including apports, automatic drawing, communications	e.g. L120
Early 1973	Contact Bander and Smythe	
June 1973	Projected outline of MM's first book to AG	Corr (70, 71)
January 1974	MM bends metal following Uri Geller	L141
Summer 1974	MM in Toronto	M1
17 December 1974	D. Manning 'Webbe' experience	S9-11
April 1975	MM in Netherlands	M88
Summer 1975	MM in U.S.	M96
Autumn 1975 (?)	MM in Freiburg	M16
January 1976	MM in Sweden	M127
June 1976	MM in Japan	M145
15 May 1977	MM 'compelled to communicate once more with Webbe' after completion of archival research	S120
Summer 1977	Himalayas experience	A1/1
1 March 1978	MM writes to AG	Corr (72)
24 July - 4 August 1978	City University experiments	(73)

Table 32. Rough chronology, T1 = taped interview, 12.11.1978; L = Manning (1974) (74); M = Manning (1977) (75); S = Manning (1978) (76) followed by page number; A1/1 = first side of first tape during investigation; Corr = correspondence; numbers in bracket see reference section.

When Matthew came to see me in March 1978 he expressed a desire to repeat some of the biological experiments in which he had just taken part in the U.S. (77, 78). He felt that at that stage he was more likely to succeed in influencing organic rather than inorganic matter. However, when we met and discussed a possible investigation he agreed to try whatever I could set up. I explained that my own major interest was in attempting to replicate the Osty/Hope experiments using the infra-red radiation with Rudi Schneider (see Chapter 2) and he readily agreed to cooperate. In fact at just about that time Mr. Brookes-Smith's instrument was ready for use, and Dave Line (see above p. 169) soon afterwards helped me to instal it in my own home.

I asked Matthew whether there were any other accomplishments he would particularly like investigated, and he asked me to devise some experiment to test an ability he felt he had acquired recently to draw pictures which were unambiguously relevant to, or illustrative of, a poem as a paranormal target.

Matthew lived some distance from London, near Cambridge, and at least at that stage preferred an intensive period of investigation rather than regular trips to London. As I also found that it would in practice be easier (or less difficult) to assemble a team of experimenters for a fortnight during the summer vacation, rather than to persuade them to commit themselves for a regular session over a longer period in term-time, it was decided to set aside a block of a fortnight.

A number of general issues arise out of an investigation of this type, relating to the organising of experiments in this manner, others to questions of authenticity, and meaning of results.

5.41 Block type of investigation

As it was found, there are advantages in organising a number of experiments over a concentrated block of time, involving several experimenters and different activities, and there are also drawbacks. The block format is more suitable for the investigation of a star subject with limited time, especially one living at a distance, than for the experimental investigation of a trait or ability in the general population.

It was also useful to have a number of diverse participants present, often at the same time, in order to witness each other's

observations and to make suggestions and corrections, and to complement each other's repertoires of competence. It was only possible in this way to arrange for cooperation between workers from different distant locations and fields of expertise.

On the other hand, assembling a team of participants with widely different backgrounds and commitments meant considerable disparity of approach, attitude and time required and devoted to evaluation of data and so forth, and there was never quite enough time to put into effect important suggestions for improvements. Also the administrative workload was substantial.

These different rather practical considerations need to be balanced against each other in the circumstances of any given set of experiments.

The questions arising out of this kind of experimental organisation are of course by no means purely administrative, since the spacing and timing of experiments and the presence and absence of different participants may well have characteristic consequences on the results obtained.

For Subject and other participants alike the setting aside of a short but quite substantial block of time such as a fortnight did indeed highlight the investigation as a special event, and generated a sociable group mood of fellow feeling and hopeful expectancy widely believed to be psi-conducive, especially in the case of physical phenomena. The team located in this case in a laboratory, replaced the home seance circle or the sitter group.

5.42 Laboratory setting

Although intangible and yet possibly vital factors such as group mood are hard or impossible to control, at least the laboratory setting and some form of continuous instrumental monitoring makes it possible to arrange for a high-spirited mood not to interfere too much with the collection of hard data. The permanent auditory record, for example, means that it is possible subsequently to check at least what was vocalised at any given time. Despite gaps and shortcomings in the recording and collection of data which will become apparent, a good deal was learned about the conduct of future experiments, and it is of course also valuable to capture the Subject's and other

participants' reactions at the time.

One of the most important features to emerge that cannot be stressed enough is that it is essential to have as complete and continuous a set of hard and preferably automatically synchronised automatic records as possible. Subsequent evaluation will depend entirely on the quality of these records.

5.43 Relative degree of informality

The assortment of experiments was determined by a compromise between the wishes of the Subject, and those of the various experimenters. Moreover, a degree of flexibility and informality was deliberately built into the time-tabling, so that if Matthew became particularly interested in, or was particularly successful at, any particular activity this could be incorporated into the schedule. The advantages of flexibility and some degree of spontaneity are obvious. The audio record certainly bears witness to a buoyant and active group mood at times when some ostensibly paranormal results were being registered.

By the same token, however, the greatest caution must be exercised subsequently when assessing results. The block type of investigation featuring a high degree of flexibility and informality is more likely to provide a prima facie case for selecting future areas for experimentation in depth than hard and definitive evidence. It may certainly militate against a rigorous methodology in terms of experimenter-initiated trials and controls for some of the activities although, as has been argued e.g. by Grattan-Guinness (79), the whole notion of a control object (or by implication a control period) is highly problematic in parapsychology in any case.

Among the other difficulties encountered arising out of the relative informality and flexibility were the following: investigators as well as the Subject may be misled into believing that impressive results have been obtained when a dispassionate subsequent scrutiny of the record shows this not to have been the case, and this leads to a good many problems later. Humouring and accommodating a difficult Subject over a prolonged period may be disproportionately trying for experimenters. The subsequent work of analysis, in this case transcription of the total audio record, of seven to eight days' of sheer talking by

two to fourteen participants, is a mammoth task which may be barely practicable, especially if this has to be plotted against some additional recording. A certain lack of hierarchical ordering may lead to friction among experimenters, some of which may be exploited by the Subject to establish his ascendancy; also sheer chaos may at times ensue as to which experiment is to take priority.

It emerged clearly that (apart from the paramount importance of hard recording) a good deal of contingency planning would in future be necessary for this type of experimental investigation, involving the preparation and agreement beforehand of emergency protocols and the appointment of an overall emergency experimenter and the careful prior definition of other roles. Preliminary briefing of participating investigators as well as the Subject concerning some of the dangers mentioned would clearly be desirable, the more so as specific debriefing at the end of the period is apt to be difficult, partly because results are not yet available, and partly because of the tensions and exhaustion that are likely to prevail at the end of such a prolonged period.

None of this would eliminate the difficulties, but they could be lessened by it.

5.5 Introduction to infra-red experiments with Matthew Manning

The use of infra-red radiation as a detector of psychokinetic activity was, as has been described in Chapter 2, introduced almost accidentally by Dr. Eugène Osty (80). Osty had attempted to monitor the target objects to be moved paranormally by Rudi Schneider, and in the process had ostensibly discovered that when the medium's secondary personality 'Olga' said 'she' would try to move the target objects, there would be partial occultation or attenuation of the infra-red (IR) beam. Eventually 'Olga' was asked to go into the beam' and apparently systematic quasi-voluntary partial eclipsing of the IR was thus produced. Similar experiments were conducted with the same medium under the auspices of Hope and Rayleigh (81), Price (82) and Schwaiger (83) with similar results.

It will be remembered that the effects were obtained at

the very end of Rudi's mediumship, when most of the other phenomena had virtually disappeared. This suggests that these were perhaps vestigial effects of some force, or else substance, extruded by one who had at some point been a more active physical medium or focus of poltergeist activity. To attempt to test the IR effect using as a Subject a person reputed to have been an effective PK Subject, therefore seemed promising for two reasons: first, the Schneider IR experiments and indeed the whole Schneider mediumship were, as has been argued, among the best documented and most scientifically controlled in the field of macro-PK, and secondly, in the absence of 'great mediums' willing to be investigated who seem to have largely disappeared off the scene, a former PK agent of much lesser accomplishment might be a suitable Subject and more easily found. Indeed it was with this hope that I had the IR apparatus constructed by the late Brookes-Smith, and when Matthew contacted me he seemed an ideal Subject for testing the tentative hypothesis that a former but still relatively recent PK subject would still produce vestigial effects in the IR, and I might note some of the characteristics of the phenomena for future experimentation.

The setting was that described above in 5.4, some further details being given in the published account in the Proceedings of the Society for Psychical Research (84).

5.6 Apparatus for IR experiments

The equipment for detecting occultation of the infra-red beam (see Figure 7) consisted of a mounting board with an infra-red source at one end and a photocell at the other. The photocell output voltage was amplified and biased in a separate amplifier unit having an output of direct voltage of 1 V under zero occultation conditions and 0 V under total occultation conditions. The output voltage was displayed on two independent analogue voltmeters, and on a digital voltmeter (Gould-Advance model Alpha iii); it was also recorded on chart paper using a Watanabe Linear-corder Model WTR 281.

The IR light source was housed in a plywood box with plywood feet extending from each side so that it could be firmly screwed down. The light source was a 6 V MES bulb focussed on a 4" lens resulting in a parallel beam.

The IR filter transmitted light $\lambda = 0.9-2.5 \mu\text{m}$ measured on a CARY 14 spectrometer.

Power for the 6 V bulb was derived from three different sources during these experiments. These were:

- (a) A bank of alkaline accumulators followed by a conventional solid state voltage stabilizer; the regulated output voltage was set at 5 V.
- (b) An unregulated rectified and smoothed direct voltage derived from the 240 V mains supply via a transformer, followed by the same stabilizer as in (a) above.
- (c) Laboratory twin stabilized d.c. supply unit (Advance model PP3) and DS 5/05 power unit adjusted to provide a direct voltage output of 5 V.

The photocell unit was contained in a plywood box similar to the lamp unit's. A 4" diameter lens was fitted at the end facing the lamp unit and a silicon solid state light sensitive cell was mounted on an adjustable stand located in the lens focus. Output connections were provided by a pair of 4 mm sockets in parallel with a 3.5 mm co-axial plug and socket connection. The plywood case again had projecting feet on each side so that when it was correctly aligned to the light beam it can be firmly screwed down.

The amplifier unit had a plywood case. Amplifier panel and power supply were in separate compartments, and there was also a small compartment for a 6 ft mains flex and its 13 A plug.

The amplifier panel was $7\frac{1}{2}$ " square. On it were mounted the panel meter, all the essential controls and the inlet and output sockets. The input circuit was essentially a d.c. Wheatstone bridge having 10 000 ohm resistance arms, and there were coarse and fine controls to obtain null balance. The bridge output was connected to a two stage 741 operational amplifier with variable gain control, overall voltage gain being between 1 000 and 10 000. The d.c. output signal was at the 1 V level.

Since the experiments were conducted in the Bio-Electricity Laboratory at the City University, use was made of the installed monitoring and recording facilities.

The infra-red source and sensor were mounted on a rigid

board, usually placed on a trolley located in the experimental area close to, and connected by an umbilical to the amplifier unit situated at the monitoring console. The amplifier unit output was plugged into the appropriate data lines at the console which provided an analogue electronic voltmeter display (3 V full-scale deflection), having an output impedance of about 1 M Ω and in addition a digital voltmeter display. This latter 3½ digit instrument, set to show 1.000 V at zero occultation was sensitive enough to display the noise in the system (0.01% per digit). Since the noise was typically between 1% and 2%, the digital reading was continually varying between about 0.980 and 1.020; this appeared to attract the Subject's interest and he used the instrument for feedback during most of the experiments.

Data lines from monitoring console transmitted the amplifier unit output to the recording studio on the mezzanine floor immediately above the experimental area. Here the data were displayed on an analogue voltmeter similar to that on the monitoring console and monitored by technician staff; the voltage was also recorded on the Watanabe chart recorder. The chart record showed the amplifier unit output on a scale providing 4 cm deflection for total occultation, together with timing pulses at 1 minute intervals. Additionally event marker pulses were recorded by operating a press button in the experimental area.

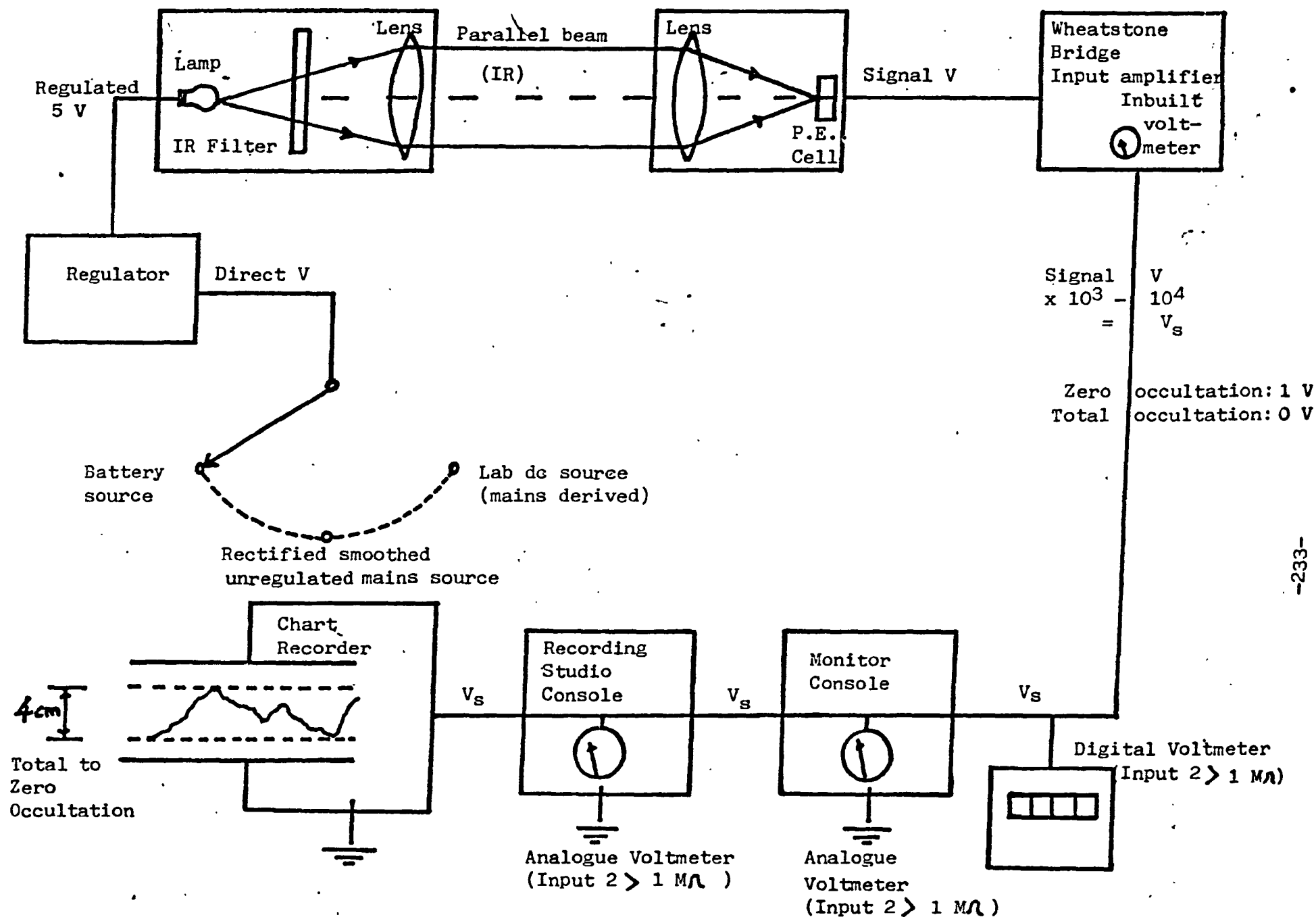
Audio recording was in use throughout the experiments. Two microphones were active, one situated centrally overhead in the experimental area, and the other, for use by the experimenters, at the monitoring console. Synchronisation of the chart and audio records was effected by manually marking the time chart and audibly recording it on the tape. Further details are given in the text. The recorder used was a Phillips model 4407 stereo. Some of the sessions were recorded on an Akai video recorder Model VT 1100.

5.7 Brief description of events and IR records

5.71 Timing

A provisional timetable was drawn up and circulated to participants. It was explicitly envisaged that arrangements

Figure 7. Schematic diagram of equipment.



would be changed, since the two weeks were regarded as a period of concentrated exploration, rather than providing definitive results. Table 33 shows departure from the original tentative timetable.

<u>IR Experiments projected</u>	<u>IR Experiments conducted</u>
a.m. Tuesday 25 July	p.m. Tuesday 25 July
a.m./p.m. Wednesday 26 July	p.m. Wednesday 26 July
a.m. Friday 28 July	noon/p.m. Friday 28 July
'any time' Monday 31 July	p.m. Monday 31 July
(if promising)	p.m. Wednesday 2 August
	(impromptu trial at Barts)

Table 33. Time-tabling of experiments

The timing was partly dictated by the needs of other experiments and partly by Matthew's decisions to turn his attention to the infra-red experiments, with the exception of Wednesday 2 August. (The circumstances surrounding these 'decisions' will be more fully discussed below.) The fact that the infra-red equipment was set up most of the time and that the experimental area was so organised that attention could be switched from one experiment to another at will facilitated maximum flexibility. As will be discussed, such flexibility combines advantages and drawbacks.

The experiments will be described in terms of the days on which attempts were made by Matthew to influence the infra-red.

5.72 Additional comments on laboratory set-up

A few words of description in non-technical terms are necessary here to clarify what follows. Phenomenologically, that is from the point of view of the Subject as well as non-technical participants, three items of equipment were of significance: the infra-red (called IR) equipment, the digital voltmeter (DVM) and the chart recorder.

The IR equipment was composed of two wooden boxes mounted on a wooden board about three feet apart: if the IR was 'on', that is, if the box containing the IR source projected its beam on to the cell, the space between the two boxes had to be seen to be perfectly vacant. Any intervening object would immediately affect the 'beam' and, depending on its size and density, would

result in a recording of a partial or total 'occultation'. Objects would occasionally be deliberately put in the beam for test purposes to see if the instrument was working, and this was noted. It was the Subject's task somehow psychically to infiltrate or invade the space between source and cell, so as to cause a partial 'occultation' of the beam (see Figure 8). (The word 'occultation' will be used in the descriptive sections for simplicity, without pre-empting the interpretation of the causal agency of deflections registered and recorded.)

The digital voltmeter registered the state of the IR beam, flickering permanently around the 1000 mV mark when the beam was 'on' and there was nothing in between the boxes, meaning 'zero occultation', 'nothing obstructing the beam' or, of course, 'all the beam is getting through to the cell'. If anything was put in the way of the beam, the reading went down, say to 750 mV, which would mean a 25% occultation. Matthew's chanting 'down, down!' reflected his attempts to reduce the voltage so as to show a figure lower than 1000. This meter was installed by D. Chapman in response to Matthew's request for feedback, so that he could see whether he had indeed affected the IR.

The permanent record corresponding to the flickering voltmeter (though with far from perfect sensitivity) was made by the large chart pen recorder on the mezzanine floor. (Unfortunately the pen registering occultation ran along a base line when there was no occultation but went up in response to lowering of the voltage or increased occultation. Thus (geometrically) 'up' on the permanent chart record corresponds to (arithmetic) 'down' in the readings of the voltmeter. This was perfectly clear to participants, but presents problems in interpreting the audio record, where 'up' and 'down' are not always qualified.

As will be seen, during three of the experimental sessions prolonged irregular deflections of the pen were recorded on the chart, indicating sudden lowering of the voltage input designed to measure amount of occultation. The colloquial term 'bout' is used deliberately to indicate sets of irregular deflections such as those depicted in Figure 9 (discussed below). The reason for the use of this term is that there is a certain arbitrariness in the sub-division of any given

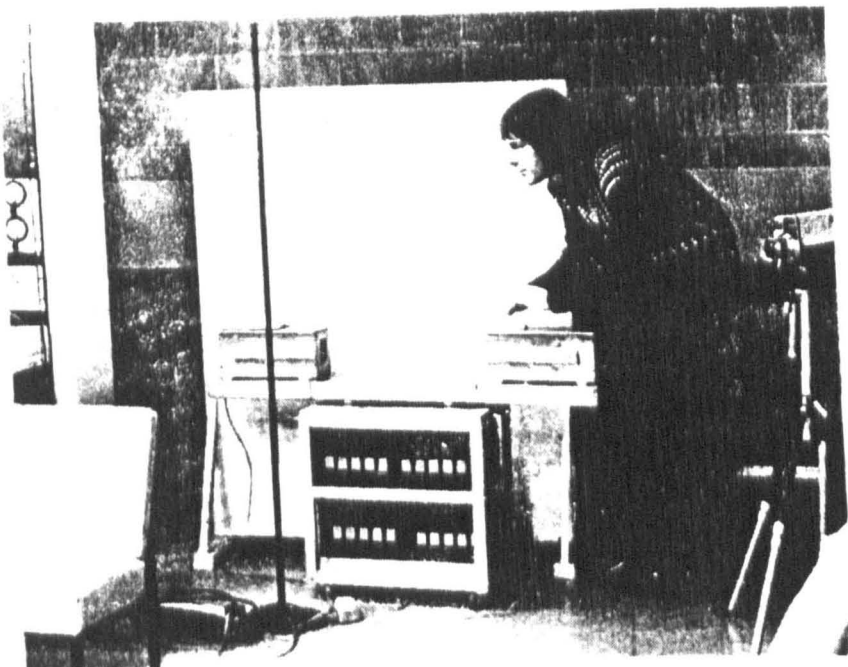


Figure 8. Matthew Manning attempting to influence infra-red equipment. He is holding hands over source box and looking at digital voltmeter (not in picture) for feedback.

group of irregular deflections. It is impossible precisely to pinpoint the time when any given bout starts and stops: there is often a gradual smooth rise in the trace which would have been reflected in a gradual lowering of read-outs of the voltmeter, and return to baseline after a set of peaks is often not to a placid horizontal line at zero (or 1000 mV). Furthermore, the clumps of irregular deflections found from about the middle of the chart for Friday 28 July onwards are often not divisible into separate incidents without doing violence to the data.

5.73 Tuesday 25 July

It had been originally planned to place some object in the path of the beam itself to see if Matthew's attempts to influence the object would affect the beam. Osty, Price, and Hope and Rayleigh had used an object such as a handkerchief for this purpose. Brookes-Smith, in his manual for the use of the infra-red apparatus, had suggested a strain gauge, but in the event we used small pots of cress as Matthew declared that he had no interest whatever in physical objects, but was keen to attempt to influence organic systems, since he believed he had been so successful in this in the United States fairly recently.

The pots of cress were placed in the path of the beam and suitable instrumental adjustments made. Matthew attempted to 'make them grow faster'. He placed his hands on either side of the beam and attempted to 'promote growth'. The path of the beam was protected by tapes stretching from source to detector box to prevent his accidentally occulting the beam with his fingers. At no point was any occultation observed while he was trying to influence the plants. However, he said that he had a feeling of coldness on his hands, 'like ether'.

There is ample evidence in the discussion recorded on the audio tape, that there was an 'upward' drift in one of the recording pens (corresponding to a gradual lowering of the voltage). Such a trend would normally be interpreted as indicating a lowering of temperature of the apparatus which would, if anything, have been expected to rise the longer it was switched on. However, this drift was subsequently (next day) attributed by Chapman to the running down of the battery, since he found that the battery, supposed to have been fully

charged but not by himself, was running down.

5.74 Wednesday 26 July

In the morning Ellison and Chapman attended to the instrumentation in an attempt to take care of the drift of the pen recording the infra-red noted the previous day. Matthew did try, at Ellison's request, to influence the IR, but without effect. After the battery was found to be apparently responsible for the drift, the apparatus was put on stabilised mains during lunchtime, and after a drift due to the normal heating up period, a stable horizontal trace was obtained.

In the afternoon, Matthew was being kept waiting for another experiment, because adjustments were being made to the equipment at his own request. Meanwhile, I administered the first of a set of poetry experiments, which I had devised in response to Matthew's request. These experiments are discussed and described in (85). It is sufficient to mention here that Matthew was handed on demand one of a set of envelopes containing an elaborately concealed and protected slip of paper on which were typed short extracts from poems. Matthew would try and produce a drawing to illustrate the target verse. The envelopes were opened immediately and contents checked against drawings. Results were negligible. The only conceivable correspondence was the first attempt he made on 26 July.

This drawing consisted of a crude sketch of a knight in armour on a winged horse with a castle and pine trees and a bird flying overhead. He wrote above the drawing: 'Knight in armour. Breughel-like image of dogs and castle - passing through trees. Reminds me of Dürer drawing of knight and devil. Horse has wings. Blindman!' The verse in the package was from Keith Douglas

Now on my dial of glass appears
The soldier who is going to die

Matthew himself was far from delighted with this as a 'hit' but he, as well as the others present, thought it reasonably relevant. In naming the apocalyptic Dürer etching, Matthew had omitted the third partner, 'death', and certainly the mood of the etching and that described by Matthew were quite apposite to the Douglas poem.

Four more poems were used, but the drawings and captions

made by Matthew bore no relationship to the enclosed verse that he or I could discern. Mary Rose Barrington, who was present, in an attempt to encourage him, tried to find some barely conceivable relationships between drawing and lines which seemed far-fetched in the extreme to the others present. Matthew clearly found this exasperating and the more irritated he grew, the more she endeavoured to provide barely imaginable correspondences of meaning. I was far from delighted by this well-meaning but in my view ill-judged attempt to jolly Matthew along. The audio recording bears witness to an atmosphere in the laboratory tense with irritation, although only Matthew, characteristically enough, expressed his vexation in so many words. I then left the lab, to see what had happened about the experiment for which we were waiting.

During this time, after some further verbal sparring between Matthew and Barrington, he got up and put his hand in the beam to see if the equipment was working. This incident is recorded on the chart and was marked as normal occultation. He was at that point under observation by Chapman and Barrington, who noted that he was not subsequently in contact with the equipment. Within a minute, Chapman pointed out that there were irregularities in the trace (see Figure 9). As can be seen, the trace begins to rise, gradually at first, and soon, severe irregularities in the trace manifested for the first time. During this time, Matthew was holding his hand well above the photocell of the IR, attempting to lower the voltage on the DVM.

It is clear from the audio record that Matthew did not consciously and deliberately initiate whatever it was that caused this irregularity in the trace. He himself is heard to describe the events as a good instance of a 'spontaneous' influence. He expressed his agitation, which he attributed at least in part to still being kept waiting. At some point which is not entirely clear, I returned to the laboratory with Professors Ellison and W.B. Brown, and immediately proceeded, together with the latter, to prepare for the next experiment (concerned to determine whether Matthew could influence the rate of haemolysis of blood in vitro (86), in which I acted as second experimenter, responsible for timing, randomising,

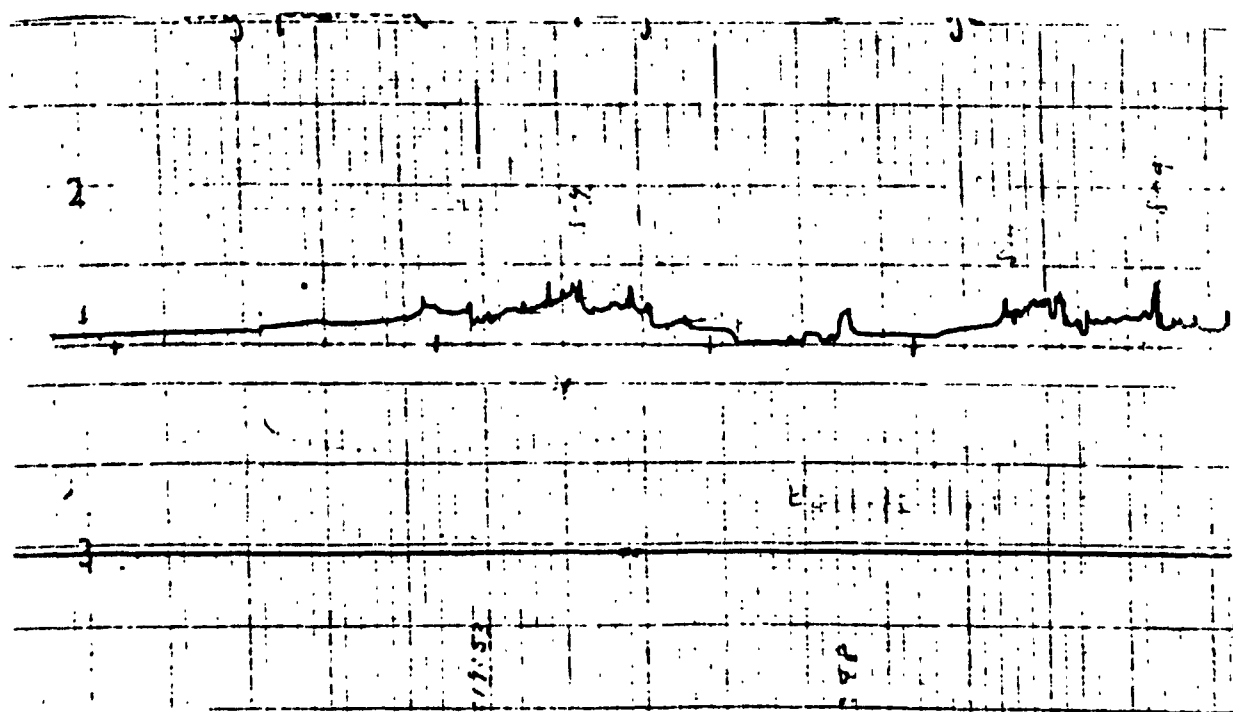


Figure 9. Beginning of the first set of irregular deflections, afternoon 26 July 1978, chart 1. Chart speed 50 mm per minute. Approximate period covered by entire figure 2 $\frac{3}{4}$ minutes.

keeping the double-blind record, and labelling specimens. I was therefore entirely pre-occupied with the preparations for an experiment on which Matthew was particularly keen, which was the most exacting in terms of experimenter time and attention, and concerning the delay of which Matthew had expressed particular impatience and irritation. Miss Barrington called repeatedly and loudly for witnesses, and Matthew observed somewhat bitterly that 'nobody else is interested in these experiments'. Chapman is heard expressing puzzlement at the irregularities of the IR chart record. The end of this bout of irregular deflections is unfortunately not accompanied by audio record because the tape ran out.

When the tape recorder comes on again, there is conversation where both Miss Barrington and Chapman testify that Matthew was not touching anything while irregularities were occurring.

The fourth bout began at approximately 16:42. Participants called out the numbers on the digital voltmeter which so far as can be ascertained on this and subsequent occasions, corresponded with reasonable accuracy in time and magnitude to the deflections shown on the chart record. The analogue voltmeter also corresponded to both other indicators. After this, Matthew was asked to go away and Prof. Ellison and I simulated the movements Matthew had made over the apparatus to test whether deflections such as those observed could be caused by normal factors such as shadow, proximity etc. However, our movements did not have the slightest effect on any of the indicators, i.e. digital voltmeter, analogue voltmeter or chart recorder. At this point video recording equipment was introduced, operated by Chapman. Matthew returned, and after another two bouts, the tracing returned to normal.

Matthew had by this time taken to addressing himself to the digital voltmeter, 'willing' it to go down. The chart recorder was set to reflect only a lowering in voltage, i.e. occultation of the beam, and not an increase in voltage. In view of Matthew's preoccupation with the voltmeter, and to see if it was the voltage rather than the IR beam that was being affected, it was decided to ask him to 'will' the digital voltmeter to go in the opposite direction, i.e. 'up'. Since

this could not be reflected on the chart, systematic reading out aloud on the audio recording was substituted. According to the audio record, the readings on the digital voltmeter rose for a period of approximately five minutes, the highest voltage read-out being 1032 mV. This, as Chapman observed at the time, was only one tenth of the value obtained in the opposite direction, and it could be deemed to be within the noise level in the system. (On subsequent occasions, Matthew showed a strong preference for 'making it go down' rather than up. Later attempts were made to re-adjust the baseline but these turned out to be unsatisfactory.) At 17:00 Matthew said, 'It'll be upset for at least an hour' and (87)

I just felt that I was controlling it and that it would do whatever I told it to do. That's why when it was going down I could make it go lower and when I decided to turn around and make it go up I just told it to go up.

Ellison: 'You just told it as though it was an animal doing what you said?'

Matthew: 'Yes. But I really believed myself at that moment that it was going to do exactly what I told it to. Just as now I believe that it will take a long time to settle down again. Something has got into that which it will hold for a long time.'

However, there was only one further bout lasting about 2 minutes after which the machine settled down to normal and the chart was signed at about 17:09 and Matthew left the laboratory soon after.

5.75 Thursday 27 July

No infra-red experiments were envisaged for this day and from the audio record it would appear that although the IR equipment was running, the chart recorder was not switched on. However, some impromptu experiments were carried out by Ellison, encouraging Matthew to affect the beam. It would seem that some instability in the voltmeter was detected. However, since Chapman repeatedly stated that he was not satisfied that the instrument was as yet stable, and since there is no chart to provide a permanent record of instrumental deflection, the results dictated on to the audio record should in my view, be disregarded. It is, however, worth noting that according

to the audio record, attempts to influence the beam by means of a magnet and by means of a hair dryer failed to affect it in any significant manner.

5.76 Friday 28 July

Infra-red experiments were expected to take place sometime during this day, although no exact timing was determined. It had been arranged to start with haemolysis sessions and on the previous evening I had arranged with Dr. and Mrs. I. Grattan-Guinness, who rang me up from Devon, that they would be attempting to 'send' some telepathic messages starting precisely at noon, to be determined by the BBC time signal. I. Grattan-Guinness had originally been asked to be a member of the investigating team, but he was unable to attend in person, since his holiday had been previously arranged. Immediately when Matthew arrived at the laboratory on Friday morning, he refused to have anything to do with the Grattan-Guinness experiment on the grounds that if they could not be bothered to be present, he could not be bothered to do experiments with them.

A chart is available for the whole day from 10:53 to 16:00. After the trace was stable, the whole infra-red equipment was moved so as to make it more accessible to the video camera. The transportation of the instrument is recorded on the chart trace by very small irregularities in the baseline. The trace then immediately settled down to complete stability, from 11:15 onwards, and a haemolysis experiment was undertaken. At 11:54 a video recording was started. Although Matthew's refusal to collaborate with the Grattan-Guinness experiment was briefly referred to in connection with the conduct of other experiments, no further attempt was made to persuade Matthew to change his mind.

At approximately 11:58 (as subsequently estimated by reference to the speaking clock) the chart trace leaves the baseline and described a shape never previously nor subsequently observed (Figure 10).

F. Sullivan (an assistant technician) was observing the chart at the time and called out to D. Chapman who immediately alerted the other participants. From the audio record it would appear that Matthew was not in the experimental area at the time, since a haemolysis control experiment was in progress. This

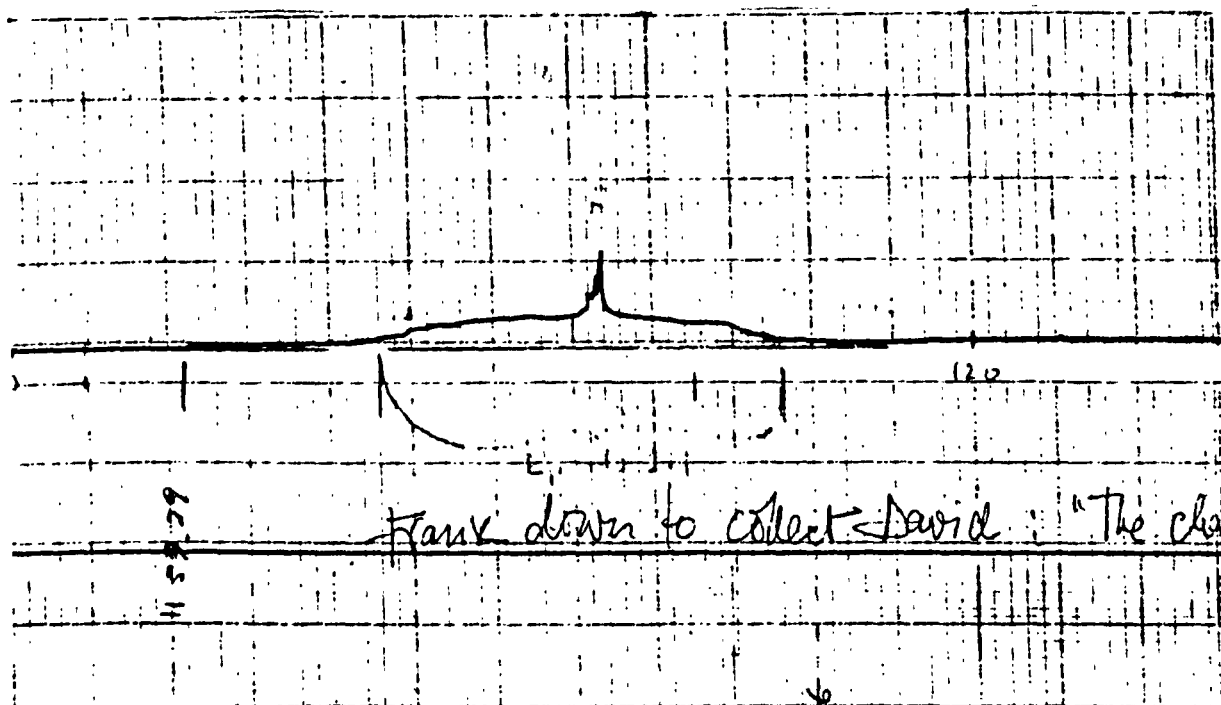


Figure 10. 'Grattan-Guinness peak', 28 July, chart speed 25 mm per minute. Approximate time covered by entire figure is 11:57 to 12:02.

was noted by Chapman, by myself and also by Matthew himself. When attempts were made to establish the exact time, since this was about noon, the conversation turned naturally enough to the Grattan-Guinnesses, who were presumably sitting somewhere in England sending unrequited messages! However, haemolysis was resumed and while my voice can be heard counting down the seconds, Matthew's voice is heard whispering 'down, down' a number of times. It seems clear from the audio record that Matthew had switched interest from haemolysis to infra-red at this stage. At this point the chart baseline is nearly stable, though not perfectly straight.

Between 12:09 and 12:10 Matthew asks 'Is Ivor Grattan-Guinness known as a great psychic?' with the clear implication that paranormality might be imputed elsewhere, and he goes on to speculate that he might have unconsciously picked up the Grattan-Guinness messages and translated them into activity in the infra-red. From this point on the chart trace shows a fairly steady rise, whilst Matthew is addressing the voltmeter, adjuring it to go down. From 12:13½ to 12:15 there is a characteristic jagged bout after which irregularities continue at a much lower level until about 12:17 at which point the trace settles down again.

At about this time, Mr. Roger Chapman, Senior Lecturer in Electronic Engineering, a member of Prof. Ellison's staff, arrived in the laboratory. There are introductions and the apparatus was explained and demonstrated.

At about 12:25 the trace begins to rise again and there is a characteristic jagged bout (Figure 11 and Table 34), Matthew being closely observed and verbally described at the same time.

The video apparatus is on during part of this incident showing Matthew passing his hand back and forth over the box. As described by Ellison the trace drifts back towards normal baseline and Roger Chapman mimics Matthew's movements. This also is recorded on video tape. Unlike Matthew, Roger Chapman repeatedly touched the box which manifests itself in vertical cross-hatching deflections, apart from which the trace is horizontal and at zero. There followed some attempts at normal simulation of the observed traces.

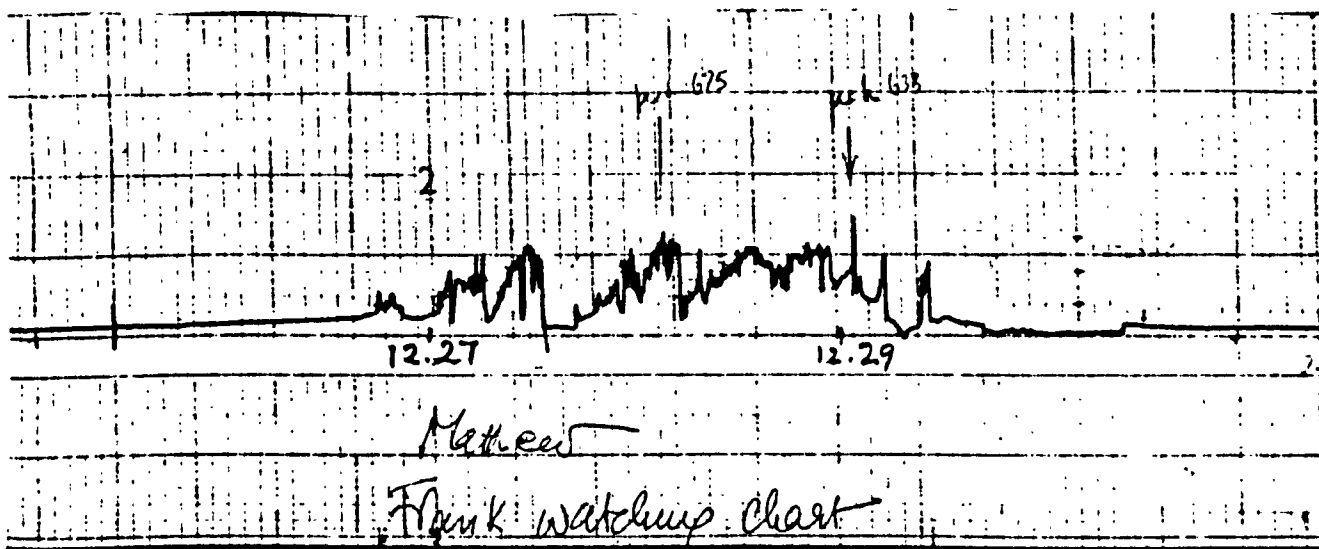


Figure 11. Irregular deflections 28 July, chart 2.
 Chart speed 25 mm per minute. Approximate
 time covered by entire figure is 12:25
 to 12:30½. Table 34 refers to coincident
 events from 12:27 to 12:29 marked in figure.

1 Notional clock - time and record- ing monitors	2 Combined audio/ chart time	3 DVM chart readings (millivolts)	4 Transcription of audio record
Audio & Chart	12h27'0"	87'26.0"	Gregory: 'Absolutely clearly-no possibility of
		850-925	any deception - Matthew's got his sleeves
		87'30.8"	rolled up.' Brown: 'And he's not in contact
		800-960	with the apparatus in any way.'
		87'35.6"	Gregory: 'Seven, seven, yes 790.'
Audio & Chart		750-870	Ellison: '78, 78, can you see that Roger? Do
		87'40.4"	you want to come and have another look?'
		745-950	R. Chapman: 'Oh it's much more interesting
		87'45.2"	watching it...' Ellison: 'We're all watching
		780-875	that space - there's nothing going in the beam.
			Just waving his hands about over the top.'
Audio & Chart	12h27'24"	87'50.0"	Gregory: 'Seven'
		725-950	
		87'54.8"	Matthew: 'I've been down to six something.'
		720-975	Ellison: 'Yes, you did -it's back up now, 89,
		87'59.6"	yes, it's going down again.'
Audio & Chart		975-900	
		88'04.4"	Gregory: '8, es.'
		900-975	
		88'09.2"	Ellison: '89, 88, 86, 5, 3, 84, 82, 80,'
		925-950	
Audio & Chart	12h27'48"	88'14.0"	Gregory: 'I am watching the space
		860-925	the whole time...'
		88'18.8"	
		825-925	
		88'23.6"	Ruth West: 'That's seven, six, up to
Audio & Chart		730-890	six.'
		88'28.4"	Ellison: 'Yes'
		740-875	Gregory: 'Nothing at all can be seen between
		88'33.2"	the
		675-820	
Audio, Video & Chart	12h28'12"	88'38.0"	cell and the receiver.'
		720-945	
		88'42.8"	Ellison: 'Make sure you get the picture
		740-900	of both things, you can see the meter
		88'47.6"	going down as he goes
Audio, Video & Chart		800-875	like this around it.
		88'52.4"	That's what we call PK
		770-825	that does this.'
		88'57.2"	
		725-775	Gregory: '...751, 752...
Audio, Video & Chart	12h28'36"	89'02.0"	...absolutely marvellous.'
		725-800	
		89'06.8"	Ellison: '72, yes, 73'
		780-850	
		89'11.6"	(Matthew makes sound like 'phew')
Audio, Video & Chart		750-850	Gregory: 'Arthur, I would just quite like
		89'16.4"	to
		715-780	go out a bit - could you
		89'21.2"	and Ruth....
		725-860	
	12h29'0"	89'26.0"	...Mary Rose would hang us otherwise if we

Table 34

This diagram shows in tabular form a two minute period of the events described from 12:27 to 12:29 on 28 July and corresponds to the chart tracing of Figure 11. The chart paper is divided into 1 cm squares which are further subdivided by five vertical fine lines and ten horizontal fine lines, such that time can be read on the horizontal and voltage on the vertical axis. This chart was run at a speed of 25 mm per minute.

Column 1 provides notional clock time; this was calculated by taking initial clock time written on the chart and measuring along the horizontal axis and converting centimeters into times. The word 'notional' is used because of possible variations in chart speed. There is, however, reasonable coincidence with frequent verbal read-outs on the audio record, and occasional timed events on the chart itself. This column also shows monitoring devices used during any given 24 second interval. A 24 second interval was chosen because 1 cm represented one 24 second interval at the chart speed on this occasion.

Column 2 combines audio and chart time. The matching of the audio tape time to chart time was done by listening to the entire audio tape for the period covering the chart time and choosing certain clear words which were to be timed to provide an approximate fit with the chart. The first clear word uttered near the beginning of the audio tape covering the chart time was assigned a time of zero, and each chosen word was timed in relation to this first word using a Casio PQ-7 stopwatch. A suitable point on the chart was selected where a total occultation of the beam was effected by myself, accompanied by the words 'I'm putting my hand in the beam now', 'now' coinciding with a reading of zero on the voltmeter. The audio tape was then started, taking the first clear word as $t = 0$, the intervening pre-selected words being timed in relation to t_0 , so that an actual time could be established for the word 'now'. It was thus possible to fit dialogue to chart by making a rough equation between Casio time and the chart time as determined by chart speed. Because the chart and audio recorder did not move at precisely identical speeds, only approximate corresponding times are available, but the coincidence between audio and chart record is reasonable. Each 24 second interval is divided into five fine vertical lines, which thus represent time markers for intervals of 4.8 seconds, shown in this column.

Column 3 shows the extreme voltmeter readings recorded on the chart in the interval represented by the chart times in column 2, beginning with the time immediately above left of the mV readings and ending immediately before the next chart time, left below of the mV readings; e.g. between chart time 87'26.0" (inclusive) and 87'30.8" (exclusive) the lowest reading reached was 850, the highest 925. (It will be remembered that the lower figure measures the higher degree of occultation if this is what is being measured.) Millivolts are determined by reference to the fine horizontal lines. Here the precision is limited by, among other things, the thickness of the pen trace and the responsiveness of the chart recorder to voltage fluctuations, which is not necessarily identical with that of the digital voltmeter.

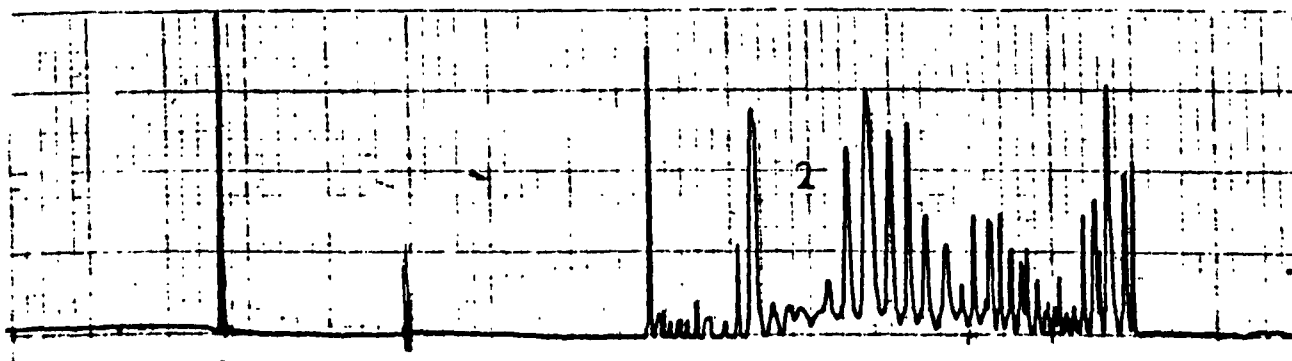
Column 4 is a transcription of the audio record for the two minutes covered by the events described from 12:27 to 12:29. In this column the dialogue is given as corresponding to the times calculated for column 2.

Between approximately 12:34 and 12:35 Sullivan physically shook the base on which the IR instrument rested without obtaining more than a thickening of the horizontal trace. I occulted the beam normally by first placing my hand in the path of the beam and then by rapidly flicking my fingers into it. Brown inserted a thin box into the beam (Figure 12). The traces of these normal occultations look entirely different from the irregular ones under investigation.

About two minutes later Matthew attempted to influence the beam from a distance of approximately four metres from the apparatus. About a half minute later, the trace leaves the baseline rising to a peak, although its height can not be exactly determined at any one moment because at that point the motor was accidentally switched off, the chart stopped moving and the pen recorder went up and down on the same spot for at least a minute. It then resumed its jagged path while Matthew was attempting to influence the beam from a distance of about three metres. There is a sudden recovery to zero and my voice is heard saying 'Matthew is now relaxing'. The trace begins to climb again soon after, while participants are making an attempt to persuade Matthew to relax. He explains that he 'cannot stop thinking about it' and expresses the opinion that the instrument will be disturbed for some time. At this point, about 12:50, Brown and I took Matthew out of the laboratory for a walk around the square, leaving the electrical engineers and technicians, Ellison, David Chapman, Roger Chapman and Sullivan, to discuss the behaviour of the equipment. Hardly had Matthew left the laboratory than the pen resumed its aberrant course. It did not settle down until six minutes later. The engineers switched off the IR source, re-set the baseline to midpoint, and switched it on again. Matthew made no further attempts to influence the IR that afternoon. He felt it unlikely that any more would happen as he 'felt drained'. The chart record was completely straight from 13:06 to 16:00, and is signed by participants.

5.77 Monday 31 July

The chart for this morning is completely level from 10:38 to 13:00, indicating that there was no occultation or fluctuation in the voltage beyond noise level throughout this period. The



Full hand
occultation

Rapid flicking
of fingers in
the beam

Lowering of thin
box in the beam

Figure 12. Normal occultations, 28 July, chart 2.
Chart speed 25 mm per minute. Approximate
time covered by entire figure is 12:35 to
12:41½.

power source is the mains. During this time there is general discussion (including a discussion of the 'Grattan-Guinness peak') and a haemolysis experiment. Shortly after midday, Matthew says 'I had to stop myself from doing it [the IR] this morning...I want to wait for this afternoon'.

The chart for the afternoon is in many ways the most problematic one for a number of reasons. Once irregularities appear on the chart recorder, they hardly cease at all for the entire period from 16:07 to 17:50. Also, timing becomes virtually impossible because at one point the tape runs out and there is no clear indication of the time at which it comes on again. There were so many changes in the apparatus that it is difficult to be certain whether a stable baseline was obtained at any time within this period.

There were two visitors in the laboratory, to be called Professor D.G. and Dr. J.B.. Matthew clearly set himself to 'hex' or disrupt the apparatus (psychically), ignoring all pleas from participants to let go and relax.

At approximately 1:40 and 1:43, there are two departures from the baseline on the chart, which is now set at midpoint, to about 900. Although the audio tape was not running at the time that they occur, subsequent dialogue between Prof. Ellison and Matthew suggests that they probably coincide with Matthew's entry into the laboratory. The horizontal trace which continues to show a very slight drift was re-set at intervals.

Brian Inglis arrived at about 15:00. At about 15:04 Chapman noted that irregularities were beginning to take place and Matthew said 'If it's started, then we should start'. There was a bout of irregularities starting at approximately 15:07 stopping abruptly about a minute later, after Matthew had accidentally hit the box. The trace is approximately horizontal, apart from a slight drift, until 15:27. During this time Matthew was attempting to influence the beam without any apparent success. He relaxed, and the slight upward drift continued.

Between about 15:27 and 15:29 Chapman returned the chart recorder to the original configuration.

At about 15:32 D.G. arrived, immediately after Brown and I

had left in search of some other apparatus. Ellison explained the apparatus to him and was called out of the laboratory by his secretary.

While Inglis and D.G. were discussing other matters, Matthew drew their attention to the fact that 'it's going down again' and at about 16:07, there was a burst of irregular activity for about half a minute, peaking at about 800 mV. At about 16:08, the trace rose again and did not really settle down until 17:50. No discrete bouts can be discerned, nor can any relationship be claimed between psychological or behavioural variables and the vagaries of the chart trace, without the most specious pleading. On three, possibly four occasions, the trace passes the 500 mV mark, corresponding to an occultation of just over 50%.

Brown and I returned about 16:13. Just before 16:20 Chapman switched off and on again the 5 V supply to the lamp to clear any possible switch contact fault. The trace continues its irregular course. At about 16:22½, Chapman changed from the rectified mains (25 V) to a battery (25 V) supply to the regulator. The trace continued irregular. Just before 16:24, there is another change of battery, the trace is normal for about 22 seconds and then becomes jagged once more. There is yet another change just after 16:31 when it was decided to change back to the mains but with an alternative voltage regulator. Chapman challenged Matthew 'now muck that one up', and the trace remains stable for three minutes. However, after some adjustments by Chapman, so marked on the chart, and while Matthew is trying to rise to the challenge, the trace rises once more, to resume its jagged irregularities.

At about 16:55 J.B. arrived. All the while the trace continued its irregular course. Matthew resisted all attempts to get him to calm down and (naturally enough) J.B. expressed his assumption that it was simply a question of discovering the fault in the equipment. He suggested CO₂, carbon dioxide, as a possible cause, and the trace reflects the introduction of CO₂ into the beam by means of a fire extinguisher by one of the participants. The resulting occultation is far more violent than anything observed previously or subsequently, and it is hard to see what such gross interference with the

apparatus could have shown, since J.B. was presumably referring to exhalation of breath. J.B.'s alternative suggestion for explaining the aberrant behaviour of the apparatus was variations in the power supply; he also suggested electrostatics as a possible cause. Before leaving he recommended for future reference that the amplifier be rebuilt and that low noise cable be used, as well as batteries sealed in metal boxes without leads or switch contacts.

At about 17:50 Chapman had completed re-setting with a new power supply and the trace returns to normal, despite several attempts by Matthew to influence the trace. Matthew pointed out that he was now exhausted.

At about 18:09 the power supply was changed back to the original mains and the trace continues perfectly steady until 18:20 when the chart ends.

There is some dialogue at the end of the audio tape for this period in which Ellison considers J.B.'s suspicions of the power supply. After further discussions, the power supply seems ruled out as a source of trouble and the only remaining possibility described by Ellison is some loose contact that cured itself.

5.78 Tuesday 1 August

The infra-red equipment was set up all day from 10:25 to 17:16. Matthew was at Birkbeck College and only Chapman and Sullivan were in the laboratory. The trace is horizontal throughout, apart from a very slight, flat upward deflection to 995 mV during the warming up period labelled 'shadow'. No sign of voltage fluctuations or loose contacts were registered.

5.79 Wednesday 2 August

The IR equipment was set up again all day and there is a chart from 10:40 to 16:53. The trace is virtually horizontal throughout; there are however, some very small occasional irregularities which it is impossible to time with any accuracy since chart speed, which is not explicitly recorded, was varied. In addition, a fault had developed in the timing mechanism of the chart recorder as reflected both in the pen trace indicating minutes, and also in recorded speech.

Mr. I. Bloomfield visited the laboratory in the morning about 11:30 to 12:00 and Matthew attempted to influence the beam for him. There are very slight irregularities in the trace not noticed at the time, but there are similar irregularities after everybody had left the lab. In any case, this chart shows a tendency for small blips to occur lasting a minute immediately following switchings and re-settings. The overall impression however, of the trace for the entire day, is flat and horizontal.

In the afternoon Matthew went to St. Bartholomew's Hospital accompanied by Ellison, Chapman, Brown and myself to have his electroencephalogram recorded by Miss Marion Smith. Matthew made his distaste for the entire proceedings more than plain. In an attempt to distract him, he was given four of the poetry envelopes. I wrote notes, using my watch (which was between one and two minutes fast as subsequently checked) to give approximate timings. At approximately 15:18 I suggested that Matthew might try influencing the infra-red in the laboratory from a distance of approximately three-quarters of a mile. Again, at about 15:20½ I asked Matthew to have a try at influencing the IR, using the words 'come on Matthew, that'll be a world first'; and he made the attempt. About a minute later a telephone call was made to Sullivan, asking him to check that the equipment was running and to leave the laboratory, locking it behind him.

From 13:10 the trace is absolutely flat, until a few perturbations appear at times subsequently calculated by Chapman (audio tape) as occurring at 15:16 and 15:22½ respectively. The first of these irregularities is a very slight but noticeable protracted wobble, lasting about a minute. The second is a very slight step in the trace, such as might occur when the apparatus is re-set. These irregularities, on balance, may not be interpreted as being significant.

The trace thereafter continues entirely flat until approximately 16:53.

5.8 Discussion of IR findings

On the face of it, similar tracings to those obtained in earlier experiments with Rudi Schneider were recorded with

Matthew Manning. However the interpretation of these traces is subject to a number of qualifications to be discussed.

5.81 Source material

The main records available were the chart tracings, and the audio tapes available for almost the entire period of investigation. Although video equipment was used from time to time and certainly corroborates the audio record, e.g. it shows Roger Chapman trying his hand at influencing the equipment, the recordings are too poor in quality and too few in quantity for more to be claimed for them than occasional illustrations and corroboration that no one was near the instrument while strong deflections were occurring.

Table 35 shows the following chart records which are available:

Chart	Date	Approximate length of chart
1	Wednesday, 26 July	1 hr. 7 min.
2	Friday, 28 July	5 hrs. 7 min.
3	Monday, 31 July (morning)	2 hrs. 22 min.
4	Monday, 31 July (afternoon)	5 hrs. 10 min.
5	Tuesday, 1 August	6 hrs. 51 min.
6	Wednesday, 2 August	6 hrs. 13 min.
7	30-31 Oct., 1-2 Nov.	33 hrs. 58 min.
8	16 November	4 hrs. 30 min.
9	3 January 1979	1 hr.
10	12-16 February 1979	

Table 35. List of chart records

Irregularities of the trace are recorded only on three days, on charts 1, 2 and 4. Photocopies of these charts are provided separately, although this was not possible for financial reasons in the printed version.* It is however possible to provide economically a rough arithmetic profile of charts 1, 2 and 4, and this is done in Tables 36, 37 and 38.

5.82 Explanation of Tables 36, 37 and 38

For reasons which will become apparent in the discussion these are unsuitable for assessment of statistical significance. They may, however, be useful for descriptive purposes, and give an idea of the durations of time during which the trace did, and when it did not, depart from its normal baseline. It also

*attached to thesis
(Labels L, M & N)

a	b	c
Chronological duration in minutes at 1000 \pm 25	Chronological duration in minutes below 975 mV	Minimal mV (maximal occultation)
	1.64 (Figure 9)	795
0.12		
	0.02	970
0.04		
	0.06	965
0.04		
	0.02	970
0.02		
	1.46	790
0.02		
	0.44	845
1.84		
	0.78	850
0.88		
	1.16	780
0.88		
	0.38	960
0.06		
	0.12	950
0.06		
	0.04	945
13.20 (*4.67 (adjusting equipment)		
	2.14	695
7.38 (Ellison and Gregory try)		
	0.18	770
0.44		
	0.38	910
0.02		
	0.14	925
0.02		
	0.28	900
0.02		
	1.60	720
0.02		
	1.34	650
8.10 (Matthew tries to <u>raise</u> DVM)		
	1.44	810
TOTAL= 33.16	TOTAL = 13.62	

TOTAL BOUT PERIOD = 46.78

Table 36: Chart 1

Lengend for Tables 36, 37 and 38

The first row down (a) in each case indicates the lengths of time in minutes and decimal fractions of a minute for which the trace remains at the baseline (i.e. no occultation).

The second row down (b) indicates the lengths of time in minutes and decimal fractions of a minute during which significant occultation is observed (as indicated by a decrease in voltage below 975 mV). These two lines must be read in conjunction as a vertical zig-zag sequence, and represent successive times, e.g. chart 1, beginning: 1.64 (below 975 mV), 0.12 (above 975 mV), 0.02 (below 975 mV), 0.04 (above 975 mV) etc. Asterisks (*) indicate that normal causes for deflection are known for the amount of time indicated. These asterisks occur in the (a) column if such normal deflection occurred during periods of quiescence, e.g. test moving of apparatus; they are put in the (b) column when normally caused occultations were made during bouts, e.g. to test that the IR was working properly. Occasional comments without asterisks describe salient events.

The third row down (c) indicates maxima of occultation (or minimal voltage measured in millivolts) reached during any particular period immediately to the left of the voltage figure. Thus, on chart 1, during the first 1.64 minutes the lowest voltage reached was 795 mV; during the next brief departure from the baseline for 0.02 minute the greatest departure was 970 mV and so on.

In all cases I have concentrated on the portion of the chart where disturbances below 975 mV occur, which I have called the 'bout period', defined as beginning at the point at which the trace leaves the baseline for the first time, and ending at the point when irregularities cease. Departure from the baseline to an extent greater than 975 mV was selected as amounting to an 'appreciable irregularity'; as the normal fluctuation (see apparatus section) of the DVM was approximately 980 - 1020 mV and the first fine line parallel to the base on the chart record indicates a level of 975 mV. In the case of the first two charts, the total bout period is described arithmetically. Chart 4, as depicted in Table 38, is described only to that point on the chart after which such measurement has become impossible, because for the chart speed used (25 mm per minute) the trace returns to zero so frequently that analysis is impossible without gross distortion. Also, so many tests were subsequently carried out, notably flooding the instrument with CO₂ by means of a fire extinguisher, that any analysis would be futile.

Throughout chart 1 there were prolonged periods of time, lasting several minutes, when the trace returns to base. Towards the end of chart 2 there is a decrease in the length of these periods of quiescence. In chart 4 this breakdown of quiescent periods occurs quite early and it can be seen clearly from the figures that there is rapid oscillation between baseline and departure from baseline.

It can also be seen from the three charts that the maximal occultation (lowering of voltage) reached increased somewhat throughout the three days, being: chart 1: 650 (once); chart 2: 630 (once); and chart 4: 450 (twice).

a	b	c
Chronological duration in minutes at 1000 \pm 25	Chronological duration in minutes below 975 mV	Minimal mV (maximal occultation)
	1.76 (Matthew not in lab; Figure 10)	720
11.60		
	0.80	965
0.12		
	3.48	640
0.04		
	0.40	915
6.48 (*.2 test occultation)		
	0.04	965
0.12		
	0.04	960
2.32		
	1.80 (Table 34 and Figure 11)	720
0.16 (Table 34 and Figure 11)		
	1.60 (Table 34 and Figure 11)	630
0.04 (Table 34 and Figure 11)		
	0.36	765
0.72		
	0.12	960
12.92 (*2.68 test occultation)		
	0.28	965
0.12		
	0.32	685
0.68		
	0.40	750
0.40		
	0.16	760
0.12		
	1.28 (Matthew at 3 metre distance)	750
0.80		
	0.12	960
0.16		
	1.56	790
0.04		
	0.12	925
.08		
	4.36 (*1.28 test) Matthew out of lab	675
.08 Matthew out of lab		
	2.08 Matthew out of lab	650
TOTAL= 37.72	TOTAL = 20.08	
TOTAL BOUT PERIOD = 57.80		

Table 37: Chart 2

a Chronologi- cal duration in minutes at 1000 \pm 25	b Chronologi- cal duration in minutes below 975 mV	c Minimal mV (maximal occulta- tion)	a con't	b con't	c con't
	7.8	740	0.004		
0.04				0.06	875
	1.84	450	0.004		
5.32 (*.04)				0.02	875
	11.76	860	0.008		
18.4 (*1.04)				0.05	875
	0.16	945	0.01		
22.6 (*2.2)				0.02	865
	0.64	835	0.19		
0.6				0.008	915
	0.88	750	0.008		
0.04				0.08	730
	0.44	660	0.02		
0.04				0.05	820
	1.48	710	0.03		
0.04				0.08	800
	0.48	660	0.02		
0.04				0.02	700
	0.20	700	0.02		
0.04				0.02	850
	0.52	660	0.01		
0.04				0.22	750
	1.80	635	0.02		
0.16				0.008	965
	1.08	650	0.02		
0.04				0.01	965
Alt. Ch. spd. .12/1.61 (=1.73)			0.02		
0.02				0.1	965
	2.54 (*.06)	560	0.07		
.12 (*.04)				0.48	770
	0.06	800	0.01		
.004				0.02	925
	2.79 (*.05)	600	0.008		
0.01			Alt. ch. spd. .79/.4 (= .83)		750
	1.3 (*0.1)	560			
0.36					
	0.22	735			
0.08					
	0.01	930			

TOTAL = TOTAL =
48.466 39.806

TOTAL BOUT PERIOD
COVERED = 88.272

Table 38: Chart 4

provides some idea of the extent of irregularity, that is, degree of occultation (or lowering of voltage).

5.83 Collation of sources

Transcriptions were made of all available audio recordings irrespective of whether there was a chart available for that day or not. Although some rough notes were available, the final transcription (118,000 words) used for interpretative purposes was the responsibility of Kathy Wilson, who was familiar with the voices of the participants but had not been a member of the original team, to avoid certain artefacts of suggestion which had been shown to be invidious. In the case of ambiguity, she omitted the words or groups of words in question. Because of the informality of the arrangements, a verbatim record of the proceedings is clearly desirable if not essential, and there is good reason to suppose recording did not unduly inhibit participants from expressing themselves. From the transcript it is possible to reconstruct, with reasonable accuracy, what was done and when. Since the infra-red experiments, although roughly timetabled, were Subject initiated, it was clearly important to know whether irregularities are found only when the Subject attempted to influence the apparatus, or whether attempts were similarly made on days when the chart record indicates no irregularity. We also wish to stress that the auditory record is vital, since it quite often showed up serious discrepancies between the recollections of some participants and what in fact happened.

For those charts where irregularities occur, every effort was made to obtain as close a correspondence as is possible between chart and audio record. Indeed, this was one of the major points in having an audio record. For example, when an attempt was made to see if Matthew could raise rather than lower the voltage this could not be registered on the chart because of the way the baseline was set; but readings from the digital voltmeter were deliberately and collectively dictated into the microphone for the purpose of subsequently correlating with the chart record. The method described in the legend to Table 34 was applied to charts 1, 2 and 4.

Precision of coincidence in time is limited for the

following reasons: only relatively approximate times were entered on the chart itself and frequently not at the very beginning of irregularities; audio tape is elastic, and repeated playing reached different numbers on the tape counter on different occasions; on several of the charts the time marker is defective, particularly on charts 4 and 6; on two of the days the audio tape ran out for short durations, the precise length of which is not known; in chart 1, not only does the tape run out at one point, but there is no definite time marker; in chart 4, the tape runs out after the time marker and only very approximate timing is possible when the tape resumes. In addition to unavoidable changing-over of tapes, there were some minor accidents, such as the accidental pressing of a switch turning off the motor of the chart recorder.

5.84 Possible interpretations

It will clearly be seen that in view of this critique of the instrumental arrangements available, any attempt at precise co-ordination of chart and audio record would be spurious and misleading. There is, however, one observation that can be made, and which could only be made because so complete a transcript is available. This observation is to the effect that over and over again Matthew noticed an instability in a downward direction on the part of the digital voltmeter and only then decided to start to try and influence the infra-red. Here the audio record is supported by the long gentle upward slope preceding jagged irregularities (see Figure 13) which clearly reflects a gradual lowering of the voltage in the first instance. Fixing a beginning for 'bout periods' is therefore to some extent arbitrary. As has been mentioned, on one occasion he actually says 'It's started, so we should start'.

It is proposed to call this the 'bandwagon effect'. This term is not intended to pre-empt interpretations of either normality or paranormality, but it certainly affects the interpretation of the phenomenon studied. What it does invalidate is any simple inference from the negative fact that other participants, such as Ellison, Roger Chapman or I, failed to influence the trace whereas, on the face of it, Matthew did. At the time, and even in retrospect, it seemed as though he

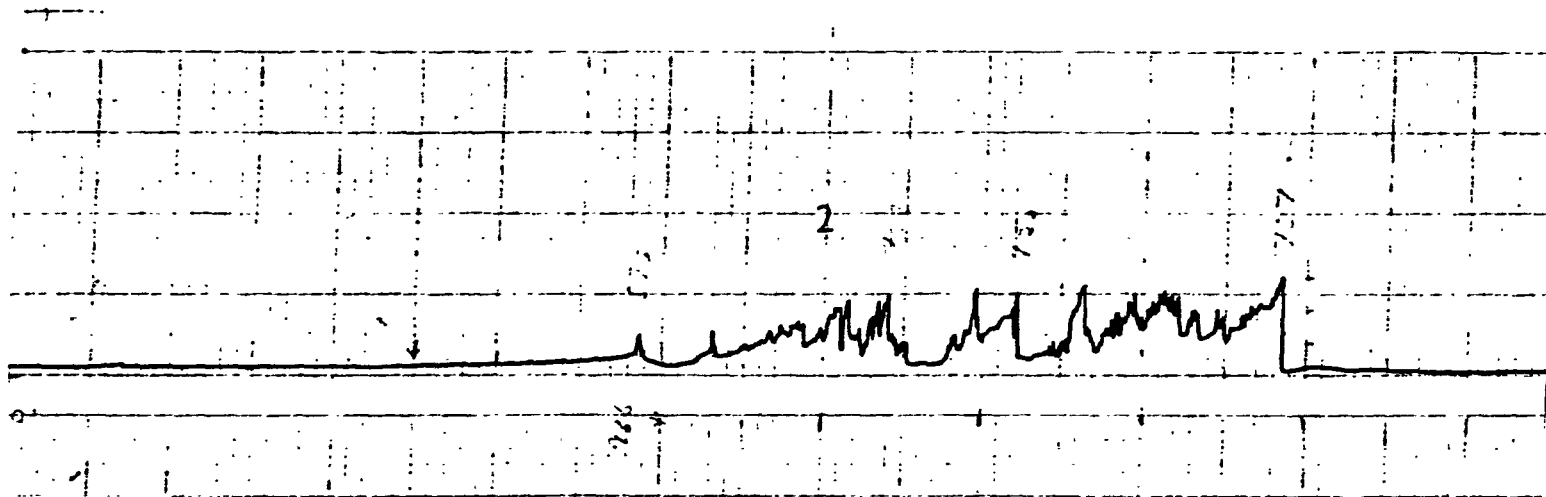


Figure 13. Irregular deflections, 26 July 1978, chart 1. Chart speed 50 mm per minute. Approximate period covered by entire figure is $3\frac{3}{4}$ minutes. Note gentle slope suggesting 'bandwagon effect' and abrupt drop at end.

could, and we could not, affect the instrument; however, scrutiny of the audio record in conjunction with the pen tracings shows that there is ample auditory evidence that Matthew tended to start from a base of instability, whereas the experimenters tried their luck when the instrument was stable. Everybody felt that it was Matthew's business, not ours, to sustain and amplify irregularities: these seemed too precious, too important to be squandered on us. This is of course interpretation not observation, retro-active introspection rather than fact. Yet such hypothetical factors, however tentative, become important when considering attribution of paranormality.

It is, however, certain that imitating Matthew's movements to the best of our ability did not create any disturbance in the record; therefore it is very unlikely that either shadows, breathing (CO₂) or electrostatics were responsible for the irregularities. It must also be pointed out that Matthew frequently exerted himself from quite a distance. On one occasion (chart 2) he was not even in the laboratory (so far as we can tell from the audio record) when deflections were first noticed (see page 243 and Figure 10) and on the same day they persisted when he had definitely left the laboratory.

While there is still a 'bout' structure, i.e. grouping in time (chart 1 and first part of chart 2) of deflections, whereas the initial slope is gentle, the end tends to be most abrupt (Figure 13). Timing is not sufficiently precise to allow us to distinguish between whether Matthew relaxed before or after the cessation of a bout of irregular deflections. He may well have seen that the digital voltmeter reverted to flickering around 1000 and relaxed his considerable muscular effort in consequence, rather than that he stopped exerting himself whereupon irregularities ceased. He might easily have been unaware of such 'bandwagoning'. It has already been described how Matthew was unable or unwilling to relax efforts while irregularities were in progress, which certainly suggests that he went along with an irregularity rather than that he in some sense voluntarily caused it.

It must be emphasised that we have no reason to doubt

Matthew's complete honesty; he was by no means the only one present to confound possible bandwagoning and definite psychic causation. Moreover, in order to simulate the trace normally it was found necessary either to wiggle one's finger in the beam in a very conspicuous manner, or to use implements such as steaming kettles. Such activities were entirely ruled out by the conditions of observation during the experiments, by the audio records, and the video records even allowing for their poor quality, and these possibilities of imitating the trace normally were discovered only later as a result of a good deal of experimenting with the equipment. In any case, Matthew was virtually never unobserved, nor did he ever make the slightest attempt to evade observation, and while irregularities were on, one person was always delegated (usually Ruth West) to watch the space between source and cell.

One of the problems for the interpretation of this set of chart records and conceivably future ones also is that, to begin with, irregular deflections cluster in groups separated by regular long stretches of quiescence. However, from the last part of the second and throughout the fourth chart, this clustering in time is lost and ever more continuous irregularities are recorded, increasing only slightly in amplitude or degree of occultation. However, the total proportion of activity over quiescence increases, as may be seen from Tables 36, 37 and 38. It might possibly be that an epidemiological or 'seizure' model might be found to be useful in interpreting data of this sort.

In view of clear evidence for the bandwagon effect, the absence of clearly defined trial and control periods and the problems of precise timing, any detailed correspondence between possible psychological states as expressed in the audio record and irregularities of the chart trace is out of the question. In any case, the cluster or bout structure of the chart records disappears, and it is clearly impermissible to pick out what seem subjectively meaningful episodes from the transcript, divide the instrumental record accordingly, and argue back and forth towards some ad hoc psychological concordance, let alone base statistical calculations on such manipulations!

It is, however, appropriate to discuss whether the irregularities could have been due to normal causes and if so which. If such causes are not deemed probable sources of the irregularities observed, it should be considered what might be possible paranormal modes of psychological influence.

5.85 Possible normal causes of irregularities

As has been mentioned, a good deal of trial and error was necessary to find out how to simulate the trace normally; and such methods would have been impossible under the conditions of the experiment. Neither vibrations, shaking of the apparatus, introducing solid particles (such as cleaning powder and cigarette ash) nor dangling threads or strips of tissue into the intervening space produced any effect remotely resembling the curves shown. Deflections of comparable size though of different shape were obtained by breathing into the beam, and these were due to misting over of the lens. On several days after the experiments (31 October, 1 November, 2 November and 3 November 1978) D. Chapman ran tests and found the equipment stable and responsive to test occultations in the normal manner. Despite the fact that the adjacent lab. was in normal use (on one occasion by eight students) and normal lighting and equipment were running, no irregularities were noted.

Fluctuations in electricity supply were among the favourite 'normal' explanations. (We have not counted the drifts that might be attributed to running down of batteries as paranormal, since Chapman was not able to guarantee that power packs had been perfectly charged when he received them. However, some sort of paranormal drain on the power supply on the lines of the Rosenheim case (89) cannot in principle be ruled out.) Although ideally there should be constant monitoring of mains or better still all power supplied by fully charged batteries, the mains seems an improbable explanation for a number of reasons: irregularities also occurred while the power was supplied by batteries; the supply was a stabilised mains in a department of electronic engineering, and furthermore, there were long periods of perfect stability before and during the investigation, and an indefinite period afterwards including the brief and unsuccessful February

trials.

Instability of the bulb would affect the chart record and D. Chapman experimentally produced such fluctuations by loosening the bulb. However, the trace looks quite different from those under investigation, and it is difficult to see how quiescence would be spontaneously restored once the bulb had become unstable.

The possibility of a loose contact somewhere in the circuit is more difficult to exclude, but is not easy to reconcile with the long periods of stability before, during and after test periods, and we are assured by Ellison and Roger Chapman that from their point of view as electrical engineers the effects observed do not resemble what they would expect to see if there had been a loose contact somewhere. Moreover, the seizure-like character and its abrupt cessation at the end of chart 4 would also seem to argue against such an interpretation. Quiescence of trace was restored only after switching back to the original supply.

In addition to possible electrical faults, a possible normal cause to be considered is carbon dioxide. (It can hardly be claimed that discharging a fire extinguisher, without warning, into the beam constitutes some adequate test; obviously if the IR was working, and if the extinguisher contained CO_2 , it was bound to work.) Subsequent attempts at deeply breathing into the beam, taking care to avoid misting over the lens, produced no noticeable deflections; it therefore seems exceedingly unlikely that CO_2 from participants' breathing affected the IR. CO_2 is also an unlikely explanation of the irregularities found for a number of other reasons, principally the stability of the trace before, and especially after, bouts of irregularity, the failure of others simulating Matthew's activities to reproduce the traces, the quiescence of the trace when all participants were in the lab. and active, and occasions while students were in the lab. all day, as well as during the entire February experiments.

Mist on the bulb resulting from direct breathing on it must be ruled out: not only is the trace quite different in shape from what we observed, but for one thing, frequently no one was near the apparatus when irregularities happened, and attempts by others to simulate Matthew's movements during periods of trace

quiescence produced nothing whatever.

Table 39 summarises possible normal causes of malfunction other than complex experimenter fraud. Perhaps the best defence against this undisprovable supposition might be that, having gone to this enormous amount of trouble, we might be credited with producing something rather more impressive!

Possible normal causes of disturbance suggested	Considerations against treating factors in left hand column as normal explanations
Mains fluctuations	Stabilised mains; persistence of irregularity after change to battery; days without disturbance before, during and since; escalation of disturbances.
CO ₂ on infra-red	Days without disturbance with numerous persons present before, during and since; escalation and total cessation of disturbances; dissimilarity of trace; onset and incidence when no participant near IR. Failure to replicate by breathing without misting.
Contact faults in circuit before infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance.
Contact faults in circuit after infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance; unilateral direction of disturbances as per DVM readings.
Infra-red bulb	No evidence that this was loose at the time; different shape of trace; days without disturbance before, during and since.
Mechanical intrusion into infra-red beam	Carefully watched and monitored, including video; totally different trace, for most such intrusions.
Mechanical vibrations	Carefully watched and monitored; totally different trace.
Faulty chart recorder	Correspondence with other monitors; stability before, during and since; only suggestion of malfunction if timing taken into account.
Faulty DVM	Stability before, during and since; correspondence with other monitors; could have no causal efficacy on chart record.

Table 39: Considerations against treating various normal causes as explanations of disturbances observed.

5.86 Paranormal hypothesis

Although as will be plain, no categorical claim to paranormal activity can be made, a sufficiently good prima facie case has been established to make it worth while discussing the hypothesis of paranormality. To this day Ellison and Roger Chapman express the view that they have not seen apparatus behaving like this before and can find no normal engineering explanation. David Chapman is more guarded. While, like the rest of us he would have preferred more time with the equipment beforehand, he has given no alternative explanation other than possibly a loosening of the bulb, which I cannot accept as plausible.

On the supposition that what is recorded does not reflect one of the artefacts mentioned or some other not envisaged, the question still arises whether it was the infra-red beam, the electronics of the apparatus, the power supply, or the monitoring instruments that were affected, normally or paranormally.

The only one of these that can be, if not ruled out, at least rendered extremely unlikely, is that it was one or perhaps all of the monitoring instruments that were affected : since all were wired in parallel and, so far as may be ascertained, all registered similar irregular deflections. The voltmeters could not have causally affected the chart recorder. One would have to hypothesise an identical tripartite paranormal force acting simultaneously on all three. Nothing in the proceedings suggests such fine and planned control.

Although on the face of it occultations of the infra-red were obtained and the infra-red apparatus responded appropriately whenever tested, it cannot, in the absence of monitors and switching arrangements in different parts of the circuit, be claimed with complete certainty that it was in fact some interference with the beam that was being measured.

It was thought at first that action on the infra-red beam was virtually ruled out by the fact that Matthew had apparently been able on at least one occasion, and on demand, to raise the voltage above the 1000 mV limit. It will be recalled that, since this trial could not be reflected on the chart, participants dictated the DVM readings into the tape recorder. However, whereas the noise level of the instrument was ca \pm 20 mV the

readings as recorded never exceeded 1032. Since this constitutes less than 10% of characteristic disturbances in the opposite direction (i.e. lowering of the voltage) obtained during periods of disturbance (see columns of minima in Tables 36, 37 and 38), this small upward fluctuation cannot be regarded as significant.

It should also be stressed that the DVM is not on record on a single occasion as having fluctuated spontaneously in an upward direction to any substantial extent (i.e. one approaching the values corresponding to downward irregularities) such as 1300 or 1500 mV. This would, without any doubt, have been spotted by participants. The unilaterally downward direction of significant irregularity tends to suggest interference with the infra-red. A fault in the DVM could not have affected the analogue voltmeter, which similarly was never observed to exceed 1000.

Normal electrical faults in the circuit after the source would have been expected to fluctuate equally in both directions; this should have shown up in DVM readings, which it did not. Hence on a paranormal interpretation, if the modus operandi was not on the IR itself, it would have to have been somehow psychologically guided in a downward direction in accordance with Matthew's subjective expectations and those of most participants, especially myself. It would have conflicted with those of Prof. Ellison, who was fairly firmly convinced that some paranormal electrical effect rather than any interference with the infra-red was involved.

Thus, if the effects obtained were paranormal (and it must be remembered that 'paranormal' is a residual category), three possible loci of paranormal influence might be isolated: power supply, electronic circuit or infra-red beam. It is of course not necessary to suppose that interference between source and cell would necessarily be due to an 'interloping substance' as hypothesised by Osty. As stated at the outset of this paper, the term occultation was used neutrally simply to indicate that ostensibly less light was recorded as falling on the cell than corresponded to 1000 mV. Roger Chapman has pointed out that, for example, the beam might also have been deflected away from the detector. None of these rival interpretations can at this stage be conclusively excluded. However, as has been mentioned, the downwards direction favours interference with the infra-red as against an

electrical effect. For the time being the hypothesis of some paranormal intervention between source and cell, suggested by Osty in the case of Rudi, is also available here. However, in the present case, the hypothesis is less strongly supported because, unlike Osty, we did not have available multiple beams which the Subject affected on demand, and on the face of it, Matthew at no time achieved anything like the control demonstrated by 'Olga', Rudi's secondary personality.

On the other hand, it must be conceded that in the days of Osty, Hope and Rayleigh an audio record of the completeness of ours was simply not technically available; therefore, any 'bandwagon effect' would almost certainly have escaped detection. It seems important to consider the paranormal hypothesis that the psychic (Rudi) was operating on slight fluctuations in noise levels of the instrument, which would reach him and others by normal sensory routes such as the slightly louder scratching of the pen of a recorder. On the other hand, the gentle upward trace at onset of irregularities observed with Matthew is missing in Rudi's case (see e.g. Figure 14), which would seem to counterindicate 'bandwagoning'. There can also be no doubt of the numerous experimenter initiated trials in Rudi's case, particularly with Osty. It is of course quite possible that it is not the same phenomenon which was perhaps captured in the two cases.

5.87 Some psychological comments on IR experiments

At no time was Matthew in trance, nor did he display any altered state of consciousness beyond that which anybody displays who is concentrating on a task. This is in sharp contrast with Rudi, who was always in trance when phenomena were reported. It may or may not be psychologically significant that Matthew increasingly displayed aggressivity towards the instrument, and also engaged in aggressive fantasies towards his enemies: he actually imported the photograph of one of them as a focus of hatred on one occasion. This clashes sharply with his own self-image as one who never uses his talents aggressively (90).

It might well be that, on a paranormal interpretation, the irregularities could be described as due to some strange direct temporary 'haunting' of the apparatus or power supply, rather than to the extrusion of some sort of 'psychic matter'. On the other hand, the systematic downward nature of the voltage

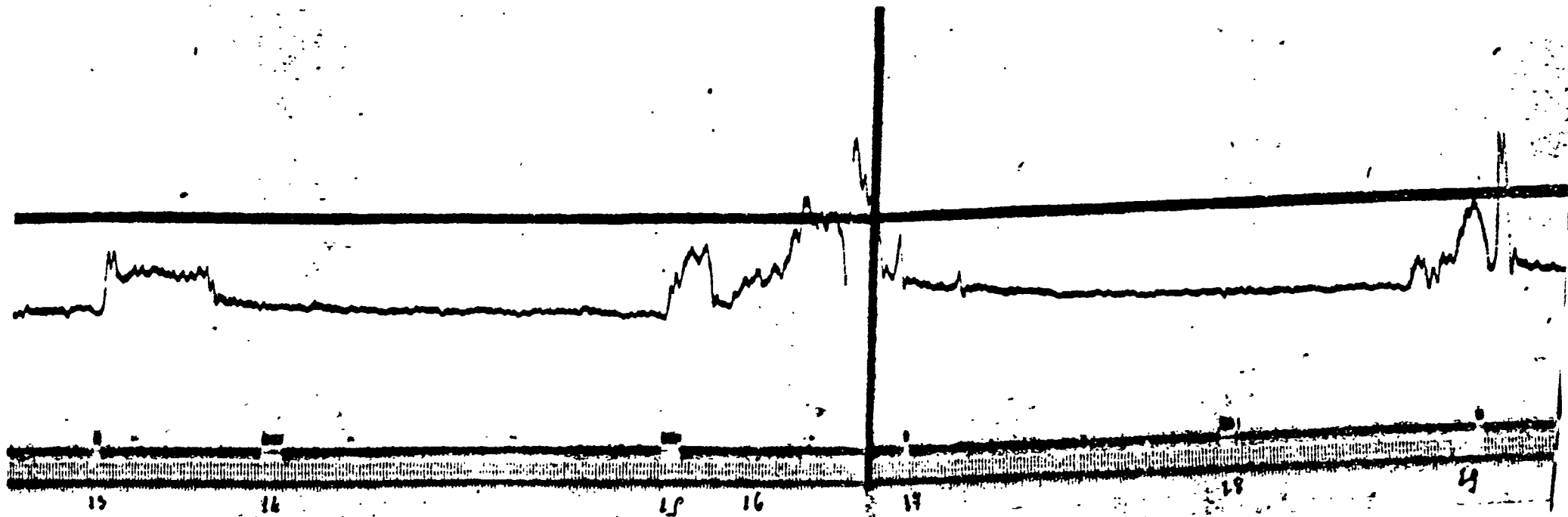


Figure 14. Part of Osty's Figure no. 35, referring to sitting of 26 May 1931. Trace ostensibly similar to tracings obtained with Matthew; note however absence of gradual initial slope.

tends to support this latter possibility.

Taken altogether, the 'deviant charts', charts 1, 2 and 4 suggest activity periodic at first and then more and more disorganised and extensive. The overall impression is that of having created and captured a limited poltergeist outbreak in a laboratory. (It will be remembered that Matthew's early poltergeist phenomena started as limited sporadic episodes and ended as a chaotic shambles.)

Matthew frequently or even usually thought or felt he had control over the disturbances but the record does not support this, quite apart from the 'bandwagoning' effect. Occasionally he tried and obtained nothing; he could not stop irregularities once they were under way; and he predicted irregularity when this did not occur.

On a paranormal interpretation it looks more as though in some way he (or the group) created instability in the system under investigation, at whatever level, and when this instability as fed back by the DVM reached a certain level, Matthew either enhanced this, or 'jumped on the bandwagon' as the irregularity increased in any case. Voluntary conscious action must be ruled out: perhaps some autonomic analogue reaching a crescendo over a period of days, or some isolated fluctuations or firings eventually coalescing into a quasi-seizure would be better models.

It is by no means wholly clear exactly who, on a paranormal interpretation, was responsible for the effects. Certainly all participants and Matthew thought of him as responsible for irregularities and indeed if he in some way introduced noise into the physical system which escalated over a period and then vanished, this would be an appropriate supposition.

However, the presence of neither Matthew himself, nor that of any of the principal participants, Barrington, Brown, Ellison, Inglis, West or my own, was essential for the production of irregularities.

David Chapman and F. Sullivan were in the laboratory most of the time in any case. Chapman was the firmest 'goat' in the team, and Sullivan was only marginally involved and was often busy with other matters. Malfunction continued

at peak intensity in the presence of J.B. who was not only a total sceptic but openly contemptuous and hostile. The 'Grattan-Guinness peak' presents another enigma, occurring as it does in Matthew's absence and while all those present are quietly otherwise occupied. On a paranormal interpretation either Matthew exerted PK on the instrument which became partially independent of him, or else the effect was a group phenomenon, with Matthew as psychological focus of the group which may have included persons at a considerable distance.

5.88 Some concluding observations on IR experiments

The present investigation was explicitly a mixture between a seance and an experimental set-up, and the standard antiseptic type of write-up is therefore not appropriate in this instance. In view of the nature of the enterprise, errors and defects in the record have been made explicit and even emphasised. It seemed clear that only by detailing these shortcomings is it possible to assess the evidence one way or another, and, more importantly, design future experiments in a field which has remained controversial despite a hundred years of dedicated experimentation. However, the prima facie evidence does seem good, and in an established and accepted field of endeavour resources, human and financial, would automatically be forthcoming to clarify the questions raised. It may seem that a large number of ambiguities found could easily have been remedied by an injection of resources relatively small in comparison with the huge sums habitually lavished on scientific projects.

5.9 Future IR experimentation

It seems possible in the light of the Manning investigation to suggest how to set about designing experiments concerning the nature of these ostensible infra-red phenomena. In order to obtain adequate normal control baselines it is essential to install, test and run equipment for substantial periods of time before experiments are conducted at all. It is often claimed, especially in the case of spontaneous phenomena, that apparatus is liable to malfunction in the presence of psychics; as D. Chapman observed, seeing that instruments are usually (under

such circumstances) used for the first time under strange conditions, it would be a miracle if they did not malfunction. This is of course a problem for all investigation of spontaneous cases and investigating physical phenomena in the laboratory almost unavoidably presents some of the difficulties like those encountered in spontaneous cases. At any rate, the instrumental problems can be overcome by an insistence that satisfactory apparatus must be installed and working prior to the introduction of subjects, and continuous records kept of normal functioning.

Automatic monitors and control switches should be introduced at each stage of the circuit, so that it is possible to ascertain at which point in the circuit the paranormal influence, if any, is exerted. This would make it relatively easy to establish whether the phenomenon is one involving power supply, electronic circuitry or the infra-red beam itself. Monitoring devices should be digital and their indications automatically recorded on multi-track audio tape for easy computer analysis, and if desired, there should be computer graphic reproduction and print-out.

Ideally from a methodological point of view, one should of course have a protocol containing pre-determined random trial and control periods; it might be possible by suitable psychological devices such as incorporating periods of activity and non-activity in a game to make a psychic Subject willing and/or able to comply with such experimental conditions. However, it is by no means certain that this is psychologically at all promising, especially if the 'bandwagon effect' plays a substantial role, or if the psychic creates some sort of noise in a physical system partially independent of him/herself. If, on the paranormal hypothesis, the Subject somehow either capitalises on normal fluctuations or initiates minor paranormal fluctuations, when he is in some (unknown) state fit to do so, the Subject is far more likely going to feel the need to initiate periods of activity him or herself. For all we know the Subject may have subtly to combine some inner physiological or psychological fluctuations with some instrumental fluctuation in order so to capitalise in a paranormal manner. Furthermore, trials should be made leaving the instrument 'uninfluenced' once slight instability becomes manifest, and also for other participants than the ostensible psychic agent to 'try' and affect the apparatus.

An alternative to trial and control periods would be the provision of independent, duplicate apparatus, such that one is the experimental, the other the control apparatus. If an effect is captured on one of the instruments it would then be possible not only to establish paranormality beyond reasonable doubt by suitable manipulation, but to begin on the investigation of some of the basic psychological variables which has barely started. However, as has been pointed out by Grattan-Guinness (91), there are major objections to such a procedure also. None the less, it would in my view be worth trying.

Once one has installed satisfactory duplicate equipment on a relatively permanent basis, attention can be devoted to the selection and preparation of suitable subjects. One possible group is that of former poltergeist children or alleged physical mediums; another would be children or others who had given indications of being 'metal benders'. It might also be useful to attempt to test individuals who felt themselves to be notably destructive, unsuccessful or clumsy with machinery. Ideally of course one would attempt to train individuals as PK subjects, although for the time being this remains a somewhat futuristic project. Intermediate between the naturally occurring special Subject and the trained one might be the joint efforts of a sitter group. An imaginary 'Philip', on the lines of Owen (92) might well influence apparatus installed on the lines that we have advocated; however, for the physical effectiveness of such an artefactual person to carry scientific conviction, it would have to influence stable, reliable equipment.

During all attempts at experimentation, conversation should be monitored by means of audio recording, if only to provide clues for the interpretation of unexpected effects; and also video apparatus of adequate quality is desirable for security reasons. Audio, video and other monitoring channels should all be linked to a common time channel so that events can subsequently be synchronised automatically with one another and with independent clock time.

Over a hundred years of experimenting with the physical phenomena suggests that to demonstrate them ad hoc to the satisfaction of many observers, even competent and critical ones,

is not too difficult, but that to capture them in a permanently satisfactory manner for those not personally involved as participants, observers or experimenters is quite another matter.

5.10 Some overall considerations

As has been indicated, Matthew's career as a definite claimant to paranormal powers seems to be over; nowadays he quite frankly uses the publicity and aura of mystery he had gained from earlier events in his life as a sort of halo-enhancer for his new role as a spiritual healer, leader and teacher of mankind. It seems useful to consider his career as a special PK Subject under a number of headings.

5.10.1 Authenticity of earlier phenomena

It will have become apparent from the biographical data that there is really no satisfactory evidence for the genuineness of Matthew's earlier phenomena, any more than there is in the Enfield case. A careful perusal of his father's letters to Dr. Owen shows that almost all the testimony for PK depends on Matthew's own claims as to what happened when he was by himself, and some of the extracts indicate that Prof. West and Mr. Cornell were right in their assessment of Mr. Manning senior's apparent extreme credulity. Whereas Dr. Owen's letters demonstrate caution against any possible spiritualist or survivalist interpretation on the part of Mr. Manning, there is not the slightest indication of even the most elementary caution against trickery, or even self-deception, by Matthew himself.

Matthew has clearly had access to the D. Manning - A.R.G. Owen correspondence for writing his books, and uses the data provided with considerable skill, presenting them to optimum advantage in suggesting that marvel was piled upon marvel. Yet the over-all impression that emerges from reading Matthew's book on the one hand, and from reading the correspondence and from compiling an actual sequential time-table on the other, could hardly contrast more sharply. From Matthew's books (and they do not, so far as I have been able to tell, contain any actual falsehoods) everything seems authentic; from the original papers everything seems bathed in ambiguity and dubiousness.

Reading the work done with Matthew by researchers in Europe and North America (93, 94, 95) one receives the impression of a small, ambiguous and very unreliable PK talent, certainly as displayed in the laboratory. Matthew has used these data with great skill to sustain his version of total authenticity of the earlier PK data. He subsequently abandoned or rid himself of poltergeist types of activity in favour of automatic writing and drawing, and it is entirely obscure to what extent he obtained all the data he needed from books, manuscripts and other documents freely available at his home and at the Cambridge Records Office. There is nothing particularly far-fetched about the supposition that concentration on a history O-level project should lead a pupil to engage in vivid daydream-like experiences of further activities by the historical dramatis personae and that these latter could take on some life of their own, especially as such active imagination is a normal part of good secondary history teaching method (96). As for the poltergeisterei, once again, in the absence of any contemporary testimony and evidence of any quality, it seems unlikely that the problem of earlier authenticity is capable of resolution. Once more incidentally we encounter the mark of Harry Price, whose book Poltergeist Over England (97) Mr. Manning senior had read, and which he found helpful in telling him what to expect.

5.10.2 Some psychological issues

It is all too plain that there are special problems in considering general psychological characteristics of an individual, named and living Subject, and one moreover to whom one has but limited access, some of which have been briefly indicated elsewhere (98).

If one adopts a limited and non-doctrinaire psychodynamic approach in considering Matthew's personal development, a profile emerges which is hardly strikingly different from what might be expected from a non-psychic Subject.

In Matthew's own view, the purpose of the early poltergeist disturbances was principally to disrupt his parents', especially his father's, ordered existence 'without being for it' (99).

My father is an extremely structured person. He does everything according to a plan, and to routine. Everything is time-tabled...if anything upsets his time-table he gets into a flat spin, he's completely inflexible whereas I do things completely impulsively and I hate any kind of inflexibility of time-table...I think it was a means of disrupting his structured existence without being given the blame for it directly.

Nothing emerges from Table 32 to suggest any particularly critical event or constellation. We cannot be certain of the mother's electric shock; perhaps some quite minor incident was subsequently exaggerated in response to persistent suggestion that there must have been something out of the ordinary about Matthew's early history.

Neither the cutting off by the tide and horror of fishes, nor the episode of the beating administered to his brother, nor nightmares in response to the death of a fellow pupil in themselves seem particularly unusual. It might be thought significant that Matthew had actually forgotten the beating of Andrew and also that he had apparently never mentioned his fellow pupil's death to his parents, until the interview in November 1978.

The very first manifestations during the poltergeist time were the moving of the father's prize tankard, and putting a bunch of flowers in front of his mother's place at table. 'Webbe' disputed the family's, presumably the father's, right to the house, and certainly to his own bed.

The early relatively sedate disturbances soon got out of hand after the manner of temper tantrums and grew more and more chaotic and frightening, and Matthew himself became frightened and took refuge in his parents' bedroom. I think it is quite apparent from reading Matthew's books that his father was more under attack during outbreaks of poltergeistery than anyone else in the house.

Reading Mr. Manning's letters, and the description, which must have originated with him, of the hallucination of being inside, swallowed by, 'Webbe', whom he threatened by invoking Matthew, it becomes all too apparent that at quite an early age Matthew gained an ascendancy over his father which is certainly on the face of it unusual. It is quite clear

that Mr. Manning senior is a sophisticated, educated and knowledgeable person, successful in his work and easy socially. Yet it is also plain that Matthew increasingly dominated him and his thinking and feeling, and occasionally his very perception of reality.

Matthew complained (100) of lack of interest on the part of his parents in his schoolwork which seemed to surprise both of them, and he also repeatedly expressed resentment at their supposed lack of interest or concern for his plight at the outset of his poltergeist phase. Yet the letters of Manning senior to Owen, the fact that his father suggested contact with the departed by automatic writing and his mother by automatic drawing, and the whole tenor of the relationship as recorded and observed suggested to me that Matthew managed very skilfully to manipulate them into feeling quite guilty about their treatment of him, whilst imposing his will and his imagination upon them.

The behaviour pattern was also discernible in his conduct in the case of the City University experiments. As has been indicated, Matthew has been at loggerheads with nearly every experimenter who has tried to work with him. It certainly seems possible to interpret this partly in terms of just a struggle for ascendancy, since the allegiance of scholars and academics is to impersonal standards, whereas Matthew is entirely committed, in this context at least, to regarding the situation as an adversarial contest. His own description of the duel he sees himself fighting with experimenters (101) lends support to such a view.

In my original discussion of Matthew, which I sent to him for his comments prior to publication, I wrote (102)

It should perhaps be mentioned here that whereas Matthew dislikes and rebels against time-tables which impose any restraints upon himself, he also intensely resents being kept waiting or in any sense inconvenienced by a failure of others to keep to a time-table. As I see him, he likes to be free himself whilst expecting others to be at his instant disposal. Most of us keep this perfectly natural set of desires slightly more in check than he does - or did.

He commented (103, 104)

Re timetables! I have refrained from altering observations on my self in this test even if I don't particularly agree with them. However, you may have misunderstood my reaction to timetables, or else I am older and wiser since we spoke about them! I don't entirely agree with your comment that I like to be free whilst expecting others to be at my instant disposal. That's not really true and makes me sound like a megalomaniac. I'll say no more!

The passage is entirely characteristic for Matthew - a curious blend of unconscious arrogance, self-deception (as I see it) and disarming charm: I would hardly have allowed, let alone invited him, to alter what I had written! His conduct during the experiments was such that (like others before me), I felt, PK or no PK, the game with Matthew was hardly worth the candle. I will give a few examples of his behaviour not with the intention of providing anecdotes, but to illustrate a more general theoretical, if admittedly obscure, issue to be indicated.

As already mentioned, Matthew would demand alterations to be made to experiments and apparatus, and then express continuous and growing irritation at being kept waiting by the experimenters as if they were unsatisfactory servants. On one occasion (at Barts') he first refused to cooperate with the EEG procedures to which he had agreed, and which had necessitated a good deal of organisation and preparation. He then changed his mind, provided I sat next to him, after which he instructed me to 'tell Arthur (Prof. Ellison) and William (Prof. Brown) to shut up'. Again, his refusal to cooperate with Dr. Grattan-Guinness was entirely typical. It was explained to him that Grattan-Guinness's holiday had been booked long ago, and he regretted not being able to be present; however, he had devised some experiments at a distance. Matthew's response was 'if he can't be bothered to be here, I can't be bothered to work with him'. All things considered, my comments about time-tables cited above were therefore as mild and diplomatic a formulation as was possible for the published paper, and Matthew's response demonstrates all too plainly his total and blissful ostensible unawareness of his conduct, and of its effects on others. Some little time later,

when we met at a reception, he told me he would sue a certain journalist for defamation on the grounds that the latter had called Matthew 'a prima donna'. I said, 'Matthew, you are a prima donna!' and he had the grace to grin.

It seems to me that this general constellation of behaviour and attitudes suggests a clue about how people like Matthew might differ from others: there does seem to be a very substantial capacity actually to impose their perspective and their imagination and view of the world on others, partly by undermining them. In the case of his parents Matthew would seem to have 'won' in what he clearly regarded as a contest. In the case of the experimenters, he likewise needed to see them as adversaries to be defeated and beaten into submission, whilst his self-image throughout was that of a long suffering victim. There is also evidence, for example from a consideration of Dr. Inglis' account (105), that Matthew has quite successfully represented himself as hard-done-by and persecuted, when the grounds for such complaints are hardly discernible in fact.

Now doubtless there is nothing particularly unusual about this generally extra-punitive set of attitudes (which in a more extreme case might be called paranoid), but there just might be a clue here for distinguishing the potential PK agent from others. Matthew has been spectacularly successful in his campaigns for psychological ascendancy from a very early age. There is here, it seems to me, a resolute and unusual refusal to become socialised into seeing the viewpoints of others at a truly basic level, and thus achieving what might, in Freudian language, be called a partial fulfilment of 'omnipotence fantasies', and in Piagetian terms be described as successful 'ego-centrism'.

5.11 Some tentative conclusions

The hypothesis of partially successful direct psychic manipulation of 'the environment' - of persons and of material objects - in an ego-centred or narcissistic direction does, admittedly, do violence to our usual way of regarding psychological development, but then so does the very hypothesis of the reality of PK. On the supposition that authenticity

may be ascribed to some, though by no means necessarily all, of Matthew's phenomena, the following points are suggested:

5.11.1 Successful struggle for hegemony

Personal development is usually described in terms of increased autonomy and independence, and of mastery of the environment via enhanced and refined recognition of reality. The sheer tendency to wish actively to dominate (as opposed to primarily defensive strategies) is only obliquely recognised in most psychodynamic versions of development, with the exception of the rather neglected Adlerian perspective (106), aspects of which, do, however, seem associated with a successful campaign for personal hegemony (as opposed to autonomy) made possible in most cases by a suitable early family constellation.

5.11.2 PK as a group function

As has been pointed out, it was by no means clear that Matthew was personally responsible for the PK effects (if such they were) either initially at his home, or even at City University, although he of course firmly assumes responsibility and claims (107) a non-existent degree of control. The present picture here again supports and extends Batchelder's idea (108) of the importance of attribution to a supposed originator of PK effectiveness, although 'scapegoating' seems hardly appropriate as a metaphor. It looks more as though a particular individual emerges as PK-dominant in the pursuit of some tacit intra-group struggle, and that subsequently by a cooperative, or perhaps quasi-collusive, effort, the psychic may release in other members of the group, or in some constellation of persons, the capacity to affect the physical environment.

If PK be accepted as genuine, and not totally due to error and to deception, then not only our social image of reality but to some extent, probably minor, reality itself is socially derived. This takes place as part of a struggle for dominance. The physical world, and not only the biological kingdom, may be seen as, to some extent however small, the result of selective social pressures.

5.11.3 Continuum of deception and PK

A picture such as this suggests a psychological continuity between deception and PK, rather than a dichotomy or complementarity. 'Contrary-to-fact' imagination and belief may be necessary (though not sufficient) precursors or conditions of paranormal physical actions. This supports Batcheldorian ideas (103) concerning the desirability of suspending disbelief and creating illusions, but adds the notion of a process of actively gaining quasi-charismatic ascendancy as an ingredient in the PK process. The 'bandwagon effect' described in Matthew's case would be half-way between deception and authenticity, a piece of semi-conscious or more likely unconscious opportunism in the active pursuit of overall dominance, collusively (again unconsciously) supported by the relevant group.

5.11.4 Psychological grounds militating against certainty about PK

If the quasi-hypnotic capacity actively to dominate, delude and manipulate others is actually related to the production of genuine PK, this would be an important reason for explaining why, in a hundred years of dedicated research, it has not been possible to give a simple answer to the deceptively simple question, 'Do objects move paranormally or do they not?' It also provides one of the reasons for the extremely violent emotional reactions (for and against) towards claims to paranormality and towards persons to whom such claims are attached.

5.11.5 Importance of automatic record

The City University investigation demonstrates if nothing else the basic importance of automatic instrumental recording. The quality of the auditory record bears witness to the fact that there need be no 'psychic sabotaging' whatever of a recording device capable of capturing many thoroughly relevant events necessary for interpreting other automatic recordings. It should also be said that all participants became sufficiently unconscious of, or at least habituated to, the ever-active microphone to express reasonably freely views and attitudes they might not, to put it mildly, wish to see published in extenso. Both the instrumental recording, and

also participants' freedom to express themselves up to a point,
are probably essential for successful investigation: the
reconciliation of these two conditions is by no means easy.

CHAPTER 6, FINAL REFLECTIONS

This thesis is addressed to the problems encountered in studying PK in Special Subjects, and to elucidate the question why after more than a hundred years of study so little progress has been made. It is hoped that the foregoing has thrown some light on this question: the sheer difficulties, practical as well as theoretical, personal as well as professional, confronting anyone who attempts seriously to investigate macro-PK in Special Subjects are such that it would be surprising if there had been more clear-cut and rapid progress. Many of the problems explored have not even been explicitly recognised, let alone resolved, and it is unlikely that without full recognition and continuous awareness of these problems adequate progress is likely to be made.

On the other hand, two of the cases do provide prima facie evidence for PK in spite of all this, and suggest some psychological and instrumental ways in which this difficult topic might be further explored.

6.1 Problems arising out of the non-recognition of the field

6.11 Economic factors

At the most basic level, as has been shown, there is a chronic and damaging lack of resources, both of money and time, not only for adequate standards to be attained, but also this lack of resources itself becomes a source of conflict and damage to investigations.

This was a factor in all the cases considered: the chronic shortage of funds emerged clearly in the Schneider case, where Osty, Hope as well as Price were vying for money as well as time on the part of suitable participants, and this despite the fact that Rudi required only expenses and minimal pay. It was clearly a factor in my encounter with SH, and the inadequate machines and methods he sold and then defended. It is less directly manifest in the

Enfield saga where, however, the sheer time available to persons better qualified than the investigators was a factor. It was, however, quite plain in the Manning case, in which not only the time available to participants, but the equipment (hardware as well as software) was far from adequate. For example, I had difficulty in persuading the technicians to 'waste' paper in running control strips of chart recordings when no experiments were in progress; only unsatisfactory video equipment was available; there simply was not enough technician support to make minor adjustments to equipment which would have made a great deal of difference. There was not the money to purchase adequate automatic timing devices and so forth. A little money was indeed made available by a private benefactor to pay for the Subject's expenses and hotel bill, and subsequent printing of the report. Yet it cannot be denied that even this by normal scientific standards small contribution was used to exert pressure on the experimenters both directly and indirectly (for which there is some evidence in the audio record).

This shortage, both of money for adequate modern equipment and above all of research time for sufficiently qualified persons to devote their energy and ingenuity to an issue, needs to be overcome if the subject is to advance. At present the non-validated nature of the field results in a Catch-22 situation amounting to an academic poverty trap: without the economic resources the standards necessary for academic recognition will not be forthcoming; and this recognition will be withheld unless such standards are attained.

6.12 Professional considerations

It has been seen that the professional expertise required for investigating even a single case is extremely manifold and varied, different methods and disciplines being necessary which any one individual is hardly likely to combine. To this must be added the ancillary skills in terms of administration, secretarial and technical assistance that are essential for the successful pursuit of in-depth investigations.

Many of these areas of competence are not in themselves different from those needed in normal subjects and professional pursuits such as psychology, counselling, physics, medicine, social work, engineering etc. However, as was seen, the ad hoc convening of relevant teams is difficult and problematic in an area which is never the actual focus of anyone's long-term and primary professional preoccupation. Even if a heroic effort is made for an actual investigation, neither is the prior preparation, nor the subsequent evaluation, at all likely to be sufficient to ensure adequate standards and progress in a peculiarly difficult area, and this is of course connected with the issues mentioned under 6.11.

The need for time beforehand, both for psychological preparation, briefing etc. and for adequate testing of equipment etc., can hardly be overstressed, nor the need for adequate time and expertise for subsequent assessment and evaluation; and it is necessary for researchers to be able to work without pressure to produce spectacular results for media consumption or to satisfy private benefactors who have made contributions towards the investigation. Without these prerequisites, the subject is likely to remain a string of tantalising episodes.

Furthermore, all the cases have shown the scars of strife between investigators from professional backgrounds on the one hand, and more frankly commercial ones on the other. In an area which is not, or barely, officially recognised, and where there are consequently no universally recognised professional qualifications, interested persons, often with more time and resources than professional people, may demand equal if not higher status than the latter, and the contact between the two sides is likely to be one of bitter hostility to the long-term detriment of the field. Criteria of excellence, respect for and treatment of records and data, priorities in conduct, all these are likely to be so different that, without specific hierarchical structuring and more precise and accepted role definition, cooperation is impossible and competition destructive.

6.2 Problems arising from the sensational nature of the field

Again, all the cases manifest the problems that are liable to arise if findings, or at least claims arising out of such findings, are of media and journalistic interest. A competent investigator may or may not combine with his research skills an ability to communicate his findings attractively. He is unlikely to be a match for, and indeed he is likely to have different priorities from, those whose main business it is to work in the media and in publishing.

Whereas undoubtedly, in the absence of systematic and adequate funding, media coverage at least keeps the subject alive after a fashion, this is also an ever-present occasion for down-grading it and keeping it amateurish, inconclusive and contentious. This hazard is exemplified in all the cases examined, quite irrespective of their merits.

6.3 Problems arising out of investigating Special Subjects

Some of the problems here are implicitly connected with issues touched on under 6.1 and 6.2, others are relatively separate.

6.31 Scarcity value of Special Subjects

No time has been devoted in the thesis to the question why 'great mediums' have so largely disappeared, although possible contributory factors will be suggested below (p. 293). It is of course widely believed that the disappearance of such mediums shows that the older cases were entirely spurious, but this is not a view suggested either by the detailed analysis of the Schneider case or by the more vestigial findings of the Manning investigation. (Some other, older cases, notably those of D.D. Home and Eusapia Palladino also are hard to dismiss.) However, the scarcity of such Subjects itself is of course a source of difficulties.

Moreover, to investigate a Subject who has become or is liable at any moment to become, a public celebrity or cult

figure entails problems particularly exhibited, though differently, by the Schneider and Manning cases. Rudi became a 'valuable property' not so much to his own benefit as that of Mr. Price, Matthew became such for the benefit first of his original publishers and then his own. The Harper children came to be semi-public exhibits for the duration of the case at their home rather than enjoying independent notoriety in their own right. If, as in the case of Matthew Manning, the Subject comes to have a vested and quasi-professional interest in being thought to have produced positive results, the sources of difficulty are all too obvious as has been indicated. In the Enfield and Schneider cases it was the publicists rather than the Subjects who in various ways stood to gain, and again the dangers have been exemplified. Specified Subjects or groups of Subjects once named, and on whose behalf large claims are made, become vulnerable both to unfair hostile attack and uncritical adulation: either presents thorny problems for investigators.

6.32 Age of Special Subjects

The fact that Subjects are likely to be children (nearly all the Subjects were minors at the onset of investigation) means that special protection is necessary, and that co-operation between researchers and parents as well as possibly teachers and others may be of great importance (and difficulty). At least some experience in dealing with children and young persons is highly desirable, and decisions may have to be taken on Subjects' behalf, e.g. as regards anonymity, priority of preoccupation, residence, etc.

6.33 Social class

This issue is related to 6.32, in that children from different social class backgrounds fare differently and present different problems. In the cases discussed, Schneider and the Harper children, both from working class backgrounds, were clearly far more vulnerable and at risk to manipulation, conscious and non-conscious, from investigators than were Matthew and CMG, both of whom came from professional middle class backgrounds. How this works out in practice, and how desirable a given state of affairs turns out to be, depends

of course on circumstances and one's viewpoint. It is doubtful whether Rudi would ever have become the docile 'scientific medium' he became had it not been for the Schrenck-Notzing - Schneider social gap; and had Rudi been from a professional family and British, Price would hardly have been able to treat him as he did. There can be little doubt that the Enfield 'investigators' would barely have lasted a week in a less deprived and more sophisticated household. Matthew and CMG, on the other hand, were protected by their more privileged and knowledgeable parents both from exploitation and manipulation, but also arguably from adequate investigation.

6.34 Working with named individuals

The discussion of individual case histories in a clinical or near-clinical manner is problematic even if anonymity is guaranteed as (hopefully) in a psychiatric context. Where the individual being discussed is known and named, the difficulties, certainly for publication, can become very acute indeed for reasons which have been indicated, particularly in the course of Chapter 5. The experimenter faced with such a Subject is in a very different position from one dealing with an ordinary Subject in a psychological laboratory experiment.

6.4 Some key issues

6.41 Personality

On the face of it, two persons could hardly be more different than Rudi Schneider and Matthew Manning. Rudi was modest, uninterested in his phenomena, unpretentious and submissive to experimenters. He was also at all times during phenomena in a state of complete dissociation. Matthew (at the time of the investigation) was certain of himself as well as self-assertive, extra-punitive, highly interested in and articulate about his phenomena, and ostensibly fully conscious when trying to exert PK.

Yet when considering 'Olga' the differences become less clear-cut. 'She' was far more like Matthew than was Rudi. 'She' was peremptory, capricious and self-confident, was forever

demanding changes and blaming others for the non-occurrence of phenomena, cooperative provided 'she' was obeyed, thoroughly interested in 'her' own phenomena, and often mistaken about them. and what 'she' could and could not do, without ever losing 'her' self-confidence in consequence.

It is tentatively suggested that such a dominant, confident but limited and extremely ego-centered (in the Piagetian sense) personality configuration may be linked with the production of PK phenomena. For most of humanity normal development takes the form of increased impartiality, social awareness, renunciation of exclusively personal viewpoints and perspectives and so forth (and that, as Donaldson in particular has shown, at far earlier ages than envisaged by Piaget (1)).

These reflections could also be couched in psychoanalytic language of infantile narcissism and omnipotence fantasies, and gradual acceptance of 'reality' and 'the reality principle'. Psychoanalytic terminology (for all its drawbacks and question-beggings) might have the advantage of stressing emotional and motivational factors over and above the more intellectual classifications considered in developmental cognitive psychology. However, strategies more active and aggressive than mere Freudian 'defence mechanisms' are here envisaged, more on the neo-Nietzschian lines suggested by Adler. Both types of terminology are used only for descriptive or, perhaps better, evocative purposes.

It is suggested that perhaps 'in' (or in conjunction with, see below 6.42) some individuals there develops a person or unintegrated part-person or sub-person or personified complex who refuses to 'decentre' yet who, instead of paying the usual price of physical incompetence, social mal-adjustment and psychopathology, to some small extent manages to subjugate the physical environment to his or her wishes. Such a 'person' is seen as, so to speak, refusing to accept 'physical impossibility', but managing to mobilise resources in the world unknown to present-day physical science, and to impose his or her own viewpoint on the world rather than to accept the world as it 'is'.

Such persons might be expected also successfully to impose (and indeed possibly to arise out of the struggle successfully to impose) their will and imagination on others,

generating conformity and compliance possibly to the point of provoking or encouraging delusory experiences. Individuals manifesting such persons, with or without the production of PK, would, whilst this person was in control, be described as charismatic.

This state of affairs would obviously constitute a particularly severe problem for the investigation of PK and would go a long way to explaining its long-standing association with fraud and deception even in the case of normally, and/or originally, sincere Subjects. Such dominant persons would unavoidably arouse (as well as reciprocate) the hostility of anyone who did not fully capitulate to their current picture of 'reality' and acknowledge their ascendancy.

Also, such persons would be unlikely to last in that condition indefinitely, since all available evidence suggests that abilities to mobilise unknown forces are limited and transitory. As 'consensus reality' inevitably asserted itself, these persons lose paranormal hegemony, which they would then have to renounce or fake.

On this picture, any scientific experimental situation would, in itself be detrimental to PK since, however ardently and genuinely the experimenters might wish for positive results, they would by definition remain objective at some level, i.e. they would at all times from the very nature of their enterprise be envisaging the non-occurrence situation. This professional 'set', even if banished from consciousness, is bound to arouse the hostility of the Subject since the scientists would be, quite correctly, felt to be demanding and undermining phenomena at one and the same time.

Thus self-annihilating conflict is actually built into the very experimental PK situation: it is in my view little wonder that in the circumstances there has been no more progress than there was.

If these considerations have any validity, then the spread of universal education, and particularly primary education with its conscious emphasis on teaching children to 'decentre', i.e. to become objective, impartial, and view the worlds of facts and of number as independent of themselves and as given and subject to constancy, would itself sharply militate

against the emergence of physical mediums. It is of course widely believed that it is the spread of education and enlightenment that leads to the unmasking of fraud and error, and doubtless this is also true. However, if my arguments are correct, then these factors militate against the development of the genuine phenomena also, and would partly account for the dearth, certainly in the Western modern post-war world, of physical mediums.

6.42 Group nature of PK

Both the Schneider and Manning cases raise forcibly the difficulty of actually correctly attributing PK to a particular named human being. (They are not unique in this, but such considerations are virtually never addressed directly.) The archaic 'faculty' in the definition of the SPR's aims and objectives (see Chapter 1, p. 3) is usually implicitly taken literally by researchers without further reflection. PK is seen as an attribute or property or talent, actual or potential, of a given individual. Yet this assumption needs to be examined and qualified. In the Schneider case it was seen how the PK-producing person 'Olga' was first manifested by Willy, changing over to become Rudi's 'control', Willy then manifesting another 'person'. Another brother, Karl, also was a physical medium and there was considerable evidence to suggest that the presence of Karl, and of some of the members of the Braunau circle, in some way contributed to the phenomena. Schrenck-Notzing, on the other hand, and the laboratory setting in general, seems to have been somewhat (though by no means fully) inhibitory, despite Schrenck's unswerving belief. This would accord with considerations outlined in 6.41. (Nobody, so far as I am aware, actually attempted to sit without Rudi himself; the notion of a mediumless sitting did not arise until the Owens' 'Philip' experiments (2).)

Although it seemed that Rudi and he alone was a necessary, though not sufficient, condition for the manifestation of phenomena, there was a participant factor, suggesting that there was some uncertainty of source and origin. Again, in Matthew's case, the first outbreak of poltergeist activity seems not to have been connected with him at all, it was only Dr. Owen's

speculation that Matthew, being nearest adolescence, might have been the 'focus' of the disturbances. Indeed, any of the Manning family, particularly perhaps Mr. Derek Manning, might have been strong contributory sources or factors.

It was seen that in the laboratory Matthew himself was only quite weakly related to the disturbances of the IR equipment, one of the most marked incidents, the 'Grattan-Guinness peak', occurring in his actual absence and without any intentionality on his part; interference with the IR began to Matthew's surprise and on occasion having once started continued in his absence and without his participation. It is also of interest that the presence of J.B., a vocally contemptuous and hostile sceptic, in no wise inhibited the phenomena.

Both the Schneider and Manning cases lend support to the notion, not so much of an experimenter effect as a participant effect, and suggest the notion that PK is the result of complex, largely obscure interactive group factors, and by no means attached securely to, or the property or characteristic of, any one individual. It has been suggested that an important aspect of such group interaction might be the strong ascendancy or hegemony of a particular 'person', together with the collaboration, perhaps unconscious 'collusive' submission, on the part of others. This ascendant 'person' is not necessarily fully co-extensive with a particularly named human being, and may only fasten on, govern, control, direct a given individual in part and/or at certain times. This suggests a re-appraisal of the old notion of the PK Subject as indeed a medium though not for discarnate spirit entities but rather for the expression of fluctuating and fluid expressions of social group factors in particular in the context of a struggle for dominance.

In the context of individual personal development it was suggested that education would tend to counteract the emergence of great physical mediums. In the group context this tendency would be reinforced by increasing Western social high valuation of personal autonomy, accountability and critical responsibility and egalitarian ideas generally, and social hostility towards 'great men', elites, chosen

minorities, charismatic individuals and the subordination and subjugation of persons, classes and social groups. Such shifts in social ethos (particularly since the Second World War) are of course neither absolute nor universal, nor necessarily permanent: they are however possibly sufficiently marked, extensive and passionately held to constitute another strong set of contributory factors militating against the production of physical phenomena on the grand scale.

6.43 Psychological continuity of deception and PK

If it is correct that there is continuity between one person's (or part person's) imposing his or her will and imagination on a group in spite of contrary signals from the physical environment and normal social priorities on the one hand, and the actual production of authentic phenomena on the other, then the psychological bi-polarity between deception and authenticity must be abandoned. This would indicate that traditional SPR policy of rejecting all phenomena of any medium once caught in fraud or trickery was in principle an erroneous one and which, on the view here advocated, also contributed to the disappearance of genuine mediumship.

Nevertheless, a consideration of the Enfield case suggests that, however reprehensible this SPR rigidity might have been in the case of, say, Palladino (3) and however misleading from a theoretical perspective, in a good many less illustrious cases, there was and is hardly any practical alternative to abandoning and dismissing a case once systematic long-term trickery and self-deception has been established; and the Price-Schneider imbroglio shows just how dangerous collaboration with an unreliable individual, in this instance the investigator, can be.

6.44 Relation between normal (including abnormal) and paranormal factors

It was seen that no special factors could be identified that were characteristic or even particularly unusual in the early histories of Rudi or of Matthew, the only possible exception being the ascendancy gained by Matthew over the rest of his family. In Rudi's case the only somewhat unusual aspect is perhaps the Schneider parents' long-term unswerving determination to have a girl, which may have partially

triumphed over mere anatomy in the genesis of 'Olga'. This could also explain some of the mild warfare between Rudi and 'Olga', and perhaps why 'Olga' remained at all times an unabsorbed secondary personality.

But when all is said and done, there do not seem to be discernible any factors here that do not operate in cases when, so far as we know, there is no question of PK, or exceedingly dubious ones like the state of affairs at Enfield.

Just as we have little or no idea why stress and conflict should manifest themselves as schizophrenic, depressive, hysterical, obsessional, psycho-somatic etc. symptoms, in different individuals, and sometimes have no apparent ill-effects, so we do not know why occasionally genuine PK is associated with particular persons or groups. This underlines the indispensability of actual psychical research authenticating skills in assessing whether genuine physical phenomena have or have not occurred.

6.5 Future research

As has been seen, research into macro-PK is beset by a number of problems and obstacles some of which may be difficult to resolve, certainly in the short term. The current economic climate is such that the academic poverty trap is likely to continue to operate, and this means that some time could elapse before the resources, professional and technological, necessary for adequate research and authentication may become available.

If, however, the financial resources were to be forthcoming, it is now at least possible to outline ideal conditions for the pursuit of macro-PK. These would involve the availability of a number of trained professional psychical researchers able to devote enough time to the preparation, pursuit and evaluation of investigations. This number of trained professionals need not be very large but should be sufficient for mutual collaboration as well as criticism. Probably small teams comprising workers from complementary backgrounds of expertise would be desirable, both psychological and physical/engineering competence being essential: the latter

would at least have to be available on a consultancy basis.

Such researchers would need the same freedom as those in other fields from pressure on the part of funding bodies to produce results etc.

They also need enough technological resources to devise, construct, test and maintain modern equipment which needs to be available and ready as and when cases are reported. Equipment needs to be of two types: portable, for use in domestic surroundings and field conditions, and permanently installed for laboratory investigation. Monitoring by means of audio and video equipment is vital in laboratory-based research, and the requisite ancillary technical, computer-based and secretarial services are also essential.

The formation of small but efficient and well-equipped research units would seem to offer the ideal organisational format for such professional nuclei, and attachment to either a university or other research institution, possibly a major research and teaching hospital, seems indispensable for the economical sharing of resources.

It has been suggested that problems are not merely practical (formidable though these have been shown to be) but arise from dilemmas embedded in the very nature of the field. If it is correct that modern education, social values and the very application of scientific research methods to the phenomena tend to diminish their occurrence, this does not augur well for the emergence of satisfactory new major Special Subjects. However, the ostensible success of the very first attempt at replicating the infra-red experiments obtained in the case of the last of the great mediums, Rudi, with a former poltergeist child, Matthew, suggests that Special Subjects need not be sought among the ranks of great mediums. Some discussion of the future of infra-red experimentation was provided in Chapter 5 (pp. 273-76), but it also seems well worth exploring other possible indicators of extra-somatic physical activity such as electrical effects, other wave lengths of light, metal bending etc.

It may also be possible, given finance and opportunity, for field work to be conducted in countries the social ethos and education of which is not yet strongly affected by Western

values, such as parts of South America, and more linking with anthropologists might be of considerable value in this connection.

More thought needs to be given to the psychology of the active partner in the transmission of thought and imagery, i.e. the function of the successful agent or sender in what is normally thought of as an ESP context. On the view here suggested, such an agent might be an early link in the chain leading to genuine PK (or unfortunately fraud). Mental influencing is seen to be in a sense continuous with PK, and a replication and extension of the experiments of Vasiliev (4) seems highly desirable.

Although for the satisfactory academic validation of macro-PK substantial financial resources are, as has been argued, indispensable for the resolution of both technical and professional difficulties, there are a number of directions in which the subject might develop in the meanwhile. Indeed this is essential if the subject is to be kept alive in the absence of adequate resources.

Re-thinking of the philosophical and theoretical psychological foundations of psychokinesis is desirable. The very notion of PK and the ostensible transcending of what is physically possible suggests a re-appraisal of concepts such as 'reality' and 'existence'. At the same time, moral and political values such as autonomy, heteronomy and hegemony should be re-examined in this context. Not only the logical but also the psychology of active willing and striving needs to be further explored. Eisenbud (5) has made one excursion into their territory which, although in my view it is not altogether successful and suffers from being too tightly linked with orthodox Freudian thinking, is bold and of some interest.

Problems in PK touch on the very springs of personal development and group interaction, and the lack of even a satisfactory language to describe these has been felt to be acute from time to time. Psychical research is faced with much the same demands as regular psychology: there is need both for the 'hard' skills of experimenting, monitoring and controlling, and the 'soft' skills of understanding, differentiated empathy and disciplined imagination. It may well be that

psychical research, and especially the interactions between investigators and Special Subjects may have a contribution to make to the field of general personal and inter-personal psychology.

One way in which the subject might be advanced is by the formation of small PK groups devoted not merely to the production of phenomena (such as 'sitter groups') but to the explicit exploration of inter-personal group-dynamics in this context. Such groups might incidentally serve a training function, and become a nucleus for providing at least moderately skilled and disciplined field workers, available for spontaneous cases as and when the need arises. Knowledge of psychical research, experience of practical investigation, at least some of the usual skills of counselling and/or therapy and some technical competence would have to be represented in such a group.

It seems to me that if the psychology of macro-PK is as intimately linked with the desire to gain ascendancy and domination as has been suggested, then the context in which PK is most likely to be explored in actual practice is in that of games, where such desires, though intense, are socially sanctioned yet rule-governed and hence limited. Computer-based strongly adversarially and competitively organised team games seem to me to present far more promising settings than the usual solitary PK tasks, and this might be further explored perhaps by the groups whose formation has been suggested. These might also conceivably serve as a way of dispensing with reliance on naturally occurring Special Subjects, at least in this context, although I suspect that 'facilitating' or perhaps 'catalytic' star participants would emerge.

Some of the speculation in this final chapter has been concerned with questions of the imposition of imagery and intention on the physical world and on persons. A field of far more practical and immediately apparent importance to every human being is the extent to which each of us can influence our own bodily - and for that matter mental and emotional - states by wishing, willing and imagining. This

may well be the normal way in which PK operates in the world, and our control is far from perfect or complete, or indeed even understood in any systematic manner. It could well be that systematic exploration of endo-somatic influence could ultimately cast important light on exo-somatic influence, and that explorations into the nature of healing, self-healing and self-development may in the long run cast important light on the origin and status of the macrophysical phenomena of psychical research.

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Abbreviations

<u>JSPR</u>	<u>Journal</u> of the Society for Psychical Research
<u>Proc SPR</u>	<u>Proceedings</u> of the Society for Psychical Research
<u>JASPR</u>	<u>Journal</u> of the American Society for Psychical Research
HPL	Harry Price Library, University of London, archives
IMI	Institut Métapsychique International, Paris, archives
SPR	Society for Psychical Research, London, archives

Introduction

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AG number, re date, L number

A. Gregory's case notes, page number (as circulated to J.R. Beloff and A.J. Ellison) re date of visit or other specified occasion; L = level as discussed in section 4.11, i.e. level 1 = general and factual, level 2 = includes personal and subjective reactions, level 3 = confidential to F.M. Branch. However, all levels are marked confidential. E.g. AG 24 re 15.9.1978, L1.

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PROBLEMS IN INVESTIGATING PSYCHOKINESIS IN SPECIAL SUBJECTS

SUPPLEMENTARY MATERIAL

List of relevant papers published in the course of preparation
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Reprinted from

Annals of Science

An International Review
of the History of Science
and Technology from
the Thirteenth Century



Published by

Taylor & Francis Ltd

10-14 Macklin Street London WC2B 5NF

A. Gregory

Ph.D. Thesis
Supplementary
Material



ANNALS OF SCIENCE

Volume 34

Number 5

September 1977

*The Editorial Board has been reorganised;
see the inside front cover for details*

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Anatomy of a Fraud: Harry Price and the Medium Rudi Schneider

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Summary

Among the most interesting of the controversies in the recent history of parapsychology and related studies is the claim made in 1933 by the psychical researcher Harry Price that the medium Rudi Schneider had on one particular occasion produced his psychic effects by fraudulent means. The background to this event, and the controversy which followed it, are described in detail in this article, which draws on many hitherto unpublished materials. The issues involved range from the design of experiments in an unusual area of science, through the relation between fringe and orthodox science, to the role of popularisers of science (such as Harry Price) and the ethics of science.

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1. Introduction: psychical research

Psychical research as a systematic and serious study is virtually 100 years old. The Society for Psychical Research was founded in 1882, and has counted among its past presidents some of the most eminent figures in the humanities and sciences. Hundreds of thousands of skilled and learned man-hours have been expended in the pursuit of the elusive phenomena of what has widely come to be known as 'parapsychology'. The American Association for the Advancement of Science has declared the subject to be worthy of academic study. Professorships of parapsychology are gradually being established.

Highly respected scholars and scientists become preoccupied with one or another aspect or claim, and risk—and occasionally retain—their reputations in the process. Meanwhile, the learned world remains deeply divided as to the propriety and importance of a study of phenomena the very definition, let alone authenticity, of which presents problems, and the study of which to date has certainly not, to put it conservatively, contributed to universally acceptable knowledge.

Only relatively recently has the attention of scientists been drawn to some fundamental questions in the history of science which suggest that the progress of science is far from being an ineluctable progress from ignorance to certain knowledge, from supersitition to truth, from darkness to light—that social and psychological factors have played a part in what is, and what is not, accepted and acceptable by a given scientific community at a given time—and that theories and tacit assumptions may colour and at times determine the interpretation of observations. The pursuit of new knowledge, especially when this involves even the possibility of shaking venerable beliefs, is a fairly impassioned affair, and the pursuit of psychical research presents an object lesson in this respect.

Quite often contemporary history cannot be written, not only because of a certain lack of perspective and because much information needed is simply not available, but also because legal complications may render any dispassionate assessment impossible. The laws of slander and libel, for example, may make it difficult or impossible even to describe how certain people behaved—especially if they are wealthy or influential. The picture may stand out more clearly in retrospect and can certainly be told more safely at a later date—which, of course, involves other problems and difficulties of establishing what took place in the more or less distant past.

There is never any shortage of writers willing to believe that current assumptions present the ultimate pinnacles of truth; that iron ships will never float, that without the devil's help light patterns cannot be captured on paper or sound patterns by mechanical devices, that the earth stands still and the sun moves, that machines heavier than air will never fly, that surgical operations without chemical anaesthesia must involve pain and that consequently hypnotic anaesthesia is a fraud, and so on and so forth. It goes without saying that the phenomena of psychical research have met with their fair share of dogmatic *a priori* denial. As Helmholtz so cogently put the matter: 'Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses, would lead me to believe in the transmission of thought from one person to another independently of the recognised channels of sense'.¹

Somewhat more recently, Dr. G. R. Price wrote that, since phenomena such as telepathy are impossible, scientists claiming positive results must be either mistaken or fraudulent, and he invoked of all authorities the philosopher Hume to underpin the *a priori* contention that such matters are inherently impossible.² Sceptical empiricists are hardly in a position consistently to pontificate concerning inherent impossibilities: at best what is involved is

¹ Quoted by R. Heywood, *The sixth sense* (1959, London: Chatto & Windus), 11.

² G. R. Price, 'Science and the supernatural', *Science*, 122 (1955), 359–367.

probability, or rather plausibility, since the latter word more clearly suggests a general aura of social belief-worthiness for people at a particular epoch. All too often the line taken in controversies concerning some phenomena or other of psychical research are in fact debates with a foregone conclusion in which one or other of the contending parties starts with an entirely unshakeable set of assumptions that, since certain things cannot happen, their occurrence *must* be either delusory or else due to some fraudulent manipulation.

Needless to say, all too often there are quite normal explanations for alleged marvels. Nowhere are mistakes, delusions and hallucinations more likely than in the conditions of the seance room, nor is there any dearth of discoveries of fraud, chicanery and racket. Serious researchers who investigate these matters and who are open to being convinced either of fraud or else of the authenticity of phenomena have to become extremely sophisticated as regards the sources of error in this field and, should they themselves become convinced of the genuineness of phenomena, be prepared for the experience of being themselves accused of fraud and conspiracy. Nowhere is this more the case than in the context of the so-called 'physical phenomena' of psychical research.

Until relatively recently the most fashionable stance in respectable psychical research circles was to accept as authentic the so-called 'mental' phenomena, that is, the 'paranormal' acquisition of information such as telepathy, whilst rejecting the 'paranormal' influencing of material objects, such as table-turning. The position has now changed somewhat. Why the 'mental' phenomena should have seemed so much more securely established than the physical is quite a complicated story, and one far from fully clarified. Certainly most, if not all, of the major *scientific* figures in the field—men like Crookes, Lodge, Richet, Rayleigh, Wallace, Driesch and Barrett—have vouched for the authenticity of at least some 'physical phenomena'. On the whole, opposition has come more from philosophers and divines such as Dr. W. F. Prince, who saw in the 'mental' phenomena support for a more spiritual and religious interpretation of the universe than materialist science appeared to permit, whereas the 'physical' phenomena, with their general boisterous and irreverent atmosphere, might be thought to suggest a more mundane, quasi-biological interpretation. Indeed, this is precisely what scientists such as Richet, Driesch and Schrenck-Notzing believed, although in this they were not necessarily supported by others, such as Alfred Russel Wallace and Sir Oliver Lodge, who accepted the 'physical' phenomena as genuine.

Perhaps William James has described most forcefully the psychological impetus towards disbelief, in his case disbelief in manifestations he had himself observed, vouched for and accepted. He describes how he himself, in excellent light, repeatedly saw a ring moving by up to six inches, and he confessed surprise that this affected him emotionally so little that, four days after the event, his mind seemed strongly inclined not to 'count' the observation. He supposed that this was due to the fact that the experience was too exceptional, and he speculates that it is the frequency rather than the quality of the records that will establish the authenticity of physical phenomena.³

³ G. Murphy and R. O. Ballou (eds.), *William James on psychical research* (1961, London: Chatto & Windus), 90–92.

No doubt the lack of 'frequency' of the 'physical' phenomena is one of the reasons for the relative disbelief that they inspire. Another is the fact that no one has, as yet, devised any set of conditions in which they might be reproduced reliably; and above all, there is the clash between the apparently 'causeless' movement of objects with our current picture of the world, coupled with the absence of a suitable theory, model or formalism to describe, if not explain, the phenomena. So far, psychical research is very much a subject with a strong 'ideographic' and historical dimension, in which the description of particular named persons and designated events play a prominent part. It could well be the case that this will always apply, even if a new cosmology were devised which satisfactorily modelled some at least of the characteristics of the world that interest parapsychologists, just as it may always apply to general psychology.

At any rate, for the time being biographical aspects of particular ostensibly talented individuals are still of importance in psychical research, especially in the realm of the so-called 'physical' phenomena, sometimes called 'psychokinesis'. A person in connection with whom such events are said to occur is called a 'physical medium'. Physical mediumship is now exceedingly rare, at any rate, physical mediumship as described over and over again in the 19th- and earlier 20th-century literature. Opponents of the phenomena are apt to attribute this to the better and more sophisticated methods of detecting fraud, especially by means of the infra-red telescope now at our disposal. Protagonists of the authenticity of such phenomena are more likely to point to the quite outstanding quality both of the experimenters and their testimony when vouching for some of the cases, as well as the often lamentable quality of the attacks on such positive testimony. The recent phenomena alleged to have occurred in connection with Uri Geller and his imitators seem to bear some family resemblance to the classical physical phenomena,⁴ and recent attempts by Brookes-Smith following the views of Batchelor may still provide some more directly empirical vindication of the scientists who vouched for the genuineness of these 'physical' events.⁵

Inevitably, if an allegation of fraud is levelled against a particular medium or experimenter, there is a storm of controversy for and against the person so accused. The public at large, and that includes the educated and scientific public, is apt to accept virtually any accusation as a criminal conviction, so strong is the prior subjective certainty of the impossibility of these happenings: those involved are assumed guilty unless proved innocent, and attempted proof of innocence is deemed to be an insult to the good sense of intelligent people.

From the point of view of those who have investigated the phenomena and vouched for their authenticity, there arise complications over and above the fact that their good faith and good sense and powers of observation are drawn into question: it is by no means always the case that a mediumship is thought of, by investigators most closely concerned with it, as an all or none affair. Mediumships are apt to be described as 'mixed'; the medium sometimes helps things

⁴ J. Taylor, *Superminds* (1975, London: Macmillan); and C. Panati (ed.), *Scientific observations on the paranormal powers of Uri Geller* (1976, Boston: Houghton Mifflin).

⁵ C. Brookes-Smith, 'Data-tape recorded experimental PK phenomena', *JSPR*, 47 (1973), 69-89. In this and later footnotes the abbreviation 'JSPR' is used for *Journal of the Society for Psychical Research*.

along in a very normal way indeed, but at times there are believed to be happenings that make such ordinary explanations difficult. Such alleged occasional fraud may, of course, be carefully and deliberately planned. On the other hand, it may be spontaneous, almost ingenuous, and performed when the medium is in a dissociated state; for example, a foot may be used to push a table, a hand may be freed from control, and controlling a writhing entranced medium is not always an easy matter. There is obviously a world of psychological as well as moral difference between the activities of a consciously scheming fraudulent deceiver, complete with accomplices, and a subject in a semi-conscious or even hypnotic condition complying with the expectation of those around him or her; yet this obvious distinction is apt to be overlooked by those not well versed in the literature and practice of parapsychology.

Accusations of fraud may be made almost at once, but they may also be made much later: fresh evidence may come to light, old testimony may be resurrected, ancient quarrels may be revived. Indeed, quite a large part of the subject of psychical research is taken up with attacks, justified or otherwise, on claims made by colleagues in the past. There is about this Penelope-like quality of forever unravelling what has been woven something distasteful and futile, and it is little wonder that modern and younger parapsychologists wish to turn away from such activities and give their attention to fresh experimental and field work. This is certainly understandable, and indeed to be welcomed; on the other hand, as long as experiments and observations are not repeatable under specifiable conditions, apparently successful experimental demonstrations have no more status than other historical events, and the truism applies that those who ignore the past are doomed to repeat it. It is as well, both from the point of view of the future of the subject, as from the intrinsic interest of historical investigation, to examine at least some of the records of such scandals.

2. An outline of Rudi Schneider's career as a medium

Among the best documented and attested records of physical mediumship are those of the Austrian medium Rudi Schneider (see figure 1). It is impossible within the compass of this article to give a satisfactory account of a mediumship that was investigated and written about at some length in six countries (Austria, Germany, Switzerland, Czechoslovakia, France and England). Dozens of investigators and well over a thousand witnesses were involved in the observation of seances in all these countries. Apart from the medium himself, no one person, spectator or researcher, was a constant factor at all investigations. However, a brief and sketchy account of Rudi Schneider's career as a medium is essential to provide a setting for what is the main purpose of this paper: to delineate the anatomy of one particular scandal—the alleged exposure of Rudi Schneider, in *flagrante delictu*, by the psychical researcher Mr. Harry Price (1881–1948).

The bare outlines, then, of Rudi's life as a medium are as follows. He was born in Braunau (the birthplace of Adolf Hitler) in 1909, and he died in 1957, apparently from a stroke. He was the youngest of nine children, of whom only boys survived. Two of his brothers, Karl and Willy, were also physical mediums. It was Willy who first attracted the attention of the scientific world as a consequence of the physical phenomena claimed to occur in his



Figure 1.

Rudi Schneider. Frontispiece of A. von Schrenck-Notzing's posthumously published *Die Phänomene des Mediums Rudi Schneider* (footnote 7).

presence. His trance personality called 'herself' Olga. Willy was widely examined, also in this country;⁶ but as is usual in such cases, his supposed powers gradually weakened and faded. One evening, at the Schneider home, when nothing was happening despite all of 'Olga's' promises and efforts, 'she' said that 'she' wanted Rudi as a medium. At this point, Rudi, then eleven years old, entered the seance room in an apparently somnambulistic state and went into a strange physiological state also characteristic of Willy when he became 'Olga', which combined rapid breathing, extreme muscle tension and restlessness, together with a whisper purporting to be the voice of 'Olga'.

From very early on in the mediumship of Rudi, his father, known to all as 'Vater Schneider', kept a regular record of seances which is still in existence and in my possession. Vater Schneider was a typesetter, and clearly an orderly and intelligent man. His records, contained in two thick notebooks, are impressive and systematic accounts of what was experienced when, where and in whose presence; sitters were expected to read and usually sign the seance accounts afterwards. I shall refer to these records as 'Schneider Journals'. It is often possible to compare these Journals with the description

⁶ E. J. Dingwall, 'Physical phenomena recently observed with the medium Willy Schneider at Munich', *JASPR*, 16 (1922), 687-698. In this and later footnotes the abbreviation '*JASPR*' is used for *Journal of the American Society for Psychical Research*.

given independently by visitors or researchers, for example, the accounts published by Dr. A. von Schrenck-Notzing⁷ or by M. René Sudre.⁸ Almost invariably when a comparison is made, there is substantial agreement between the Schneider record and the eye-witness account, although in some cases there are minor discrepancies. As will be seen, from time to time the Journals could be successfully invoked to remind investigators who subsequently denied having witnessed anything of paranormal interest that their later views were at variance with those recorded at the time.

Rudi was investigated over a number of years and under increasingly sophisticated control conditions by Dr. von Schrenck-Notzing in Munich at the latter's purpose-built private laboratory. He was subjected to three investigations in London by Harry Price at the National Laboratory for Psychical Research.^{9, 10} The Rev. Dr. W. F. Prince conducted a series of experiments in Stuttgart in the home of Studienrat Dr. Lambert.¹¹ Rudi was investigated at length by Dr. Eugène Osty and his son Marcel at the Institut Métapsychique in Paris;¹² Lords Charles Hope and Rayleigh conducted a series of systematic experiments under the auspices of the Society for Psychical Research in London.¹³ Mr. Theodore Besterman and Mr. Oliver Gatty conducted another (negative) series in London.¹⁴ Experiments were performed in Prague by Professor O. Fischer and Dr. Karel Kuchynka,¹⁵ and finally a major series of experiments was performed by Professor G. Schwaiger in Vienna.¹⁶ This list is not exhaustive, but it includes the most important. Mention should perhaps also be made of a number of visits to the Schneiders' home and other private houses by Professors Meyer and Przibram,¹⁷ Mr. and Mrs. W. V. Vinton,¹⁸ and Dr. Malcolm Bird of the American Society for Psychical Research.¹⁹

Of those conducting systematic investigations, only Prince believed that fraud had—or to be more precise, could have and therefore must have—been

⁷ A. von Schrenck-Notzing, *Die Phänomene des Mediums Rudi Schneider* (1933, Berlin and Leipzig: de Gruyter).

⁸ R. Sudre, 'A seance with Rudi Schneider', *JASPR*, 21 (1927), 295–403.

⁹ H. Price, *Rudi Schneider—a scientific examination of his mediumship* (1930, London: Methuen).

¹⁰ H. Price, 'An account of some further sittings with Rudi Schneider', *Bulletin IV of the National Laboratory of Psychical Research*, (6 March 1933), 1–199.

¹¹ W. F. Prince, 'Experiments with physical mediums in Europe', *Bulletin VII of the Boston Society for Psychical Research*, (1928), 1–113.

¹² E. and M. Osty, 'Les pouvoirs inconnus de l'esprit sur la matière', *Revue métapsychique* (1931), 1–60, 393–427; (1932), 81–122.

¹³ C. Hope and others, 'Report of a series of sittings with Rudi Schneider', *Proc. SPR*, 41 (1933), 255–330. In this and later footnotes the abbreviation 'Proc. SPR' is used for *Proceedings of the Society for Psychical Research*.

¹⁴ T. Besterman and O. Gatty, 'Report of an investigation into the mediumship of Rudi Schneider', *Proc. SPR*, 42 (1934), 251–286.

¹⁵ K. Kuchynka, 'Expériences à Prague avec Rudi Schneider en 1930, 1935 et 1936', *Metapsychica*, 11 (1956), 1–12.

¹⁶ G. A. Schwaiger, unpublished report on sittings with Rudi Schneider, 4 November 1935–30 June 1936, SPR files, S8–10. 'SPR files', followed by a reference number, refers to the archives of the Society for Psychical Research, 1, Adam & Eve Mews, London W8 6UQ.

¹⁷ A. von Schrenck-Notzing (footnote 7), 2–3.

¹⁸ W. J. Vinton, 'The famous Schneider mediumship—a critical study of alleged paranormal events', *Psyche*, 28 (1927), 3–45.

¹⁹ M. Bird, 'The current status of the Schneider mediumships', *JASPR*, 23 (1929), 351–367, 407–423.

committed. Bestermann and Gatty's experiments were negative, in that no phenomena were observed, or at least satisfactorily witnessed. Meyer and Przibram and the Vintons made definite accusations, as did Bird, based on visits to the home of the Schneiders. Harry Price was alone in claiming to have unmasked Rudi by demonstrating, by means of a photograph, that he had, during a crucial episode during a seance, freed a hand, thus enabling him to manipulate an object allegedly moved by paranormal agency. Virtually all other investigations resulted in generally quite unambiguous testimony that, in the view of the major experimenters, paranormal physical phenomena had been observed, no fraudulent activity whatsoever had been observed or even attempted, and the medium had accepted without demur or hesitation all control conditions imposed by investigators. This is most impressive in the case of those experiments where the medium was separated from friends and family, in an alien environment such as laboratories and drawing rooms in Paris and London, and, in the hands of scientists of repute in other fields as well as being well-versed in the subject of psychical research, such as the Hope-Rayleigh series in London.

Phenomena systematically reported over a substantial number of Rudi's seances can be divided into four categories: (1) the movement of objects at some distance from the medium; (2) the appearance of a visible substance or matter, frequently in the form of a body or part of a body, a so-called 'pseudopod', or a thin mist, named 'materialisation'; (3) the experience of persons present at a session that they were being touched by an invisible hand usually on the head; and (4) the levitation of the entire body of the medium without visible means of support. In addition, there were frequent reports from participants of experiences of extreme localised cold, such as might be felt at the mouth of a flask of liquid air. Interestingly enough, reports of being touched by an unseen hand were the least frequently reported phenomena.

Initially, levitations of the medium's whole body were reported at virtually every seance, and these were described and reasonably well attested in Schrenck-Notzing's laboratory in Munich. However, this phenomenon was not observed in the French and English experiments, and was the first type of occurrence to disappear altogether. 'Materialisations', at first as frequently reported as movements of objects, became gradually less intensive and less frequent. The reported movement of objects, however, persisted to the end of the mediumship (though in a much attenuated state), as did the 'cold air' manifestation. It is certainly the case that the alleged phenomena were far more spectacular, vigorous and abundant in the early days of the mediumship: bells and a cardboard figure named 'August' would sail through the air, a broken-down musical box would play, a typewriter would type by itself, invisible hands would trim a bonnet, a boot would be torn off a foot with some violence, windows would be shattered, and a 'hand' visible or otherwise might be described as playing tug-of-war with an object such as a wastepaper basket, or a handkerchief, which might be torn in half by the struggle.

'Olga's' repertoire was doubtless somewhat limited by the imagination of experimenters. Eventually, as is usual, the phenomena weakened and lessened, and negative sessions were more and more frequent, participants at sessions would wait for hours for a small, fine handkerchief to be lifted off a table for a short period.

It is at this unpromising point in the mediumship that there occurred what might have been and may still, after all, prove to be, a turning-point in the scientific investigation of physical mediumship. Dr. Eugène Osty employed at the *Institut Métapsychique* in Paris, as part of an anti-fraud control system, an infra-red network surrounding the handkerchief to be levitated. Such a device, entirely novel in the early 1930s, had just been installed as a burglar alarm to guard some priceless jewels on exhibition at Burlington House. If any solid object such as an arm or a reaching rod were to approach the handkerchief, it had to cross the infra-red beams, triggering off a system of alarm bells and/or photographic apparatus. To cut a long story short, the alarm system was indeed set in motion, but by no visible agency. Photographs revealed that the beam had been interrupted—but by nothing on record. At first it was supposed that the tripping-off of the alarm system was due to instrumental failure, but eventually it was found that such interruptions, or rather partial interruptions or occultations of the radiation, coincided with 'Olga's' declared intention to move the handkerchief.

'Olga', who had at first complained bitterly that the flash (set off by the alarm system) had disturbed and upset 'her' and prevented 'her' from picking up the handkerchief, was pacified and persuaded by Dr. Osty that 'her' ability to 'go into the ray' was a considerable achievement. After this, experimenters concentrated more and more on getting 'Olga' to affect the infra-red beam, and this was registered by means of galvanometric apparatus and automatically recorded.

The interference with, or absorption or occultation of, the beam was never complete as it would have been if a solid object had been interposed: usually only something of the order of 15% of the beam was absorbed. Observers in Paris and London noted independently that at times the beam oscillated at a period of twice that of the medium's (abnormally fast) respiratory cycle. Superimposed on this oscillation would be marked deflections of the galvanometer, coinciding with 'Olga's' announcement that 'she would go into the beam' or that 'she' would 'try and lift up the handkerchief', located on the other side. Infra-red beam observations of this type were made independently in Paris, London, and finally in Vienna. No satisfactory 'normal' explanation of the results has been suggested to date.

3. Early accusations of fraud

It is the aim of this paper to subject to a detailed examination the alleged exposure of the medium Rudi Schneider by Harry Price on 28 April 1932, and the events related to it. This will be preceded by a rather briefer examination of such claims made by certain investigators prior to the Price accusation.

The first of the serious attacks on Rudi's mediumship came from two Professors at the University of Vienna, Meyer and Przibram. Meyer was Director at the Vienna Radium Institute, and Przibram his chief assistant. Both attended a few seances, mainly in the home of a Major Barauski. In February 1924 various newspapers published accounts that Rudi had been unmasked by Meyer and Przibram.

What had happened was that one Sunday, after a sitting with Rudi, Meyer and Przibram gave a party to which some forty guests were invited. A marvellous new medium was introduced who produced a number of feats, including

total levitation of the medium's body to the entire satisfaction of the spectators, who were apparently persuaded that a new star had risen on the firmament of mediumship. When Meyer subsequently introduced the new 'medium' as none other than his colleague Przibram, and informed the audience that the entire gamut of phenomena they had so enthusiastically applauded had been performed by means of trickery, the learned world breathed a collective sigh of relief: there was, evidently, nothing in the Schneider mediumship that could not be duplicated by an academic gentleman endowed with sufficient ingenuity and gymnastic agility.

Przibram made two important claims: namely, that he was able to free one hand for manipulating objects in such a manner as to give the audience the impression that they were floating about; and that he was able to support himself on one leg whilst raising the other horizontally in the air (after slipping one foot out of the tie that bound the luminous piece of cardboard to the feet), thus imitating the feat of 'floating' in the air in the dark. Both tricks, of course, required complete complicity of the controller, that is, the person in charge of seeing that the hands were firmly held whilst objects were moving about, and of checking that both feet and indeed the rest of his anatomy were clear of the ground during 'levitation'. During these seances both legs of the medium were marked by luminous pins and his feet were tied, the knot being secured with a luminous pin. In order to give the illusion of floating, not only would Rudi need a hand in order to remove the pin securing the knot and to untie one of his feet, so as to use it for standing on; he would also have to transfer one row of pins from the standing to the floating leg and to repin them after the performance. He would then have to re-transfix the knot with the luminous pin.

Protests, not unnaturally, came from persons who had themselves controlled Rudi. It was all very well for Meyer and Przibram to claim that they had found a 'natural explanation': they had merely *simulated* the phenomena under conditions that were not in the least comparable. The most comprehensive counter-claim came from Schrenck-Notzing, who obtained signed statements from Meyer and Przibram which he proceeded to examine in detail.²⁰ In this the professors state that they attended one seance on 8 December 1923, which had enabled them to exclude certain explanations such as mass hypnosis and the introduction of special apparatus for lifting up the medium. They claimed that at the next seance, dated 26 January 1924, the medium freed a hand from control—the right hand, that is the one nearest to the objects to be moved—and slipped it back into the controller's hand before the end of the sitting. As regards 'levitation', they asserted that—mass hallucination and an accomplice having been excluded—only one interpretation remained, namely that the medium freed one leg from its tie, stepped on the chair with one leg and balanced himself stretching one leg forward whilst bending backward the upper part of his body. This, they said, would also account for the 'fact' (not actually correct) that the medium always landed precisely on the chair after his supposedly paranormal aerial excursions. Przibram wrote that Meyer 'had noticed by feel that the medium had got the right hand free—after re-insertion the hand was noticeably cooler'.

²⁰ A. von Schrenck-Notzing (footnote 1), 2–3.

Schrenck-Notzing replied to the attacks of Meyer and Przibram at considerable length and in great detail, so much so that the two issued a press statement to the effect that they had not exactly 'unmasked' Rudi, but merely found ways of producing his phenomena in an entirely natural manner. Thus they satisfied themselves that this was the way in which the phenomena must have been brought about.

There was, from the point of view of Meyer and Przibram, one very awkward fact: the seance of 26 January 1924 is recorded in Vater Schneider's Journal, and there, for all to see was Meyer's signature after the account of the events of the seance: 'Die Kontrolle war einwandfrei' ('The control was perfect', literally 'free from objections'), signed 'Professor Dr. Stefan Meyer' (see figure 2). Why did Meyer go out of his way to testify to the excellence of the control conditions if he (and especially Przibram) considered them so poor as to be useless, and if he had actually observed fraudulent manipulations?

The full story, like all accounts of events of this sort, is extremely involved, but these are the bare outlines. Meyer and Przibram had examined the phenomena; Meyer had himself controlled the medium and vouched for the control. They had subsequently simulated the phenomena, or some of them, and accused the medium of fraud. They had, on being confronted with additional testimony and Meyer's own signature, withdrawn the accusation but retained the assertion that they had 'explained' the phenomena. They left unanswered the question how, without the controller's full complicity (that is, on one occasion at least that of Meyer), the phenomena could be explained.

Schrenck-Notzing formed his own group of medical and scientific colleagues largely from the University of Munich, and subjected Rudi to a long series of tests during which the majority of these became entirely satisfied that the phenomena claimed were genuine.²¹ Rudi meanwhile continued to give sittings at home, at numerous private houses nearby, in Vienna, Munich, Prague and Zürich. The entry in the Schneider Journal for the seance of 20 June 1925 in Zürich may perhaps be of some interest because of the signature of C. G. Jung (see figure 3).

The next heavily publicised attack on Rudi came from a Mr. Warren J. Vinton.²² Again, a full account of this set of events would be exceedingly long and, in view of the poor quality of this report dealing extensively with his and his wife's 'feelings' about events and persons, and generalities about the seances rather than precise accounts, it does not seem to me worth while to go into this at length. Had Vinton testified in favour of the phenomena it seems quite plain that no one would have taken seriously his tissue of sentiments, suppositions and generalities. However, one particular claim of his is of interest, in that it constitutes what has subsequently been accepted as eye-witness testimony of fraud, for instance, by T. R. Tietze.²³ Vinton said that during one sitting, that of 9 August 1926, he saw a large figure crouched in the cabinet. He apparently related this to Vater Schneider the next morning (why he did not do so at once is not explained), and Vater Schneider

²¹ A. von Schrenck-Notzing (footnote 7). Schrenck-Notzing died in 1929, and his papers concerning Rudi Schneider were edited posthumously by Dr. Gerda Walther.

²² W. J. Vinton (footnote 18).

²³ T. R. Tietze, 'Ursa Major: an impressionistic appreciation of Walter Franklin Prince', *JASPR*, 70 (1976), 1-35.

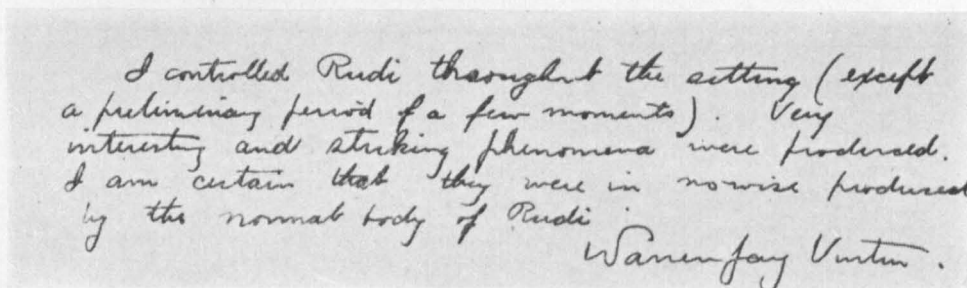
Sitzung in Wien ^{Meyer}
 in der Wohnung des Herrn ^{Paraniskij}
 am 26. Januar 1924.

Otto Neumer
 Hermann Bäck ^{Prof. Dr. Stefan Meyer}
 Prof. Dr. Stefan Meyer
 Viktor Muntendorf ^{Prof. Dr. Stefan Meyer}
 Titina de Faurz ^{Herrn Baumgartner}
 Triana Gfn. Beck-Rykowski
 Mit Karl Göttschmann ^{Sitzung ohne Kabinett}
 Sitzung ohne Kabinett ^{Prof. Dr. Stefan Meyer}
 Die Kognition konnte genau
 kontrolliert werden die absolute
 Realität des kognitiven Vor-
 ganges wird hierdurch bestätigt
 26.1.1924 ^{Prof. Dr. Stefan Meyer}
 Die Kontrolle war einwandfrei
 Prof. Dr. Stefan Meyer
 Paraniskij ^{Viktor Muntendorf}
 Titina de Faurz
 Triana Göttschmann

Figure 2.

Page from the Schneider Journals, showing signature 'Prof. Dr. Stefan Meyer' before the sitting 26 January 1924 (top left, 3rd line of signatures), and his comment and signature after the sitting (5th and 6th line from below): 'Die Kontrolle war einwandfrei Prof. Dr. Stefan Meyer'.

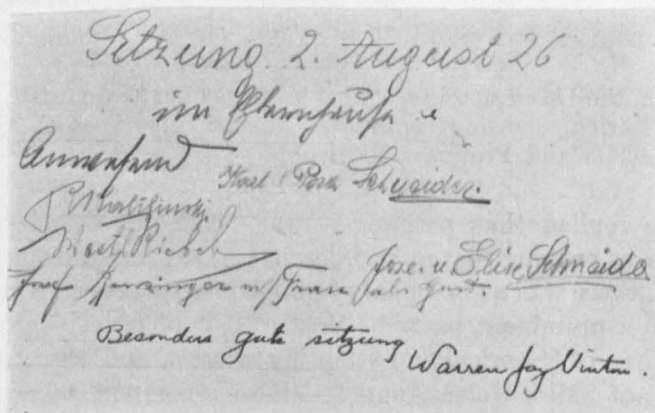
handwriting, in English (see figure 4): 'I controlled Rudi throughout the sitting except a preliminary period of a few moments. Very interesting and striking phenomena were produced. I am certain that they were in no wise produced by the normal body of Rudi'. The next sitting is 2 August 1926, Dr. and Mrs. Dingwall seem to have left, and there is a comment in Mr. Vinton's handwriting (in German): 'particularly good sitting, Warren Jay Vinton' (see figure 5).



I controlled Rudi throughout the sitting (except a preliminary period of a few moments). Very interesting and striking phenomena were produced. I am certain that they were in no wise produced by the normal body of Rudi.
Warren Jay Vinton.

Figure 4.

Detail from 196th sitting, 30 July 1926; comment by Warren Jay Vinton.



Sitzung 2. August 26
im Phosphor
Anwesenheit Karl Paul Schneider.
J. W. Vinton
W. J. Vinton
Prof. Springer u. Frau Paul
Besonders gute Sitzung Warren Jay Vinton.

Figure 5.

Detail from 197th sitting, 2 August 1926, comment by Vinton:
'Besonders gute Sitzung'.

The next entry is peculiar. It is dated 9 August 1926, and in addition to Vinton and Schneider circle members there were three Americans, a Mr. and Mrs. Cannon from New York and a Mr. Gubisch from Meriden, Connecticut. There is a testimony concerning the control: 'Controlled the first and second periods of the seance—perfect conditions, Mr. W. J. Vinton', in the latter's handwriting, followed by testimony concerning the third period by Mrs. Wm. M. Cannon. And sandwiched in between the general signatures and Mrs. Vinton's testimony, there is in Vinton's handwriting: 'see note by me written after the sitting of August 19th, 1926, Warren Jay Vinton'. Under Mrs. Cannon's testimony are the words 'also seen' in Vinton's handwriting (see figure 6).

Sitting on 9 August 1926 and Rudi
 Cannon
 Mrs. M. M. Cannon
 Wm. J. Vinton
 Mrs. W. J. Vinton
 Frank Lubin
 Marjorie Green
 Rosa Schneider
 Earl Schneider
 See note by me written after sitting of Aug 19, 1926
 Controlled the first and second periods
 of the seance — perfect conditions
 Mrs. W. J. Vinton
 The third period I had absolute control of
 with Karl and Rudi. Her hands and knees
 and feet were controlled at all times
 Mrs. M. M. Cannon
 The seance was very successful, good results were
 obtained. Karl and Rudi, later on, were very
 much interested in the seance.
 also see

Figure 6.

Report of 198th sitting, 9 August 1926, showing signatures of those present, including the Cannons and Vintons. The note by Vinton in the middle, 'See note written by me after sitting of August 19, 1926', clearly was not inserted at the time but 10 days later. At bottom of page in Vinton's writing 'also see [n?]', crossed out.

Why did not Mr. Vinton write his observations there and then on 9 August? Why did he wait until right at the end of the sittings? Obviously he inserted these comments afterwards, on 19 August, when he had the book in his hands at his leisure for the first time. The note that is to be found after the sitting of 19 August reads: 'I was told to go in cabinet and pick up tambourine. While doing so I felt a large crouched form in corner of cabinet behind medium. I reported this to Vater Schneider next morning who explained that it was collected teleplasm' (see figure 7). Now this is surely very odd. Why wait till next morning? Why ask Vater Schneider? Why not enter this occurrence right away? No one other than Mr. Vinton seems to have recollected the incident. Why wait a full ten days before reporting on this presumably significant observation? And, most important, why did he not try to sit in the cabinet again?

One can only speculate why Mr. Vinton waited for so long before writing down his comments. Would the other American participants, the Cannons, perhaps have challenged his entries (which were in English) and made him test the allegations for which he was plainly preparing the ground, by sitting, or making him sit again, in the cabinet? Or was the observation an afterthought, some secondary elaboration of what 'must' have happened?

Note to sitting of Aug 9 1926.
 I was told to go in cabinet and pick up tantaraine
 while doing so I felt a large crouched
 form in corner of cabinet behind medium. I
 reported this to Vater Schneider next morning
 who explained it was collected cataplasma.

Figure 7.

Detail from 201st sitting, 19 August 1925, postscript by Vinton, allegedly referring back to sitting 10 days previously.

I want to express my thanks to the whole
 Schneider family for their invairable
 kindness, courtesy, and friendliness
 during a stay of nearly three weeks
 in Braunau.

August 19. 1926.

Warren Jay Vinton.

WARREN JAY VINTON
 16A JOHN STREET
 ADELPHI
 LONDON
 ENGLAND

Figure 8.

Final remark by Vinton, 19 August 1926, in Schneider Journals, at end of 201st sitting.

Mr. Vinton ended his entry in the Journals: 'I want to express my thanks to the whole Schneider family for their invairable [*sic*] kindness, courtesy and friendliness during a stay of nearly three weeks in Braunau, Warren Jay Vinton, 16A, John Street, Adelphi, London, England, August 1926' (see figure 8). It is perhaps not surprising that, when his article appeared, those whose hospitality he had enjoyed in Austria and Germany and whom he had assured of his conviction of the paranormality of the Schneider boys' mediumship, were somewhat displeased.

The Vinton accusations, if carefully examined, amount to very little. His 'feelings' that the whole thing was mere horseplay, his 'feelings' that there was a crafty sly look in Vater Schneider's eye, his 'feeling' of dislike for Major Kalifius or of contempt for Mutter Schneider; none of these amount to anything, particularly since he also allegedly had 'feelings' of awe, fear, bewildered astonishment and so forth. His actual observations, or alleged observations, of a figure crouching in the cabinet is highly dubious and, even if factual, ambiguous. It was unwitnessed, reported far too late, and so far as I have been able to ascertain, no one had ever heard of Vinton before, or for that matter since, as a researcher.

What remains are his assertions about how an accomplice could have got into the cabinet and the categorical statement that, whatever produced the phenomena, it was not the natural body of Rudi. Professors Meyer and Przibram, it will be remembered, had satisfied themselves that they had excluded the possibility of accomplices and that the phenomena *must* have been produced by what Vinton called 'the natural body of Rudi'. Now if Meyer, Przibram and Vinton were all of them right in believing the phenomena to have been wholly fraudulent, then the phenomena were produced in entirely different ways (excepting levitation of the medium's body which was not observed by Vinton): by normal manipulation on Rudi's part for the Viennese professors, and by accomplices for the young Americans.

Whereas Schrenck-Notzing had had a relatively easy task in demonstrating that Rudi could not have manipulated the objects flying about the room under the conditions imposed at his Munich laboratory, he had a far harder task trying to prove that the seances laid on at Braunau were not a collective Schneider family effort. In fact, to disarm this attack Schrenck had to conduct seances in his own laboratory whilst excluding anyone who could reasonably be thought of as an accomplice of the medium. And since he was successful in this, he himself inevitably fell under suspicion of being in the plot.

The next two attacks on the Schneider mediumship came from two rather more expert sources, but they are in point of fact nearly equally vulnerable when examined in some detail. The Rev. Dr. Walter Franklin Prince, an American psychical researcher of considerable importance and prominence, wrote a long and condemnatory report of his sittings with Rudi Schneider, most of which were conducted in the home of Studienrat Dr. R. Lambert.²⁵ So far as I can see, there is not one shred of evidence that suggests that Dr. Prince actually detected any fraudulent procedure during any one of his 13 seances: there is, however, a fair amount of presupposition and inference. In a recent article Mr. T. R. Tietze takes me to task for dismissing the Prince investigations of Rudi Schneider, but his strictures on my views seem to me so inapposite that I can see no reason for changing my attitude towards Prince and his sittings. Indeed, quite the reverse applies: Mr. Tietze unwittingly supplies good additional reasons why Dr. Prince's opinions as regards the Schneider phenomena should be approached with reserve.²⁶

Prince, despite the fact that he ran and organised the seances, found no direct evidence of fraud. His main suspicion was that somehow or another Rudi used his mouth to produce the phenomena, either by blowing at the cabinet curtains, or by extracting from some hiding place on his person a secreted reaching rod and conveying this to his mouth, possibly with Vater Schneider's help. Yet, when on one occasion a sitter cried out: 'Dr. Prince, Dr. Prince, his [Rudi's] face is in your hands', Prince complained bitterly that the sitter was distracting his attention from the curtain movements!²⁷

The nearest that Dr. Prince came to an actual claim that something suspicious was actually observed was a rather unsatisfactory story that a person 'X', whose identity he refuses to disclose, wrote to Prince that he saw a small luminous narrow oval shape surrounding a black disc hovering about by

²⁵ W. F. Prince (footnote 11).

²⁶ A. Gregory, 'Rejoinder to T. R. Tietze', *JASPR* (in the press : 1977).

²⁷ W. F. Prince (footnote 11), 48.

Rudi's head, and then withdraw through the keyhole. Prince thought this could have happened 'if a small oval were cut from thin cloth with a rubber coating, luminous paint were applied and, a hole being cut out of the centre, it were firmly fastened around the end of a slender reaching rod, we should have the exact appearance drawn and described. . . .'²⁸ Vater Schneider had been told not to attend this seance, and his whereabouts were therefore, it seems, unchecked and unknown: the maid had, apparently, gone to bed.

The innuendo that Vater Schneider was, in a strange house, attempting to pass a reaching rod to his son through the keyhole, was never in any way tested or even duplicated for feasibility. Such anonymous testimony, even if it did amount to anything, would hardly be acceptable as incriminating evidence in any other context. Why the secrecy about 'X'? Perhaps 'X' was not all that certain that he had seen anything at all, or else that he had seen exactly what Prince described. It is easy to understand why a professional person of some repute (if 'X' was such) should have refused to allow himself to be quoted when testifying in favour of the authenticity of phenomena; but why should he refuse to disclose his name if he had evidence suggesting trickery? In any case, even if Prince had come to the conclusion that Rudi's phenomena were genuine, psychical researchers might well have declined to give too much weight to his views since he was extremely deaf, and far from fit during much of the investigation. He was moreover so prejudiced against physical phenomena that even an admirer as devoted as Mr. Tietze admits that Prince would hardly have seen any genuine phenomena if there had been any.²⁹

The same does not apply to Dr. Malcolm Bird, another American investigator, who also threw doubt on the genuineness of Rudi's mediumship.³⁰ Bird's paper is written in a manner that inspires somewhat more confidence than the sensationalistic brashness of Mr. Vinton or the tetchy irritability of Dr. Prince. In my view there cannot be any doubt that, had he been convinced of the authenticity of the phenomena, he would not have lacked the courage to say so. He did not disbelieve in principle in the 'physical phenomena': in fact, he was a supporter of the American medium 'Margery', who was at that time dividing the American Society for Psychical Research into two embittered camps. However, Bird only had a single sitting in Braunau, on 11 October 1927, and he refused to stay on any longer to satisfy himself. His suggestion was that while he deliberately allowed his attention to be deflected, someone slipped into the room at 11 p.m. who then worked matters as from the 'cabinet'. This hypothetical accomplice could have been let out again by Major Kalifius, one of the principal sitters, under pretext of letting in Franz Schneider, one of the brothers. On the other hand, Bird himself held the key and opened and locked the door. Why did he part with it? Dr. Bird wrote that all invitations to him to inspect the cabinet ceased during the time when he considered an accomplice could have slipped in. But there is no suggestion anywhere that anyone stopped him: merely that people ceased asking him. And there could have been no possible damage to the medium if Bird had looked into the cabinet during the interval. Why did he—a most

²⁸ W. F. Prince (footnote 11), 34.

²⁹ T. R. Tietze (footnote 20), 17.

³⁰ M. Bird (footnote 19).

experienced and knowledgeable investigator—fail to take this elementary precaution?

If the Schneider Journal is consulted, it is found that there is one discrepancy between it and Bird's report: according to Schneider, feeble phenomena began at 11.20, but Bird's account leads one to suppose that phenomena began at 11.00. Had the Schneiders wished to skate over the fact that an accomplice was smuggled in at 11.00, one would have expected them to err on the side of giving too early rather than too late a time, and perhaps to insist that 'weak' phenomena occurred during the first part of the sitting. Instead, both Schneider Journal and Bird's report insist that the first part was totally blank. Bird signed the book before, but not after, the account, so that, had the Schneiders wished to blur the issue, this would have been very easy.

Bird, then, based his confident assertion of large-scale conspiracy on one single seance during which he admits he took no precautions worthy of the name, or checked up on a single one of his suspicions. He refused to remain in Braunau to satisfy himself despite the fact that he was virtually begged to stay. He may well, as he says, have been busy. In that case, it would seem that a tentative note, rather than a categorical and lengthy accusation, would have been appropriate.

It may perhaps be seen from the above examples that, whatever the truth about the genuineness of the 'physical phenomena', the standards of evidence in hostile reports are, at times, not of the highest.

4. Harry Price and the London background of his first invitation to Rudi Schneider

On 12 February 1929 Schrenck-Notzing died suddenly and unexpectedly of an attack of appendicitis, and there was no-one in Germany to carry on his work. Schrenck possessed not only a passionate interest in psychical research, limitless patience and industry in carrying out investigations and an adequate toughness of fibre to conduct outspoken campaigns against frequent vitriolic attacks; he also had the financial means to devote himself to his chosen subject. His death marks a fundamental turning-point in the career of Rudi Schneider, who was almost immediately plunged into the whirlpool of international psychical research.

Harry Price wrote that he 'found himself' in Munich in March 1929.³¹ In a later book he admitted that, as soon as he heard of Schrenck's death, he made a dash to Munich in order to secure Rudi's services for his venture, the National Laboratory of Psychical Research in London.³² Price signed up Rudi for six seances in London, from 11 to 22 April 1929. Rudi duly arrived in England, gave the seances, and results were sufficiently impressive for Price to invite him to London for a more prolonged series of sittings for the autumn of 1929 and early 1930.

Price had had sittings with Rudi earlier in Braunau. After Schrenck's death he 'took over' Rudi and turned him into a newspaper celebrity, at any rate in England. In his book *Rudi Schneider* (1930) Price wrote that he had

³¹ H. Price (footnote 9), 3.

³² H. Price, *Fifty Years of Psychical Research* (1939, London: Longmans), 102.

two aims in inviting Rudi to London: to examine him under the most stringent conditions of control and thus to 'settle, once and for all' the present status of the mediumship; and to 'inform the press'. Concerning this latter goal, Price said:

That we have succeeded beyond our most sanguine hopes is proved by the fact that there is not a man, woman or child in Great Britain who has not read about Rudi, his phenomena and the conditions controlling the experiments. The British public has learnt more about scientific psychical research in the last few months than it did in the previous fifty years. That we have rescued the science from the mire of charlatanry in which it has been wallowing for generations is proved by the fact that the public is *at last* beginning to realise the difference between modern organised scientific psychical research and 'spiritualism'.³³

It is quite impossible to understand Rudi's subsequent career and to attempt any valid estimate of his phenomena without forming some picture of the international background of psychical research and of the personality of Harry Price in particular. Until Schrenck's death Rudi had been, from the point of view of English, French and American psychical researchers, a rather questionable and not particularly interesting Austrian medium. Vinton's, Prince's and Bird's attacks were designed, if not to damn Rudi, at any rate to place by his name a large question mark. Schrenck's papers on Rudi were not published until years after his death, in 1933. When Harry Price claimed that Rudi was indebted to him for what fame he won outside Austria and Germany, he was in a sense right. If Price had not, immediately upon hearing the news of Schrenck's decease, rushed to Munich and invited Rudi to London, the medium would probably never have been heard of again outside his intimate circle.

Price's establishment, the National Laboratory of Psychical Research, published a journal called *The British journal of psychical research*. Its January/February 1929 number contains an article by him entitled 'A plea for a better understanding'.³⁴ In it he analysed the mutual inter-relationships of the main psychical research institutions and of the principal personalities in the field as they existed early in 1929. The paper presents a devastating and shrewdly observed sociogram of the mutual detestation in which the different organisations and personalities at the time held one another. It also bears involuntary but eloquent testimony to his own touchiness, quarrelsomeness and need to dominate the scene. His remedy for the ills of psychical research was wholesale amalgamation of societies and organisations. It is not easy to see how he imagined this would have improved the hideously bad inter-personal relations that he described: would these not have been, if anything, exacerbated if all these old rivals and enemies, instead of running their own show, had been fighting for control of one and the same set-up? Would not all these bitter antagonists have fought one another to the last breath for the inevitably smaller numbers of controlling appointments in the new larger units?

Harry Price's recommendation that all psychical research organisations should merge was not a mere pious sentiment. He himself made active and

³³ H. Price (footnote 9), vii.

³⁴ H. Price, 'A plea for a better understanding—a seasonable effort to repair some shattered friendships', *British journal for psychical research*, 2 (1929), 129–140.

determined efforts to merge his National Laboratory for Psychical Research with numerous other organisations. A very large body of correspondence on this subject survives, not only in Harry Price's archives now at the University of London, but also at the *Institut Métapsychique* in Paris and at the London Society for Psychical Research. His negotiations for these mergers were often secret, and his motives for pursuing them not wholly transparent. None of them came to anything despite the fact that on the face of it his offers were generous. It seems that at least some of the recipients of his offers to join forces with them regarded his terms as being in the nature of a Trojan horse—a take-over bid. Price attempted to amalgamate with Osty's organisation in Paris, with the London Society for Psychical Research and (after personal negotiations with Adolf Hitler) with the University of Bonn.³⁵ Some of these episodes will be described more fully later.

The last words of Price's 'A plea for better understanding' are: 'I have purposely excluded the name of Lord Charles Hope from among the active investigators because I am not aware that he is at loggerheads with anyone—except perhaps a few mediums of doubtful reputation'.³⁶ Lord Charles Hope and Dr. Eugène Osty were virtually the only researchers of note exempted from Harry Price's universal anathema upon all investigators. Within three years from the date of this paper, Price was engaged in some of the bitterest quarrels of his tempestuous career, in the course of which he did what lay in his power to destroy the reputations of both Hope and Osty. If one were writing a tragedy in the Greek manner called 'Rudi Schneider', most of the material for an omniscient, prophetic (if somewhat ironical) chorus would be contained in Harry Price's 'Seasonable effort to repair some shattered friendships' at Christmastide, 1928.

It is impossible within the compass of this article to give full accounts of the investigations that followed. However, it is important to make it clear that if, in the opinion of the numerous experienced and often eminent researchers who conducted experiments with Rudi Schneider after the death of Schrenck-Notzing, there had not been obtained impressive and interesting evidence in favour of the authenticity of paranormal physical phenomena, the claimed exposures of the medium would hardly have excited much interest. Table 1 provides an outline of the investigations of Rudi Schneider following the death of Schrenck-Notzing in 1929.

It is precisely because of the apparently epoch-making breakthrough accomplished by Osty and repeated by Hope and also incidentally under Price's own aegis, that Price's denunciation had the importance and impact it did have. Moreover, Price was the publicist of psychical research *par excellence* in England in the 1920s and 1930s. To thousands of people to this day psychical research *means* the books of Harry Price. His accounts of

³⁵ Hitler referred the matter to the *Reichs- und Preussisches Ministerium für Wissenschaft, Erziehung und Volksbildung*, the *Innenministerium* and the *Auswärtiges Amt*. It was then passed on to the University of Bonn, on behalf of which Professor Dr. Hans Bender wrote to Price on 20 March 1937 that his offer was in principle acceptable and that the German government would confer upon Price the Red Cross Medal, 1st Class. Nothing ever came of this deal. The correspondence is preserved in the Harry Price Library (see footnote 38 below). Some of it is reprinted in H. Price, *Search for truth* (1942, London: Collins), 113.

³⁶ H. Price (footnote 34), 140.

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- 1929 Rudi in London, April. First Series of experiments at National Laboratory of Psychical Research, Harry Price. Electrical control of medium and all sitters. 5 sittings. Phenomena 'brilliant'.
- 1929-30 London, November to January. Second Series of experiments at National Laboratory of Psychical Research. Electrical control. 21 sittings. Phenomena 'brilliant'.
- 1930-31 Rudi in Paris, *Institut Métapsychique*, Eugène Osty. 15 months of experimentation. 90 sittings. Tactual control, infra-red apparatus. Occultation of infra-red rays. Results 'positive'.
- 1931 Party of four from National Laboratory (H. Price, Miss E. Beenham, Mrs. K. M. Goldney and Miss M. Walker) visit Braunau. 3 sittings. 'Very good phenomena'.
- 1932 Third Series of experiments at National Laboratory of Psychical Research; series of 27 sittings, February to May. Tactual control, infra-red apparatus. Phenomena 'often good but not so brilliant as in 1929/30'. Contentious seance of 'freed hand', 28 April 1932.
- 1932 Hope-Rayleigh sittings, Society for Psychical Research, London. 27 sittings. 'Telekinetic phenomena' and partial occultations of infra-red rays.
- 1933 Price accusation of fraud, 5 March 1933, referring to 28 April 1932.
- 1933 Paris, *Institut Métapsychique*, February/March. 17 sittings, negative.
- 1933 Party of 5 from Society for Psychical Research visit Rudi in Weyer, Austria, September. Usual phenomena under 'non-evidential' conditions.
- 1933-34 London, October to March, Besterman-Gatty sittings, negative. March, informal sittings, Lord Charles Hope; familiar surroundings and sitters. 'Some phenomena, restricted to curtain movements and slight telekinesis'.
- 1935-36 November to June. Schwaiger experiments, Vienna, 'positive'.
- 1937 London International Institute for Psychical Investigation. 6 sittings, 'negative'.
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Table 1.

Outline of investigations of Rudi Schneider after the death of Schrenck-Notzing (based on data and evaluations kindly supplied by Mrs. K. M. Goldney).

paranormal phenomena inspired interest and often conviction where far more academic and detached champions failed lamentably.

Harry Price was a businessman who, in his middle years, devoted a very large portion of his time, energy and money to the pursuit and popularisation of psychical research. The Harry Price Library at Senate House, University of London, is an eloquent testimony to his zeal as a collector of books and documents, as a tireless correspondent on psychic topics, and an expert on the art of conjuring. This library also contains a huge number of newspaper cuttings, all of them collected by Price, regarding the reaction of the press to his own activities in the psychic field.

Price's theoretical position can be gathered from the extracts from his writings already quoted: by and large he championed a belief in the occurrence of all the phenomena, although he was widely regarded as a fearless unmasker of mediums. As regards spiritualism he vacillated between two poles: when he wished for widespread popular support he would court spiritualist opinion, conceding that belief in survival was accepted among the majority of those who occupied themselves with such matters, and hinting that he himself shared this belief; when, on the other hand, he wished to present himself as the champion of a new scientific discipline, he would belabour spiritualism as a

mire of benighted superstition from which he personally had rescued the subject. This dual attitude, which is by no means confined to Price, must also be taken into consideration when assessing anyone's claims in this field. Public and private utterance, unguarded and official comment, are by no means identical, and the would-be enquirer has to decide for himself which he is going to accept, and to what extent.

Harry Price was, without question, passionately interested in psychic phenomena. Whereas the image of himself that he most of all desired—and indeed managed—to project was that of the keen, dispassionate, critical researcher who fearlessly denounced and exposed, a careful perusal of what he actually wrote and published suggests at times an almost uncritical credulity. For example, the same number of the *British journal for psychical research* that contained his 'Plea for a better understanding' also contains an article by himself entitled 'Psychic experiments in the Roman Catacombs'.³⁷ In this he describes how he introduced into the Roman catacombs an unnamed clairvoyant, who proceeded to give his or her 'visions' of the life of St. Agnes which differed considerably from the traditional Catholic version. It is an interesting enough tale, though it is hard to tell what it could possibly show. Yet Harry Price purports to take this tale perfectly seriously as being clairvoyant vision of the true past—using as confirmation the fact that a picture of an 'old master', supposedly a 16th-century artist, seemed to Price to bear a fair resemblance to the scene as depicted by the clairvoyant. The similarity sounds so vague and the time-lag so long (between St. Agnes's death in the early 4th century and the 'old master' there lies over a millennium of non-history) that one wonders how Price had the nerve to write this article at all, let alone suggest that the 'psychic story [received] substantial confirmation from a 16th century "old master" who may have been conversant with the true account of the girl's martyrdom which he delineates—with the usual artistic license—on the canvas . . .'. One receives the impression of an immense superstructure of careful detail, such as Price's skilful and familiar dealings with Church dignitaries and civil authorities, super-imposed on a very meagre tale indeed; but such was the teller's skill that he managed not only to fascinate his audience but in no wise impaired his reputation for being critical.

One thing emerges with certainty: Harry Price was a superb propagandist, and a tireless worker. There can be no doubt, at least in my mind, that he was genuinely committed to the subject of psychical research and was anxious to establish it as a respectable subject in the universities. Unfortunately (and of this I feel equally certain) he was also determined that he personally should be responsible for this innovation. He saw himself as the great amateur scientist, presenting the world of learning with a new discipline. However, he was not the retiring, eccentric, saintly type of dedicated enthusiast to whom nothing mattered by the impersonal truth: his own part in the drama mattered to him supremely and in the last resort his own part mattered more to him than the subject to which he had given and sacrificed so much: he was willing to bring the edifice crashing down rather than to take second place in it. From a perusal of hundreds of letters a clear enough picture emerges of a man

³⁷ H. Price, 'Psychic experiments in the Roman catacombs, a clairvoyant life of St. Agnes', *British journal of psychical research*, 2 (1929), 140–155.

passionately devoted both to psychical research and also to his own self-aggrandisement. Regrettably (like others before and after him) he managed to convince himself that these two devotions were identical and could not conflict; and occasionally the truth, in a very ordinary and uncontroversial sense of that much-debated word, became a casualty in the process.

5. Price's First Series of experiments

Rudi was accompanied to London by a Dr. Amereller, a very convenient choice from Price's point of view, since he was an engineer and brought with him Schrenck-Notzing's blue-prints for the electrical control of the medium; actually Amereller had made up the switchboard itself 'to save time' before he got to London. This electrical control was to become one of the major emotional issues in the controversy that exploded over the question of Rudi's mediumship.

As developed by Schrenck, the device, which consisted of low-voltage warning lights, controlled the hands and feet of the medium and controller by four different circuits. Price's innovation was to control the hands and feet of all sitters in the same way, making six separate circuits in all. On Schrenck's system sitters could see a panel of four lights which, if the medium was properly controlled, were all of them alight. If, for example, the controller let go of Rudi's right hand, the corresponding bulb would go out. Price added two additional circuits, one of them for the combined hands of all the sitters, the other for the combined feet of all the sitters. If any sitter broke contact then (if, of course, the device worked properly) the appropriate light would go out.

The principle of an electrical control was something of a controversial issue in England. A good many, if not most, British psychical researchers felt that such a supposed safety measure was an unnecessary complication deflecting attention from the medium and the phenomena, that with sufficient ingenuity such electrical gadgetry could be circumvented like any other device, and that the traditional dual manual control by responsible and experienced persons was more satisfactory. This was quite an issue as between Price on the one hand, and various scientists and senior members of the Society for Psychical Research on the other.

Harry Price's seance protocols are rather similar to Schrenck-Notzing's; but they are, in one respect, decisively inferior. Schrenck circulated his accounts of seances to the sitters for their comments and signatures prior to publication; but Price did not. The drawbacks of this omission will be obvious after a moment's reflection. Much of the force of testimony is lost if sitters are not asked to endorse an account. Even Vater Schneider, who had no scientific pretensions, appreciated the importance of obtaining sitters' signatures and, if possible, testimony. If reports of seances are not circulated among sitters, subsequent disagreements will be virtually impossible to settle since the dissentient will only be speaking from long-term memory. No participant had the opportunity to comment, whether by way of corroboration or disagreement, before Price's accounts were published, often months, in one case a year, later. Consequently, these accounts are little more than Price's own detailed claims and observations.

It could be argued that Harry Price would hardly be likely to publish detailed accounts of sittings, including names of participants, which were totally different from what actually occurred. On the other hand, there could well have arisen—as eventually there did arise—a bitter controversy about *exactly* what happened and when; and this controversy was to bring out the essential weakness of his procedure from an evidential point of view. Looked at in another light, the controversy could, of course, be regarded as having demonstrated just how decisive a strategic advantage failure to circulate reports gave Price: he, and he alone, had the relevant contemporary records.³⁸

After the first seance Harry Price approached Sir Richard Gregory, the Editor of *Nature*, and asked him to take part in the Rudi investigation. Sir Richard replied as follows (in HPL):

8th April 1929

Dear Mr. Price,

I am sorry it is impossible for me to be present at the suggested seance with Rudi Schneider on Friday evening next, as I am leaving on Thursday evening until Monday morning. I suggest that you might communicate with Prof. Rankine, Imperial College of Science, South Kensington, Prof. [Julian] Huxley, King's College, Strand, and Lord Rayleigh, Terling Place, Chelmsford, Essex, to see if they would care to attend seances at which, I suppose, evidence will be demonstrated of the reality of ectoplasmic phenomena.

Sincerely yours,

[signed] R. A. Gregory

Price says that these names 'had already occurred to' him: he asked Rayleigh and Rankine, and Huxley had already been notified in the usual way as a member of the National Laboratory for Psychical Research.

The seance at which Lord Rayleigh, Professor of Physics at Imperial College, London, attended was a fiasco: nothing happened, at least until he left. Price says that it was the atmosphere that was bad: the sitters did not know each other and were bad mixers, everyone was stiff and formal, whereas what Rudi needed most of all for the production of good phenomena was an atmosphere of jollity and good fellowship. He quoted Professor Hans Thirring on the subject of the conditions needed by the Schneider brothers:

In all our sittings the strongest telekinetic phenomena occurred amidst a roar of laughter when the sitters were joking or when some rhythmical chorus was sung . . . I believe that the production of the phenomena must necessarily depend on the mutual feelings of good will between medium and sitters. . . . It is obvious that a good many average men would not even be able to fall asleep in their own beds at 10 p.m. if half a dozen university professors were sitting around them waiting in deadly silence for the occurrence of the phenomenon. The far more delicate metapsychical phenomena cannot be produced by the mere will of the medium. Some psychic emotion seems to be necessary in the same way as certain sexual functions are started by emotions and imaginations. In the case of our medium the necessary emotions seemed to be furnished by rhythmical music, by the touch of a woman, or by the

³⁸ Price left his correspondence, papers, photographs and plates, books and other documents to the Harry Price Library, which is at present kept in the University of London Library at Senate House, University of London. Letters and papers relating to Price's investigation of Rudi Schneider are not kept in any one file, and relevant documents are not organised on any particular principle. I shall refer to documents to be found in the Harry Price Library by 'HPL' in the text and in footnotes.

buoyant spirit of a cheerful circle. Whenever the atmosphere of the circle resembles a lawcourt with the medium as the poor delinquent, or, even worse, when the sitting takes the form of a college examination, no phenomena will occur. . . .³⁹

Price quoted this in order to contradict the rumour that spread at once to the effect that it was Lord Rayleigh's presence that had inhibited the phenomena, presumably that in the presence of the eminent scientist trickery had been impossible and that consequently nothing had happened. However, there is no reason to suppose that Lord Rayleigh came away with a particularly unfavourable impression of the medium or the proceedings. In fact, he was to be partly in charge of the most impressive investigation of Rudi ever conducted, and in which positive results were certainly claimed;⁴⁰ and his subsequent defence of Rudi against unnamed accusers—in point of fact, Price—was to form a substantial portion of his Presidential Address to the Society for Psychical Research in 1937.⁴¹ However, none of these later happenings were dreamed of in 1929 when Rudi first came to Price in London. Price was Rudi's manager and champion, and he made every effort to interest the scientific world in his protégé.

The seance room was on the 4th floor at the back of the house at 16 Queensberry Place, London, S.W.7, a house now occupied by the College of Psychic Studies. It is entirely clear that the seances were fully and exclusively under the control of Harry Price. The first six sittings Rudi gave in London between 11 and 22 April 1929, Price's 'First Series', terminated to everyone's satisfaction. 'Olga' demonstrated her usual repertoire: curtain movements, playing of the little toy zither, writing on pieces of paper, tying knots into handkerchiefs, knocking over tables, pulling wastepaper baskets about. She also 'showed her hand', or rather people reported seeing formations ranging from a 'hand' to three or four fingers of more or less indefinite formation manipulating objects and vanishing. Moreover, vague 'snowmanlike' masses, so often seen, were also reported; 'a curious mass is visible between the opening of the curtains; it seems to have life; it slowly disappears'. On the occasion when Professor Rankine was 'guest of the evening' sitters 'distinctly [saw] the pseudopod supporting' a wastepaper basket floating about. Sitters saw a shapeless white mass form between the opening of the curtains. It seemed luminous to a certain extent and fairly solid. Price says he distinctly made out a fairly elderly woman's face with the figure of a child wearing either a child's frock or nightdress, about three feet high, remaining for about two minutes.

Mr. Price was in his element. Every opportunity, he says, was given the Press for attending seances, and the newspapers were almost wholly sympathetic and reported the experiments fully, faithfully and seriously. He says that he 'casually mentioned' to Hannen Swaffer that he would give £1000 to anyone who could produce the same effects under identical conditions to the satisfaction of the same independent observers, provided that, if the would-be medium failed, he would pay a like sum to the National Laboratory. 'To my

³⁹ H. Price (footnote 9), 22.

⁴⁰ C. Hope (footnote 13). The series of experiments described in this paper are usually referred to as the Hope-Rayleigh investigation.

⁴¹ Lord Rayleigh, 'The problem of physical phenomena in connection with psychical research', *Proc. SPR*, 45 (1938), 1-18.

amusement', he writes, 'the "challenge" duly appeared in the *Daily Express* the next morning and was published by the evening papers the same day. Sunday papers repeated the offer and one or two "featured" it, devoting the whole front page to the challenge. . . '.

Challenges to magicians to duplicate the feats of mediums are by no means new, and Price, who was an expert conjuror and extremely learned on the subject of the history of magic and legerdemain, was well versed in these episodes. Clive Maskelyne had accepted, and then backed out of, the £1000 challenge of Dennis Bradley to simulate the phenomena of the medium Valiantine. Price says he only received one or two timid enquiries, but when particulars were given nothing further was heard: one bright young man, for example, wanted to bring three of his friends and his own cabinet.

When Rudi had returned to Austria, Price received a letter from the Hon. Secretary of the Occult Committee of the Magic Circle asking for a seance. Price, 'as a friendly gesture', asked them to come to the Laboratory to ascertain if any of them could produce a single phenomenon worthy of the name under the identical conditions imposed upon Rudi. The Committee declined to entertain the offer: 'Even if the phenomena proved to be normal, it is not always simple to copy the specialist; we were challenged on one occasion to duplicate the Zanzigs' performance; comment was needless'.

Price, however, did comment: he said he knew that the Zanzigs' performance took years to perfect and several hours of practice daily, and that he had in his possession all their signalling codes. Since Rudi was known never to practise, or indeed to 'work' at his mediumship in any way outside seances, the Zanzigs were hardly a comparable case. 'All the conjurors' delegate had to do was to sit in a chair, his hands held by two persons and each limb controlled electrically, to make a noise like a steam engine for three hours; and to produce a single of Rudi's phenomena. Can it be that the reason asked Price, 'why the conjurors refused my cordial invitation to demonstrate, was because they knew they could not produce a single effect under the prescribed conditions? I wonder!' (HPL).

It seems to me that Price's challenge to the conjurors was indeed fair: Rudi was known to accept any experimental conditions whatsoever that were imposed upon him. True, once he was in trance 'Olga' might make re-arrangements, as we have already seen; but Rudi as Rudi always complied with whatever he was told to do by the persons in charge of the experiment. Schrenck had already commented upon his exemplary behaviour particularly in this respect, and every subsequent experimenter (with one single exception, oddly enough that of Price himself, which will be discussed fully later on) has agreed that Rudi never made any conditions or objected to any safety precautions. To the best of my knowledge no conjuror has ever accepted the conditions to which Rudi was submitted evening after evening, let alone produced any effects of any kind, whilst thus controlled. The magician who needed three accomplices and possibly his own cabinet probably assessed the situation fairly correctly.

6. Price's Second Series of experiments

In the autumn of the same year, 1929, there followed the 'Second Series' of Price's investigations. There were altogether 21 sittings, from October 1929

until January 1930. Price said that it was 'useless' to try and experiment in the summer, although Schrenck had certainly reported positive results in summertime. However, there was a widespread feeling among experienced sitters that the colder seasons were better for seances, and although this is not captured by the records there may well have been qualitative differences between 'good' and 'bad' seances. 'Olga' asserted that the weather made no difference.

The first experimental seance was on 14 November 1929. Those present were Mrs. Mitcheson (sister of the late Professor J. B. S. Haldane), Dr. William Brown, the psychiatrist, Wilde Reader in Mental Philosophy at Oxford and later founder and first Director of the Oxford University Department of Experimental Psychology, Miss Elizabeth Williamson, who was assistant at the University of London Observatory, Dr. Norman Jeans, anaesthetist at Salop County Hospital, Shrewsbury, Price himself, Miss Baggallay, Lord Charles Hope and Major Kalifus.

There was a long delay, after which there were some slight phenomena: the zither knocked on the table, a basket was taken out of Miss Baggallay's hand and floated about, and 'cold breezes' were felt. After some time Rudi was possessed (if that is the right word) not by 'Olga' but by 'Dr. Meier' (an entity that occasionally put in an appearance), who asked for pencil and paper. 'Meier' wrote that 'Olga' was unable to come.

The second seance took place four days later, on 18 November 1929. Again, nothing especially impressive seems to have happened. Captain F. McDermott, who acted as second controller, made the observation that he found that the medium's leg muscles contracted and trembled before and during the occurrence of phenomena. Another sitter, the Rev. Digby B. Kittermaster, a school-teacher at Harrow, remarked that it was a pity that Miss Kaye was not controlled, since sceptics might seize on this point. Eventually this criticism was met by placing the assistant behind a curtain, but Price writes that before this she was always in the habit of placing her hands on the shoulders of one of the sitters once phenomena had started.

Mr. Kittermaster, 'unwisely' according to Price, made the same remark to 'the one and only Press representative who was present at the seance'. The only person who could be meant was Mr. Charles Sutton. He had attended several of the earlier sittings of Price's First Series and had apparently been impressed then. However, he changed his mind and it appears that he told Price that 'if he were to see the phenomena a hundred times, under any conditions, he would not admit the genuineness of Rudi'.

Mr. Sutton's paper (the *Daily Mail*) then started a systematic attack on the National Laboratory of Psychological Research. Dr. E. J. Dingwall joined in by writing a letter in which he described the sittings as 'burlesque entertainment' and referred to the general 'howling' at 'Olga's' bidding. Price observed that Dr. Dingwall 'conveniently omitted to mention that he himself certainly did his share of the "howling" when he had the chance', and had joined Price in signing his famous statement that he believed Willy Schneider to be genuine.

Mr. Sutton did not claim to have found any evidence of fraud, nor did he believe that Miss Kaye had produced the phenomena by trickery, nor even that she could have produced them all had she wished: he was merely impressed

with the same experimental flaw as the Rev. Kittermaster.⁴² His campaign was evidently directed against Harry Price rather than against Rudi Schneider, and that for reasons which he was unable to print in Price's lifetime: he considered that he had evidence that Price had fraudulently contributed to the phenomena at Borley Rectory, but his editor killed the story, after consulting the newspaper's lawyer, as being too dangerous from a legal point of view.⁴³

The third seance was on 21 November. On the previous morning, Price received a letter from Major Hervey de Montmorency, D.S.O., which is certainly of some interest:

34, Clifton Hill,
St. John's Wood, N.W.8
21.11.'29

Dear Sir,

My uncle often used to tell me that when he was a young man in Paris about 1840—I believe—the notorious 'Lola Montez' (then 22 years old) was his mistress. I suggest you ask Olga through the medium Rudi Schneider what was my uncle's name what were the circumstances under which my uncle escaped from her when she attempted to kill him. You can take down her reply without letting me know what it is, and when I have narrated my story of my uncle's escapade you can see if it agrees with the medium's account of it.

Yours truly
[signed] Hervey de Montmorency

P.S. I have written out the story and will post it to you at 9 p.m. to-night.

It seems the greatest possible pity that this fascinating matter was not in any way followed up systematically. Apparently 'Olga' was only asked twice about the incident, rather casually, and on both occasions she said she would 'tell the sitters next time' or she would 'go to find out'. Then the matter was dropped. Harry Price, who reprinted Major de Montmorency's letter on page 70 of his book *Rudi Schneider*, refers to the matter again *en passant* on page 216, where he reprints de Montmorency's second letter:

Arthur's
St. James's Street, S.W.1
21.11.'29

Dear Mr. Price,

With reference to our conversation on the telephone this morning, my uncle to whom I referred was Mr. Francis Leigh of Rosegarland, Co. Wexford, Ireland. As he was born in 1815, in 1840 he must have been 25 years of age; he was then—or had been—a lieutenant in the 10th Hussars.

In Paris in about 1840, Lola Montez became his mistress and one day in a fit of jealousy she seized a pistol and fired it at my uncle; my uncle, in order to escape, jumped out of the window. As Lola Montez' apartment was in the rez de chaussée, my uncle escaped with a shaking.

Yours sincerely,
[signed] Hervey de Montmorency

Price does not refer to a telephone conversation anywhere, so it is not clear where this came in. Supposing 'Olga' *had* displayed some interesting inside

⁴² K. M. Goldney, note concerning statement made by Mr. C. Sutton on 12 June 1948. Personal files of Mrs. K. M. Goldney (in her private possession). These will be referred to below as 'KMG'.

⁴³ C. Sutton, 'Meditations', in *Inky way annual*, No. 2 (undated, purchased in 1948), 125 (KMG).

information concerning the doings of Lola Montez as described by de Montmorency, this would have been rendered completely valueless by the fact that Price communicated with the Major in addition to the written correspondence. One wonders whether the telephone conversation is the reason why the second letter is reprinted more than 150 pages later than the first. As it is, all we can say is that 'Olga' gave no evidence whatsoever that she knew anything about Lola: indeed, she did not claim to be Lola or even to know anything about her since she had to 'go and find out'.

Another instance of the extreme casualness with which experiments were carried out is that of attempts made for an observer to sit in the cabinet during seances. Lord Charles Hope and A. F. C. Pollard, Professor of Optical Engineering (Mechanical) and Instrument Design at Imperial College, sat in the cabinet on several occasions. Another time an attempt was made to get Mrs. Baggallay to slip into the cabinet during one of the pauses: Price and Miss Baggallay were to be controllers. To everyone's surprise 'Olga' asked that Mrs. Baggallay should take her daughter's place as controller, thus frustrating the plan. Mrs. Baggallay had never controlled before.

Why was this not followed up? 'Olga' was, on the face of it, extraordinarily cooperative as regards control conditions, and she had often allowed, even invited, sitters to be in the cabinet while phenomena were in progress. Supposing, for argument's sake, that 'she' wanted to show off her prowess in sensing people's plans; then 'she' could have been complimented on her perspicacity and her permission could then have been asked to allow Mrs. Baggallay or someone else to sit in the cabinet. There seems to me to be something extremely unsatisfactory about the activity of sitting with Rudi night after night, watching the phenomena as if they were a theatrical performance which had to be shown to as many people as possible, and without systematically following up anything. It could perhaps be objected that there was, at that time, some talk about Rudi's retiring as a medium, and therefore it was wiser to show the phenomena to as many people as possible. Price claimed that Rudi would not sit again, except possibly in Paris in April 1930, after the Second Series of seances, 'as he wants to devote himself to his career'. There is some evidence from letters that Vater Schneider was concerned that Rudi was losing time and money during his trips which should have been devoted to establishing himself in his trade as a motor mechanic.

Press coverage continued. Mr. Will Goldston, Founder and President of the Magicians' Club, 'the premier British conjuring society', attended a seance, was thoroughly satisfied with what he found, and duly wrote up his experiences for the *Sunday Graphic* of 22 December 1929, under the title 'A night with the ghosts':

Because I am an illusionist and a conjuror I made a special point of being the first sitter to arrive for the seance. . . . That gave me time to have a good look at the seance room. When I said 'a good look' I mean a conjuror's inspection which is severe and detailed. No objection was made against my examining the room and its fittings, so I tapped the walls, looked carefully at the floor for trapdoors and felt every inch of the two curtains which hung in the corner of the room forming the cabinet. . . . But more than that, I tested the electrical control . . . ingenious system . . . I could find no fault in this system of control or in the way it works.

I examined also the cabinet as well as the stool and the waste-paper basket

which was placed in front of the cabinet. The four ribbons attached to the curtains were just ordinary luminous ribbons . . . I kept my eyes wide open and my sense alert . . . I understand German and followed everything said.

It was suggested that an interval of 10 minutes should take place to allow the control to gather force and we adjourned to the next room for a smoke, Rudi having come out of his trance. I was the last to leave the room and I was the first to return to it. Rudi was the third person to walk from the room. He seemed tired.

We saw the stool on which stood the basket illuminated by phosphorous paint move towards us. It moved in a peculiar way and then suddenly toppled over. Curtains flew apart. We felt a fearful icy draught blowing. It was uncanny. I watched keenly for signs of trickery, but saw none. Raps. One of the students (Mr. Oliver Gatty) suggested nine . . . I am convinced that what I saw was not trickery. No group of my fellow magicians could have produced those effects under those conditions.

However successful the sittings were, 'Olga' continued to demand solo singing performances from those present. On one memorable occasion when she was presumably feeling peculiarly tyrannical, she insisted that each person present must sing 'absolutely solo'. Price started off with 'Oh Katharina!'; Mrs. Eileen J. Garrett then had difficulty in thinking of a song but rendered three lines of 'Rule Britannia'; Mrs. Baggallay sang 'In Lauterbach hab' ich mein Strumpf verloren', which 'Olga' liked; Miss Baggallay sang 'Way down upon the Swanee River'; Lord Charles Hope sang 'Au clair de la lune'; and Miss Kaye did her best with 'O Tannenbaum'. There followed, we are assured, plenty of phenomena.

At the 19th and 20th seances, on 14 and 16 January respectively, Professor Nils Hofsten, who held the Chair of Comparative Anatomy in the University of Uppsala, was present. Hofsten tried afterwards to simulate certain of the curtain movements but failed. Apparently the seance was a great success. The professor got away with a few lines of the Swedish National anthem. He declared himself deeply impressed and displayed his emotions of shock and surprise rather freely. On 22 January he wrote to Price from Uppsala, saying how impressed he was with the phenomena and asking to be allowed to take part in further seances should they be held. He added that he had had an interesting talk with Rudi and had formed a 'favourable impression of him' (HPL).

In Price's First and Second Series altogether 110 persons were present during sittings. Of these 21 acted as controllers. Harry Price himself was present at 23 seances, Lord Charles Hope at 20, Miss Virginia Baggallay at 14, Professor A. F. C. Pollard at 9, Mrs. Herbert Baggallay at 7, Mrs. Mitcheson and Miss Elizabeth Williamson at 6, three persons were present 5 times, one 4 times, five 3 times, ten twice, and the remaining ninety-odd participants attended only once. Dr. Brown was said to have been chosen as controller because of his 'obviously sympathetic nature and charming personality', and Professor A. F. C. Pollard 'on account of his experience with psychics, medical knowledge and his genial personality'.

Of those who were previously known to the medium Dr. Karl Amereller attended five sittings and Major Kalifius three. Price was present at the majority, and he controlled a great deal of the time. Lord Charles Hope had provided much of the finance for the enterprise, but he was quite manifestly not in charge of the proceedings; Harry Price was. The majority of sitters

seem to have been deeply impressed with what they experienced, and the scientists, notably Professor Pollard, Dr. Brown and Lord Rayleigh, were sufficiently interested to think it worth while participating in further investigations.

Mr. Price was entirely satisfied with the results:

If Rudi were 'exposed' a hundred times in the future, it would not invalidate or affect to the slightest degree our considered judgment that the boy has produced genuine abnormal phenomena while he has been at the National Laboratory of Psychical Research. . . . We have no fault to find with Rudi; he has cheerfully consented to our holding any test or any seance with any sitter or controller. He is the most tractable medium who has ever come under my notice. Mediumship amuses him—and bores him. He would much rather be playing football. . . . 'What does Rudi make out of his mediumship? This question is usually asked by someone who is completely ignorant of the Schneider brothers or their mediumship, because it is quite well known that the Austrian boys have never received payment for their service, as such *though there is no earthly reason why they should not*. I once saw this question hinted at in the Press, but I suppose it never occurred to that particular newspaperman that Rudi was just as much entitled to be paid for *his* services as was the journalist who made money by writing about him. The most extraordinary notion has got abroad that if you are a medium you must work for nothing!

As a matter of fact, we paid Rudi only what he would have earned at his trade, from which we took him. We *ought* to have paid him more, but so many other expenses were incurred through the investigation that we did not remunerate him as much as we would have liked. . . . Our Rudi investigation may well become a classic, and the cylinders of the dictaphone—which are being preserved—may some day adorn a museum devoted to the birth of a science which is destined to revolutionise—and perhaps even regenerate—mankind.⁴⁴

By no means everyone shared Mr. Price's exalted opinion of his investigation. He was very widely disliked and distrusted, though this strong antipathy, for obvious reasons, hardly ever found its way into print. It does, however, emerge quite clearly from the unpublished correspondence.⁴⁵ Sir Arthur Conan Doyle, for example, in a letter to Dr. Eugène Osty in Paris dated 6 September 1928, alludes to an incident in New York at which Sir Arthur was alleged to have kissed his (presumably deceased) mother's hand; Sir Arthur writes that this seance was entirely fraudulent, and that it must be Price 'who must be cheating' (IMI). Conan Doyle was during those years one of the very active members of the Society for Psychical Research, and antagonism on his part doubtless counted for a good deal. I do not doubt that one of the 'enemies' of his to whom Price alluded in his 'Plea for a better understanding' was Conan Doyle.

Although the latter was not, so far as I am aware, opposed to the idea of physical phenomena as such, a considerable and influential section of the Society for Psychical Research has in the past taken the line that, whereas

⁴⁴ H. Price (footnote 9), 219; italics inserted.

⁴⁵ Much of the correspondence that follows is preserved in the archives of the *Institut Métapsychique International*, 1, Place Wagram, 75 Paris 17 C. It is not organised in any particular way apart from being included in the general correspondence of the *Institut* in very roughly chronological order. I shall refer to these documents by 'IMI' in the text and footnotes. Translations, unless otherwise stated, are by me.

the evidence for mental phenomena is virtually cast-iron, that for physical phenomena is so ephemeral as to be negligible. Among these was Mrs. Sidgwick, Principal of Newnham College, Cambridge.

After the publication of Price's book *Rudi Schneider* in 1930 Mrs. Sidgwick wrote a letter to Mr. W. H. Salter marked 'private' in which she gave her candid and wholly unflattering opinion of Price's Rudi investigations in his First and Second Series.⁴⁶ Mrs. Sidgwick did not unfortunately permit her privately expressed views to be published at the time. This is a pity although, in view of the virulence of Harry Price's vindictiveness against those who crossed him, understandable. Her suggestion was that Rudi might have employed some smart boy or girl, perhaps some servant, to perform tricks from within the cabinet, and that this confederate could have escaped the notice of, for example, Lord Charles Hope and Professor Pollard, who were sitting in the cabinet at the time with the express purpose of catching any hypothetical confederate there. Since the cabinet was formed by an arc of curtains of width 7 ft. 3 inches, height 8 ft., and sides of cabinet from angle of wall to curtains 4 ft. 10 inches, it is difficult to see how two grown presumably sane men could have missed even the 'sharpest' accomplice.

Could Rudi have employed a 'sharp boy or girl'? The principal argument against it is that no evidence (other than the phenomena themselves) has ever been produced or even hinted at to the effect that Rudi hired anyone, or was in a position to do so. His English was virtually non-existent, he had no contacts in London and little money, his phenomena in London started almost as soon as he arrived, and I also find it hard to imagine how such a servant or other intruder could have, for so long, gone undetected and unsuspected by the sitters, many of whom were highly critical.

Furthermore, the fact that the experimenters should have been in a position to introduce a person into the cabinet *without the medium's knowledge* is of course by no means the same as the supposition that someone could have been smuggled in *without the experimenters' knowledge*. It is one thing for a number of people to arrange to secrete someone in the cabinet whilst Rudi's attention could be deflected; it is quite another for a confederate, a stranger to all concerned, to evade the sitters and experimenters night after night. Of course, by positing a sufficiently complex conspiracy such complicity could certainly be envisaged, and in the case of the Harry Price investigation quite a sizeable number of people were not at all prepared to assert categorically that it was unthinkable to suppose that Mr. Price was staging, for purposes of publicity or self-aggrandisement, or some other disreputable motive, a fraudulent charade. Indeed, he could have done this without the faintest complicity on Rudi's part.

Such a supposition could of course never be uttered in so many words. A quite specific incident had taken place between the First and Second Series in the summer of 1929, which had put Price's good faith in question among the inner circle of those who most concerned themselves with the investigation of psychical phenomena. Mr. Charles Sutton of the *Daily Mail* subsequently claimed that he had caught Price helping things along at Borley, but his Editor refused to allow him to publish this. Sutton's were not the only suspicions that were aroused at Borley: Lord Charles Hope also became

⁴⁶ E. Sidgwick to W. H. Salter, letter of 24 September 1930 (KMG).

convinced that Price was responsible for at any rate some of the Borley phenomena. Price had visited Borley for the first time on 12 June 1929, and Lord Charles Hope was present on two occasions, namely 5 and 29 July. Hope's suspicions date back to his first visit, that of 5 July,⁴⁷ and he was now in the unenviable position of thoroughly distrusting Rudi's chief investigator whilst being increasingly convinced that the Rudi phenomena were genuine. He had largely financed the Price investigation of Rudi, and he had been present at most of the seances. He had come to distrust Price between the first preliminary spring seances, and the main series in the autumn and winter of 1929. He had satisfied himself, however, that whatever might apparently have been the explanation of the Borley phenomena, it did not seem that Price could have been producing Rudi's effects. We can be quite certain that after the events of the summer of 1929, at Borley, Lord Charles kept a very careful eye on the doings of Harry Price.

Hope was a keen and painstaking student of psychical research. He kept very much in the background and only relatively few people were, or are, aware of the years of effort and patience, and of the large sums of money, that he devoted to psychical research. He was generally respected as a keenly critical and cautious investigator, and considerable quantities of detailed and informative letters (in, alas, appalling handwriting) bear eloquent testimony to his industry, his accuracy, his tact, his caution and his scrupulous fairness.

I have asked Miss Elizabeth Williamson, who was a frequent sitter at Harry Price's, to comment on her experience with Rudi. Her reply was that the sittings left her with a suspended judgment: Rudi himself struck her as a simple and honest person whereas, to the best of her recollection, she found Price distasteful and untrustworthy in the extreme.⁴⁸ Like so many other sitters, she disliked the gramophone records and the 'surrounding haze of nonsense'. The latter shortcomings were certainly not Price's fault: they were the same, much to most investigators' fury, wherever Rudi went. Now Miss Williamson (at that time assistant at the University of London Observatory, Mill Hill) was certainly not in the charmed S.P.R. circle which could be said to have been carrying on a long-standing feud with Harry Price, and this was and remained her only experience of paranormal phenomena. She says, however, that her aversion to Price may to some extent have been increased by, later on, seeing him through Lord Charles Hope's eyes. Concerning Lord Charles Hope, Miss Williamson says that his interest in psychical research was serious and that he worked hard at it, and that he never seemed lacking in a critical and intelligent standard in these matters.⁴⁹

In October 1930 Rudi went to Paris as previously arranged, to be investigated by Dr. Osty at the *Institut Métapsychique*. Phenomena were ever weaker and less frequent. However, as has been mentioned, Dr. Osty made the interesting and important observation that whatever it was that Rudi, or 'Olga', could accomplish psychokinetically, apparently affected an infra-red beam even when it was too feeble to produce macrophysical phenomena such as lifting a handkerchief.

⁴⁷ E. J. Dingwall, K. M. Goldney and T. Hall, *The haunting of Borley Rectory* (1956, London: Duckworth); also in *Proc. SPR*, 51 (1956).

⁴⁸ E. Williamson, personal communication, 6 June 1967.

⁴⁹ E. Williamson, personal communication, 18 June 1967.

Mr. Theodore Besterman, who was among the most critical of all of Rudi's investigators (he subsequently participated in an all but negative series) and who carefully examined the laboratory set-up in Paris, considered that it would have been impossible for Dr. Osty and his collaborators to engage in a fraudulent conspiracy: they could not, in his view, have produced the results actually obtained. Besterman concluded: 'I am satisfied, failing the discovery by the investigators of unforeseen circumstances, that the medium Rudi Schneider did during the sittings under review [the Osty Paris sittings] extrude under partial mental control invisible and non-photographable substance capable of partly absorbing and/or refracting an infra-red ray and of oscillating in it at a rate double that of his rate of respiration . . .'.⁵⁰

7. Price's Third Series of experiments

After spending Christmas 1931 at home Rudi returned to London on 3 February 1932 for the famous (or infamous) Third Series at Price's National Laboratory of Psychical Research. He was accompanied by his fiancée, Miss Mitzi Mängl, who had also been with him in Paris. Looking back over the press cuttings with all the wisdom of hindsight, one can tell that there was something ominous about his reception by the British press. Most of the daily papers carried pictures of Rudi and Mitzi complete with commentary to the effect that the world's highest paid medium had just come to London for tests.⁵¹ In fact, Rudi was being paid £10 per week in addition to his expenses and those of his fiancée. Gone were the days of the former visit, when Harry Price had ridiculed persons who had dared to suggest the notion that mediums should work for nothing; when he had asserted that, if only it had been possible, of course he would and should have paid Rudi more than the equivalent of what he would have earned as a motor mechanic! It must have been obvious to anyone in the know that Harry Price, who always managed Rudi's British publicity, was turning against him.

Rudi gave 27 seances in all during his sojourn in London, from 9 February to 5 May. Of these eight were totally negative, and many others very nearly so. However, as will be seen, some of the occasions on which 'the force' was operative were sufficiently impressive to a number of eminent scientists to induce them to stake their reputations on public declarations that what they had witnessed could not be explained by normal means. Harry Price decided not to employ the electrical control by which he set so much store. Why not must remain a matter for conjecture. But two innovations were introduced at his Third Series of sittings at the National Laboratory. One of these was devised by Price himself, a mechanism for automatically photographing the phenomena (see figure 9). He

constructed a box table on four legs (15 inches square, 18½ inches high and weighing 10 pounds 9 ounces) with a loose top balanced on knife edges. The underside of this loose top, at one end, was weighted with a strip of copper (weighing a little less than a pocket handkerchief) which caused the top to just overbalance, coming to rest on another strip of copper. The two pieces of copper were part of an electric circuit, and when they came in contact the circuit was closed.

⁵⁰ T. Besterman, 'The mediumship of Rudi Schneider', *Proc. SPR*, 40 (1931-32), 428-436.

⁵¹ For example, *Daily Sketch*, 8 February 1932; *Daily Telegraph*, 4 February 1932; and *Evening News*, 18 April 1932.

If a handkerchief or other light object were placed on the unweighted side of the balanced top, it would cause the copper strips to part, thus breaking the circuit. In other words, the handkerchief just overbalanced the copper strip.

From the copper strips were taken insulated wires connected to a flashlight apparatus placed to the left of the counterpoised table. The apparatus could accommodate in its reflector from one to three flash bulbs. . . . These bulbs were fired electrically by means of a 4 volt battery contained in the apparatus.

On the table and the handkerchief placed upon it were focused during the experiments a number of cameras with lenses uncovered. A half-plate camera, a quarter-plate camera and a stereoscopic camera suspended by struts from the ceiling and immediately above the table, were used to automatically photograph the handkerchief if it were displaced by normal or supernormal means. A fourth 'control camera', half-plate, was set up at the far side of the seance room, and its wide-angle lens included in its focus the cabinet, medium, controllers, sitters, table, etc. The plates in all these cameras were exposed simultaneously by the same flash thus giving us several pictures of the same objects taken from different angles. . . .

To sum up, when the handkerchief was lifted (by any means) thus disturbing the poise of the balanced table-top, the flash bulbs were instantaneously ignited electrically and the handkerchief, table, etc., were automatically and simultaneously photographed from various angles. . . .

The proper functioning of the automatic photographing apparatus was made possible only because of the existence of silent, smokeless flashlight bulbs similar to those which I employed during the investigation of the claims of Mrs. Duncan. . . . These bulbs are similar to electric light bulbs, 6 inches in length and $2\frac{1}{2}$ inches in diameter, filled with a crumpled mass of aluminium foil, . . . the bulb is exhausted and pure oxygen is then admitted at low pressure. In fact, the bulb is a capsule or silent detonator, fired by a 4 volt battery, serving to ignite the metallic aluminium which burns with an intense and highly actinic light and quite silently. These bulbs are made in Germany where they are known as the Vaku-Blitz. The speed of the flash is about $1/75$ th of a second.⁵²

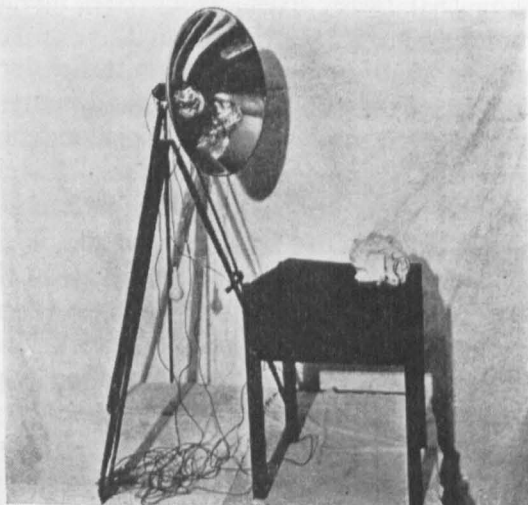


Figure 9.

Counterpoise box table and 'Vaku-Blitz' apparatus employed by Harry Price during Third Series for automatically photographing the displacement of an object. Plate 11, p. 24 of *Bulletin IV of National Laboratory of Psychical Research* (footnote 10).

⁵² H. Price (footnote 10), 11-12.

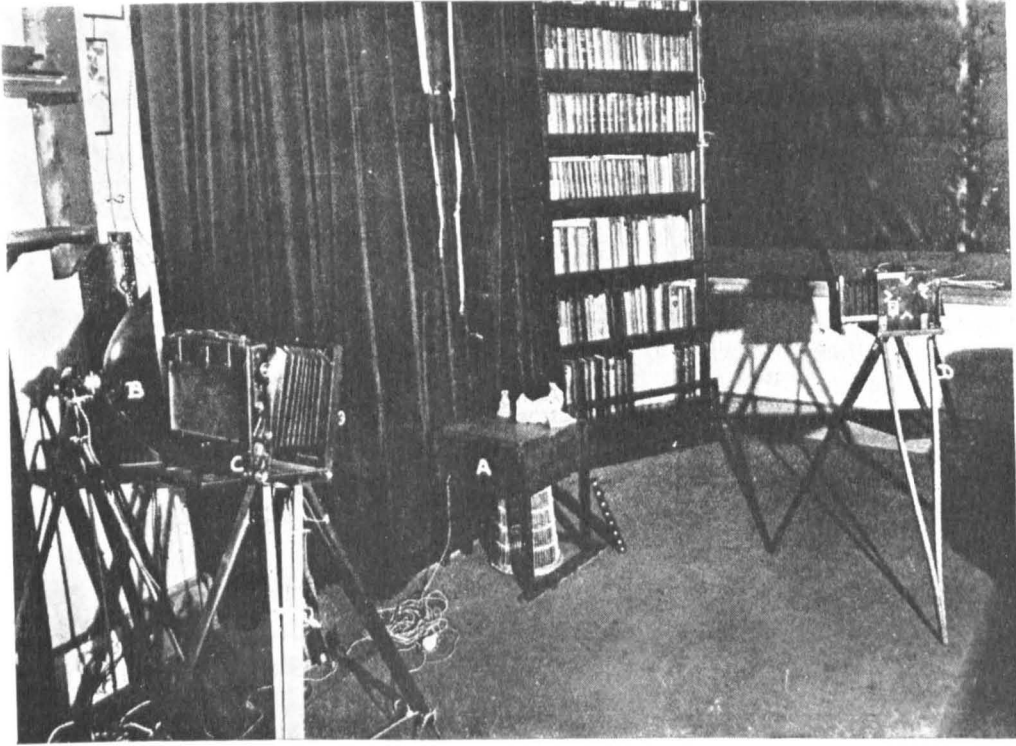
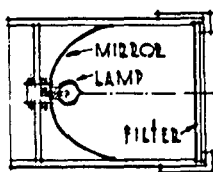
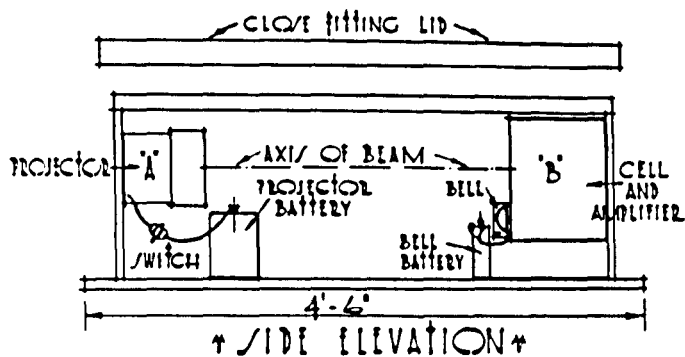
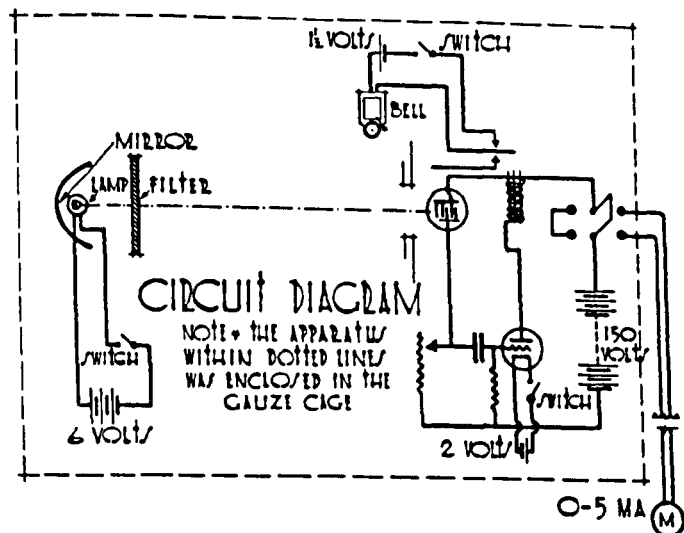


Figure 10.

Electrical installation for automatic photographing of an object placed on counterpoise box table A; 'Vaku-Blitz' flash light B on the left; cameras C and D. Overhead camera is not included. Seance arrangements by Harry Price during the Third Series. Plate III, p. 24 of *Bulletin IV* (footnote 10).

The other novel feature—new at least for the English investigators—was the introduction of infra-red ray installation similar in principle to that employed by Dr. Osty in Paris (see figure 10). This however was not made by Price; it was constructed at the University of London Observatory by C. C. L. Gregory, who was the Observatory's Director, Head of the Department of Astronomy and Wilson Observer in the University of London, together with C. V. C. Herbert (now the Earl of Powis), who was at that time a research student at the Observatory. The object of this device was to see whether Dr. Eugène Osty's observations could be duplicated in London. Price describes it as follows:

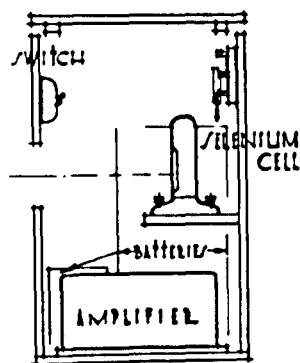
Inside a gauze cage 4 feet 4 inches long, by 1 foot 9 inches high, and 1 foot 7 inches wide, was placed at one end a 6 volt lamp and reflector in a light-tight box. In front of the box was placed a filter which allowed only infra-red rays to pass through it. The infra-red beam traversed the length of the box, striking a selenium cell or 'bridge' at the other end. By means of a suitable amplifier, relay and battery a bell could be made to ring if the beam were interrupted. If a foreign body (such as the hand) were placed in the cage so as to intercept the beam, the bell would ring. But if something less dense intercepted the beam, the bell might not ring, but the amount of interference could be read on the 0.5 milliammeter which was connected to the apparatus by insulated wires led to the note-taker's table, where an observer



SECTION OF 'A'

INFRA-RED ABSORPTION
APPARATUS
FOR EXPERIMENTS WITH
RUDI SCHNEIDER, 1932

THE NATIONAL LABORATORY OF PSYCHICAL RESEARCH.



SECTION OF 'B'

(THE IRON PARTS ARE NOT TO SCALE)

Figure 11.

Circuit diagram and sketch of infra-red absorption apparatus used at Harry Price's Third Series. Plate IV, p. 32 of *Bulletin IV* (footnote 10).

was placed with the meter in front of him. Except for the wires and the milliammeter, the apparatus was totally enclosed in the gauze cage or box, over which was placed a close-fitting lid with a gauze top. This lid was sealed to the box by means of adhesive tape. It will be seen that to affect the needle of the meter (which was set to read 6 milliamps for a full scale deflection) the disturbing object, 'force', 'power' or 'energy' would first have to penetrate the gauze before it could intercept the infra-red beams.

In order to test whether Rudi or 'Olga' could project some emanation which could affect the beam, a handkerchief was placed inside the cage under the beam. If an attempt were made to displace the handkerchief from above, the beam would be intercepted and the bell would ring—that is, if the foreign body were dense enough. In order to understand the functioning of the selenium bridge, the non-technical reader should know that selenium possesses the curious property of changing its electrical resistance when light strikes upon it. . . .⁵³

The circuit diagram and sketch of infra-red absorption apparatus are reproduced as figure 11.

The phenomena observed during the Third Series were similar to those that had taken place in previous investigations. Objects were moved, the wastepaper basket was wrenched from people's hands, the curtains billowed, the handkerchief was knotted and tugged and displaced, people felt themselves touched, and experienced the curious feeling of extreme cold that is so characteristic of physical sittings, 'materialised' partial forms were seen, and the infra-red apparatus worked and recorded occultations similar to those obtained by Dr. Osty. Harry Price obtained a number of photographs of the phenomena. These purported to demonstrate the displacement of objects taken by the same flash that shows sitters and medium in place whilst the movements were taking place (see, for example, figures 12–15). The plates are still in existence at the Harry Price Library, at Senate House, University of London.

Rudi left for Austria on 6 May 1932. So far as those involved in the investigation were concerned, nothing particularly spectacular had happened that had not occurred time and time again under rather better conditions of control. However, a number of scientists, notably Dr. William Brown and Professor Fraser-Harris, became absolutely convinced of the authenticity of the phenomena and took the very bold step of vouching for their conviction in print. At the same time, tension, to put it mildly, became manifest between Harry Price and the other investigators. Lord Charles Hope made arrangements for an independent investigation under his own control and that of Lord Rayleigh in the autumn of 1932, the 'Hope-Rayleigh investigation'.

On 5 March 1933, nearly a year after the conclusion of the Third Series and immediately before the publication of the Hope-Rayleigh results, Harry Price published an article in the *Sunday Dispatch* denouncing Rudi as a fraud. On the following day appeared the *Bulletin IV of the National Laboratory of Psychical Research*, 'A minute-by-minute record of 27 seances', being Price's version of the Third Series in which he published, in addition to the photographs already reproduced earlier and several others, certain pictures purporting to show that on one occasion, namely on 28 April 1932, when Price himself had acted as controller, Rudi had freed an arm at the same time as the allegedly

(continued on page 491)

⁵³ H. Price (footnote 10), 13–14.

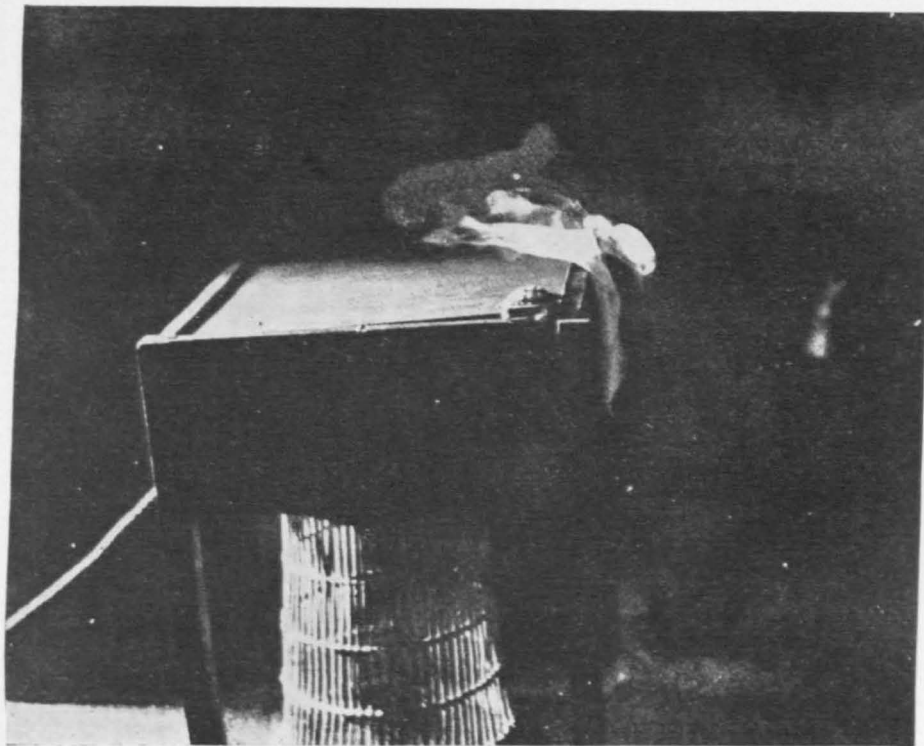


Figure 12.

Photograph purporting to show handkerchief climbing into space after displacement from the counterpoise table automatically photographed at the 9th seance (Third Series), 3 March 1932. The little toy tortoise is not disturbed. The lifting up of the handkerchief has released the automatic photographic arrangements. It is not clear from the seance record at exactly what point in time during the seance this photograph was taken, nor whether the handkerchief is supposed to be in the process of knotting itself. Plate IX, p. 88 of *Bulletin IV* (footnote 10).



Figure 13.

Photograph taken by a ceiling camera referring to the same incident as that photographed in figure 12. Plate X, p. 88 of *Bulletin IV* (footnote 10).

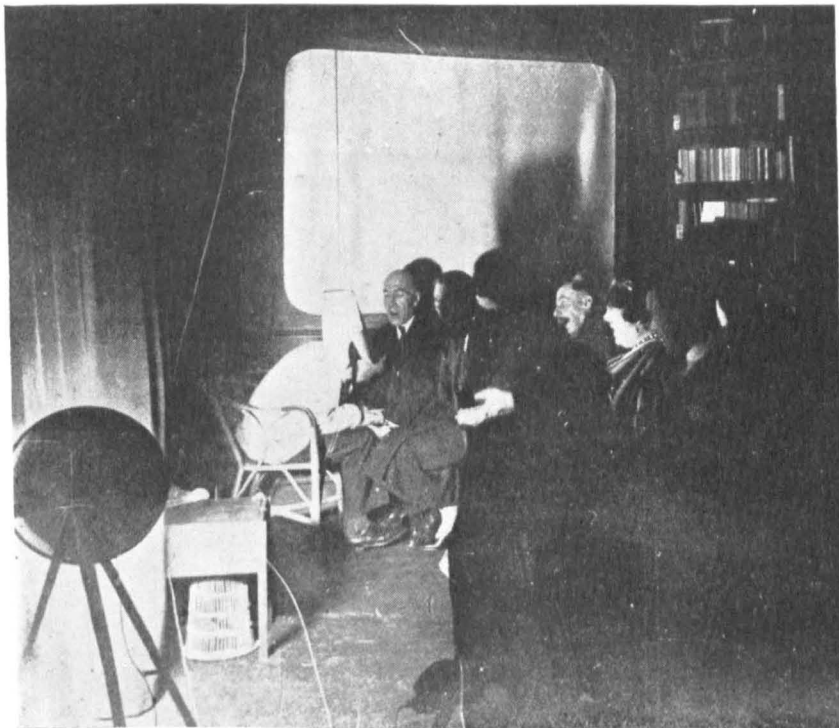


Figure 14.

Control photograph showing medium, sitters and counterpoise table with handkerchief, said to be triggered off automatically by the same flash that produced figures 12 and 13. Price is 'controlling'; note his left hand not actually holding Rudi's hands. Note also Rudi's cane arm chair. Plate XI, p. 98 of *Bulletin IV* (footnote 10).



Figure 15.

Handkerchief alleged to have been knotted paranormally at the 9th seance, 3 March 1932; Plate XII, p. 114 of *Bulletin IV* (footnote 10).



Figure 16.

A photograph prepared from plate No. 530, box 35, at Harry Price Library, after the obscuring brown paper had been removed. This is clearly the plate from which plate XX, on p. 176 of *Bulletin IV* (footnote 10) had been taken, on which Price's accusation of fraud against Rudi Schneider was based. The original illustration is too poorly defined to be worth reproducing in addition.

paranormal phenomenon of displacement of a handkerchief was taking place (see figure 16). Price added a good deal of disparaging comment on Rudi's mediumship, throwing in for good measure a photograph supplied by Captain Kogelnik eight years previously, referring to an unspecified seance some dozen or more years ago when *Willy Schneider* was supposed to have cheated (see figure 17).

Price's alleged exposure of Rudi caused a furore, and it may well be the case that psychical research has never had a more serious set-back than the Price-Schneider scandal. To the casual observer it will not be immediately obvious why the ensuing row was quite such a debacle. Even if one takes Harry Price's allegations at their face value and accepts that on one occasion Rudi managed to free his arm when Price was controlling, what of it? Supposing that at that particular seance Rudi pushed the handkerchief off the table by very normal means either deliberately, since his powers were undoubtedly waning, or possibly because his secondary personality induced him to do it; this still left a large number of occasions to be explained when he did not free an arm and phenomena were observed and recorded. The



Figure 17.

Detail from a flashlight photograph taken in Braunau by Captain Kogelnik, showing Rudi's elder brother Willy at a home seance. This was sent to Harry Price by Captain Kogelnik on 23 May 1925. The arrow points to what could be a safety pin fixing white shapes to the curtain. Kogelnik was a frequent early sitter at the Schneiders' house, fully persuaded of the authenticity of phenomena produced by both brothers, but at times critical of early seance conditions. Plate XXII, p. 188 of *Bulletin IV* (footnote 10).

most important of these phenomena indeed could not have been duplicated by a freed arm.

Scientifically what was and is at stake is whether or not physical effects are sometimes produced by other than the usually accepted physical means, and not whether a given medium always produces genuine phenomena whenever his investigators thought he did. Harry Price knew this perfectly well. However, he also understood how his *Sunday Dispatch* denunciation would be taken by the public at large: namely, as a total repudiation of Rudi's phenomena—until such time as the public at large had forgotten all about it. Price never had any intention of dismissing all Rudi's phenomena. Indeed, his motives were only too obvious to all those involved: to discredit his 'enemies', that is, those researchers who had 'taken Rudi away from him' and who had declined to accept him as the ultimate and final authority on the phenomena of Rudi Schneider; and to establish his own 1929 series, with 'his' electrical control in action, written up in his book *Rudi Schneider* (1930) as the sole authoritative investigation of Rudi's phenomena. His case, stated baldly, now was that Rudi's powers had since 1929 waned to such an extent that he had ever since resorted to fraud, and that consequently the Osty and the Hope-Rayleigh investigations, both of which claimed positive results, were worthless. He, Price, had been astute enough to catch Rudi, whereas they had been fooled.

Although scientifically it is not in itself especially interesting whether on a particular occasion Rudi had or had not, deliberately or otherwise, freed an arm while phenomena were in progress, both morally and emotionally it was a major issue. An accusation of fraud was brought against a young man who had never objected to any conditions imposed upon him by his investigators, who was a stranger in a country the language of which he did not even understand, who had unreservedly put himself into the hands of his hosts, and who regarded his investigators as well-wishers and called Price 'Onkel Harry'.

This accusation was brought nearly a year after the alleged incident, and referred to an occasion when it was Price's responsibility as controller to see to it that Rudi did not evade control. None of Price's co-investigators, with one exception that will be discussed, had been consulted or shown the evidence. On the contrary, *after* the alleged incident Price continued to vouch for Rudi's total genuineness and honesty to all and sundry, defended him in letters to the Editor of *Nature* and other scientists, and more than encouraged members of the investigating body to stake their scientific reputation on Rudi's genuineness. Their feelings towards Price can readily be imagined, particularly when he virtuously censured them for having been rather hasty in rushing into print as regards their convictions!

By no means everyone was satisfied that Price's double exposure constituted any demonstration of fraud or attempted fraud on Rudi's part. It was very odd that Price himself was controlling on the incriminating photograph. Could he really have let go Rudi's hand without knowing that he had done so? Price said he had had a violent attack of tooth ache on that occasion and was not really in a fit state to control. Why then did he do so? If he was in so very poor a condition of health, why not ask one of the others present to take his place? Why had he immediately disconnected the photographic apparatus after the double-flash seance, so that no further photographs could be taken? And above all, if the double exposure really, in his opinion, constituted proof

of fraud, why had he kept the photograph a deep secret and sprung it on the world over ten months later, instead of at once sharing this vital information with his fellow investigators, especially those whom he had, during this time, pressured to vouch for Rudi's authenticity in print? And why had he unreservedly vouched for Rudi's integrity, in writings published and unpublished, over and over again, after the alleged 'discovery'?

8. Background of Price's denunciation of Rudi Schneider

In order to understand what turned Harry Price's particularly high regard for Lord Charles Hope and Dr. Eugène Osty and his admiration and even affection for Dr. William Brown and Professor Fraser-Harris and their colleagues into virulent hatred, one must go behind the scenes and examine contemporary records. To make it easier to follow the sequence of events, a time table of some of the relevant key happenings and documents is given in table 2.

October 1929	Price's attempt at merger with Osty's <i>Institut Métapsychique</i> .
November 1930	Price's attempt at merger with London S.P.R.
February 1931	Annual General Meeting of S.P.R., at which merger is turned down.
March 1931	Price's attempt to induce Osty to publish Duncan 'teleplasm' paper.
July 1931	Price, K. M. Goldney and E. Beenham visit Braunau; Rudi and Mitzi invited to London in October.
August 1931	Price dis-invites Mitzi. Rudi refuses to go to London.
November 1931	Quartier's scathing review of Price's teleplasm paper appears in <i>Revue métapsychique</i> .
15 January 1932	Osty warns Price against Mitzi.
3 February 1932	Rudi arrives in London for Third Series.
22 March 1932	Infra-red apparatus constructed at University of London Observatory works. For the first time it is used at a sitting.
12 April 1932	First written reference to Hope-Price row, letter Hope to Price.
18 April 1932	German Brocken 'experiment' organisers in London.
26 April 1932	Major row at Council meeting of National Laboratory of Psychical Research.
28 April 1932	<i>Seance No. 25, at which double exposure purporting to demonstrate fraud was allegedly taken.</i>
3 May 1932	Sitting by which Dr. William Brown is particularly impressed.
4 May 1932	Letter from Price to Fraser-Harris: Osty sittings claimed as being authoritative and Brown 'worth a dozen of Hope and scientific friends'.
7 May 1932	William Brown's first letter to <i>Times</i> .
8 May 1932	Rudi leaves London.
9 May 1932	Fraser-Harris's letter to <i>Times</i> .
10 May 1932	Price to Professor Ach; a 'new epoch' as result of Brown's letter to <i>Times</i> .
10 May 1932	Brown to Price, asking him to cut down on publicity.
12 May 1932	Price to von Hofsten, throwing him out of National Laboratory for belated denunciation of Rudi.
14 May 1932	William Brown's second letter to <i>Times</i> .
14 May 1932	Price to Brown; 'never once has Rudi dictated conditions', and protesting against supposed disparagement of Osty's <i>Institut Métapsychique</i> .
25 May 1932	Hope asks Osty to give Myers Memorial Lecture; offers financial support for Osty-Rudi investigation.

Table 2 (*continued*)

27 May 1932	Price letter to Rudi, warning him against S.P.R., 'you will regret it all your life'.
17 June 1932	Night on Brocken mountain.
6 July 1932	Article on Brocken 'experiment' in <i>Listener</i> .
13 July 1932	Price to Fraser-Harris 'not a shred of evidence that Rudi ever cheated'.
27 July 1932	Price to Editor of <i>Nature</i> , upholding Osty's work, claiming full responsibility of his co-investigators, and vouching for Rudi's authenticity.
1 September 1932	Long letter from Price to Rudi; 'suspicious looking photograph' reference in paragraph 8.
September 1932	Culmination of Price-Brown written quarrel concerning Brown's allegedly 'soliciting publicity'.
23 September 1932	Osty to M. Olliver, who had in London heard rumours of Rudi's 'accomplice'.
October November December	} 1932 Rudi in London for Hope-Rayleigh investigation.
5 March 1933	
6 May 1933	Price's article in <i>Sunday Dispatch</i> denouncing Rudi as fraudulent. Price to Editor of <i>Nature</i> , claiming that Rudi had refused point blank to sit without Mitzi, denouncing Rudi as fraudulent.

Table 2.

Timetable giving some of the key events and documents relating to Harry Price's 'exposure' of Rudi Schneider on 5 March 1933.

8.1. *Relations with French researchers.* In October 1929, whilst Price was in the middle of his Second Series and Osty was examining Rudi in Paris for the first time, Price approached Osty with a view to amalgamating his own National Laboratory of Psychical Research with the Paris *Institut Métapsychique International*. These negotiations seem to have been kept completely secret, but the correspondence is preserved at the Paris *Institut*. I was certainly most astonished when I came across it there.

In a long letter dated October 25 1932 Price set out the conditions of the proposed amalgamation (IMI). The contents of his laboratory, an itemised account of which he gave, were to be transferred to Paris as well as Price's library, at the cost of the *Institut Métapsychique*, in return for which Price was to be given a position on the Council or other committee, and to have access to the work of the *Institut*. The address and title were to remain in London for correspondence purposes. The property was valued at £7000, of which £5000 was estimated to be the value of the books. Price was to make the first announcement of the merger. 'It is understood, of course', he wrote, 'that the suggested amalgamation has also to be confirmed by our Council but I think that is merely formal . . . '.

Whatever may have been Price's hold over his own Council (and events were to show that this was by no means composed of such yes-men as he imagined) his offer was eventually turned down by the Paris researchers: on 4 November 1929 Osty wrote to Price, thanking him in the name of the members of the Committee for the generous offer and declining it on the grounds that in the view of the Committee they needed all their rooms for their own purposes (IMI). Osty added that he personally had hoped for a

different outcome; but new projects had been planned requiring much more extensive instrumentation than previously, and he ended by expressing his personal goodwill.

Price did not take this refusal in good part. His reply dated 8 November 1929 is curt, saying that 'this is quite different from what Professor Richet said in his letter' (IMI). I have not been able to find Richet's letter to Price on this subject. There can be little doubt that Price took the French rejection as a personal affront on Osty's part, the latter's affability notwithstanding.

There followed a cooling off in relations between Price and Osty. When Price suggested that his secretary, Miss Lucy Kaye, should accompany Rudi to Paris as a companion, Osty thanked him politely and declined on 17 February 1930, on the grounds that he would prefer someone who spoke Rudi's language (IMI). Price replied: 'I note that you do not want to see Rudi established . . .' (IMI). On 28 February 1930 Price wrote to Osty that he was sorry that Rudi was having a bad patch: 'I am wondering if "Olga" would be more gracious if "Uncle Harry" were present! I suppose Rudi is not home-sick or unwell?' (IMI). But Osty did not rise to the bait: at any rate no reply is on record.

On 9 April 1930 Dr. Gerda Walther, Schrenck-Notzing's former personal assistant, who was also a friend of the Schneider family and very active on the German parapsychological scene, wrote a letter to Price (in HPL) which I personally, in view of what happened, regard as fateful and significant. In this she wrote:

April 9 1930

. . . Is it true the investigators in Paris intend to let Rudi sit without the least control, just to put him on a chair without even holding his hands, and then let things go as they may, photographing all with their new apparatus? I think that would be a very bad thing indeed. You know Rudi has very violent jerks in trance very often, he might fall from his chair or hurt himself some way if nobody holds him. Besides he might make some movement of the hands or feet towards the cabinet or the objects that are to be moved, meaning no harm, not even knowing what he does, when he isn't held. Yet if this movement was photographed his adversaries would be sure to jump upon it and say it was a proof of fraud. I don't like the idea at all. Why not control him electrically or at least by two sitters as usual and then photograph the phenomena in the dark? Can't you do something to press upon Osty how important this is? You know how it was in Budapest when Mr. Besterman 'controlled' the medium Layos Papp. He let go Papp's hand and then said he tried to make the phenomena himself with his free hand. . . .

Price does not seem to have passed on the warning to Osty. However, in my view at least, he remembered it well enough.

It will be recalled that meanwhile Lord Charles Hope had reached the conviction that Price was not entirely trustworthy on account of his conduct in connection with the poltergeist phenomena at Borley rectory. On 13 July 1930 he wrote to Osty (IMI):

. . . I know that Mr. Price at one time entertained the idea of asking Rudi Schneider to visit the National Laboratory again this coming autumn. I have told Mr. Price that in my opinion nothing further could be achieved by another similarly conducted series of seances in London and that I consider it of much greater importance that Rudi should visit the *Institut Métapsychique* . . . I would be willing to make a considerable donation for that special purpose . . . I make this proposal in a purely private capacity and not as in any way connected with the National Laboratory of Psychical Research. . . .

Osty gratefully acknowledged the offer, and eventually the sum of 2000 francs. Despite his misgivings it was Lord Charles Hope also who largely financed Price's investigations in London.

In March 1931 Osty received a typescript of an article by Harry Price on the subject of the microscopic analysis of 'a teleplasm'. This analysis, so Price stated in an accompanying letter dated 9 March 1931, was supposed to have been performed by 'the most eminent analytic chemist in England' (HPL). Osty's reaction was: '... I have read your article on the chemical and microscopic examination of the alleged teleplasm. Do you now think that it would be of vital importance to be quite certain, *absolutely certain*, that you are dealing with actual teleplasm? The publication of such an article would meet with tough opposition. I don't advise it ...' (HPL). Price replied (HPL and IMI):

... Re the portion of teleplasm which I secured. Personally I am convinced of its genuineness. Several portions were taken and other persons including Sir Oliver Lodge have received specimens. Owing to the jealousy of another Society I am unable to give full particulars as to how I got it and where it came from. But someone will eventually write a paper on it and I want to be the first. A piece of my portion was handed to the most eminent analyst in London and his report coincides with my deductions. ...

Price was evidently undeterred by Osty's judgment and, determined to 'be the first', he went ahead and published his article in the Italian journal *Luce e ombra* in May 1931. However, as Osty was later to put it:

Mr. Harry Price, having failed to get himself invited to the seances of the medium whose teleplasms he had secretly received and analysed, could not content himself with the glory of being the first to publish his analysis; he also wished to anticipate the report of this group by one by himself in order, as usual, to shine in the pages of the press. To this end he had to obtain secretly the services of the famous medium. He got in touch with her husband and managed to have sittings with her without the knowledge of the investigating group. ...⁵⁴

In point of fact, as a result of these investigations Harry Price published a lengthy article, 'Regurgitation and the Duncan mediumship', purporting to show that Mrs. Helen Duncan, the medium in question, was a fraud who produced the supposed materialisations by means of cheesecloth that she swallowed and regurgitated during seances.⁵⁵

Osty took the line that the engineer had been hoist with his own petard, and published in the *Revue métapsychique* a most ironical review of Harry Price's own work on Mrs. Duncan by C. Quartier.⁵⁶ Harry Price's reaction (in IMI) was:

2 November 1931

In the article 'L'ectoplasme de Mrs. Duncan' which I find very amusing I think you do me rather an injustice as the article reads as if I believed—or rather accepted—the genuineness of the 'teleplasm'. As a matter of fact I reiterated that I was only concerned with the *analysis* of it. If *Luce e ombra*

⁵⁴ E. Osty, 'L'étrange conduite de M. Harry Price', *Extrait de la Revue métapsychique*, (Avril 1933), 110–125 (p. 114; my translation).

⁵⁵ H. Price, 'Regurgitation and the Duncan mediumship', *Bulletin I of the National Laboratory of Psychical Research*, (1931), 1–120.

⁵⁶ C. Quartier, review of Price (footnote 55), *Revue métapsychique*, 6 (1931), 447–448.

and other publications have elected to say more than I did, that is their affair. The reason why I so much desired to publish my article on the teleplasm was to give the Duncans confidence that they would come to my Laboratory. You know the rest. . . .

It is hard to see how Osty could have been expected to take Harry Price seriously after this. Price had plainly and in writing stated his faith in the 'teleplasm' and expressed his eagerness for priority of publication—only to repudiate both on the most barefaced grounds of expediency without any apparent recollection that he had ever thus expressed his faith, or that he had attempted to let Osty himself in for publishing the article—which he now declared to have been a pack of lies to deceive the medium and her husband! However, there was no immediate and open rupture. As we know, Price succeeded in obtaining Rudi's services, despite Lord Charles Hope's discouragement, for his Third Series February to May 1932, and even prevailed upon Hope to finance the venture to a large extent. If Hope had had more inside information concerning Price's dealings in connection with the Duncan mediumship, it seems more than doubtful if he would have had anything further to do with Price.

On 15 January 1932 Osty wrote a letter to Price which he was to regret bitterly, in which he advised Price not to allow Mitzi, Rudi's fiancée, to be present at seances: 'When there are no phenomena she tries to produce some and does so' (HPL and IMI). Osty also said that from October 1931 Rudi had not produced in Paris any physical phenomena discernible by the senses, and that without the instrumental arrangements it would have been a complete waste of time. Price replied on 18 January (HPL and IMI) to the effect that he was

. . . *very* grateful for the information. . . . If we are compelled to have Mitzi in the seance room sometimes, we will see that she is specially controlled. At our last sitting in Braunau in July Mitzi sat between Mrs. Goldney (a very intelligent member of our Council) and Miss Beenham, our Secretary. I am convinced that she could not have helped in any way as she was being thoroughly controlled. Now that Rudi has turned his mediumship into a profession (we have agreed to pay both their fares, hotel expenses and £10 per week) it is a great temptation for him to cheat if the genuine phenomena are not forthcoming. We were compelled to offer him £10 per week in addition to hotel expenses) because he said that was what he received in Paris. . . .

Osty answered (HPL) that Rudi himself had never occasioned the slightest suspicion.

In fact there had already been a good deal of feeling between Price and the Schneiders on the subject of Rudi's remuneration and conditions of employment. Price had, beyond a shadow of doubt, been deeply impressed by the seance he had in Braunau in July 1932 when he, jointly with Mrs. Goldney, Miss Beenham and Dr. Gerda Walther, had imposed very stringent control conditions indeed. He had on that occasion invited Rudi to London for sittings in September. Rudi had just become engaged to Mitzi Mängl who was, from that summer onwards till the end of his days, his constant companion. Rudi said he would not come to London without Mitzi, which is even more understandable if one remembers that he did not speak any English. So Price invited Mitzi with whose conduct *he* was obviously satisfied, to accompany

Rudi to London. Then, in September 1931, Price suddenly went back on this invitation without offering any explanation. Rudi had not been investigated in the meanwhile.

The reason for Price's sudden withdrawal of Mitzi's invitation was in fact financial: his Council were concerned about the expense involved in investigating Rudi.⁵⁷ Rudi, who had agreed to come to London provided he could bring his fiancée,⁵⁸ reacted by refusing to come at all, and Price sent him a sharp telegram demanding the advanced sum of money back. What had happened can be gathered from a letter by Vater Schneider to Harry Price's secretary:

13 October 1931

... We deeply regret that Rudi's intended journey came to nothing but Mr. Price is really to blame for that. Had he not sent the telegram demanding the return of the money in not exactly flattering words, Rudi would have left for London on the first of September. Mr. Price should understand that Rudi would like terms which should include everything, even the money question. The first time Rudi was in London he paid out of his own pocket 160/-, and in any case Rudi wanted to make sure how much money he would have to take along from home. Mr. Price's reproach that Rudi was beyond all price and greedy hurt Rudi's feelings very much indeed, for I myself know only too well how many hundreds, even thousands, of sittings Rudi has given without the smallest remuneration. This, and the fact that Mr. Price had originally included Mitzi in his invitation, caused Rudi to return the money and to unpack his bags. I myself know that Rudi had an invitation to come to London and was promised, apart from his board and lodgings, a salary of 1,000/- per month which, however, he declined only in consideration of Mr. Price.⁵⁹

Osty had invited Mitzi to Paris and also paid Rudi a salary of £10 per week and expenses. Rudi was about to get married, he was concerned about his financial security, and he was no longer as pliable as regards 'Onkel Harry' as he had once been. If Price wanted him, he had to make some definite financial arrangements with him. It was, as a matter of fact, Lord Charles Hope who was paying most of the expenses, but Price did not like to be crossed: he had expected Rudi to come to heel and had looked somewhat foolish in the eyes of Professor William MacDougall, who had been promised a seance, when Rudi failed to turn up in London in response to Price's peremptory telegram. Price was doubly angry because he considered that his hand had been forced as regards terms by Dr. Osty, who had just gently laughed at him over the Duncan 'teleplasm' affair, and whose work on Rudi was being taken far more seriously by influential members of the Society for Psychical Research than Price's own.⁶⁰

All this explains quite adequately the fact that when Rudi arrived for the fateful Third Series, the press unanimously proclaimed that London was witnessing the arrival of 'the highest paid medium in the world'.

⁵⁷ For example, a letter from V. A. B. W. Cochran-Baillie to C. Hope of 26 May 1933 (KMG).

⁵⁸ K. M. Goldney, special note with regard to Price's letter to *Nature* of 6 May 1933 (SPR files, ref. 8-10, special folder).

⁵⁹ Original and translation, probably by Mrs. de Gernon, in HPL.

⁶⁰ T. Besterman (footnote 50); also correspondence between T. Besterman and E. Osty, 1930 to 1932, in IMI.

8.2. *Relations with the London Society for Psychical Research.* Meanwhile there had been another important development exacerbating relations between Mr. Price and the world of psychical research which one has to consider in order to understand the emotional atmosphere behind the scenes before and during Rudi's third visit to London. Harry Price, having failed in amalgamating with the Paris *Institut*, about a year later launched an all-out offensive to merge his National Laboratory with the London Society for Psychical Research. On 12 November 1930 he addressed to all members of that Society a circular printed letter, marked 'Private and Confidential' and proposing this amalgamation. In it he said that his health was poor; that he was averse to joining forces with a society frankly spiritualistic, and that he was reluctant to let his Library and his Laboratory fall into foreign hands; that there remained the Society for Psychical Research. . . . He offered his books, his apparatus, the mediums he could attract and the transfer of all members of the National Laboratory who cared to join the move. 'In return I should of course expect to take a major part in all investigations brought about through my agency and generally to cooperate with the SPR [Society for Psychical Research]' (HPL). As the negotiations with the French *Institut Métapsychique* had been secret, no-one was in a position to point out that his library and apparatus had just escaped 'falling into foreign hands' solely because foreign hands had been averse to having them.

A second printed letter from Price appeared dated 19 December 1930, in which he regretted that the Society for Psychical Research had turned his offer down: 'I have failed in my endeavour to instil into the London Society for Psychical Research some new and active blood'. However, he was nothing if not persistent. The matter was raised again at the Annual General Meeting of the Society on 26 February 1931, that is, when Rudi had just begun his second long sojourn in Paris under Osty. Sir Oliver Lodge was in the Chair. Mrs. K. M. Goldney moved the resolution 'That this meeting approves of the amalgamation proposed by Mr. Harry Price and supported by an overwhelming majority of SPR voters of the National Laboratory of Psychical Research with this Society and requests the Council to appoint a committee to discuss the details of the suggested plan with the Administration of the National Laboratory'.⁶¹

Mrs. Goldney's mention of support by the overwhelming majority of members of the Society referred to a private census among members conducted by Mr. Price. There was, however, very strong feeling against Price among members of the Society's Council: his methods were said to be different from those of the Society, 'a certain amount of publicity was involved'—a delightful under-statement; the word 'National' was used for a privately managed enterprise; there had seemed to be 'impulsive haste' about the move; members had been privately circularised over the heads of the Council whilst the proposal was still *sub judice*; and members had been expected to vote without knowing the details and exact terms of the propositions. The Society, it was decided, must adhere to its traditional methods. After some discussion in which Lord Balfour, Mr. de Brath and Mr. Bousfield are on record as having taken part, it was decided that the motion be withdrawn and further detailed

⁶¹ Report of the Annual General Meeting of the Society for Psychical Research, 26 February 1931, in *JSPR*, 27 (1931), 53–58.

proposals submitted to the Council. In short, the Council of the Society for Psychical Research, like the *Comité* of the *Institut Métapsychique*, did not favour a merger with Mr. Harry Price's National Laboratory of Psychical Research.

On 8 April 1932 Mr. Theodore Besterman asked for permission to sit with Rudi, and Price replied on 12 April that, as far as he was concerned, Besterman was welcome to attend Thursday seances,⁶² but that Rudi had taken a dislike to Besterman, who should address his application to Rudi (HPL). This was probably a complete invention. Rudi was quite happy for Besterman or anyone else to be present, and he subsequently allowed Besterman to investigate him at length. What was Price's objection? As has been indicated, Besterman had meanwhile established extremely cordial relations with Osty whose work he greatly admired. This emerges with great clarity from the correspondence preserved at the *Institut Métapsychique*. On 13 April 1932 Besterman wrote to Osty: ' . . . Although as you know I have not hesitated to express myself in very critical terms of parapsychical phenomena as such, I must acknowledge myself convinced. On the basis of your report I am fully persuaded of the genuineness of the phenomena you witnessed . . . ' (IMI).

Furthermore, Besterman had in print taken Osty's work seriously and damned Price's with faint praise. In the *Proceedings of the Society for Psychical Research* he had written a lengthy account of Rudi's mediumship to date, in which he stated that Price had extended Schrenck's electrical control to all sitters: ' . . . unfortunately this method is very defective and deprives the seances . . . of much though by no means all their value '.⁶³ This was indeed the unforgiveable sin: not only had he called the electrical control Schrenck's when Price set particular store upon being called its inventor, he had also dismissed it!

On 22 March 1932 C. C. L. Gregory and C. V. C. Herbert, who had completed the construction and testing of their infra-red apparatus in the workshop of the University of London Observatory, took it to a sitting at Harry Price's Laboratory. C. C. L. Gregory (my late husband) repeatedly told me how he and Herbert took the apparatus in Gregory's car that evening and that, so as not to leave the equipment unattended for a single minute, he watched over it and went without his supper. Neither Rudi nor Price knew what to expect, and the instrument registered occultations of the infra-red beam the first time it was used. I have asked the Earl of Powis for his recollections in this connection, and he writes:

18 August 1967

The infra-red apparatus was, as you say, made by Gregory and myself and taken by us—without any warning—to one of Price's seances where Rudi made it work the first time under what seemed fraud proof conditions. We could only reproduce the effect normally by blowing cigarette smoke into it—breathing into it had no reaction—and we could not see how Rudi could have produced any smoke in a completely dark room, especially as he had no reason to suppose that this—or indeed any—new apparatus had been used. . . . of course he [Price] was no scientist; but in my view he was absolutely brilliant as a master of ceremonies at a physical seance. He had unbounded

⁶² There were normally two sittings per week: those on Tuesdays were for Price's Council and for special sitters, and those on Thursdays for fee-paying sitters of lesser importance.

⁶³ T. Besterman (footnote 50), 432.

enthusiasm and was absolutely tireless—he would willingly go on all night. He certainly got far better results with Rudi than anyone else in this country.

Although C. C. L. Gregory's opinion concerning Price was somewhat more reserved—partly because he had, as he told me, on one occasion caught Price altering a temperature chart after a seance—both he and Herbert were absolutely convinced that something quite remarkable had happened in the seance of 22 March 1932 when their own apparatus had worked in registering infra-red absorptions, and I know that from this moment on Gregory took the investigation seriously. He had not really expected his apparatus to register anything; when it did, he felt satisfied that something wanted investigating.

Lord Charles Hope had, as we now know, been for some time trying to organise an investigation of Rudi independent of Price, indeed as far back as 1930. He mistrusted Price ever since the Borley incident, and felt profoundly uneasy at inducing a number of his scientific friends to commit themselves as to Rudi when any day some scandal might burst over Price's integrity and methods. When Hope realised that, as a result of the success of the infra-red experiments, Rudi stood a chance of being taken seriously by scientists in Britain, he became resolved that Rudi's future should not be inextricably bound up with the reputation of Harry Price, and he decided that an independent investigation was now absolutely essential.

It seems that Hope expected that the matter could be smoothed over without too much unpleasantness. He merely proposed that Rudi should be asked to come to England again, or remain in England, and be investigated by another committee, not at the National Laboratory and not under Price's direction. Price, however, was livid with rage, as emerges from the following letter (HPL) from Hope to Price:

April 12 1932

I have received your letter of April 8th. It is a little difficult for me to grasp your point of view. Surely you made it clear at the last Council meeting that you had no intention of prolonging Rudi's engagement with the National Laboratory after May 4th. I remember your saying it would be quite impossible to have him at the same time as the Polish medium. Also you yourself told me that Professor F. Harris had telephoned you about Rudi being engaged for May . . . so why you should consider yourself in any way hard used passes my comprehension. I am loth to attribute to you any 'dog in the manger' sentiments, but really you make it seem that way. I must also remind you that I myself paid the expenses incurred by Rudi's visit 1929-30 and have always done my best to help you with investigations of his powers and will be willing to do so again if he should come again later on.

Surely the important thing is to further psychical research and not to allow one's personal feelings to carry any weight. Surely, too, experiments with two mediums are likely to be more valuable than one, and you will be fully occupied with the Polish medium who, I hope, will prove a great success. It should be a matter of great satisfaction to you to feel that experiments with Rudi will continue at the same time. . . .

Price made it amply plain that it was not, so far as he was concerned, a matter of great satisfaction that Rudi should be examined by others. He replied (HPL):

April 16th 1932

I have received your letter and I am astonished at the attitude you take in the matter under discussion. The brutal facts of the case are that the very

people I am running the Lab. for have conspired in a miserable plot against me, and no amount of sophistry will alter them. Evelyn (probably hearing that I had discovered the plot) invited me to tea on Friday and was honest enough to make a clean breast of the whole affair.

Your remark that it would 'look bad' if Rudi is not tested elsewhere is ridiculous because he has just been to Paris for 15 months. But my sole complaint is that it was thought necessary to conspire against me to get a few more sittings with Rudi. It is like the host at a dinner party having his throat cut by his guests.

As regards Rudi, we can save ourselves further discussion in the matter, because he has had a letter from his friend in Vienna who offers to put him in charge of a branch he is opening in Braunau if the boy can take up the appointment during the next few weeks. Rudi is leaving London on May 6th.

From this point onwards Price was hardly on speaking terms with most of the other investigators. He regarded it as a personal affront and as a vote of no confidence that an attempt should be made to secure Rudi's services for a series of which he was not the moving spirit, and one must do him justice; it was, when all is said and done, a vote of no confidence. He must have realised that Hope no longer trusted him since the Borley incident; and Price also knew all about luring mediums from their investigators!

A more disciplined, a more dedicated or a less egocentric man than Price might have come to terms with the situation, however painful; he might have taken what credit there was to be had for launching Rudi on the English-speaking world and for extending Schrenk-Notzing's electrical control, turned his attention to other mediums, and waited whether Hope's next investigation would confirm his own and Osty's results. But Harry Price just did not have it in him thus to fade from the scene. He felt he had been betrayed and rejected and, with all the considerable vigour and resource at his disposal, he proceeded to behave like the bad fairy who had not been asked to the christening.

8.3. *Relations with members of the National Laboratory of Psychical Research.* There was a major quarrel at a Council meeting of the National Laboratory of Psychical Research on 26 April 1932.⁶⁴ Tempers rose very high; Price insulted Hope and Hope resigned from the Council. There ensued a ferocious correspondence between Price and Fraser-Harris in which Price came close to calling the latter a liar over the question of just who had mentioned what date and when, in connection with Rudi's departure. Price may have felt he had gone too far, and he wrote in a more conciliatory vein (HPL):

28 April 1932

... I am very sorry that you should have been drawn into this altercation at all. I have had trouble with Lord Charles Hope before. He insists upon 'bossing' things—but he will not 'boss' anything with which I am connected if I am paying the piper and am doing all the work like I have done for the last 10 years. I should never dream of trying to 'boss' another person's show, nor should I ever dream of doing such a mean, contemptible trick as a certain section of the Council apparently tried to play on me.

I am very fond of you and I think your personality is delightful. . . . Let us forget all about this wretched affair. . . .

⁶⁴ K. M. Goldney, personal communication; also letter from C. Hope to K. M. Goldney of 12 March 1933 (KMG).

The wretched affair, however, was not forgotten by anyone, and Fraser-Harris, not in the least mollified by being complimented on his delightful personality, replied in the most frigid terms (HPL):

3 May 1932

I have to thank you for your letter of April 28th . . . I cannot allow the expression 'conspiring against me to take away the boy from the Laboratory' to go unnoticed because that was what none of us contemplated doing. That was why I began 'arising out of the conversation at the Council Meeting, would you have any objection to some of us having sittings with Rudi after his contract is closed', etc., and in the meeting with him at the hotel we asked [Rudi] 'After your contract is closed at the Laboratory would you be willing . . .

All our plans related to the dates after May 4th. At the same time, I do not see that you can have any valid objections to my group of genuine investigators, who are prepared to remunerate R., having seances with him. For you to object to this might lead to the *gravest* misconceptions becoming current. You are right in thinking that I am deeply interested in the proceedings at the Laboratory and in gaining unique experiences. I also acknowledge once more your kindness in regard to my position on the Council. . . .

Price's reply to Professor Fraser-Harris is of some importance because of its date, 4 May 1932. The row at the Council of the National Laboratory had been on 26 April. Price's alleged secret 'unmasking' of Rudi by means of a double exposure took place on 28 April. Yet there is not a hint in the letter from Price to Fraser-Harris that there was anything amiss on 4 May, and on 5 May Fraser-Harris was asked to control Rudi without being given the faintest inkling that anything suspicious was supposed to have occurred. I quote from Price's letter (in HPL) to Fraser-Harris of 4 May:

The sting of the whole affair was that the very people for whom I run the Laboratory and who were, to all intents and purposes, my guests, should go and do something behind my back whether they were 'entitled' to do so or not. I regard it as the last word in ingratitude.

Your argument that the 'gravest misconception' might arise unless Lord Charles Hope tested the boy independently is knocked on the head by the fact that he has been in Paris for 15 months under Osty. Also he has been all over Europe by himself. Also I have purposely refrained from attending several seances [four out of twenty-seven] in order that other members of the Council, Lord Charles Hope, etc., could have the boy to themselves . . . I do not call Lord Charles Hope critical in the slightest . . . I reiterate that Lord Charles wants to 'boss' anything psychic with which he is connected and I know him much better than you do. . . .

We had a brilliant seance last night and Dr. William Brown (whose opinion is worth a dozen of those of Lord Charles Hope and his scientific friends) was very impressed. I understand that in tomorrow's *Daily Mail* there will be a statement of some sort concerning last night's experiment. . . .

The two important points to note are that Price at this point and on this date, that is *after* his alleged discovery of fraud by Rudi, represented Osty's experiments as validating his own, and that he similarly regarded the championship of Dr. William Brown as vindicating his own Third Series.

9. The Brown and Fraser-Harris *Times* correspondence

There cannot be any doubt that Dr. William Brown *was* deeply impressed, and that at Harry Price's prompting he overcame his very considerable

reluctance to commit himself in print by giving newspaper interviews and writing to the press. In particular, he wrote a letter to the *Times* referring to the sitting of 3 May at which the controllers were Capt. the Hon. V. Cochrane-Baillie and Mrs. Dorothy de Gernon. Dr. Brown's letter appeared in the *Times* of 7 May 1932 and ran as follows:

Sir, As a certain amount of publicity, unsolicited by me, has been accorded to a striking experience which I had last Tuesday night at the National Laboratory of Psychical Research, I should like to give a brief record of my impressions in your journal while they are still fresh in my memory. A detailed record of every incident was made at the time by a shorthand writer, but that is another matter.

I was one of a circle of seven people at a seance with Rudi Schneider, the Austrian medium. We sat, holding hands, in front of a 'cabinet' or small recess curtained off by two heavy curtains, edged with luminous strips, with the medium to the side, about 2 ft. from the left-hand curtain and fully controlled as to his hands, feet and head by one of the sitters. A shaded red lamp hung in front of the curtains and on the floor in front of them, illuminated by the red rays, was a small table with a detachable top on which was placed a knotted handkerchief. A luminous wastepaper basket was also nearby. Inside the cabinet, on the floor, were vases containing different kinds of flowers—daffodils, anemones, etc. I searched the cabinet carefully and found nothing else there. Then the room plunged into darkness, apart from the red light, and Rudi went into trance characterised by rapid breathing which was kept up over long periods of time. His trance personality 'Olga' appeared and spoke in whispered German.

Later the curtains began to shake and billow out as if blown by a breeze and we had a feeling of cold. After some experiences with the basket which 'Olga' directed me to place on my knees and to hold near the curtain, and which was moved and tugged by some unseen agency, sounds of movement seemed to come from the small table and a loud bang was heard in a far corner of the room. Subsequently, on turning up the light, we found that the detachable top had been hurled over our heads into that corner, and that the knotted handkerchief had been transported over our heads into the opposite corner of the room.

Then a mass of flowers suddenly emerged from the curtains at a level of about 4 ft. from the floor and fell under the red lamp. They had been caught up from the vases in the cabinet by some unseen agency. Mr. Harry Price, the director of the Laboratory, who was sitting immediately in front of the shaded red light, was asked by 'Olga' to hold a flower by its head, with the stalk pointing towards the curtains, under the red light. It then disappeared towards the cabinet. I was asked to do the same with another flower. I felt an impact at the end of the stalk and then the flower was drawn from me into the darkness. These were the outstanding events of the sitting. I could find no evidence of fraud or trickery, and while retaining an alert and critical attitude of mind throughout, had a strong feeling of some mysterious power working from within the cabinet, a power for which I could imagine no mechanical or pneumatic contrivance as a cause—at least such as would be possible under the conditions of the seance.

I have had sittings with other 'physical' mediums, and in no case have I received the impression of genuineness in their manifestations such as I had with Rudi last Tuesday night. Undoubtedly the phenomena are worthy of the closest scientific investigation.

Oxford

I am, etc.,
William Brown

Professor Fraser-Harris decided to support Dr. William Brown, and within a couple of days also wrote to the Editor of the *Times*:

May 9 1932

Sir, As a certain amount of wholly unsolicited publicity has been accorded to myself as well as to Dr. William Brown, in connection with the recent investigation in the National Laboratory of Psychical Research, S.W.7, I should like to be allowed to corroborate Dr. Brown's impressions and to add a few of my own.

Through the courtesy of the Director, Mr. Harry Price, I was invited to join a number of persons in the scientific investigation of the alleged paranormal powers of the Austrian medium, Rudi Schneider, who left London yesterday after a visit of three months. This young man specializes, if one may so say, in 'telekinesis', or the raising of solid objects from their places and the transporting of them elsewhere without the intervention of any human or other obvious agency. At an early date we satisfied ourselves of the genuineness of these phenomena—that is to say that this medium (and only when in trance) is possessed of some paranormal capacity or power to effect physical displacement of ordinary, but in some cases quite heavy, objects.

The explanation (if there is one) of these things is another and extremely difficult matter; nor is this the time to discuss it. I merely wish to say that I and my wife have on several occasions seen phenomena quite as remarkable as those testified to by Dr. Brown. Some of these have been—a four-legged table lifted up and thrown forward so violently that two of its legs were smashed off; a basket tugged out of my hand; a closed cigarette case pulled from an experimenter's hand and later flung open inside a large chest closed with a heavy lid; a cigarette floating through the air and then forced between my fingers. I have furthermore been touched on the right thumb under a red light by a white, diaphanous, rod-like homogeneous structure that felt cool and moist, a so-called 'pseudopod' or 'phantom'.

Out of 27 seances 18 have been blank,⁶⁵ not at all what one would expect of a fraudulent person. As Dr. Brown said, here indeed is material for further research, for at present we are very far from framing any satisfactory explanation or adequate theory. May I be permitted to say in the clearest manner possible that I am not a 'spiritualist'?

Yours, etc.,

D. Fraser-Harris

The Athenaeum May 7

To say that Harry Price was pleased by these two letters to the *Times* would be an understatement. As Mrs. Goldney wrote in a contemporary note, he was 'absolutely DELIGHTED'; he was indeed jubilant and triumphant and made no secret of his joy. When Professor N. Ach wrote to Price asking him whether he might make arrangements to investigate Rudi in Göttingen in September or October rather than June or July (as Price had apparently suggested) Price replied (HPL):

10 May 1932

. . . Rudi gave a few brilliant sittings during the last three months and has at last convinced Dr. William Brown, F.R.C.P., the distinguished psychologist, and Wilde Reader in Mental Philosophy at Oxford, that his phenomena are absolutely genuine. Dr. Brown declared his conviction in a most important interview published in the *Daily Mail* (enclosed) last week and also in a letter to the *Times* (enclosed). Last week saw the commencement of a new epoch in psychical research. . . .

He wrote in a similar vein to others, including Vater Schneider (HPL):

10 May 1932

. . . Rudi has absolutely convinced Dr. William Brown, the eminent psychologist—and this marks a new epoch in British Psychical Research. Rudi is

⁶⁵ Actually, not quite that number were totally negative, according to Price's *Bulletin IV* (footnote 10).

returning to the Laboratory in September and I am hoping to carry out some experiments in Oxford University. Several people are trying to get Rudi to London (including SPR people) but I have warned him it would be dangerous for the Laboratory, as there are several who are only too anxious to set a trap for him into which he might unknowingly fall. It is just possible that some of us may be in Braunau during the summer. . . .

Vater Schneider's reply was touching: he could not 'find words to express how delighted' he would be if Price were to come to Braunau, and added some friendly lines from Rudi and Mitzi.

Things had meanwhile not gone at all smoothly for Dr. William Brown. He was subjected to a good deal of ridicule at Oxford, notably by Professors Einstein and Lindemann (later Lord Cherwell), who simply laughed at the notion of physical phenomena. As Lord Charles Hope wrote to C. C. L. Gregory on 13 May 1932:

. . . I too think there is almost no doubt that Rudi can produce genuine paranormal phenomena. It is a great pity we could not have the proposed sittings, but I still hope it can be managed later on. You have, perhaps, seen Dr. William Brown's and Fraser-Harris's letters which appeared in the *Times* last week. I enclose the cuttings in case you did not. Will you please return them. Dr. Brown was very displeased at being 'starred' in the *Daily Mail* front page. I was sorry too but did not feel responsible, as for his series he went to Price's at P's invitation and not at mine.

Dr. Brown has since then been having rather a time of it at Oxford being laughed at by Lindemann and even Einstein among others. Of course they will not even hear of such phenomena being genuine. . . .⁶⁶

On 10 May Brown wrote to Price (HPL): 'I am rather sorry you referred to me so much (though I realise it was kindly meant) in the *Empire News* . . . and especially as you put the F.R.C.P. after my name—I fear the publicity, i.e. D.M. [Doctor of Medicine] too, will be a great hardship to me with doctors and scientists . . .'; but he was pleading in vain.

In fact Dr. Brown, in writing to the *Times* and by implication associating himself with Harry Price, had laid himself open to a good deal of merriment. Had Dr. Brown and his scientific colleagues stooped so far as to read the more sensationalist reports in the press during the first six months of 1932, they would have come across some very choice publicity concerning the doings of Harry Price in connection with a solemn *Walpurgisnacht* rite of black magic to be performed at midnight on the Brocken mountain in Germany, the object of which was to disprove the alleged superstition that, with suitable incantations and the intervention of a virgin, a goat would turn into a handsome young man. For example, the *Evening News* of 18 April 1932 (the date between the 22nd and the 23rd of Rudi's London seances) carried an article about a German official who was arranging a solemn black magic test at the top of the Brocken at the full moon and who had just arrived in London, doubtless to complete arrangements with the chief moving spirit of the enterprise, none other than Mr. Harry Price. 'He took a grave risk. Cockney humour is not very kind to professors [Price] who believe that an anointed he-goat, led by a maiden "pure in heart" will change to a "fair youth of surpassing beauty". But

⁶⁶ C. Hope to C. C. L. Gregory, 13 May 1932, in C. C. L. Gregory, 'Rudi Schneider—recollections and comments', *JSPR*, 39 (1958), 209–214.

perhaps a professor who leads goats by a silken cord up a mountainside in the light of the moon is proof even against Cockney humour'. The German press was even more scathing, and their special target too was the English savant, Herr Harry Price, who was purporting to take this affair with perfect solemnity. On 20 April 1932 the *Neue Berliner Zeitung*, in referring to the repeated delays of the Brocken celebrations, said in so many words that it seemed that the organisers were manifestly less concerned with celebrations of any sort than with a sensational spectacle that would doubtless be exploited accordingly.

The festivities eventually took place on 17 June and were duly described in *The Listener* for 6 July 1932 by Price himself, where he explained how, accompanied by 42 press photographers, 73 pressmen and a cinematographic set-up, he demonstrated to everyone's entire satisfaction that 'the ancient magic ritual' did not work; despite a mystic circle accurately designed in a mosaic that had been laid down near the granite altar, despite the scrupulous observance of the venerable ritual, despite the appearance of a maiden pure in heart in the person of Miss Urta Bohn and the use of a white kid specially chosen at birth, the goat remained a goat. Price concluded: 'The scoffers [who, one wonders?] will tell us that because we had no faith the experiment was not conclusive; in other words that the magic formula will not work automatically. That is all very well but what sort of state do we have to induce in order that the magical metamorphosis shall take place?'

This of course was weeks later than Dr. William Brown's first letter to the *Times*. But we do know that there was a great amount of uproarious advance publicity in the press over Mr. Price's proposed magical 'experiment' on the Brocken especially, as I have mentioned, in the German press. This publicity had evidently not reached Brown and Fraser-Harris; but I think it entirely possible that it had indeed come to the attention of Professors Einstein and Lindemann, whose native tongue was German. Just as Brown and Fraser-Harris were committing themselves to Rudi's mediumship in the *Times*, Mr. Price's Brocken stunt was receiving a climax of notoriety in the popular press.

Whether this *Walpurgisnacht*-mongering on Price's part was forcibly drawn to Dr. Brown's attention by Einstein or Lindemann, or whether Hope had a heart to heart talk with Brown on the subject of the desirability of conducting an independent investigation of Rudi, Dr. Brown wrote a second letter to the *Times* which was published on 14 May:

Sir, In my letter of last Saturday I endeavoured to give a concise, accurate account of a 'successful' seance with the young Austrian medium Rudi Schneider, at which I was present on Tuesday night, May 3rd. I briefly described the chief events that occurred and also my own mental reaction to them. At the time I had a feeling of conviction that the phenomena could not be accounted for in ordinary terms—in other words that they were supernatural. But intellectual conviction comes, if at all, only after reflection upon all the relevant circumstances, and this involves in its scope a very wide circle of facts both physical and psychological. Direct observation free from any inaccuracy may point imperiously in one direction and yet a full consideration of all the circumstances may indicate a very different interpretation. In psychical research, as in the investigation of crime, circumstantial evidence is of supreme importance.

My experiences are paralleled by those of Dr. D. Fraser-Harris and of all the other sitters (a large number) who were present at one or another of the

more successful seances, and I understand that their conviction of the genuineness of the phenomena was at least as strong as mine. Moreover, at the *Institut Métapsychique* in Paris the Director, Dr. Eugène Osty, held a large number of seances with Rudi last year, using infra-red rays and elaborate photo-electric receiving apparatus whereby he seems to have demonstrated absorption of such rays to the extent of 30 per cent and more by action at a distance under trance conditions. These interesting researches are described in *Les pouvoirs inconnus de l'esprit sur la matière* (Libraire Felix Alcan 1932). Similar results with infra-red rays have been obtained in one or two sittings here in London with less elaborate apparatus.

Nevertheless one is not relieved from the necessity of the closest scrutiny of all the circumstances of these researches, and during the past week my mind has hardly ceased from an active review of every conceivable aspect of the problem. The human element must be closely considered, the temperament, situation and motives of every person concerned, both here and abroad, so far as it is at all possible to discover them. The order of events, the conditions of the seance, certain observations which seemed insignificant at the time but which may prove important in the light of later events—these and many other considerations must play their part in helping one to come to a final verdict.

In the light of such considerations as these I am still unable to dispute the genuineness of the phenomena that I experienced. On the other hand extensive *lacunae* in my knowledge of this wider circle of facts prevent me from going sponsor for the phenomena in spite of their immediate impressiveness. Intellectual conviction can only come, if at all, after much more stringent scientific investigation carried out in a university laboratory or in the seance room of the S.P.R. with trained scientists and psychical researchers as sitters. In saying all this I am not depreciating the very important preliminary work that has already been done both here in England and on the Continent, but I am emphasizing the need of confirmatory evidence and of systematic verification. Further knowledge is required of the exact physiological and psychological nature of Rudi's trance state or so-called trance personality 'Olga'—whether genuine or spurious. An outstanding difficulty in this kind of research, distinguishing it from ordinary scientific research, is that one has to adapt oneself to the whims and preconceptions of the medium on pain of getting no 'phenomena'. One cannot dictate the conditions of the investigation, but has to submit to dictation from him or from his trance personality. Therefore indirect and hidden control of the investigation needs to be exceptionally stringent and far-seeing.

Whether genuine or spurious, Rudi Schneider's trance manifestations are worthy of the closest scientific study and will repay such study. I am, etc.

Oxford May 11

William Brown

Price was, to put it mildly, furious. He immediately wrote a very long letter to Brown, protesting against the latter's second *Times* letter:

May 14 1932

... When I opened the *Times* ... my appetite vanished! My first reaction was how Salter, Lord Charles Hope and the rest of the S.P.R. people (mad with jealousy at the success of the Laboratory) will chuckle at your reference to them. ... The sting of your letter ... is the fact that you say that seances at the S.P.R. would be convincing and, by implication, those at the Laboratory are open to suspicion. I think it a most unmerited slur on the Laboratory, and, being published in the *Times*, we shall be the laughing stock of the kingdom. ... the S.P.R. is thoroughly incompetent ... and a joke ... living on its traditions. As for the seance room, I should imagine that it is the worst equipped in existence.

Conversely, the only research work worthy of the name during the past 10 years has been done by the Laboratory. ...

I hope that Osty does not see your letter. He and his Sorbonne collaborators are, I am sure, under the impression that they have carried out a most 'stringent scientific investigation' . . .

I do not propose to publish any report of our last series of sittings with Rudi. It would cost £120 and would be a sheer waste of money after your remarks.

There is one statement in your letter which is incorrect. You mention the 'whims and preconceptions of the medium' and state that one 'has to submit to dictation from him'. This is not true. *Never once has Rudi 'dictated' or even suggested* (except on one occasion when he thought a black cloth on the floor would show up the 'pseudopods' better). *Conversely he has never questioned any control or experiment and has cheerfully obeyed every order. Osty mentioned this same compliance on the part of Rudi in a letter to me a few weeks ago.* Certainly 'Olga' dictates. . . .

. . . I expect you will say that I have misinterpreted your letter but as I . . . read it, its effect is to whitewash the S.P.R. at the expense of the National Laboratory. . . .⁶⁷

Dr. Brown's reply was brief and rather distant. In referring to the 'seance rooms of the S.P.R.' he was not advocating research under S.P.R. auspices but merely naming one neutral place among others: 'Rudi's phenomena if they prove what they seem to prove are of such transcendent importance that verification under the most stringent conditions on independent ground, both of seance room and of sitters, is imperative—if the scientific world is to be convinced—and I have merely stated the minimum conditions for this. No possible disparagement of your lab. or Osty's or anyone else's is suggested in the faintest degree . . .' (HPL).

Price lost no time in contacting Osty and promptly sent him Brown's second letter to the *Times* (which he had just piously hoped Osty would never see) with the comment: 'He is not very complimentary to your Paris experiments and regards them as being merely "preliminary" work to that which ought to be done in England . . .', adding that Brown was trying to ingratiate himself with the S.P.R. as he hoped to be that Society's next President (IMI). Osty reacted by asking Lord Charles Hope what Brown had against himself and Hope replied on 30 May 1932: 'It is not true that Dr. Brown is going to be the President of the SPR next year and it is not true that he has a poor opinion of your experiments' (IMI).

Price also now started a string of letters (in HPL) to Vater Schneider warning him most urgently against sending Rudi to the S.P.R.:

27 May 1932

. . . it would be absolute madness for Rudi to go to this Society . . . I am very grateful, dear Vater Schneider, for all your kind words concerning our Laboratory, our methods and myself. We have always had particularly good successes with Rudi and have without doubt had far better results than Dr. Osty ever succeeded in obtaining in Paris. . . . Dr. William Brown wrote to the *Times* that he would have been more convinced if he had seen the phenomena in the seance room of the S.P.R. He hopes to become the President of the S.P.R. next year; and this explains his great interest in that Society. . . .

There is also a letter to Rudi marked 'personal and strictly confidential' (*privat und streng vertraulich*) dated 27 May 1932 (HPL), containing a solemn warning against having anything whatever to do with either the SPR or with

⁶⁷ In HPL. All italics in this passage are added by me.

Dr. William Brown: 'You will regret it all your life'—a thinly veiled threat.

In June *Nature* published an unsigned review of Osty's work with Rudi, which was a somewhat oblique attack; as Lord Charles Hope put it in a letter to Osty (IMI):

1 July 1932

. . . you will notice that the review, which is unsigned, attacks Rudi's early reputation and not actually your report. I think that is, in a way, a compliment to your report. . . .

There are several scientists who regret the tone of this review in *Nature* and consider it unfair but you will understand it is very difficult to start a controversy in that journal without referring to sittings held by Mr. Price and unfortunately Mr. Price has not improved his position in scientific circles by his journey to the Brocken.

As I have already told you, satisfactory results were obtained in London with the infra-red apparatus completely covered in a gauze box and this experiment was repeated on several occasions. The gauze box (and apparatus complete) was made by two friends of mine, one of whom is a scientist, and was brought in one evening just before a 'sitting'. It was successfully worked by 'Olga-Rudi' which has duly impressed most people who have heard of the experiment and unfortunately it would be difficult for several reasons for the scientist in question to write to *Nature* on this subject. . . .

The scientist in question was C. C. L. Gregory, and his major reason for not wishing to commit himself in *Nature* was a feeling of uneasiness about the reliability of Harry Price. Price, on the other hand, wrote a long and indignant letter (in HPL) to the Editor of *Nature*:

27 July 1932

. . . It is absolutely untrue that the scientists whom I invited 'were merely privileged spectators of phenomena produced under conditions over which they had little control and in circumstances where accuracy of observation was almost impossible'. The very best phenomena, including those indicated by the infra-red installation, were witnessed when the seances were under the sole control of Professor D. F. Fraser-Harris and Mr. C. J. Gregory [*sic*] (of the Mill Hill Observatory) and remained under their care and direction at every seance at which it was used. . . .

. . . It is utterly false to state that the gentleman named had been 'invited' to witness phenomena. They were *responsible* for the conditions under which the phenomena were witnessed and for the control of the medium. . . .

It does not seem to have occurred to Price that the scientists were entitled to decide for themselves under just what conditions they were willing to be held responsible for the results of the investigations. The Editor of *Nature*, Sir Richard Gregory, declined to publish the letter and returned it. Had he retained it, he might have been wryly amused to compare it with another letter which he was to receive from Mr. Price about a year later, making a completely conflicting set of assertions about the same set of events.

The point raised by Price as to the status of the sitters is an interesting and important one. In one sense it was quite true that sitters were given an opportunity to control the medium and to satisfy themselves as to conditions and, in the case of the infra-red apparatus, to introduce their own equipment into the room. On the other hand, Price never tired of pointing out to them that they were his 'guests', they were enjoying his hospitality, were indebted to him for unique experiences, and were under an obligation to him to confine

their activities as regards the medium within strictly delimited bounds. He absolutely declined to allow them to investigate the medium separately from himself, although it is true that he had, as he stressed over and over again, absented himself on a few isolated occasions at his own discretion so that they had the medium to themselves in his Laboratory: his absences were without warning, and on his own terms. Also, he interfered with and dismantled apparatus without consulting or informing anyone.

The next development was a lengthy and heated exchange of letters between Price and Dr. William Brown centering around the question whether Brown had or had not 'solicited' the publicity that accrued from his letter to the *Times* and various interviews he had given to the Press. Price's chief arguments were that Brown had actually rung up the *Daily Mail* and had been heard to observe that he would not have missed the seance with Rudi of 3 May for £1000. Brown maintained on 10 September that he never took the initiative but had merely, out of gratitude to Price, offered to add his testimony to that of Price: 'I fear you have a complex about the S.P.R.', wrote Brown, not perhaps unreasonably (HPL). Price's reaction was an immensely long letter, five pages of single-spaced typing dated 20 September 1932, entirely devoted to an attempt to prove that Brown had in fact 'solicited' publicity at various times.

Price's determination to prove that Brown was seeking press publicity was perhaps largely due to the fact that Price's own penchant for such publicity was being used as an argument why he should be excluded from the next investigation of Rudi. One cannot help feeling also that Price must have derived much joy from taxing Dr. Brown of all people with avidity for notoriety. Brown emerges from his letters as peculiarly conscious of his medical dignity and fearful of academic sneers, and had actually begged Price, for instance on 10 May 1932, to go easy on press publicity in hand-written letters, copies of which Dr. Brown himself had evidently not kept (HPL).

What seems to have happened is that Dr. Brown was indeed carried away after the seance of 3 May 1932 which had led him to write to the *Times*. However, I have no doubt whatever that the idea of ringing up the *Daily Mail* in the middle of the night from Price's Laboratory after that seance came from Price and not from Brown: the *Daily Mail* was the paper of Sutton, Price's old enemy who, he knew perfectly well, harboured against him the darkest suspicions as regards Borley rectory, and it was the *Daily Mail* that had hinted that officers of the National Laboratory might have faked Rudi's phenomena.⁶⁸

Dr. Brown, in his admitted initial state of excitement at a major scientific change of heart, was doubtless quite ready to fall in with Price's suggestion to ring up the *Daily Mail* to testify to his belief that he had observed genuine paranormal phenomena. Unlike Brown, Price was perfectly aware of that most reliable of all parapsychological phenomena, the inconstancy of conviction, and it was important for him to get Brown to commit himself as rapidly as possible, to strike while the iron was hot. Brown rose to the bait—only to escape by adding a cautionary footnote to his initial enthusiasm. The more furious Price became with Brown for second thoughts and hedging, the more

⁶⁸ R. Lambert to H. Price, 10 May 1932, letter in HPL.

certain Brown grew that second thoughts and hedging were justified and that Price would not be a suitable colleague for a future collaboration; and the more cautious and reserved Brown became, the angrier Price grew . . . and so on, until the two men reached a veritable crescendo of mutual detestation.

10. Price's campaign against Eugène Osty

On 25 May 1932 Lord Charles Hope had approached Osty and asked him to give the Myers Memorial Lecture to the Society for Psychical Research the following year. At the same time he offered him £150 for completing his Rudi investigation. These negotiations eventually came to nothing because Rudi's powers had waned to such an extent that experiments in Paris were abandoned. However, Osty did give the Myers Memorial Lecture in 1933, 'The supernormal aspects of energy and matter',⁶⁹ and his cordial relations with the Society for Psychical Research cannot have been a secret from Harry Price.

In the summer of 1932 the first whisperings against Osty began. C. W. Olliver, author of 'L'extension de la conscience', wrote an undated letter to Professor Charles Richet (who had contributed a preface to this book) saying that he had heard rumours in London against Osty's experiments with Rudi Schneider and in particular concerning the presence of a possible accomplice which had not been mentioned in the report. Richet forwarded the letter (in IMI) to Osty. Osty wrote a long and detailed reply, dated 23 September 1932 (IMI). All accusations against his work, Osty said, emanated from one source, Mr. Harry Price, and were twofold: (1) that witnesses were not given by name; and (2) that an accomplice ('une complice'—feminine) was present at sittings who had not been mentioned in Osty's report. As regards (1), Osty said he had deliberately not cited witnesses as he wished to break with the method of witnessed sittings since in his view this prevented parapsychology from becoming a proper science. The rest of the letter is concerned with (2).

Altogether, Osty writes, he had over 90 sittings with Rudi. During the 14th of these, on 19 November 1930, he first observed occultations of the infra-red. On 19 May 1931, at the 51st seance, '*our experiments having already yielded everything we have published*', Miss Mitzi Mängl, Rudi's fiancée, a young girl of 18 who had arrived two days previously, attended a seance for the first time. Rudi was exhausted and was about to return to Braunau for a rest; his fiancée was invited out of politeness to Rudi. Her presence was entirely harmless in the given control conditions and coincided with a series of seances that were at first virtually and then wholly negative. From 27 May to 8 July not even the feeblest absorption of the infra-red was registered, and this was the very last sitting when Rudi was due to return:

It happened at this sitting the 62nd, that a sitter in the first row said he had been touched on the knee as if by a hand, a thing which had never happened before. Rudi in trance, who was asked what this meant, answered [speaking as 'Olga'] that the phenomenon had nothing to do with him.

A moment later my secretary, who was sitting next to me, near the instrument cupboard where I was watching the galvanometric spots, felt a hand touching her knee. It was Miss Mängl's hand. Miss Mängl was sitting between us, behind some of the other sitters, and it would have been a physical

⁶⁹ E. Osty, *Supernormal aspects of energy and matter* (Frederic W. H. Myers Memorial Lecture; 1933, London: Society for Psychical Research).

impossibility for her to get to the scene of action of the mediumistic force where, besides, she would immediately have released a photograph of her presence, if she had got that far—through the first row of sitters and the veil separating these from the apparatus; and this without mentioning the total occultations which I should have observed at the galvanometer spots. . . .

We discussed the incident after the seance. It was not totally certain that Miss Mängl had wished to simulate phenomena, and it was in any case unimportant since we only took note of what happened in the part of the room that was inaccessible [*inviolable*]. But we were irritated by the idea that there might have been some naive idea to make us believe in a mediumistic phenomenon.

We thought perhaps the young girl might have had the wish to prolong her stay in Paris and, seeing her fiancée was exhausted, might have had the idea of making those present believe in a phenomenon in order to encourage us to continue seances. In our state of ignorance we decided not to say anything to Rudi so as not to endanger the affection he felt for the young girl.

There were four more sittings during which Miss Mängl was severely watched. Then she left Paris with her fiancé on July 17th.

This is the story of the 'accomplice' if one can call it that.

I have not mentioned the incident because it was entirely irrelevant to our results . . . Rudi returned, alone as we had wished, in October 1931. We had another 24 sittings until and including December during which we attempted to verify precisely the principal results which we had previously made, especially as regards the synchronisation of the oscillations with Rudi's respiration.

Before, during and after the brief interlude of his fiancée, Rudi produced identical phenomena, absolutely incapable of simulation in the conditions in which the experiments were conducted. . . .

Knowing that Mr. Price did not have at his disposal serious control devices, I thought it my duty to write to him letters, copies of which I enclose. . . .

Osty added copies of his letters of 15 and 22 January, which contain the sentence: 'when there are no phenomena she tries to produce some and does so' (IMI). He continued:

And what has Mr. Harry Price done with my confidential communication, designed to safeguard Rudi's self-respect and to ensure that he should not refuse to be further investigated? Mr. Harry Price invited Mlle Mängl to come to London with her fiancé. He let her assist at every seance in his laboratory entirely devoid of serious control precautions. He said nothing whatever of this information to any of his co-investigators (my communication was confidential only as regards Rudi). And then he goes and plays about with my confidential letters in order to get people to believe that Miss Mängl has been present at all our sittings and that she has produced *the very phenomena* we were investigating. . . .

Osty was distinctly displeased. He continued about Price's 'satanic behaviour' and obvious determination to bring about the moral ruin of his, Osty's work.

Thus Price had indeed performed a complete *volte face*: instead of treating Osty's work as corroborating his own and rendering superfluous another investigation (as he had done over and over again in May, June and July 1932 in order to persuade all concerned that there was not the slightest need for a fresh investigation) he now, from August 1932 onwards, took the line that Osty's work was totally worthless because he had suppressed the presence at seances of an accomplice. From the sequence of letters it is entirely clear that this change of tactics occurred immediately Price realised that he had

failed to persuade Osty to have nothing to do with the Society for Psychical Research.

One can imagine Osty's feelings readily enough—Price had indeed put his friendly warning to monstrous use. Worse still, Price, knowing what he did, had allowed Mitzi to be present at every single one of the Third Series of sittings in London! As every investigator before Price has confirmed, and as Price had assured every critic over and over again who accused him of letting the medium dictate to him, Rudi never made any conditions whatever as regards sittings.⁷⁰ But Price had letters in his possession in which Rudi had refused to come to *London* without Mitzi, and it was quite easy to convey a very unfavourable interpretation of this refusal by omitting the relevant circumstances.

Moreover, Osty's warning had been kept from his co-investigators who—or so at least Price had assured the Editor of *Nature* when it suited him—were fully as responsible as he himself for the control conditions and conduct of experiments. Indeed, Price was much nearer the mark when he treated his sitters as guests, occasionally privileged to make certain tests at his own discretion. If Osty's work was not 'worth the paper it was written on', as Price was to put it later, because of the presence of an unacknowledged accomplice, what of his own Third Series, at which this subject of suspicion was present at every sitting? But Price, in his passionate hatred for his enemies, was willing to sacrifice his own sittings, provided this would bring to nought his rivals' efforts also.

Osty, then, had indeed reason for annoyance. But there is perhaps room for a third point of view. From Osty's own account as given to M. Olliver, there really was no case against Mitzi. There were perhaps some slight grounds for suspicion, but nothing had taken place which entitled Osty to write 'when there are no phenomena she tries to produce some and does so'. At most he might have been justified in uttering a word of caution. Price's conduct in allowing Mitzi to attend all sittings and in keeping the information from his fellow researchers is impossible to defend. But what right had Osty to blacken Mitzi's character in so uncompromising a manner on virtually no evidence at a time when he no longer entertained any regard for Price's judgment or probity on account of the Duncan 'teleplasm' affair?

We know from numerous accounts of other sittings in Braunau, in Munich, in London and elsewhere that sensations of being touched by a hand were indeed quite common phenomena at Rudi seances, and were not always readily explicable by a handy accomplice; and we also know that at least on some occasions another part-personality, 'Anton', said he was responsible for these sensations when 'Olga' was ostensibly not there.⁷¹ It may therefore well be that during the Paris sittings some phenomena took place which were new to that setting.

Osty's letter to Price about Mitzi shows up a very important difficulty that has always haunted psychical research. There is a natural tendency for an investigator to consider himself as being somehow in a different and superior class from that of the medium, and as therefore being under no obligation to display towards the medium or his friends the same standard of conduct by

⁷⁰ For example, H. Price to W. Brown, 14 May 1932 (HPL).

⁷¹ A. von Schrenck-Notzing (footnote 7), 136–137.

which he might feel bound towards his other fellow human beings. (Psychical researchers tend, equally naturally, to forget that once they become convinced of the reality of a medium's phenomena, so far as the outside world is concerned, they themselves, the investigators, are classed along with the medium.) So Osty thought of Price as a colleague, and as such in a privileged position *vis-à-vis* the medium and his fiancée. It probably never occurred to him that there was anything improper or unjust about making a categorical accusation of fraud against a young girl on virtually no evidence—and that to Price of all people, who had just published an article on 'teleplasm' which, the latter now asserted, was a hoax designed to lure another medium, Mrs. Helen Duncan, to confide in him! Certainly Osty paid very dearly for his rash accusation, even though he immediately reassured Price that Rudi had never occasioned the slightest suspicion.

11. Price's policy towards members of the National Laboratory between 28 April 1932 and 5 March 1933

The rumours concerning Osty's work began to circulate in August 1932. The double-exposure photograph, on the other hand, seems to have been kept a tight secret right through until Price was ready to publish his denunciation of Rudi in March 1933. There is a letter from Professor Fraser-Harris to Price dated 12 July 1932 (in HPL) mentioning some photographs, from which it seems quite clear that no suspicion had crossed Fraser-Harris's mind that Price might turn his championship of Rudi into hostility:

Don't you think it would be a good thing if some of us met and discussed the photographs taken during the recent sittings? I, for one, have never studied them, and only glanced at them in artificial light casually before one or two sittings. Certain appearances are in need of interpretation and I think there ought to be a 'round table conference' (as fashionable nowadays) to come to some conclusion about what the photographs show. Something of this sort is necessary before they are incorporated in any report of the sittings. . . .

Fraser-Harris went on to discuss some points arising out of the anonymous review in *Nature*. He reassured Price that he himself had written to the Editor of that journal to the effect that the phenomena were worthy of the notice of 'official science'; however, he reproached Price for not having let him know earlier that there had been any previous controversy about the genuineness of the Schneiders, as there had been in *Nature* some allusion to previous attacks, notably Vinton's.

Price replied curtly and by return of post (HPL):

13 July 1932

. . . Yes, do come along and examine the Rudi photographs. There are only fairly rough prints of them but they are quite clear. I doubt whether it would be of any use having a conference about the pictures, as they are here for anyone who cares to examine them.

There has not been a shred of evidence published that Rudi ever cheated. The two cases cited in *Nature* were, I think, merely theories or general assumptions. The same with Vinton's article. . . .

This is certainly not the kind of letter one might have expected from a man who had, as he subsequently alleged, obtained during the previous May incontestable photographic evidence that Rudi had cheated during a seance,

and who was persuaded that all Rudi's phenomena during the Third Series should be dismissed as suspect! Fraser-Harris does not seem to have pursued the matter. Apparently Price let it be supposed that he would never publish a report of the Third Series,⁷² and no one was ever allowed during his lifetime to see his negatives. The tenor of Price's letter, especially the sentence 'there has not been a shred of evidence published that Rudi ever cheated', must have confirmed Fraser-Harris in the impression that Price was vouching for Rudi's phenomena and for his complete integrity, and that he was sulking because the scientists wanted an independent check.

A further incident must have completely misled the world of psychical research as regards Price's eventual line of action. Professor Nils von Hofsten, who had been present at two of Price's 1929 sittings, wrote to Price on 9 May 1932 (HPL):

During my visit to London in January 1930 I wrote a report of my two sittings with Rudi Schneider. You know that I felt very sceptical already after the first sitting. I should have liked some further experiments but Rudi Schneider did not return to London until recently, perhaps, other duties and interests filled my time and my notes remained in a drawer. Finally I took them out and wrote them in English with a few additions. I didn't know where to publish such a non-believing account, but sent it at last to America. Now I have been told that it will be published very soon. I wish to inform you; I prefer that you would know it from me. . . .

In point of fact, Professor von Hofsten (like Price himself after the 25th seance, on 28 April) had at the time given no indication whatsoever of his scepticism. Furthermore, he had written a letter dated 16 October 1931 to Rudi in which he said that he found Rudi employing quite normal means and had proof; he offered Rudi to go easy on him if he would own up as to just how he performed his tricks (HPL). Rudi had sent the letter to Price in London. Price had taken no cognisance of von Hofsten's letter to Rudi, but he did reply to that of 9 May 1932 in a long and quite furious epistle (in HPL) which makes ironic reading, knowing what we do about Price's own subsequent conduct:

May 12th 1932

. . . I am astounded that you should wait two and a half years before launching an attack on Rudi Schneider. Why did you not say you were dissatisfied during or after the seances? You reiterated to me that you were very much impressed and said something similar to me in your letter of January 22nd 1930. During the seances themselves you were *enthusiastic* about the phenomena and among other exclamations (as recorded in our official protocol) you remarked during the phenomena 'aber Olga, es ist undenkbar; es ist wunderschön: ich bin overwhelmed as you say in English'. . . . What did you mean by all that enthusiasm if you were not impressed?

Do you seriously think that your opinion, voiced after a lapse of two and a half years, will have the slightest weight with scientists anywhere in the world, after Dr. Osty's 15 months of experimenting with Rudi in Paris, and our own three series of very carefully planned experiments? Your opinion will not be worth the paper it is written on. . . .

Do you seriously consider that anyone will believe that you had 'proofs' of Rudi's 'fraud' and kept silent for two and a half years without saying a word about it? . . .

. . . Do you not think it a terrible thing for you to *pretend* that you have found out something about Rudi in order to frighten him into a 'confession'?

⁷² For example, in H. Price to W. Brown (footnote 70).

You continue: 'I should do my best to explain matters in such a way that you would not be judged too severely'. In other words, having induced Rudi to 'confess', as a reward you would do your best to 'explain the matter'. . . . Do you not think that it is a damnable proposition to put to a youth, and do you not think that you ought to be thoroughly ashamed of yourself? . . .

I have much pleasure in removing your name from our list of Foreign Correspondents . . .

This virtuously indignant letter was dated 12 May 1932—a fortnight *after* Harry Price subsequently alleged he was already convinced that Rudi was dishonest, and that probably the entire Third Series was fraudulent. How could anyone have supposed for one moment that in late April and early May 1932 Price himself was planning, with a time lag of nearly a year, to expose Rudi as a fraud, having in the meanwhile written him and his family the most amicable and affectionate letters?

There were in London at least two persons in addition to Price who knew about the double-exposure photograph before its publication. They were his secretary, Miss Ethel Beenham, and Mrs. K. M. Goldney, at the time joint Honorary Treasurer of the National Laboratory of Psychical Research, subsequently Organising Secretary of the Society for Psychical Research and one of the leading personalities in modern psychical research. At some date which she cannot now precisely recall but some time after 28 April 1932, when Mrs. Goldney visited Price at the National Laboratory she found him in a state of pleasurable excitement: he said he had a secret to tell her but she must promise not to tell anyone else. She guessed that he was alluding to a proposed visit to the National Laboratory by the physical medium Carlos Mirabelli; and, knowing how happy Price always was to spring publicity surprises upon the world, she promised. He said—jocularly, she thought—'word of honour?', and Mrs. Goldney, in the same spirit, gave her word and crossed her heart. Price then turned to Ethel Beenham and said: 'Now you are my witness, Ethel, she has sworn that she won't tell'. Harry Price thereupon showed her the double exposure photograph of Rudi saying something like 'we caught him'; Price told her he would publish the photograph if and when it suited *him*, and that it would serve the investigators right who were trying to make an independent investigation of Rudi; he would show them who was the more astute.

Mrs. Goldney tells me that she was horrified, begged to be released from her promise and insisted that Price should immediately call a meeting of his Council and put the full facts before them. However, Price held her to her 'word of honour'. She agreed most reluctantly to keep silent since she had given her promise. Mrs. Goldney says that later on, after much discussion with Lord Charles Hope, she came to doubt the rightness of her decision at the time to keep silent. So far as she knows, she was the sole recipient of this information and she continued to press unavailingly at intervals for a full disclosure. However, at this period Mrs. Goldney was under heavy pressure, working at Queen Charlotte's Hospital, and had few opportunities for seeing Price.

Many years later, after Harry Price's death in 1948, Mrs. Goldney had an interview with Miss Beenham in which the subject of the double-exposure was raised. Miss Beenham, who admitted that her memory was not very precise,

after all these years, stated that she herself never saw anything suspicious in connection with Price or Rudi. The following relevant extract from Miss Beenham's testimony concerning Price, which is preserved in the archives of the Society for Psychical Research, is copied from Mrs. Goldney's copy:

Coming to the photograph showing Rudi's arm free, I arrived at the office and can distinctly remember Mr. Price was in the darkroom developing the photograph and called me in. When the negative showing the freed arm was before us, we both were so astonished that we were speechless. Certainly Mr. Price behaved as if he were amazed and incredulous, as I was. Hardly had we time to discuss the possible implications before Mitzi and Rudi arrived. Mr. Price spoke some very halting German and I had been studying at the Berlitz School of Languages and spoke definitely better than Mr. Price but certainly not fluently. Between us we made Rudi understand the photograph; he suggested it must be a spirit arm (it seems he was as surprised and amazed as we had been) but Mr. Price or I pointed out it could not be because it was in our own pyjama jacket. I was embarrassed naturally, as Rudi was such a nice boy that we all liked, and he left with Mr. Price saying we must continue with the sittings. No definite accusation of fraud was made in so many words.

Mr. Price forbade me to say a word about it to anybody until he had thought it over . . . I cannot remember how long Mr. Price kept silence, whether weeks or months—without reference to the report or other papers. But I know that he intended issuing this accusation in spite against Lord Charles Hope who was having further experiments with Rudi to Mr. Price's annoyance. He regarded it as Lord Charles almost 'stealing' the medium he had brought over, paid for, etc. I was most embarrassed and uncomfortable, knowing this to be a fact, but could do nothing. I very much wanted, I remember, to tell Mrs. Goldney about it, but he held me to silence. Mrs. Goldney reminded me [at the interview after Price's death] that he eventually told her himself. He was terribly spiteful against Lord Charles and he was waiting for a chance to hit back at him and used this report for that purpose.

I don't remember his making remarks against Rudi all this time.

Lord Charles Hope, in a letter to Mr. H. W. Salter of the Society for Psychical Research wrote after Price had sprung his accusation:

May 5 1933

As regards Miss Beenham, I quite see a written statement *ought* to be made by her but the question is how to get it. . . . If you think it any good I could write to Mrs. Goldney. She said Miss B. let out that Price had told Rudi at the time that the matter of the photographs was 'of no importance' and that is what Rudi says he said. But Miss B. is hardly likely to repeat that in writing. . . .⁷³

12. Price's denunciation, and its repercussions

In the autumn of 1932 Rudi came to London for the Hope-Rayleigh experiments and remained until the early spring. This was the investigation that Price had moved heaven and earth to prevent—and failed. However, if he intended to *eclipse* it he certainly succeeded. By his article in the *Sunday Dispatch* of 5 March 1933 and the subsequent publication of his *Bulletin IV of the National Laboratory of Psychical Research* containing the double-exposure, purporting to be a photographic demonstration of fraud,

⁷³ C. Hope to W. H. Salter, 5 May 1933 (SPR files, reference S3).

coupled with his tireless energy as a letter writer, he certainly stole the scene (see page 16 in section 7 above, and figures 18 and 19 in section 13 below). His intentions were quite manifest: to deprive both the Osty experiments and the Hope-Rayleigh investigations of scientific certitude and of that quiet respectability at which they aimed.

Lord Charles Hope was in France on holiday when the scandal broke. He wrote at once to Price, requesting an explanation (HPL):

10 March 1933

I was interested yesterday to receive a cutting from a friend out of the *Sunday Dispatch* and learn that you claim to have a photograph taken during a sitting showing Rudi S. with an arm free (while you yourself were controlling him). The article also quotes you as saying it is your opinion that he was, on that occasion at least, achieving his phenomena by fraudulent means.

I shall be interested to hear from you who are the 'we' referred to several times in the article as having made this discovery—whether it is meant to refer to your 'Council' or to whom? Also, I would like to know why you did not tell all the Council of your suspicions, especially I should have thought those you asked to subscribe the cost of bringing out the Report. . . .

Also it is pertinent to ask you whether you proposed to collect donations towards the cost of the proposed further visit of Rudi to the N.L. of P.R. last autumn without informing those supporting the scheme financially of your 'discovery'. Certainly Evelyn and I were invited by you to join in that scheme and you never mentioned the existence of any such photograph or such a suspicion in your mind. . . .

Price's reaction, in a letter (in HPL) dated 13 March 1933, was that he had indeed informed those members of the Council who had been 'loyal' to him and that the initiative for donations had not come from him. 'I do not know why such a fuss is being made over these pictures . . . '.

Of course Price knew, no one better, why such a 'fuss' was being made over the pictures. Once he published his *Bulletin IV* and the *Sunday Dispatch* article, he took the line that all investigations of Rudi other than his own 1929 series had been invalidated by the discovery that Rudi 'could' evade hand control; that Osty's work was worthless because there had been an 'accomplice' and because Osty had not used the electric control; that the Hope-Rayleigh investigation, the results of which were about to be published, was worthless because there had been no electrical control; and that Drs. Fraser-Harris and William Brown had been extremely foolish to rush into print and vouch for Rudi's phenomena when he, Price, had been far more cautious.

Perhaps the letter that most clearly conveys the stance now taken up by Price is that to the Editor of *Nature* of 6 May 1933. After claiming that Rudi had point blank refused to sit without his 'accomplice', he continued:

It had been arranged to hold a series of sittings during the summer of 1931 but negotiations broke down because we refused to allow the 'confederate' (a close friend of Rudi's) to accompany him. When we found that Rudi would not visit the laboratory without his friend, we consented to have her but—after Dr. Osty's experiences—special precautions were taken to keep her under close observation during the seances.

Although Dr. Osty now admits that he detected Rudi's friend producing the 'phenomena' herself, his report contains no mention of this and the fact that she was present at any experiment is not recorded in the account prepared for the public.

Stress has been laid on the fact that our cameras caught Mr. Schneider evading control, but so long ago as 1924 Professors Meyer and Przibram detected Rudi producing 'telekinetic' movement of objects by means of a freed arm. That he *can* free an arm from the usual tactual control (and without the controller's being aware of the fact) has now been proved by our photographic evidence.

Mr. Schneider has promised to visit us during the coming autumn. . . .

Unlike Price's earlier defence of Rudi in which he had stood by Osty, and which the Editor had returned to him, this letter *was* duly published in *Nature*. It is of course a tissue of *suggestio falsii*: Dr. Osty did not 'now' (that is, May 1933) after Price's supposed exposure 'admit' that there had been a 'confederate'; it was Osty who had warned Price in January 1932 to take care that Mitzi might try to reproduce the phenomena. This friendly warning was the only communication that Price ever had from Osty on the subject, and it had certainly been in his possession for six months when he defended Osty to the Editor of *Nature* in July 1932. Four months after Osty's warning (14 May 1932) Price had in writing assured Dr. William Brown that Rudi had never dictated any conditions whatsoever, and on 13 July 1932 Price had reassured Fraser-Harris that none of the earlier 'exposures' alluded to in the *Nature* article amounted to anything. Price indeed had known about the Meyer-Przibram affair at least as early as 1927, when his own house journal, the *British journal of psychical research*, volume 1, March/April 1927, No. 6, had contained an article by Professor Hans Thirring who had dismissed the incident as not proving anything because 'at least one of the controllers was in the plot' (page 171); it had been precisely Thirring's point that such evasion of control was impossible without the connivance of the controller. Moreover, although Rudi had indeed refused to come to London without his fiancée after she had been invited, there is absolutely no reason to suppose that Rudi had insisted on having her at *sittings*—a rather significant difference. Mrs. Goldney wrote a note in 1933 after a visit to the National Laboratory at which she talked to Ethel Beenham

whom I knew very well indeed and with whom I visited Rudi in Braunau (his home). I asked her what was meant by the statement in the letter [to *Nature*] that Rudi had refused point-blank to sit without his 'confederate' being present, and whether this was the truth. Miss Beenham stated emphatically that it was not true, her words being approximately 'no, of course it's not true—I can't imagine why he goes and says such things; I'm sure Rudi would never refuse such a thing. He always consents to anything'. . . . Then Mr. Price walked in and I asked HIM. He at once said 'Certainly' he refused point blank and turned to the secretary and said, 'Didn't he, Ethel?' implying that she should corroborate his statement. Ethel remained silent and Mr. Price hurried on to other statements.

I should say that I had occasion shortly before to refer to something of this sort and THEN Mr. Price said 'that he had begun to ask Rudi to sit without his fiancée, Mitzi (the 'confederate') being present, but that Rudi had not hopped at it and he had therefore dropped the subject at once, as he did not wish to put him out of humour about anything. . . .'⁷⁴

One can see of course that once Price was forced to admit, however obliquely, that Osty had warned him of Mitzi *before* Price's own 1932 sittings the only possible explanation why Mitzi should have been present at every

⁷⁴ K. M. Goldney (footnote 58).

single one of his own sittings that did not reflect grave discredit upon Price himself was the allegation that the medium had refused to give seances without her. Nor did Price ever offer any explanation for failing to tell his fellow investigators (according to Price, co-responsible for control conditions with him) that he had had a warning about Mitzi and that special attention must therefore be paid to her, and that Rudi was supposedly insisting upon having her at sittings despite Price's alleged protests. There are, however, numerous letters in which Price endeavoured to explain why he had kept the supposed evidence of Rudi's fraud to himself since 28 April 1932. Perhaps the most telling one is addressed to the Hon. Everard Feilding (HPL):

July 3 1933

... You mention that you had to say that you could not understand why I did not show those incriminating photographs to my colleagues. But that is exactly what I did do—to those colleagues who were loyal.

I do not suppose that you are aware that Hope, Fraser-Harris, Evelyn and Cochran-Baillie were meeting Rudi in secret before he had been many weeks in the Laboratory. I discovered the 'plot' and there was a terrific row—naturally! When I discovered these photographs I was not on speaking terms with Hope; Dr. Brown had played me a dirty trick in publishing that second letter in the *Times* and I had fallen out with him. Fraser-Harris and Cochran-Baillie and Evelyn were so hand-in-glove with Hope that I dared not show the photographs to them if I did not want Hope to know about them. My relations with those members of the Council whom I have mentioned were such that I was under no obligation to tell them anything. But all the other members of the Council saw the pictures; I also told you about them; Sudre and Dingwall saw them as far back as last October—so there was no secret about the pictures. But as I know that Hope and his friends were doing their best to get Rudi away from us, I did not see (especially as we had come to the end of our experiments) that I should throw all my cards on the table for their benefit. . . .

That Price was not on speaking terms with Charles Hope when the pictures were allegedly 'discovered' by him on 29 April 1932 is undoubtedly true; the fracas at the Council meeting, which seems indeed to have been memorable, took place on 26 April. But he was on the most excellent terms at that point with Dr. William Brown. On 5 May 1932 (certainly well and truly after 29 April, when he had supposedly developed and printed the famous double exposure) he had written to Fraser-Harris that Brown's opinion was worth a dozen of that of Lord Charles Hope and his scientific friends. Dr. Brown's *first* letter to the *Times* came out on 7 May, and Price was 'absolutely delighted' and spoke and wrote to that effect to a large number of persons, even that this letter to the *Times* inaugurated 'a new epoch in psychical research'.

William Brown's second letter to the *Times* did not come out until 14 May after Rudi, whom Price had supposedly taxed with fraud on 29 April, had returned to Austria. The question is not why Price failed to show his discovery *after 14 May*, when Brown had 'played him a dirty trick' by counselling caution towards the phenomena in the pages of the *Times*, but *before 3 May*, when Brown committed himself to the press. Indeed, had Price really been in possession of evidence of fraud when Brown wrote his second letter to the *Times*, his proper course of action would of course have been to congratulate

Brown on his insightful addendum and to show him his 'proof of fraud' instead of abusing him and calling his counsels of caution 'a dirty trick'.

It is entirely plain that on 14 May, and later on, Price took the line that Rudi was wholly genuine and that fraud was out of the question; and it is equally plain that some time, at the very latest between 28 April (the fateful seance) and 8 May (when Rudi left London) Price secretly laid the foundations for an accusation of fraud—the 'cards' he would throw on the table when it suited *him*. This much is quite certain, since we know that he showed Rudi and Mitzi and Ethel Beenham a photograph of a double exposure before Rudi left England. If a particularly discreditable interpretation of his unquestionably dishonest behaviour is accepted, and this will be discussed below, then he must have begun to lay his foundations months earlier, and his conduct throughout the Third Series was a prolonged set of double dealings.

It is not at all hard to imagine the feelings of Dr. William Brown when he read on page 173 of *Bulletin IV of the National Laboratory of Psychical Research* that the seance of 3 May 1932 'received considerable publicity on account of the various reports which appeared. This publicity was not initiated by any officer of that Laboratory and I [Price] rather deplored it on account of what had happened at the previous sitting and also because several inaccuracies were published in the Press'. The only thing that Price *had* deplored was Brown's 'dirty trick' in qualifying his initial statement to the press, and the only press statement he had said was false was Brown's statement that the medium dictated conditions. It is little wonder that Brown eventually wrote to Hope on 21 March 1933: '... I have written formally asking that my name be removed from the Research 'Group' of the National Laboratory of Psychical Research and also from the bulletin and all other publications, but I've received no reply. I should have done this months ago, but couldn't bring myself to communicate with the man'.⁷⁵

Numerous other resignations followed. On 16 March 1933 the following letter was sent to Rudi by all those members of the National Laboratory for Psychical Research who had been concerned with the investigation:

16 March 1933

Dear Rudi

We do not think Mr. Price's report on your 1932 sittings can possibly prejudice against you any scientific investigator of psychical research.

But in view of the possibility of hasty readers of the Report coming to a false conclusion, we, as Council Members of the National Laboratory who were present at these sittings, wish to state emphatically that we dissociate ourselves from the implications of the Report (about which we were not consulted) and have faith in your integrity.

We are quite willing for you to give any publicity you like to this letter.

Yours truly,

[signed] H. G. Bois
V. Cochran-Baillie
D. Fraser-Harris
Alex Dribbel
Kathleen M. Goldney
Clarice Richards⁷⁶

⁷⁵ W. Brown to C. Hope, 21 March 1933 (letter SPR files, reference S3; and KMG).

⁷⁶ H. G. Bois and others to Rudi Schneider, 16 March 1933 (copy of letter in SPR files, reference S3).

Dr. Gerda Walther also resigned from the National Laboratory and gave her reasons in some considerable detail and at great length (HPL) pointing out the quite astounding inconsistencies in Price's conduct:

April 4 1933

. . . You blame Professor Fraser-Harris, Dr. Brown, etc., for being rather hasty in their favourable reports on Rudi because they published before your book [*Bulletin IV*] had come out and without talking things over with you. Well, firstly, there are other examples of hastiness in psychics (cf. your report of your examination of Mrs. Duncan's alleged teleplasm before you exposed her). Besides here too it was, in my opinion, very unfair of you to your collaborators to keep such important things as your alleged discovery from them, especially as you had published your first Rudi book and in an article published on May 20th 1932 in *Light* (i.e. nearly a month *after* your alleged discovery of fraud!) you had written Rudi had passed everyone of the most stringent laboratory tests with flying colours!

You say you thought it your duty to publish your discovery 'for the sake of truth in psychics' and 'for the sake of further investigators'—but I certainly don't understand then why you didn't think it your duty to do so immediately, and equally why you kept that photo of Willy from publicity for 8 years making ironical statements about Dr. Prince, Bird, Vinton, Dingwall because of their attacks on the Schneiders as they had no proofs of fraud? . . .

Re the other sittings in your book, I was amazed how poor were the conditions of control under which they took place, I simply can't understand how an investigator of your experience could conduct experiments under so poor conditions. . . .

Price's reaction was to write at once (HPL) to Count von Klinckowstroem, whom he had, a few days before, warmly thanked for drawing his attention to the Meyer-Przibram 'exposure' (about which Price had, of course, known years previously):

6 April 1933

I have just had a letter from Dr. Gerda Walther—a letter I had expected weeks ago. As a spiritualist and a personal friend of Rudi's she has taken offence at my Report and resigned her post as our Munich correspondent. She cannot bear to hear the *truth* about Rudi.

As we wish to have a Correspondent in Munich, I am wondering if you will act for us in this capacity. There is nothing to do and no responsibility. . . .

Klinckowstroem does not seem to have been any too eager to be associated with Price. I have not been able to find his reply, but Price's response (HPL) was to be 'delighted to know that you will be our Munich correspondent. Certainly I will keep this matter private, except that I would have liked to include your name among the Correspondents we issue with our Bulletins . . . '.

Price's onslaught on Osty could hardly have been fiercer, and in his correspondence he made his meaning even plainer than in his printed statements. For example, in a letter to Baron von Winterstein he wrote: 'As for Osty, he is making a living out of psychical research (I understand that his position is worth about £1000 a year) and I can understand his attitude towards the whole business' (HPL).

Rudi himself did not realise at first what had happened, or take in its implications, which is hardly surprising since Price's letters were long, involved, numerous and cryptic. Rudi seems to have been under the impression that

'Onkel Harry' was still angry with him because he had not turned up for the William MacDougall sittings in September 1931. On 7 March 1933 he wrote: 'The reason why I didn't come to you in autumn 1931 was that when you were in Braunau you invited Mitzi and then you wrote the opposite from London' (HPL). Eventually Dr. Gerda Walther made the matter plain to him, and drafted for him a letter refusing to go to Price's Laboratory again in non-committal business-like terms.

13. Interpreting the photographic evidence supporting Price's denunciation

The foregoing must have made it abundantly clear just how complex a skein has to be disentangled before one can get a clear picture of what happened, let alone what it meant. Who, without access to the numerous letters written at the time and kept in different countries and private collections, many of them secret and confidential, and without having kept a close time-table of *what* was said and done *when*, could have emerged with any impression other than that something rather reprehensible was going on?

On the face of it, at some point during the 25th sitting, Rudi's hand was not held by the controller, Mr. Harry Price, whose interpretation was:

The first flash caught Rudi's left arm as it was held out straight behind him, the second flash ignited when the medium had got into position again with his head and body turned towards the screen reflector. . . . The double exposure was of great value in showing us the two positions of Rudi: (1) with his left arm free and behind him, and (2) in position again with head and shoulders bent forward and sideways to the left, facing the screen.⁷⁷

In other words, Rudi shifted the handkerchief from the counterpoise table, thus igniting the flash that released the picture, and then quickly got his arm back into position by the time—about half a second later—when the second flash occurred. Price says in his report that after experimenting in the week following the alleged incident of 28 April 1932 he himself succeeded in snatching an object off the counterpoise and getting in position again before the bulbs ignited.⁷⁸ But of course Rudi presumably did not have access to the seance room there to experiment at snatching handkerchiefs off the counterpoise table to his heart's content. In any case, Price letting go Rudi's hand on one occasion when he was according to himself 'unfit to control' is one thing; the notion that Rudi, evening after evening, freed his hand from Price's and several other reputable and independent controllers' grip, without anyone ever noticing anything amiss or, for that matter, setting in motion the guarding photographic apparatus, seems hard to believe. In his denunciation of Rudi, Price relied on quite a simple psychological device: he first of all gave his interpretation of the photograph as though it were an established fact; he then proceeded to build upon this supposed fact speculations such as 'The question now arises as to whether any of the phenomena we saw at the seance on April 28th were genuine. The fact that Rudi can evade control has set us wondering . . .'. He knew, none better, that the minute the slightest doubt was cast on a single phenomenon and the doubt called certainty of fraud, this

⁷⁷ H. Price (footnote 10), 151.

⁷⁸ H. Price (footnote 10), 152.

was quite enough as regards the vast majority of people to make them dismiss the entire mediumship as so much nonsense. By 4 April 1934 in a letter to Sir Richard Gregory (in HPL) Price stated that he thought the entire Third Series was fraudulent.

On the other hand, all of Price's co-investigators, to the best of my knowledge, placed a different interpretation upon the double-exposure, namely that the first picture showed Rudi under control, but that the flash led him to give a violent jerk thus freeing his hand, and that therefore it is the second picture that shows the freed arm. For instance, Professor Fraser-Harris, after pointing out the numerous irregularities in connection with the picture's publication, wrote that his explanation of the hand freed for an instant 'is that, when after being hours in the dark, the levitated handkerchief was suddenly lifted off the counterpoise, the suddenness and intensity of the flash-light caused Rudi to jerk reflexedly backward out of Mr. Price's admittedly inadequate grasp'.⁷⁹

The Earl of Powis wrote to me:

August 18 1967

... I know nothing about the double exposure affair. ... When the flash went off, Rudi always gave a violent movement and could easily have broken away from his controllers. A photographic expert who saw the picture as reproduced in Price's paper told me that in his opinion the image with the free hand was the *second* of the two. He based this on the fact that an emulsion which has already been exposed is more sensitive than a virgin one and that the free hand image was darker. ...

Lord Charles Hope, after discussing the difficulty of establishing which exposure came first and which second, wrote:

It appears ... that at 10.41 Mr. Price as controller was definitely holding the medium's hands and that at 10.44, 10.46 and 10.49 he could hardly have helped knowing whether or not he was holding the medium, since at those times there were either phenomena or an injunction to hold tight. Nevertheless we are told that one minute later, at 10.50, the medium had got his hand out of Mr. Price's control without Mr. Price's knowledge and had faked at least one phenomenon. By 10.59 or 11.00 at the latest Mr. Price must again have become 'hand-conscious'.

Altogether phenomena were reported as occurring on at least twenty occasions that evening, and Mr. Price suggests that they may all have been faked as, he alleges, that occurring at 10.50 was faked. What was Mr. Price doing with his right hand while all this was happening? It must be remembered that the usual method of evading hand-control [substituting a sitter's hand for the medium's] was not possible here, for there was nobody on Mr. Price's right and hence nobody whose hand could be mistaken for Rudi's. Are we to believe that twenty times during that evening alone Rudi freed his left hand from Mr. Price's hand without Mr. Price knowing it, and that twenty times he succeeded in getting it back into Mr. Price's hand, also without Mr. Price knowing it? Or that Mr. Price for minutes together, even hours, was holding nothing in his right hand and making no effort to find Rudi's left wrist? I find either of these suppositions incredible in a man of anything like Mr. Price's experience.

⁷⁹ D. F. Fraser-Harris, 'Spirit Medium or ...', from *Armchair science* (undated SPR files, reference S3, about 1933). A detailed account by Professor Fraser-Harris of the circumstances leading up to the Harry Price accusation is contained in a statement dated 15 July 1933 (SPR files, 8-10 special folder; also D. F. Fraser-Harris, 'Genuineness of Rudi Schneider, definite conclusions confirmed by recent series of private sittings', *Light*, (17 March 1933), 161-162).

Again, are the distances such as to make Mr. Price's accusation plausible? The chair of the medium (A) and the position of the table (B) on which lay the handkerchief was as shown in this sketch

B

A

not to scale.

The distance between the table and the nearest point of the medium's chair is given on p. 192 [of Price's *Bulletin IV*] as 2 ft. 5½ inches. The position of the medium's chair was in no way fixed, but from a subsequent examination of the room I am convinced that usually the distance was at least 2 ft. 10 ins.

In any case, however, two inches must be added for the distance the handkerchief was from the edge of the table. Rudi's legs and knees were at the time of the movement of the handkerchief in their normal position between the controller's legs: this is clear from the photographs. Only the upper part of his body therefore was capable of any appreciable movement. Rudi is rather below the average height. Could he, while his legs were immobilised, twist sufficiently round to enable his left hand to move an object at least 2 ft. 7½ inches from the right side of the chair? . . .⁸⁰

Anyone can reconstruct this scene with the aid of a table and a chair and will, I think, see that it would have been quite impossible for the medium to have done this in the position in which he was photographed without exciting the immediate suspicion of controller and second controller. As Thirring had said in connection with the Meyer-Przibram affair, the controller would have to be in the plot.

From what has been said so far and the documents cited, it will be quite clear that at the time Price did not believe that Rudi had cheated on 28 April 1932, or at least behaved in every respect as if he did not believe it, until many months later. He seems to have kept the double exposure as a 'card' up his sleeve, to play if and when he saw fit in the light of circumstances. At one point he had obviously hoped to play Dr. William Brown off against Lord Charles Hope, when he had said that Brown's opinion was worth that of a dozen of Hope's scientific friends, even when this failed he still tried by every means in his power to prevent an independent investigation of Rudi. However, in October 1932 Rudi came to London for the Hope-Rayleigh investigation and Price had finally lost his bid for a monopoly, or at least a British monopoly, of Rudi's services. We can say with certainty that Price did not seriously consider treating the double exposure as proof of fraud on Rudi's part until August or September 1932. Once he had committed himself to this view in print, in March 1933, he stuck to it irrespective of anything he himself might have said, done or written earlier.

As regards the photograph of the double exposure, there are several possibilities. To begin with, I had only seriously considered two alternatives: either that the sequence of the two exposures was what Price suggested, namely that Rudi freed an arm, was caught doing so by the first flash, and quickly re-inserted his hand into Price's controlling grip, this being the second flash; or that the handkerchief was levitated as it had been on previous occasions,

⁸⁰ C. Hope (footnote 13), 289.

thus releasing the flash of the first exposure, Rudi gave his usual convulsive jerk, Price let go, and the second flash caught Rudi's freed arm. To the best of my knowledge, these are the only possible alternatives which have been seriously discussed to date. If Price's version is correct, this hardly even accounts for the manifestations of this particular seance in a normal manner. It certainly does not follow that Rudi was free to produce phenomena when anyone other than Price was controlling; and it in no wise explains the partial occultations of the infra-red rays.

If, on the other hand, those of Price's opponents were right who believed that Rudi was wholly innocent and that Price had exploited an ambiguous photograph to gain his own ends, a number of possibilities are open. Most of Price's former collaborators, such as Mrs. Goldney, Professor Fraser-Harris and Major C. V. C. Herbert (Lord Powis) took the view that Price had made use of a photographic accident, or misinterpreted such an accident. Osty and probably Lord Charles Hope were somewhat more reserved as regards the 'accident' theory: there were altogether too many coincidences involved; the incident occurred soon after a virulent row in the Council; the flash-light *happened* to misfire just when Rudi had freed his hand; the ceiling camera plate *happened* to be fogged on the same occasion; Price *happened* to be controlling at the time and *happened* to be suffering from a disabling tooth ache; and the photographic equipment *happened* to be dismantled immediately after the 'accident'. Osty, in his 'L'étrange conduite de M. Harry Price', whilst not dotting his 'i's and crossing his 't's, plainly let it be inferred that he thought the photograph was no accident, especially when viewed against the background of Price's documented behaviour before and after the crucial seance.

Indeed the 'accident exploited by Price' theory is open to a number of grave objections, among the most important of which is the fact that Price himself was controlling Rudi at seance no. 25. How could he have failed to notice that Rudi had freed his hand, especially during a flash? The 'toothache' seems hardly an adequate explanation. And if he was in such agony, why had he not allowed Rudi to be controlled by somebody else? Actually we have some interesting testimony concerning Price's toothache from one of the sitters, a medical man, Dr. Hutton Chisholm, whose only sitting with Rudi was the famous seance of 28 April 1932. In a letter to Professor Fraser-Harris Chisholm wrote that Price claimed that 'Olga' had cured him of toothache, that Price had gone out of his way to insist on the excellence of control conditions, and that in his view fraud was quite out of the question.⁸¹ Miss Irene Manby, another sitter at this particular seance, also stated that Price went out of his way to demonstrate the perfection of his control.⁸² In the report itself there is a reference to 'Olga' stroking 'Onkel Harry's' cheek to cure his neuralgia. Why should Chisholm, whose only seance this was, have misremembered Price's claim to have been 'cured'?

If the double exposure photograph was no accident, then Price must have contrived it. Osty had suggested no more than that Price had deliberately arranged in advance for a double flash so that he, as Rudi's controller, could let go of the medium's arm, thus obtaining a double exposure of a freed and a controlled arm: the neuralgia claim would then simply be an alibi for poor

⁸¹ J. H. Chisholm to D. F. Fraser-Harris, letter published in *Light*, (17 March 1933), 183.

⁸² I. Manby, letter to the Editor of *Light*, (26 May 1933), 324.

controlling. Price himself had installed the photographic equipment and, without informing or consulting anyone, he dismantled it immediately after the famous seance. As a matter of fact, it would have been easy for Price to arrange for a double flash because he used two bulbs in his Vaku-Blitz apparatus after an alleged failure at an earlier seance (seance no. 7, 25 February 1932) on the part of a 'faulty detonator' in the Vaku-Blitz.⁸³ The theory that Price, in his bitter resentment against his colleagues and Rudi, decided to rig two successive flashes is certainly somewhat more in keeping with the train of events than the concatenation-of-accidents theory, and it fits in far better with Price's conduct before and after the fateful seance.

It is a curious feature of the extremely bitter controversy that ensued that Price's opponents hardly seem to have bothered to examine with any degree of care the pictures he had printed. Even from the ludicrously poor prints published by Price, it is clear that there is a major objection to the 'Price-letting-go-Rudi's-hand-on-purpose' theory, as well as to the 'Rudi-accidentally-wrenching-his-hand-free' theory; and that is the actual appearance of Rudi's back and arm on the photograph. If Price deliberately released Rudi's wrist and possibly gave it a push, relying on the medium's constant convulsive activity in trance, one would have expected his *left* arm to fly back to the left side of the chair back (on the far side of the chair back relative to the control camera), or, if he twisted himself round, so that his *left* arm shot past the *right* side of the chairback (the nearside of the chair relative to the control camera) one would have expected considerable rucking and twisting of the striped pyjama jacket; but the picture shows Rudi's back in placid near-repose. The appearance of the photo is quite inconsistent with the theory that Rudi's *left* arm flew, or was thrown, back past the *right* side edge of the back of the chair (see figure 16 in section 7).

There is, however, another possibility, and if this is taken seriously, then Rudi's hand was never free during the double flash at all. One cannot rule out the possibility that the pictures published by Price, purporting to show Rudi's free arm, were carefully prepared fakes.

There is something extremely odd about the photographs published by Price as plates XVIII, XIX, XX and XXI. My corresponding figures are 19, 18 (see below), 16 (see section 7 above) and 20 (see below). As published by Price in his *Bulletin*, these were of such poor quality that Dr. Spencer, President of the Royal Photographic Society, remarked 'that all were out of focus or defective to such an extent as to be almost useless as evidence'.⁸⁴ However, Price never published any prints other than the ones reproduced in his *Bulletin* and in the *Sunday Dispatch*, and declined to allow anyone to inspect his negatives. His main picture is Plate XX (my figure 16) which shows a view of the sitters, some of the apparatus and the medium with his back to the camera, partly obscured by the hugely draped dark lampshade which is hanging down extremely low. The half-tone reproduction is poor in quality, and small details are indistinguishable. According to Price's legend under the figure, 'handkerchief has been dropped behind curtain. Corner of handkerchief can

⁸³ H. Price (footnote 10), 44.

⁸⁴ W. T. L. Becker to H. Price, 7 January 1938 (copy in SPR files, reference S9). Dr. Becker sent his correspondence with Price to the Society for Psychical Research 'as it shows how H.P. reacts to criticism of his work and findings'.

be seen under table'. What can in fact be seen under the table is what looks like the vertical edge of a fold of the curtain.

If one now examines the stereogram, plate XIX (my figure 18) to see what is underneath the table, we find that, according to these twin pictures, there is a flattened white oblong shape which looks nearly horizontal and corresponds to the rectilinear white patch in plate XX (my figure 16) partly obscured by the curtain. This might perhaps be the corner of a handkerchief most of which is in the shadow cast by the curtain and table. It seems strange that Price should have particularly drawn attention in the legend to the peculiarly poor details of whatever was the 'corner of handkerchief . . . under table'. Furthermore, one is struck by the strange white vertical object in the upper part of the right-hand edge on the left stereogram, which cuts across the medium's arm. At first one thinks this must be the medium's back, seen from the slightly different and lower angle of the stereocamera. However, it cannot be the back, since this is curved forwards whereas this white object is straight; nor can it be the lamp covering which is plainly much darker.

Now in Price's plate XVIII (my figure 19), which is supposed to be an enlargement of the left-hand stereogram of plate XIX (my figure 18), this important detail is completely different in shape from the 'same' detail in plate XIX (my figure 18). This something or other hiding a large part of Rudi shows every sign of having been painted in; and figure 19 is plainly not an enlargement of figure 18 (left side) as claimed by Price. Yet this extraordinarily dubious 'enlargement' (figure 19) is the first of the photographs, Plate XVIII, purporting to demonstrate fraud, to be reproduced.

If one scrutinises Plate XXI (my figure 20, which is truly an enlargement of the central portion of his plate XX (figure 16)), one finds that Rudi is sitting in a very peculiar attitude, with his back to the camera and therefore nearly at an angle of 90 degrees to his 'controller', Price, and that during both exposures. If Rudi had been under proper control, that is, facing Price (as was that evening particularly demonstrated to sitters, according to Dr. Chisholm and Miss Manby), then one of his aspects should show Rudi sideways on, the other turned as in fact shown. If he had not thus turned anti-clockwise, through nearly a right angle to the back of the chair, then he could not have passed his almost straight *left* arm past the *right* (near side) of the chair back, towards the counterpoise table. Yet the enlargement of the central detail does not show *any* sideways view, as can be clearly seen by examining the pyjama stripes. There are 'two back views', and in one of these the medium bends forward a little more.

Returning to a closer examination of the main plate XX (figure 16), one also asks: what on earth is the bolster or cushion doing that has been hung up by a piece of string from a book case behind the medium's outstretched arm? There is nothing like it in any other photograph of a seance or the seance room. Another puzzling detail is the absence of a hand in the pyjama jacket of the outstretched arm. In all the relevant photographs this sleeve is plainly visible, all the stripes on the jacket standing out clearly. Yet the sleeve opening of the jacket is empty. There are some blurs so faint as to be barely visible under a strong magnifying glass that might conceivably be ghostly—very ghostly—fingers, but which could not be a human hand. Is the hand supposed to be hidden in the pyjama sleeve? In that case, one might have expected Price



Figure 19.

Price's alleged enlargement of figure 18. This can clearly be seen not to be identical with figure 18: in particular, the vertical patch at the top right is different in shape and coloration. Both details show evidence of having been painted in. Plate XVIII, p. 154 of *Bulletin IV* (footnote 10).

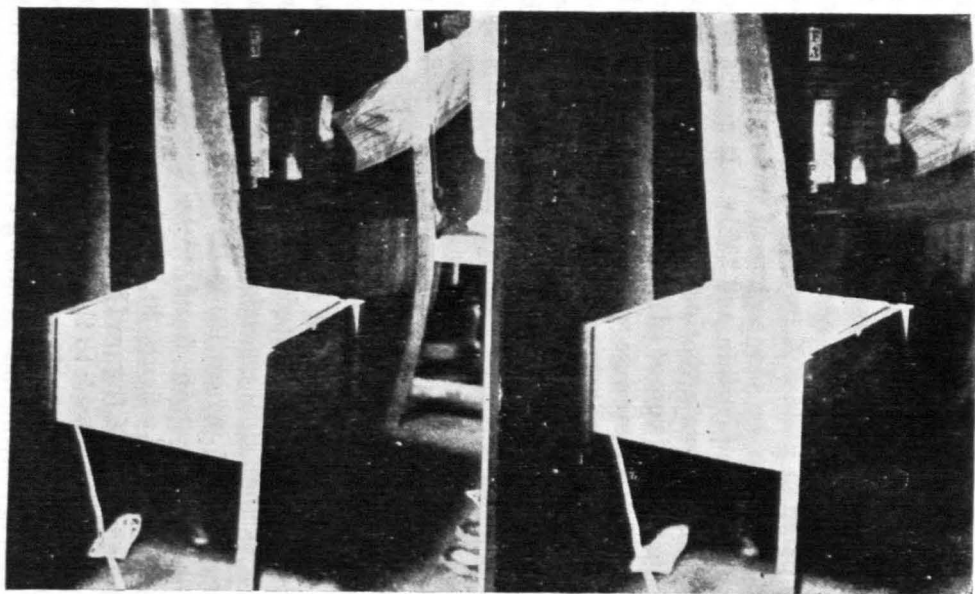


Figure 18.

Stereogram allegedly referring to the same incident as that depicted in figures 16, 19 and 20. The oblong white patch at the bottom left is curious when compared to the corresponding detail in figure 16. The legend for this figure, which is plate XIX, p. 160 of *Bulletin IV* (footnote 10), states that 'handkerchief had been dropped behind curtain. Corner of handkerchief can be seen under table'; but to what this refers is far from clear. The most questionable aspect of the left stereogram is the vertical white patch at the top right of the picture, which does not seem to correspond to anything in figure 16, and looks as though it had been painted in



Figure 20.

Central portion of 'control photograph' (figure 16, Price's plate XX) allegedly showing Rudi's freed left arm. This is the only portion of the plate originally left unobscured by brown paper in the Harry Price Library archives. Price's legend states that this detail 'shows Rudi sideways on chair facing screen'. The feet, which face 90° to the right as if Rudi had his back to the back of the chair, are of course cut out and were covered by brown paper. Plate XXI, facing p. 188 of *Bulletin IV* (footnote 10).

to discuss how Rudi could have removed the handkerchief from the counterpoise table in a pyjama jacket the sleeves of which were many inches too long for him. One might have thought the sleeve would droop or seem empty where Rudi's hand ended, but the portrayed sleeve has a well-formed opening. Is the suggestion that Rudi swept the table with the sleeve or some implement hidden in the sleeve? (Reconstruction of the event shows that he would have needed a reaching device of at least about 15 inches in length to touch the table top in the position shown.) Price's failure to discuss the empty sleeve is a very curious omission indeed, particularly from an expert in conjuring.

The negative plate from which Price's illustration XX (my figure 16) was made is preserved in the University of London Library as No. 530 of the Harry Price Collection. I obtained permission from the Collection's Curator, Dr. A. H. Wesenecraft, to have it copied. However, it was found that a detailed examination was quite impossible because brown paper was stuck over it, leaving only a 'window' in the middle exposing the central detail shown in Price's plate XXI (my figure 20). Mr. Colin Brookes-Smith, an instrumentation engineer and expert in photography, who gave me extensive and generous help in connection with the photographs, their enlargement and

interpretation, suggested that a firm of professional photographers should be employed to remove the covering as this might demand extremely skilful handling of such an important photographic plate. Several prominent psychical researchers, including Dr. D. J. West, past-President of the Society for Psychical Research, recommended that the possible risk entailed in removing this brown paper should be taken. Dr. Wesencraft kindly gave permission for this to be done and for enlargements to be made. In the event, the photographers employed, Messrs. Blinkhorn of Banbury, had little difficulty, since the brown paper turned out to be stuck on to the glass, not the emulsion side of the plate. Figure 16 is a reproduction of plate 530 after the removal of the brown paper.

Several enlargements of the parts previously covered by the brown paper tape were made. The most compromising details of all was perhaps an enlargement showing part of Rudi's left leg, sock and shoe (figure 21). They can be seen between Harry Price's right foot and Mrs. de Gernon's feet, which are partly obscured by some flowers with large leaves resting on the upturned wastepaper basket immediately under the suspended lamp. Rudi's left foot is



Figure 21.

Maximum enlargement of details from print of Price's Plate 530 (my figure 16) after the brown paper covering had been removed, showing part of Rudi's left leg, sock and shoe and right trouser leg. The left foot is clearly pointing to the right, that is, at right angles to direction of the back in figures 16 and 20. It is also clear that between the flashes of the double exposure the medium's foot only moved very slightly.

facing Price and is quite clearly at an angle of about 90 degrees to the camera. Rudi's foot has moved slightly between flashes by not more than about half an inch; in other words, corresponding to the foot and leg one would expect in the main picture two *side* views of Rudi. Yet there are two *back* views, and this is amply borne out by the new enlargements. Little is visible of Rudi's right leg except the kneefolds, but it seems to have been virtually stationary between the two flashes. The conclusion seems to me inescapable that the back belongs to a different occasion from that on which the legs were taken.

Once one has seen a well-printed enlargement of the plate and has one's attention drawn to Rudi's foot and legs, so far as they are visible, it does indeed look very odd that the foot should be pointing one way and the back another. However, Price's half-tone illustration is so poor that one would hardly be likely to notice Rudi's foot at all. Why did Price tell Professor Fraser-Harris that he only had 'some poor prints'? He could quite easily have made excellent ones! No one was ever allowed to inspect the actual plate. Had they persisted, they would presumably have found it largely obscured by brown tape that hid the foot. So far as I could ascertain, no other plate in Price's large collection of plates was so obscured.

Mr. Brookes-Smith gave it as his opinion that the photograph purporting to be an exposure of the medium Rudi Schneider, and reprinted in *Bulletin IV*, was a fake. He suggested that Price superimposed a secretly taken photograph of an extended arm on a photograph taken at an actual seance. Price contrived a double flash photograph of this seance so as to enable him to explain away any mis-matching between the two super-imposed pictures as being due to the effects of the double flash. In Mr. Brookes-Smith's view, there were originally three plates: an 'A' plate showing an extended arm and a back; a 'B' plate, the double exposure taken at an actual seance; a 'C' plate resulting from a re-photographing of a retouched composite print made by superimposing enlarged images of 'A' and 'B'. Price almost certainly destroyed 'A' and 'B', and the surviving plate No. 530, originally largely covered with brown paper, is this 'C' plate, from which the illustrations in Price's *Bulletin IV* as well as the enlargement (figure 21) were made.

Whether or not one accepts the interpretation of plate 530 as an elaborate fake, there cannot in my view be any doubt that figure 20 is not an enlargement of the left-hand stereogram of figure 18. On the face of it, some gross re-touching has taken place.

14. Summary of possible interpretations of the photographs

To summarise the situation, the following are the different possible interpretations of the photograph published by Mr. Harry Price in the *Sunday Dispatch* of 5 March 1933 and in the *Bulletin IV*:

(1) Rudi freed his hand from Price's control without Price noticing what he was doing, and stretched out his hand towards the counterpoise table. (He could not have reached this without an implement in the attitude shown.) Rudi then re-inserted his hand into Price's. On this, Price's interpretation, the first flash shows Rudi's freed arm; the second took him controlled once more.

(2) Rudi gave a convulsive jerk in response to the first flash; Price let go and there happened to be a second flash taking Rudi's freed arm. Price

did not notice that this had happened, but he discovered the double exposure when developing the negatives and made use of this accident to discredit his enemies.

(3) Price contrived a double flash. He released Rudi's hand *after* the first flash and threw it back; the first shot would then show Rudi controlled, the second flash free.

(4) Price contrived a double flash and knew exactly when it was coming because he himself controlled it. Just beforehand he swivelled Rudi round by 90 degrees so that the latter had his back to the camera, released or pushed back his hand, then took one picture, seized the hand again and the second flash caught Rudi controlled again.

(5) The picture is a cleverly contrived fake, the result of super-imposing a hypothetical plate containing an exposure of a back and an outstretched arm upon the double-flash picture taken at seance no. 25, when Rudi's hand was in fact held at the time and the medium remained facing his controller, Price.

(6) The camera at seance no. 25 photographed a materialised arm, and Price used it to score off his enemies.

Hypotheses (2) and (3), which at first sight look the simplest explanations, must, at least in my view, be ruled out. How in the time available could a person, on having his *left* arm released or even pushed, manage to get it round the *right* side of the back of the chair? If he swivelled round by about 90 degrees, this would be clearly shown by the disposition of the pyjama stripes. Furthermore, a person released during trance and wildly swivelling about the upper part of the body would not keep feet and legs virtually stationary facing in their original direction. The photograph shows that at least Rudi's left foot was not constrained by Price.

On hypothesis (1) Rudi sat at 90 degrees to his controller before and during both flashes and stretched out his hand just before the first flash, bending forward slightly whilst doing so. He then, in time for the second flash, put his hand forwards so that only his elbow was still visible from the angle at which the camera took his picture. Even with the most complaisant controller, there is a limit to what a medium can do with one freed hand, his left, and one free hand would not suffice to explain the other phenomena observed even at this seance. It could not explain the partial occultations of the infra-red beam observed on other occasions in this series of Price's. There is also the problem of the empty sleeve, the distances involved, and the other considerations discussed at length.

If Rudi and Price were confederates, one might have expected something like this fact to emerge from the correspondence between them, certainly after Price had published his double exposure photograph and his accusations of fraud. Yet the lengthy correspondence shows every indication of being that between a sophisticated and rather condescending investigator and his naive, somewhat simple medium, Rudi sadly lamenting that it should still be necessary to convince 'Onkel Harry' of his innocence. And, after all, much the same set of phenomena were observed by Osty beforehand and by the Hope-Rayleigh investigators subsequently.

Theory (4) might be considered on the supposition that Rudi's secondary personality was amenable to suggestion from Price to the extent of swivelling

round and sitting on the chair sideways and putting his hand back, and then rapidly re-inserting into Price's hand. There may have been no movement, paranormal or otherwise, of the handkerchief accompanying the double flash; there is no satisfactory photographic evidence of any movement (or for that matter existence) of any handkerchief on Plate XX (figure 16). However, it seems barely credible that 'Olga' (of all 'people') should have been quite so malleable to Price; and furthermore, (4), like (1), has to meet the difficulty of Rudi's leg and foot. If Rudi was sitting with his back to the camera so as to be able to get his left arm past the right side of the back of the chair, why were his legs and foot unambiguously facing Price, sideways onto the camera? Also, on both theories (4) and (1), just as on theories (2) and (3), it is difficult to account for the absence of Rudi's hand.

On theory (5) Price never let go of Rudi's hand at all, but rather created a plate by superimposing upon the double-flash picture another posed plate on which either the medium or another person or a dummy was shown with his back to the camera and with his arm outstretched. This would fully account for the fact that the back is facing one way and the feet another; why there was blatant retouching of the enlargement of the left half of the stereogram; why the sleeve is empty (if a dummy was used); why there is no side view of Rudi visible on either exposure; and why a black backcloth was hung up by a bit of string from a bookcase behind the 'freed arm'. Nor do we have to suppose that the whole plan would have been contingent upon the paranormal levitation of a handkerchief. Both flashes could have been triggered off at Price's own discretion by means of some concealed electrical connection between the flash bulb apparatus and a switch, perhaps under the carpet, that he could operate with his foot.

There are two *prima facie* objections to theory (5), the fake photo interpretation. One is the fact that some of the retouching is so clumsy and crude as to be quite incompatible with the highly expert and subtle faking that would have been required. The other objection is that several essential conditions and precautions for it must have been created or devised long before Price quarrelled with Lord Charles Hope, Professor Fraser-Harris, Dr. William Brown and the other scientists.

As regards the first objection—that the mixture of clumsiness and expertise seems inconsistent—this can be met when it is recalled that Price was a highly-skilled conjuror, a past-master in the art of deflecting attention from what was really essential to a proper understanding of the situation. His constant drawing attention to the ambiguous 'handkerchief' under the table quite effectively deflected attention from the placid back, the feet pointing in the wrong direction, the faked shapes in the stereograms. Again, no-one but Price had access to his negatives during his lifetime. His opponents only had extremely poor prints published by him. They could not tell what was a fault in the paper and what was a carefully produced effect, and Price knew he would never give them any opportunity for a closer scrutiny: what he did produce over all was a superb impressionistic sketch that conveyed just what he wished to convey. Furthermore, Price's co-investigators were entirely deflected from what, on hypothesis (5), he had really done, by his almost unbelievably underhand conduct in keeping an allegedly compromising picture secret for a year; in using their money and their names for the purpose of

bringing out his report which they had never seen and which was manifestly designed to damage them; in testifying to his belief in the genuineness of the phenomena when he was preparing to denounce them. This latter manoeuvre was what, to most of his fellow investigators, constituted his main crime; and most of them were far too angry to examine dispassionately just exactly what Price had done. The scientists who were not specifically psychical researchers, such as C. C. L. Gregory, Professor Pollard and several others, seem hardly to have taken any notice of the fracas over the freed arm picture, contenting themselves with the general reflection that Rudi could easily have jerked an arm free during a seance and that in any case it did not matter since the phenomena had by then been replicated under different conditions and auspices. The objections to the fake theory, then, on the grounds that a mixture of crude and subtle faking is incongruous and could hardly have escaped detection, falls to the ground: the crude re-touching, and Price's blatant breach of good faith could have served successfully to deflect attention from the real, major, faking.

What of the time factor? I originally rejected theory (5), the faked photograph hypothesis, because it seemed as though, for this to be true, Price would have had to lay his plans for his forgery months before his open quarrel with his Council. I could not reconcile this careful plotting with his obvious and passionate desire to have Rudi accepted as genuine by the learned world in England and all over Europe until as late as July 1932 when (13 July 1932) he assured Fraser-Harris that there was 'not a shred of evidence that Rudi ever cheated' and when he solemnly protested on 27 July to the Editor of *Nature* against an attack on Rudi in that journal, and upheld all of Osty's work. Nor can there be one moment's doubt that Price was violently and genuinely angry with William Brown on 14 May 1932 for having written his second letter to the *Times*, adopting a more reserved attitude towards Rudi's phenomena: 'Never once has Rudi dictated conditions', he wrote to Brown on 14 May 1932 (HPL), although about a year later he was to claim that Rudi had insisted on having his 'accomplice' present during sittings and that he had caught Rudi cheating on 28 April 1932.

The first written reference we have to a quarrel between Price and his Council occurs in a letter from Lord Charles Hope to Price dated 8 April 1932, after which relations deteriorated disastrously. It was from this time onwards that Price was faced with the determination of his Council to conduct an independent investigation. I find it extremely hard to credit that Price definitely organised a frame-up of Rudi before April 1932, though judging by the violence of his feelings and his undoubted duplicity afterwards I find no difficulty in believing that he decided on such a nefarious course subsequently. He could have prepared such a fake exposure, intending to use it if and only if his enemies actually succeeded in capturing Rudi.

Taking into account the physical conditions of seances, it looks at first as though, for the crucial photograph to be a fake constructed by super-imposition of plates, Price must have begun his campaign much earlier than 28 April 1932, since certain preparations were essential for such a fake. The most important of these as described in the *Bulletin IV* are the substitution of two flash bulbs for one (25 February, page 44), the substitution of an armless chair (10 March, page 66), and the covering and lowering of the lampshade (7 April, pages

109-112). In fact, however, we only have Price's word for it that he made these changes as early as he said he did: *Bulletin IV* was published nearly a year later, and he was the only person who had a minute-by-minute seance record dictated to his secretary at the time. What would have been easier than for him to slip in less than half a dozen comments suggesting that changes were made at an earlier date than was the case? We have only Price's word for it that any bulb failed to ignite on 25 February 1932; and even if such a bulb failure occurred at that date, if the passage is read attentively, it will be seen that there is no assertion as to precisely when two bulbs instead of one were fitted—for all we know it could have happened only at seance 25, or even conceivably not at all except for purposes of publication in *Bulletin IV*!

Once one realises that there is no need to accept any part of Price's published record as necessarily truthful or accurate, one can see at once that, by slipping in no more than four or five sentences by way of comment he could, without even arousing his own secretary's suspicions, have conveyed the impression that at least some of the arrangements essential for a fake were made at dates so early that no one would have been willing to interpret them as preparations for a frame-up. Furthermore, we have only Price's own word for it that he showed Rudi 'the incriminating photograph' on the morning after the seance. He could have done so at any time before Rudi's departure, and he could have shown Rudi any photograph he chose in which a pyjama clad arm was seen behind the medium's back. Mrs. K. M. Goldney has no recollection of just when Price showed her the 'incriminating photograph': she says it could have been any time after 28 April 1932. But clearly, it could not possibly have been earlier than 23 May 1932 when she wrote a long letter to William Brown, taking him to task for his second letter to the *Times* in which he had expressed some reservations about the phenomena and implied doubts about Price's Laboratory and urged the need for further investigation. She concluded her letter to Dr. Brown:

23 May 1932

. . . Will you forgive this long epistle? I have been drawn into it by disappointment on Mr. Price's account that his initiative, his really hard work and energy in running the Lab and putting through the investigation of Rudi should apparently be 'damned with such faint praise' as is implied by your second article. . . .⁸⁵

How could she have written in this vein had she at this point been in possession of the information that Price had a photograph purporting to be an exposure of Rudi which he would keep a secret but which he would publish if and when it suited him to revenge himself on his 'enemies', notably Dr. William Brown? On the contrary, Mrs. Goldney would presumably have been relieved that Dr. Brown had, by his second and more cautious letter, safeguarded himself to some extent against Price's eventual 'exposure' of Rudi.

If all these factors are taken into account, it now becomes quite possible to suppose that Harry Price decided some time, not necessarily earlier than 28 April 1932 and possibly later, to stage his double exposure photograph; and he could at any time subsequently have arranged for what Mr. Brookes-Smith called the 'A' plate by means of a dummy. Price therefore had plenty of time in which to plan for a fake and absolute discretion as regards the date of

⁸⁵ K. M. Goldney to W. Brown, 23 May 1932 (KMG).

publication of his Report; indeed, he was free to decide whether to publish at all or not, and he had considerable latitude as regards its contents. There can be no question concerning his technical skill or lack of scruple and fierce aggressiveness.

This, then, is theory (5), namely that Price faked the photograph of seance no. 25. Another variant of (5) is that Price had all along intended some sort of frame-up of Rudi during the Third Series. Anyone wishing to take this harshest of all possible views would stress the questions: why did he omit the electrical control by which he set such almost fanatical store? Why did he arrange for such very unfortunate advance publicity for Rudi when the latter arrived in England? Why did he keep secret from his fellow investigators Osty's warning concerning Mitzi and, worse still, why did he have Mitzi present at sittings, and outside the circle of sitters, next to his secretary?

There is yet another possible interpretation, (6), of the episode, that should at least be mentioned; that the photograph at seance no. 25 recorded an actually materialised arm, and that Price either honestly mistook this paranormal appearance for fraud, or at least used it as such. Although I am willing to concede the possibility of such an occurrence in principle, there is so much evidence of tampering, chicanery and contrivance in connection with this photograph that it seems to me gratuitous to hypothesise any paranormal explanation. The theory should however at least be mentioned because it is at any rate *an* alternative to an elaborate and deliberate fraud on Harry Price's part. On this interpretation he would have made use of what might be called a paranormal accident to revenge himself on those who had slighted him in the belief that he really had proof of fraud which he decided to publish or not according to how he, Price, was treated by his colleagues. Such conduct would disqualify him as a serious investigator, but it would at least not involve him in a charge of painstaking faking and a blatant frame-up.

To me, the fake photograph theory presents the fewest difficulties, but others may judge differently. On *no* interpretation, unfortunately, can Price's conduct be vindicated. Even on the view that he was fully persuaded that the photograph constituted unambiguous proof of Rudi's guilt, he lied, verbally and in writing, to a large number of people in vouching for the complete authenticity of the medium and Rudi's entire trustworthiness and compliance at all times *after* the alleged event; and if he had what he deemed adequate proof of guilt his co-investigators, several of whom had in fact financed the venture and whom he claimed to be jointly responsible with himself for the investigation, were plainly entitled to know that he had such proof.

Either Price put some accident to most nefarious use; or else he staged a bogus accident. There is no escape from this dilemma.

15. A variety of conclusions

A number of issues arise out of a detailed consideration of the impact of Price's denunciation of Rudi Schneider. These might be summarised under the following headings: (1) The general question as to what constitutes orthodoxy in science and what are the criteria for separating genuine and pseudo-science; (2) The special problems in designing and interpreting experiments in psychical research; (3) Ethical questions arising out of investigations in unorthodox fields; (4) The recording and communication of findings in

unorthodox fields; and (5) Fraud and scientific research. I shall treat them in turn.

15.1. *Orthodoxy in science.* There is at the very heart of the scientific enterprise a conflict which does not become apparent unless one is faced with the investigation of a set of assertions currently outside the scientific framework. On the one hand, science is concerned with the dispassionate and systematic investigation of any aspect of the world that happens to come under scrutiny; on the other hand, science is also based on the assumption that certain things cannot happen, and may be safely dismissed as superstition. One such ostensibly impossible set of happenings is the movement of large material objects without a physical cause. Laboratory observation (or for that matter any other kind) would become difficult if, say, specimens to be weighed were to affect balances without any known physical increase or decrease in weight. Now it is precisely this type of phenomenon that is being subjected to scrutiny in the case of the investigation of a physical medium such as Rudi Schneider. The movement of a handkerchief, or the partial occultations of an infra-red beam are, on the face of it, physical impossibilities without some agency; and yet it is obviously proper to examine such a claim, and to do so with a foregone negative conclusion clearly offends against the whole spirit of the scientific enterprise.

This is not the place to discuss at length the 'demarcation problem', and the question whether belief or method should be at the basis of science, issues discussed as long ago as 1954 in connection with psychical research.⁸⁸ However, the stormy events of Rudi's career point up sharply the basic issues that arise in practice if the conflict in theory is not explicitly faced. If certain happenings are deemed impossible *a priori* by the scientific community, then the provision of contrary evidence becomes a virtually hopeless task since any evidence against these happenings, however ludicrously inadequate, will seem acceptable to the 'educated' public. In any field more accepted than that of psychical research, the Osty, Hope-Rayleigh and Schwaiger investigations of Rudi Schneider would have far outweighed the allegations of Harry Price, the threadbare nature of which would have been apparent once it was carefully considered. One practical consequence of a theoretical negative *a priori* assumption is that evidence in favour of the phenomena in question will tend to be discounted uncritically whereas evidence against them will be accorded privileged status.

There is here a real danger of rendering certain phenomena in principle, and hence in practice, invulnerable to demonstration.

15.2. *Some special problems in designing experiments in psychical research.* Supposing the basic theoretical problem were overcome, there are a number of special problems for the field of psychical research or parapsychology, several of which are highlighted by the Schneider investigation. The most widely discussed perhaps is the elusiveness of the alleged manifestations: at present the phenomena can neither be produced to order nor can general circumstances be described in which they might reasonably be expected. In the case of a

⁸⁸ C. C. L. Gregory and A. Kohsen (Anita Gregory), *Physical and psychical research: an analysis of belief* (1954, Reigate: Omega Press).

medium such as Rudi where phenomena are fairly constant over a number of years, experiments can be, and of course were, conducted with him; but in the absence of a more general theory quantifying the forces involved, no satisfactory predictions can be made concerning other individuals or circumstances.

The fact that phenomena are apt to be so intimately associated with a particular named individual creates all sorts of practical problems for experimentation. For one thing, as will have become quite clear, a medium of this type is immensely in demand: he is, so to speak, a valuable property both from the point of view of scientific investigation and also from the less disinterested motives of the quest for publicity, publication potential, money. In the case of Rudi, who had been trained by Schrenck-Notzing as a purely 'scientific medium' from the age of eleven, the complication that he himself might quite understandably decide to cash in on his ostensible unusual gifts, did not arise; but the possibility is plainly always there. The problems arising from the rarity of such persons are all too obvious from the Schneider case. Unless there is harmony among the different groups of investigators, who see themselves as having a common interest, there is here a permanent source of potential disaster. Not only is there the ever present opportunity for obtaining automatic credit as a critical person for exposing the medium, in addition there is also the temptation to score off rival researchers by unmasking a medium that a rival has vouched for.

Unfortunately such 'sociological' pitfalls of unorthodox investigation, of which there are a good many, are further aggravated by the elusive nature of the alleged phenomena and their tendency to weaken and decline as time goes on. In the case of the Schneider investigation, as is usual, the phenomena decreased in vigour and quantity over the years, so that an ever-greater number of sittings were entirely negative. There is no reason to suppose that ever-greater sophistication and vigilance in control methods was responsible since, among other things, *some* phenomena persisted and were attested by some of the most critical of later researchers. On the whole, the gradual attenuation of psychic manifestations is universally observed. It is therefore quite possible that a later experimenter may fail to find paranormal phenomena, not because he is a better observer but because the phenomena have ceased, or become so weak and sporadic as to amount to the same thing; a medium may have taken to 'helping things along' not necessarily because (s)he always was a fraud but because the effects have now become so attenuated that cheating is resorted to so as to preserve the credibility of the earlier and authentic effects, or to retain all the gratifying attention and other benefits that have accrued from earlier glories.

By the time the Price scandal broke, Rudi's phenomena had already waned to a considerable extent. How far the attendant inevitable upset contributed to the totally negative results of the subsequent Besterman and Gatty investigation cannot now be decided.⁸⁷ But it is not at all impossible to suppose that a gross breach of trust such as that committed by Price (on any interpretation) would be bound to affect Rudi profoundly, and anything affecting the medium might, for all we know in our present state of ignorance, affect the phenomena. A medium is thus multiply at risk.

⁸⁷ T. Besterman and O. Gatty (footnote 14).

Furthermore, the evidence such as it is suggests that what is required in the case of 'physical phenomena' is an atmosphere of boisterous jollity in which participants throw off their normal restraints and inhibitions. Now unfortunately this is not the best possible set of conditions in which to observe in an impartial, cool, critical and dispassionate spirit, nor are the most impeccable scientists necessarily gifted at creating situations of uproarious and rather simple-minded fun. On the other hand, we know that Price was peculiarly talented at promoting the sort of atmosphere at which phenomena were quite likely to be observed; see, for example, the comments of Lord Powis in sub-section 8.2 above. Without injustice, Price might be described as having had the defects of his virtues in this regard: no one could have described him as scrupulous, meticulous, inhibited, modest or restrained. It would be too easy, and in any case question-begging, to suppose that staid, careful and scrupulous people inhibit results because they prevent fraud, whereas uninhibited extroverts are apt to obtain them because they cheat, or make cheating easier. The Rudi investigation shows that this is not an adequate description of what is apt to happen.

Supposing that a general lack of inhibition and restraint is conducive to authentic phenomena, this presents obvious enormous practical problems for careful observation. Instrumental registration becomes indispensable, and the general control conditions must be such that those participating in the experiments can let themselves go without fear of their being deluded or duped. It may well be that such letting go is of great importance for obtaining positive results: at the same time, it may also well be that scientific and academic discipline may tend to impair people's ability to be good psychological promoters of psychical phenomena in this sense. These factors need to be explicitly faced in this context.

On a more theoretical level, it may be attractive and convenient to suppose that everything is what it is and not some other thing, and that our thinking about things does not in itself make any difference: such a supposition may, however, be erroneous. It may be that, not only through our overt, though possibly subtle, behaviour as well as through our inward attitudes we can affect the world around us; in fact, this is one of the hypotheses under consideration when we investigate the paranormal! We may not rule it out *a priori* without unscientific dogmatism. We are thus faced, in the case of parapsychology, with a peculiarly difficult version of the 'experimenter effect'. At the heart of the subject there lies a psychological complementarity that certainly requires close scrutiny: we must face the possibility that our attitudes affect what really happens in some direct way.

Another related problem that has to be tackled is what is sometimes called the unconscious nature of 'psi'. Although 'unconscious' is both ambiguous and misleading, the fact remains that in general those associated with these phenomena do not experience themselves as being responsible for them. In Rudi's case this ostensible secondary personality 'Olga' (a co-conscious person in Morton Prince's sense)⁸⁸ considered that 'she' was moving and knotting handkerchiefs and 'going into the [infra-red] ray'. Rudi, so far as anyone was ever able to show, was totally unconscious of Olga and her paranormal

⁸⁸ Morton Prince, *The dissociation of personality* (1905, New York: Longmans & Green).

doings. This means, among other things, that, for phenomenological intents and purposes, an experimenter may be faced with two different personages with different temperaments and proclivities. It was 'Olga' that experimenters had to court and please to produce phenomena: if people, for example, refused to sing 'her' songs or play 'her' music *she* would refuse to move any objects. (Schrenck-Notzing in particular tended to go on strike in this respect, only to find that, as a punishment, his seances were apt to be totally blank when it most mattered to him that they should demonstrate results.) In other words, the researcher is faced with problems that are characteristic of psychiatric situations and psychological experimentation in a particularly acute form: he is dealing with individuals with likes and dislikes, and a will and caprice of their own. Although the scientist may be investigating ostensibly physical effects, these are, whatever interpretation be adopted, mediated by some psychological agency.

Only the crudest behaviourism would fail to distinguish between the movement of physical objects in accordance with physical forces, and the goal-directed actions of persons. In order to interpret the acts of persons we need, among other things, an appreciation and understanding of the function of movements. Nowhere is there a more dramatic substantiation of this philosophical point than when we are endeavouring to interpret the ostensible paranormal movement of physical objects. On the face of it, the Schneider phenomena were produced by a personification called and calling herself 'Olga' for purposes of her own, such as showing off, pleasing and impressing favoured experimenters, doing as she was asked generally. This personage, though inaccessible to Rudi, had some degree of control over the alleged phenomena. At the same time 'her' sophistication was very limited: for example 'she' at first did not have the least understanding of her apparent impact on Osty's infra-red installation until this was explained to her in very simple terms, and it seems quite astounding that 'she' should not at any time have picked up Price's growing hostility and willingness to destroy Rudi rather than allow him to be investigated by Hope: there is no hint in any of the numerous accounts of sittings before and after the 25th Price seance that she had any idea of what he was hatching. One might have thought that ordinary sensitivity to atmosphere without paranormal talent, or a fairly rudimentary sense of self-preservation, would have been sufficient to signal some warning. It is most interesting that there is no hint of this kind. On the face of it, the paranormal talents of such sub-personalities seem to be specialised; but to date we have little knowledge of how to categorise, group or predict such abilities. There is some evidence that 'Olga' could see in the dark and move objects, none that she could read thoughts or even be responsive to the most violent (though unexpressed) feelings; yet a boisterous atmosphere of laughter and singing was apparently conducive to paranormal performance.

15.3. *Ethical questions.* That all sorts of ethical issues arise out of investigations in unorthodox fields goes without saying. This general topic has been discussed elsewhere at some length.⁸⁹ The most fundamental ethical issue arises, in my view, out of the need to face the basic methodological issue. It is,

⁸⁹ Anita Gregory, 'Ethics and psychical research', *JSPR*, 47 (1974), 283-305.

I believe, obligatory for those engaging in research in these heterodox fields to make it clear and explicit whether (so far as they know consciously) they would be willing to grant the authenticity of such phenomena should they encounter them. To pretend to embark on an unbiased enquiry and profess willingness to be convinced, whilst being wholly unwilling to find a particular set of data, is, and should be, clearly seen to be unscientific duplicity rather than scientific tough-mindedness. That this raises difficulties goes without saying: but why should one expect moral problems to be simple?

In fields of enquiry which lie outside the main body of accepted knowledge there is, inevitably, far less by way of accepted conventions regulating conduct to colleagues and to experimental subjects. There is no sphere of human activity in which Price's double-dealing (even on the kindest of interpretations) would be explicitly acceptable from an ethical point of view. On the other hand, the very fact that an enquiry deals with officially discounted phenomena facilitates conduct which, in any other context, would be subject to scrutiny and to universal criticism.

In such a field, as in others, mutual confidence tends to spring up between groups of workers with similar standards such that A will trust B but not C. Since the phenomena being investigated are not readily repeatable (which means that they are unreliable rather than that they necessarily do not happen) there is also a greater danger of the perpetuation of prejudices than in areas where with sufficient training, care and control, manifestations can readily be produced, or at least found. Individuals and groups of psychical researchers are apt to have *a priori* prejudices about what types of phenomena they will and will not countenance. For example, the Sidgwicks who were, deservedly in most respects, immensely influential in the field of psychical research, tended to throw the full weight of their prestige behind a repudiation of the so-called physical phenomena, so that, despite Mrs. Sidgwick's arrogant contempt for Harry Price, the latter could count on the unilateral scepticism against physical phenomena supported by Mrs. Sidgwick to lend maximum impetus to his denunciation of Rudi.⁹⁰ There is reason to believe that Price had no intention whatever of undermining either the physical, or even most of Rudi's, phenomena. Yet a consequence of his action, almost certainly, was to finish the topic for a generation of researchers.

It is no doubt a special temptation for those who risk their academic reputations by engaging in unorthodox fields to reassure themselves and the academic community of their continued sanity by disallowing certain phenomena which somehow seem particularly implausible, so as to enlist support for those that do seem to them worthwhile and acceptable. However difficult it may be to avoid such a temptation -- indeed even to be aware that one is in danger of succumbing to it -- it is essential to be fully alive to it. Not only does such conduct offend against the basic openness and impartiality of scientific enquiry; it may also, in a field in which we know very little indeed, for all we know preclude some important factor for better understanding. The dangers of academic politics are even greater in heterodox than they are in orthodox domains.

⁹⁰ Alan Gauld, *The founders of psychical research* (1968, London: Routledge & Kegan Paul), provides details of the great contributions to psychical research of Professor Henry Sidgwick and Mrs. Eleanor Sidgwick, née Balfour.

That subjects of psychological investigation and experimentation (human and infra-human) are entitled to a certain consideration is now universally recognised. In parapsychological research such protection is at least as essential: the account of the phase of Rudi Schneider's mediumship given above is sufficient to show what can happen if some basic human considerations are omitted. Part of the danger arises from the ambiguous light in which such a subject is apt to be seen, varying from super-human talented demi-god to cunning fraudulent charlatan, *via* pathological abnormal specimen. Such stereotyping may lead to unjust and brutal treatment of the experimental subject.

The case also illustrates dramatically the ethical and human problems that arise out of collaboration between professional and qualified people with one set of standards, values and objectives on the one hand, and interested and quite possibly more knowledgeable amateurs subject to no such discipline and restraints, and whose objectives may be wholly different. In a field of uncertain status such collaboration will inevitably be much more frequent, and hence pose these problems more often and more dramatically.

15.4. *Recording and communicating of results in unorthodox fields.* The very fact that a field is heterodox implies that the status of the expert is ambiguous. In an accredited and well-established discipline there are authorities on a given topic or subject area who, however controversial they may be within the field, yet command a certain respect in virtue of their training and standing. There is also a fairly clear line of demarcation between investigators and popularisers—or investigators as researchers and investigators as popularisers. Moreover, as time passes, conventions grow up as regards recording and publication of findings.

In a field which is not clearly established, then the question who is and who is not an established and credible authority is, from the very nature of the case, inevitably in dispute. Not only will there be rival factions with deep divisions as regards belief, standards and methods, there will also tend to be somewhat different reference groups in different countries. For example, in the case of the Schneider investigation, transatlantic investigators were on the whole perfectly willing to throw out the work of Germans such as Schronck-Notzing without much ado, whilst respecting the work of British researchers, including Price. The British scene, as has been shown, was deeply divided between the Society for Psychical Research (which was and probably remains the nearest approach to an established learned society in this field), and Harry Price's National Laboratory of Psychical Research. Both had their officers, councils, meetings and publications. All the people involved had, in a sense, amateur status: the fact that, for instance, Professor Fraser-Harris was an eminent biologist, or Professor Pollard an authority on engineering, or Lord Rayleigh an important physicist, or Dr. Brown a well-known psychiatrist and hypnotist, gave them *some* claim to be qualified to conduct aspects of a research programme in psychical research, but it did not automatically bestow the title of expert. Perhaps the only professional psychical researcher at that point was Dr. Eugène Osty, whose *Institut Métapsychique International* was *reconnu d'utilité publique* and financed by the French state. Harry Price was a successful retired businessman, who through extensive flamboyant and highly

publicised researches certainly had some claim, if one wishes to be fair, to be considered as an authority on the subject.

As has been indicated, collaboration between people of disparate standards and loyalties is apt to be a difficult matter. When it comes to the question of recording, communicating and publishing results, such problems may, and in this case did, become explosive. It has been shown how catastrophic was the failure to circulate seance reports as soon as possible after the relevant seances to all participants. This, in effect, left it open to Price to doctor what he published to an indefinite extent; but no one at the time, however hostile to him, seems to have questioned the accuracy of his reports. Vater Schneider had seen the importance of instant recording and signing for his son's reputation, and by the time Hope and Rayleigh conducted their investigation, the procedure was standardised: all seance reports were circulated to and agreed by all participants as soon as the official note-taker's manuscript had been typed, and a point was made of doing so within a day or two. (A set of these typed notes is in my possession.) The publication in the *Proceedings of the Society for Psychical Research* is based on these notes, and compiled by Lord Charles Hope, individual sitters having added their comments, observations, interpretations, reservations, and so on.⁹¹ It is the very high standard in this respect that probably makes the Hope-Rayleigh investigation one of the most important ever conducted in the field of psychical research. Osty's was certainly more original and adventurous; indeed, the Hope investigation is in a sense merely a confirmation of aspects of Osty's. But Osty's policy as regards publication was explicitly not to enumerate and involve individual participants and provide detailed records: he wished to get away from the whole unholy cycle of accusation and counter-accusation. His wish in this respect is understandable, but as his brush with Price over the participation of Mitzi showed, his omission rendered him somewhat vulnerable to attack. The Hope-Rayleigh report, though it was utterly eclipsed at the time by Price's denunciation and may only be fully appreciated now, over 40 years later, remains a scholarly and scientific document of more than historical interest.

Perhaps the most obvious publication problems arise for those involved in the scientific exploration of the so-called paranormal because of the notoriety value of the subject matter. Price, though a clear example of one who enjoyed and exploited this aspect to the full, is far from alone in this respect. His case is, however, an ideal illustration. Examples given included his brush with Osty over the papers on Mrs. Helen Duncan's teleplasm, which Price, not to put too fine a point on it, had had stolen for him at a seance from which he had been excluded; his violent clash with Brown whom he pressured into committing himself in the *Times* and then accused of courting publicity; the publication of his popular book *Rudi Schneider*, in which he made it plain that he considered himself to have made an epoch-making set of observations; and his *Bulletin IV*, in which he provided his evidence of fraud against Rudi Schneider a few years later. It is plain that the issue in all cases was public acclaim and notoriety as well as claim to scientific priority and respectability.

Who, in the case of a joint venture, should determine on the format and timing of publication? To whom should the credit go? Which should have

⁹¹ C. Hope (footnote 13).

priority: a cautious and careful scientific account, or the stirring popular claim to proofs of new (or ancient) marvels? What, with respect to recording and responsibility for publication, are the relative roles, rights and duties of the organisers of an investigation, and the experts consulted? What the Schneider investigation made abundantly plain is the need for explicit agreement and conventions that are made clear to all involved right from the beginning of a research project. Matters of confidentiality, publicity, records and publication cannot be left unformulated and at the mercy of the most enterprising and possibly least scrupulous of all those participating. At the same time, it is of obvious importance to preserve and encourage the enthusiasm and enterprise of capable, energetic and knowledgeable workers in this, as in any other, field, and not to discourage them by too many restrictions, too much galling anonymity, and an unrealistic denial to them of the more worldly incentives. It is no easy matter to achieve such a balance; yet such is all too clearly essential for success.

15.5. *Some psychological aspects of Price's fraud.* The whole question of deliberate fraud in science has received relatively little attention, although there has been some recent discussion.⁹² In psychical research the topic of fraud has always been of major importance and interest because of the very obvious need for vigilance against marvels manufactured by wonder workers; and it is precisely the precautions against deception ranging from gross fraud to the most subtle and unconscious self-deception that constitute a great part of the psychical researcher's expertise. This is not the place to examine the topic of mediumistic fraud which, as has been mentioned, ranges from carefully prepared artifice to unconscious compliance with group suggestion. The issue is rather that of deliberate experimenter fraud, which also has received more attention in the context of parapsychological research than in other fields: indeed, it is among the most frequently discussed issues, precisely because of the methodological problems discussed. Because of the acceptance of the inherent implausibility of the authenticity of these phenomena, as conditions have been tightened up, it is more and more the experimenters who tend to come under suspicion and from time to time such a fraud is actually discovered by fellow researchers, proved and admitted.⁹³ The Price-Schneider episode is unusual in that it is the exposure of the medium rather than the production of paranormal phenomena that can be shown to be fraudulent. Either an accident was exploited to reach a false and irrelevant accusation, or else—and this is far more probable from the evidence—a very careful falsification of the evidence was staged. I hope I have shown how it could have been, and probably was, done.

Some of the reasons will also have become apparent: for example, Harry Price's anger against various other researchers and determination to discredit their work rather than be deprived of the monopoly of acclaim he considered his just due. Price was a businessman by origin and a publicist by inclination. He saw an investigation as an entrepreneurial transaction from which he

⁹² I. St. James-Roberts, 'Cheating in science', *New scientist*, 72 (1976), 466–469.

⁹³ For example, J. B. Rhine, 'A new case of experimenter unreliability', *Journal of parapsychology*, 38 (1974), 215–225.

expected credit and acclaim. To him the experts whom he consulted were more like specialists hired by an industrialist. If they confirmed what he had engaged them for, well and good, they were prestigious scientific experts, subsidiary to himself but still of some importance; if, on the other hand, they developed views of their own and, worse, attempted to impose restrictions and qualifications on the conclusions he wished to publish to the world, they had only themselves to blame if he disposed of them in any way that he saw fit. Indeed, the very fact that he *could* discredit them by sleight of hand no doubt proved to his own satisfaction that he was a better expert on fraud and its discovery than they. Only Osty and probably Hope came near to suspecting something of Price's machinations: they were ultimately frustrated by not having available the actual photographic plates and by not having access to the evidence of quite monstrous double-dealing contained in his archives.

Price counted on others not to construct any very detailed plans such as table 2 in section 8 above, displaying exactly what he had said and written and to whom and when, not to scrutinise his published plates too closely, and not to recall with detailed precision just what had happened minute by minute about a year previously at particular seances. He could count on the privacy of his archives, the British laws of libel and slander, the universal prejudice against the phenomena, and his own supreme conjuror's skill at deflecting attention, to protect himself against being found out, certainly from having his fraud brought home to him.

It would, I think, be a mistake to see Price as solely motivated by the quest for popular glory and notoriety. He also longed for scientific status and respectability; he saw himself as an expert, a pioneer in a new field, an innovator. Almost alone among the investigators of Rudi Schneider, he was without academic training or professional standing; yet he knew himself to be more knowledgeable and experienced in psychical research matters than many if not most of them, and very much their superior in tactical shrewdness—in what he would probably have called something like practical commonsense and a working knowledge of human nature. Why should they receive glory and credit for his work and expertise? Why should they be in a position to relegate him to a status of relative unimportance when he was, in his own estimation, a better man than any of them? Ironically, what he really wanted was to lay the foundations of an academically respectable science of psychical research, to found a university department or similar unit with himself at the head. He may have hoped for an honorary doctorate—not a wholly crazy hope, since the University of Bonn more or less offered him one, together with the Red Cross Medal First Class, although nothing came of it.⁹⁴ He may well have hoped for some academic appointment, by-passing the normal university channels.

He had sufficiently good judgment to see that the Rudi Schneider investigation offered the best possible starting point for some academic venture. The relative respectability of the phenomena over a long period, the unblemished record of the medium together with his intellectual limitations and compliance to experimenters' demands, the fact that different teams had made similar observations in different countries, and the non-doctrinal and unspiritualistic

⁹⁴ See footnote 35.

aspects of the mediumship, all these contributed to making it as respectable a case as had ever been investigated.

It does not seem that Price himself believed that his action would destroy his own past record as Rudi's investigator, or the Rudi mediumship itself. Indeed, on 24 January 1934 the Senate of the University of London resolved, largely on the strength of the Rudi Schneider investigation, 'that Psychical Research is a fit subject of University Study and Research' while continuing to decline to accept Price's offer of his library equipment on Price's terms.⁹⁵ Price himself of course realised with crystal clarity that all he had—at best or worst—shown was that on one occasion when phenomena were supposed to be happening, Rudi had freed one hand; and that this was neither here nor there as regards the overall genuineness of phenomena. However, the scandal arising from his timing and the manner in which he elected to publish his 'discovery' were such as to undermine what he most passionately wanted to achieve. It was a price he was probably willing to pay if *he* was not to be the recipient of the consequent glory: he preferred to risk destroying his cause rather than be relegated to a secondary position.

What is perhaps surprising is that, over the years, Price seems to have forgotten his own part in the discrediting of Rudi Schneider. His last letter, written two days before his death and addressed to Mrs. Goldney, referred to Dr. E. J. Dingwall's rejection of Price's Rudi Schneider investigation:

27 March 1948

... Talk about thrashing a dead horse! It would have been impossible by any means to have secreted an 'accomplice' at the Rudi sittings. The lights were on half the time, door locked or sealed, and similar seances have been held all over the Continent. And how can Dingwall apply that theory to the seances we had with Willi at Munich? You will remember that he and I signed a joint statement that we were perfectly satisfied with the arrangements (*our* arrangements) and that we regarded the phenomena as genuine. And I wonder if D. has forgotten that signed statement he gave me after a Stella sitting (when he was lying on the floor!) to the effect that he saw a bulbous materialisation, attached to an ectoplasmic cord, writhing about near Stella's feet; D. is a strange chap. . . .⁹⁶

Acknowledgments

I would like to thank the following for their help: Mr. C. Brookos-Smith, for advice and assistance in connection with the photographs; Mrs. K. M. Goldney, for information, advice and permission to consult and quote her files; The Editor of *Nature*, for permission to quote from Price's letter to the Editor of 6 May 1933; The Secretary and President of the *Institut Métapsychique International*, Paris, for help in searching the archives and permission to quote relevant material; The Polytechnic of North London, Educational Development Division, for photographing relevant material; The Earl of Powis, for permission to quote from his letters; The Society for Psychical Research, for permission to quote from archives and publications; The Secretary and former Librarian of the Society for Psychical Research, for help in searching through the archives of the Society; Mr. Paul Tabori, for permission to quote from source material in the Harry Price Library and from Price's publications;

⁹⁵ Minutes of the Senate of the University of London, 24 January 1934, 1307/59.

⁹⁶ H. Price to K. M. Goldney, 27 March 1948 (KMG).

Dr. Gerda Walther, for advice and information; Dr. A. H. Wesenraft, Curator of the Harry Price Library, University of London, for assistance in searching through the archives and permission to remove paper covering from photographic plate No. 530; and Dr. D. J. West, for advice and guidance.

Much of the material was collected during my tenure of a Perrott-Warriek Studentship. I am grateful to the Electors of that Studentship for financial assistance. The material has been re-organised, re-considered and re-written as part of a project submitted to the Council for National Academic Awards for a research thesis for a Master of Philosophy Degree. I would also like to express my thanks to the Editor of *Annals of Science* for help and advice with the presentation and organisation of this paper.

TESTING BY MEANS OF RANDOM NUMBER GENERATORS

By Anita Gregory

It is widely supposed that statistical procedures and technical apparatus bestow some sort of automatic scientific respectability on parapsychological research. Nothing could be further from the truth. Statistical procedures have to be both reliable and valid to be of any use, and apparatus has to conform to specifications, be adapted for the investigation in question, and be used in accordance with its properties. Nor is the maker's manual necessarily the best possible guide to the evaluation and use of a piece of equipment. On the contrary, it could be said that research involving complex apparatus is more likely to be misleading than a simpler investigation, the quantitative and technical shortcomings of which would be all too obvious from its description.

This is not a new issue in psychical research. For instance, in the case of past investigations of physical mediums, controversies used to rage as to whether it was preferable to use the "manual control"—a conventionalized pattern of holding the medium—or to employ some variant of the more sophisticated "electrical control," by which the medium, controllers and possibly sitters were all included in an electrical circuit, such that if anyone freed a hand or a foot, a lamp would light up showing just where the contact had been broken. Whatever the rights and wrongs of that controversy, there can be no doubt that the electrical control not only solved certain problems, it also raised fresh ones of its own.¹

One of the most important technical innovations in quantitative research in recent years is the introduction of random number generators, which obviate the need for tiresome, as well as often questionable, recourse to random number tables.² On the other hand, it is of course vitally important that such devices should in fact be random, otherwise all we would be measuring could be, for example a subject's ability to learn a pattern. It is, therefore, essential for would-be researchers to test for randomness in the manner most

appropriate to the instrument in question.

Some time ago I purchased a piece of equipment manufactured by Paratronics Inc. called ESP-1. This turned out to be heavily biased. However, in the manual issued by the makers the procedure for checking its randomness was such that it cloaked the very type of bias that I discovered. I should say, right away, that once I was able to persuade the representative of the firm through whom I had purchased it that my complaints might be justified, I was able to return it to the makers who very promptly offered credit or a substitute in accordance with their warranty. It was eventually agreed that my criticisms of both machine and manual had been quite correct, and the makers promised to inform all past purchasers of ESP-1 of the fault in the tests for randomness described in the manual and to issue fresh and appropriate instructions in future manuals.

The matter is, in my view, of considerable importance for would-be experimenters with commercially available apparatus apt to be displayed at psychical research conferences, who could be misled into believing and publishing quite absurd results. Moreover, the risk of wasting time and energy, temper and money on lengthy correspondence is by no means wholly negligible.

ESP-1 (see figure 1) is a random number generator such that there are 4 equally probable target lights (1, 2, 3 and 4) and 4 corresponding buttons (1a, 2a, 3a and 4a). There is an on/off switch (S) which, apart from turning on the apparatus, causes one of the lights to light up. There is, furthermore, a display panel (D) which lights up after every 10 trials to show how many "hits," i.e. coincidences between selector button and target light, have been scored. (A screen barrier can be easily inserted between buttons and target lights.) There is also a fifth feedback button (FB) which gives the score in figures on the display panel at any time if pressed.

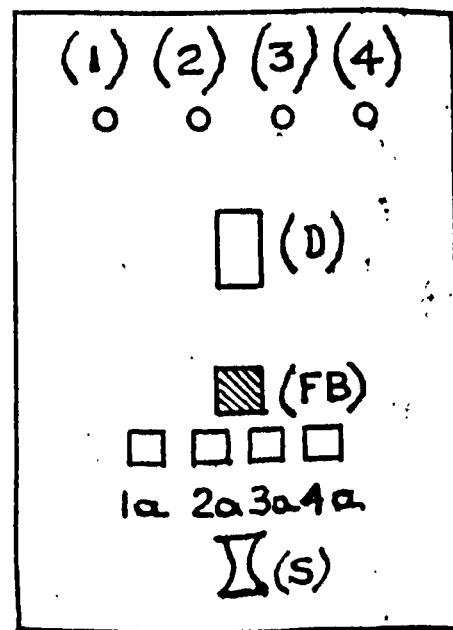


Figure 1

Sketch (not to scale) of display panel of ESP-1. For explanation of symbols in brackets see text.

According to the manual supplied to me, the electronic portion of the apparatus is activated when one of the target select buttons is pressed. It includes two randomly phased oscillators which are used to "mix" the targets at an "unpredictable rate."

"The delay after the button is pressed is a function of the random bounce, associated with closing the switch. This is the mechanical element of the random process.

"The length of time that the target continues to mix depends on how long the operator holds the button down. This feature adds an unknown human influence over the target selective process."

As I was primarily interested in using the machine as a PK instrument, I concentrated on "trying" to get a particular target to light up by pressing the corresponding button. I soon obtained gratifyingly—and suspiciously—high scores with highly significant

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results. When I ran control tests, as per the manual, I obtained strictly random results. The instructions were as follows:

“Testing for Randomness

“A simple and convenient way to test the machine for chance operation when ESP is not involved is to press the push buttons in the non-random sequence 1-2-3-4, 1 - . . . etc. (at a rate no faster than two buttons per second). Try to avoid thinking about the position of the targets to minimize the possibility of injecting PK into the results. At the end of each run of 10 trials, record the displayed score . . . Use table 1 to evaluate. . . .” This is followed by a little table, and a footnote: “If you achieve ESP-results during a randomness test, you should not assume that the machine is in error without additional testing. This is because certain individuals such as Uri Geller have been able to exert a PK influence over electronic devices without significant mental effort. Therefore, if the machine appears to behave non-randomly, consider repeating the test with a different operator.”

However, there was no trace of non-random behavior on the part of the machine during randomness testing runs, and very appreciable deviation during those runs when I “wished” or “willed” a particular target to light up. My next step, rather than to rejoice in my new found paranormal talent, was to question whether the randomness testing procedure was satisfactory, and to devise my own much slower and more plodding, but far more searching procedure. Instead of using the display panel for summarizing, I recorded the result of every button pressing and switching on. For a particular set of 1000 systematic button pressings, the targets lit up as shown in Table 1.

It will be seen that the differences from mean chance expectation together amount to 0, despite the fact that individually they are very large and highly significant.

When I depressed the switch(s) only to see which number target would light up, I obtained the results shown in Table 2.

No. 1	No. 2	No. 3	No. 4
166	311	207	316
N = 1000 MCE (mean chance expectation) 1000/4 = 250			J = 13.9
D (differences between the observed result and mean chance expectation):			
D ₁ = -84	D ₂ = 61	D ₃ = -43	D ₄ = 66
CR ₁ = -6.04	CR ₂ = 4.39	CR ₃ = 3.09	CR ₄ = 4.75
Table 1			

No. 1	No. 2	No. 3	No. 4
79	164	88	169
N = 500, MCE = 125			
D ₁ = -46	D ₂ = 39	D ₃ = -37	D ₄ = 44
Table 2			

Again, it will be seen that between them results are random, individually they are not. Moreover, a simple calculation shows that they are linked in such a way that target Nos. 1 and 3 and Nos. 2 and 4 differ from mean chance expectation in equal and opposite directions.

For Table 1 D₁ + D₃ = -127
 D₂ + D₄ = +127

For Table 2 D₁ + D₃ = -83
 D₂ + D₄ = +83

Since thus these deviations from chance cancel one another out precisely, the results of testing as recommended in the manual were totally misleading.

The suitability of a device such as ESP-1 for testing PK might in any case be questioned since, even if it were random, “the length of time the targets continue to mix depends on how long the operator holds the button down”; I would therefore be inclined to regard it as, at best, a primarily cognitive ESP-type task — “knowing when to let go” rather than “influencing the apparatus.” It also seems to me extremely confusing to have a gadget which is supposed to count and summarize both ESP type coincidences (where the button is depressed after the light is already shining), and also PK/precognition type coincidences, where the button is depressed before the wished-for light comes on. How can the machine “know” whether I want to guess the existing light or influence the future one? The arithmetic

is different. The apparatus is at best, i.e. if and when properly random, useful for clairvoyance and telepathy testing.

Attractive, light and reliable equipment for the purpose of ESP and PK testing and training are highly desirable. It would, however, be a regrettable error to suppose that the availability of commercial equipment will automatically lead to widespread and successful amateur parapsychology. Amateur activity in this field is, in my view, very much to be welcomed. However, where difficult technical questions such as randomness are concerned, very high standards for assessing of evidence are needed for the experimenter’s results to be of any value.

REFERENCES

1. This was a hotly debated issue in the case of the mediumship of Rudi Schneider. Dr. v. Schrenck-Notzing devised and Mr. H. Price developed the “electrical control.” See A. v. Schrenck-Notzing, *Die Phänomene des Mediums Rudi Schneider*, 1933, Leipzig, de Gruyter. H. Price, *Rudi Schneider, a Scientific Examination of his Mediumship*, 1930, London, Methuen. The latter book was reviewed by V. J. Woolley, *JSPR*, 1930, No. 468, XXVI, 125-26. Price’s reply and Woolley’s rejoinder are in *JSPR*, 1930, 469, XXVI, p. 139. See also reference to the topic by T. Besterman in “The Mediumship of Rudi Schneider,” *Proc. SPR*, 1931-32, 125, 40, 428-36.
2. See, for example, H. Schmidt, “Instrumentation in the Parapsychology Laboratory” in J. Beloff (Ed.) *New Directions in Parapsychology*, 1974, London, Elek, pp. 13-37.

THIS HOUSE IS HAUNTED, AN INVESTIGATION OF THE ENFIELD POLTERGEIST. By Guy Lyon Playfair. Souvenir Press, 1980. 288 pages. £6.95.

Early in September 1977 a poltergeist outbreak was reported in a council house in North London occupied by the "Harper" family: mother, Mrs. H., and four children, Rose (13), Janet (11), Jimmy (7) and Pete (10, away at a boarding school during term time). Among those first involved, and subsequently serving as witnesses, were Mrs. H.'s brother, Mr. John Burcombe, and Mrs. Peggy Nottingham, the H.s' next door neighbour. There were reports of raps and noises and of objects moving about in the time-honoured manner. The police were called in, as was the press. Eventually someone rang the SPR, whose Secretary, Miss Eleanor O'Keeffe, suggested to Mr. Maurice Grosse that he might like to look into the matter. Mr. Grosse was soon joined by Mr. Guy Playfair, and between them they adopted the case and spent a very substantial number of hours with the H. family. This book is Mr. Playfair's account of the affair.

DECEMBER 1980]

Book Reviews

The author quotes from my introduction to Oesterreich's "Possession" to the effect that the phenomena described were still very much in need of an explanation:

"It would be very simple for me and acceptable to others if I were to say that all these people were dupes, frauds, lunatics and psychopaths, and to suggest that this constitutes some sort of explanation," said the writer, who turned out to be our colleague, Mrs. Gregory. . . .

The following day I received a note from Anita Gregory and John Beloff accompanying the report they had written jointly the morning after their visit to Enfield. It was their opinion, they told me, that the girls were playing tricks with us.

Maurice Grosse and I, who knew they were not playing tricks with us *at least certainly not all the time* (my italics) plunged back into the battle on 12 December." (p. 137).

Now in his letter Dr. Beloff did say (13 December 1977) that both he and I thought the girls were "playing games", but also stressed that he was not asking Guy to deny what he had witnessed with his own eyes. My own position is similar: I never suggested that everything was at all times faked by the girls, so perhaps Guy's gentle irony is somewhat misplaced. Why should *we* be expected to take the claims at face value as a package deal on the strength of one visit?

However, it is quite true that I have not the least doubt that on that occasion the girls were thoroughly enjoying themselves giving us the run-around. We were all excluded from the bedroom, and over and over again there would be a thump and a squeal, Janet would be sitting on the floor, and it was claimed that an "entity" had thrown her out of bed. By way of evidence we were assured that Janet could not possibly have jumped that far. Maurice Grosse also challenged whoever it was to speak, and whoever it was obliged in a hoarse deep whisper, either somewhat monosyllabically, or with standard and monotonous swearwords.

Nor was my impression that the girls were play-acting in the least mitigated by the subsequent occasions when I was allowed to stay inside the bedroom (provided, that is, I faced the door and covered my head with the girls' dressing gowns) whilst slippers and pillows were shied at me. There is not the least doubt in my mind either that the girls, with their mouths invariably covered up by sheets or blankets, made up the "voices" as they went along, at any rate to begin with. (I was allowed to turn round in order to talk). But I cannot vouch for the fact that all this could not have got out of hand and presented quite a different picture, and that dissociation and near-hysteria might have been reached when I was not there, and when these "voices" were taken dead seriously. I certainly received the impression (which isn't evidence, of course) that the children were

positively relieved that I, at least, was treating the dialogue and pelting as games and a bit of a joke, and I always made a point of stating firmly that all good poltergeists go to sleep at 11 o'clock. My candid refusal to take any of this at face value was never held against me by any of the family, who always welcomed me most cordially, nor by the "voices" who allowed me into the sanctum when true believers were excluded. As Guy correctly quotes me as having said in Cambridge, "fraud" was quite an unsuitable word in the context. (p. 228).

The trouble is that this sort of case is very likely to be an unwholesome and painful mixture of personal and social pathology, play-acting, group interaction, self-deception, trickery, ego-tripping and—conceivably—some authentic paranormality: the real problem is disentangling them. Unfortunately the book is not likely to make much of a contribution towards this end: it does provide testimony which I personally accept as being sincere, and some of which it is reasonable to accept as competent, for occasional paranormal physical movement of objects, especially in the earlier phases of the case.

The book is impressionistic and somewhat chaotic, and the absence of an index makes it difficult to trace any particular theme or person. At times it is positively confusing. For instance, I can form no reasonable picture as to what was and was not done, and how many times, and when, with the video equipment which, by arrangement with Professor John Hasted, I transported to Enfield in my car, together with David Robertson, on 15 January 1978. The reader is told (on p. 248, i.e., nearly at the end of the book) that it was decided to set up the video recording apparatus so that the bedroom could be monitored without the girls' knowledge:

"It seemed like a great idea at the time, but it was a total flop. No sooner were Maurice and David out of the house than Janet hopped out of bed for no apparent reason and peered through the keyhole of the back bedroom, and by a particularly unfortunate coincidence the TV monitor and revolving recorder reel happened to be in direct line with the keyhole. Janet saw them and . . . realized we were playing a trick on her. So nothing happened. Janet later told me she had suspected our trick at once.

We all finally decided that Janet had to get out of the house. . . . She left home on 16 June 1978. . . ."

Surely, they didn't really wait until May or June before trying to monitor the bedroom where most of the action was supposed to be? And what of the video recording that David Robertson took *before* Janet knew he was doing so, and in which she can be seen bending spoons and attempting to bend a bar in a thoroughly normal manner, and then bouncing up and down on the bed making little flapping movements with her hands? True, like all evidence this needs inter-

preting, but it does require sorting and discussing at the very least!

John Burcombe and Mrs. Nottingham seemed to me sensible and reliable witnesses. Both testified and stuck to belief in some of the phenomena observed in the earlier phases of the case. On 23 December 1977 John Burcombe told me he thought that Janet taught herself the trick of talking in a deep voice and that she enjoyed keeping strangers hopping about: that now the family were keeping things going, and that the researchers were, without perhaps fully realizing it, "egging them on". On 15 January 1978 Peggy Nottingham told me that what was going on *now* was "pure nonsense", and it was kept going by the investigators. But she stressed over and over again Maurice Grosse's marvellous kindness to the family, to which I also can testify. It is of course not at all easy to know what best to do when one has become a participant in a difficult and painful family situation, nor is one necessarily oneself the best judge, especially when deeply emotionally involved, as to what is one's own contribution to a complex situation.

The book, then, may be enjoyed as entertainment, and it also adds another case to the centuries of personal testimony for paranormality embedded in quite other facets of life. It is far too sketchy, unsystematic, incomplete, imprecise, ambiguous and confusing to be seen as a contribution to research, except in an oblique way: it points compassionately to the setting of personal tragedy, conflict and suffering with which would-be investigators may have to come to terms; and it also emerges towards the end (although one would hardly have thought this at the beginning of the book) that the author came to realize that a truly adequate investigation is a very time-consuming, expensive, expert enterprise, requiring near full-time commitment and involvement as well as skills drawn from many professions and disciplines. What is more, there can be no doubt that some of these skills do not yet exist!

ANITA GREGORY

REFERENCE

- ¹ Oesterreich, T. K. *Possession, Demonic and Other Among Primitive Races in Antiquity the Middle Ages and Modern Times*. London: Kegan Paul. 1970.

Madam,

In Anita Gregory's review of 'This House is Haunted' (*Journal*, No. 786) it is alleged that the two leading witnesses of the Enfield poltergeist case, John Burcombe and Peggy Nottingham, made statements to the effect that the case was 'being kept going by the investigators'; does this mean Guy Lyon Playfair and myself or the media? Both Mr. Burcombe and Mrs. Nottingham, questioned in January 1981, denied having made any such allegation, and I would be glad to know if Mrs. Gregory has any record of her reported conversations with them.

May I also mention that both Mr. Playfair and I were well aware, long before the dates of their alleged statements, that the children were adding to the phenomena. We each caught them, without much difficulty, playing tricks on more than one occasion, and we would have been surprised if they had not done what all children tend to do—imitate what they see around them. However, as

William James pointed out (*The American Magazine*, October 1909), 'If we look at imposture as a historic phenomenon, we find it always imitative' . . . In other words, in order to imitate something, there has to be a genuine original.

On the matter of the controversial deep voice, Mrs. Gregory and Dr. Beloff are quite entitled to their opinions that the girls were 'playing games'. But they overlook two facts. One is that the original deep voice was produced on demand (in their presence), and the other is that I have repeatedly issued a challenge to any critic to produce a girl of twelve who can make the same sound under the same conditions, using the false vocal folds, and keep it up for three hours. If Mrs. Gregory is correct, she should be able to do this. By asking others to repeat a witnessed effect, I am merely following accepted scientific practice. I am sorry that she cannot do the same, but prefers to retreat under a barrage of psychological jargon that explains nothing.

Finally, Mr. Playfair has provided abundant evidence in his book of genuine phenomena witnessed not only by ourselves but by about 30 other people, including a police constable, and I am surprised to find this evidence totally ignored. I am led to wonder if some members of this Society are prepared to admit the existence of the phenomena the Society was formed to study.

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MAURICE GROSSE

Madam,

In reply to Mr. Maurice Grosse's comments on my review of Mr. Playfair's 'This House is Haunted', the statements made to me by Mr. Burcombe and Mrs. Nottingham on 23 December 1977 and 15 January 1978, to the effect that the investigators were keeping things going, were written down at the time in my own case notes, and these were circulated in instalments and at the time to, among others, Dr. Beloff and Prof. Ellison. I hope my review made it clear that I never attributed to Mr. Grosse any but the most kindly concern for the family's welfare.

I can only reiterate what I wrote in the review: Dr. Beloff and I merely stated that on the occasion to which our statements referred the girls were in our view playing games; we never said they were doing so at all times: indeed Dr. Beloff explicitly made this point in his letter quoted by me in the review. I therefore strongly resent the insistent insinuation that we indiscriminately over-generalized on the strength of inadequate experience. Indeed, I made quite a few visits subsequently and these greatly strengthened my initial impression.

I am hardly likely to overlook the fact that 'the voices' were produced on demand in Dr. Beloff's and my presence. We were both struck by the extent to which 'the voices' were elicited in response to leading questions. No evidence has been offered to the effect that the deep voices were continuous for three—or even one or two—hours on end. If records had been published, referring back to primary sources, showing the ratio of 'deep-voice-time' to 'non-deep-voice-time' over long periods, this might be a matter of some interest and could then be discussed by those qualified to consider physiological aspects. I never heard any but quite brief snatches of chat from 'the voices'. I was much struck by the fact that the investigators seemed to think that the standard gutter language uttered, often punctuated by girlish giggles, was some sort of corroboration for extra-sensory or spirit origin! I would really like to refer Mr. Grosse to the whole literature on possession, hypnosis and split personality (This is not sheltering behind psychologic- technicalities, of which Mr. Grosse rather unfairly accuses me. Can he cite a single sentence, or even phrase, let alone a 'barrage of psychological jargon' in my review?) Such considerations are essential before one makes judgements of abnormality or paranormality. I would recommend starting with Oesterreich's 'Possession' to which both Mr. Playfair and I refer.

It is misleading to suggest that the whole matter of 'the voices' could be settled quite simply by replication. If 'the voices' really were seriously abnormal then, quite apart from other considerations, one could hardly risk another child's health. It is quite a usual observation that subjects in hypnotic and allied states are capable of feats of athletic and other prowess which they cannot replicate in a normal state; and a conscientious experimenter would hesitate to attempt to replicate abnormal athletic or physiological feats even in an abnormal state: what may be happening is that the normal physiological safety factors are being ignored and over-riden, and damage to the body could be risked. However, so far I cannot say that we have been presented with any evidence that strongly suggests that there was anything so grossly abnormal about the Enfield voices that a couple of youngsters could not have produced without too much trouble.

Mr. Playfair does not 'present abundant evidence': what he had done is to produce a readable book containing assorted claims of varying degrees of persuasiveness but, to quote my review, it is 'far too sketchy, unsystematic, imprecise,

ambiguous and confusing to be seen as a contribution to research'. What is required for purposes of adequate evidence is systematic presentation, signed testimony, production of ordered results, reference to and discussion of primary sources, careful preparation of secondary sources, dispassionate appraisal of the data, discussion of alternative interpretations and a fair representation of the views of those whose assessments do not accord with those of the author. These are conspicuous for their absence. (So in Mr. Grosse's rejoinder is any reference to the episode of the videotape to which I alluded in my review.)

The reflections expressed in my last paragraph lead to the final and, I hope, constructive point I wish to make. Mr. Grosse suggests that I am not 'prepared to admit the existence of the phenomena the Society was formed to study'. Now what I actually wrote was that the book 'does provide testimony which I personally accept as being sincere and some of which it is reasonable to accept as competent, for occasional paranormal physical movement of objects, especially in the earlier phases of the case'. The story as presented simply does not entitle us to do more than that. The fact that I know and value and trust Maurice Grosse (which I do), just is not evidence. The Society was founded, not for the purpose of establishing the phenomena so that a few individuals should get a subjective feeling of reasonable certainty, but for the objective examination and evaluation in a scientific and scholarly manner; that is, for the provision of the type of evidence—if the case warrants it—that would impersonally convince tough-minded, critical, independent persons, provided their minds are not pathologically closed. I think that this is possible and desirable, but exceedingly difficult, and Mr. Playfair himself seems to agree. If Mr. Grosse will re-read the last sentence of my review, he will see that this point too was made. Where, I believe, some of the more eminent members of the Society, including several of its founders and chief luminaries, have been at fault is in belittling to an unreasonable degree sound evidence for physical paranormality. I have been one of the most persistent and earliest exponents of this view. I personally believe that it is this sincere but misguided disparagement that has retarded the proper development of the subject of psychical research, its legitimation by 'the establishment', and also prevented, until quite recently, proper explicit standards and criteria for spontaneous and other physical phenomena from emerging and being formulated.

I would wish that the present controversy might serve as a starting point for such a progressive formulation.

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A. Gregory
Ph.D. Thesis
Supplementary
Material

Journal of the Society for Psychical Research, 51 (1981) 195

Madam,

I do not want to continue a point scoring correspondence with Anita Gregory. If she does not agree with the methods used by the Enfield investigators, or with the opinions they express, that is her prerogative. I must add, however, that the principal eye-witnesses, Mr. Burcombe and Mrs. Nottingham, still do not accept the total accuracy of her case notes.

Guy Playfair and I believe that we conducted, in a conscientious manner, probably the most sustained and intensive investigation of poltergeist activity in recent times. Our methods may not accord with the views and disciplines of some of our SPR colleagues, but then we do not take it for granted that their approach to the matter is necessarily either correct or practical. We know that we adhered strictly to the truth in our reporting, making every effort to acquire as much witnessed evidence as possible in the way of recordings, signed statements, photographs and instrumental data. The signed statements, Guy's long detailed report, and the original report are in the Society's archives. The photographs and tape recordings are with us, the authors, and may be consulted.

Our case rests on our efforts. History will judge whether our evidence, gathered over months of hard and often tedious work, will endure, or whether the opinions of occasional visitors (even those labelled experts) will predominate.

This remarkable case taught me one lesson I will never forget. It is a lesson that has been confirmed in other cases I have investigated, namely: be as clinically scientific in your approach as you wish, but if you choose to play 'Hunt the Poltergeist'—'Confusion' is the name of the game.

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INVESTIGATING MACRO-PHYSICAL PHENOMENA

By Anita Gregory

There is an old Nasruddin teaching story whose anti-hero insists on looking for a coin under a lamp post in the street, not because that is where he dropped it (in fact, he knows that he lost it in his own unlit home), but because the light is better there. This tale is highly relevant to the question of experimental method in parapsychology generally, but especially in the case of the macro-physical phenomena with which I shall be concerned in this paper. I shall be concentrating on problems encountered in investigating some of the more large-scale effects usually associated with individual subjects, such as the movement of physical objects or sizeable deflections in experimental apparatus—what are usually known as “physical phenomena.”

These phenomena are the step-children of parapsychology, the most spectacular, the most ridiculed and happily jettisoned, the most readily dismissed and yet, ironically, in principle the most scientifically accessible manifestations of the paranormal. There is something more tangible about physical and material existence than about counter-chance bets. Either an object moved—in that case the question is whether or not someone threw it in some normal manner—or else it did not move and then the question arises why did people say it did? Were they lying? Deceived? Hallucinating? Did the recording apparatus malfunction?

The fact that so ostensibly simple a question has not been settled in well over a hundred years of experimenting, but remains a matter of fierce controversy, shows that there must be special difficulties in its resolution and I propose briefly to examine some of these.

In the investigation of the physical phenomena all possible approaches, methods and techniques need to be applied, modified or invented. There is no one single paradigm. To pursue the Nasruddin parable, we must investigate the coin where it is or where we can transport it as best we can.

In this conference we are asked to present our own approach to research and I will, therefore, illustrate this by reference to three cases in which I have been involved to a greater or lesser extent and which illustrate basic methodological issues in the three major contexts in which these are encountered: a domestically centered poltergeist case which I regard as weak; a well-documented mediumistic case history, partly domestic, partly laboratory based, which seems to me strong and a recent laboratory investigation which has not yet been published. I am using these as illustrations of method rather than as providing evidence.

The RSPK or poltergeist case is the “Enfield” case which has created a certain amount of stir in England.

Early in 1977 a poltergeist outbreak was reported in a council house in North London occupied by a Mrs. H. and her four children. There were stories of raps and noises and of objects moving about in the time-honored manner. The police and press were called in. Miss O’Keeffe, Secretary of the Society for Psychical Research, suggested to Mr. M. Grosse that he might like to look into the matter and he was soon joined by Mr. G. Playfair, a writer. I was not centrally involved myself, but went to the house as a fairly frequent visitor, the first time in company with Dr. John Beloff, but subsequently on my own or with others, often when neither Mr. Grosse nor Mr. Playfair were present. I also gave some help and advice to David Robertson, then an undergraduate first year physicist intermitting, for a year, who spent a fair amount of time at Enfield, among other things setting up video equipment to try to document the phenomena. After our visit to Enfield, John Beloff and I wrote to Mr. Playfair expressing our opinion that nothing had happened in our presence that required or even suggested any other than a normal explanation on that occasion, but we explicitly left open the possibility that genuine phenomena might have occurred at other times. I kept a journal of my own visits and circulated each installment within a day or so after each visit to a number of parapsychologists, including Dr. Beloff and Professor Arthur Ellison.

I wrote not only an account of what happened during each visit behaviorally, but also noted some of my own subjective and emotional reactions as honestly as possible, trying to combine the roles of observer and admitted participant. Inevitably such an account, in which one attempts to report very candidly one’s own reactions, must be confidential, at least those parts of it which contain the more personal features. It is quite possible to write such a journal in parts for differential circulation, which I did. Such an account could no more be for publication in full than the partly self-analytic case history notes of an analyst in training, which to some extent they resemble. Indeed, in order to preserve as much objectivity about my own reactions as possible, I also systematically discussed these with F.M.B., an analytical psychologist, a former principal psychiatric social worker at a London teaching hospital, with special expertise in the field of gifted children and who has also done a great deal of work with actors and singers, important in a case where alleged odd “voices” play a major part. I believe that this attempt at

Paper presented at the Parapsychology Foundation Conference on “Parapsychology and the Experimental Method,” held November 14, 1981, in New York, New York. Anita Gregory, a long-time investigative parapsychologist, is on the faculty of the School of Education, Polytechnic of North London, England.

disciplined quasi-analytical and introspective self-monitoring is a promising adjunct to empirical investigation, particularly in RSPK cases, where one is almost invariably precipitated into a disturbed human situation in which it is impossible, even if it were desirable, to maintain impersonal neutrality. Mental states, whether immediately accessible or more hidden, almost certainly play an important part, both in the occurrence of these phenomena and also in their appraisal by investigators.

Eventually Mr. Playfair wrote a book on the subject.¹ I reviewed it for the *Journal of the Society for Psychical Research*,² Mr. Grosse and I exchanged letters in the *Journal* concerning this review,³ a correspondence that may well not yet be at an end at the time of writing. This correspondence confirmed the usefulness of the device of writing and circulating accounts at the time, since Mr. Grosse, among other things, challenged some of my recollections.

I was not in this case directly engaged in an attempt to capture any phenomena instrumentally, except for transporting apparatus to Enfield for David Robertson. I was later shown a video film in which one of the girls is seen in her bedroom by herself, bending a spoon and metal bar in an all too normal manner and jumping up and down on a bed. To me this interpretation of perfectly ordinary, conscious and rather pathetic imitative trickery is irresistible. Yet in Guy Playfair's book the reader is told that video recording apparatus was set up so that the bedroom could be monitored without the girls' knowledge, but that the attempt was "a total flop . . . Janet hopped out of bed for no apparent reason and peered through the keyhole . . . saw [the TV monitor] and realized we were playing a trick on her. So nothing happened . . . We all finally decided that Janet had to get out of the house . . . She left home on June 16, 1978." But I had transported Robertson plus equipment to Enfield on January 15, 1978. Where was the recording I had seen taken? Why is there no mention of it in the book or Mr. Grosse's rejoinder to my review or his rejoinder to my reply? Why does Mr. Playfair himself not take issue with me?

The point I wish to make here is not that in my view a proven example of cheating by the subject disqualifies a case from serious parapsychological consideration. On the contrary, I firmly believe that the traditional SPR methodological stance "once a fraud always a fraud" is gravely mistaken, quite apart from being logically invalid. I would like to put at the very center of the stage the burden of emotional ambivalence that is part and parcel of the lot of the would-be objective and open investigator and which must be faced and shouldered if a worthwhile piece of work is to emerge. Anyone reading the correspondence in the *Journal of the Society for Psychical Research* can satisfy himself of the extreme pressure under which researchers such as myself are placed, somehow to overlook all the nonsense and to admit the excellence of a poorly researched and doubtful case in which there is, nevertheless, some good evidence and testimony. The investigating parapsychologist has to keep an extremely uncomfortable balance between doctrinaire skeptic and dedicated devotee and it

is quite difficult not to allow oneself to be coerced into either camp. It is not appropriate here to go into details of the interpersonal and inner conflicts involved, merely to draw attention to the fact that they exist and form part and parcel of the reporting of such cases and that all subsequent evaluation and testimony and, for that matter, instrumental recording must come to terms with them. Also, I have no doubt that this type of emotional pressure alienates scientifically minded would-be investigators and sympathizers.

"The investigating parapsychologist has to keep an extremely uncomfortable balance between doctrinaire skeptic and dedicated devotee and it is quite difficult not to allow oneself to be coerced into either camp."

Moreover, as I see it, the element of play-acting and trickery which is so frequently encountered in RSPK cases is not an epiphenomenon, a side-effect to be discounted and disregarded and which only a hostile and unreasonable skeptic would dwell upon; rather it is part of the important phenomenology of physical paranormality. It is to be taken seriously in its own right, if only because it is likely to shed important light on two quite vital as well as obscure issues: the psychological setting of such cases and the fundamental and so far totally unknown question of *how much* physical paranormality there is or might be in a universe in which there are physical laws or regularities.

The mediumistic case history I wish to refer to is that of Rudi Schneider, of which I have made an extensive study.⁴ As critical a parapsychologist as J. Fraser Nicol considers that, to this day, a strong case can be made out for genuine phenomena for this mediumship.⁵ It would be neither appropriate nor indeed possible here to review the entire history of Rudi, merely to highlight some of the features that appear to me to be of importance from the point of view of experimental method. Very briefly, Rudi was investigated in his native Austria as well as in Germany, Switzerland, Czechoslovakia, France and England in the 1920s and '30s. Documentation concerning him, both in manuscript and published form, is probably unrivaled and it is this which makes possible a combined literary as well as scientific exploration. Rudi was subjected to a very great deal of experimentation, ranging from the most amateur to the most scientific that the technology of the day would permit and the scientific issues raised are still of fundamental importance as well as being unresolved.

A mediumistic case which goes on over a long period of time is intermediate between a "spontaneous" poltergeist outbreak and a systematic experimental investigation. It seems to me that a physical medium might well be regarded as a temporarily socialized poltergeist focus, the element of socialization consisting of the recurrent ritual of seances and the habits that grow up around the production of the phenomena. Investigators have to become partners in this ritualized performance if they are to be able

to do any investigating and experimenting. The freedom they have to experiment is severely limited by the nature of the situation they are exploring, which is, of course, quite usual in the human sciences. All sorts of social and personal constraints govern, for example, a psychologist's freedom to experiment with children's performance in the classroom or a clinician's with his patients.

One important reason why Rudi was so thoroughly accessible to investigation was, no doubt, that Schrenck-Notzing, one of the noted psychical researchers of his day and a friend and colleague of Richet's, from the earliest days of Rudi's mediumship impressed both on the 11-year-old boy and his parents the importance of scientific control and proper and systematic documentation. There can be no doubt that this was greatly facilitated by the almost caste-like class distinctions of the day, which made the Herr Baron Dr. von Schrenck-Notzing's word law in the small-town artisan Schneider household. It was made plain to the boy that he must accept whatever control conditions experimenters might demand. So far as we know he never refused any conditions whatsoever.

However, at the seances which crystallized, Rudi's control "Olga" reigned supreme, speaking through his mouth in a hoarse whisper. "Olga" certainly did dictate, at any rate up to a point; she pontificated not so much concerning social factors which might be said to affect the mood of the meeting. One of the most recurrent themes of seance accounts is "Olga's" insistence that sitters should be cheerful (*lustig*), sing, recite, chatter, laugh and generally shed some of their inhibitions concerning sobriety and dignity. "She" frequently demanded light popular music, hateful to many of the researchers.

There is good reason to suppose that a light and boisterous group mood is necessary (though certainly not sufficient) for the production of physical phenomena and this undoubtedly presents problems from a methodological point of view. Very careful prior preparation and planning are needed if a general atmosphere of uncritical jollity is not to interfere with accuracy and thoroughness of observation and experimentation. Moreover, there is no reason to suppose that extraverts, who do not mind singing solos to order whilst holding hands with colleagues and strangers, necessarily make the most meticulous and scrupulous experimenters. The late Harry Price, for whom I cannot be accused of cherishing any unqualified partiality,⁶ was by all accounts thoroughly "psi-conductive." Greater, not less care must go into the planning of apparatus, research protocol, etc., than in the context of normal laboratory research, where abandoned hilarity and excitement are not expected as part of the scientist's expertise and stock in trade. Yet, it seems almost certain that something like this needs to be created if major physical phenomena are to be hoped for. It is also plain that researchers must cooperate with whoever or whatever person produces the phenomena and relate to them in a manner likely to elicit cooperation. To do so is one of the human arts necessary for the competent pursuit of the social sciences, yet less time is devoted to this question in parapsychology than it deserves. The subjective is apt to

be swept under the tables for the sake of the semblance of "scientific objectivity."

Mention has been made of the wealth of documentation in this case study. I have in my possession, through the good offices of the later Dr. Gerda Walther and the generosity of Mrs. Mitzi Schneider, Rudi's widow, the journals kept by Schneider senior, two dog-eared exercise books in fading, now archaic "Sütterlin" script, referring to 269 sittings between September 8, 1923 and January 1, 1932, signed by, so far as I could decipher, 796 different persons. It is possible to subject a record such as this to a certain amount of quantitative analysis, precisely because of the ritualized nature of the proceedings and the orderly and regular way in which records were kept in this case: Such analysis and evaluation of primary sources is, I believe, of vital importance for the progress of parapsychology, not only for elucidating past happenings, but also and above all for suggesting working hypotheses and improved records for future investigation. Such analysis should be thought of as, so to speak, paper and pencil (and possibly computer) experimentation.

"... I firmly believe that the traditional SPR methodological stance 'once a fraud always a fraud' is gravely mistaken, quite apart from being logically invalid."

It was possible to group phenomena into types. The categories I eventually chose were movements of objects, visible materializations, levitations of the medium's whole body and reports by sitters that they had felt themselves touched. These categories were in a sense dictated, or at least limited, by the records. I would very much have liked to have added reports of "cold air," for example, and some indication of the intensity and frequency of phenomena. However, the records were not sufficiently systematically explicit on these points.

By preparing tables of the data given in accounts of sittings, one can trace what types of phenomena were reported as occurring at different times, in different circumstances and places and in the absence and presence of certain persons. It becomes plain that seances were far more varied in the presence of certain sitters, that no single sitter was necessary, however, for any given type of phenomenon to be reported, that the presence of no given person guaranteed any particular phenomenon and that there were answers to many other questions which it would be impossible to answer without such painstaking quantitative analysis.

It emerges clearly from an analysis of this type that quantification is one tool among others and a very useful one for promoting understanding, examining characteristics of situations and discriminating between hypotheses.⁷

The Schneider investigation bridges the gap between classical seance accounts of phenomena and modern instrumental recording and documentation. Perhaps the most interesting feature of the case is Dr. Eugène Osty's brilliant utilization of ostensible instrumental malfunction-

ing. He had devised an infra-red burglar alarm-style system as an anti-fraud precaution, guarding the objects to be moved. This device kept signaling—ostensibly malfunctioning—when nothing visible had, in fact, entered the beam and Osty realized that the interference with infra-red radiation could itself be viewed as the principal paranormal phenomenon to be studied. The episode is a clear instance of the adage that chance favors the prepared mind; a lesser man might have simply decided that the infra-red control system was too much of a complicating nuisance and discarded it. However, he used the device to obtain instrumental records of Rudi's (by that time) declining mediumistic prowess. He demonstrated his more human skills to obtain "Olga's" whole-hearted collaboration in a set-up where "she" tried to "go into the beam," increasing only on a pre-arranged signal such as a count of five or ten and where differently located beam set-ups showed that "she" could localize her interference. He also based upon these results one of the few important working hypotheses in the realm of the major physical paranormal phenomena, namely, that these phenomena are produced by a form of matter invisible in white light, but detectable by infra-red radiation.

It is one of the problems of parapsychology that there is apt to be little continuity in investigation, compared with the degree of systematic follow up, replication and cross checking in normal science. The reasons for this are various, ranging from the relative economic poverty of the subject, *via* the idiosyncratic nature of researchers, to the instability, plasticity and unreliability of the phenomena. Still, it is surprising that so little systematic effort was made to attempt to replicate the Osty⁸ and Hope-Rayleigh⁹ infra-red effects in the case of other claimants to physical paranormality.

Such an attempt was made, ostensibly with some success, in the third case I mentioned earlier, namely, in the course of the SPR investigation of Matthew Manning, which I convened at City University, London, in the summer of 1978. My own primary experimental aim was to attempt to replicate Rudi Schneider's infra-red effects. The rationale was as follows: here was a young and still active psychic, who had started as a poltergeist focus, for whom very strong macro-physical phenomena had been claimed, which had by 1978 largely, if not wholly, vanished. In Rudi's case, the IR effects had persisted when gross PK movements had virtually ceased. It was (and remains) my working hypothesis that some vestigial instrumentally recordable effects linger on after overt gross movements have ceased and that such vestigial effects may well be far more abundantly distributed among the population than is usually supposed, even when no gross movements have ever been manifested. I had much earlier asked the late Mr. C. Brookes-Smith, an instrumentation engineer, to construct IR apparatus similar to that used by Osty and, fortunately, this was available when Matthew approached me in the spring of 1978 and asked to be investigated.

Dealing with a sophisticated late 20th century international psychic star subject, one, moreover, who works in

the waking state, is very different from dealing with a relatively uneducated trance medium of the '20s and '30s. On the other hand, it is distinctly helpful to work with a highly intelligent subject like Matthew who can contribute his own ideas as to what he did and did not wish to do and who would leave one in no doubt as to what he did and did not like. Matthew was quite willing to try and humor me, for instance, as regards the infra-red, whilst making it plain (before it ostensibly worked) that this was of little or no interest to him. He was by this time keen to do experiments with biological targets such as plants, animals and samples of blood. He felt he had outgrown mere physical displacement of objects and that he had, in a sense, cured himself of physical phenomena by means of his rather exquisite automatic drawings, purporting to be by deceased artists. This self-observation may well be of considerable interest and could be a perfectly useful illustration of something rather like the Freudian concept of sublimation.

The investigation took place at the City University's Bio-Electricity Laboratory in the Department of Electrical and Electronic Engineering, whose head is Professor A. J. Ellison, President of the SPR, who participated in and contributed to the experiments. It is impossible, as well as inappropriate, here to summarize activities and findings more fully described elsewhere¹⁰ beyond illustrating the topic of the present conference, namely experimental method.

"Mental states, whether immediately accessible or more hidden, almost certainly play an important part, both in the occurrence of these phenomena and also in their appraisal by investigators."

Every attempt was made to meet, as far as possible, Matthew's own wishes. In particular, three experiments were specifically planned to comply with these, namely, a "poetry experiment" in which snatches of verse were complexly sealed into envelopes for him to illustrate psychically (A. Gregory); a "bean experiment" to see if he could affect the growth of shoots (M. P. Barrington) and a "hemolysis experiment" to replicate an effect claimed in Texas by William Braud using more sophisticated and rigorous methods (W. Byers Brown). I made an attempt in the earlier stages to adapt my infra-red experiments to Matthew's preferences for biological targets, by placing growing plants in the beam, so that any "influence" from Matthew would have to cross the beam at least partially to reach the target. Other experiments included attempts to influence a very delicate pendulum (A. J. Ellison) and the clairvoyant, or else out-of-the-body, viewing of a sequence of figures on a random event generator (A. J. Ellison).

The experiments were deliberately planned in a manner not wholly dissimilar from the organization of a primary school day in a reasonably "child centered" classroom. In other words, there were a number of activities Matthew

could do as and when he felt like it, whilst others required a more rigid setting and time-table. Like all such activity methods, a great deal of preparation is needed in advance if free choice and flexibility are not to degenerate into a chaotic shambles. The most time-consuming experiments requiring the most detailed and disciplined timing and cooperation were without a doubt the hemolysis experiments involving a first experimenter (WBB) and a second experimenter (AG). These experiments (which did not yield positive results) involved a certain amount of what might be thought of as repetitive ritual, which provides both constraint and irritation on the one hand, as well as a certain sense of security and holding together of sessions on the other. At the other extreme were the "poetry" envelopes, which Matthew could do on demand.

In the event, the positive effects in the infra-red rose out of a context of hemolysis and poetry experiments and possibly Matthew's (and probably not only Matthew's!) irritation with experiments and colleagues. Whilst he was being kept waiting (which he very much disliked) for a hemolysis experiment and was attempting some poetry experiments, the interpretation of which caused a certain ill-concealed friction between various members of the investigating team, myself included, Matthew addressed himself to the digital volt meter, which signaled strong deviations from the base-line of the IR beam, whilst the chart recorder traced corresponding deflections. Nothing had happened at earlier sessions, when Matthew had consciously tried to influence *Letidium Sativum* (cress) in the beam.

The IR equipment, with its meters and chart recorder, was permanently set up during all sessions, as was audio equipment, video recordings being made during some of Matthew's attempts to influence the infra-red. Professor Ellison's staff, particularly Mr. D. Chapman, his chief scientific officer, changed and monitored power sources during such attempts, to make sure that the instability was not due to fluctuations of the sources. Members of the team read aloud the digital volt-meter readings which corresponded closely to the trace of the chart record. It is, therefore, unlikely that Matthew influenced meters and recorder directly and it is also, in view of the extreme care and considerable expertise of the engineers involved, reasonable to believe that the effects obtained were paranormal. It was not, however, possible to be quite certain that it was the infra-red that was affected, as opposed to the production of some paranormal electrical effects. There was, unfortunately, no mechanism for isolating the infra-red from the rest of the circuit and not time for effecting such a change.

Although in the case of the Rudi Schneider phenomena it seems most plausible to suppose that the IR was in fact affected by some proto-material substance, for the time being we cannot be certain that this was so in the case of Matthew Manning, although it seems that physical paranormality of some sort was probably present. Different modes of action are almost certainly involved in different psychokinetic effects, possibly by following some as yet obscure law of least effort.

At first sight it looked as if the record of the (ostensible) occultations of the infra-red beam in the presence of Matthew Manning could be divided into "episodes." It was hoped that these might be analyzable in terms of different factors obtaining at different times, such as who was present, what records (e.g., video, audio, etc.) were in use, so that different "profiles" might be compiled for episodes in a manner analogous to the characterization of Schneider sessions. On closer analysis it turned out that division into "episodes" would impose a spurious method of classification on the records, and that even the appearance of "episodes" is absent during some sessions.

It was also found that no very close timed coincidence between audio and chart records was possible, although there is reasonable over-all correspondence. It became clear that if such timing is deemed desirable, then reliable automatic synchronizing apparatus is essential.

"Either an object moved—in that case the question is whether or not someone threw it in some normal manner—or else it did not move and then the question arises why did people say it did?"

Although there can be no doubt that an automatic audio record is a considerable improvement on the earlier secretarial seance record, new difficulties arose. Not only is total transcription costly and time-consuming, there is in addition to the timing problems already mentioned the difficulty that interpretation of the audio record is often ambiguous, especially where participants spoke softly or far away from the microphone or, as often happened, at the same time. Also, qualitative factors, obvious when listening to the recording, are apt to be lost in transcription. Moreover, the auditory record must be treated with considerable discrimination, since obviously not everyone will at all times accurately express exactly what he thinks the moment he thinks it! The auditory record, therefore, although it is an invaluable aid and has considerable evidential and corroborative value, must not be over-estimated as a methodological tool in interpreting data. I believe that our best hope lies in continued cooperation with psychics and/or groups of experimenters in which previously prepared systematic protocols and precisely timed automatic recordings can be combined with spontaneous interaction after the manner of a game which, from its very nature, is subject to rules.

It would seem to emerge from this brief survey of three cases characteristic of the three main types of setting—home, seance and laboratory environments respectively—that investigative and experimental methods are, at any rate for the present, similar in principle. Testimony is required not only for the domestic and seance situation, but is also appropriate for the laboratory setting. Self-analytical and introspective reports, both by subjects and experimenters, may I believe be of importance in all settings, although the difficulties here are obvious and classical; not only a buoyant mood, but also tensions between partici-

pants and their effect on the subject may well be highly relevant, if embarrassing. Instrumental monitoring, which is clearly easier the more nearly a situation approximates to a laboratory context, is at least ideally part and parcel of the investigation in all settings. Visual and audio-recordings and chart recordings where some measurable variable is being monitored are at all times desirable.

Lord Kelvin once said "When you can measure what you are speaking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."¹¹ This, as is frequently claimed, represents "an expression of the scientific attitude." It is, however, as I see it, a very partial, meager and unsatisfactory approach to knowledge and understanding. Quantities and numbers are indeed important and indispensable aspects of its pursuit and no one engaged in parapsychological research would wish to deny this. These characteristics abstracted from the world, however, are always and at all times subject to interpretation and incorporation in some semantic fabric, however imperfect and provisional, if they are to have any relationship to human understanding. No form of record, automatic or other, can ultimately replace the selecting, conceptualizing and imaginative as well as, for good or ill, fallible human observer and interpreter. In the last resort, the adequate pursuit and practice of science is an art.

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PA Elects

New Parapsychological Association officers are John Beloff, President; Stanley Krippner, President-Elect; Rhea White, Secretary and Charles Tart, Treasurer. Remaining members of the PA Council are Robert Morris, John Palmer, K. Ramakrishna Rao, William Roll and Robert Van de Castle.

Death Notice

Tony Agpaoa, most well-known of the controversial Philippine "psychic surgeons," died January 23, 1982, at the age of 43. "Dr. Tony" supposedly could perform operations with his bare hands, using his fingers to make incisions, removing diseased organs and closing the wound without scarring. Critics contended that the procedure was done by sleight-of-hand and that organs said to be removed from the body were in fact animal parts.

Psi Research Machine

According to an article in the *Journal of the American Society for Psychi-*

cal Research, W. J. R. Dunseath, F. F. Klein and E. F. Kelly have designed a field instrumentation package for the study of psychophysiological variables in psi. The portable package is based on an FM/FM radiotelemetry system with which psychophysiological data are broadcast from a small device attached to the subject to a nearby receiving unit. It is said to avoid technical problems that caused errors in previous telemetry applications. The article outlines future developments and uses for the system.

Parascience Conference

August 10 to 15, 1982, the International Parascience Institute held its 12th Annual Conference at the University of San Francisco, California. Experimental and theoretical aspects of psi research were covered in the categories of consciousness, cognition and the brain/mind question; extra-sensory perception, psychokinesis and psychic healing. Workshops, review and discussion seminars and experiential reports were included.

PRG Lecture

May 24, 1982, the Parapsychology Research Group, San Francisco, California, presented Patric Giesler in a lecture entitled "A Multi-method Investigation of Afro-Brazilian Psi." He discussed his recent year-long investigation of psi and psi-related activities among the three prominent Afro-Brazilian shamanic cults of São Paulo and Bahia in Brazil. The need for combining and integrating ethnographic and experimental approaches to psi studies in field settings was emphasized. Slides of the cults' ceremonies and psi-relevant activities were shown.

Death Notice

We have just heard that Colin Brookes-Smith died in England, March 3, 1982 at the age of 83. He was an associate of the Churches' Fellowship for Psychical and Spiritual Studies and a member of the London Society for Psychical Research. An instrumentation engineer, he was noted for his experiments and studies in psychokinesis and paranormal electrical conductance phenomena.

Journal of the Society for Psychical Research,
52 (1983) 92-95

A. Gregory
Ph.D. Thesis
Supplementary
Material

To the Editor,

Anita Gregory in a paper presented to the Parapsychology Foundation Conference in 1981, and subsequently published in *Parapsychology Review*, September 1982, cites further 'questionable' evidence in the Enfield Poltergeist Case. I stated in a previous letter to the *S.P.R. Journal* that I did not intend to take part in further controversial discussion about the case, but I cannot allow her latest criticism to pass without a reply.

It concerns a video tape produced in January 1978 in the house in Enfield, in which 'one of the girls is seen in her bedroom by herself, bending a spoon and metal bar in all too normal a manner and jumping up and down on the bed'. Anita wishes to know why this tape was never mentioned in my rejoinder to her review. I have news for Anita Gregory. Not only do I continually refer to the children's antics in my lectures, but also to the other tape David Robertson and I made which shows both of the girls indulging in tomfoolery in their bedroom while David and I were recording in the boxroom. Both these videos were made after we asked the girls to produce levitation and metal bending 'to order'. We could not get the girls to take the tests seriously, and I remember saying to David at the time, 'You had better not let anyone see that rubbish or someone will take it as serious evidence'.

I have not taken issue on the point before, as I did not think it was really possible for anyone to take these antics seriously, but obviously I was wrong. The children, in January 1978, were perfectly aware they were being video taped, and no attempt was made, as in June 1978, to disguise the camera. Many attempts at video recording were made, and discarded, because they presented no positive evidence. It is possible that Anita considers the present fashion for negative

evidence very important, but I can assure her that my priority has always been in the opposite direction.

She calls the Enfield Case 'a poorly researched and doubtful case in which there is nevertheless *some* good evidence and testimony'. Those patronising words perhaps say more than any reply I could make to our critics.

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Anita Gregory replies:

I am more than a little surprised that Maurice Grosse should, over two years later, choose to re-open the issue of the video-tape on which he failed to comment when he replied to my review of Guy Playfair's book^{1,2}. Even now he does so in ambiguous terms, quoting himself to the effect that it would have been wiser 'not to let anyone see that rubbish or someone will take it as serious evidence'. If there was a simple answer, why the years of silence, and why does Mr. Grosse now refer to 'the other tape'—not the one I am talking about? It would also be interesting why he refrained from referring to this videotape at his talk to the SPR on 10 September 1981 which I chaired.

There are two issues, not to be confused, the 'video-tape' and the 'antics' questions.

As regards the videotape, the question-mark hangs over an earlier tape, presumably the one shown me by Prof. Hasted, on 11 January 1978 according to my diary, showing Janet merrily cheating away and giving not the slightest indication that she was aware of being filmed. On that occasion Prof. Hasted asked me to ferry David Robertson to Enfield, as well as video-equipment. This I did, on 14 January 1978.³ David, then an undergraduate physics student who had completed one year, installed the video equipment in what Mr. Grosse calls 'the boxroom'. In this he was assisted by a friend of mine, Mr. David Line, an electronics engineer who came along at my request. (Mr. Line, whilst being satisfied with Dave Robertson's competence with respect to the installation of the video equipment, was entirely unconvinced by the alleged paranormality of what was happening at Enfield.⁴

Now prior to this, on 31 December 1977, at a demonstration of 'psychic painting', Mr. Playfair told me over and over again how he had *now*, by means of a video camera, caught the girls cheating. He made a very heavy production indeed of this episode (my line being that it didn't take a video camera), and told me, against anguished protests from myself, that he would now insist on Janet permitting herself to be hypnotised: he appeared to be under the impression that this would infallibly uncover the truth. On January 3rd 1978 there was another demonstration of 'spirit painting' at Acacia Hall, West London, and on that occasion Mr. Playfair and Mr. Grosse told me they had caught the girls cheating by video. They repeated these accusations when talking to Mrs. M. Branch, to whom I introduced them. She 'vividly' remembers how proud they both were at their own prowess as detectives and researchers for having caught the girls in trickery by means of video equipment.

In Playfair's book the impression is conveyed (and I quoted a long passage in

my review) that the bright idea of using a videotape had only just occurred to Grosse and Playfair in June 1978, and that there was something quite remarkable, perhaps even paranormal, about Janet's spotting the video camera in June 1978. This seems to me gratuitous mystification since video cameras had been at Enfield off and on since December 1977. (There was one there in December, in January, Prof. Hasted took one in April, there was one installed in May when I was there, and there seems to have been one there in June!) Mr. Grosse now admits that he and Dave Robertson had been playing at recording foolery by means of a video camera since mid-January 1978! The notion that the girls might have been surprised, in that tiny house where video equipment had been hanging about for six months, and with all those trial runs, is entirely preposterous. The whole matter needs clarification also in the light of Mr. Melvin Harris' devastating analysis of the Enfield photographs.⁵ I understand that, when Mr. Harris asked Mr. Playfair about the video film mentioned in my review, Mr. Playfair replied that he knew nothing of any video film.

As regards 'antics', it cannot be stressed enough that it is only *now* that Maurice Grosse talks indulgently of 'antics' on the part of the girls. He seems to have forgotten that he did not do so at the time. Indeed, both he and Mr. Playfair took John Beloff and myself severely to task for suggesting that *any part* of the tragi-comedy acted out at Enfield was antics and play-acting. I certainly repeatedly heard Mr. Grosse solemnly admonishing Janet that if he *ever once* caught her cheating, he would *never* come back. It is Mr. Grosse and Mr. Playfair who took it all, absolutely all of it, dead seriously at the time, indignantly repudiating any admixture of pranks. The indulgence of the 'antics' interpretation is sophistication of more recent vintage forced upon Guy and Maurice by, I submit, primarily myself.

I well remember saying in front of some 150 people at the March 1978 SPR Conference in Cambridge⁶ that the word 'fraud' was not applicable to the case: it was a matter of children play-acting. Mr. Grosse was so pleased with this that he withdrew some of the vehement allegations he had made, and Mr. Playfair complimented me on my good sense, both in Cambridge and later after a meeting on 8 April 1978. I never in the least took the children's antics at all seriously, nor did I inhibit phenomena Mr. Playfair and Mr. Grosse believed to be paranormal. Nor did the children take my friendly scepticism amiss: they allowed me into their bedroom while 'tomfoolery' was going on, whilst excluding true believers like Grosse and Playfair. Let us try not to re-write history more than we can help.

From the point of view of psychical research, the whole issue was and is this: was there ever any decent evidence of paranormality at Enfield at all? Most of it is pathetic. The SPR's working party presented a long and composite dossier and report,⁷ from which it becomes clear that virtually the only eye-witness testimony to anything paranormal in addition to the family, neighbours and relatives, was that of Mr. Grosse, Mr. Playfair, when it came to the crunch, claiming to have witnessed relatively little. Mr. Grosse quotes me quite correctly as having said that the Enfield case was a 'poorly researched and doubtful case in which there is nevertheless some good evidence and testimony'. Having once more gone carefully into the evidence, I feel, alas, that this was an over-enthusiastic estimate. Allow me to give an instance.

The 'good testimony' I had in mind was that of Woman Police Constable Carolyn Heeps to the effect that she had seen a chair move of its own accord and satisfied herself that this could not have happened in a normal way. Her testimony, as coming from outside the charmed circle, and relating to the very night of the onset of the case, is crucial. I rang up Miss Heeps on the morning of 5.11.1982. She instantly remembered the whole case, and volunteered various correct details. She said, without the least prompting from me, that at the time she had thought the children or their friends had done it all, but later 'Dr. Grosse of the Psychical Research Society' had told her that he and various media people had 'proved' it could not have been the children. She provided more detail and repeatedly assured me that *now* she herself believed it had not been the children since 'Dr. Grosse of the Psychical Research Society' had 'proved' it could not have been—originally she had thought otherwise.⁸ The incident took place in the early hours of 1 September 1977, WPC Heeps' statement is said to have been signed 10 September, 1977⁹ and Mr. Playfair first went to Enfield on 12 September 1977.¹⁰

Nobody doubts Maurice's good faith and kind intentions, certainly not I, although he apparently considers me sufficiently lacking in courage or honesty or both to bow to 'fashion' rather than objectively and truthfully to consider the evidence. Anyone, including myself, can be mistaken or self-deceived. So, to put the record straight: I think there is some quite good evidence for PK before Enfield, and even probably since Enfield, and I have published my views to that effect. The Enfield case, however, unfortunately withers away on closer inspection.

Yours sincerely
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In the minds of millions, Matthew Manning, London: W.H. Allen, 1977, 169 pp. £3.95.

"*In the minds of millions*", writes Matthew Manning, "is intended to be a record of the experiences shared with scientists, journalists and people in the public eye—events which can be corroborated by those scientists and journalists and individuals" (p. 160). The book depicts some of this young psychic's encounters, as seen by himself, with people of all sorts—ecclesiastics, TV personalities, academics, and his reactions to them.

As he quite rightly says, it is not his job to devise watertight experiments or to evaluate the findings of pieces of research; so, if the overall effect is that of an international psychic whistle-stop tour, Matthew cannot be held entirely responsible for the impression of breathless chaos intensified by paranormality. Quite the reverse: it is of the greatest interest to have the reactions of a very unusual and a very shrewd young man to the conduct and the human limitations of those who are supposed to investigate *him*. Far too little attention is paid to the feelings and experiences of "subjects" of parapsychological (and for that matter of psychological) experimentation and, read with understanding and discrimination, the book should be extremely illuminating and helpful to researchers.

There are some interesting incidental observations, for example that he works better in the evenings; that "sending" is more successful if drawings and plans are prepared well in advance; that diagnosis, though ostensibly paranormal, may refer to the patient's idea of what is the matter with him rather than to the correct diagnosis. The book also contains one really important chapter, "Harlequin out of love", which should be read and pondered by all who are concerned with parapsychology and mental illness, and which more than compensates for the padding elsewhere.

It is very much to be hoped that there will some day, in the not too distant future, be a systematic and sensitive investigation of Matthew Manning, and a monograph to match. Meanwhile, Matthew's own set of sketches is a worthwhile contribution.

Anita Gregory

A. Gregory

Ph.D. Thesis
Supplementary
Material

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PROCEEDINGS
OF THE
SOCIETY FOR PSYCHICAL RESEARCH

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LONDON EXPERIMENTS WITH MATTHEW MANNING

Edited by Anita Gregory

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1

INTRODUCTION

ANITA GREGORY

Biographical details in this introduction are mainly based on conversations with Matthew Manning and his parents and on Matthew's books.

The following conventions are used to indicate the source of the information: (T1) refers to a taped interview with Mr. and Mrs. Manning and Matthew, 12.11.1978. During virtually the entire investigation described in this paper, tape recordings were made in the main experimental area at the Bio-Electricity Laboratory, City University, London. These tapes were labelled A1/1, A1/2, A2/1, etc., and references marked in the form (A1/1) denote these recordings. In the Introduction references to Matthew's own books are indicated by an initial letter followed by the page number; thus (L3) refers to *The Link* page 3, (M3) to *In the Minds of Millions* page 3, (S3) to *The Strangers* page 3.

The investigation of Matthew Manning, born 17.8.1955, to be described took place in the summer of 1978, after he had written to me earlier that year offering his services as a subject of research. He has published three books about his experiences to date (Manning 1974, 1977, 1978). What follows is based on these. Terms such as 'alleged' are avoided and should be taken for granted.

OUTLINE SKETCH OF PHENOMENA

Poltergeist phenomena began in the Manning household on 18.2.1967 when Matthew was 11, his sister Rosalind 8 and his brother Andrew 6. The onset was marked by the displacement of a tankard from a shelf, and of a vase of flowers on a table. The phenomena increased in frequency and intensity, being at their height between 7 and 7.30 am. Knocks, other unexpected taps, creaks and a batlike 'pinging' were heard in the modern detached house, doors would open and shut, and objects of varying size flew about. At about Eastertime 1967 a mislaid rubber that slowly rose in the air and gently floated down and landed beside its owner, Matthew's sister, seems to have been the only incident to inspire acute terror during this phase, and it marked the end of manifestations for the time being. Dr. A.R.G. Owen, who had been called in, expressed the view that the happenings had been genuine, and that Mathew was the most likely source on the grounds of age, although there was no other factor to indicate this especially (L25).

After this Matthew took Common Entrance examinations and went to public school. In 1968 the family moved to beautiful house built and extended between 1550 and 1730, where there were no ostensibly paranormal incidents for some time, at any rate no extended ones. Matthew gives the impression that he thought there was a long and entirely blank gap so far as the paranormal was concerned. There do seem, however, to have been some stirrings during this time. In his third book he says that the family had become accustomed to 'odd happenings' ever since they had moved to this house (S11). Also late in 1968 there was a 'craze' for seances at Matthew's school, and phenomena seem to have been more prolific when he was a participant (L26). However the seances were abandoned when, because as quite often happens in schools, all concerned became scared.

Disturbances started up again in the home in July 1970 and gradually increased in intensity. There were footsteps and raps, opening of cupboard doors, boots were thrown about, cushions began to take on a life of their own, and so forth. This time the manifestations clearly centred around Matthew, but he says that his parents for a considerable time simply declined to believe him. He felt safe only at his boarding school. By Easter 1971 the phenomena had become so violent that one night Matthew refused to sleep in his own room 'until something was done about it' (L35). The dinning room looked next day 'as though a bomb had hit it' (L36).

In the summer term of 1971 the disturbances finally followed Matthew to his school, where there was genuine concern that the upheavals in the dormitory could endanger pupils' O-level studies. The headmaster, matron, fellow students, as well as Dr. Owen (who was again consulted, this time by correspondence) were convinced of the genuineness of the happenings, and it was with some difficulty that the headmaster was persuaded to keep Matthew at the school. Levitating beds, flying objects, the formation of pools of water, hot spots on walls, upturned book cases and the appearance of knives of unknown origin, were among the episodes of havoc that occurred.

In May 1971, during a week-end at home, Matthew had his first experience of 'hearing' a spirit voice — one 'Henrietta Webbe' — and in June 1971 he first found himself writing automatically in a handwriting quite different from his own whilst trying to prepare a pre-O-level essay. In the following months he increasingly engaged in automatic writing, which purported to come from numerous departed persons, and concerning the origin of which Matthew himself is admirably cautious. There is some suggestion of paranormal content, and indications that Matthew was also beginning to acquire some other psychic accomplishments at this stage. In my view it is of special significance that by means of deliberately developing his automatic writing Matthew seemed

to be able to attenuate and eventually abolish the poltergeist manifestations.

In June 1971 there was the first communication by way of automatic writing from 'Robert Webbe', who was to become a frequent and garrulous communicator over the next years, and who as late as May 1977 'compelled' Matthew to communicate with him (S120). In July 1971 names began to appear on the walls of Matthew's room; virtually all the panels and much of the ceiling of which are covered with names and dates. Matthew and his family estimate that the bulk of these appeared in 1971, and that this activity has now virtually ceased.

On 2 November 1971, while Matthew was home for half term, his mother suggested he should try his hand at automatic drawing, calling for inspiration on the spirit of Sir Alfred Munnings, since it was believed that Matthew's limited skills as an artist would make sure he could not himself draw a good horse. He did produce a horse of apparently no great artistic merit, but still beyond his own abilities. This started a new phase; Matthew's automatic drawing activities in the style characteristic of large numbers of deceased artists, among them Dürer, Picasso, Beardsley, Keble-Martin, Rowlandson, Leonardo, Beatrix Potter, Goya and Klee. The drawings certainly look very accomplished, and were done at some speed. These continued until 1975 and have since ceased.

Later in November 1971 Matthew had his first actual 'visual' encounter with 'Robert Webbe'. During the winter of 1969/70 Matthew had been engaged on an O-level history project on the Webbe family from contemporary sources held in the Cambridgeshire Public Records Office. He selected the Webbes from the names predominant in the documents because the name 'John Webbe 1731' was scratched into a brick of the house, and it was known that a Webbe family had once occupied it. The Webbes turned out to be reasonably, but not by any means amply, documented in local records. In this school record project Matthew had described how he would imagine a typical 18th century gentleman to look, and this was strikingly different from the apparition he experienced in 1971: this convinced Matthew that what he had seen was no mere hallucination.

From the summer of 1971 poltergeist phenomena disappeared, and Matthew engaged more and more in automatic drawing and writing. Early in 1973 he came to the attention of Mr. Colin Smythe and Mr. Peter Bander, directors of the publishing firm Colin Smythe Ltd. Messrs. Bander and Smythe at that time contacted me with the suggestion that I should take charge of Matthew, investigate and help him to write a book about his experiences. However nothing came of these plans. In fact my copy of *The Link* kindly presented to me by Matthew years later, is inscribed 'The book you nearly wrote with me'. Matthew started a career as a writer, and he visited a large number of countries

and institutions at somewhat breathtaking speed to demonstrate some of his phenomena.

In January 1974, following Uri Geller's demonstrations of metal bending, Matthew followed suit and bent numerous metallic objects in front of large numbers of witnesses. He came to dislike this activity and soon discontinued its practice. He has visited Canada, the United States, Sweden, Japan, New Zealand, Germany and the Netherlands. Matthew is now active mainly as a healer (see Postscript).

PERSONAL REFLECTIONS BY AND ABOUT MATTHEW

Matthew's earliest memory (A2/1) is at the age of 18 months sitting in a high chair at the side of some french windows (which details apparently fix his age), 'refusing to eat chicken because it was cruel to eat birds'. He did not, until his poltergeist episodes, feel himself to be different from other children. Mrs. Manning had an extremely severe electric shock while she was carrying Matthew three weeks before his birth (T1, A2/1). She recalled an occasion when, at the age of 7 or 8, his life was endangered as, during a seaside holiday he was cut off by the tide. Matthew himself remembers this incident as terrifying especially because he found himself surrounded by fishes which frightened him. He has had recurring nightmares about being cut off by water with fishes swimming in it ever since (T1).

Matthew also recalled during the interview in November 1978 nightmares he had had at 10 or 11, when a boy died at his prep school. His parents could not remember his ever telling them this before. Matthew said:

I'd suddenly wake up in the middle of the night and I'd suddenly see somewhere probably within three feet of me, though it's difficult to gauge the distance in the dark, I'd see a disembodied face somewhere close to me and I knew that I was awake because if I close my eyes the face disappears but I get a kind of direct communication between whatever it is straight into my head, and I can communicate with it. Some of the faces I recognise and some of them I don't.

The passage is interesting for a number of reasons. It shows Matthew's sophistication and detachment vis-à-vis his experiences, it is a clear description of experience, and it indicates that these visions have continued since, and the original link with a death might also be of importance.

Mrs. Manning thought of the near-drowning episode as having left a

particularly deep impression. Mr. Manning recalled another incident which he felt to have been of special significance, and I particularly appreciate his candour in this matter; which of us, who are parents, cannot think of things we would rather we had not done? In the incident in question, Andrew did something which, as Mr. Manning said,

incurred my extreme displeasure and I belted him for all I was worth. I think I really did lose my temper and perhaps lost control of him and myself as well, but Andrew was remarkably resilient and it went over him, and he came up smiling and he'd beaten me at the end of it. But Matthew nevertheless I remember was extremely distressed as if it had all happened to him and he'd experienced it personally, and I have never seen anybody so distressed as Matthew at the fact that his brother was given a hiding.

The reason for citing this is that Mr. Manning strongly felt that the incident may have been important, and we know so little of these matters we cannot afford to ignore his feelings. Moreover, Matthew himself had totally forgotten it, and since we were able to narrow down the time to when Matthew was 10 or 11, this seems somewhat unusual. At first what precisely had been Andrew's misdeed seemed forgotten, but Mr. Manning finally recalled that it involved something being thrown down the stairs, probably a tray, with such violence that it smashed a plate glass window. Matthew, when he recalled the forgotten incident, said that he thought Andrew had slipped on the tray. 'I remember the window being broken but not him doing it'. Mrs. Manning contributed that she thought it was just stupidity, but Mr. Manning was certain it was deliberate. Now according to Matthew and his parents quite a substantial number of incidents of poltergeisterei in fact centered around things being thrown about on the staircase of the house near Cambridge, which was all the noisier since during much of the time there was no carpet down, and indeed the first 'Webbe' apparition took place on the staircase. The original incident involving Andrew happened in the earlier home of the Manning family in Shelford.

Matthew was due to take examinations at the time of both poltergeist outbreaks. He himself now regards this as irrelevant, as he was not 'bothered one way or another' although he claimed that 'compared with other people at school you [his parents] were far less interested in what I was doing at school than most parents' (T1). He thinks that at some unconscious level he was eager to disrupt his parents' structured existence 'without being for it' (T1 and A1/1). In particular, Matthew

and his parents agree that there could have been a strong element of defiance of Matthew against his father, whose special favourite tankard was the first object to be displaced. It might also be of interest that 'Webbe' disputed the Mannings' ownership of the house.

Matthew is generally held not to have any talent for drawing in his normal state, nor did he ever show any promise at school in this respect. He never took art lessons since these clashed with Latin. He does however feel himself to be 'an artistic person by nature' (T1). He liked pottery and hated games.

His mother says that as a young child he did a great deal of reading, far more than his contemporaries. He also day dreamed a lot— the headmistress of the school he attended at 5 said: 'Matthew always had his head in the clouds, he's miles away' (T1). The only religious feelings he can recall were 'anti-religion', and he never had any religious faith. The family are not religious except in a very generally conventional way: Matthew considers having been confirmed 'a waste of time' (T1).

In 1977, while in the Himalayas he had an experience which deeply impressed him whilst looking at the mountains and watching the sunrise. He

realised how completely unimportant I was physically, how transient human life is... I just felt a tremendous feeling of harmony and unity. And I just... felt some Presence while I was up there that told me what I should do and what I shouldn't do. In fact, I should do what I wanted to do and not what other people told me to do (A1/1).

Table 1.1 provides a sketchy chronological record pieced together from Matthew's published books and tape recordings of his and his parents' statements.

ARRANGEMENTS FOR THE INVESTIGATION

Matthew wrote to me on 1 March 1978 suggesting that I should arrange for him to be investigated, and we met for the first time in person and discussed what form the experiments should take. Matthew was keen to try and replicate some of the biological experiments which he had done in the United States: he felt that he was at this stage more likely to succeed in influencing organisms than inanimate matter. However he agreed to try whatever I could set up. I explained that my own major interest was in an attempt to replicate the ostensible occulting of an infra-red beam obtained with the physical medium Rudi Schneider in the 1930s (Osty 1931, 1932; Hope 1933; Gregory 1983).

Date	Event	Source
Pre-natal	Mother's electric shock during pregnancy.	T1
17 August 1955	MM's birth.	Pers comm
Summer 1961/62	At 7 or 8, MM cut off by tide.	T1
1965 or 1966	Age 10 or 11 boy died prep school, nightmares.	T1
1966/7	Age 11 or 12 father belted Andrew.	T1
18 February 1967.	Onset phenomena; A.R.G. Owen called in; date not quite certain, MM 11, Rosalind 8, Andrew 6.	L15
Easter 1967	Rubber episode ends this bout of phenomena.	L24
1968	MM takes Common Entrance.	
	Family move home. MM to public school.	L26
Late 1969	Craze for seances at school.	L26
July 1970	Onset recurrence of home phenomena.	L28
Christmas 1970	Increase home phenomena.	L33
Winter/early Spring 1970	MM compiling Webbe family project for O-level project.	L83
Easter 1971	Violent phenomena; MM's bed moves, room 'as if bomb hit it'.	L34/36
Summer term 1971	Disturbances start at school; violent disruptions.	L41
May 1971	Weekend at home: first communication 'spirit' entities, 'Henrietta Webbe' voice.	L59
June 1971	Automatic handwriting, different from own; virtual fading of poltergeist phenomena.	L62
June 1971	Ostensible successful ESP.	L66
June/July 1971	Takes O-levels	
June 1971	Automatic writing characteristic of 'Robert Webbe'.	S16
31 July 1971	Names on wall begin.	S20
2 November 1971.	Half term; automatic drawing suggested by mother.	L92
November 1971.	Vision of 'Webbe' on stairs.	S11
Some months after November 1971	MM discovers old school project with different Description of 18th C. gentleman.	S15
1972	Various phenomena, including apports, automatic drawing, communications.	e.g. L102
Beginning months 1973	Contact Bander and Smythe.	
June 1973	Projected outline of MM's first book to AG.	Corr. 1.6.73 3.6.73
January 1974	MM bends metal following Uri Geller.	L141
Summer 1974	MM in Toronto.	M1
April 1975	MM in Netherlands.	M88
Summer 1975	MM in U.S..	M96
Autumn 1974 (?)	MM in Freiburg.	M16
January 1976	MM in Sweden.	M127
June 1976	MM in Japan.	M145
15 May 1977	MM 'compelled' to communicate once more with 'Webbe' after completion of archival research.	S120
1977	Himalayas experience.	A1/1
1 March 1978	MM writes to AG.	Corr.

24.7.78

TABLE 1.1

Rough chronology. T1 = taped interview 12.11.1978; L = Manning (1974); M = Manning (1977); S = Manning (1978) followed by page number. A1/1 = first side of first tape during investigation. (See first paragraph in this section.)

In fact I had just had an instrument constructed for me by Mr. C. Brookes-Smith in the hope that a former poltergeist focus or physical medium would come forward to be examined. Matthew was also keen to devise some experiment to test the ability which he felt he had acquired recently, namely to draw pictures which were unambiguously relevant to or illustrative of a poem as a paranormal target.

While the planning of these experiments was in progress, an incident occurred which should perhaps be mentioned. Mary Rose Barrington and I, while spending an afternoon and evening together preparing amongst other things a sheep-goat questionnaire to be administered to participants in the Manning investigation, heard four or five exceedingly loud but dull thuds or thumpings for which in the event no normal cause could be found. It did not in the first instance occur to me that they might be anything other than normal builders' noises, since I did not even know that the floor above us was uninhabited. Neither of us had ever heard any alleged paranormal noises before nor, to the best of our knowledge, have either of us been subject to any hallucinations, collective or otherwise. Mary Rose Barrington immediately investigated all conceivable possible normal sources of these noises, that sounded as if a muffled battering ram were hammering against the walls of the house immediately adjacent to where we were sitting.

As was subsequently ascertained, at the very time when we 'heard' the hammering, a client of Mary Rose Barrington's was longing to ring her up about a thorny problem, but did not dare to do so on a Saturday evening. (Miss Barrington is a solicitor, and her client was in the painful position of not being able to gain possession of her own house.) Now this client, Mrs. 'M', had been involved many years previously in a set of ostensibly paranormal experiences where she reported loud hammering noises emanating from the ceiling (Barrington (1965)). At the time of the noises Matthew was in fact in the middle of delivering a lecture (A1/1).

Mary Rose Barrington had a further subsequent experience conceivably connected with Matthew when she together with her mother and Mr. Jack May heard a loud explosive sound. Everyone immediately supposed it to be the explosion of a light bulb in a lamp standing on a sideboard. At the foot of this lamp stood a tray of mung beans 'treated' by Matthew in the course of the experiments described below. However, neither this nor any other bulb or TV tube was found to be damaged, nor was there any other plausible explanation for the sound. Prior to this incident Mary Rose Barrington had been talking about psychical matters.

Full accounts are too long to be appropriate here, and details of the incidents and of the sheep-goat questionnaire are deposited in the archives of the SPR. It does however seem worth mentioning these

incidents since there is some reason to suppose that there might conceivably be a connection between Matthew and ostensibly paranormal events reported by other persons after they have established contact with him.¹ In an obscure field it seems wise not to ignore obscure data that have a possible connection, the more so since the relation between psychical phenomena and meaningful coincidences or synchronicities remains to be explored both theoretically and empirically.

The investigation was arranged for the period 24 July to 4 August 1978. Professor A.J. Ellison offered us his cooperation and that of his technical staff, as well as his hospitality at the Bio-Electricity Laboratory in the Department of Electrical and Electronic Engineering at City University, London. Experiments took place there as arranged, with the exception of two sessions in the Department of Physics at Birkbeck College, University of London, arranged by Professor J.B. Hasted.

In addition to my own projected infra-red experiments which have been mentioned, the following were invited to plan experiments:

Miss M.R. Barrington, Professor W.B. Brown, Professor A.J. Ellison, Dr. I Grattan-Guinness and Professor Hasted. Dr. Grattan-Guinness was unable to be present but offered to try some experiments from a distance (see p. 322). Professor Hasted was in the country only during the latter part of the period; as mentioned, he conducted his experiments at Birkbeck College.

Mr. David Chapman, a Research Assistant in the Department of Electrical and Electronic Engineering, was present throughout and participated in virtually all experimental sessions. He was in charge of instruments and recording apparatus, which included a chart recorder for use in the infra-red experiments, audio recording which was running throughout all sessions at City University, and video recording from time to time. (Original recordings are in the archives of the SPR.) Mr. Chapman was assisted by Mr. F. Sullivan.

I asked Matthew to bring a friend both to keep him company and to act as his witness during experiments. Matthew suggested several names among whom I chose Dr. Brian Inglis, who was unfortunately only able to participate occasionally. His place in this respect was taken for much of the time by Miss Ruth West, who also helped me in the, at times formidable, task of organising, convening and chairing the many meetings before, during and after the investigation. She also gave invaluable clerical and other practical assistance.

The experiments were, as will be seen from the individual accounts, of very different types and durations. Provisional time-tables were drawn up with the clear understanding that flexibility would be needed to meet unexpected contingencies. Obviously some element of compro-

¹ Matthew tells me that people have often reported this to him; see for example *Here's Health*, December 1981.

mise between rigid adherence to schedules and spontaneous activity had to be faced, somewhat after the manner of a free activity day in a fairly informal school; the visit to St. Bartholomew's Hospital for Matthew's EEG had to be a fixture, as had his visits to Birkbeck for John Hasted's *mucor* experiments.

Dr. Peter Fenwick, Consultant Neurophysiologist at St. Thomas's Hospital, kindly examined Matthew's EEG taken at Barts by Miss Marion Smith, and found that the EEG revealed a few paroxysmal bursts and central theta normally found only in somewhat younger persons, depending however on the normative group studied. Matthew's left hemisphere showed a greater activity than the right while he was attempting 'psychic' tasks which, in a right-handed subject, is unusual (Dr. Fenwick, personal communication, 9.6.1982).

Professor Brown's haemolysis experiments in which I acted as second experimenter took longer overall than anticipated but, once the design was agreed, time for all the sessions had to be found and each session had to be continuous once the collection of the blood had taken place. An account of the haemolysis experiments was not available by the time set for collating this volume. These experiments were attempts to confirm the findings of Braud (1979) using an improved methodology. No significant results were obtained. The bean germination experiments could be done at any time once the beans were available. The poetry experiments could be initiated by Matthew at odd moments whenever he chose, and he was also free to try the pendulum and the RNG experiments on demand. Infra-red sessions were provisionally time-tabled (see below p. 316).

In addition to the experiments planned, Matthew at one point felt he would like to try 'sending' drawings to us. This, being a spontaneous activity, was not planned and designed beforehand and, although there seemed to be at times some subjectively quite impressive similarities and relationships between sender's and percipient's drawings, subsequent scrutiny, including independent appraisal by the ESP Committee of the SPR, suggested that nothing could be claimed on the strength of these.

The sheep-goat questionnaire (administered during a light-hearted social gathering at my home on the first evening of the investigation) might be regarded as an exercise in group interaction rather than as one conveying definitive information, especially since it is by no means clear that answers represented settled convictions rather than responses to a particular social situation. It was clear that most of us with the possible exception of David Chapman were willing to countenance a certain varying spectrum of paranormal phenomena, and expressed reasonable optimism about the forthcoming experiments. It is hardly surprising that Matthew himself showed a greater readiness to accept paranormal explanations than any of the experimenters and observers,

all of whom gave at least one thoroughly 'goat' style response.

A further experimental week was organised 12 to 16 February 1979, during which further infra-red, haemolysis and several further experiments were conducted. No positive results of any kind were obtained: it cannot be claimed that conditions, either physical or psychological, were similar. Matthew was unwell, and it was the middle of term as well as winter. Rather few of us could attend consistently.

COMMENTS ON THE INVESTIGATION

A number of general issues arise out of an investigation of this type, relating to the organising of experiments in this manner, others to the question of authenticity, *modus operandi* and meaning of results.

Block type of investigation

There are advantages in organising a number of experiments over a concentrated block of time, involving several experimenters and different activities, and there are also drawbacks. The block format is more suitable for the investigation of a star subject with limited time, especially one living at a distance, than for the experimental investigation of a trait or ability in the general population.

It is also useful to have a number of diverse participants present, often at the same time, in order to witness each other's observations and to make suggestions and corrections, and to complement each other's repertoires of competence. It may be easier, indeed this may be the only way, to arrange for cooperation between workers from different distant locations and fields of expertise.

On the other hand, assembling a team of participants with widely different backgrounds and commitments may mean considerable disparity of approach, attitude and time required and devoted to evaluation of data and so forth, and there is unlikely to be enough time to put into effect important suggestions for improvements. Also the administrative workload is very substantial.

These different rather practical considerations need to be balanced against each other in the circumstances of any given set of experiments.

The questions arising out of this kind of experimental organisation are of course by no means purely administrative, since the spacing and timing of experiments and the presence and absence of different participants may well have characteristic consequences on the results obtained.

For subject and other participants alike the setting aside of a short but quite substantial block of time such as a fortnight may highlight an investigation as a special event, and generate a sociable group mood of

fellow feeling and hopeful expectancy widely believed to be psi conducive, especially in the case of physical phenomena. The team located in this case in a laboratory, replaces the home seance circle or the sitter group.

The audio record certainly bears witness to a buoyant and active group mood at times when some ostensibly paranormal results were being registered. By the same token, however, the greatest care and caution must be exercised subsequently when assessing results. The block type of investigation is more likely to provide a *prima facie* case for selecting future areas for experimenting in depth than hard and definitive evidence. The very flexibility and informality may militate against a rigorous methodology in terms of experimenter initiated trials and controls for some of the experiments.

Laboratory setting

Although intangible and yet possibly vital factors such as group mood are hard or impossible to control, at least the laboratory setting and some form of continuous instrumental monitoring makes it possible to arrange for a high-spirited mood not to interfere too much with the collection of hard data. The permanent auditory record, for example, means that it is possible subsequently to check at least what was vocalised at any given time. Despite gaps and shortcomings in the recording and collection of data which will become apparent to readers, a good deal was learned about the conduct of future experiments, and it is of course also valuable to capture the subject's and other participants' reactions at the time.

One of the most important features to emerge that cannot be stressed enough is that it is essential to have as complete and continuous a set of hard and preferably automatically synchronised automatic records as possible. Subsequent evaluation will depend entirely on the quality of these records.

SOME PSYCHOLOGICAL ASPECTS

A number of questions naturally present themselves about Matthew Manning himself. On any interpretation he is a most unusual young man, and I personally was convinced of his sincerity. There are obvious problems about discussing in print a named individual and his psychology. I will therefore confine myself to questions which I hope will be relevant, relatively inoffensive, and lead to parallels and clues in the case of other subjects and future investigations.

Problems about using biographical data as explanations

There is inevitably an element of *post hoc* reasoning about any attempt

to unearth biographical factors so as to 'explain' a person's subsequent development. On the other hand, almost all of us tend to look at our own and other people's past to try to understand the present, and most psychologists, whatever their allegiance, will at least consider past events and experiences when trying to understand a person's current or eventual state. It would certainly be unwise to neglect such potential sources of information in a field as obscure as psychical research, if only to rule out certain factors as individually critical.

In asking how significant such events might be, we are facing the problems of all attempts to be 'scientific' about human understanding. Different individuals react differently to ostensibly similar events. For one thing, similar events have different meanings for different persons—it is precisely this factor of meaning which makes it so difficult, if not intrinsically impossible, for psychology ever to become wholly a normal scientific subject.

In psychical research, especially when we are concerned with a specific star subject, this difficulty shows itself in a fairly acute manner. There is the well-known gulf between experimentally and physiologically oriented psychologists on the one hand, and those concerned with understanding individuals on the other; normally the problem is dealt with by ambiguous and ambivalent co-existence. This is not the place for a discussion of this issue; it is however essential to make some reference to it because what counts and what is picked out as relevant, what is selected as a possible set of explanatory constructs, depends upon one's basic assumptions; and if one is discussing why one person should be different from others, one is immediately faced with the need to make reference to some explanatory framework; and a plurality of frameworks may be applicable to discuss diverse characteristics. It is pertinent here to point out that one of the best recent general books on psychology was written by one of our most eminent psychical researchers and is named 'Psychological Sciences' (Beloff (1973)).

Limited psychodynamic approach

In considering biographical particulars in the present context, then, apart from listing events and vicissitudes for future comparison with other psychics and control individuals and groups, the most sensible procedure would seem to me to be to adopt a non-doctrinaire and tentative limited psychodynamic approach, with the obvious restrictions entailed, and without any suggestion that this is the only possible method. This does not prevent supplementation by a more learning theory oriented analysis: any behaviour pattern may presumably be affected by reinforcement. My reflections are based upon what Matthew himself has said and written, and is not therefore, I hope, too impertinent or absurd.

In Matthew's own view, the purpose of the early poltergeist disturbances was principally to disrupt his parents', especially his father's, ordered existence 'without being for it' (T1).

My father is an extremely structured person. He does everything according to a plan, and to routine. Everything is time-tabled...if anything upsets his time-table he gets into a flat spin, he's completely inflexible whereas I do things completely impulsively and I hate any kind of inflexibility of time-table...I think it was a means of disrupting his structured existence without being given the blame for it directly (A1/1).

It must be said that there is nothing immediately striking if one considers Table 1.1. The pre-natal electric shock may perhaps be of importance; there are quite frequent accounts of shocks and injuries and close encounters with death in the early histories of psychics and mediums. But neither the cutting off by the tide and horror of fishes, nor the episode of the beating administered to his brother, nor nightmares in response to the death of a fellow pupil in themselves seem particularly unusual. It might be thought significant that Matthew had actually forgotten the beating of Andrew and also that he had apparently never mentioned his fellow pupil's death to his parents, until the interview in November 1978.

The very first manifestations during the poltergeist time were the moving of the father's prize tankard, and putting a bunch of flowers in front of his mother's place at table. 'Webbe' disputed the family's, presumably the father's, right to the house, and certainly to his own bed.

The early relatively sedate disturbances soon got out of hand after the manner of temper tantrums and grew more and more chaotic and frightening, and Matthew himself became frightened and took refuge in his parents' bedroom. I think it is quite apparent from reading Matthew's books that his father was more under attack during outbreaks of poltergeisterei than anyone else in the house.

The onset of both bouts was loosely linked with public examinations. Matthew does not consider that he was 'all that bothered' about these, but he clearly did resent what he felt to be his parents' relative lack of interest in his school work, a view which seemed to surprise them (T1). It should perhaps be mentioned here that whereas Matthew dislikes and rebels against time-tables which impose any restraints upon himself, he also intensely resents being kept waiting or in any sense inconvenienced by a failure of others to keep to a time-table. As I see him, he likes to be free himself whilst expecting others to be at his instant disposal. Most of us keep this perfectly natural set of desires slightly more in check than he does — or did.

Probably the fairest way of dealing with Matthew's own comments on this passage is to quote his own words, in a letter to me of 15. 2. 1982:

Re timetables! I have refrained from altering observations on myself in this text even if I don't particularly agree with them. However, you may have misunderstood my reaction to timetables, or else I am older and wiser since we spoke about them! I don't entirely agree with your comment that I like to be free whilst expecting others to be at my instant disposal. That's not really true and makes me sound like a megalomaniac. I'll say no more!

If the earlier poltergeist phenomena are viewed as transcending the bounds of normal behavioural effectiveness (and Dr. A.R.G. Owen is an impressive and expert witness, quite apart from other considerations), then these phenomena might be regarded as expressing externalised aggression and self-assertion in a particularly stringently dissociated form.

There is nothing unexpected about the element of dissociation, in the sense of Matthew's not wishing to be held responsible for the havoc he created. It was a voice in the Himalayas that *told him* to do what he wanted to do, and not what others expected of him. But there is of course something thoroughly heterodox about the working hypothesis that Matthew's energy and aggression dissociated themselves from his body! From a strict classical psychoanalytic point of view even to entertain such a possibility might be thought of as countenancing 'omnipotence fantasies'. In Freud's cosmology it is precisely our subordination to the 'reality principle' that forces upon us the dilemma of acceptance, resignation, limitation and frustration on the one hand, or delusion and mental disturbance on the other. Now the psychic and, for that matter, the psychical researcher who is in principle willing to allow genuine and especially physical phenomena, are persons who have implicitly at least refused to submit to this Freudian dilemma: acceptance of conventional limitations or mental illness. It will be remembered how violently Freud himself reacted when he felt threatened by what he considered the 'tide of occult mud', the very ocean of irrationality that he felt to be represented by Jung's querying of the pan-sexual theory, accompanied by ostensible poltergeist phenomena (Jung (1963)). Here too, paralleling Matthew, there was a direct challenge to a father figure (Freud (1909)).

It is impossible in the present state of knowledge to provide an adequate discussion of this issue, but it is in my view relevant to mention these considerations. It is a far cry from 'omnipotence' or even 'omnipotence fantasies' to the relatively slight practical deviance from normality represented by the throwing about of everyday objects by unknown

means. Just exactly what are the limits of physical reality and potential, or of the human condition, are not *in themselves* psychiatric issues; but this is apt to be obscured by the fact that the psychiatrist is called upon to provide the rest of society with practical guidance as to what constitutes pathological perception and experience, and morbid or deviant action. It is here that a psychic like Matthew, or an open-minded psychical researcher, or for that matter any pioneer who rightly or wrongly challenges belief in existing limitations is bound to present a difficult and painful challenge to the world at large: established authorities, renunciations made and safeties taken for granted, all these are thrown open to question; the result is bound to be anxiety and hostility. These are important aspects of the psychology and sociology of innovation.

The crux of the question of the genuineness of psychical phenomena is: has there been, or has there not been, a transgression, or transcendence, of conventional limitations by, or in connection with, the psychic? If not, the question is one of the psychology of error, deception and self-deception, of ordinary deviance; but if so, if there has been exosomatic action, then the issues are rather different: are the psychic's psychology and development manifestly different from those of the rest of mankind?

Problems of normality and paranormality

If it felt that Matthew's effects are basically genuine, and we ask 'is there something strikingly different about Matthew from other people?', I think the initial answer must be 'no'. Development and motivation seem to follow on the face of it quite normal patterns: the only really unusual characteristic is the emergence and subsequent transformation of the phenomena themselves.

This transformation however is itself worth noting. It looks as if Matthew managed somehow to control the increasingly disruptive physical manifestations by the practice of automatic drawing. Now as Matthew himself shrewdly as well as honestly says, these drawings, however accomplished or attractive, were not really creative so far as he was concerned; they are in the fullest sense of the word derivative. Nevertheless, the activity of abandoning conscious control whilst engaging in the act of drawing did produce the most remarkable artefacts and this did, on the face of it, abolish destructive and chaotic physical manifestations. It seems to me that this is almost like witnessing something like the process of 'sublimation' in action, a particularly spectacular and successful application of the therapeutic exercise of 'active imagination'.

How are we to account for the astonishing and instantly recognisable family resemblance of these brilliant pieces to the work of so many

artists? Matthew does not on the whole believe that they are the work of deceased persons any more than would most parapsychologists, and he is of course not unique in accomplishing in a dissociated state work vastly superior to that which he is able to produce normally. It might be interesting in a future collection of clinical data to adopt the working hypothesis that automatic performances are a defence against physical or physiological disruption, usually hysterical or psychosomatic, i.e. *endo*-somatic, but exceptionally paranormal or *exo*-somatic; it might further be asked whether the greater the superiority of the automatic performance compared to normal accomplishments, the more often are there accounts of spontaneous psychokinesis.

As has been mentioned, Matthew has not continued with the automatic drawings, and when he approached me he was becoming more interested in biological than in physical effects. This was the reason for planning the seed germination, haemolysis and *mucor* experiments.

He described his attempts to influence Mary Rose Barrington's beans as follows:

... I'm imagining that I am expanding my consciousness right out round me through out of the walls, through the building and out across London. And I just imagine that I keep going round the globe till I start to go right into space. And at some point something clicks and I just feel then that I am just a part or in harmony with it, and that there is then some energy which is then coming through me which goes through my hands. Which is why I use my hands for this. And when I've got that feeling, I then put my hands over the seeds to make them grow faster, I keep an image in my mind of the seeds sprouting very quickly. And I imagine that they're in a very humid atmosphere with everything they need, with water and soil and so on. But when I try and retard them, then I go through the same mental process but I imagine they've been thrown into the desert, or put on a rock or something, where they can't grow and there's no water and no sunshine (A1/1).

Positive results were, as will be seen, obtained in this experiment, but it is clear that such imagining is for him at most a necessary and certainly not a sufficient condition, and may remain without effect, as in the case of the haemolysis experiments, where he imagined:

I've surrounded the entire test tube with some kind of white light that comes through my hands with that white light surrounding every single cell. The...cells won't break open...(A1/2)

Matthew's attitude to the pendulum and random number generator experiments was explicitly hostile. He disliked the heavy machinery surrounding the pendulum and the fact that the bob to be moved was in fact a light Christmas bauble made no difference to his subjective feeling. In the case of the RNG experiment, this was basically an 'out of the body' or clairvoyance test to 'see' random numbers, and not an attempt to generate a physical effect. It may of course be the case that the instrument's physical malfunctioning, i.e. continuing to produce the number 8, after the same manner as it had done for Ingo Swann, was an accident. Matthew himself thought he had probably made it go wrong: 'It's also one of the experiments I didn't want to do. That's not the first time it's happened. Tart's machine produced 8's' (A1/2). It might also be akin to the curious observation made by Bierman, DeDiana and Houtkooper (1976) in connection with Matthew, where in a random number generator they failed to obtain the effect they sought, but their program got stuck in a loop 'with a probability that such a loop would turn up in this condition $p = .028$ '. Houtkooper and DeDiana (1977) is also of interest in connection with Matthew. Palmer, Tart and Redington (1979) also reported a tendency for a random event generator repeatedly to generate a particular target number after Matthew started to concentrate on it while planned analyses were non-significant.

There can be no doubt that in Matthew's case at least, the ideas and quite conscious fantasies accompanying attempts to produce 'physical' effects were aggressive, competitive and destructive, whereas those accompanying more 'mental' or 'healing' modes of action were on the whole peaceable, expansive, relaxed and benevolent. This reflection is not intended to constitute a value judgment about these modes: both are integral to living.

Although Matthew is plainly an unusual and exceptionally determined person as well as a highly intelligent one, I can see no reason to describe him as particularly saintly or inordinately altruistic or considerate of others. He does not consider that he is unique in possessing gifts in which he believes, but is of the opinion that others could do much the same if they admitted their own potential in this respect. He professes no idea of their nature or origin. Matthew can be very difficult indeed, as well as exceedingly charming.

As it now appears to me, the investigations of which this is an account, may be regarded as a series of experiments, concerning which views will no doubt differ, and also as encounters with an unusual and talented person. This dual perspective is as it should be, and is characteristic of a field in which we are concerned to investigate *objectively* characteristics of human beings, an enterprise in which the *subjective* experience of all concerned is likely to be of crucial importance.

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2

BEAN GROWTH PROMOTION PILOT EXPERIMENT

MARY ROSE BARRINGTON

THE EXPERIMENT

Trays of Mung beans were prepared for submission to Matthew Manning, who was to treat half the population, the other half being set aside as controls.

As originally prepared, four beans were placed in each compartment of ice-cube containers, the compartment having been lined with two layers of white blotting paper. It was intended that one out of each pair of compartments (the one to be selected by a random method) should be treated, and 40 pairs were prepared.

This did not suit Matthew's style of treatment, so the assistant experimenters (M.R. Barrington being absent from the proceedings) set aside one of the seven boxes, and selected three boxes for treatment as a whole.

One of these pairs of boxes was discarded by M.R. Barrington, as the boxes were not identical, and the first pair, each containing 48 beans, were handed over to K.L. Fudge, a biology teacher, for nurture and observation. The results are set out below in Table 2.1.

Experimental Sample (Promoted): 48 Beans		Day	Control Sample: 48 Beans	
Emergence of Radicles	Plumials		Emergence of Radicles	Plumials
20	0	1	23	0
12	0	2	3	0
5	1	3	2	3
2	17	4	1	11
2	7	5	3	4
1	5	6	0	1
0	4	7	0	2
<hr/> 42	<hr/> 34		<hr/> 32	<hr/> 21

TABLE 2.1

Taking the appearance/non-appearance of radicles and plumials within 7 days as an index of success/failure, 2 x 2 tables may be set out as follows in Table 2.2.

RADICLES:	<u>Succeeded</u>	<u>Failed</u>	<u>Total</u>	
Promoted	42	6	48	Allowing for Yate's correction Chi-square = 6.26 (1d.f.) P less than .02
Control	32	16	48	
PLUMIALS:				
Promoted	34	14	48	As above, Chi-square = 6.13 (1d.f.) P less than .02
Control	21	27	48	

TABLE 2.2

As an afterthought, Matthew was asked to treat a further sample of beans (in a small sealed container) for retardation, and these were grown under the same conditions as before. The retarded beans did slightly worse than the promoted, but better than the controls, in neither case to any significant degree.

Experimental Sample (Retarded) : 48 beans. Total of Radicles: 39
Plumials: 27

The second pair of boxes will now be treated to see if the same differential effect can be obtained.

ATTEMPTED FOLLOW UP

Regrettably it has not been possible to carry out a satisfactory replication of this experiment.

When the first experiment was carried out, Matthew operated on the beans on 24 July 1978, and they were given over to K. Fudge for cultivation on the following day.

As soon as it was known that the first experiment had produced promising results, Mr. Fudge was given the remaining beans; however he was not able to attend to them until the last week in August, some four weeks later.

The results were decisively non-significant, though not without curiosity. In the case of both radicles and plumials, and both in the promoted and the control sample, the number that succeeded amounted to 28. The oddity of this coincidence in numbers may be judged by showing in brackets the result of the first experiment (see Table 2.3).

RADICLES:	Promoted Beans	28	Succeeded	(42)
	Control Beans	28	..	(32)
PLUMIALS	Promoted Beans	28	..	(34)
	Control Beans	28	..	(21)

TABLE 2.3

Despite the encouraging statistics the first results may of course have been due to chance; if however they were indeed due to successful treatment then it is evident that the results will not be repeated unless the beans can be cultivated soon after treatment. Unfortunately it has not been possible to arrange for this to be done, the three persons concerned living at some distance from one another and having other commitments.

The first results suggest that it would be well worth while for a full-time researcher with the required botanical knowledge to carry out some extended tests using Mung beans.

3

PENDULUM EXPERIMENT

A. J. ELLISON

On an earlier occasion an apparatus had been installed in the laboratory for certain tests of PK. The equipment consisted of a long pendulum installed in a transparent plastic tube. A very light bob (a Christmas tree bauble) was supported on a very flexible fine glass thread (this

material absorbing very little moisture so having a consistent stiffness and length with temperature and humidity changes). The pendulum base was mounted on a heavy wooden table itself on a concrete slab in the building. Transparent screens prevented voluntary or involuntary mechanical interference with any of this equipment.

In one version of the experiment the aim was to cause the pendulum to move from rest by PK. As the pendulum was approximately 1 metre long and very light, the force required to produce a small movement was very small. In another version of the experiment the aim was to change the motion of the pendulum from approximately circular paths to appreciably elliptical ones by sideways PK force at one place on the orbit. If this occurred, the pendulum was arranged gently to brush microswitches and trigger an electrical indication of the change. Two microswitches, adjustable in radial and angular positions, were arranged at positions 90° apart. Continuous chart recordings were made of the state (open or closed) of the microswitches.

If the pendulum had been approached by the subject (Matthew) and he had attempted to use PK on it to cause motion, and if motion had then resulted, it might have been suggested that the cause was building vibration, due perhaps to other equipment or to road traffic. The equipment was therefore kept switched on for some days and the absence of any movement observed. Movement starting coincident with PK intention of the subject would then be much less likely to be attributed to coincident normal causes of any kind.

Matthew stated when he inspected this equipment that he did not consider it likely that he could affect it. He was kind enough to try on several occasions but no positive results were achieved.

4

RANDOM EVENT GENERATOR EXPERIMENT

A. J. ELLISON

On an earlier occasion equipment had been built primarily for tests of pure clairvoyance related to out-of-the-body experiences. The equipment consisted of a box carrying electrical circuitry so arranged that depression of a button at the front produced in a window at the back a display consisting of a 3-digit random number. Each digit was produced from the common arrangement of seven straight bars illuminated selectively to form the numbers. The original aim was to ask volunteer subjects, who were able to have an OBE on request when in hypnotic trance, to report the number in the window, invisible at the back to the subject in the normal state, to the experimenter and to the independent

witness(es). The experimenter would then set up the digits reported by the subject on dials at the front of the box. The button would then be depressed for another random number, the first one being lost, but the circuitry kept a record of rights and wrongs, i.e. agreements or disagreements between the numbers at the back and those set up at the front. At the end of a series of trials a dial at the back could be set to show the number of agreements in the window at the back, either for all three digits together or for each separate digit.

It is perhaps instructive to describe earlier uses of this equipment before the present series of experiments. The first OBE subject was unable under the conditions described to have the OBE. The second subject had a satisfactory OBE and on the first and pilot occasion, reported the numbers to the experimenter (myself) who looked to see what they were. There was almost complete agreement between the statements of the subject in trance at the front of the box and the numbers at the back. With this auspicious start, the first experiment was begun, the number at the back not being observed normally at any time. The subject then expressed difficulty in seeing the numbers in the window and the run had to be abandoned for practice elsewhere with numbers of a similar size.

Some time later the well-known American psychic Ingo Swann briefly visited the laboratory socially and, *inter alia*, this equipment was described to him. He at once asked to do a run. The box had not been in use for some months but was taken out, switched on, and appeared, on the basis of one or two rapid tests, to be working correctly. Ingo Swann stated that the OBE was not necessary for him to succeed in an experiment of this kind and he simply sat at one end of the table with the experimenter (again myself), stating what he considered the numbers to be. At the end of the run of about 20 trials the display indicated an agreement of 8 of the 3-digit random numbers with his statements. This was received by the experimenter with some surprise but arrangements were tentatively made to do similar experiments across the Atlantic, using the telephone. The following morning the experimenter did a run of his own and also achieved an indication of 8 agreements. There was clearly something wrong. A fault in the microcircuit contacts causing the illumination of all seven LCD bars would cause an indication of 8. The microcircuit contacts had no obvious fault but were nonetheless carefully cleaned. The apparatus was then found to have returned to normal, the experimenter scoring agreement between front and back numbers only when he observed the numbers at the back first.

The present subject (Matthew Manning) kindly agreed to do several runs in a way similar to that used by Ingo Swann. The equipment was switched on and achieved a steady temperature well before the first experiment. Both experimenters (myself and D. Chapman) carefully

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checked it for accuracy, with success, all indications being correct. The Subject then did a run of 20 trials. The box indicated 8 successes. With some disquiet the experimenters themselves did another test run and found that they also scored 8. Careful cleaning of the microcircuit contacts again restored the functioning to normal.

Careful checks were made exactly as described above and the Subject did another run on the following day. Again he achieved an indication of 8 and again cleaning of the contacts was necessary.

These salutary experiments are clearly of little use as indicating pure clairvoyance to a sceptical observer. The experienced parapsychologist, however, would observe that similar results are not infrequent. It may be hypothesised that the unconscious machinery of the gifted psychic found it easier to bring about faults in the contacts by PK than to discover the numbers by clairvoyance. Sadly this could not be proved at the time and that experiment had to be left in this unsatisfactory state.

5

POETRY EXPERIMENTS

ANITA GREGORY

ORIGINS

In the course of preliminary discussions between Matthew Manning and myself, Anita Gregory, Matthew expressed the wish that a means might be found of testing an ability he felt he had recently developed of drawing a picture that aptly illustrated a poem he had to sense by psychic means. I suggested to Matthew experiments on the lines of Besterman (1933) and Dingwall (1924) with Ossowiecki, who had produced drawings that bore a striking similarity to the contents of carefully sealed prepared envelopes.

It was explicitly intended to use these experiments as occasional tests as and when Matthew felt like it, so that he would decide whether and when trials were made. It was also anticipated that there would be gaps between other experiments, and the poetry experiments were partly devised to bridge these gaps by means of prepared material.

METHOD OF PREPARATION

The preparation was devised and carried out by myself and no one other than I knew the details until after the opening of the first package.

20 brief extracts from poems, most of them two to six lines, were typed on slips of paper. They were chosen because they seemed to

evoke a clear mental image. Each extract was allotted a number from (1) to (20) which was typed on the slips. The slips of paper were folded in two, wrapped in aluminium foil and punctured in four places through the slips and foil, so that light could be seen through the holes. The punctured foil sachets were inserted into a first stout manilla envelope ca. 19.2cm x 12.5cm. The flap was stuck down, I signed my initials across the closed flap and body of the envelope, and, while the ink was still wet, sealed sellotape across my initials, flap and envelope. This was inserted into a second larger manilla envelope, ca. 25.2cm x 17.5cm. This envelope was again stuck down at the flap and I initialled both the flap just stuck down as well as the opposite end, sellotaping across as before. A blank piece of paper ca. 21 cm x 14.5 cm was taped onto the front of the outer envelope. The packages were thoroughly shuffled and were then labelled on the outer blank paper E1 to E20. I was unable to tell which package E1 to E20 contained which slip with verse (1) to (20).

PROCEDURE

I carried the 20 packages with me in a brief case. Out of these ten packages altogether drawn out of the case at random by myself were used on different days.

On the first occasion that a package was handed to Matthew he was told that it contained a verse. He was asked to draw on the blank piece of paper stuck onto the envelope. On this and later occasions he then drew and wrote whatever came into his mind. He certainly liked the experiment and initially expressed confidence in his ability to achieve results.

Immediately after each test I opened the successive envelopes, checked that the punctures were intact, invited any one present to do likewise, and compared poem with drawing. The opened and used packages complete with drawing and verse were then returned to the briefcase.

RESULTS

Ivor and Enid Grattan-Guinness were asked some days later on 12.8.1978 to blind-match the drawings with the snatches of verse, giving first, second and third choice. Similarities between drawings and poems were negligible. They subsequently applied a procedure devised by I. Grattan-Guinness (see note below). Again, results were negligible.

One drawing, package E5 containing verse (19), awarded the highest marks by I. and E. Grattan-Guinness, may be used to illustrate the

difficulty in making qualitative judgments of this type. The drawing, the first one Matthew made, was a crude sketch of a knight in armour on a winged horse with a castle and pine trees and a bird flying overhead. Matthew wrote above the drawing: 'Knight in armour. Breughel-like image of dogs and a castle — passing through trees. Reminds me of Dürer drawing of knight and devil. Horse has wings. Blindman.' The verse in the package, by Keith Douglas, was

Now on my dial of glass appears
The soldier who is going to die

Matthew was far from delighted with this as a 'hit', but he, as well as others present, thought it reasonably relevant. In naming the apocalyptic Dürer etching, Matthew had omitted the third partner, 'death', and certainly the mood of the drawing and that described by Matthew were quite apposite to the Douglas poem. Matthew commented that a knight was a soldier and the mood was of doom and death.

The attempt took place in the laboratory while we were waiting to do a set of haemolysis experiments, delayed because of adjustments to the equipment requested by Matthew. He made several subsequent attempts at other poetry packages on that and subsequent occasions, which both he and I agreed showed no relation either of us could imagine between verse and drawing, although there were quite frequent allusions and associations that subjectively felt relevant.

DISCUSSION

It is plain that no positive results can be claimed. The whole endeavour highlights once again the problem of quantitative assessment of basically qualitative material. A better technique for blind-matching might have been by, for example, presenting judges with several independently selected extracts of verse in addition to the critical one, and asking them to rank these for goodness of fit. In my view there is also room for I. Grattan-Guinness' suggested *post hoc* grading, especially if a sophisticated design were devised by which the judges did not know whether they were assessing the drawing by the Subject under test, or some control drawing. Alternatively the Subject could have been presented with several extracts and asked for *his* preference.

In assessing the method as a whole, a number of considerations arise which may have a bearing on future experiments of this type.

1. The packaging was most effective. The puncturing of the metallic wrapper and slips after the two outer envelopes were removed remained intact for well over three years and survived posting by first and second class mail of control packages (19.1.1982). The packages can be

recommended for GESP tests.

2. The format of the experiment was attractive to the Subject despite the fact that it did not ensure success.

3. As has been pointed out, a more refined set of techniques for judging should be devised.

4. The somewhat ambiguous part played by these experiments in the setting of the investigation as a whole should be pointed out. Whereas they were regarded as important in their own right and had required a good deal of preparation, and precautions were incorporated in such a way as to safeguard spontaneity at the time of test administration, the packages were *in effect* often used as fillers and side occupations while we were waiting for other and perhaps by psychological implication 'more important' things to happen. Had time been specifically set aside this would almost certainly have made a difference to the group mood and expectations surrounding these experiments and consequently — possibly — the results. Again, quite different results might have been obtained under Ganzfeld conditions.

5. The 'psychometry' mode was chosen deliberately for safety, ease of administration and spontaneity. However, it could well be that in order to test Matthew's ability to match drawings and verse, a more plainly 'telepathic' mode of experimenting might have been more appropriate. The principle of collaborating with a Subject by attempting to incorporate a spontaneously experienced phenomenon into a more systematic set of tests should be further explored.

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A NOTE ON THE METHODOLOGY OF BLIND MATCHING

by IVOR GRATTAN-GUINNESS

The usual procedure in blind matching is to place the sensitive's product in choice order (up to 3rd, usually) with the given data. When faced with matching Matthew's 10 drawings (some with annotations) against the 10 extracts from poems, I felt overwhelmed by the 100 possibilities, especially as *any* aspect of correlation was allowable. I also noted that each order is internal to the extract, and gives no indication of the

quality of the correspondences which led to the choice of ranking that was made.

In this matching experiment I followed the alternative of assessing the quality of *each* extract with *each* drawing, without having to have the other extracts to compete for ranking with this or any other drawing. Three categories of correspondence were used: 'good', 'fair' and 'marginal'. Assignments to categories had to be made qualitatively, but it turned out not to be difficult to do so. I revised some of my judgements afterwards. I allowed for no, or more than one, extract, to be placed against each category.

My wife and I made the correspondences independently, and then did a joint assessment without looking at our previous individual judgements. The three tables of results were pooled, and a cumulative quantitative analogue produced from the measure good = 5, fair = 3 and marginal = 1 for any judgement which we made of the correct extract for each picture. While there are obviously still considerable difficulties in interpreting the results we felt that a procedure based on grading, rather than the more normal one using ranking, was easier to produce and led to more satisfactory judgements.

6

INFRA-RED EXPERIMENTS

ANITA GREGORY AND KATHLEEN WILSON

INTRODUCTION

The first researchers to use infra-red radiation as a detector of psychokinetic activity were Eugène Osty, Director of the Institut Métapsychique in Paris, and his son Marcel, an engineer (Osty and Osty (1933), (1932)). As it happened, their discovery was a by-product of quite another endeavour. Eugène Osty had originally been concerned with the perennial problem of controlling a physical medium during trance, particularly in the dark. While ostensibly physical paranormal phenomena were occurring, the traditional way to achieve this was by means of controlling the medium either manually or, more elaborately, by means of electrical signalling devices of various sorts. As usual, each method has its advantages and drawbacks. Among the difficulties that may be urged against immobilising a medium are that this does not exclude trickery by some other participant, that watching either the medium or a control panel deflects the attention of some of the most critical people present, and that there is felt to be something inelegant and clumsy, as well as less than humane, about physically restraining a person. Osty suggested that instead of (or in addition to) immobilising the medium, arrange-

ments should be made to monitor automatically and instrumentally the objects to be moved, and he devised an infra-red burglar alarm style device for thus guarding the objects to be moved paranormally by the physical medium Rudi Schneider: any normal encroachment on the areas to be guarded would immediately be detected, signalled and automatically recorded by cameras. When the device was first put into operation, signals were obtained suggesting that security had been breached and that something tangible had indeed penetrated the infra-red network. However, the target objects were left undisturbed, and the monitoring cameras, using magnesium flashes which had been triggered off by the infra-red alarm system, showed no sign of any intruder. After initially assuming that the repeated triggering of the alarm system was due to malfunctioning of the apparatus, it was noted that in fact the interruptions of the beam were correlated with attempts by the medium's control personality 'Olga' to move the target objects: 'Olga' (or Rudi speaking as 'Olga') would announce that now 'she' would lift up a handkerchief, and at this point the alarm would be set off. There ensued a good deal of experimentation, and the results with the same medium were replicated in England, both under the auspices of Harry Price (1933) (although the apparatus was not under his control) as well as by Hope *et al.* (1932) and again by Schwaiger in Vienna (1935-36). Osty delivered the Myers Memorial Lecture on the subject of these experiments in which he discussed his view that the medium exteriorised in trance some form of force or matter, invisible in white light, but which could be detected by means of infra-red radiation of suitable wavelength.

Parapsychological interest in the field of PK in the intervening years apart from metal bending in the wake of Uri Geller, has focussed more on the meaningful or intentional influencing of physically random events than on attempts to detect and evaluate directly the exertion of paranormal physical influence. However, in view of the outstanding quality of the evidence we have for the Schneider phenomena, especially the IR effects, it seemed important to attempt to replicate the observations. Not only would it be of considerable interest to obtain evidence corroborating the hypothesis of some tenuous and transitory form or phase of matter, such a method might also be expected to yield results in an area of investigation from which promising experimental subjects, i.e. physical mediums, have all but disappeared, at least in Europe and in the U.S.: the results with Rudi were obtained towards the very end of his mediumship when even fewer, if any, phenomena were still being observed. The working hypothesis therefore naturally presents itself that effects in the infra-red might be vestigial manifestations found in conjunction with persons who had earlier given evidence of physical paranormality, such as former poltergeist children.

When Matthew Manning approached Anita Gregory as described in the

introduction, this appeared to be a promising opportunity to test out the equipment previously prepared by Mr. Colin Brookes-Smith. It was decided to carry out pilot experiments to see if Matthew would affect the infra-red apparatus in a manner similar to that of Rudi Schneider, thus providing a *prima facie* corroboration for the working hypothesis that former PK agents may still produce vestigial paranormal influence in the form of ostensible partial occulting of infra-red radiation and, in the event of positive effects being obtained, to note some of the characteristics of the phenomena for future experimentation. Moreover, results indicative of a vestigial form of matter might also have a bearing on the fundamental theoretical problems trenchantly outlined by Braude (1981).

APPARATUS

The equipment for detecting occultation of the infra-red beam (see Figure 1) consisted of a mounting board with an infra-red source at one end and a photocell at the other. The photocell output voltage was amplified and biased in a separate amplifier unit having an output of direct voltage of 1 under zero occultation conditions and 0v under total occultation conditions. The output voltage was displayed on two independent analogue voltmeters, and on a digital voltmeter (Gould-Advance model Alpha iii); it was also recorded on chart paper using a Watanabe Linear-corder Model WTR 281.

The IR light source was housed in a plywood box with plywood feet extending from each side so that it could be firmly screwed down. The light source was a 6 volt MES bulb focussed on a 4" lens resulting in a parallel beam.

The IR filter transmitted light $\lambda = 0.9\text{--}2.5\mu\text{m}$ measured on a CARY 14 spectrometer.

Power for the 6v bulb was derived from three different sources during these experiments. These were:

- (a) A bank of alkaline accumulators followed by a conventional solid state voltage stabilizer; the regulated output voltage was set at 5v.
- (b) An unregulated, rectified and smoothed direct voltage derived from the 240 volt mains supply via a transformer, followed by the same stabilizer as in (a) above.
- (c) Laboratory twin stabilized d.c. supply unit (Advance model PP3) and DS 5/05 power unit adjusted to provide a direct voltage output of 5v.

The photocell unit was contained in a plywood box similar to the lamp unit. A 4" diameter lens was fitted at the end facing the lamp unit and a silicon solid state light sensitive cell was mounted on an adjustable stand located in the lens focus. Output connections were provided

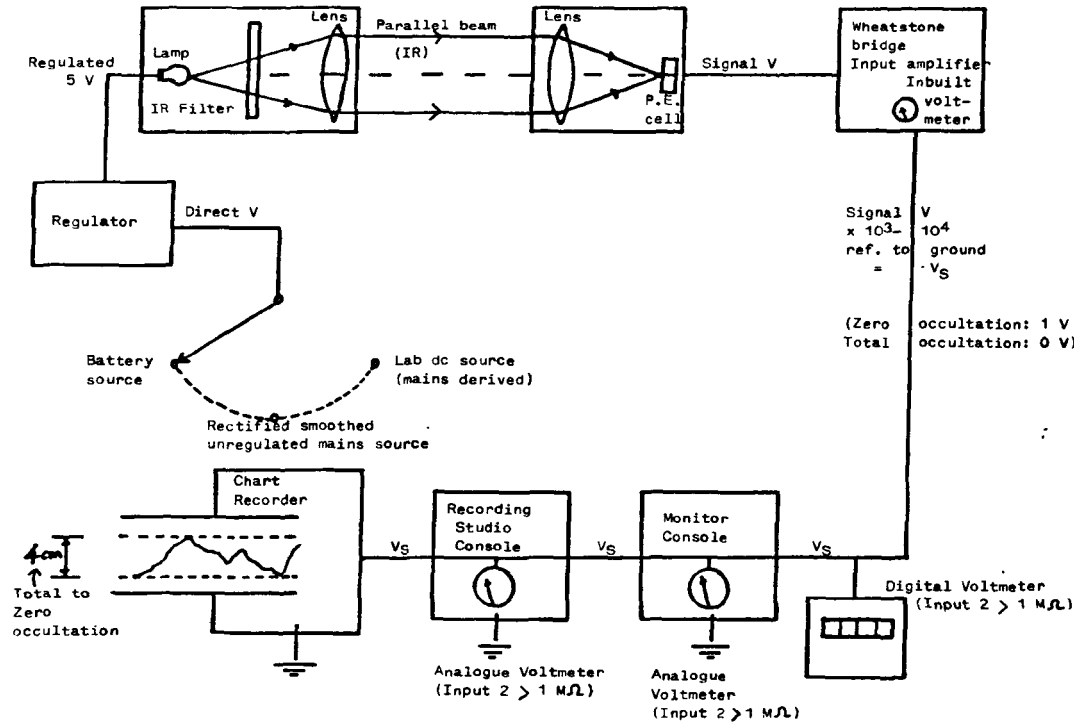


Figure 1. Schematic diagram of equipment.

by a pair of 4mm sockets in parallel with a 3.5mm co-axial plug and socket connection. The plywood case again had projecting feet on each side so that when it is correctly aligned to the light beam it can be firmly screwed down.

The amplifier unit had a plywood case. Amplifier panel and power supply were in separate compartments, and there was also a small compartment for a 6ft mains flex and its 13 A plug.

The amplifier panel was 7½" square. On it were mounted the panel meter, all the essential controls and the inlet and output sockets. The input circuit was essentially a d.c. Wheatstone bridge having 10,000ohm resistance arms, and there were coarse and fine controls to obtain null balance. The bridge output was connected to a two stage 741 operational amplifier with variable gain control, the overall voltage gain being between 1,000 and 10,000. The d.c. output signal was at the 1v level.

The experiments were conducted in the Bio-Electricity Laboratory at the City University, use being made of the installed monitoring and recording facilities.

The infra-red source and sensor were mounted on a rigid board, usually placed on a trolley located in the experimental area close to, and connected by an umbilical to the amplifier unit situated at the monitoring console. The amplifier unit output was plugged into the appropriate data lines at the console which provided an analogue electronic voltmeter display (3v full-scale deflection), having an output impedance of about 1MΩ and in addition a digital voltmeter display. This latter 3½ digit instrument, set to show 1.000v at zero occultation was sensitive enough to display the noise in the system (0.01% per digit). Since the noise was typically between 1% and 2%, the digital reading was continually varying between about 0.980 and 1.020; this appeared to attract the Subject's interest and he used the instrument for feedback during most of the experiments.

Data lines from monitoring console transmitted the amplifier unit output to the recording studio on the mezzanine floor immediately above the experimental area. Here the data were displayed on an analogue voltmeter similar to that on the monitoring console and monitored by technician staff; the voltage was also recorded on the Watanabe chart recorder. The chart record showed the amplifier unit output on a scale providing 4cm deflection for total occultation, together with timing pulses at 1 minute intervals. Additionally event marker pulses were recorded by operating a press button in the experimental area.

Audio recording was in use throughout the experiments. Two microphones were active, one situated centrally overhead in the experimental area, and the other, for use by the experimenters, at the monitoring console. Synchronisation of the chart and audio records was effected

by manually marking the time chart and audibly recording it on the tape. Further details are given in the text. The recorder used was a Phillips model 4407 stereo. Some of the sessions was recorded on an Akai video recorder Model VT 1100.

BRIEF DESCRIPTION OF EVENTS AND RECORDS

Timing

A provisional timetable was drawn up and circulated to participants. It was explicitly envisaged that arrangements would be changed, since the two weeks were regarded as a period of concentrated exploration, rather than providing definitive results. Table 6.1 shows departure from the original tentative timetable.

IR Experiments Projected	IR Experiments Conducted
a.m. Tuesday 25 July	p.m. Tuesday 25 July
a.m./p.m. Wednesday 26 July	p.m. Wednesday 26 July
a.m. Friday 28 July	noon/p.m. Friday 28 July
'any time' Monday 31 July	p.m. Monday 31 July
(if promising)	p.m. Wednesday 2 August
	(impromptu trial at Barts)

TABLE 6.1

The timing was partly dictated by the needs of other experiments and partly by Matthew's decisions to turn his attention to the infra-red experiments, with the exception of Wednesday 2 August. (The circumstances surrounding these 'decisions' will be more fully discussed below.) The fact that the infra-red equipment was set up most of the time and that the experimental area was so organised that attention could be switched from one experiment to another at will facilitated maximum flexibility. As will be discussed, such flexibility combines advantages and drawbacks.

The experiments will be described in terms of the days on which attempts were made by Matthew to influence the infra-red.

Some comments on apparatus and terminology

A few words of description in non-technical terms are necessary here to clarify what follows. Phenomenologically, that is from the point of view of the Subject as well as non-technical participants, three items of equipment were of significance: the infra-red (called IR) equipment, the digital voltmeter (DVM) and the chart recorder.

The IR equipment was composed of two wooden boxes mounted on a wooden board about three feet apart: if the IR was 'on', that is, if the box containing the IR source projected its beam on to the cell, the

space between the two boxes had to be seen to be perfectly vacant. Any intervening object would immediately affect the 'beam' and, depending on its size and density, would result in a recording of a partial or total 'occultation'. Objects would occasionally be deliberately put in the beam for test purposes to see if the instrument was working, and this was noted. It was the Subject's task somehow psychically to infiltrate or invade the space between source and cell, so as to cause a partial 'occultation' of the beam (Figure 2). (The word 'occultation' will be used in the descriptive sections for simplicity, without pre-empting the interpretation of the causal agency of deflections registered and recorded.)

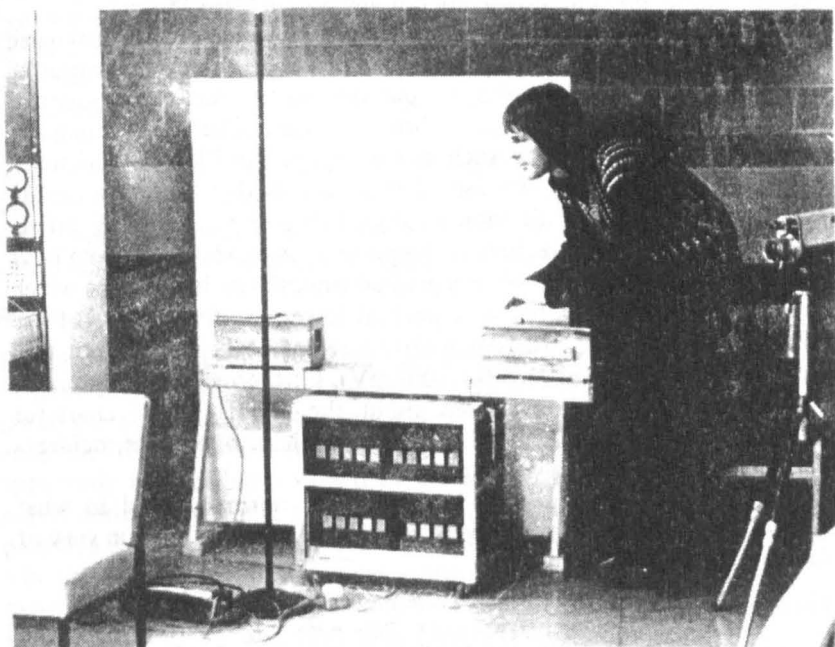


Figure 2. Matthew Manning attempting to influence infra-red equipment. He is holding hands over source box and looking at digital voltmeter (not in picture) for feedback.

The digital voltmeter registered the state of the IR beam, flickering permanently around the 1000 mark when the beam was 'on' and there was nothing in between the boxes, meaning 'zero occultation', 'nothing obstructing the beam' or, of course, 'all the beam is getting through to the cell'. If anything was put in the way of the beam, the reading went down, say to 750, which would mean a 25% occultation. Matthew's

chanting 'down, down!' reflected his attempts to reduce the voltage so as to show a figure lower than 1000. This meter was installed by D. Chapman in response to Matthew's request for feedback, so that he could see whether he had indeed affected the IR.

The permanent record corresponding to the flickering voltmeter (though with far from perfect sensitivity) was made by the large chart pen recorder on the mezzanine floor. (Unfortunately the pen registering occultation ran along a base line when there was no occultation but went *up* in response to *lowering* of the voltage or increased occultation. Thus (geometrically) 'up' on the permanent chart record corresponds to (arithmetic) 'down' in the readings of the voltmeter. This was perfectly clear to participants, but presents problems in interpreting the audio record, where 'up' and 'down' are not always qualified.)*

As will be seen, during three of the experimental sessions prolonged irregular deflections of the pen were recorded on the chart, indicating sudden lowering of the voltage input designed to measure amount of occultation. The colloquial term 'bout' is used deliberately to indicate sets of irregular deflections such as are depicted in Figure 3 (discussed below). The reason for the use of this term is that there is a certain arbitrariness in the sub-division of any given group of irregular deflections. It is impossible precisely to pinpoint in time when any given bout starts and stops: there is often a gradual smooth rise in the trace which would have been reflected in a gradual lowering of read-outs of the voltmeter, and return to baseline after a set of peaks is often not to a placid horizontal line at zero (or 1000mV). Furthermore, the clumps of irregular deflections found from about the middle of the chart for Friday 28 July onwards are often not divisible into separate incidents without doing violence to the data.

Surnames only (except for Matthew) are normally used in what follows to save space. Initials were considered too confusing in view of the number of participants.

Tuesday 25 July

It had been originally planned to place some object in the path of the beam itself to see if Matthew's attempts to influence the object would affect the beam. Osty, Price, and Hope and Rayleigh had used an object such as a handkerchief for this purpose. Brookes-Smith, in his manual for the use of the infra-red apparatus, had suggested a strain gauge, but in the event we used small pots of cress, as Matthew declared that he had no interest whatever in physical objects, but was keen to attempt to influence organic systems, since he believed he had been so successful in this in the United States fairly recently.

The small pots of cress were placed in the path of the beam and suitable instrumental adjustments made. Matthew attempted to 'make

*See footnote on page 348.

them grow faster'. He placed his hands on either side of the beam and attempted to 'promote growth'. The path of the beam was protected by tapes stretching from source to detector box to prevent his accidentally occulting the beam with his fingers. At no point was any occultation observed while he was trying to influence the plants. However, he said that he had a feeling of coldness on his hands, 'like ether'.

There is ample evidence in the discussion recorded on the audio tape, that there was an 'upward' drift in one of the recording pens (corresponding to a gradual lowering of the voltage). Such a trend would normally be interpreted as indicating a lowering of temperature of the apparatus which would, if anything, have been expected to rise the longer it was switched on. However, this drift was subsequently (next day) attributed by Chapman to the running down of the battery, since he found that the battery, supposed to have been fully charged but not by himself, was running down.

Matthew suggested at this time that he would like to attempt haemolysis experiments in the infra-red beam which were subsequently put into effect. It should be stated at this point that at no time was there any irregularity in the infra-red record in response to Matthew's attempts to influence the samples of blood. No attempt was made to examine the cress itself, which would have been quite impracticable.

Wednesday 26 July

In the morning Ellison and Chapman attended to the instrumentation in an attempt to take care of the drift of the pen recording the infra-red noted the previous day. Matthew did try, at Ellison's request, to influence the IR, but without effect. After the battery was found to be apparently responsible for the drift, the apparatus was put on stabilised mains during lunchtime, and after a drift due to the normal heating up period, a stable, horizontal trace was obtained.

In the afternoon, Matthew was being kept waiting for a haemolysis experiment because Brown was making adjustments to his equipment. Ellison went to try and find him. Meanwhile, the first poetry experiments were being tried out by Gregory at Matthew's request (see page 309).

After one conceivably relevant correspondence, four more poems were used, but the drawings and captions made by Matthew bore no relationship to the enclosed verse that he or Gregory could discern. Mary Rose Barrington, in an attempt to encourage him, tried to find some barely conceivable relationships between drawing and lines which seemed farfetched in the extreme to the others present. Matthew clearly found this exasperating and the more irritated he grew, the more she endeavoured to provide barely imaginable correspondences of meaning. Gregory was far from delighted by this well meaning but in

her view ill-judged attempt to jolly Matthew along. The audio recording bears witness to an atmosphere in the laboratory tense with irritation, although only Matthew, characteristically enough, expressed his vexation in so many words. Gregory then went to collect Ellison and Brown.

During this time, after some further verbal sparring between Matthew and Barrington, he got up and put his hand in the beam to see if the equipment was working. This incident is recorded on the chart and was marked as normal occultation. He was at that point under observation by Chapman and Barrington, who noted that he was not in contact with the equipment. Within a minute, Chapman pointed out that there were irregularities in the trace (Figure 3). As can be seen, the trace begins to rise, gradually at first, and soon, severe irregularities in the trace manifested for the first time. During this time, Matthew was holding his hand well above the photocell of the IR, attempting to lower the voltage on the DVM.

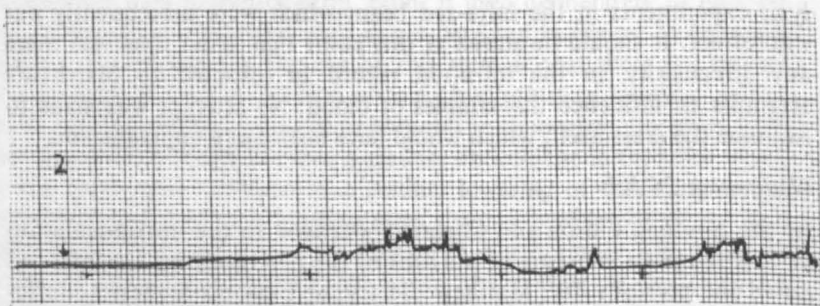


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Figure 3. Beginning of the first set of irregular deflections, afternoon 26 July 1978, chart 1. Chart speed 50 mm per minute.

Approximate period covered by entire illustration is 2¼ mins.

Neither from the audio record, nor from anybody's recollection, does it appear that Matthew consciously and deliberately *initiated* whatever it was that caused this irregularity in the trace. He himself is heard to describe the events as a good instance of a 'spontaneous' influence. He expressed his agitation, which he attributed at least in part to still being kept waiting. At some point which is not entirely clear, Ellison, Brown and Gregory returned to the lab., the two latter proceeding immediately to the preparation for the next haemolysis experiment. Chapman expressed puzzlement at the irregularities and Barrington called for witnesses. Matthew is heard to observe, somewhat bitterly, that 'nobody else is interested in these experiments'. The end of this bout of irregular deflections is unfortunately not accompanied by audio record because the tape ran out.

When the tape recorder comes on again, there is conversation where both Barrington and Chapman testify that Matthew was not touching anything while irregularities were occurring.

The fourth bout began at approximately 16:42. Participants called out the numbers on the digital voltmeter which so far as can be ascertained on this and subsequent occasions, corresponded with reasonable accuracy in time and magnitude to the deflections shown on the chart record. The analogue voltmeter also corresponded to both other indicators. After this, Matthew was asked to go away and Ellison and Gregory simulated the movements Matthew had made over the apparatus to test whether deflections such as those observed could be caused by normal factors such as shadow, proximity, etc. However, their movements did not have the slightest effect on any of the indicators, i.e. digital voltmeter, analogue voltmeter or chart recorder. At this point video recording equipment was introduced, operated by Chapman. Matthew returned, and after another two bouts, the tracing returned to normal.

Matthew had by this time taken to addressing himself to the digital voltmeter, 'willing' it to go down. The chart recorder was only set to reflect a lowering in voltage, i.e. occultation of the beam, and not an increase in voltage. In view of Matthew's preoccupation with the voltmeter, and to see if it was the voltage rather than the IR beam that was being affected, it was decided to ask him to 'will' the digital voltmeter to go in the opposite direction, i.e. 'up'. Since this could not be reflected on the chart, systematic reading out aloud on the audio recording was substituted. According to the audio record, the readings on the digital voltmeter rose for a period of approximately five minutes, the highest voltage read-out being 1032. This, as Chapman observed at the time, was only one tenth of the value obtained in the opposite direction, and it could be deemed to be within the noise level in the system. (On subsequent occasions, Matthew showed a strong preference for 'making it go down' rather than up. Later attempts were made to re-adjust the baseline but these turned out to be un-satisfactory.) At 17:00 Matthew said, 'It'll be upset for at least an hour' and

I just felt that I was controlling it and that it would do whatever I told it to do. That's why when it was going down I could make it go lower and when I decided to turn around and make it go up I just told it to go up.

Ellison: 'You just told it as though it was an animal doing what you said?'

Matthew: Yes. But I really believed myself at that moment that it was going to do exactly what I told it to. Just as now I believe

that it will take a long time to settle down again. Something has got into that which it will hold for a long time. (A3/1)

However, there was only one further bout lasting about 2 minutes after which the machine settled down to normal and the chart was signed at about 17:09 and Matthew left the laboratory soon after.

Thursday 27 July

No infra-red experiments were envisaged for this day and from the audio record it would appear that although the IR equipment was running, the chart recorder was not switched on. However, some impromptu experiments were carried out by Ellison, encouraging Matthew to affect the beam. It would seem that some instability in the voltmeter was detected. However, since Chapman repeatedly stated that he was not satisfied that the instrument was as yet stable, and since there is no chart to provide a permanent record of instrumental deflection, the results dictated onto the audio record should, in our view, be disregarded. It is, however, worth noting that according to the audio record, attempts to influence the beam by means of a magnet and by means of a hair dryer failed to affect it in any significant manner.

Friday 28 July

Infra-red experiments were expected to take place sometime during this day, although no exact timing was determined. It had been arranged to start with haemolysis sessions and on the previous evening Gregory had arranged with Dr. and Mrs. Grattan-Guinness, who rang her up from Devon, that they would be attempting to 'send' some telepathic messages starting precisely at noon, to be determined by the BBC time signal. I. Grattan-Guinness had originally been asked to be a member of the investigating team, but he was unable to attend in person, since his holiday had been previously arranged. Immediately when Matthew arrived at the laboratory on Friday morning, he refused to have anything to do with the Grattan-Guinness experiment on the grounds that if they couldn't be bothered to be present, he couldn't be bothered to do experiments with them.

A chart is available for the whole day from 10:53 to 16:00. After the trace was stable, the whole infra-red equipment was moved so as to make it more accessible to the video camera. The transportation of the instrument is recorded on the chart trace by very small irregularities in the baseline. The trace then immediately settled down to complete stability, from 11:15 onwards, and a haemolysis experiment was undertaken. At 11:54 a video recording was started. Although Matthew's refusal to collaborate with the Grattan-Guinness experiment was briefly referred to in connection with the conduct of other experiments, no

further attempt was made to persuade Matthew to change his mind.

At approximately 11:58 (as subsequently estimated by reference to the speaking clock) the chart trace leaves the baseline and described a shape never previously nor subsequently observed (Figure 4).

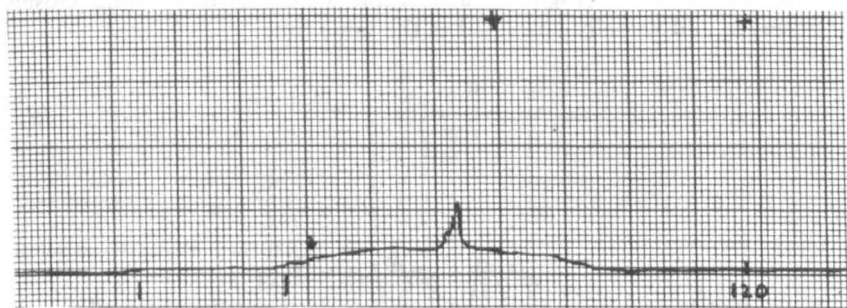


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Figure 4. 'Grattan-Guinness peak', 28 July, Chart speed 25mm per minute. Approximate time covered by entire illustration is 11:57 to 12:02.

Sullivan was observing the chart at the time and called out to Chapman who immediately alerted the other participants. From the audio record it would appear that Matthew was not in the experimental area at the time, since a haemolysis control experiment was in progress. This was noted by Gregory, Chapman and Matthew himself. When attempts were made to establish the exact time, since this was about noon, the conversation turned naturally enough to the Grattan-Guinnesses, who were presumably sitting somewhere in England sending unrequited messages! However, haemolysis was resumed and while Gregory's voice can be heard counting down the seconds, Matthew's voice is heard whispering 'down, down' a number of times. It seems clear from the audio record that Matthew had switched interest from haemolysis to infra-red at this stage. At this point the chart baseline is nearly stable, though not perfectly straight.

Between 12:09 and 12:10 Matthew asks 'is Ivor Grattan Guinness known as a great psychic?' with the clear implication that paranormality might be imputed elsewhere, and he goes on to speculate that he might have unconsciously picked up the Grattan-Guinness messages and translated them into activity in the infra-red. From this point on the chart trace shows a fairly steady rise, whilst Matthew is addressing the voltmeter, adjuring it to go down. Between 12:13½ to 12:15, there is a characteristic jagged bout after which irregularities continue at a much lower level until about 12:17 at which point the trace settles down again.

At about this time, Mr. Roger Chapman, Senior Lecturer in Electronic Engineering, a member of Professor Ellison's staff, arrived in the laboratory. There are introductions and the apparatus was explained and demonstrated.

At about 12:25 the trace begins to rise again and there is a characteristic jagged bout (Figure 5 and Table 6.2), Matthew being closely observed and verbally described at the same time.

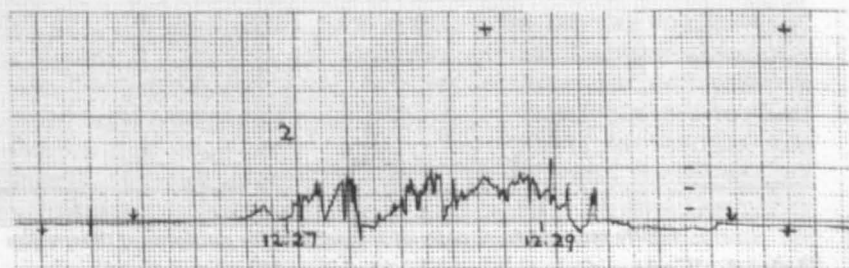


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Figure 5. Irregular deflections 28 July, chart 2. Chart speed 25 mm per minute. Approximate time covered by entire illustration is 12:25 to 12:30½. Table 6.2 refers to coincident events from 12:27 to 12:29 marked in illustration.

The video apparatus is on during part of this incident showing Matthew passing his hand back and forth over the box. As described by Ellison the trace drifts back towards normal baseline and Roger Chapman mimics Mathew's movements. This also is recorded on video tape. Unlike Matthew, Roger Chapman repeatedly touched the box which manifests itself in vertical cross-hatching deflections, apart from which the trace is horizontal and at zero. There followed some attempts at normal simulation of the observed traces.

Between approximately 12:34 and 12:35 Sullivan physically shook the base on which the IR instrument rested without obtaining more than a thickening of the horizontal trace. Gregory occulted the beam normally by first placing her hand in the path of the beam and then by rapidly flicking her fingers into it. Brown inserted a thin box into the beam (Figure 6). The traces of these normal occultations look entirely different from the irregular ones under investigation.

About two minutes later Matthew attempted to influence the beam from a distance of approximately four metres from the apparatus. About a half minute later, the trace leaves the baseline rising to a peak, although its height can not be exactly determined at any one moment because at that point the motor was accidentally switched off, the chart stopped moving and the pen recorder went up and down on the same

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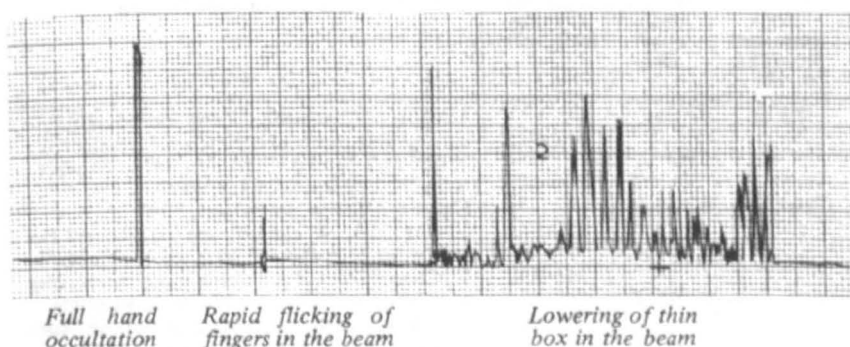


Figure 6. Normal occultations, 28 July, chart 2. Approximate time covered by entire illustration is 12:35 to 12:41½.

spot for at least a minute. It then resumed its jagged path while Matthew was attempting to influence the beam from a distance of about three metres. There is a sudden recovery to zero and Gregory's voice is heard saying 'Matthew is now relaxing'. The trace begins to climb again soon after, while participants are making an attempt to persuade Matthew to relax. He explains that he 'cannot stop thinking about it' and expresses the opinion that the instrument will be disturbed for some time. At this point, about 12:50, Brown and Gregory took Matthew out of the laboratory for a walk around the square, leaving the electrical engineers and technicians, Ellison, David Chapman, Roger Chapman, and Sullivan, to discuss the behaviour of the equipment. Hardly had Matthew *left* the laboratory than the pen resumed its aberrant course. It did not settle down until six minutes later. The engineers switched off the IR source, re-set the baseline to midpoint, and switched it on again. Matthew made no further attempts to influence the IR that afternoon. He felt it unlikely that any more would happen as he 'felt drained'. The chart record was completely straight from 13:06 to 16:00, and is signed by participants.

Monday 31 July

The chart for this morning is completely level from 10:38 to 13:00, indicating that there was no occultation or fluctuation in the voltage beyond noise level throughout this period. The power source is the mains. During this time there is general discussion (including a discussion of the 'Grattan-Guinness peak') and a haemolysis experiment. Shortly after midday, Matthew says 'I had to stop myself from doing it [the IR] this morning... I want to wait for this afternoon'.

The chart for the afternoon is in many ways the most problematic

1 Notional clock time and recording monitors	2 Combined Audio/ chart time	3 DVM chart readings (millivolts)	4 Transcription of audio record
12h27'0" Audio & Chart	87'26.0"	850-925	Gregory: 'Absolutely clearly - no possibility of any deception - Matthew's got his sleeves rolled up'. Brown: 'And he's not in contact with the apparatus in any way'. Gregory: 'Seven, seven, yes, 790'. Ellison: '78, 78, can you see that Roger? Do you want to come and have another look?' R. Chapman: 'Oh it's much more interesting watching it....' Ellison: 'We're all watching that space - there's nothing going in the beam. Just waving his hands about over the top'.
	87'30.8"	800-960	
	87'35.6"	750-870	
	87'40.4"	745-950	
	87'45.2"	780-875	
12h27'24" Audio & Chart	87'50.0"	725-950	Gregory: 'Seven' Matthew: 'I've been down to six something'. Ellison: 'Yes you did - it's back up now, 89, yes, it's going down again'. Gregory: '8, yes'. Ellison: '89, 88, 86, 5, 3, 84, 82, 80'.
	87'54.8"	720-975	
	87'59.6"	975-900	
	88'04.4"	900-975	
	88'09.2"	925-950	
12h27'48" Audio & Chart	88'14.0"	860-925	Gregory: 'I am watching the space the whole time...' Ruth West: 'That's seven, six, up to six'. Ellison: 'Yes' Gregory: 'Nothing at all can be seen between the
	88'18.8"	825-925	
	88'23.6"	730-890	
	88'28.4"	740-875	
	88'33.2"	675-820	
12h28'12" Audio, Video & Chart	88'38.0"	720-945	cell and the receiver'. Ellison: 'Make sure you get the picture of both things, you can see the meter going down as he goes like this around it. That's what we call PK that does this'. Gregory: '...751, 752.....
	88'42.8"	740-900	
	88'47.6"	800-875	
	88'52.4"	770-825	
	88'57.2"	725-775	
12h28'36" Audio, Video & Chart	89'02.0"	725-800absolutely marvellous'. Ellison: '72, yes, 73' (Matthew makes sound like 'phew') Gregory: 'Arthur, I would just quite like to go out a bit - could you and Ruth.....
	89'06.8"	780-850	
	89'11.6"	750-850	
	89'16.4"	715-780	
	89'21.2"	725-860	
12h29'0"	89'26.0"		...Mary Rose would hang us otherwise if we don't.....

TABLE 6.2

Legend for Table 6.2

This diagram shows in tabular form a two minute period of the events described from 12:27 to 12:29 on 28 July (chart 2)*. The chart paper is divided into 1cm squares which are further subdivided by five vertical fine lines and ten horizontal fine lines, such that time can be read on the horizontal and voltage on the vertical axis (see page 318). This chart was run at a speed of 25mm per minute.

Column 1 provides notional clock time; this was calculated by taking initial clock time written on the chart and measuring along the horizontal axis and converting centimeters into times. The word 'notional' is used because of possible variations in chart speed. There is, however, reasonable coincidence with frequent verbal read-outs on the audio record, and occasional timed events on the chart itself. This column also shows monitoring devices used during any given 24 second interval. A 24 second interval was chosen because 1cm represented one 24 second interval at the chart speed on this occasion.

Column 2 combines audio and chart time. The matching of the audio tape time to chart time was done by listening to the entire audio tape for the period covering the chart time and choosing certain clear words which were to be timed to provide an approximate fit with the chart. The first clear word uttered near the beginning of the audio tape covering the chart time was assigned a time of zero, and each chosen word was timed in relation to this first word using a Casio PQ-7 stopwatch. A suitable point on the chart was selected where a total occultation of the beam was effected by Gregory, accompanied by the words 'I'm putting my hand in the beam now', 'now' coinciding with a reading of zero on the voltmeter. The audio tape was then started, taking the first clear word as $t=0$; the intervening pre-selected words being timed in relation to t_0 , so that an actual time could be established for the word 'now'. It was thus possible to fit dialogue to chart by making a rough equation between Casio time and the chart time as determined by chart speed. Because the chart and audio recorder did not move at precisely identical speeds, only approximate corresponding times are available, but the coincidence between audio and chart record is reasonable. Each 24 second interval is divided into five fine vertical lines, which thus represent time markers for intervals of 4.8 seconds, shown in this column.

Column 3 shows the extreme voltmeter readings recorded on the chart in the interval represented by the chart times in column 2, beginning with the time immediately above left of the mV readings and ending immediately before the next chart time, left below of the mV readings; e.g. between chart time 87'26.0" (inclusive) and 87'30.8" (exclusive) the lowest reading reached was 850, the highest 925. (It will be remembered that the lower figure measures the higher degree of occultation if this is what is being measured.) Millivolts are determined by reference to the fine horizontal lines. Here the precision is limited by, among other things, the thickness of the pen trace and the responsiveness of the chart recorder to voltage fluctuations, which is not necessarily identical with that of the digital voltmeter.

Column 4 is a transcription of the audio record for the two minutes covered by the events described from 12:27 to 12:29. In this column the dialogue is given as corresponding to the times calculated for column 2.

*see Table 6.3

chart for a number of reasons. Once irregularities appear on the chart recorder, they hardly cease at all for the entire period from 16:07 to 17:50. Also, timing becomes virtually impossible because at one point the tape runs out and there is no clear indication of the time at which it comes on again. There were so many changes in the apparatus that it is difficult to be certain whether a stable baseline was obtained at any time within this period.

There were two visitors in the laboratory, to be called Professor D.G. and Dr. J.B.. Matthew clearly set himself to 'hex' or disrupt the apparatus (psychically), ignoring all pleas from participants to let go and relax.

At approximately 1:40 and 1:43, there are two departures from the baseline on the chart, which is now set at midpoint, to about 900. Although the audio tape was not running at the time that they occur, subsequent dialogue between Ellison and Matthew suggests that they probably coincide with Matthew's entry into the laboratory. The horizontal trace which continues to show a very slight drift was re-set at intervals.

Brian Inglis arrived at about 15:00. At about 15:04 Chapman noted that irregularities were beginning to take place and Matthew said 'If it's started, then we should start'. There was a bout of irregularities starting at approximately 15:07 stopping abruptly about a minute later, after Matthew had accidentally hit the box. The trace is approximately horizontal, apart from a slight drift, until 15:27. During this time Matthew was attempting to influence the beam without any apparent success. He relaxed, and the slight upward drift continued.

Between about 15:27 and 15:29 Chapman returned the chart recorder to the original configuration.

At about 15:32 D.G. arrived, immediately after Brown and Gregory had left in search of some other apparatus. Ellison explained the apparatus to him and was called out of the laboratory by his secretary.

While Inglis and D.G. were discussing other matters, Matthew drew their attention to the fact that 'it's going down again' and at about 16:07, there is a burst of irregular activity for about half a minute, peaking at about 800mV. At about 16:08, the trace rises again and does not really settle down until 17:50. No discrete bouts can be discerned, nor can any relationship be claimed between psychological or behavioural variables and the vagaries of the chart trace, without the most specious pleading. On three, possibly four occasions, the trace passes the 500mV mark, corresponding to an occultation of just over 50%.

Brown and Gregory returned about 16:13. Just before 16:20 Chapman switched off and on again the 5 volt supply to the lamp to clear any possible switch contact fault. The trace continues its irregular course. At about 16:22½, Chapman changed from the rectified mains (25 volts)

to a battery (25v) supply to regulator. The trace continues irregular. Just before 16:24, there is another change of battery, the trace is normal for about 22 seconds and then becomes jagged once more. There is yet another change just after 16:31 when it was decided to change back to the mains but with an alternative voltage regulator. Chapman challenged Matthew 'now muck that one up', and the trace remains stable for three minutes. However, after some adjustments by Chapman, so marked on the chart, and while Matthew is trying to rise to the challenge, the trace rises once more, to resume its jagged irregularities.

At about 16:55 J.B. arrived. All the while the trace continued its irregular course. Matthew resisted all attempts to get him to calm down and (naturally enough) J.B. expressed his assumption that it was simply a question of discovering the fault in the equipment. He suggested CO₂, carbon dioxide as a possible cause, and the trace reflects the introduction of CO₂ into the beam by means of a fire extinguisher by one of the participants. The resulting occultation is far more violent than anything observed previously or subsequently, and it is hard to see what such gross interference with the apparatus could have shown, since J.B. was presumably referring to exhalation of breath. J.B.'s alternative suggestion for explaining the aberrant behaviour of the apparatus was variations in the power supply; he also suggested electrostatics as a possible cause. Before leaving he recommended for future reference that the amplifier be rebuilt and that low noise cable be used, as well as batteries sealed in metal boxes without leads or switch contacts.

At about 17:50 Chapman had completed re-setting with a new power supply and the trace returns to normal, despite several attempts by Matthew to influence the trace. Matthew pointed out that he was now exhausted.

At about 18:09 the power supply was changed back to the original mains and the trace continues perfectly steady until 18:20 when the chart ends.

There is some dialogue at the end of the audio tape for this period in which Ellison considers J.B.'s suspicions of the power supply. After further discussions, the power supply seems ruled out as a source of trouble and the only remaining possibility described by Ellison is some loose contact that cured itself.

Tuesday 1 August

The infra-red equipment was set up all day from 10:25 to 17:16. Matthew was at Birkbeck College and only Chapman and Sullivan were in the laboratory. The trace is horizontal throughout, apart from a very slight, flat upward deflection to 995mV during the warming up period, labelled 'shadow'. No sign of voltage fluctuations or loose contacts were registered.

Wednesday 2 August

The IR equipment was set again all day and there is a chart from 10:40 to 16:53. The trace is virtually horizontal throughout; there are however, some very small occasional irregularities which it is impossible to time with any accuracy since chart speed, which is not explicitly recorded, was varied. In addition, a fault had developed in the timing mechanism of the chart recorder as reflected both in the pen trace indicating minutes, and also in recorded speech.

Mr. I Bloomfield visited the laboratory in the morning about 11:30 to 12:00 and Matthew attempted to influence the beam for him. There are very slight irregularities in the trace not noticed at the time, but there are similar irregularities after everybody had left the lab. In any case, this chart shows a tendency for small blips to occur lasting a minute immediately following switchings and re-settings. The overall impression however, of the trace for the entire day, is flat and horizontal.

In the afternoon Matthew went to St. Bartholomew's Hospital accompanied by Ellison, Gregory, Chapman and Brown to have his electroencephalogram recorded by Miss Marion Smith. Matthew made his distaste for the entire proceedings more than plain. In an attempt to distract him, he was given four of the poetry envelopes. Gregory wrote notes, using her watch (which was between one and two minutes fast as subsequently checked) to give approximate timings. At approximately 15:18 she suggested that Matthew might try influencing the infra-red in the laboratory from a distance of approximately three-quarters of a mile. Again, at about 15:20½ she asked Matthew to have a try at influencing the IR, using the words 'come on Matthew, that'll be a world first'; and he made the attempt. About a minute later a telephone call was made to Sullivan, asking him to check that the equipment was running and to leave the laboratory, locking it behind him.

From 13:10 the trace is absolutely flat, until a few perturbations appear at times subsequently calculated by Chapman (audio tape) as occurring at 15:16 and 15:22½ respectively. The first of these irregularities is a very slight but noticeable protracted wobble, lasting about a minute. The second is a very slight step in the trace, such as might occur when the apparatus is re-set. In our view these irregularities on balance may not be interpreted as being significant.

The trace thereafter continues entirely flat until approximately 16:53.

DISCUSSION OF FINDINGS

It should be remembered in discussing findings that these experiments were part of a block investigation, and were designed in the first instance, to see whether the results obtained with Rudi could be replicated with

another Subject in whose case physical phenomena had been claimed in his earlier years. On the face of it similar tracings were obtained. However, the interpretation of these traces is subject to a number of qualifications to be discussed.

Source material

The main records available were the chart tracings, and the audio tapes available for almost the entire period of investigation. Although video equipment was used from time to time and certainly corroborates the audio record, e.g. it shows Roger Chapman trying his hand at influencing the equipment, the recordings are too poor in quality and too few in quantity for more to be claimed for them than occasional illustrations and corroboration that no one was near the instrument while strong deflections were occurring.

Table 6.3 shows the following chart records which are available:

Chart	Date	Approximate length of chart
1	Wednesday, 26 July	1hr. 7min.
2	Friday, 28 July	5hrs. 7min.
3	Monday, 31 July (morning)	2hrs. 22min.
4	Monday, 31 July (afternoon)	5hrs. 10min.
5	Tuesday, 1 August	6hrs. 51min.
6	Wednesday, 2 August	6hrs. 13min.
7	30-31 Oct., 1-3 Nov.	33hrs. 58min.
8	16 November	4hrs. 30min.
9	3 January 1979	1hr.
10	12-16 February 1979	

TABLE 6.3

Irregularities of the trace are unambiguously recorded only on three days on charts 1, 2 and 4. It would be too expensive to reproduce these here.

It is however possible to present rough arithmetical profiles of some aspects of charts 1, 2 and 4, (see Tables 6.4, 6.5 and 6.6).

Explanations of Tables 6.4, 6.5 and 6.6

For reasons which will become apparent in the discussion these are unsuitable for assessment of statistical significance. They may, however, be useful for descriptive purposes, and give an idea of the durations of time during which the trace did, and when it did not, depart from its normal baseline. It also provides some idea of the extent of irregularity, that is, degree of occultation (or lowering of voltage).

The first row down (a) in each case indicates the lengths of time in minutes and decimal fractions of a minute for which the trace remains at the baseline (i.e. no occultation).

The second row down (b) indicates the lengths of time in minutes and decimal fractions of a minute during which significant occultation is observed (as indicated by a decrease in voltage below 975 mV). These two lines must be read in conjunction as a vertical zig-zag sequence, and represent successive times, e.g. chart 1, beginning: 1.64 (below 975mV), 0.12 (above 975mV), 0.02 (below 975mV), 0.04 (above 975mV) etc. Asterisks (*) indicates that normal causes for deflection are known for the amount of time indicated. These asterisks occur in the (a) column if such normal deflection occurred during periods of quiescence, e.g. test moving of apparatus; they are put in the (b) column when normally caused occultations were made during bouts, e.g. to test that the IR was working properly. Occasional comments without asterisks describe salient events.

The third row down (c) indicates maxima of occultation (or minimal voltage measured in millivolts) reached during any particular period immediately to the left of the voltage figure. Thus, on chart 1, during the first 1.64 minutes the lowest voltage reached was 795mV; during the next brief departure from the baseline for 0.02 minutes the greatest departure was 970mV and so on.

In all cases we have concentrated on the portion of the chart where disturbances below 975mV occur, which we have called the 'bout period', defined as beginning at the point at which the trace leaves the baseline for the first time, and ending at the point when irregularities cease. Departure from the baseline to an extent greater than 975mV was selected as amounting to an 'appreciable irregularity'; as the normal fluctuation (see apparatus section) of the DVM was approximately 980-1020mV and the first fine line parallel to the base on the chart record indicates a level of 975mV. In the case of the first two charts, the total bout period is described arithmetically. Chart 4, as depicted in Table 6.6, is only described to that point on the chart after which such measurement has become impossible, because for the chart speed used (25mm per minute) the trace returns to zero so frequently that analysis is impossible without gross distortion. Also, so many tests were subsequently carried out, notably flooding the instrument with CO₂ by means of a fire extinguisher, that any analysis would be futile.

Throughout chart 1 there were prolonged periods of time, lasting several minutes, when the trace returns to base. Towards the end of chart 2 there is a decrease in the length of these periods of quiescence. In chart 4 this breakdown of quiescent periods occurs quite early and it can be seen clearly from the figures that there is rapid oscillation between baseline and departure from baseline.

It can also be seen from the three charts that the maximal occultation (lowering of voltage) reached increased somewhat throughout the three days, being: chart 1: 650 (once); chart 2: 630 (once); and chart 4: 450 (twice).

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a Chronological duration in minutes at 1000 ± 25	b Chronological duration in minutes below 975 mV	c Minimal mV (Maximal occultation)
	1.64 (Figure 3)	795
0.12		
	0.02	970
0.04		
	0.06	965
0.04		
	0.02	970
0.02		
	1.46	790
0.02		
	0.44	845
1.84		
	0.78	850
0.88		
	1.16	780
0.88		
	0.38	960
0.06		
	0.12	950
0.06		
	0.04	945
13.20 (*4.67 adjusting equipment)		
	2.14	695
7.38 (Ellison and Gregory try)		
	0.18	770
0.44		
	0.38	910
0.02		
	0.14	925
0.02		
	0.28	900
0.02		
	1.6	720
0.02		
	1.34	650
8.1 (Matthew tries to raise DVM)		
	1.44	810
TOTAL = 33.16	TOTAL = 13.62	
TOTAL BOUT PERIOD = 46.78		

TABLE 6.4 Chart 1

a Chronological duration in minutes at 1000 ± 25	b Chronological duration in minutes below 975mV	c Minimal mV (Maximal occultation)
	1.76 (Matthew not in lab; Figure 4)	720
11.60		
	0.8	965
0.12		
	3.48	640
0.04		
	0.4	915
6.48 (*2 test occultation)		
	0.04	965
0.12		
	0.04	960
2.32		
	1.8 (Table 6.2 & Fig 5)	720
0.16 (Table 6.2 & Fig. 5)		
	1.6 (Table 6.2 & Fig 5)	630
0.04 (Table 6.2 & Fig. 5)		
	0.36	765
0.72		
	0.12	960
12.92 (*2.68 test occultation)		
	0.28	965
0.12		
	0.32	685
0.68		
	0.4	750
0.4		
	0.16	760
0.12		
	1.28 Matthew at 3 metre distance	750
0.8		
	0.12	960
0.16		
	1.56	790
0.04		
	0.12	925
.08		
	4.36 (*1.28 test) Matthew out of lab.	675
.08 Matthew out of lab.		
	2.08 Matthew out of lab.	650
TOTAL = 37.72	TOTAL = 20.08	
TOTAL BOUT PERIOD = 57.80		

TABLE 6.5 Chart 2

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London Experiments with Matthew Manning

a Chronological Duration in Minutes at 1000 ± 25	b Chronological Duration in Minutes below 975 mV	c Minimal mV (Maximal occultation)	a con't	b con't	c con't
	7.8	740	0.004		
0.04				0.06	875
	1.84	450	0.004		
5.32 (*.04)				0.02	875
	11.76	860	0.008		
18.4 (*1.04)				0.05	875
	0.16	945	0.01		
22.6 (*2.2)				0.02	865
	0.64	835	0.19		
0.6				0.008	915
	0.88	750	0.008		
0.04				0.08	730
	0.44	660	0.02		
0.04				0.05	820
	1.48	710	0.03		
0.04				0.08	800
	0.48	660	0.02		
0.04				0.02	700
	0.2	700	0.02		
0.04				0.02	850
	0.52	660	0.01		
0.04				0.22	750
	1.8	635	0.02		
0.16				0.008	965
	1.08	650	0.02		
0.04				0.01	965
Alt. ch. spd. .12/1.61(=1.73)			0.02		
0.02				0.1	965
	2.54(*.06)	560	0.07		
.12 (*.04)				0.48	770
	0.06	800	0.01		
.004				0.02	925
	2.79(*.05)	600	0.008		
0.01			Alt. ch. spd. .79/4(=.83)		750
	1.3(*0.1)	560			
0.36			TOTAL =	TOTAL =	
	0.22	735	48.466	39.806	
0.08			TOTAL BOUT PERIOD		
	0.01	930	COVERED = 88.272		

TABLE 6.6 Chart 4

Collation of sources

Transcriptions were made of all available audio recordings irrespective of whether there was a chart available for that day or not. Although some rough notes were available, the final transcription (118,000 words) used for interpretative purposes was the responsibility of Kathy Wilson, who was familiar with the voices of the participants but had not been a member of the original team, to avoid certain artefacts of suggestion which had been shown to be invidious. In the case of ambiguity, she omitted the words or groups of words in question. Because of the informality of the arrangements, a verbatim record of the proceedings is clearly desirable if not essential, and there is good reason to suppose recording did not unduly inhibit participants from expressing themselves. From the transcript it is possible to reconstruct, with reasonable accuracy, what was done and when. Since the infra-red experiments, although roughly timetabled, were Subject initiated, it was clearly important to know whether irregularities are found only when the Subject attempted to influence the apparatus, or whether attempts were similarly made on days when the chart record indicates no irregularity. We also wish to stress that the auditory record is vital, since it quite often showed up serious discrepancies between the recollection of some participants and what in fact happened.

For those charts where irregularities occur, every effort was made to obtain as close a correspondence as is possible between chart and audio record. Indeed, this was one of the major points in having an audio record. For example, when an attempt was made to see if Matthew could raise rather than lower the voltage this could not be registered on the chart because of the way the baseline was set; but readings from the digital voltmeter were deliberately and collectively dictated into the microphone for the purpose of subsequent correlating with the chart record. The method described in the legend to Table 6.2 was applied to charts 1, 2 and 4.

Precision of coincidence in time is limited for the following reasons: only relatively approximate times were entered in the chart itself and frequently not at the very beginning of irregularities; audio tape is elastic, and repeated playing reached different numbers on the tape counter on different occasions; on several of the charts the time marker is defective, particularly charts 4 and 6; on two of the days the audio tape ran out for short durations, the precise length of which is not known; in chart 1, not only does the tape run out at one point, but there is no definite time marker; in chart 4, the tape runs out after the time marker and only very approximate timing is possible when the tape resumes. In addition to unavoidable changing-over of tapes, there were also some minor accidents, such as accidentally pressing of a

switch turning off the motor of the chart recorder.

Possible interpretations

It will clearly be seen that in view of this critique of the instrumental arrangements available, any attempt at precise co-ordination of chart and audio record would be spurious and misleading. There is, however, one observation that can be made, and which could only be made because so complete a transcript is available. This observation is to the effect that over and over again Matthew noticed an instability in a downward direction on the part of the digital voltmeter and only *then* decided to start to try and influence the *intra-red*. Here the audio record is supported by the long gentle upward slope preceding jagged irregularities (see Figure 7) which clearly reflects a gradual lowering of the voltage in the first instance. Fixing a beginning for 'bout periods' is therefore to some extent arbitrary. As has been mentioned, on one occasion he actually says 'It's started, so we should start'.

We propose to call this the 'bandwagon effect'. This term is not intended to pre-empt interpretations of either normality or paranormality, but it certainly affects the interpretation of the phenomenon studied. What it unfortunately invalidates is the negative success of other participants (such as Ellison, Gregory and Roger Chapman) to influence the equipment, whereas Matthew ostensibly did influence it. At the time, and even in retrospect, it seemed as though he *could*, and we *could not*, affect the instrument; however, scrutiny of the audio record in conjunction with the pen tracings shows that there is ample auditory evidence that Matthew tended to start from a base of instability, whereas the experimenters tried their luck when the instrument was stable.

It is however, certain that imitating Matthew's movements to the best of our ability did not create any disturbance in the record; therefore it is very unlikely that either shadows, breathing (CO₂) or electrostatics were responsible for the irregularities. It must also be pointed out that Matthew frequently exerted himself from quite a distance. On one occasion (chart 2) he was not even in the laboratory (so far as we can tell from the audio record) when deflections were first noticed (see page 323 and Figure 4) and on the same day they persisted when he had definitely left the laboratory.

While there is still a 'bout' structure, i.e. grouping in time (chart 1 and first part of chart 2) of deflections, whereas the initial slope is gentle, the end tends to be most abrupt (Figure 7). Timing is not sufficiently precise to allow us to distinguish between whether Matthew relaxed *before* or *after* the cessation of a bout of irregular deflections. He may well have seen that the digital voltmeter reverted to flickering around 1000 and relaxed his considerable muscular effort in consequence, rather than that he stopped exerting himself whereupon irregularities

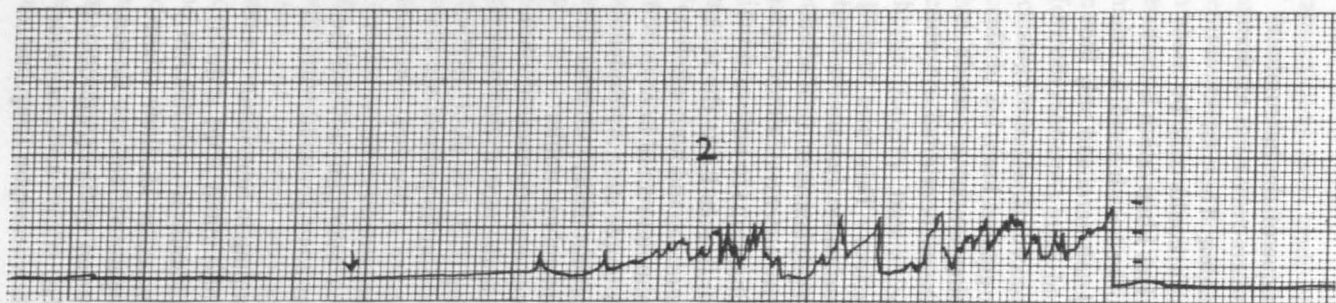


Figure 7. Irregular deflections, 26 July 1978, chart 1. Chart speed 50mm per minute. Approximate period covered by entire illustration is 3¾ minutes. Note gentle slope suggesting 'bandwagon effect' and abrupt drop at end.
Figure reduced from copy of the original for purposes of illustration

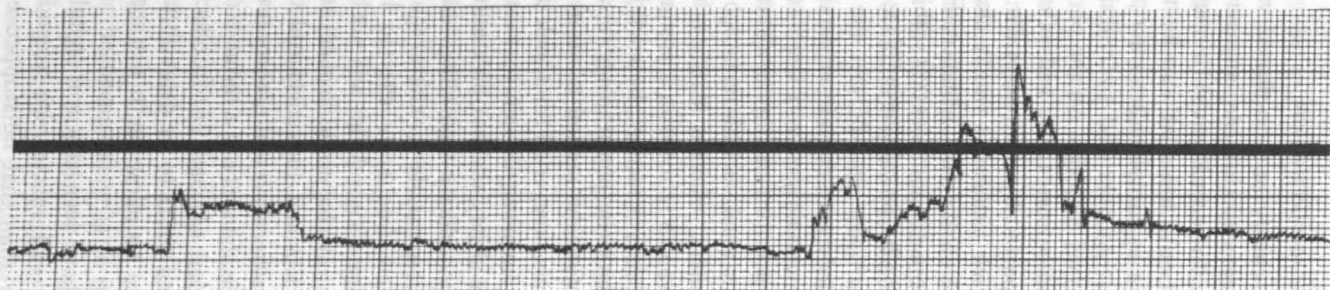


Figure 8. Part of Osty's Figure no. 35, referring to sitting of 26 May 1931. Trace ostensibly similar to tracings obtained with Matthew; note however absence of gradual initial slope.

ceased. He might easily have been unaware of such 'bandwagoning'. It has already been described how Matthew was unable or unwilling to relax efforts while irregularities were in progress, which certainly suggests that he went along with an irregularity rather than that he in some sense voluntarily caused it.

It must be emphasised that we have no reason to doubt Matthew's complete honesty; he was by no means the only one present to confound possible bandwagoning and psychic causation. Moreover, in order to simulate the trace normally it was found necessary either to wiggle one's finger in the beam in a very conspicuous manner, or to use implements such as steaming kettles. Such activities were entirely ruled out by the conditions of observation during the experiments, by the audio records, and the video records even allowing for their poor quality, and these possibilities of imitating the trace normally were only discovered later as a result of a good deal of experimenting with the equipment. In any case, Matthew was virtually never unobserved, nor did he ever make the slightest attempt to evade observation, and while irregularities were on, one person was always delegated (usually Ruth West) to watch the space between source and cell.

One of the problems for the interpretation of this set of chart records and conceivably future ones also is that, to begin with, irregular deflections cluster in groups separated by regular long stretches of quiescence. However, from the last part of the second and throughout the fourth chart, this clustering in time is lost and ever more continuous irregularities are recorded, increasing only slightly in amplitude or degree of occultation. However, the total proportion of activity over quiescence increases, as may be seen from Tables 6.4, 6.5 and 6.6. It might possibly be that an epidemiological or 'seizure' model might be found to be useful interpreting data of this sort.

In view of clear evidence for the bandwagon effect, the absence of clearly defined trial and control periods and the problems of precise timing, any detailed correspondence between possible psychological states as expressed in the audio record and irregularities of the chart trace is out of the question. In any case, the cluster or bout structure of the chart records disappears, and it is clearly impermissible to pick out what seem subjectively meaningful episodes from the transcript, divide the instrumental record accordingly, and argue back and forth towards some *ad hoc* psychological concordance, let alone base statistical calculations on such manipulations!

It is however appropriate to discuss whether the irregularities could have been due to normal causes and if so which. If such causes are not deemed probable sources of the irregularities observed, it should be considered what might be possible paranormal modes of psychological influence.

Possible normal causes of irregularities

As has been mentioned, a good deal of trial and error was necessary to find out how to simulate the trace normally; and such methods would have been impossible under the conditions of the experiment. Neither vibrations, shaking of the apparatus, introducing solid particles (such as cleaning powder and cigarette ash) nor dangling threads or strips of tissue into the intervening space produced any effect remotely resembling the curves shown. Deflections of comparable size though of different shape were obtained by breathing into the beam, and these were due to misting over of the lens. On several days after the experiments (31 October, 1 November, 2 November and 3 November 1978) D. Chapman ran tests and found the equipment stable and responsive to test occultations in the normal manner. Despite the fact that the lab was in normal use (on one occasion by eight students) and normal lighting and equipment were running, no irregularities were noted.

Fluctuations in electricity supply were among the favourite 'normal' explanations. (We have not counted the drifts that might be attributable to running down of batteries as paranormal, since Chapman was not able to guarantee that power packs had been perfectly charged when he received them. However, some sort of paranormal drain on the power supply on the lines of the Rosenheim case (Bender 1974) cannot in principle be ruled out.) Although ideally there should be constant monitoring of mains or better still all power supplied by fully charged batteries, the mains seems an improbable explanation for a number of reasons: irregularities also occurred while the power was supplied by batteries; the supply was a stabilised mains in a department of electronic engineering, and furthermore, there were long periods of perfect stability before and during the investigation, and an indefinite period afterwards including the brief and unsuccessful February trials.

Instability of the bulb would affect the chart record and D. Chapman experimentally produced such fluctuations by loosening the bulb. However, the trace looks different from those under investigation, and it is difficult to see how quiescence would be spontaneously restored once the bulb had become unstable.

The possibility of a loose contact somewhere in the circuit is more difficult to exclude, but is not easy to reconcile with the long periods of stability before, during and after test periods, and we are assured by Ellison that from his point of view as an electronics engineer the effects observed do not resemble what he would expect to see if there had been a loose contact somewhere. Moreover, the seizure-like character and its abrupt cessation at the end of chart 4 would also seem to argue against such an interpretation. Quiescence of trace was only restored after switching back to the original power supply.

In addition to possible electrical faults, a possible normal cause to be considered is carbon dioxide. (It can hardly be claimed that discharging a fire extinguisher, without warning, into the beam constitutes some adequate test; obviously if the IR was working, and if the extinguisher contained CO₂, it was bound to work.) Subsequent attempts at deeply breathing into the beam, taking care to avoid misting over the lens, produced no noticeable deflections; it therefore seems exceedingly unlikely that CO₂ from participants' breathing affected the IR. CO₂ is also an unlikely explanation of the irregularities found for a number of other reasons, principally the stability of the trace before, and especially after bouts of irregularity, the failure of others simulating Matthew's activities to reproduce his traces, the quiescence of the trace when all participants were in the lab and active, and occasions while students were in the lab all day, as well as during the entire February experiments.

Mist on the bulb resulting from direct breathing on it must be ruled out: not only is the trace quite different in shape from what we observed, but for one thing, frequently no one was near the apparatus when irregularities happened, and attempts by others to simulate Matthew's movements during periods of trace quiescence produced nothing whatever.

Table 6.7 summarises possible normal causes of malfunction other than complex experimenter fraud. Perhaps the best defence against this undisprovable supposition might be that, had we gone to this enormous amount of trouble, we might be credited with producing something rather more impressive!

Paranormal hypotheses

Although as will be plain, no categorical claim to paranormal activity can be made, a sufficiently good *prima facie* case has been established to make it worth while discussing the hypothesis of paranormality. To this day Ellison and Roger Chapman express the view that they have not seen apparatus behaving like this before and can find no normal engineering explanation. David Chapman is more guarded. While, like the rest of us he would have preferred more time with the equipment beforehand, he has given no alternative explanation other than possibly a loosening of the bulb, which we cannot accept as plausible.

On the supposition that what is recorded does not reflect one of the artefacts mentioned or some other not envisaged, the question still arises whether it was the infra-red beam, the electronics of the apparatus, the power supply, or the monitoring instruments that were affected, normally or paranormally.

The only one of these that can be, if not ruled out, at least rendered extremely unlikely, is that it was one or perhaps all of the monitoring instruments that were affected since all were wired in parallel and,

Possible normal causes of disturbance suggested	Considerations against treating factors in left hand column as normal explanations
Mains fluctuations	Stabilised mains; persistence of irregularity after change to battery; days without disturbance before, during and since; escalation of disturbances.
CO ₂ on infra-red	Days without disturbance with numerous persons present before, during and since; escalation and total cessation of disturbances; dissimilarity of trace; onset and incidence when no participant near IR. Failure to replicate by breathing without misting.
Contact faults in circuit before infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance.
Contact faults in circuit after infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance; unilateral direction of disturbance as per DVM readings.
Infra-red bulb	No evidence that this was loose at the time; different shape of trace; days without disturbance before, during and since.
Mechanical intrusion into infra-red beam	Carefully watched and monitored, including video; totally different trace, for most such intrusions.
Mechanical vibrations	Carefully watched and monitored; totally different trace.
Faulty chart recorder	Correspondence with other monitors; stability before, during and since; only suggestion of malfunction if timing taken into account.
Faulty DVM	Stability before, during and since; correspondence with other monitors; could have no causal efficacy on chart record.

TABLE 6.7
Considerations against treating various normal causes as explanations of disturbances observed.

so far as may be ascertained, all registered similar irregular deflections. The voltmeters could not have causally affected the chart recorder. One would have to hypothesise an identical tripartite paranormal force acting simultaneously on all three. Nothing in the proceedings suggests such fine and planned control.

Although on the face of it occultations of the infra-red were obtained and the infra-red apparatus responded appropriately whenever tested, it cannot, in the absence of monitors and switching arrangements in different parts of the circuit, be claimed with complete certainty that it was in fact some interference with the beam that was being measured.

It was thought at first that action on the infra-red beam was virtually ruled out by the fact that Matthew had apparently been able on at least one occasion, and on demand, to *raise* the voltage above the 1000mV limit. It will be recalled that, since this trial could not be reflected on the chart, participants dictated the DVM readings into the tape recorder. However, whereas the noise level of the instrument was $\text{ca } \pm 20\text{mV}$ the readings as recorded never exceeded 1032. Since this constitutes less than 10% of characteristic disturbances in the opposite direction (i.e. lowering of the voltage) obtained during periods of disturbance (see columns of minima in Tables 6.4, 6.5 and 6.6), this small upward fluctuation cannot be regarded as significant.

It should also be stressed that the DVM is not on record on a single occasion as having fluctuated spontaneously in an upward direction to any substantial extent (i.e. one approaching the values corresponding to downward irregularities) such as 1300 or 1500mV. This would, without any doubt, have been spotted by participants. The unilaterally downwards direction of significant irregularity tends to suggest interference with the infra-red. A fault in the DVM could not have affected the analogue voltmeter, which similarly was never observed to exceed 1000.

Normal electrical faults in the circuit *after* the source would have been expected to fluctuate equally in both directions; this should have shown up in DVM readings, which it did not. Hence on a paranormal interpretation, if the *modus operandi* was not on the IR itself, it would have to have been somehow psychologically guided in a downward direction in accordance with Matthew's subjective expectations and those of most participants, especially Gregory's. It would have conflicted with those of Ellison, who was fairly firmly convinced that some paranormal electrical effect rather than any interference with the infra-red was involved.

Thus, if the effects obtained were paranormal (and it must be remembered that 'paranormal' is a residual category), three possible *loci* of paranormal influence might be isolated: power supply, electronic circuit or infra-red beam. It is of course not necessary to suppose that interference between source and cell would necessarily be due to an

'interloping substance' as hypothesised by Osty. As stated at the outset of this paper, the term occultation was used neutrally simply to indicate that ostensibly less light was recorded as falling on the cell than 1000mV. Mr. Roger Chapman has pointed out that, for example, the beam might also have been deflected away from the detector. None of these rival interpretations can at this stage be conclusively excluded. However, the downwards direction favours interference with the infra-red. For the time being the hypothesis of some paranormal intervention between source and cell, suggested by Osty in the case of Rudi, is also available here. However, in the present case, the hypothesis is less strongly supported because, unlike Osty, we did not have available multiple beams which the Subject affected on demand, and on the face of it Matthew at no time achieved anything like the control demonstrated by 'Olga', Rudi's secondary personality.

On the other hand, it must be conceded that in Osty, Hope and Rayleigh's day an audio record of the completeness of ours was simply not technically available; therefore, any 'bandwagon effect' would almost certainly have escaped detection. It seems important to consider the paranormal hypothesis that the psychic (Rudi) was operating on slight fluctuations in noise levels of the instrument, which would reach him and others by normal sensory routes such as the slightly louder scratching of the pen of a recorder. On the other hand, the gentle upward trace at onset of irregularities observed with Matthew is missing in Rudi's case (Figure 8), which would seem to counterindicate 'bandwagoning'. There can also be no doubt of the numerous experimenter initiated trials in Rudi's case, particularly with Osty. It is of course quite possible that the same phenomenon is simply not captured in the in the two cases.

Some psychological comments

At no time was Matthew in trance, nor did he display any altered state of consciousness beyond that which anybody displays who is concentrating on a task. This is in sharp contrast with Rudi, who was always in trance when phenomena were reported. It may or may not be psychologically significant that Matthew increasingly displayed aggressivity towards the instrument. It might well therefore be that, on a paranormal interpretation, the irregularities could be described as due to some temporary 'haunting' of the apparatus or power supply, rather than to the extrusion of some sort of psychic matter. On the other hand, the systematic downward nature of the voltage tends to support this latter possibility.

Taken altogether, the 'deviant charts', charts 1, 2 and 4 suggest activity periodic at first and then more and more disorganised and extensive. The overall impression is that of having created and captured a limited

poltergeist outbreak in a laboratory. (It will be remembered that Matthew's early poltergeist phenomena started as limited sporadic episodes and ended as a chaotic shambles.)

Matthew frequently or even usually thought or felt he had control over the disturbances but the record does not support this, quite apart from the 'bandwagoning' effect. Occasionally he tried and obtained nothing; he could not stop irregularities once they were under way; and he predicted irregularity when this did not occur.

On a paranormal interpretation it looks more as though in some way he (or the group) created instability in the system under investigation, at whatever level, and when this instability as fed back by the DVM reached a certain level, Matthew either enhanced this, or 'jumped on the bandwagon' as the irregularity increased in any case. Voluntary conscious action must be ruled out: perhaps some autonomic analogue reaching a crescendo over a period of days, or some isolated fluctuations or firings eventually coalescing into a quasi-seizure would be better models.

It is by no means wholly clear exactly who, on a paranormal interpretation, was responsible for the effects. Certainly all participants and Matthew thought of him as responsible for irregularities and indeed if he in some way introduced noise into the physical system which escalated over a period and then vanished, this would be an appropriate supposition. However, neither Matthew himself nor any other participant had to be present in the laboratory during irregularities: the presence of neither Barrington, Brown, Ellison, Gregory, Inglis nor R. West was essential. David Chapman and Sullivan were in the laboratory most of the time in any case. Chapman was the firmest 'goat' in the team, and Sullivan was only marginally involved and was often busy with other matters. Malfunction continued at peak intensity in the presence of J.B. who was not only a total skeptic but openly contemptuous and hostile. The 'Grattan-Guinness peak' presents another enigma, occurring as it does in Matthew's absence and while all those present are quietly otherwise occupied. On a paranormal interpretation either Matthew exerted PK on the instrument which became partially independent of him, or else the effect was a group phenomenon, with Matthew as psychological focus of the group.

It seems fitting to conclude this discussion of findings by referring to the dual aspect that we as experimenters are likely to present to our Subjects at the actual time of experimenting: we appear to be and indeed are enthusiastic, encouraging, positive, extravert and generally 'sheepish'; when - and if - we finally publish we are critical, skeptical, dispassionate, doubting goats. From the point of view of the Subject this may simply look like bad faith; yet we know it is not. This unavoidable duality needs to be faced much more explicitly in parapsych-

ological experimentation, and allowance be made in briefing and debriefing of subjects.

Concluding observations

The present investigation, although exploratory in nature, was nevertheless a fairly ambitious and time-consuming enterprise, and was moreover, explicitly a mixture between a seance and an experimental set-up. The standard antiseptic type of write-up is therefore not appropriate, and readers might feel that it reads more like a sad chronicle of errors and deficiencies. However, we believe that only by making these shortcomings explicit is it possible to assess the evidence one way or another and, more importantly, to design future experiments in a field which has remained controversial, despite a hundred years of dedicated experimentation. However, we are of the opinion that the *prima facie* evidence is extremely good and that in an established field resources, human and financial, would automatically be forthcoming to clarify the questions raised. It can be seen that virtually all the ambiguities we have listed could easily be remedied by an injection of resources relatively small in comparison with the huge sums habitually lavished on scientific projects.

FUTURE EXPERIMENTATION

It seems possible in the light of the Manning investigation to suggest how to set about designing experiments concerning the nature of these ostensible infra-red phenomena. In order to obtain adequate normal control baselines it is in our view essential to instal, test and run equipment for substantial periods of time before experiments are conducted at all. It is often claimed, especially in the case of spontaneous phenomena, that apparatus is liable to malfunction in the presence of psychics; as D. Chapman observed, seeing that instruments are usually (under such circumstances) used for the first time under strange conditions, it would be a miracle if they did *not* malfunction. This is of course a problem for all investigation of spontaneous cases and investigating physical phenomena in the laboratory almost unavoidably presents some of the difficulties as those encountered in spontaneous cases. At any rate, the instrumental problems can be overcome by an insistence that satisfactory apparatus must be installed and working prior to the introduction of subjects, and continuous records kept of normal functioning.

Automatic monitors and control switches should be introduced at each stage of the circuit, so that it is possible to ascertain at which point in the circuit the paranormal influence, if any, is exerted. This would make it relatively easy to establish whether the phenomena is

one involving power supply, electronic circuitry or the infra-red beam itself. Monitoring devices should be digital and automatically recorded on multi-track audio tape for easy computer analysis, and if desired computer graphic reproduction and print-out.

Ideally from a methodological point of view, one should of course have a protocol incorporating pre-determined random trial and control periods; it might be possible by suitable psychological devices such as incorporating periods of activity and non-activity in a game to make a psychic Subject willing and/or able to comply with such experimental conditions. However, we are by no means certain that this is psychologically at all promising, especially if the 'bandwagon effect' plays a substantial role, or if the psychic creates some sort of noise in a physical system partially independent of him/herself. If, on the paranormal hypothesis, the Subject somehow either capitalises on normal fluctuations or initiates minor paranormal fluctuations, when he is in some (unknown) state fit to do so, the Subject is far more likely going to feel the need to initiate periods of activity him or herself. For all we know the Subject may have subtly to combine some inner physiological or psychological fluctuations with some instrumental fluctuation in order so to capitalise in a paranormal manner. An alternative to trial and control periods would be the provision of independent, duplicate apparatus, such that one is the experimental, the other the control apparatus. If an effect is captured on one of the instruments it would then be possible not only to establish paranormality beyond reasonable doubt by suitable manipulation, but to begin on the investigation of some of the basic psychological variables which has barely started.

Once one has installed satisfactory duplicate equipment on a relatively permanent basis, attention can be devoted to the selection and preparation of suitable subjects. One possible group is that of former poltergeist children or alleged physical mediums; another would be children or others who had given indications of being 'metal benders'. It might also be useful to attempt to test individuals who felt themselves to be notably destructive, unsuccessful or clumsy with machinery. Ideally of course one would attempt to train individuals as PK subjects, although for the time being this remains a somewhat futuristic project. Intermediate between the naturally occurring special Subject and the trained one might be the joint efforts of a sitter group. An imaginary 'Philip' on the lines of Owen (1976) might well influence apparatus installed on the lines that we have advocated; we are convinced however that, for the physical effectiveness of such an artefactual person to carry scientific conviction, it would have to influence stable, reliable equipment.

During all attempts at experimentation, conversation should be monitored by means of audio recording, if only to provide clues for the interpretation of unexpected effects, and video apparatus of adequate

quality is desirable for security reasons. Audio, video and other monitoring channels should all be linked to a common time channel so that events can subsequently be synchronised automatically with one another and with independent clock time.

Over a hundred years of experimenting with the physical phenomena suggests that to *demonstrate* them *ad hoc* to the satisfaction of many observers, even competent and critical ones, is not too difficult, but that to *capture* them in a permanently satisfactory manner for those not personally involved as participants, observers or experimenters is quite another matter, and has not so far been achieved. We believe that the instrumental and technical means for such capturing of these phenomena have only fairly recently become available, and that we have suggested the conditions necessary for such capture. In any other context such requirements would appear essential, indeed minimal. If they were met the nature of some of the most puzzling and important phenomena of psychical research could at long last be elucidated.

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The illustrations (Figure 3-7) in this paper are artist's copies of the original charts; the original background being too faint to reproduce photographically. The graph paper used in the background of the copies is slightly different in that the fine lines are at millimeter square intervals both horizontally and vertically whereas on the original chart paper the vertical lines occur at 2mm intervals and the horizontal lines at 1mm intervals. The illustrations are also reduced so as to fit on to the pages of the *Proceedings*. Legends and descriptions in the paper refer to the original charts, photocopies of which are deposited in the SPR.

EXPERIMENTS ON POSSIBLE PSYCHIC EFFECTS
ON THE GROWTH RATE OF MOULDS

J.B. HASTED

The mould *Mucor hiemalis* was chosen for growth rate experiments because of its many convenient features (Ingold (1973)). Mutant strains occur only very infrequently. A typical strain was kindly provided by Dr Brian Plunkett of Birkbeck College, and was checked for purity both during and after the twenty week period of experimentation. It was grown on nutrient jelly in 8.5 cm diameter Petri dishes, each of which was inoculated in the centre by placing on it a 7 mm diameter disc of *Mucor* and jelly aseptically cut by means of a cork-borer and transferred from a single previous specimen. The *Mucor* specimens were allowed to grow in the laboratory for about 24 hours, when two orthogonal diameters d'_1 and d''_1 were measured. Subsequent measurements d'_2 , d''_2 and d'_3 , d''_3 were made at approximately 24 hour intervals, so that the growth rate measurements extended over 48 hours; the period was not extended further because of the proximity of the outer wall of the Petri dish. All experiments were conducted double blind, the measurement of the growth rate being made by myself without knowledge of whether the mould had been exposed or not.

Batches of eight were made up and the dishes kept under a glass dome, or during transport in a polythene bag. Four, randomly selected, by blind choice of a technician, were kept as controls and the other four exposed to the subject, Matthew Manning, for a period of approximately five minutes. The temperature difference between the control and exposure laboratories was always within $\pm 2^\circ$. The instructions given to the subject were simply to 'interact' with the moulds, without any preconception as to whether accelerated or retarded growth rate could be expected. He was permitted to touch the Petri dishes, but not to remove the lids. During this period the control *mucors* were kept in another room in the same building, its location being unknown to the subject; in the first runs even the existence of controls was unknown to the subject. Immediately after exposure, the moulds were placed with the control moulds and returned to their original numerical order. The five minute separation, involving a small temperature change, would not affect the growth appreciably.

Eight separate experiments, numbers M1 to M8, were carried out. A further series of ten separate experiments, letters MA to MK, were carried out after an interval of three months. It was usual to conduct either two or three experiments in a day, and at least one week separated

Experiment	Δ_{21}	Δ_{21}^c	δ_{21}	Δ_{32}	Δ_{32}^c	δ_{32}	δ_{31}
c=A to J							
A	2.57	2.59	-0.02	-	-	-	
B	2.08	2.16	-0.08	2.31	2.30	+0.01	-0.07
C	1.98	1.94	+0.04	2.39	2.38	+0.01	+0.05
D	1.99	1.99	0.00	2.39	2.38	+0.01	+0.01
E	2.24	2.18	+0.06	2.43	2.46	-0.03	+0.03
F	2.26	2.19	+0.07	2.44	2.44	0.00	+0.07
G	2.24	2.25	-0.01	2.43	2.43	0.00	-0.01
H	2.46	2.42	+0.04	2.53	2.59	-0.06	-0.02
J	2.44	2.41	+0.03	2.54	2.56	-0.02	+0.01
σ_c			0.044			0.024	0.041
m=I to 8							
M1	2.20	2.17	+0.03	1.89	2.48	-0.59	-0.56
M2	2.10	2.10	0.00	2.14	2.15	-0.01	-0.01
M3	2.16	2.10	+0.06	2.12	2.19	-0.07	-0.01
M4	2.10	2.10	0.00	1.99	2.04	-0.05	-0.05
M5	2.11	2.08	+0.03	2.07	2.02	+0.05	+0.08
M6	2.28	2.29	-0.01	2.04	2.04	0.00	-0.01
M7	2.38	2.39	-0.01	2.06	2.07	-0.01	-0.02
M8	2.32	2.36	-0.04	2.08	2.09	-0.01	-0.05
σ_m			0.029			0.193	0.186
n=A to K							
MA	1.94	1.93	+0.01	2.40	2.42	-0.02	-0.01
MB	1.88	1.90	-0.02	2.45	2.44	+0.01	-0.01
MC	2.22	2.26	-0.04	1.86	1.82	+0.04	0.00
MD	2.20	2.18	+0.02	1.88	1.92	-0.04	-0.02
ME	2.18	2.15	+0.03	2.35	2.32	+0.03	+0.06
MF	2.19	2.18	+0.01	2.28	2.27	+0.01	+0.02
MG	2.08	2.21	-0.13	1.81	1.79	+0.02	-0.11
MH	2.15	2.13	+0.02	1.78	1.84	-0.06	-0.04
MJ	1.93	1.92	+0.01	1.81	1.83	-0.02	-0.01
MK	1.88	1.91	-0.03	1.81	1.86	-0.05	-0.08
σ_n			0.045			0.033	0.046
σ_{m+n}			0.040			0.137	0.132

TABLE 7.1

Twenty-four hour growths Δ differences δ and standard deviations σ .
All measurements in cm.

successive exposure days. Between the two series, a complete set of nine 'control blank exposures', denoted A-J, were performed with identical protocol except that the subject was absent from the College and was not informed of the proceedings. The measured growths in this case represent the normal difference between growth rates of similar batches in the absence of exposure to the subject. These differences include both biological variations and errors of measurement.

In each series, arithmetical means $\bar{d}_i = \frac{1}{4} \sum_1^4 d_i$, where $d_i = \frac{1}{2}(d'_i + d''_i)$ of the four pairs of diameters (in cm.) of exposed *mucors* measured after twenty-four hour period i were compared with the arithmetical means of the four pairs of diameters of control *mucors* also measured after twenty-four hour period j (i and j are running integers). The difference $\Delta_{ij} = \bar{d}_i - \bar{d}_j$ represent growths for the appropriate twenty-four hours.

The average growth of the exposed batch between days i and j is then $\Delta_{ij} = \bar{d}_i - \bar{d}_j$ and the average growth for the control *mucors* is $\Delta_{ij}^c = \bar{d}_j^c - \bar{d}_i^c$. There is only one exposure to the subject, made at the end of twenty-four hour period i . The growth differential between exposed and control batches between days i and j is then $\delta_{ij} = \Delta_{ij} - \Delta_{ij}^c$.

All Δ and δ are tabulated in Table 7.1. The scatter in Δ values is not considered to be significant, because there were differences in temperature, light and humidity from trial to trial, and also some small variations in the twenty-four hour periods between measurements, due to pressure of administrative duties on the experimenter. But these should not affect the differences between exposures and control batches, since individual diameters were measured consecutively, controls and exposures together, the whole operation taking less than five minutes.

An inspection of the values of δ_{21} and δ_{32} readily shows that the consistency of the growth rates in the absence of exposure is good; most of the exposed specimens also grow at consistent rates, showing small values of δ_{21} and δ_{32} . But in the cases M1, and possibly MG, exposure has seriously affected the growth rates.

For interest, the differences between the exposed standard deviations such as σ_m , and the control standard deviation σ_c were analysed using Fisher's 2-tailed F-test. The parameters

$$F = \left(\frac{\sigma_{m,n}}{\sigma_c} \right)^2 \left(\frac{1-1/N_c}{1-1/N_{m,n}} \right) \text{ are given in Table 7.2.}$$

Experiment	Periods		
	21	32	31
m	0.44 n.s	68.7 $P \leq 1.3 \times 10^{-5}$	21.1 $P \leq 6.7 \times 10^{-4}$
n	1.02 n.s	2.25 n.s	1.26 n.s
m + n	0.77 n.s	31.9 $P \leq 5.3 \times 10^{-5}$	9.79 $P \leq 0.0028$

TABLE 7.2

On this basis we might claim that the entire experiment was significant at $P < 0.003$, and that various parts of it were of greater significance.

However, this significance clearly rests upon the single experiment M1, actually the first to be conducted, although the experimenter had already many hours of routine experience with identical experiments using other subjects. The question therefore arises, could some error have been made with this batch? As soon as the double-blind was broken, and it became clear that there was a significant effect, possible sources of error were searched for and not found. A later search revealed nothing, although an arithmetical error was actually found in a subsequent batch previously believed to show significance.

The exposure seems to have affected three of the four *mucors* in the batch M1 (and in the case of batch MG, where the significance is less, all four of the *mucors* in the batch). Moreover, the growths have been radially symmetrical; the standard deviation for randomly chosen orthogonal measurements on 8 *mucors* in a batch was 0.08 cm, and the accuracy of measurement by ruler, (from underneath the transparent dish, so as to avoid parallax) was deemed to be ± 0.05 cm. However, it is possibly significant that there is for the M1 case a conspicuously large growth increment Δ_{32}^c for the controls, as well as a small decrement Δ_{32} for the exposures.

We may claim that whilst a consistent effect on growth rate has not been produced by the subject, there has been one exceptional exposure after which an extremely unlikely retardation of growth rate occurred. We are unable to fault the experimentation for this exposure, but are of course aware that it would be unwise to claim the capture of an effect on the basis of a single anomalous batch.

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8

THE SUBJECT'S REPORT

MATTHEW MANNING

It was against a background of unfounded claims by detractors that I asked for a series of tests to be organised by the Society for Psychical Research; and, I wanted to produce further evidence that would defeat my critics.¹

I was interested also in replicating some of the successful tests in which I had been involved with Dr. William Braud in San Antonio, Texas (Braud (1979)). These tests in particular had the added attraction of obviating the need for a magician to be present as none of them could be reproduced by simple conjuring tricks. Even so, I wanted to have a conjuror present either watching the experiments, or approving their controls, and I made this clear to Anita Gregory when the plans for the tests were first being laid. Although at first my idea seemed attractive, it was rejected. Anita wrote (1978):

I have had second thoughts about a magician. I can quite see why you should feel like that; it is wounding to be attacked and goaded the way you no doubt have been and unfortunately are likely to be. However,

(a) A magician of your choice would never silence your critics: look what happened over Uri Geller when an eminent magician *did* vouch for him! The magician would simply be discredited by those who didn't like his conclusions.

(b) A magician likely to be hostile at all costs would spoil the sessions and disrupt the very atmosphere we are all trying to create.

(c) A magician's word *as such* is not necessarily more acceptable to, say, academic people in any case. After all, his business is to entertain and to sell himself and to make fools of people in an amusing way. He is as John Taylor has so often insisted, 'a professional deceiver';

(d) The kind of experiments we are planning are not, I would hope, such as could be at all easily counterfeited by a magician; so it could be totally useless!

If, as my detractors would have the public believe, I am helpless in a

¹ Matthew's somewhat edited contribution was written in the summer of 1979, a few months after the final February 1979 experiments.

controlled experiment, it is odd that I should even volunteer to participate in tests with researchers. I have a quiet confidence, some might say arrogance, about my performance under controlled conditions. This confidence is based firmly upon the knowledge that whenever I have engaged in tests there have always been some positive results, even if every test is not successful. I therefore start each of my series of tests with a psychological advantage because I have produced phenomena under controlled conditions in Canada, the United States, Germany, Sweden, Holland, and other countries.

There are, I have learned, disadvantages in working with researchers from different countries. The greatest drawback is that there is limited value to the work that can be conducted in a relatively short period of time if I am working abroad. Short periods invariably provide some initial insight into those experiments which suggest promising results but there is never time to do the necessary follow-up work, either because time or funds run out. After three or four weeks' work one is left with statistics and evidence that ESP or PK phenomena have occurred but nobody is any closer to understanding *how* or *why* it works. I have now decided to cease 'laboratory-hopping' because I have concluded that it is unproductive to both myself and the researchers. Thus I will now work only with organisations who are in sympathy with my desire to conduct experiments that have some useful application and benefit to humanity, where possible. Thus many tests have interesting implications for healing and medicine.

Another sad aspect of laboratory-hopping is that so few researchers seem to share their findings with colleagues elsewhere, except in the form of published reports. Often, I feel, that which is not published or written about is as interesting, if not more so, than the subject matter of the final report. I know it is difficult to get papers published but I find it deplorable that certain so-called scientists can engage a subject in a series of tests and then fail to write anything at all about them. This is another reason for my decision to work only with a few organisations, as so often in the past I feel I have been used by researchers who have not even the courtesy to explain to me why they decided not to write up a series of experiments. This selfish attitude benefits neither the subject nor psychical research as a whole.

I feel also that it is of great importance that a good psychological 'gestalt' exists between the subject and the researchers before any long-term study is undertaken. Of course, it is more difficult to establish a feeling of rapport with scientists during a short period of testing. I am sure that the feeling of rapport that developed during the S.P.R. investigation contributed to the success of the work. I never felt as if I was being used or manipulated, and neither did I feel that I was there to prove myself. This was a pleasant contrast to experiences that I have

had in the past with some researchers who perhaps lacked the experience that the S.P.R. team possessed.

Only once did I feel any sense of friction between myself and the researchers, although there may have been one or two occasions when I felt frustrated at the speed at which the tests were progressing. One such instance is reported by Mary Rose Barrington in connection with my first occultation of the infra-red beam. However, that was boredom rather than friction. Also, during the first session of the haemolysis test with William Byers Brown, I felt that he was concerned that he was having a 'negative experimenter effect' on me because the results were not as good as either of us would have liked. In trying to camouflage a non-existent apprehension he was actually succeeding in producing the atmosphere from which he was trying to escape!

I also feel that far from being suppressed, psychic ability should be developed. So often one hears of people, especially children, who hear voices or see non-existent figures, who pick up the thoughts of others or have premonitions of forthcoming events, and who are unaware that they are having a psychic experience. Instead they are scolded for telling lies, or laughed at for being stupid. If those people understood what was happening to them I am sure they would feel happier and relieved to know that they are not mad, unbalanced, or imagining things.

Although it is not widely known, virtually all my automatic writing and drawing was produced during a four year period from 1971 to 1975. In retrospect I feel that these were transitory phenomena, leading to other abilities which were perhaps of greater use. I learnt from the writings and drawings first of all how to bring spontaneous poltergeist phenomena under some form of control; later I feel that they taught me how to still myself and my mind, so that I could release all thoughts, apprehensions, fears etc. This was most important, especially now when I am healing. However, I became disinclined to pursue them too much because I found that after engaging in automatic writing or drawing for any period of time up to forty-five minutes I experienced considerable difficulty in producing the normal motor movements enabling me to sign my own name myself, for example. It was as if some circuit in my brain had been switched off so that I was unable to form words on paper by myself without difficulty for several minutes.

During the experiments in August 1978, I learnt a considerable amount about myself and my abilities through the frequent questioning of the researchers. Until then I suppose I had never really thought much about *how* I produced psychokinetic phenomena. It simply had not occurred to me to reflect with detachment about what I was doing. Perhaps I was frightened that by doing so it would inhibit my PK effects. Of course, even now neither I nor anyone else is any wiser about why or how I produce an occultation of an infra-red beam or a

promotion of growth in bean seeds when others are unable to do so. Although I have two distinctly different procedures for producing PK phenomena there are certain principles which apply to both methods:

1. It is essential that I *desire* that an event occur. If the task is something useless I will lose interest in it and thus fail to be mentally engaged by it.
2. I must *believe* that the event can reasonably take place. If I have no belief my mind works against it.
3. I must *expect* the event to occur. This is rather a subtle law because the first two are simple and passive whilst this third one introduces some dynamics. It is possible to both desire an event, and believe that it can occur, but still not EXPECT it to take place.

These three mental attitudes become easier to attain with practice and experience. Because I know that I have in the past been successful in my attempts to influence mentally a variety of physical and biological targets, I always expect that something will happen when I start to desire that an effect occur. Obviously I have few, if any, barriers of belief, and my degree of desire is related to the purpose and potential application of the test. I also think that to a certain degree one has to learn to recapture an essence of child-like simplicity and naivety, to disregard what one has been taught is real or unreal. Everything is real when you think it. Thoughts are energy and energy influences that which is material. You create your own reality. It's really that simple! We lose so much of our innate potential through formal education which strives to expand other areas of the mind.

I use two very different techniques for producing psychokinetic phenomena that, for me, fall into two very different categories. The first technique I use when I'm engaged in what might be considered the more dramatic visible effects, such as the occultation of the infra-red beam or the influence of electrical apparatus generally. I have less control over the phenomena once I have initiated it and usually I find difficulty in then being able to stop it. The phenomena brought about by this first method are by nature more spontaneous, and to my mind, of little practical application. They seem to be closely linked to physical exertion or restlessness and/or psychological irritation or friction.

Historically, many PK phenomena, whether premeditated or spontaneous, have occurred in situations of psychological stress or strain. This is my experience also; there have been many instances of spontaneous PK effects taking place when I have been irritated or stressed. I feel too that I am made up of several contradictory traits, the friction of which occasionally produces or contributes to physical phenomena around me.

When I first succeeded in influencing the infra-red beam equipment at

City University, it began in an almost spontaneous way without any real concentration or effort from me. At the time I was feeling very irritated and bored; I am a fairly kinetic person by nature – I will pace a room or clap my hands together when I feel restless. On this occasion I was stalking around the infra-red equipment, looking at it and wondering just how easy it would be to disturb it. As I remember, I then thought to myself that there was no point in even attempting to influence the apparatus as there was nobody there to take any interest in it. I started clapping my hands together out of frustration. Almost immediately David Chapman, who had been occasionally observing the monitoring devices, called out and asked what I was doing. The pen recorder linked to the infra-red beam equipment was indicating that the beam was being partially occulted.² Of course, my interest was immediately piqued and I started to make a deliberate attempt to consciously upset the apparatus by making exaggerated movements of my hands and arms within the general vicinity of the apparatus.

Whenever such a phenomenon occurs, it generates great excitement. I think that I feed on this energy which is conducive to the development of PK effects. The best PK phenomena that I have produced, either spontaneously or consciously, have been in the presence of sympathetic researchers who are relaxed and jovial. I believe that the researcher contributes to the results of any parapsychological experiment at some level.

Whenever I produce, say, an occultation of the infra-red beam, I imagine that I am involved in a game with the researchers, but a game at which I have the advantages. I know that I am producing a PK effect. I also know from experience just how the researcher will react. He searches for every conceivable, and sometimes inconceivable, logical explanation! The game is to baffle the opponent (researcher), in the full knowledge that I am producing a paranormal effect for which, try as he might, will not be explicable.

Charles Tart and John Palmer, in reporting a series of tests in which I participated at the University of California (Tart and Palmer (1979)) made a very perceptive observation: 'If our hunches about the perversity of Mr. Manning's psi are correct, the experimenter is involved in a game of trying to outwit this perversity, a game at which he is at a distinct disadvantage.'

The best method of confusing and baffling the opposition is to repeatedly ask for a normal explanation of the phenomena. I would, for example, repeatedly ask David Chapman and Arthur Ellison for their explanation of the infra-red beam occultation. Arthur learnt to

²Matthew's recollection here is at fault; he was indeed bored and irritated and restless. The actual event is described in the IR section on page 320, as supported by the audio record. A. Gregory.

ignore me but David always rose to the bait. I felt that he was growing irritated at his inability to find a feasible explanation, as well as my persistent questioning. Later he admitted this was so.

During the later sessions when I consciously occulted the infra-red beam, I would stand in the vicinity of the apparatus and clap my hands together. Sometimes I would merely clench my fists and tense my muscles, before jumping up and down, making large circular movements with my arms. By the end of the first week of tests my muscles were so strained from this activity that I had difficulty even scratching my back! I had no conscious thoughts in particular during these exertions but merely a great determination that I was to be successful. Interestingly, the greater my distance from the equipment, the greater my exertions had to become in order to generate the phenomena. I do not feel that my physical movements alone were responsible for what happened, especially as others imitating my movements were quite unable to produce the same effect. I have noted in the past that PK phenomena often occur whilst I am in a state between intense concentration and distraction. Perhaps the physical activity provided the necessary degree of distraction whilst mentally I concentrated and focused my attention on the infra-red equipment. I think an interesting study could be made to correlate physical fitness with PK phenomena.

This technique of producing PK effects will not work when used for tests which I consider have a positive application, for example the promotion of seed growth, the influence on cancer cells or blood. For such 'positive' phenomena I use a second method, which will not disturb electrical apparatus or generate the 'negative' effects. This, incidentally, is one of the reasons I feel that the results I have achieved in influencing biological systems are not the result of my disturbing through PK the electrical monitoring equipment, as some have claimed, but an actual effect directly upon the biological target.

Relaxing with my eyes closed, I calm myself, and expel all thoughts from my conscious mind until there is a state of blankness. Sometimes the image of a still mill pool is very effective in this respect. I then imagine that I am slowly expanding my consciousness through 360 degrees, rather like the ripples made by a pebble tossed into the mill pond water. These waves of consciousness float away from me, through the walls of the room, and out all around me. Then I imagine that I start to follow a stream of water flowing from a spring bubbling from a hill. It becomes a river and I flow with the water which eventually opens out into a wide estuary. I flow out to the sea, continually expanding until I see nothing but sky and water. I become a part of the water, a part of the sky. On the horizon, climbing out of the sea is a vast snow-capped mountain, reaching up to touch the sky. I ascend, very smoothly and gently, the mountain. Both the mountain and sea

give me a sense of peace and strength. I am reminded that I am a part of Nature. At the summit of the mountain I reach up to the sky. I leap. Slowly I am drawn out into space and I watch as the Earth sails away from me. After a few moments it hangs like a beautiful blue ball in the black space. I flow. I am in harmony. I reach a point where I feel that I am a part of everything around me. I am a part of the Universe. The Universe is a part of me. There are no barriers in this state. I am. At that point I feel as if I am a channel. I feel energy flowing through me; physically I sense warmth and tingling in my hands and forearms. It seems that this visualisation, which is very powerful for me, is somehow a symbolic expression of the start and ultimate living solution.

When I reach this point which I call 'oneness', I visualise a brilliant light flowing through me to permeate that which I am healing or influencing. I visualise the cells in the test tube or flask surrounded with this light. Mentally I talk to the cells. If they are blood cells being stressed I give them assurance that the light and energy will protect them; if they are cancer cells I explain to them that their useful purpose on this level of reality has ended and that they must progress. I never wish that anything should die, not even cancer cells. I always use positive thinking. I have a belief, derived partly from my psychic experiences and which some will call a superstition, that everything I think comes back to me ultimately. I am sure this is a universal law. Again, you create your own reality. Everything you think, everything you do, somehow, some time, comes back to you.

I am sure that many of the researchers with whom I have worked are unaware of this principle. Recently I listed every *different* test in which I had participated in a two year period between 1977 and 1979. There were a total of 32 experiments, of which 17 could be judged successful. However, that was not the most interesting fact. I was interested to note that there were certain researchers with whom I consistently produced successful results; there were a number of researchers with whom I consistently failed to produce anything at all. Sometimes I would successfully participate in a test with a particular scientist. Later, during an attempt at replication by another researcher, success eluded me. Perhaps the most interesting category of experimenters were those who engaged me in a test with a specific task. Although the desired result did not occur, another PK effect did occur, often on the other side of the room. Are those researchers unconsciously frightened of witnessing a PK event, or of being involved in a successful test, but insufficiently strong to prevent *some* effect occurring? It is evident that the researcher plays a vital role in the ultimate result of the experiment. He is a part of it just as much as the subject, which is obviously why hostile researchers and scientists fail to produce results. It is time for the scientist to look at himself and to break down the barriers created

by his role. Of course, this has devastating implications. At present I can be watched, observed, viewed, probed at a distance, by a detached and uninvolved scientist. He is able to make an objective analysis. This objectivity must necessarily be destroyed if *he* is also part of the experiment. He must analyse himself and there is the quandary. It is no longer a simple exercise of studying someone else, it is a subjective experience.

Coincidences have always intrigued and, latterly, amused me. The occasions upon which electrical apparatus designed to test me has malfunctioned are too numerous to recall. It is almost as if there is a cosmic joker whose sole job it is to incapacitate researchers' machinery. I can hear him saying in my more lucid moments, 'Why do you use all these gadgets? What use are they? You will never measure a non-physical energy with physical apparatus. Look, I can break it just like this. You don't need it!' And as his laughter dies away I am reminded of the words of Lyall Watson (1976): 'Looking for physical explanations of the mind is like attacking a piano with a sledgehammer to get at the concerto imprisoned inside. It is a lunatic endeavour.' Sadly, I suspect this is true. We know no more now about how these phenomena occur, we know no more about their mechanics, than we did one hundred years ago – in spite of our beautiful machines. Something is happening, but is there any benefit in collecting piles of statistics and statements from witnesses? I think the key lies not in studying a few gifted subjects, but in studying the scientists too. Is it so important to find out how and why? I am more interested in using what I have to benefit humanity in a practical way. If I can reach more people through my work with scientists I shall continue that work. I believe that many of the results of tests in which I have participated have a message. At some level we are all part of one another, linked through our unconscious minds. We are all part of every living organism, no matter how small. We are cogs in a cosmic system. If I can make people aware of these interconnections, of the fact that we have our roots in Nature, I believe there would be a greater respect for one another and the environment.

And so I would like to thank all those who contributed or helped in the tests. Each person plays his part, whether he is my critic or my supporter. Without my detractors these experiments would not, I think, have been conducted. I believe that eventually the explanations offered by my detractors will become more implausible than a paranormal explanation.

For those who understand, no explanation is necessary.

For those who do not understand, no explanation will suffice.

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9

COMMENTS

BRIAN INGLIS

In his 'Introduction to the Earl of Dunraven's record of experiences with D.D. Home', published in the *Proceedings* of the S.P.R. in 1924, Sir Oliver Lodge surveyed the problems which confront psychical researchers who are investigating physical phenomena, with a view to ascertaining their laws. 'For laws, of course, they must have', he argued; 'though inasmuch as the activity of live things is involved, those laws are bound to be complicated by the general difficulties inseparable from the activities of life'. It ought to be assumed, he felt,

that all well-ascertained laws are rigorously obeyed, whether by animate or inanimate nature. But it is a familiar fact that live things introduce a supplementary element, a guiding or controlling element, an infusion of will and or intelligence, something not suspending but supplementing the ordinary processes of nature.

It appeared to me, when the idea of trials with Matthew Manning was first mooted, that they would serve two purposes. They would show what, if anything, he could do in his capacity as a psychic. But they would also provide experience in the related field of learning how to allow for, and if possible to harness, the supplementary 'guiding or controlling element'. Assuming that what a psychic can do is influenced by a variety of subjective considerations, how can tests be so organised as to bring the best out of the psychic, in the sense of enabling him to demonstrate his capabilities in ways which will carry conviction to the investigators and later, to anybody who reads their report of the inquiry?

I have to admit that in my capacity as occasional onlooker at the City University laboratory, I was chiefly concerned with the problems presented by Manning's personality and attitude. He has no reason to respect the S.P.R. His recollection of the way he was treated by some

members of the S.P.R. who came to investigate the poltergeist activity which he describes in *The Link* are bitter; and, even allowing for exaggerations, justifiably so. His experiences with psychical researchers in general, too, have not encouraged him to feel well-disposed to them. Researchers are apt to forget that the psychic is not a human guinea-pig, resigned to being put through a succession of tedious tests. He may be – and in Manning’s case, he is – somebody who has a clear idea of what he can do, and the circumstances in which he can do it. Boredom or irritation may wreck the proceedings.

Alternatively they may conceivably influence them in unexpected ways. Manning’s ‘psi’ output, if the term is permissible, appeared on at least one occasion to be triggered off by the mention of the name of one of the conjurors who make a sleazy living out of doing imitations of psychics, and accusing them of fraud. The implications for research into poltergeist phenomena are obvious. But as a rule, Manning’s results appear to be related, to some extent, to the degree of rapport which is established between him and the investigator(s). And this, it appears to me, represents one of the most serious problems confronting psychical research.

Partly it is simply a matter of investigator and subject ‘getting on’ together; but I doubt whether in Manning’s case this is so much a matter of trust and liking, in the conventional sense, as of some intuitive feeling that here is somebody he can work with. But this inevitably brings up the question how far the psychic and the investigator are involved in a co-production. If they are, of course, it hammers yet another nail into the coffin of ‘replication’, as it would be absurd to expect Manning to demonstrate the same results in tests irrespective of the circumstances in which he is tested.

At this stage, more research is needed into experimenter effect along the lines exemplified by Fisk and West. Orthodox science is also occasionally being confronted with the evidence of experimenter effect: either directly, as in Rosenthal’s 1963 report, or indirectly, as in Neal Miller’s account of the results of his research into control of the autonomic nervous system with biofeedback, which were repeated until they appeared ‘robust’, but suddenly could be repeated no longer. Here, then, may be the way to slit open orthodoxy’s soft underbelly.

Subjective aspects of parapsychology are now attracting increasing attention; but how best to understand them, and perhaps bring them under greater control, remains a task for the future. For the present, one lesson from these tests with Manning is that it is no longer sensible for the Society’s investigators to conduct their task, as some have done earlier, in the frame of mind of an inquisitor. To treat psychic subjects as partners – though I suspect that, by disarming suspicion it could also make deception easier to detect. But the Society’s investigators should be sheep – as in this series they have done their best to be – rather than goats.

10
POSTSCRIPT

ANITA GREGORY

Matthew Manning has recently marketed two cassettes called 'Fighting Back' (1982) which provide some further information about his recent development and thought processes. The recordings are concerned with his approach to self-healing - the 'fighting' is against illness - and purport to teach people how to defeat sickness and improve their quality of life.

At the start of the first tape he says that, as a healer, he has treated hundreds of people from all walks of life and with a wide range of problems 'which, as a rule, orthodox medicine has been unable to help'. Even if he were to practise for 24 hours a day, he continues, he would not be able to treat everyone wanting his help:

Obviously, then, I've had to devise a method for selecting those people with whom I do work. If I have to choose between an elderly person with arthritic joints and a hearing loss, or a young child with cancer, my choice will almost certainly be the same as yours. I think it's fairer to work with a child that has a whole life in front of it than with an older person who is suffering from what one might call fair wear and tear.

The gentle sagacity of this passage betrays perhaps even more clearly than anything that follows the all of 26 years of its author. The effect is slightly modified by the ensuing statement: 'I often tell those that I am working with that what I am doing for them they could just as easily do for themselves'.

Usually, he says, people do not believe him: they expect to have things done to them rather than to do things for themselves. A little later he says he remembers being told on a radio programme 'that it was cruel to suggest to people that they could actually do something for themselves. I realised then the effect that the National Health Service had on many people'.

The element of cruelty suggested by the interviewer, one would have supposed, did not consist in encouraging people to make an effort but in raising false hopes. Matthew Manning of course does not believe his implicit promises to be false, on the contrary he considers that unfaltering faith in the possibility of self-healing is essential to eventual success. At the very end of the second side he explains his rejection of the word 'try': 'I feel that the word has somehow negative connotations in it....

...so long as you say you're going to try something you are allowing yourself the option of not succeeding...'. He concludes: 'Remember with the self-healing programme, that even if you don't succeed what is worse than that is not making the attempt to succeed in the first place'.

A lot of what follows on the cassettes is interesting and potentially perhaps even helpful: relaxation through a few breathing exercises, rather rough and ready examination of one's own motives, a bit of active imagination, auto-suggestion and positive thinking. Some of it, especially delivered as it is under conditions of hypnotic and hypnoidal suggestion, should be queried rather more critically, for instance the suggestion that past mistakes were ever only in one's own mind.

If one listens carefully, there are actually few if any concrete claims being made. Is there anyone today who would disagree about the hazards of 'stress' and the likely beneficial effects of counteracting it? Or who would doubt that a great deal of illness has a psychosomatic component? The question arises, just what is Matthew Manning's own exact contribution to medicine and healing?

If it is really the case that, as he now says, he does nothing more for people than they can do for themselves, he has presumably no special *cachet* as a psychic in promoting health: he has adopted the roles of teacher, counsellor and therapist, and is in competition with countless other practitioners, orthodox and alternative, physiological, psychological and spiritual, all of them purporting to help suffering humanity, and many of whom have years of training, professional expertise, experience and tradition at their disposal. His methods and techniques (if indeed they are specifically his which at least most of them do not seem to me to be) presumably will be subject to the same criteria of efficacy as anyone else's, and to establish their validity and reliability he would have to persuade clinical, psychological and social researchers to conduct relevant tests. This should not in any direct way necessitate his own cooperation over and above formulating what, in his view, people should do to get better.

From the point of view of the psychical researcher, on the other hand, the question arises what, if any, link does there exist between Matthew Manning's ostensible success as an experimental Subject (if that is a legitimate interpretation of some of the results) and his claims as a healer?

Claims to healing themselves need to be sub-divided into possible claims (on the face of it disowned on these cassettes) to direct para-normal influence over people's bodies, and claim to authoritative pronouncements on how to achieve health.

The link that I feel he would probably make is *via* his own mental imagery in the process of exerting influence on physical and biological targets. And yet it cannot be said that the record would support this:

he does have these images, but they do not clearly correlate with the effects associated with him; certainly there cannot be any question of having established a causal link. The problem of the role of the unswerving, contrary-to-fact symbolic imagination in paraspsychology and, for that matter, in self and health management, is far too important to be left suspended between being dogmatically asserted and stonily ignored.

Matthew Manning does not claim any paranormal element in his cures, nor is evidence offered to support any contention that there has ever been any. Nevertheless, it seems to me just about conceivable and worth exploring that if he (or any other healer at least of his style) is therapeutically effective over and above normal suggestion and chance factors, this could be due to a two-stage process: that he could have a disruptive effect on an organism, probing its weaknesses and disabilities, after the manner of his ostensible PK effectiveness on the infra-red and random event generator equipment, and that, by then suggesting soothing as well as likely positive images, the patient is occasionally able to cure himself having been (paranormally?) shaken out of prior pathological adjustment.

The entire subject is one of the greatest possible human interest, and Matthew Manning must be congratulated for struggling so valiantly in so difficult and age-old a battle. The functions of belief, faith, imaging and right thinking in correct living have, so far as we can tell, occupied our race from time immemorial. It is precisely because these issues are of such vital and perennial human importance that, I believe, psychical researchers have always been particularly cautious about endorsing any claims that might conceivably suggest support of future cults and cult persons. So long as researchers are clear about exactly what they have and have not established, and so long as Subjects are willing to go on recorded tape as candidly as Matthew Manning, the danger is at least containable.

REFERENCES

Fighting Back, A Guide to Self-Healing, © and ® 1982, Matthew Manning Cassettes Ltd., Catalogue Number MMC3, music played and composed on side 4 by Robert John Godfrey, © and ® Enid Songs.

ACKNOWLEDGMENTS

We would like to thank Matthew Manning for his participation in these experiments. We are grateful to Mr. I. Bloomfield and the KIB Foundation for contributions towards the expenses of the original investigations, and substantial help towards the printing of this report. Mr Bloomfield also put at our disposal the time of Miss Ruth West during the investigation. We would like to acknowledge gratefully her extensive help with administrative and clerical work, and above all her constant tactful, gentle and constructive presence. Miss Kathy Wilson's tireless help in compiling the final report should also be mentioned. We owe a considerable debt to Mr. David Chapman for his technical assistance and sustained valuable advice and support. His substantial modifications of the original section on the infra-red apparatus have all been incorporated. Thanks are due to Professor Ellison for revision of the final draft of the section on apparatus and Figure 1, and to Mr. Alan Smith for the fair drawing of the sketch of Figure 1, the IR circuit diagram. We would like to express our indebtedness to Mr. K. Fudge in connection with the seed germination experiments, to Dr. I. and Mrs. E. Grattan-Guinness for their part as judges in the poetry experiments, to Miss Marion Smith for taking Matthew's EEG at St. Bartholomew's Hospital, and to Mr. Roger Chapman for advice in connection with the interpretation of results of the infra-red irregularities. However, only named writers are responsible for the views expressed. The list of acknowledgments would be incomplete without the mention of the late Mr. Colin Brookes-Smith, in constructing, at his own expense, the IR apparatus used in these experiments.

PRESIDENTIAL ADDRESS 1980

THE WAY IT LOOKS

LOUISA E. RHINE

A deep rooted tendency of the human mind seems to be that of taking very seriously the world around about, just as it looks in the here and now. That tendency, of course, begins in infancy and, only gradually, with age and experience, does it come in for modification. We slowly and often painfully learn that things are not necessarily what they seem, that other dimensions, not only the obvious ones around us, exist too, and lie back of, nay even contradict, the surface appearance.

The history of parapsychology over the last century bears out this observation. Even though workers in the field today are too sophisticated to claim to know with finality just which of the conclusions to which they come on the basis of their discoveries will stand unaltered into the future, still they often hold such conclusions with fairly adamant certainty. So it has been that at each stage into which one might divide this hundred year period, an impression based on the way the field looked then, however superficially, was, to some extent, crystallized into a prevailing viewpoint, and became a more or less accepted idea of the way it is.

Then, as knowledge, experience, and information increased, changes of outlook, of general impressions, slowly came about. and the way it had looked before was superseded by the way it looked later. My purpose here is to go back over some of those general concepts of the various stages as history recorded them, and try, briefly, to trace in essence the changes that came with increases in knowledge and modified those earlier concepts from the way it had looked then to the way it appeared later. In so doing, I hope to review something of the progress and direction of research in parapsychology.

Since my own work has been concerned mainly with the study of spontaneous psychic or psi experiences, I will draw upon that study for the basis of observation on these changes as I have noted them, beginning before, during, and after my own active research.

Of course, in an undertaking like this I must go back again, as so many have done in the past, to the beginning of the organized study of psychic experiences, 1882, and the founding of this Society. Inevitably, too, one must take note again of the dominant reason for that founding and

recognize that basically, whether nominally or not, it was the question of survival; and it was that, regardless of the way the question was phrased or recognized by different ones of the individuals who became involved in it.

The question whether the spirit survives the death of the body became the basis of a formal study then, because, as we all know, certain human experiences looked as if a deceased person had communicated with a living one, whether through a medium or more directly, as an apparition of the dying or the dead. Such psychic or psi experiences naturally suggested that the deceased survived. Even though immortality had long been assumed by religion it had not necessarily carried with it the idea of actual communication from the deceased and neither had it been subjected to concerted scientific investigation. And so it was that experiences suggested the survival of the spirit were now seen as material for investigation to find out if they indeed were what they appeared to be.

Logically it was very important that such occurrences be studied carefully and the weight of testimony they carried be evaluated. A general project for this evaluation then developed beginning with a widespread collecting of the data. In this it is scarcely necessary to recall the illustrious names that were involved, for they already are very familiar to all true students of the field. These names, of course, are not only familiar to all of us in this present generation; they were true and earnest and competent students in their own fields, as they now became in this new one. They meant to find out, if it was possible, whether the dead survive and whether the signs that they do so, as suggested in certain psychic experiences, are bona fide evidence of it.

It followed, of course, as clearly as that night follows day, that if any of these reported experiences were indeed messages from the deceased then methods of communication were involved that were different from those used by living persons. Those of the living, of course, depend on the senses which directly and indirectly supply us humans with our knowledge of the world around us. But, obviously the senses are not involved in psychic experiences. Some other method must come into play to afford the interchange. The only apparent explanation was that it could be telepathy, a hypothetical method that could operate between minds in which one person's thought could be obtained by another without outward signal. If deceased persons survive, then messages from them could thus be transferred. Hypothetically, this was the way it looked and it gave a logical basis on which psychical research could begin. Fifty years ago, when my husband, J.B. Rhine and I approached the field, something like this was the way it looked.

Then, under Dr. William McDougall, the British psychologist newly at Duke University, J.B. was given the opportunity to study intensively in

the field of psychical research. He began there in 1927 and I watched from the sidelines, mostly domestic, for the next twenty years.

J.B. thought the place to begin research was on telepathy, since it would have to be the medium of communication necessary to support the thesis that the dead can communicate. Does it occur even between living minds? Over the following twenty years research on the topic continued using experimental methods in which results could be statistically evaluated, the method which the developing sciences in other fields were using; methods that hypothetically could answer questions with a no as well as a yes, or a maybe.

By the end of the 1940's, convincing evidence had accumulated within the Parapsychology Laboratory, which came into being in the 1930's, that yes, communication can occur between two living minds when no sensory influences are involved; not only between minds, however, but also between a mind and inanimate elements in the environment. The distinction between these two types depended on the kinds of targets involved: thoughts or things. Later, future events as well were added to the list. And so instead of just telepathy, clairvoyance and precognition had to be included as different types of extrasensory exchange; eventually, also psychokinesis or PK, an effect that mind can have on matter without muscular involvement. It looked now as if telepathy, or mind-to-mind exchange, was only one of four different types of possible psychic communication, at least between living minds. Whether also between the living and the dead was a topic for later stages of research.

(Parenthetically here, I need to say that not all of these developments were the results of research at the Duke Laboratory alone. Some were reported from England and other places, as for instance, the work of Tyrrell, Jephson, and others. But here I must skip over all that too briefly.)

In the years up to the end of the 1940's, especially in England and in spite of the dominant research which was on mediumship, considerable interest in spontaneous cases had continued to be shown. Without attempting a complete review here of papers on the topic, two most pertinent for the present purpose appeared in the 1940s. One was by Mr. G.N.M. Tyrrell and one by Dr. Donald West.

Tyrrell, in his paper, apparently failed in a way, to appreciate the experimental approach to research in the field and that it was a necessary one because of its conclusive value, and that it was not intended as a method that could immediately yield all of the qualitative answers he sought. Fortunately, however, he concentrated on the processes that seem to be involved in spontaneous cases, especially in apparitional experiences, and he did so more specifically than anyone before him had attempted to do.

In a logical analysis of what must go on in a psychic experience,

Tyrrell simplified the concept of that process by thinking of it as involving two stages. In Stage I, the information secured by the living person, the percipient in a psi experience, would have to be acquired by him without sensory mediation. This stage of course was an unconscious one and entirely inexplicable. It was the essentially parapsychological one.

But then, in stage II the information from Stage I was produced into consciousness. This was obviously a more explicable stage for it was accomplished by familiar psychological means, which Tyrrell called vehicles, dreams and certain waking experiences, which, as he recognized, were the same as those used in ordinary cognition.

Dr. West's paper was a careful and critical review of the spontaneous cases that had been collected and published by the Society. He attempted to assess their value as evidence of survival. By examining those cases one by one he showed that even the best of them was not perfect if considered as conclusive evidence of its thesis. In each one some kind of possible weakness could be shown. The conclusion he came to was that more and still better cases would be necessary before this material could finally answer the survival question.

Incidentally, following West's paper, Mr. Salter made a patient and rather low-key rejoinder to West's implicit criticism of the methods of authentication used in the previous years by the SPR. He contended that the cases were essentially stronger than West had given them credit for being, although, of course, he did not go so far as to say that there were conclusive. The general bearing of the issue, however, was that the method even when carried out as carefully and conscientiously as had been done traditionally by the various SPR researchers, could not by itself support a final conclusion even though the cases en masse certainly *looked as if* communications from the deceased had been involved. This, of course, was the kind of reasoning on which the experimental approach used in the work at Duke and elsewhere had been based.

The point in that experimental research, of course, was that by taking one relatively simple objective, like the question of whether thought transference actually occurs and subjecting it to the experimental approach with statistical evaluation of results a conclusion, yes or no, could be reached which would be more reliable than any based on anecdotes could be, however carefully they were verified. If the answer then, was yes, future research would have a reliable basis. Dr. West's paper very well justified the experimental approach.

This, very briefly, was the way research on spontaneous cases looked in 1948, when my own studies began. But as already indicated, the general situation in the field then was very different from what it had been earlier. Now an extra-sensory ability including telepathy as the awareness of another person's unexpressed thought had been shown to

be a reality. And now, for me, instead of a few psychic experiences to be studied carefully by a number of researchers over the years, hundreds of assorted cases were coming in to the Parapsychology Laboratory to be studied by just one individual, myself, who, at that, could give the project only part-time attention. Obviously something had to be changed. Fortunately, it could be because the objective of my proposed study was very different from that of the earlier ones. Then the attempt had been to use cases as proof of the occurrence of psychic phenomena. But that was no longer necessary now because the experimental investigations had proved the reality of psychic or psi ability.

Cases, however, had always had suggestive value as to the nature of psychic ability and that value was as great as ever, in the opinion of my husband, J.B., under whom I would be working. He thought suggestions from cases very well might enhance the continuing experimental research. And so I would not be attempting to prove anything, but simply to raise any suggestions possible, which then could be tried out by appropriate laboratory research. And so a different method of handling the material than the earlier one would be justified, a method based on suggestions that might arise from numbers of experiences showing a given characteristic, rather than on individual episodes.

The main difference in procedure this change of objective entailed concerned the time-consuming practice of authenticating individual cases. Instead of that, I could use a different procedure, for in spite of all the possible weaknesses of human testimony so carefully enumerated by Dr. West, it is also a fact that human testimony can promulgate the truth. This too can add up while the kinds of inaccuracies being different from case to case will not be so likely to do so, at least at the same rate. We reasoned therefore that by using numbers of cases, say fifty or more, each showing the same element, their true aspects should out-number the inaccuracies sufficiently that viable suggestions could result. Then, as a further check, such suggestions could be tried out by proper experiments and their truth or falsity determined.

The result of this procedure was to enable me over the years to collect thousands of cases that came from individuals who appeared to be trying to tell the truth. Some of the suggestions that the study and observation of them raised by now have experimental confirmation while others still await it. In the meantime they offer a picture of the way it looks that does have considerable suggestive value, even if it still lacks complete experimental confirmation.

The specific objective of my research was to try to see what processes must be involved in the production of psychic experiences. It was a line of inquiry that then had scarcely been touched upon in the laboratory. One of the main contributions to it from the past was Tyrrell's analysis of the process into two stages. I concentrated, of course, on Stage II,

which being psychological was amenable to analysis as the parapsychological Stage I was not.

Turning now to a few of the results of my study, an important one was that all of the thousands of experiences that could be called psychic because the percipient seemed to get information that was not supplied by the senses could be classified into one of the three types of ESP that by then had been distinguished in the laboratory and the psychokinetic as well. None of the cases was left out. It was a finding that seemed to say that the laboratory studies had indeed covered the entire list of possible psi types.

The differences between the types on which the distinctions could be based and which presumably must be the results of different psychological processes lay in the kinds of imagery involved. In the dreams one form seemed to be the result of a realistic kind of copying of the reality it reflected, the other a fantasy of sorts based upon the reality. The waking experiences involved one kind that lacked imagery, but seemed to be a 'just knowing', an intuitive process. The other had imagery but not that of dreams. Instead, it was imagery based on sense experience, pseudo-sensory or hallucinatory.

With these four forms thus distinguished, each one could be examined separately to see what kind of mental process could have produced it. The fact that in the laboratory clairvoyance, telepathy and precognition had been distinguished on the basis of their different kinds of targets rather than on any ostensible difference in their psychological processes led naturally to the question whether those processes were the same.

In analyzing those processes, an obvious difference was that in clairvoyance and precognition the percipient himself somehow obtained the information shown in his experience. In a sense, he could be considered to have been 'active' in getting the information. But in telepathy, the presence of the second person, the agent, seemed to mean that the percipient was passive and that the agent had been the active party, and that he brought the information to the percipient. But if so, then the telepathic process was different from the other two. Already in the laboratory however doubt had been cast on this idea of the nature of the telepathic process because of the positive results that had been obtained in clairvoyance and precognition experiments. In those types at least, an agent was not necessary. Tests then had been made to see whether the results would be better if an agent tried to send his thought to a percipient than if he made no such an attempt. But data from these tests did not support the conclusion that the sending was necessary or had any basic effect.

And so it was that when in my case study I approached the question of the nature of the telepathic process, the way it looked was ambiguous, to say the least. It seemed quite necessary to see what the evidence of

the cases in the collection on the question of agency seemed to be. I therefore examined all the cases I had classified as telepathic because the unexpressed thought of one person had been received by another. I found three different situations. In one an agent had 'sent' his thought, in a second, he had not consciously done so but had been thinking strongly of the percipient. In the third, he had not even thought of the percipient and in some cases he did not even know him. But the result was the same in all three. The percipient received the thought, thus making the 'sending' irrelevant. The conclusion had to be that the telepathic was not necessarily different from that in clairvoyance and precognition.

However, if the telepathic process did not depend on an active agent, its relevance in survival cases was eliminated, for that depended on an active sender who, if deceased, could give evidence of his survival by sending his thought to the living percipient. It was a hypothesis based on the way it looked. One must remember, of course, that these early workers were not trying to find out the nature of the telepathy process. Instead, they devised a hypothesis of it that would explain the phenomenon of messages that looked as if they came from the deceased. For that an active agent who could be either living or dead was necessary.

In my approach, in contrast, I was only trying to see what process seemed to be involved. I had no necessity to interpret it in any particular way. And my observations simply showed the process in telepathy to be the same basically as it appeared to be in the other types.

This change in the concept of the telepathy process, of course, affected the explanation of the other line of evidence that had earlier been taken as a possible sign of survival, that of apparitions. In fact, Tyrrell's careful explanation of apparitions depended basically on the assumption that the agent, by telepathy transfers his thought to the percipient in whose mind then the apparition is created to match the agent's idea.

But now the assumption that the idea originated with the agent was unnecessary. The percipient, not the agent, could be the author of the entire phenomenon. By ESP he could secure unconsciously the pertinent information without an agent, just as in the other two types, and then, as Tyrrell thought, he could use the hallucinatory form or vehicle by which to express it in consciousness. It could even be hypothesized that a deceased agent by PK could cause a percipient to create the hallucinatory form or vehicle by which to express the information in consciousness. The apparition then could be construed as a manifestation of the deceased sender's PK. As yet, however, no method has been found to trace the source of psi. Even in experimental research where for long any psi effect was credited to the subject who was obviously trying to produce it, it now seems equally probable, at least in some cases, that the effect is produced in part, if not entirely, by the

experimenter whose interest in the result is just as great, or even greater, than that of the subject.

As a consequence, while it was still true that influence from the discarnate could have a part in the structuring of the hallucinatory effect, the assumption that it necessarily did so was no longer necessary, for the combination of ESP and the percipient's ability to hallucinate could produce the effect. The suggested evidence of survival in apparitions was thereby deflated. It was replaced by knowledge of human psi ability, and the function that the psychological forms, Tyrrell's vehicles, play in its expression in consciousness. The way apparitional experiences had looked was no longer the way they looked now.

This change in the concept of the probable meaning of the psi process raised another question. If the meaning is not that of survival evidence, then what is it? What is the function of psi experience in human life? What purpose does it serve? The question now stood out more sharply than it ever had in the past. But how could it be answered?

Obviously, a summation of the individual meanings of all such experiences might show the purpose they serve. But no such complete inventory could ever be made, for many, perhaps most psi experiences, are never even recognized and certainly only a few of those that are recognized are reported. These few then quite likely are only a small sample of all those that occur. But even so, they should tell something about the phenomenon they represent. Therefore, it seemed that an initial approach to this question of the function of psi might be made by cataloguing the content or meaning to the percipients concerned, of the cases in a collection like my own, which had not been selected on the basis of their content. The result of such a study might serve as a sort of indicator, a poll, let us say, at least to suggest an answer, if not to give one with finality. It could test the general assumptions sometimes made about the reasons for psi experiences, as for instance, that they concern topics of highly emotional quality which the percipient would want or 'need' to know, topics which usually involve family crises, such as deaths and other critical disasters.

These general impressions seemed worth testing to see to what extent they would be supported by actual cases. Even though shortage of man-or-woman power prevented a giant project such as assessing the meaning to the individual percipients of all the available reported cases, I could undertake a more modest one, and see whether the result covered in these experiences would at least suggest something about the meaning of psi in human life by indicating the range of topics they covered.

To begin with I decided, however, not to use all the thousands of cases in my collection because even that many would have been too great an undertaking. However, when I had collected and classified the cases, I had gained the impression that the forms of the experiences in

consciousness had little if anything to do with their content, I had noticed, for instance, that those about death or other serious calamity might be expressed as either realistic or an unrealistic dream, an intuition or an hallucinatory experience. I felt too that those four forms varied in the degree of reliability with which they could be taken as instances of ESP rather than as just coincidences between the experiences and the reality to which they seemed to pertain. The most reliable of them seemed to be of the realistic form because of the matching details of the experience and the event. Therefore, for the sake of reliability and also because, as Dr. West had observed earlier, it was the most frequently reported of all the forms. I decided to use as the material on which to base my study, the realistic cases in my collection, of which, 2878 were available, many more than those of any other form.

I decided to test the two general assumptions already mentioned. First that the topics of psi experiences are of highly emotional content for the percipient and accordingly likely to concern close family members, and second, that the events involved are mainly crises such as death or other critical disasters.

The realistic cases included both precognitive and contemporaneous experiences in the proportion of nearly two to one. In classifying the cases originally I had not noticed any outstanding difference between the two time groups other than that in the contemporaneous one the event involved in the experience was happening at the time, or had already occurred, while in the precognitive cases it occurred sometime after. But the frequency of the precognitive cases raised a persistent question: Why did individuals look ahead so often in their psi experiences and select topics still in the future? That it could be the result of anxiety seemed one possibility, though probably not the only one. I therefore treated the two time groups separately for any differences between them that might be disclosed.

In order to rate the relative strength of the emotional involvement of the percipients I divided the possible levels into four sub-groups according to the relationship to the percipient of the individual most involved in the experience. These sub-groups, in descending order, were: Group I, the percipient himself; Group II, the percipient's immediate family and close friends. These two sub-groups thus constituted the 'high-emotion' division of cases. The 'low-emotion' division then included Group III, the percipients remote family and acquaintances; and Group IV, Strangers.

In similar fashion I divided the possible topics of cases into four Sections according to their seriousness to the percipient. They were: A, Death; B, Illness and serious but not fatal crises and calamities; C, Important but non-critical topics; D, Trivial topics.

Case by case then I judged and recorded the place of each one on both scales. The general results did and did not bear out the expectations.

They did so because under both scales the majority of cases fell into the expected categories of emotionally close relationships and highly critical events. But on neither scale was the majority as large as might have been expected.

Specifically, nearly three-quarters (over 72%) of all the cases fell into Groups I and II, those involving the high-emotion relationships. The remaining ones fell into low emotion Groups III and IV, and were equally divided between them.

On the other scale the majority fell into the more serious Sections, A, Death, or B, Illness and serious but not fatal crises and calamities. But these two together made up only slightly more than half of all (nearly 51%) leaving a large minority in the two non-critical divisions, Section C and D, of these, Section C, the important but not critical topics, included over a third (34.5%) of all cases. It was thus the largest division of topics of all.

The remaining cases, Section D, on trivial topics, although the smallest divisions, still included nearly a seventh of all. That even as many as this should be on unimportant topics called for special attention and particularly so because over half of the trivial topics were listed under emotional Group I in which the percipient himself is mainly concerned.

This fact then calls for a discussion of Group I, which has had little emphasis in the past. In fact, expecting as I did that the percipient's close relatives would be the main ones involved in his psi experiences, I did not even anticipate this group when I began to consider the levels of emotional involvement in different cases. But when I came upon actual instances in which only the percipient himself was concerned, such a group had to be included. Also it had to rank emotionally as the highest of all, and therefore to be placed as Group I.

The next general observation about the cases of Group I is that over three-quarters of them are precognitive while all of the other groups are only slightly over half precognitive. This raises the question whether reporting bias may be greater or at least different, when the percipient's own experience rather than one involving someone else, is involved. But since I could see no way to determine this, I could only record the data as it was reported.

According to the face value of the reports, then, it appears that in his psi experiences concerning himself, the percipient looks ahead into the future even more than he does when his close relatives are the ones most concerned. However, one other qualification must also be made. The percipient cannot have contemporary experiences concerning his own death or serious crisis. Therefore the proportion of his precognitive experiences in contrast to his contemporaneous ones is somewhat artificially increased. But even so, the proportion of precognition experiences in the Sections C and D is considerably higher than those of any

other group. And in Section D the cases on trivial topics make up the largest proportions of any group, and they are also largely precognitive.

These trivial topics of course are especially interesting because they run counter to the general expectations that psi experiences tend to be about important and especially critical events. This raises the question, what are these unimportant topics that engage the percipient's attention so noticeably and to which he even looks forward? The attempt to answer that question involves the percipient's Section C cases, which, it will be recalled, are those on important but not critical topics. However, before going into the connection between these cases and the trivial ones of Section D, the Section C cases deserve a bit of description on their own. They deserve it because of the diversity of the topics that are included. As a matter of fact, they cover so wide a range of subject matter as to include, it seems, practically any kind of important event that humans may experience. In general terms and in something of the order of their relative numbers I found that these Section C cases could be listed under the following general headings:

1. The locating of lost articles, the 'finding cases', in which individuals dream the location of a lost object.
2. Incidents of all kind involving the percipient's job or profession.
3. The outcome of bets, races, or contests.
4. Fires of major concern to the percipient.
5. Ceremonies, speeches, etc., in which the percipient will be involved.
6. Natural calamities, floods, earthquakes, etc., that will affect him.
7. Miscellaneous topics, out of order here because of the size of this category, since about a quarter of all the Section C cases were in it. The topics here fitted none of the above categories. This diverse array along with the others makes the list of possible topics a practically unlimited one.

Beside this wide distribution of topics in Section C, another peculiarity of these cases was that about an eighth of all of them were of a similar kind regardless of their content. They were precognitive dream scenes - like photographic 'stills' - from the percipient's later life. When, upon awakening he recalled a dream of this kind, he usually considered it 'just a dream' for, although it was clear and detailed, it had no special relevance for him. But later he did have an experience, see a scene, which he recognised as the original of the dream.

The level of importance of the dream scene however varied greatly from case to case. At one extreme might be, for instance, the detailed picture of a future home or an unanticipated changed job situation. At the other, a landscape in detail, only noticed in passing on a later casual trip.

When I tried to grade these scenes as to their importance to the percipient, I could find no clear dividing line. They formed a sort of continuum, but I tried to put those that seemed to have a degree of importance to him into Section C, the rest, of course, into Section D.

As it turned out more than half of these 'scenes' were unimportant. And they made up nearly all of the trivial cases of Section D, where they posed the special question of the reason for their selection. However, I could find no obvious reason of motive why they should have been selected and since none of the other forms included counterparts, it seemed possible that a basic tendency of the human mind might be showing here. Perhaps, in the relaxation of sleep, this effect might be similar, in a way, to the one on the sensory level, of looking ahead and building 'air castles'. If so, however, it was expressed here by a kind of mental toying with innocuous experiences yet to come, instead of using fantasy or imagination as in most dreaming.

Incidentally, once I recognized the relative frequency of these unimportant precognitive dream scenes, I realized that they would make the perfect back-drop for one of the commonly reported, but still inadequately explained, psychological effects, the *déjà vu* experience.

Of course, it has often been suggested that this feeling of familiarity, frequently of situations of the most casual importance, must be the residue of a forgotten dream. However, that is an explanation that could only be really adequate after the discovery and recognition of precognitive dreaming, and even of this tendency to dream ahead about topics of only slight importance. This explanation is based on the actual observation of such dreaming and it seems to me to be a more nearly perfect fit than any of those that have been advanced without benefit of parapsychology.

Coming now, after that digression, to the largest division of the four, Group II, the one in which the percipient's immediate family is concerned, the distribution of topics among the sections is considerably different from that of Group I.

First, the overall timing of the majority of the cases, although still precognitive, is much less highly so than was that of Group I, when the percipient himself was mainly involved. Unless reporting bias is the cause of this, it would seem to say that although the percipient does look ahead to future affairs of his relatives, he does not do so to the same extent to which he does when his own are involved.

In the sections of this Group II, the highest majority of precognitive cases comes in Section A, the one involving deaths. Nearly three-fourths of all of these are precognitive, a figure so high as to raise the question whether anxiety for loved ones may cause the percipient to borrow trouble and look ahead to future tragedies among his relatives. If so, however, it seems not to be supported by Section B, their serious

although non-fatal calamities in which it would seem that the percipient's concern would be nearly as great. Yet here the majority of cases is not even precognitive. Instead, this is the only section in which the majority of cases is contemporaneous. It should be noted too that in neither of the remaining Sections C and D, is the precognitive majority as large as in the corresponding ones of Group I. This looks as if, although the percipient may be anxious and concerned about the welfare of his relatives, present and future, in general the effect is not quite equal to that which arises in connection with his own affairs. This impression is supported by the nature of the topics under Section C, that comes next.

In this section, not only is the proportion of cases compared to that of the analogous one of Group I, only about a quarter as large, but it differs too in another significant way. The topics here, generalized as before, are:

1. Scenes at the location of the relative, as when a mother dreams of her distant son's environment.
2. Miscellaneous troubles of the relative.
3. Visits from the relative to the percipient.
4. Fires of special concern to the relative.
5. Miscellaneous topics.

The point to note here is that items like 1 and 3 above actually involve the percipient almost as directly as the relative. And, because of the high level of emotional attachment, most of the rest also involve him closely. And so, here again, although the percipient is shown to be deeply concerned about the affairs of his close relatives, his concern is at a reduced level from what it is when his own affairs are directly involved. It even includes to a large extent those occurrences in which he himself has also an immediate part as when his relatives visit him.

After the two high emotion Groups I and II, the numbers and proportions of Group III, although reduced from the extremes of those two look a bit like pale reflections of them. This, of course, should not be too surprising for even with the distant relatives involved here, a degree of emotional linkage still does exist. If the emotional linkage is a factor or a deciding influence in the selection of topics in Groups I and II, here too it appears to be operative and to a degree reduced in proportion to the personal and emotional involvement of the percipient. And this involvement seems inescapably to concern first of all his own affairs and only secondarily those of his nearest relatives, in a mix in which the distinction between the two relationships seems almost to be obliterated.

But now we come to the cases of Group IV in which the persons most

involved are strangers to the percipient. As already noted, the number of instances in this group is about the same as in Group III. The question, why would strangers be selected as the subjects of psi experiences of course lends special interest to this group.

The first general characteristic to note about these cases is that they are highly precognitive. The proportion of precognitive to contemporaneous instances is, in fact, the next highest of all and in each section, except A, Death, more than half of the cases are precognitive. Even though in case material such as this one cannot know with certainty how large a number must be in order to be considered significant, certainly this predominance of precognitive cases involving strangers is unexpected, to say the least. Obviously, the personal influences that seemed to affect the range of topics in the other three groups have no force here, nor would the tentative motivation, anxiety, that it seemed might have been involved in at least some of the selections in the other groups, be applicable. But if that explanation for the selection of future events cannot be invoked, then some other one must account for it.

However, a survey of these cases yielded no general reason or motive for their selection. A few could be construed as the result of concern about world affairs, presumably similar to interest in the news on the sensory level. The rest, however, had no discernable present reason. A few, however, seemed to have a future one as when a girl dreamed of an incident involving a stranger which 'came true' later after she had met and married him. The fact that an episode involving a future reason rather than a present one, should become the subject of a psi experience, perhaps should not be too unexpected because, after all, time, in the extrasensory world, is not the restriction that it is in the sensory.

However, the possible future reason in most of the cases was a very unimportant one, as when a stranger was merely a figure who later appeared casually in the news. Or still farther out, he was only a detail in a trivial precognitive dream, yet depicted so clearly as to be recognizable later.

Since I could find no general motive either present or future for the selection of strangers as figures in psi experiences, the idea of assigning reasons or motives for the selection of topics seemed to be called into question. After all, reasons imply timing, first the reason, then the result. But, since psi is not timed according to sensory standards, perhaps the very logic of trying to find reasons for individual psi events breaks down here and forces us to face a difficulty that parapsychologists still face. It is that of *believing our own results* to the extent of applying them in our own thinking! At least it looks here as if the idea that a specific motive or reason for the selection of individual topics could be named was based on an analogy with the sensory world. In that world the percipient seems to have such a motive, most commonly,

perhaps that of satisfying a need, be it only his curiosity about the world in which he finds himself.

As we all know, this urge is shown by the news the person reads, the devices he invents, the scientific and cultural progress he makes. All of this, of course, however, is limited by physical conditions, guided by attention and directed by personal interests and motivations. But when the extra-sensory level is added, the percipient is shown to be equipped to get information about his environment on a much wider front. His urge to satisfy his needs, his curiosity about the world is no longer restrained by space and time. But still, as we see here, it is not unlimited but constrained by his personal horizons. Civilization, with its philosophies and religious influences may be a force to broaden and correct this self-oriented tendency, but apparently it has not yet made a change on the unconscious level where psi experience originates. So far it looks as if any such observable change is still only skin deep.

From all of this it appears then, that through his psi experiences as through his sensory ones the percipient simply gains information about his environment, whether in matters of importance to him, or, incidentally as it may be, those of only passing interest. If, in a collection of reported experiences like this one, the numbers seem to favour items of strong emotional interest to the percipient one can well suspect reporting bias, for certainly in the first place important items will be noted more frequently than trivial ones and they will also be more likely to be reported.

What, then, has this survey of the meanings of psi experiences shown about their function? It has shown at least that the original list of cases even the several hundred reported in *Phantasms of the Living* and other early sources dealing mainly with individual items are actually only a selected few from so wide an array of others that those original ones are all but lost, although not that they have lost their meaning and possible implication concerning the reach of psi ability. However, now the function of that ability must be envisaged differently. Now it appears to be not so much different from, but mainly broader than, that of the senses. Apparently on both the sensory and extrasensory levels, the percipient must get information by which to adapt to the world around him.

However, broad as this possible range of information from both sensory and extrasensory levels may be, that on the extrasensory level even though not limited by space and time, as is the sensory, still has a limitation on the practical level. That limitation of course is the fact that it only operates spontaneously, and relatively rarely and not upon demand. Obviously, it is constrained by factors still largely hidden from researchers. Some of these apparently relate to the personality of percipients since the frequency of spontaneous psi occurrences varies with

individuals. The need to find the answers here that could lead to the control of psi effects and their production at will is the great objective of current psi research.

Observations such as these on the function of psi in the personality make it look quite different from the way it looked when the reason for its spontaneous occurrence could be taken generally to be of considerable importance to the percipient. The inclusion of unimportant and even of unimportant future topics makes the principle of their selection look much more inclusive than before when it could be said to be based on need. Now the idea of need has to be watered down until it loses most of its meaning. However, even so it could be that although psi information may at times become available because of need or motivation, it could also be that it enters as it were, spontaneously, and without the kind of personal influence that could be called reason or motivation. Perhaps both methods operate upon occasion.

But even so, and even if the principle of selection be as wide as suggested here, a basic implication is involved in the very idea that the topics are selected, and it calls for recognition.

The idea that the topics of psi experiences are unconsciously selected by the percipient is easy to assume, to take for granted. But upon reflection, it would seem that this may be an instance when an answer to one question only raises another even more difficult one. Here that more difficult question involves the process of selecting itself. The idea implies an unlimited background of other items from which one is judged to be preferable. But then not only that one alone, but all of those not selected must also be accessible to the percipient as he surveys them all (unconsciously) and makes a choice. But does this not imply an element of omniscience for the human mind? That, however, is an idea I for one find difficult to entertain but so far I cannot see an alternative. This may be again evidence of the difficulty I mentioned earlier, that of believing the evidence of parapsychology to the extent of thinking on the extrasensory instead of the habitual sensory level.

Finding the final answer to the question and implications already raised by the results of parapsychological research looms all the larger when their range and character come into view. But the answer can only come by that research as it slowly, patiently, in spite of the great difficulties that such research faces, turns the way it looked in the past into the way it will look tomorrow – and tomorrow.

After my husband's death a little pack of poems, clipped and gathered from various sources was found in the back of a drawer in his desk. Some of them were stanzas from well-known poems copied off in his hand-writing. One of these was a verse from Kipling's 'The Explorer', and I think it epitomizes his life-long endeavor. But not his alone. It is just as apropos for all true psychic researchers:

Something hidden. Go and find it.
Go and look behind the ranges
Something lost behind the ranges.
Lost and waiting for you. Go!

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PRESIDENTIAL ADDRESS 1982

PSYCHICAL RESEARCH: AFTER 100 YEARS, WHAT DO WE REALLY KNOW?

ARTHUR J. ELLISON

First, may I express the pleasure and honour I feel, having been entrusted by the Council of the Society with the Presidential chair in this centenary year. I shall do my best to justify that trust.

A Presidential Address is necessarily a personal and perhaps idiosyncratic statement. I must remind you all that the Society for Psychical Research has no corporate opinions. All the views I express are my own, alone.

The public interest in psychical research is today enormous: it has never been greater. The SPR has a reputation, 100 years old, for reliable, objective, balanced, scientific information (perhaps occasionally even verging on excessive scepticism). I shall do my best tonight to live up to that reputation. But I propose to include, where I have it, relevant personal experience. I will avoid jargon, and hope that everyone will at least understand my views, whether or not they agree with them.

The SPR started 100 years ago as a result of unusual phenomena, principally in the Spiritualist movement but also encompassing such matters as hypnotism, water divining and hauntings. The claims of the Theosophists were also exciting great interest. Many sensible people - like all of us here this evening - wished it to be found out whether or not the Spiritualists really were in touch with people in a 'next world', whether or not normal people did sometimes see ghosts, whether or not human beings could be in touch with one another in some way transcending the five senses . . . and so on. So, stimulated by the physicist William Barrett, various Trinity College, Cambridge Fellows, joined with him, with A. J. Balfour and others, to form the SPR in 1882, with Henry Sidgwick as President. Sidgwick suggested that 'the dispute as to the reality of these marvellous phenomena - of which it is quite impossible to exaggerate the scientific importance, if only a tenth part of what has been alleged by generally credible witnesses could be shown to be true - I say it is a scandal that the dispute as to the reality of these phenomena should still be going on, that so many competent witnesses should have declared their belief in them, that so many others should be profoundly interested in having the question determined, and yet

that the educated world, as a body, should still be simply in the attitude of incredulity'.

So the attempt was started, using scientific method, to determine whether certain phenomena did in fact occur and, if so, to see whether we could understand them. The aim was, and is 'To examine, without prejudice or prepossession and in a scientific spirit, those faculties of man, real or supposed, which appear to be inexplicable on any generally recognised hypothesis.'

In the SPR and similar scientific bodies in other countries we appreciate that because the phenomena which we study are so unusual and, indeed, appear not to fit current scientific theories, the standards of evidence must be, if possible, higher than those in more generally accepted areas of science. May I quote Sidgwick again: 'We must drive the objector into the position of being forced either to admit the phenomena as inexplicable, at least by him, or to accuse the investigators either of lying or cheating or of a blindness or forgetfulness incompatible with any intellectual condition except absolute idiocy.' Both of Sidgwick's views were from his first Presidential Address, delivered 100 years ago in July. How wise he was!

So we have been working hard this last 100 years; and we have learned a great deal, we in the SPR and our colleagues, some in those other similar societies in other countries formed as a result of the efforts of SPR members. Let us now consider the answer to the question What do we really know? This is *my* answer,

But first, some general considerations. 'Science', it has been truly said, 'is a process . . . a process of asking questions of nature . . . and receiving answers, in accordance with the nature of these questions'. Sometimes we have asked the wrong questions; sometimes even foolish unanswerable questions. This applies to all areas of science, not only to psychical research (or parapsychology, as many call it these days). We are all learning together. Is an 'electron' a particle or a wave? Neither . . . both. It depends on you and your thoughts and your experiment. Does a human being survive the death of the physical body? What exactly *is* a human being? What *is meant* by death?

Many people, not excluding many scientists, think that science is the business of *describing* nature more and more accurately, nature being all that out there in the physical world, independent of ourselves. This is the philosophical position which I shall call 'classical realism', and it could hardly be more mistaken: it is utterly, entirely and fundamentally wrong. Science is a process of *building mental models representing our experiences*. If the models are useful, and enable us to predict future experiences, then they become the theories of science. If they are not useful they are scrapped. Whether the models seem sensible, or nonsensical, that is, whether we *understand* them or not, is irrelevant.

The models often change radically. Newton's ways of looking at the movements of objects like planets, and apples, lasted several hundred years. Then increasing accuracy of measurement showed breakdowns in his model representing objects as attracting each other in a certain way and it was replaced by an utterly different one, devised by Einstein, in which objects did not attract each other, they 'distorted the space-time continuum'. Objects moved through this by the easiest path. Einstein's model now holds the field. It is surely not right to suggest that there are certain 'laws of nature' which we aim to discover, or have indeed already discovered. And anything claimed which appears to conflict with them cannot be true. This is indeed the language of the second-rate scientist. *Human experience is pre-eminent*. The question is: Do people have this kind of experience? Can we build a model, a *mental construct*, representing it? The 'laws of nature' are thus temporary useful mental constructs.

But what *are* the facts – *the facts of experience*? The first major piece of SPR research, the famous Census of Hallucinations gave results, later confirmed by another census, which showed that about one person in ten has the experience, once or more, of perceiving an apparition. There is no doubt whatever of that. I have had one such experience myself. But modelling the experience, 'understanding it', as representing a dead person, a ghost, wandering about this world in a semi-tenuous next world body – this model breaks down too often to be tenable. That is not the model we use today.

Today we know that hypnotism, despite its name, is a reality quite different from sleep, and is usually misunderstood. It is of great value to medicine: so valuable that some doctors are suggesting, now that they are finally convinced of its existence, that only doctors should use it. In an earlier time most doctors denied that it existed. Psychical researchers helped to validate it.

Thought transference, or telepathy as it is now called – the transference of impressions from mind to mind independently of sensory channels – does not at all fit most current western scientific theories, but it has been shown to occur many times, both spontaneously and experimentally. It does not appear to be mediated, through space as ordinarily understood, by electromagnetic waves. Clairvoyance – the acquiring of knowledge of physical objects or events independently of sensory channels – also occurs and again has been shown to do so both spontaneously and experimentally. Precognition – non-inferential knowledge of future events (a real sticking point for the classical realist) – has also been shown to exist, and in the same two ways.

The action of the mind directly on physical matter – psychokinesis or PK – has been shown to occur, spontaneously in poltergeist phenomena, and by dice throwing experiments in laboratories as well as in other

more startling ways not appearing to need statistics for their analysis, namely, by non-contact or very light contact metal bending. And the effect of the mind on radioactive decay processes (via the Schmidt machine) is also clear.

Perhaps equally astonishing for those of us brought up in a western science-based culture is the excellent evidence strongly suggesting, at least for certain individuals, reincarnation. Certain individuals, usually children, remember the facts of an earlier life claimed to have been lived by them, explanations other than reincarnation appearing exceedingly unlikely. If the excellent evidence is accepted, 'survival of death' follows, at least for those individuals. The question of whether or not human beings survive death is certainly one of our more important questions. Our fellow SPR member Mr. W.E. Gladstone said of our investigations in this and the other areas that it was the most important work which is being done in the world, 'by far the most important'.

The question of human survival of bodily death we should therefore consider now. One of the most intriguing and complex pieces of work in the last 100 years is also the best evidence for survival. It appears to have been planned and produced entirely by deceased former members of our Society, especially by Myers, Professor Butcher and Dr. Verrall. It is referred to as the 'cross-correspondences' and was apparently the result of a plan devised and put into effect in the next world (if you will permit the expression) to prove their survival and to eliminate the usual difficulty, well understood from the early days, of telepathy between medium and sitter. (Any facts given by the medium as from an ostensible communicator and recognised as true by the sitter are most readily attributed to telepathy between sitter and medium, which takes place, unknown to both, at the unconscious mental level, the facts being 'dramatized' by the medium's unconscious mental machinery, to appear to the medium as if given by a 'communicator', psychically perceived by the medium via hallucinatory experiences.)

In the case of the cross-correspondences, separate fragments of information, based on classical Greek literature concerning which Myers and company were expert and which were not known to most of the mediums, were produced (by automatic writing). Each piece was incomplete and not understood by the medium who wrote it and it ended with a request to send it to the SPR. When all the fragments were sent to London by the various automatic writers (all of whom, in different parts of the world, did not know each other) and were examined together they were found to form pieces of a puzzle, some pieces providing clues to other pieces. Many such puzzles were so transmitted, it appears, by our deceased experts, who claimed that they had devised the plan to eliminate cross-telepathy between the mediums, each medium not understanding her own piece and not having any familiarity with the

subject matter, and, secondly, to demonstrate their conscious planning presence, but not here in this world. Complex puzzles of this kind were transmitted to us for some 30 years and provide by far the most persuasive evidence for survival. The very style of the scripts was also characteristic of the personalities of the claimed communicators. The hypothesis, alternative to survival, of so-called 'super ESP' in which all those mediums in different parts of the world got together unconsciously in order to deceive us I find a little hard to accept. I know of no evidence for such wide-ranging powers of ESP.

There is other more recent evidence relating to survival. As a result of medical advances we now have studies of so-called near-death experiences, in which people have been clinically dead for several minutes with no breathing, no heart action, no brain rhythms, and have then been resuscitated. A high proportion of such people have an experience not of nothingness but of quite the reverse. Sometimes, after hearing the doctor pronounce them dead, they experience movement down a dark tunnel and come out into the light. They find themselves surrounded by relatives and friends who have already died. Then often they meet a warm and compassionate being, a 'Being of Light', a personality, who runs over with them in an evaluative way, with telepathic communication and pictorial review, the life that has just ended. Sometimes it asks them if they are ready to die. In the hundreds of cases studied the individuals were not ready and go back to their bodies - or are told that they must go back. And they regain consciousness, finding that the doctors had made a final successful attempt to revive them. They find great difficulty in putting the experience into words, but usually their lives are drastically changed for the better as a result.

There is no doubt that a high proportion of people who have been clinically dead have had that experience. I discovered one myself from the chaplain of a large London hospital who knew nothing of the literature but had received the same sort of description from a near-death resuscitated patient. I am not a medical doctor but I must ask myself: if those patients had not been resuscitated, surely the experience would have continued? Why should it cease as the body gets gradually colder and begins to decay?

The near-death experience is very like the so-called out-of-the-body experience, in which the experiencer often appears to see their physical body from another position and another body in physical space. Differences are usually, but not always, the absence of the 'Being of Light' and the absence usually, but not always, of discarnate friends and relatives. These experiences are, it seems to me (and I have had two of them), scientifically valuable. The full 'explanation', that is, the interpretation in terms of the models by which we represent human beings, is not yet clear, but there is no doubt of the light that the

experience casts in regard to our nature. It is important to realise that perfectly healthy people sometimes have out-of-body experiences. Secondly, dual consciousness is sometimes experienced, 'part' of the consciousness occasionally appearing to be in each of two physical locations; thirdly, people in that state occasionally feel themselves to be partly or wholly 'somebody else'. There is a loosening of the bounds of the personality, made definitive normally, perhaps, by the brain. The ultimate experience is the mystical experience, which is of a complete unity in which all life appears to be experienced as one (but in some inexplicable way, without loss of the personality centre). It is intriguing that out-of-body experients sometimes notice a sort of flexible tube joining the two bodies, referred to in the Bible as the 'silver cord', which is said to break at death.

Time does not permit careful analysis of this experience but it must not be taken too literally as movement in physical space: it is clearly not that. However, a number of good experiments show that it does appear at times to allow information about other parts of the physical world to be acquired. (Sometimes the experiences appear to be pure fantasy.) This *may* be another example of the unconscious dramatization of material acquired, where it is true, by telepathy or clairvoyance. We all have the machinery to do it in the unconscious mind. We are here in deep waters. The excellent scientific work done on the out-of-body experience leads to results just as anomalous as do those of modern particle physics experiments. We have to go deeper, as I shall suggest later.

Work on multiple personality in which quite different personalities appear at times to inhabit the same body, and the remarkable powers of creation of certain individuals of what appears to them to be normal physical objects and extending even to the creation of 'duplicates' of other people and even, in one case, a duplicate of themselves, extending to complete delusions of the physical senses, these all show how complex a human being really is. And I am *not* referring to the mentally ill.

What I have so briefly and inadequately described thus far has, with the fairly recent exception of near-death experiences, been well known to psychical researchers for many years. Attempts have been made, with considerable success, to bring some parts of the subject under the control normal in the laboratory.

What seems to be getting clearer is that for a long time we carried out wrongly orientated experiments. Psychologists tell us (it has been fully appreciated for only a few years) that the brain's two hemispheres have different modes of operation. By and large, the left is predominantly logical, rational, analytical, reductionist, - and is perhaps over-emphasised and developed by our education system. The right hemisphere is wholistic, intuitive, and carries out such tasks as recognizing faces, and

appreciating beauty. Psychic faculties, it now strongly appears, are mediated by the *right* hemisphere. Recent psychoneurological research with psychic subjects seems to be showing that a significant proportion of psychic people show differences from the normal in the functioning of their right temporal lobes.

So our experiments are taking a rather different direction. For example, the number of scientists carrying out Ganzfeld (whole field) experiments is growing. In these, a comfortably relaxed subject having experience of a uniform diffuse light field and a uniform auditory field (with no information via the five senses) describes what appears in his relaxed mind, while another person, the agent, looks at randomly selected pictures of various kinds. An independent jury then attempts to match the Ganzfeld experiences with the chosen pictures. The matching can be of symbolic as well as of literal features of the pictures. (The unconscious often works in terms of symbols.) The results are analyzed statistically. Some experimenters are having outstanding success with experiments of this type. (Others have very little success.)

This brings me naturally to two of the most important discoveries of this century, namely the 'sheep/goat effect' and the 'experimenter effect'. Repeated carefully controlled experiments have shown conclusively that some subjects are more likely to score positively in experiments than are others. The positive scorers are found often to have a belief that such results (conflicting apparently with 'normal' scientific beliefs) may be possible. Subjects who dispute even the possibility of such things as ESP are more likely to score negatively. The former are referred to as 'sheep', the latter as 'goats'. Scoring negatively is actually just as statistically 'unlikely' as scoring positively: both require a functioning ESP faculty.

In addition it also appears strongly to be the case that some *experimenters* are much more likely to achieve positive results with given subjects than are other experimenters. The former are called 'catalysts', and the latter 'inhibitors'.

It becomes ever clearer that an experiment is a 'gestalt', a whole. The protocol; the beliefs, conscious and unconscious, of the subjects, experimenters and witnesses; the features of the environment; all play their part in the experiment. Somewhat similar factors appear to obtain in experiments in modern particle physics. The experimenter is part of the experiment and affects the results. The same effect occurs in medicine and necessitates double-blind experiments. It is even more obvious in sociology. Psychical research is not unique.

One of the most important experiments of this century relates to 'belief'. This is the so-called 'Philip experiment'. Here a group of experimenters (none psychic) wrote a fictitious story set in England several hundred years ago and involving strong and colourful characters. They

read history books of the period; set the story in a well-known stately home and obtained photographs of it; they drew a portrait of the leading character they called Philip. When all was clear in their minds they attempted, by use of the method of 'table rapping' well known in Spiritualist circles, to 'communicate' with the fictitious Philip. After a relatively short time they had success, Philip answering their questions with paranormal raps: one for 'yes' and two for 'no'. The raps were recorded and had an amplitude/time characteristic quite different from those of normally produced raps. The table was a thin round disk on a thin central support and the experiments were carried out in a good light. (It was perfectly clear to me, when I attended, that the raps were paranormal.) Philip's answers to questions agreed with the facts of the story and sometimes extended it, the extensions not always being accurate, historically. When the members of the group strongly 'believed', or rather imagined, that Philip was standing there in his 'other world body' the raps were strong, when their belief in him weakened the raps faded away. Philip's beliefs tended to depend on who was present. These phenomena were always in light and were strong enough to stand the presence of more than one sceptical observer. They have been filmed and taped many times. Other groups have repeated the experiment, using different stories, with similar results.

This production of physical phenomena by a 'thought form' (a clear 'belief structure') is of far-reaching importance to science. I will refer to this again shortly. (And may I add that I certainly understand the defence reaction of a prejudiced scientist who has spent his life thinking and carrying out experiments.)

Let us now consider what we have learned in other areas - especially related to the beliefs of fellow scientists, particularly those antipathetic to this subject.

First, generally, we are *all* conditioned to the normal, the familiar: anything which does not appear to fit tends to be rejected. As psychologists would say, we tend to reduce 'cognitive dissonance'. Lawyers also tell us, in connection with accidents, that we see (or imagine afterwards) what we *expect* to see. Our 'observations' depend largely on the state and content of our minds, dependent on the culture in which we were nurtured. Most educated people here in the West look upon science, and the views of scientists, in the same way that religion and priests were regarded in the Middle Ages. And most scientists are classical realists, at least for most of the time. They speak of the Laws of Nature as though they were quite independent of the observer, as I mentioned earlier, and nothing can conflict with the Laws of Nature, they assume. But these so-called laws change, as we also considered earlier and illustrated by gravity. Newton's laws have changed to the quite fundamentally different Einstein's laws. The 'laws of nature' - may I re-emphasise this

- are just mental pictures, models, fitting our culture, representing our experiences, and changing for new experiences.

In particle physics, especially quantum mechanics, there is today a sharpening and clarification and extension of this fact. The fundamental difference between Newtonian physics and quantum mechanics is that the latter is *based* on observations, *measurements*, that is, on *us*, and what we do. Schroedinger's wave equation generates an endless profusion of possibilities: when perception takes place one part of the wave function actualises into 'reality' - 'by chance'. No one knows why one part rather than another. This is the sort of view taken by the greatest particle physicists. There is nothing to say about what happens between the measurements. As Heisenberg said, 'The term "happens" is restricted to the observation ...'. This is an important new philosophy of science. Quantum theory is closely linked to philosophy and to theories of perception, just as is psychical research. A second fundamental differences between Newtonian physics and quantum theory is that the former predicts events and the latter the probability of events.

It is important to appreciate this clearly: science is a process of building models representing our experiences; our experiences depend largely on the state and content of our minds. Scientists are not describing a real physical world around us and independent of ourselves. Front rank scientists do not fall into this serious error even though it is generally adequate for most of us to assume most of the time that the physical world is independent of ourselves. Psychical researchers (parapsychologists) were presented with this, in clear form, a long time ago and it has been the source of many difficulties. (Most of the questions we get asked are based on unappreciated classical realism.)

Many of the problems experienced by some scientific people in dealing with this subject have arisen, therefore, because the well ascertained facts of experience in this subject, widely but not generally available, do not appear to fit classical realism - the mechanistic, reductionist, Cartesian, 17th century view of the universe. It is sometimes suggested, therefore, that healthy normal people who have unusual (psychic) experiences are in some way mentally ill, or that the investigators who describe and discuss those experiences are being deluded, by others or by themselves. As I suggested earlier, psychical researchers have long learned the need for scientific standards of evidence higher than those adequate in other more generally accepted areas of science. (I do not suggest that we always attain the highest standards; but we do our best.) It is surely clear: the facts of experience come first. If current scientific models do not fit, they must be extended or altered until they do. This is the normal way scientific understanding grows. And some of the models of physicists are today purely mathematical and unimaginable.

So an important subject we have learned much about in the last 100 years has been human behaviour. One cannot separate in the traditional way the observer from the world, from the observations. (This has long been well known to philosophers and psychologists.) We have learned that a blinkered scientist having an old-fashioned view of science, fearing that the new facts discovered by psychical researchers will demolish the foundations of his life's work, reacts emotively rather than scientifically. Reason and logic often disappear: so many times have we seen this. In the guise of scientific criticism and claiming to protect science from reversion to witchcraft he is actually an anti-scientist, casting aside all the open-mindedness to new ideas supported by good evidence which a true scientist would have. The greatest scientists show an especially keen interest in claimed new facts, well evidenced, which appear to have impact on their work. Fellow members of ours who were, or are, also Fellows of the oldest and most distinguished scientific body having its headquarters in this building (The Royal Society) illustrate this: Sir William Barrett, Lord Rayleigh, Sir William Crookes, Sir Oliver Lodge and, more recently, Professor F.J.M. Stratton, Professor W.A.H. Rushton and Sir Alister Hardy. Several of these, with others, were also honoured with the Order of Merit. May I mention, in addition, Dr. Gilbert Murray, The Earl of Balfour and C.G. Jung to illustrate the point that we are probably not *all* being deceived – at least not all the time!

The greatest scientists, such as Einstein, Planck, Bohr and Heisenberg, can break out of the conditioning into which we have all been fitted by our culture, especially by our educational system, and consider the universe in entirely new ways. The greatest physicists are accepting, it is quite clear, that we, our thoughts, our actions, are important components of the universe that we *appear* to be perceiving. Perhaps – and this is a perfectly seriously held view (remember the Philip experiments) – perhaps the thoughts of physicists who are ‘discovering’ new particles are playing a major part in *creating* them. The difficulty in obtaining repeatability in many psychical research experiments is relevant too. Statistics has to be used by psychical researchers in the macro-universe just as particle physicists need to use it in the micro-universe of the nucleus. Where the mind plays its biggest part, there repeatability is difficult to obtain. A psychical researcher studies nothing if not the mind.

That is probably sufficient, in a short address, on the fog of conditioning through which we all, scientists and laymen alike, look upon this very illusory universe. I suggest to my fellow psychical researchers that our difficulties in gaining general scientific acceptance for our subject are little different from those experienced by other scientists, particularly nuclear physicists, in theirs. Let us be a little readier to speak positively

and vigorously of what we have discovered, even though the phenomena may not occur every time because of unknown or uncontrollable factors. Some of us, it seems to me, in our efforts to be 'scientific', are over-doing it and appear negative and destructive. And the general public, and some scientists, may be misled, and not realise that the evidence is as good as it is.

Our discoveries during this period of review concerning the mind/body relationship and the physically creative (or destructive) power of thinking and belief is surely of relevance both to philosophy and to medicine. In philosophy (and neurology) these discoveries should be part of considerations of mind/brain monism or dualism. Also, if a clear thought form can produce the physical phenomena of the Philip group or metal bending, and bearing in mind the stigmata of saints and similar well-authenticated phenomena, it would appear likely that the right mental attitude combined, of course, with sound habits of living, should help to lead to a healthy body or to healing processes in a diseased body. Holistic medicine, in which the whole patient, body, mind and living patterns, are all considered, is now under serious discussion by many doctors, as well as just the set of symptoms (and their removal) related only to the part that has broken down. There seems but little point in removing symptoms chemically or surgically without a clear delineation of what led to them and some help to the patient in correcting that state. It is sad, however, that in our health service few doctors have time for other than attempts at symptom removal. By bio-feedback, it is becoming clear, and this too is important, many of the body's automatic processes can be brought under the control of the mind if their state can be made overt to the subject.

Perhaps many of the so-called 'complementary' healers of various kinds are in this area of mind/body interaction. Some of the phenomena I have observed with healers fall well within the subject matter of this address. Though their theories are not always to the taste of scientists, and indeed often are not in accord with other related self-consistent theories, perhaps their results and the great and growing public interest in them will lead to serious research in greater volume and a stronger movement towards holistic medicine, in which the skills and insights of all healers, main stream doctors and complementary practitioners alike, can be effectively deployed, to the advantage of us all.

Reductionism in medicine will not begin to disappear quickly until the doctors of our seriously over-loaded health service have more time and, more importantly, until medical courses change. Similarly, some of the problems we have with some scientists will not begin to disappear until the education of scientists is more affected by the clear implications of modern physics and by the even clearer implications of psychological research.

Let us now consider further matters briefly touched on earlier, and the way ahead. I mentioned evidence showing the importance of the right brain hemisphere in experiments in this subject. The further and deeper study of neuro-physiological correlates of states of consciousness is surely of great importance. Research would be better directed and accelerated if the psychic faculties could be reliably picked out; or perhaps, even better, if by the methods of bio-feedback, we the researchers could train ourselves to be the subjects; and if we could train other subjects to move into the right states to optimise the results. (We must not forget that the investigator's mental state may be important too.)

The success of Ganzfeld experiments was also mentioned earlier. The reason may be the reduction in the so-called 'noise', that is, normal information input from the environment, and random thoughts. The technique reduces the noise and appears to allow the ESP information a better chance of getting through. That view is to some degree confirmed by hypnosis, which also appears sometimes to increase positive ESP scoring. There are a number of ways of producing deep relaxation besides Ganzfeld and hypnosis. The various methods of meditation are important, as is bio-feedback. Traditionally, psychic faculties are supposed to result from the practice of raga yoga (meditation) and tests of yogis appear to confirm this. Their physiological parameters have been observed and that information can be used for training others, similarly.

The use of modern measurements, recording and analyzing techniques, much improved during our century and lately involving electronic computers, has become of great value. I think of the recorded paranormal voltage fluctuations, impossibly high frequency of telephone calls, filmed paranormal movements of objects, of the outstandingly investigated 'poltergeist' phenomena of the well-known Rosenheim case. (There are also other good competently studied poltergeist phenomena.) This type of effect, as old as the subject, is now better recorded as a result of adequate instrumentation. The effects are random, unconsciously caused and, it is not forgotten, frequently (and crudely) imitated by stressed or deprived children for obvious psychological reasons. My reference was to the genuine variety of phenomena.

Since Uri Geller started spoon and key bending, many metal bending children and others have been studied in laboratories in several countries using properly monitored and controlled specimens and, it is claimed, have succeeded at a distance. I have myself observed at close quarters two children gently stroke heavy steel tea spoons I had myself brought and seen both bend like putty after one minute. There is no doubt in my mind that the phenomenon occurs. However, the theory, the model, to fit it is lacking as yet.

Here I must offer a word of caution. Often physical laboratory experiments on children, on psychic healers attempting to influence biological specimens, and others, are carried out by new scientific 'converts' to this subject. Their experiments are often inadequate scientifically as well as parapsychologically and sometimes lead to misinterpreted extensions to normal science rather than to evidence of psychic faculty. Confusion sometimes results and the psychic subjects wonder why the investigators are so long in publishing what they thought at the time were successful results. This is a very difficult multi-disciplinary subject. Protocols should be very carefully examined by advisers with experience in all the relevant areas of science, including both the normal and the paranormal. And the presence of reputable expert witnesses is often vital if the results are to be seriously considered.

So far we have described, very briefly, the excellent evidence for experiences which do not fit current scientific models representing our normal experiences of this universe. We have considered the normal conditioning to which we are all subjected, habituating us to the familiar, fitting the culture in which we have been nurtured. We have given brief consideration to the defence mechanisms, transcending reason and logic, which arise emotively in some scientists who think (rightly) that their belief system, based on a philosophy of classical realism, and on which their work is based, is challenged. We have learned a great deal in the last 100 years. So what is now the way ahead?

Clearly we must continue to adhere to the highest scientific standards of accuracy, objectivity, balance and integrity. Even though the quality of the evidence for just about all the phenomena generally understood as falling within the scope of this subject is, after 100 years, very good (and may I interject that nothing - repeat nothing - in *all* areas of human knowledge is 100% certain) we must on no account allow those standards to slacken. But we must not - at least it seems so to me - expend our limited resources on repeating the same evidence in different forms in attempts to convince the unconvincible, but we must move on. The search must continue for better models, better representations than those we now have, which will in time encompass the whole of present science as near approximations in a greater scope. There are signs of movement in the right direction. It is fascinating to observe that the methods of science developed here in our Western culture appear to be leading inexorably towards the philosophical views of reality which appeared in various forms in the eastern cultures, especially of India, many centuries ago.

Bohm, a physicist, suggests a more fundamental 'matrix' from which the phenomena, the apparently inconsistent, ambiguous, confusing phenomena of experimental physics, especially of the very small, appear to spring: an 'implicit' unified universe from which the 'explicit'

universe we think we know so much about via our senses, arises. I am not a physicist and perhaps not well qualified to judge, but certainly such ideas appear to agree with the experiences of the mystics, who speak of an unbroken undifferentiated reality from which the phenomenal manifold universe springs. If Bohm is proved correct then a great many pieces of our psychical research jigsaw will fall into place – telepathy, precognition, psychokinesis, and others – side by side with the anomalies of particle physics.

There are semantic differences in the way of discussion, as language is based on the left brain hemisphere and on our shared experiences of the 'normal' universe. If we are to discuss deeper underlying 'realities' then we the discussers will need shared experiences of them as a prerequisite; or we could find discussion impossible. We can acquire these experiences only by transcending the well-developed ratiocinative machinery of the left hemisphere. Some subjects, in controlled scientific experiments using LSD appear to have achieved this and have confirmed many of the statements of the mystics, referred to by Bohm. A few others appear to have achieved it under sensory deprivation conditions, with or without LSD. However, the traditional way is meditation, leading to those altered states of consciousness. I have had discussions with a number of people who have the wider and deeper experiences to which I refer and their advice is the traditional one – look at your 'perceiving mechanism': you will find that it is also a creating mechanism, playing a key part in the production of this physical world. This you have to experience and transcend.

But here is the difficulty! Scientists trained to go into the laboratory to measure and model the 'world out there', assumed to be real and permanent, and whose experiments show that it is far from that, will not and do not find it to their taste (as I said earlier), to turn from their left brain hemispheres and develop the right (perhaps the source of the intuitions on which the great scientific advances have been made) and, after long effort, to achieve experience of other types of consciousness. Surely, with the intensity of determination shown so often by the leaders in science, and with the help of such methods as bio-feedback, we can take steps in this direction. We must train and develop ourselves, become our own subjects, have first-hand experience. It is perhaps easier today than it has ever been.

At the end of our first century we have the satisfaction of real achievement. The old mould of classical realism is beginning to break up all around and new ideas are being constructed. The principles on which future advances will probably be based are becoming clearer. Our experimental work has found its fruitful directions along different lines, holistic lines. The scientific community is beginning to appreciate that psychical research, parapsychology, is a valid discipline, of great impor-

tance. (The Parapsychological Association, 25 years old, has been since 1969 affiliated to the American Association for the Advancement of Science.) More rapid advance will take place only when Government research funding bodies provide the means for the interdisciplinary psychical research needed on a larger scale in, or closely linked with, universities. Parapsychologists, psychologists, engineers, physiologists, physicists, bio-chemists, philosophers, will all be needed in teams. The very few PhD students at present supported by the SPR research funds, supervised by other more senior research workers often in their spare time can, in the nature of things, advance only slowly.

The perennial questions which we are trying to answer are the most difficult which humanity faces. Each fresh item of knowledge, of insight, shows new vistas ahead. However, general public acceptance of the validity and importance of the subject is almost here. But the difficulties facing us are orders of magnitude greater than those in other subjects. We are at last seeing that the problem is in the nature of the human mind. The next steps involve perhaps not the development of new instruments but of ourselves.

**PROCEEDINGS
OF THE
SOCIETY FOR PSYCHICAL RESEARCH**

Volume 56

Parts 207 – 212

1973 – 1982

**SOCIETY FOR PSYCHICAL RESEARCH
1 ADAM & EVE MEWS, LONDON, W8 6UG**

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BEST COPY

AVAILABLE

Variable print quality

LATE AFTERNOON

WEDNESDAY 26th JULY 78

MM/SPR/C1

5.15 PM
10.15 PM
11.15 PM
12.15 AM

BE LAB

TCU

DATE: 26th July 1978
 TIME: 16:03

26th July 1978
 The City University, London
 Dept. of Elec. Engng.
 Bio-electricity Lab.

Chart speed
 = 50 mm/minute

16:03

16:03

Take left wing
 16:10:00

16:20 hrs

Normal Occulting bearing
 by m.

m.m.'s hands over box containing photocell

16:16:02

'check' marker
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 failed

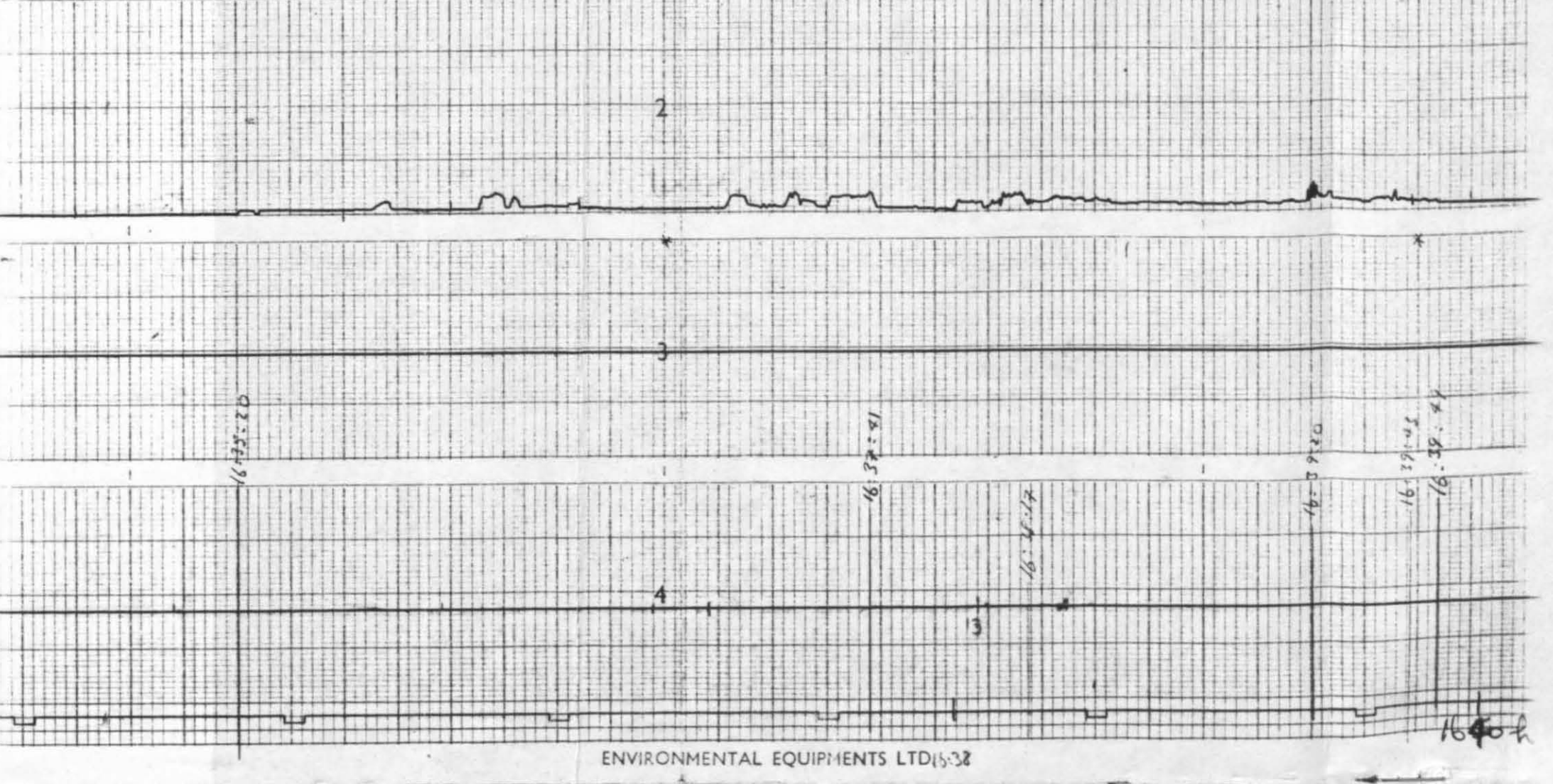
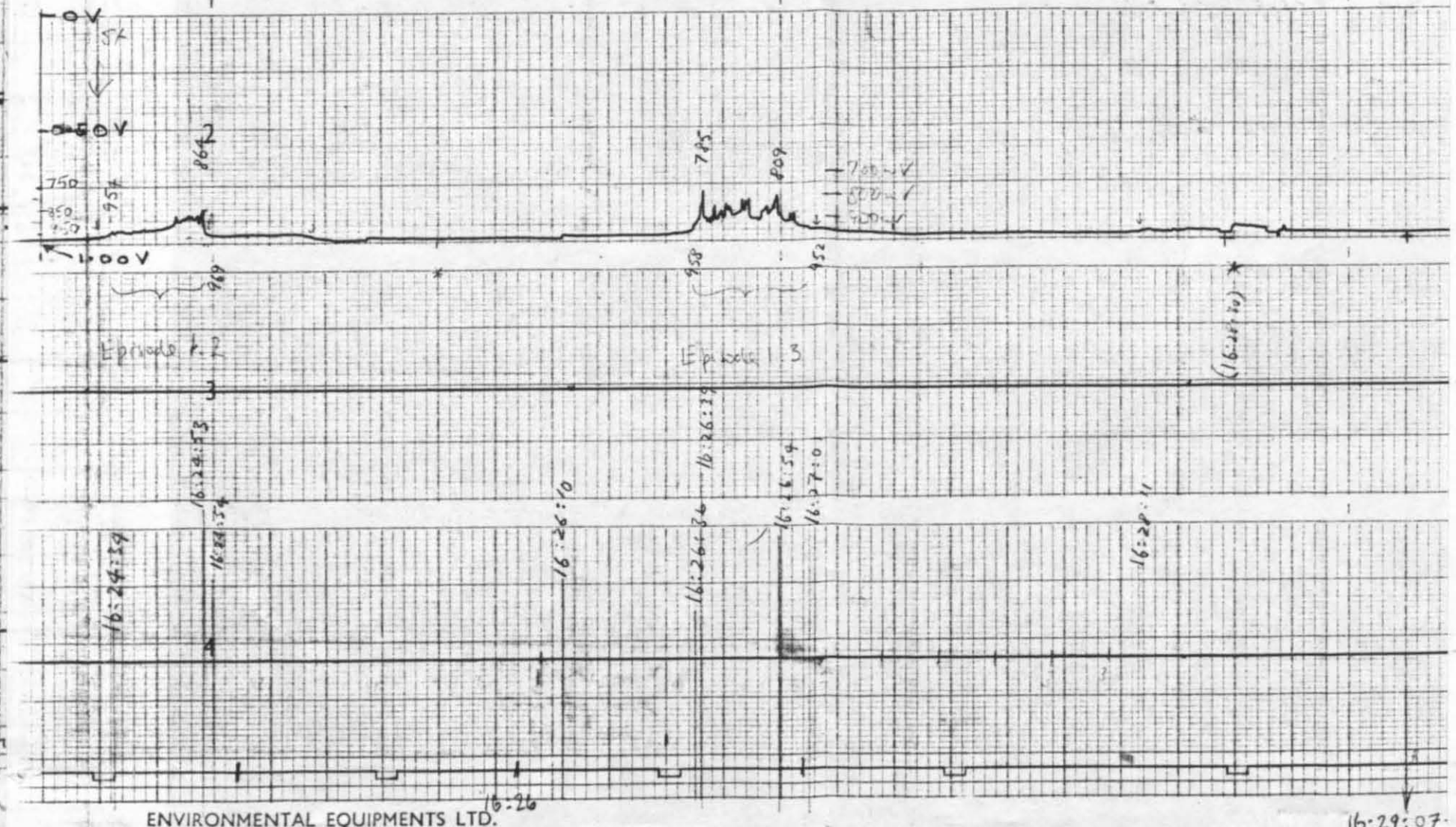
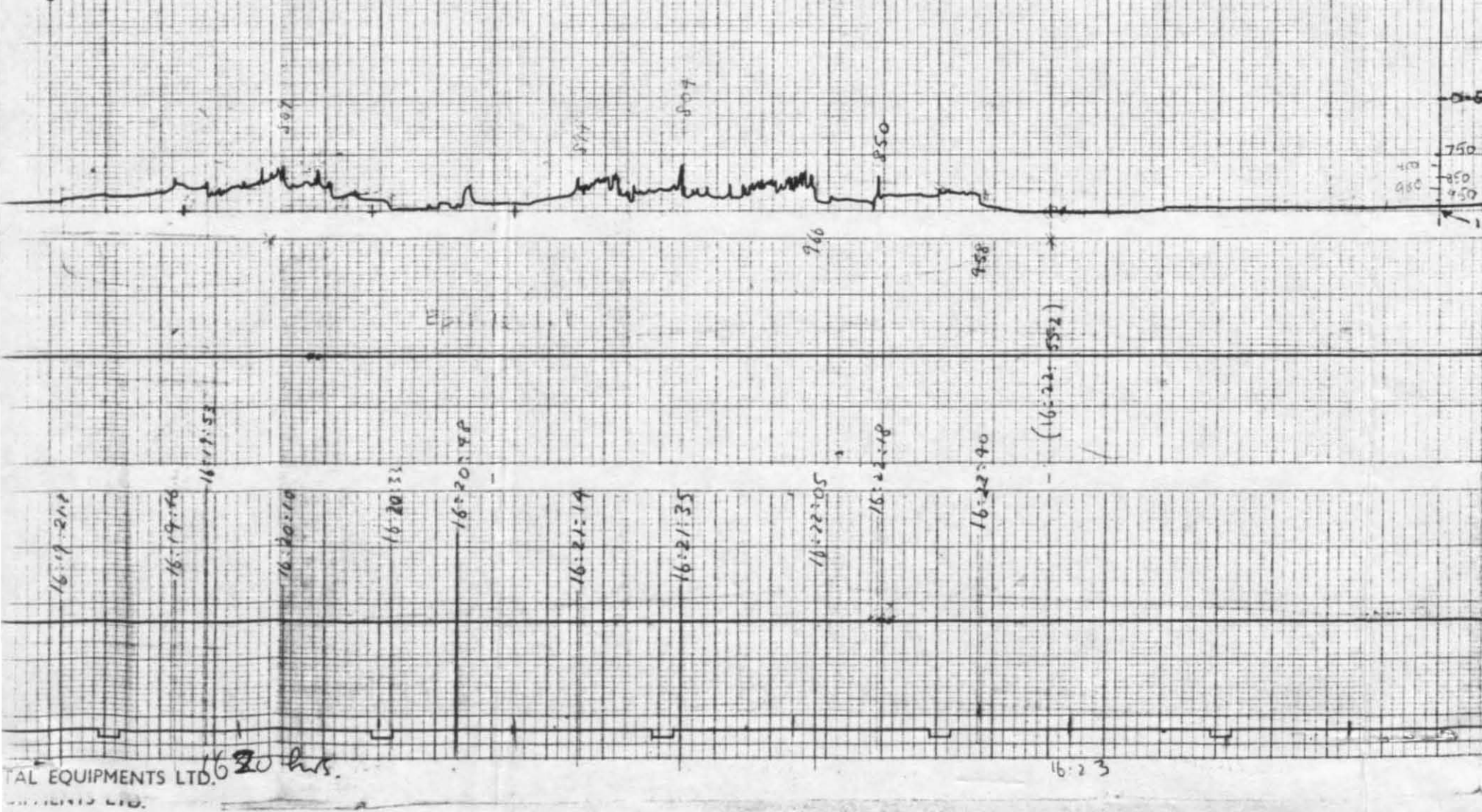
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16:17:21.8

16:19:06

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ENVIRONMENTAL EQUIPMENTS LTD

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- 800 mV
- 900 mV

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- actually 17:08

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MR Bennett
Litts West
Dorothy
W. Byers Brown

M. R. ...
from ...

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most of them

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after Matthew's departure on 6:30 PM

+ 700 mV
+ 800 mV
- 900 mV

Shift
↑

26 July 1978



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18:00:28

17:00 hrs

ALL DAY. FRIDAY 28th JULY 1978

MM/SR/C₂

A. Gregory
Ph.D. Thesis
Supplementary
Material

8/5/78

8001

BE LAS

TCU

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1053

28/7/78

1053

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ENVIRONMENTAL EQUIPMENTS LTD. 1053

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ENVIRONMENTAL EQUIPMENTS LTD.

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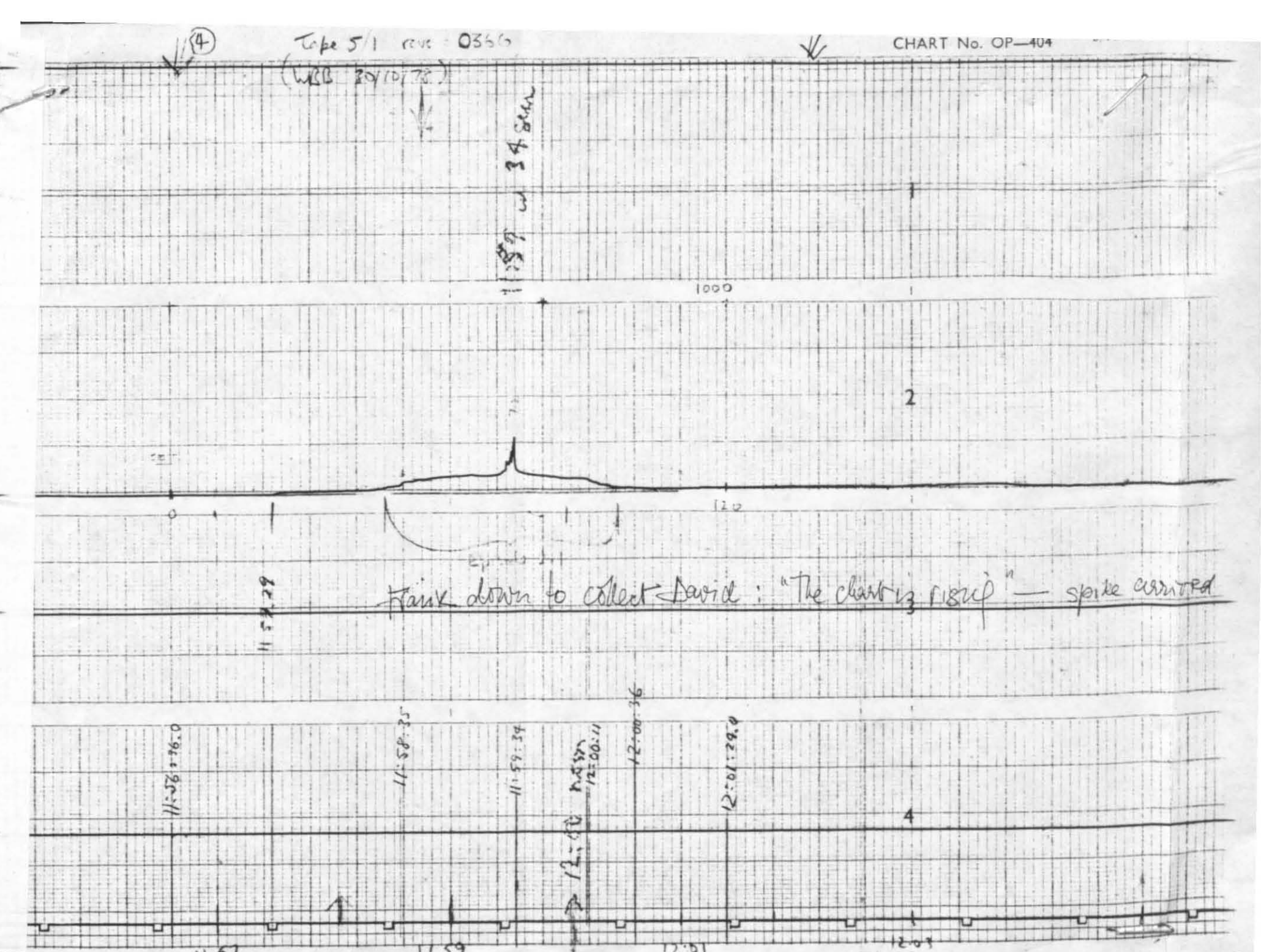
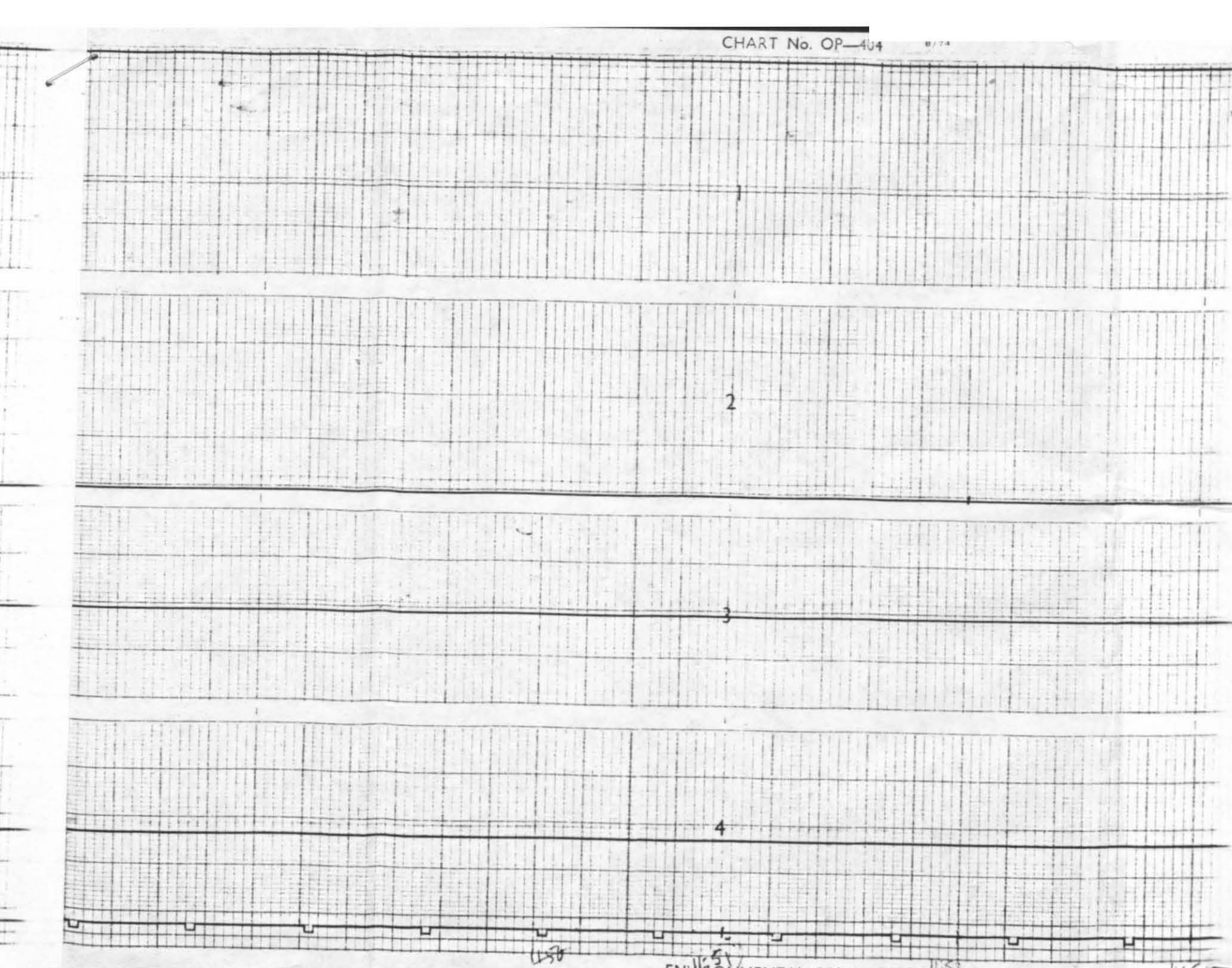
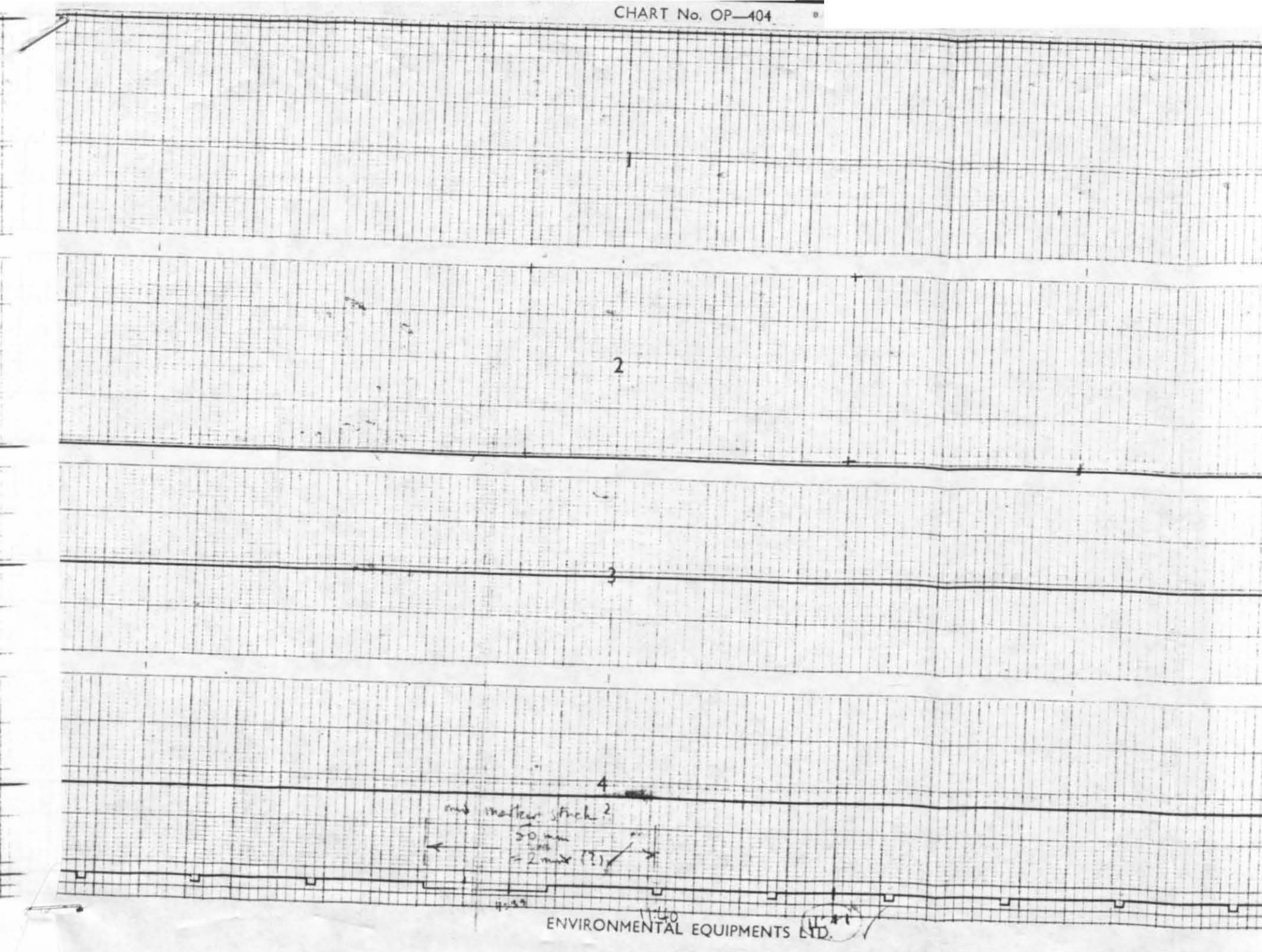
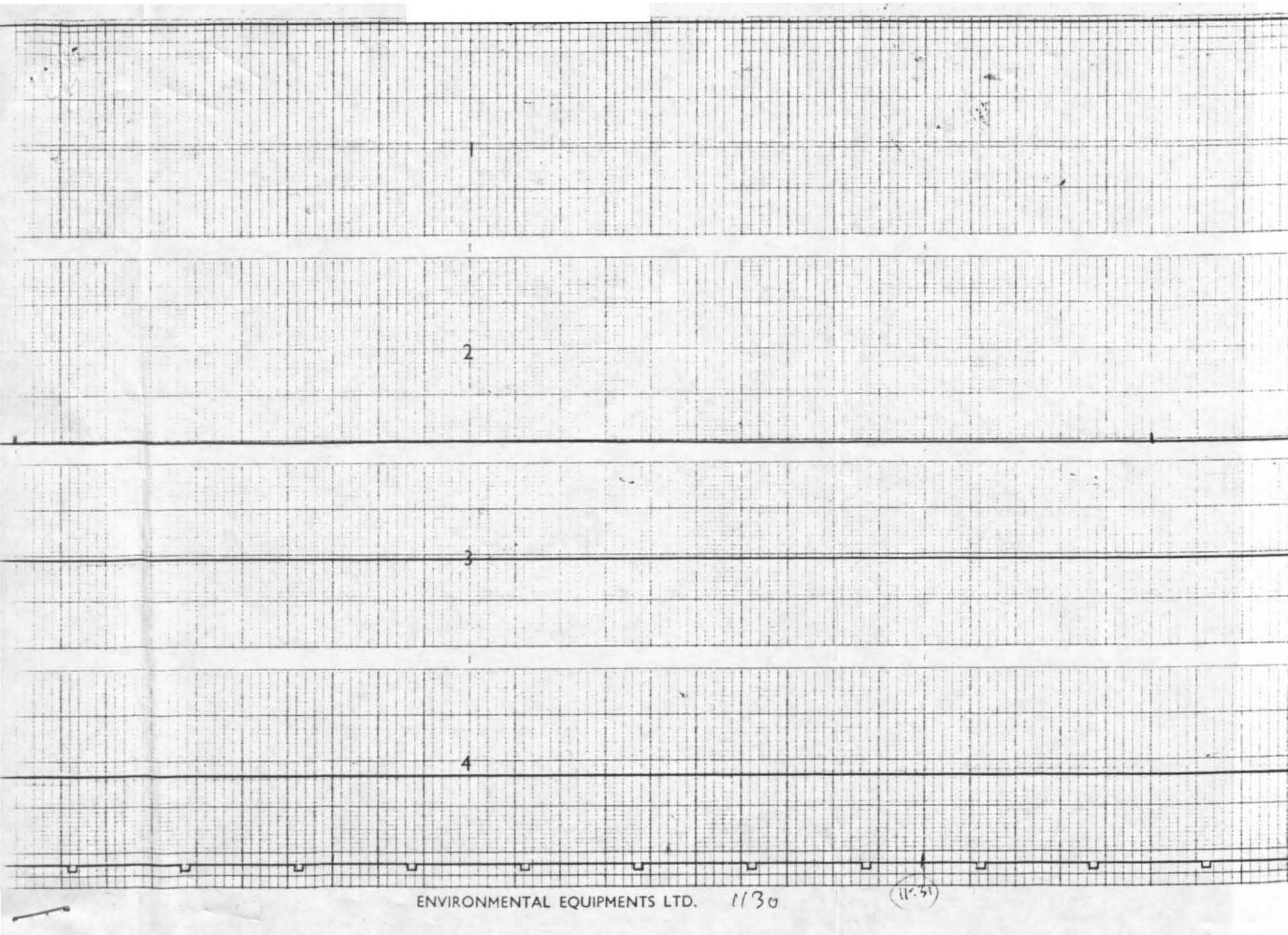
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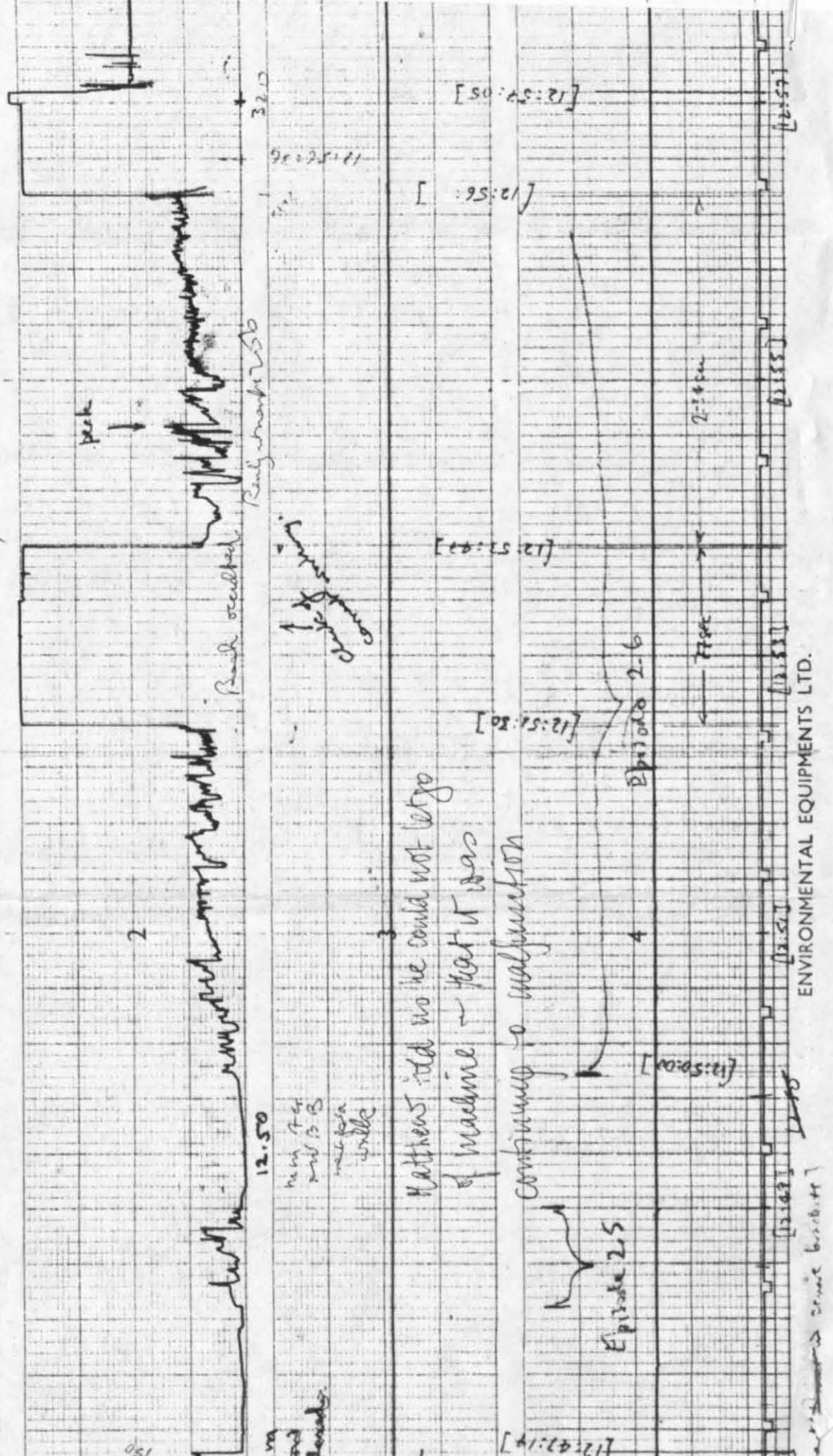
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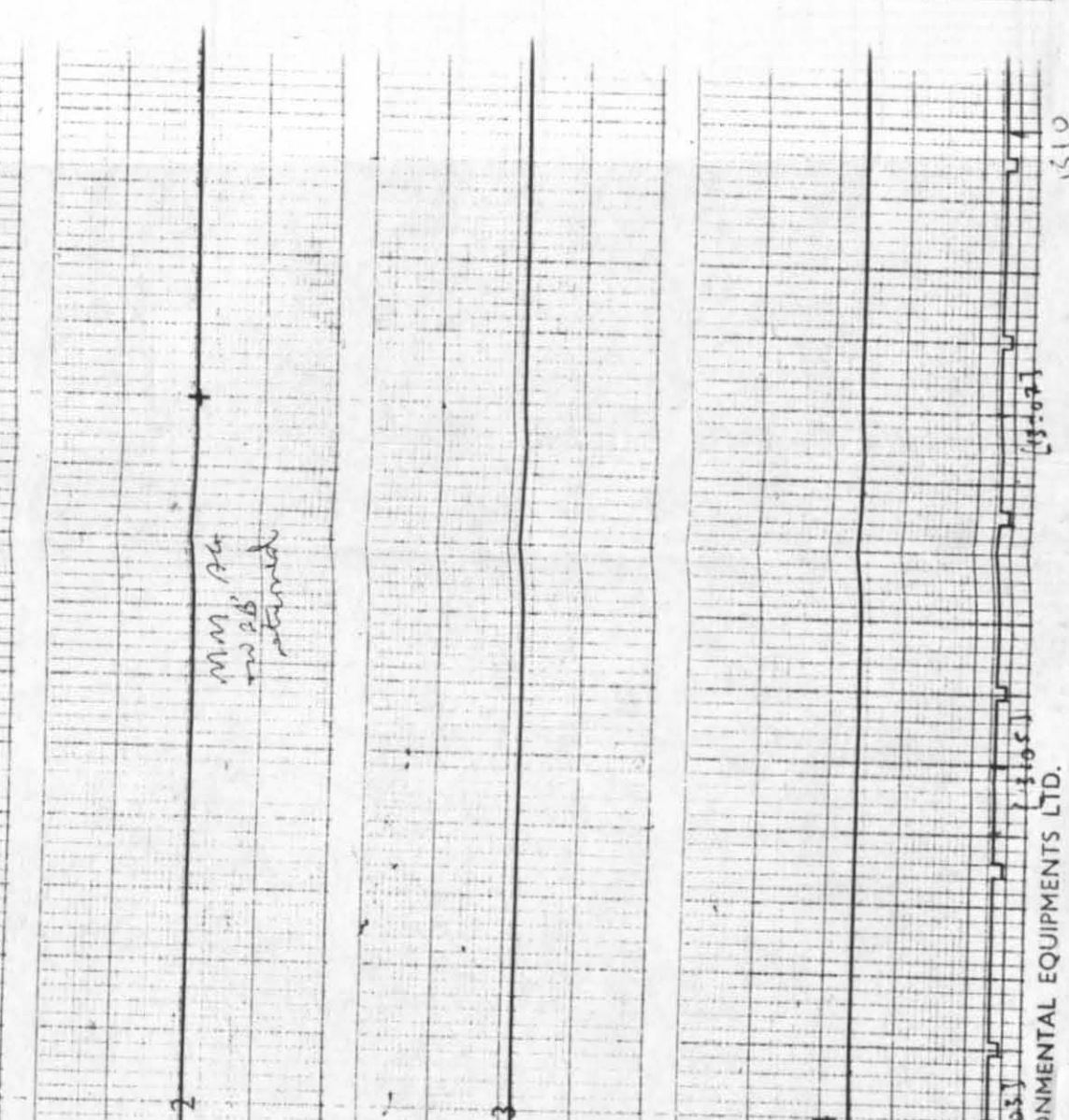
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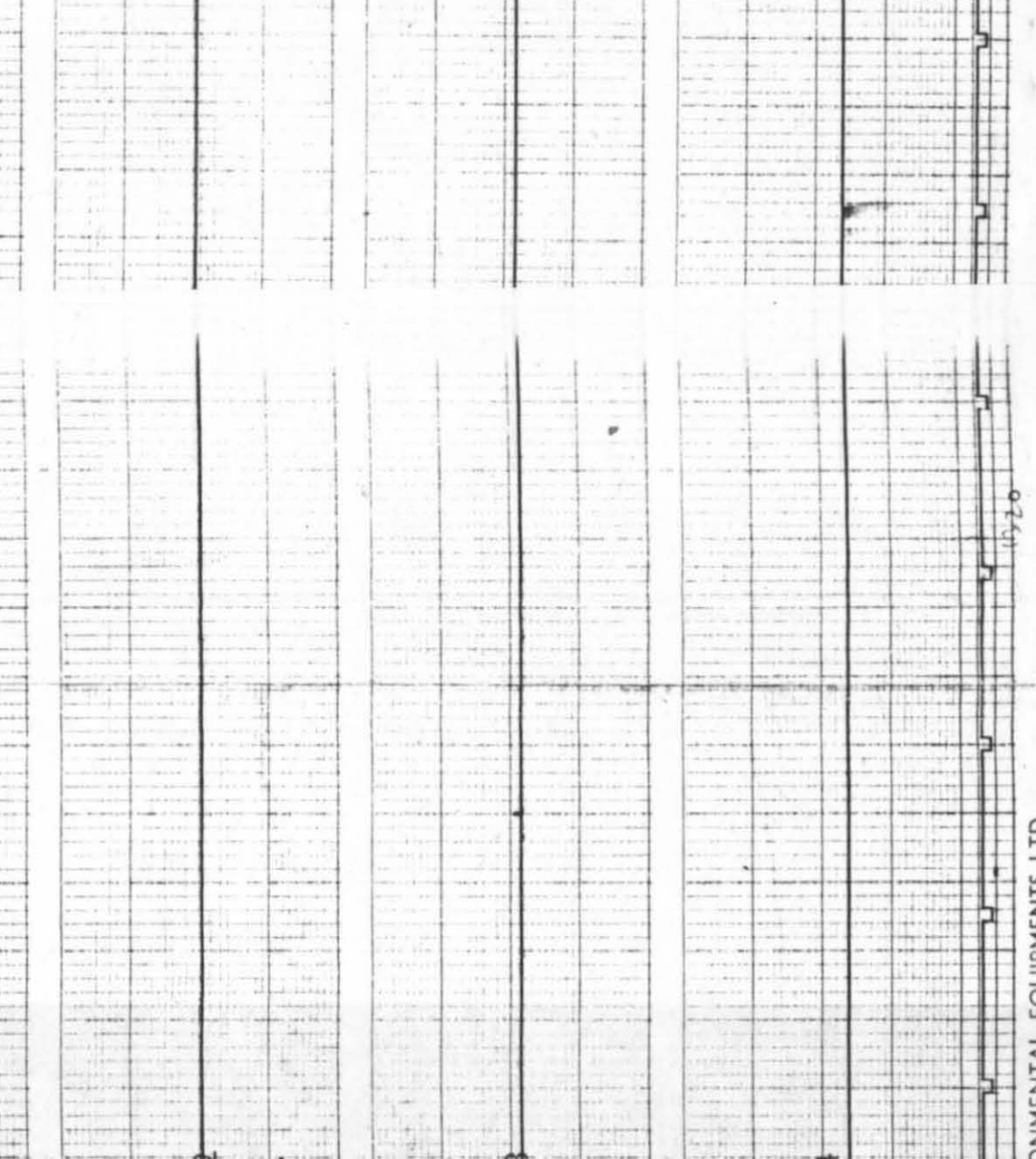
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Bio - Chemistry Lab.
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off-gas
C. G. Miller
Pittsford, N.Y. 14534
Matthews (FRANK)



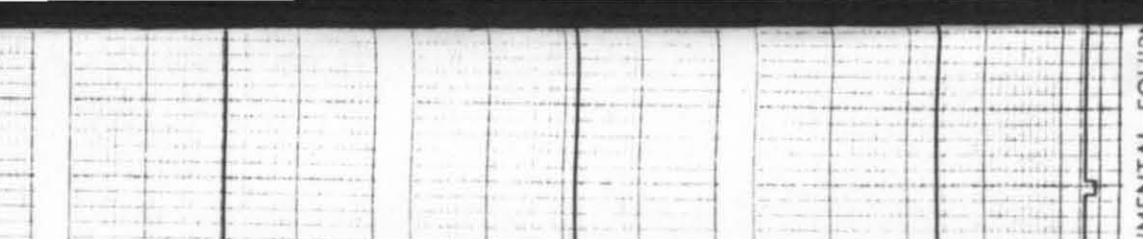
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Matthews (FRANK)



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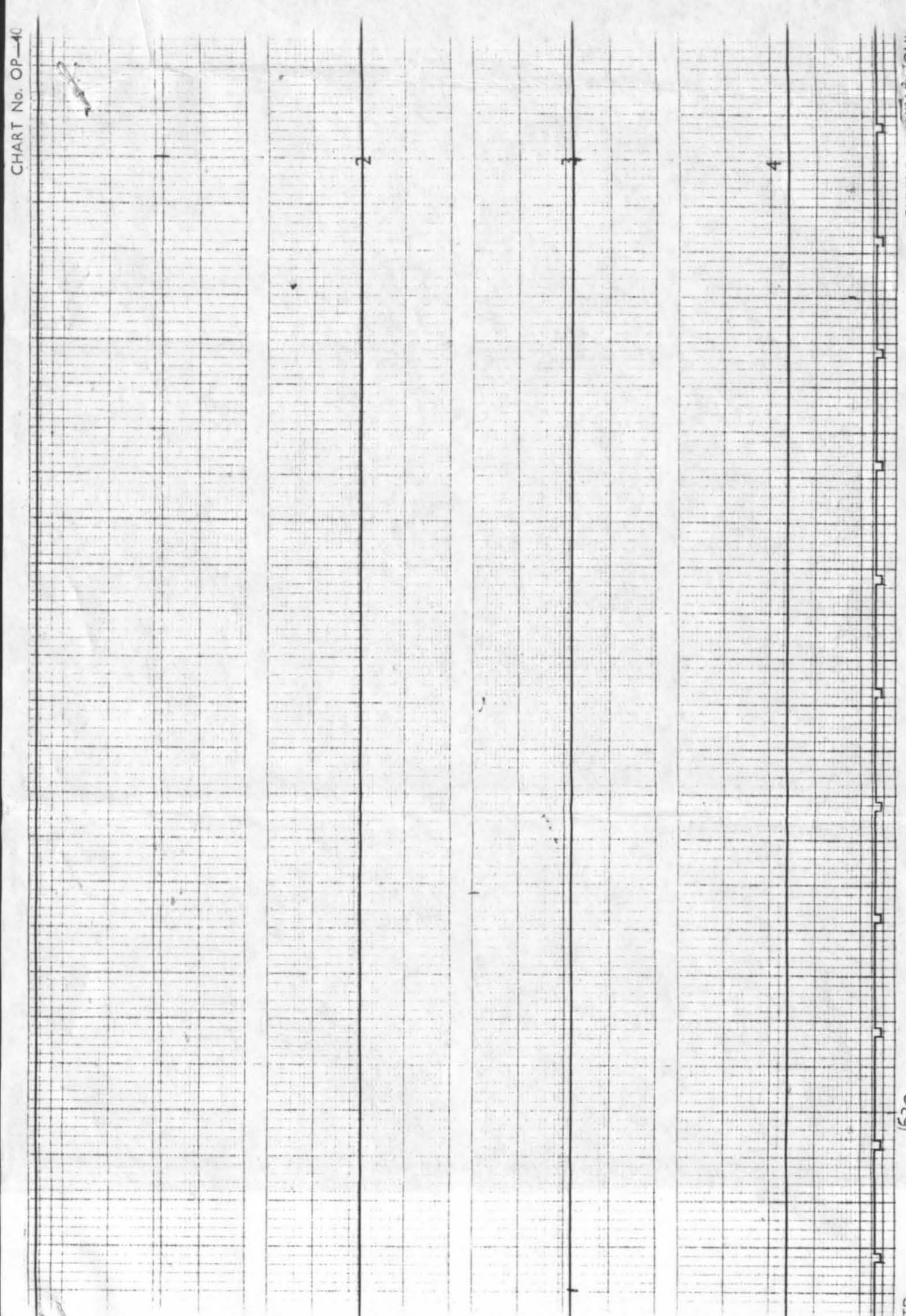
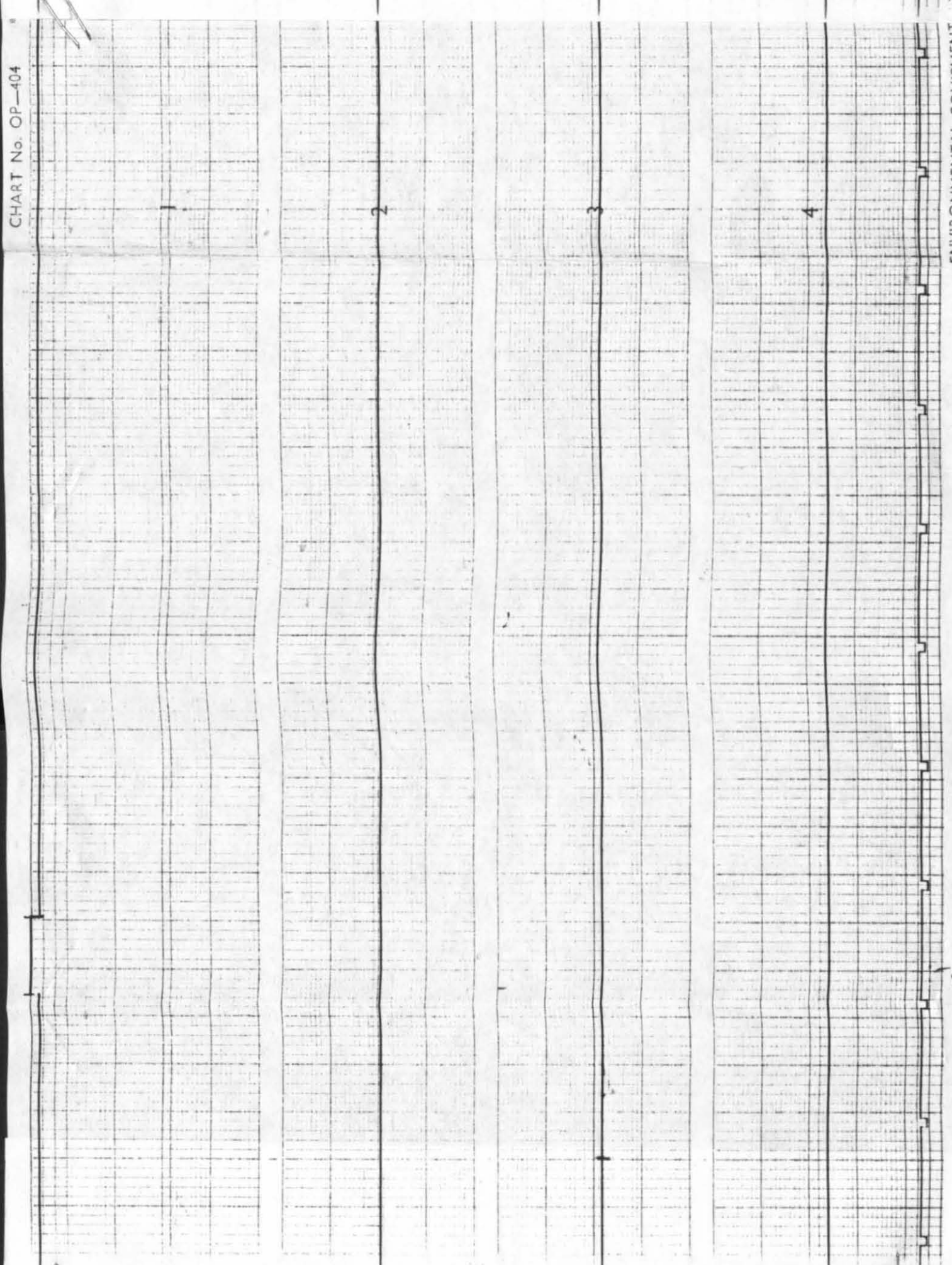
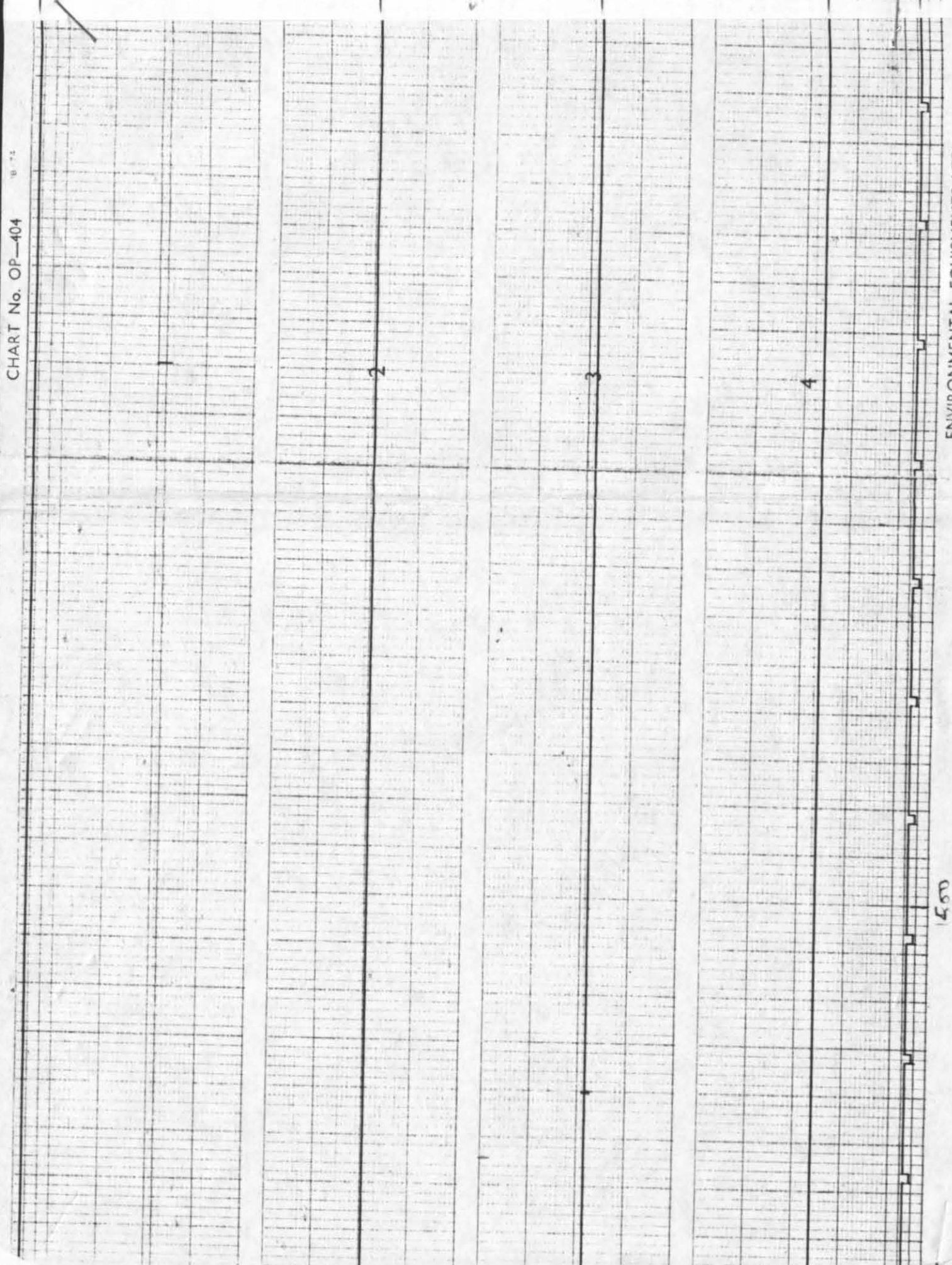
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ENVIRONMENTAL EQUIPMENTS TO 1530



28/7/78
1600

A. Gregory
Ph.D. Thesis
Supplementary
Material

AFTERNOON OF

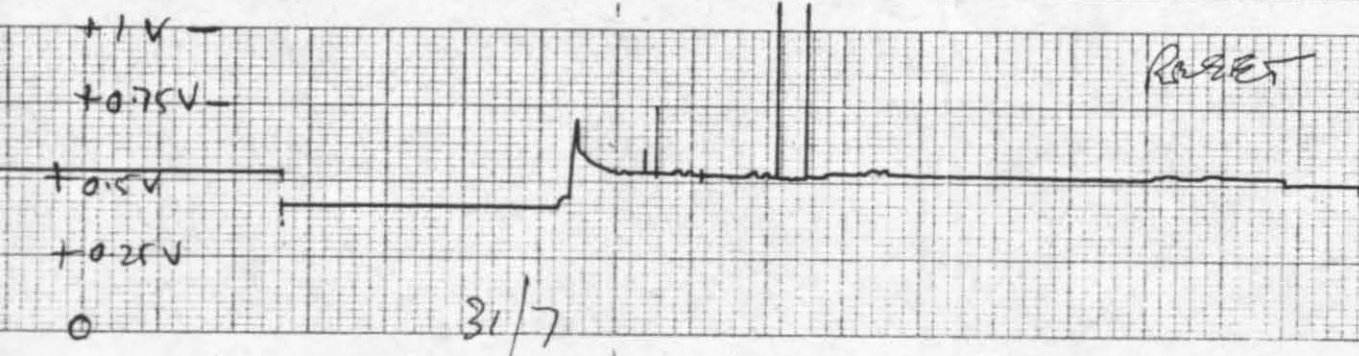
MONDAY 31st JULY 1978

MM/SPR/C 4

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phenomena on magnetic tape (FM recording)
TESTING



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Reset

Reset

431

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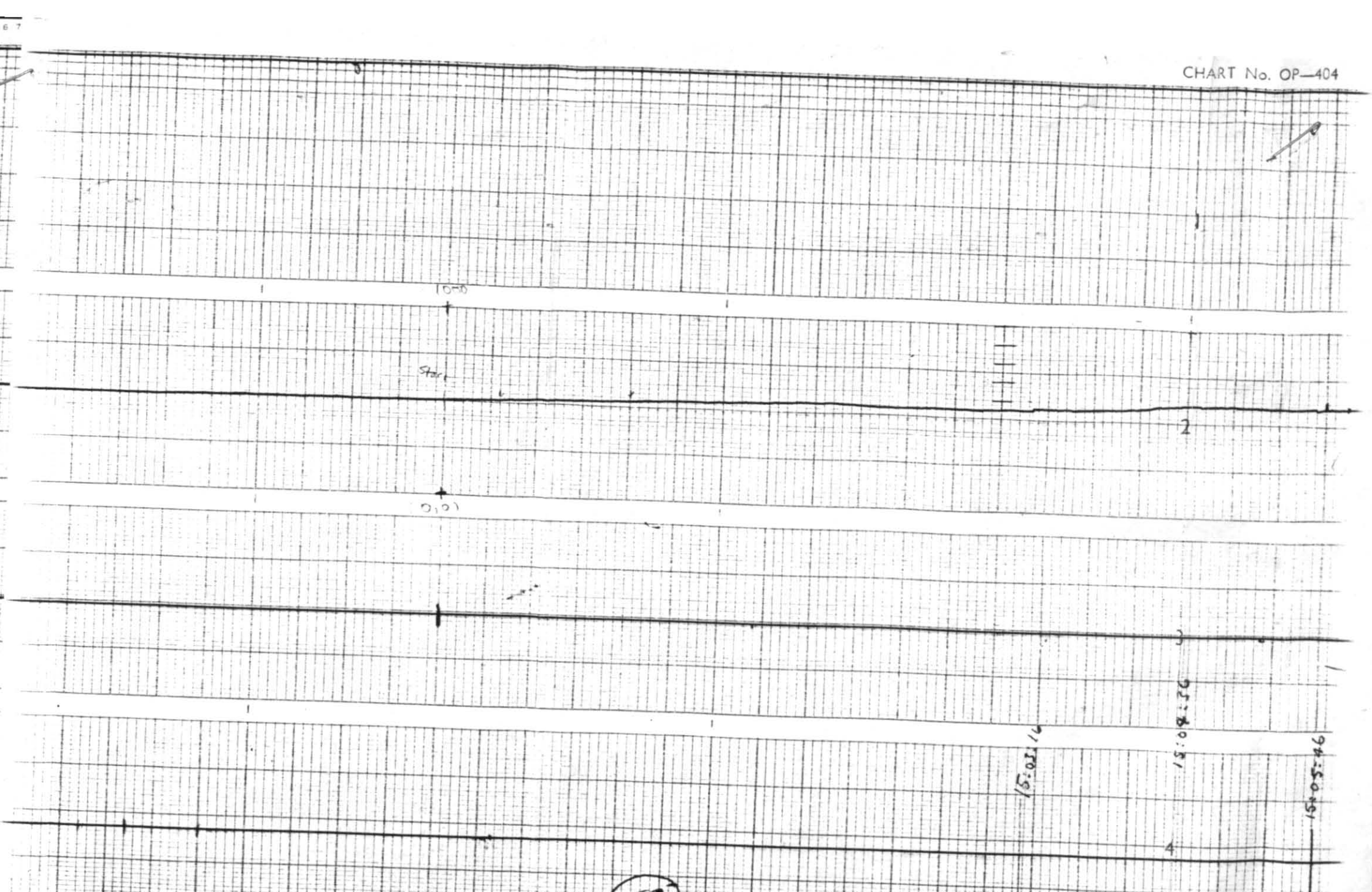
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ENVIRONMENTAL EQUIPMENTS

ENVIRONMENTAL EQUIPMENTS

ENVIRONMENTAL EQUIPMENTS

TESTING

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15.52.48

REST

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15.59.05

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16:09:24

16:09:24

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16:10:55

16:12:22

16:12:25

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Episode 4.2

Episode 4.3

Start of Episode 4.4

16:07 Mon 31 July 1978

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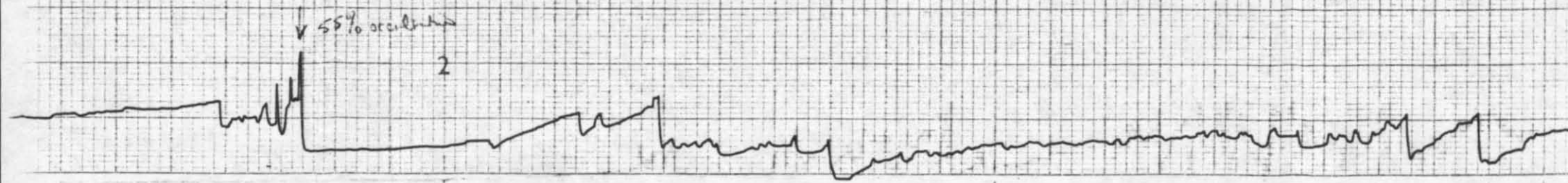
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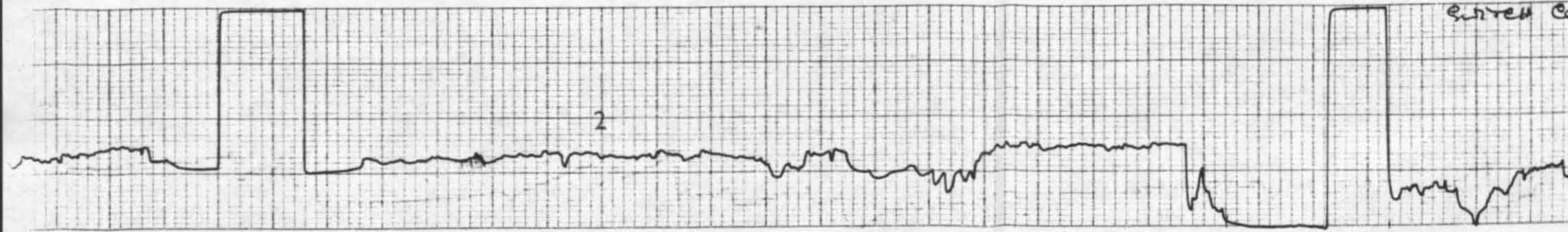
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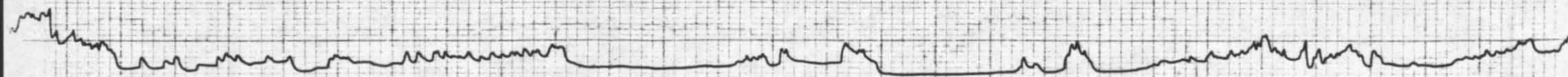
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16.19.52
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1621

SW 500

SWITCH OFF 80V
AT MAINS SUPPLY
TO CLEAR POSSIBLE
RECTIFIER REGULATOR
CONTACTS FAULT



CHANGE FROM
RECTIFIER MAINS (25V) TO
BATTERY (25V) SUPPLY TO
REGULATOR

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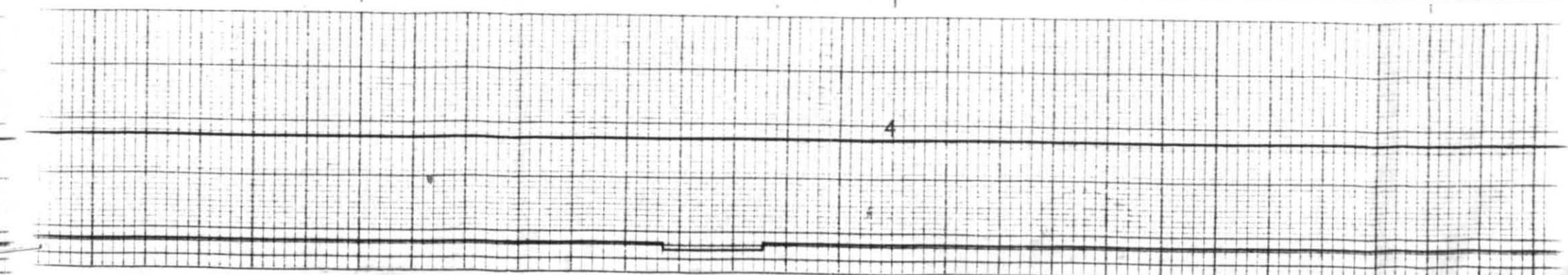
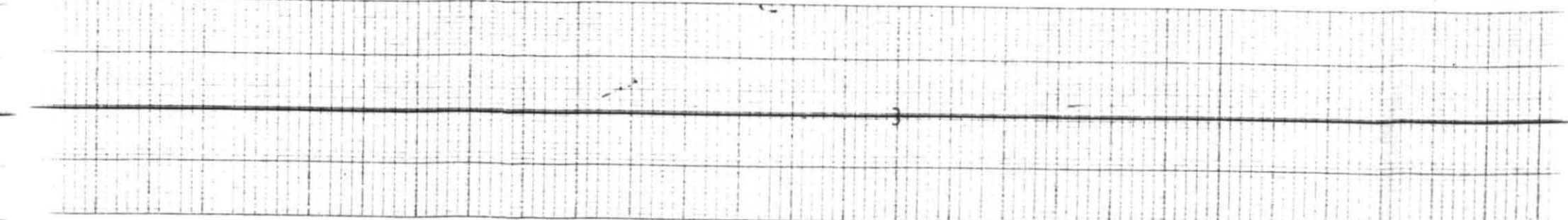
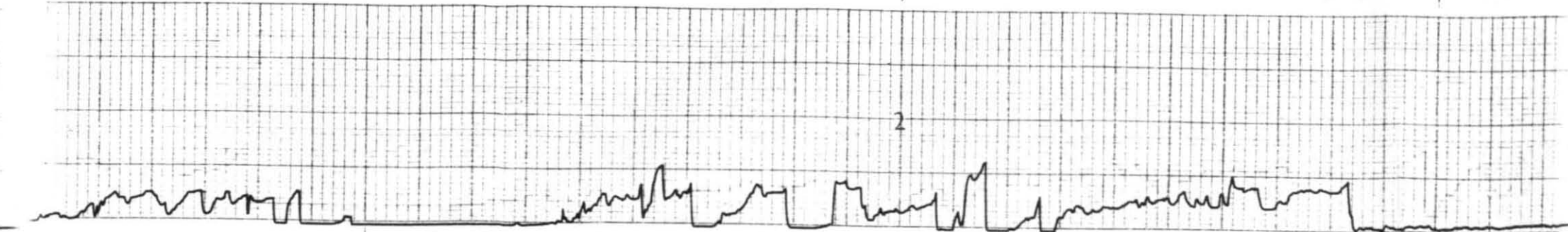
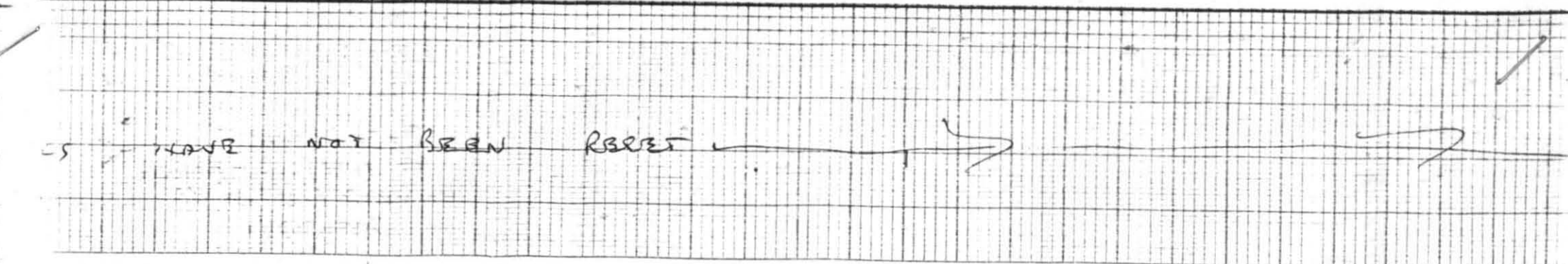
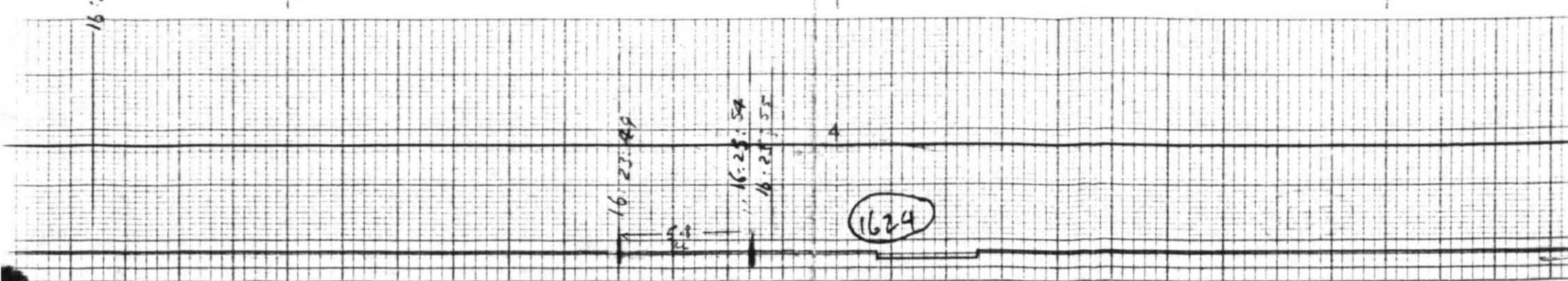
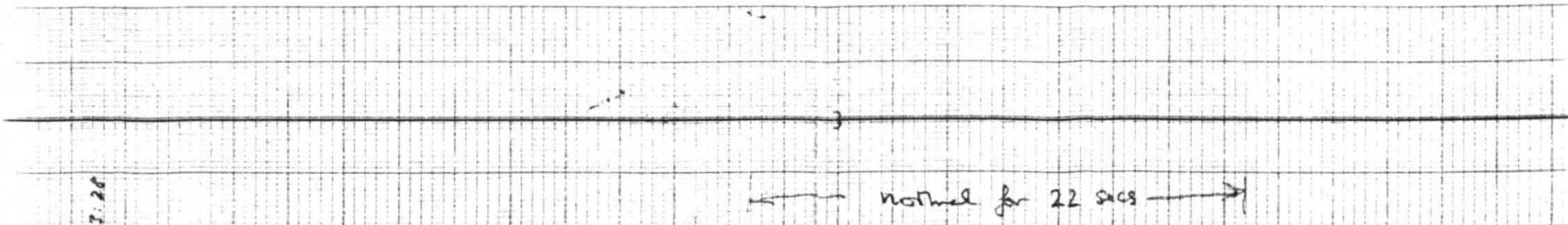
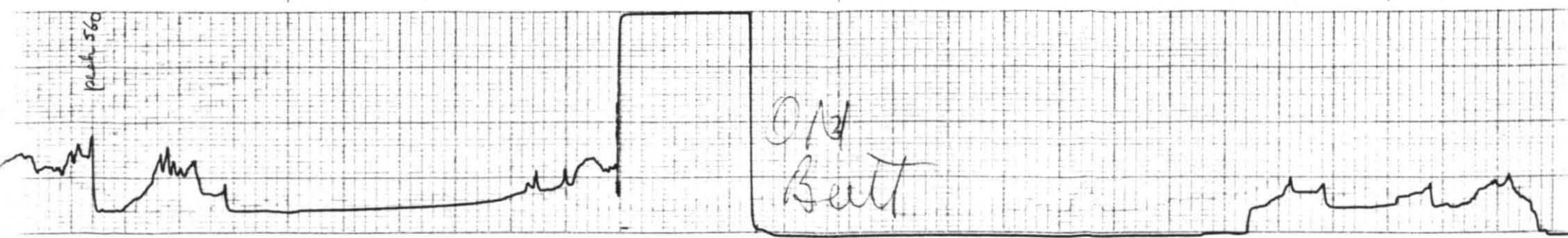
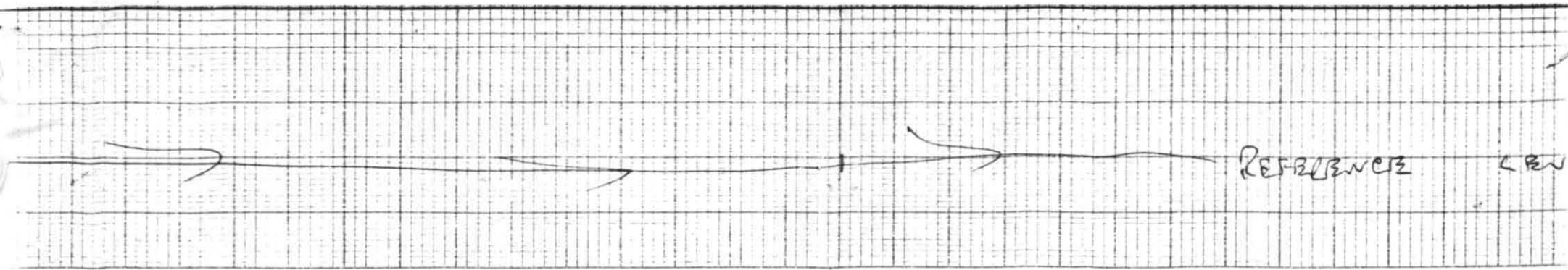
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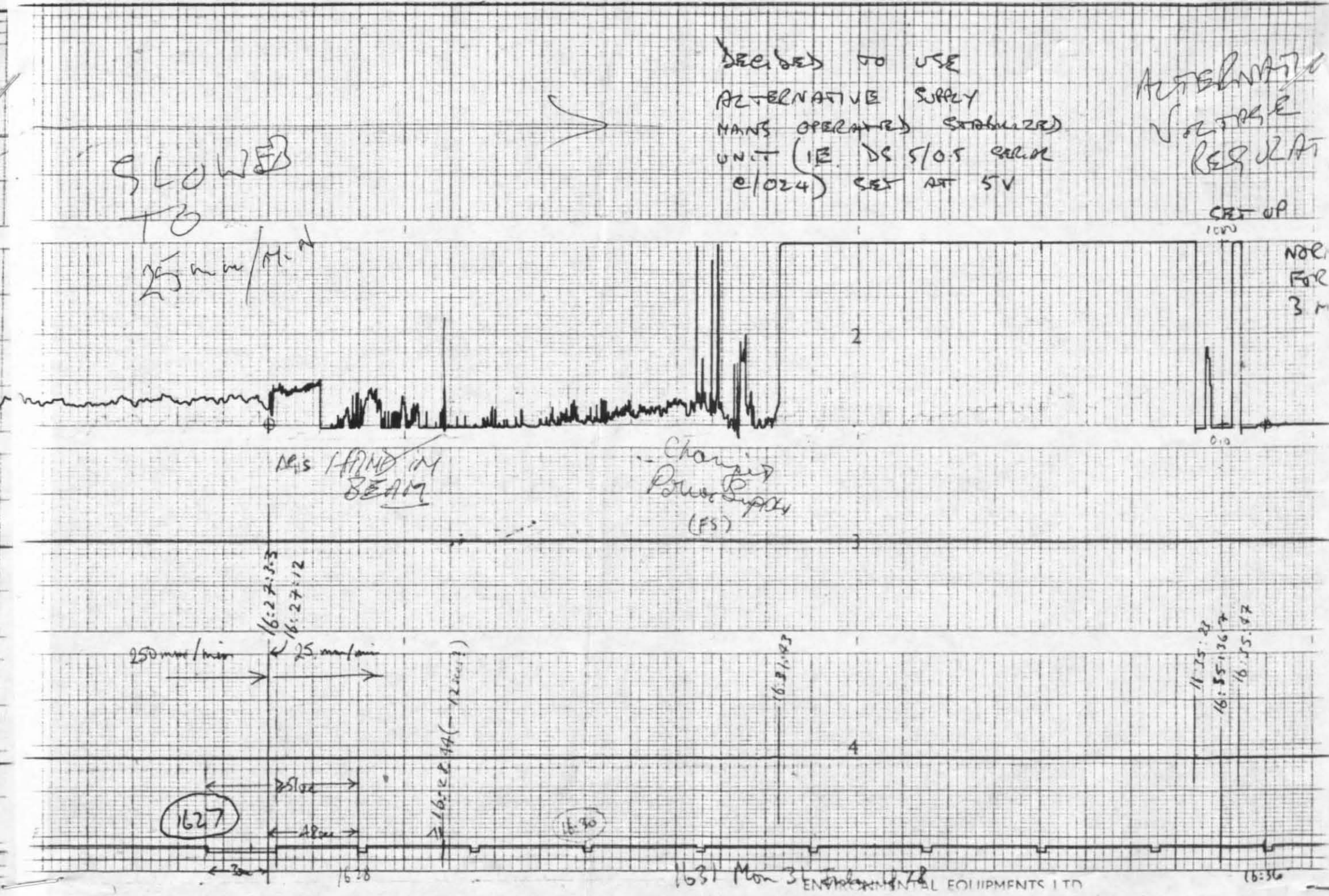
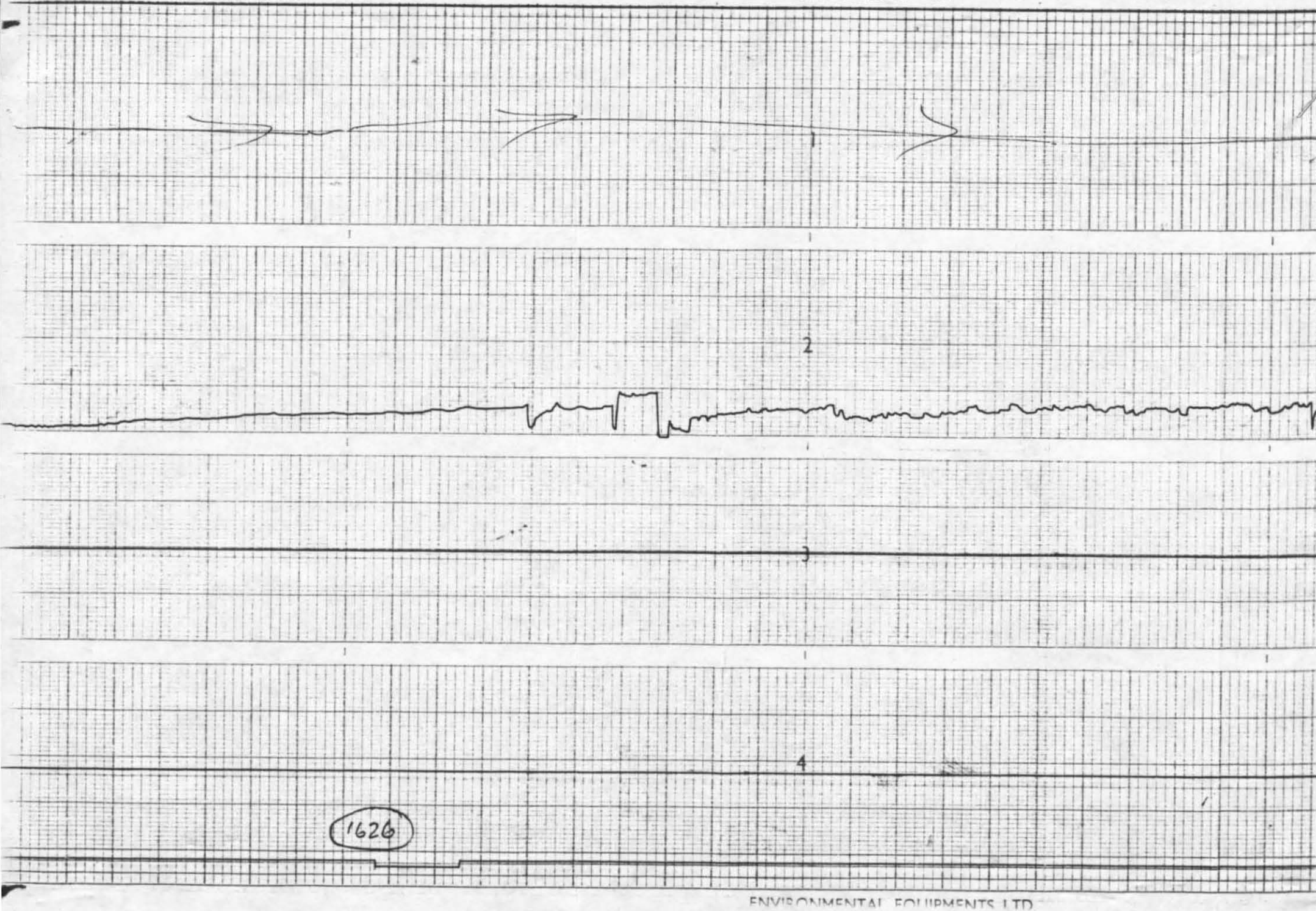
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1628





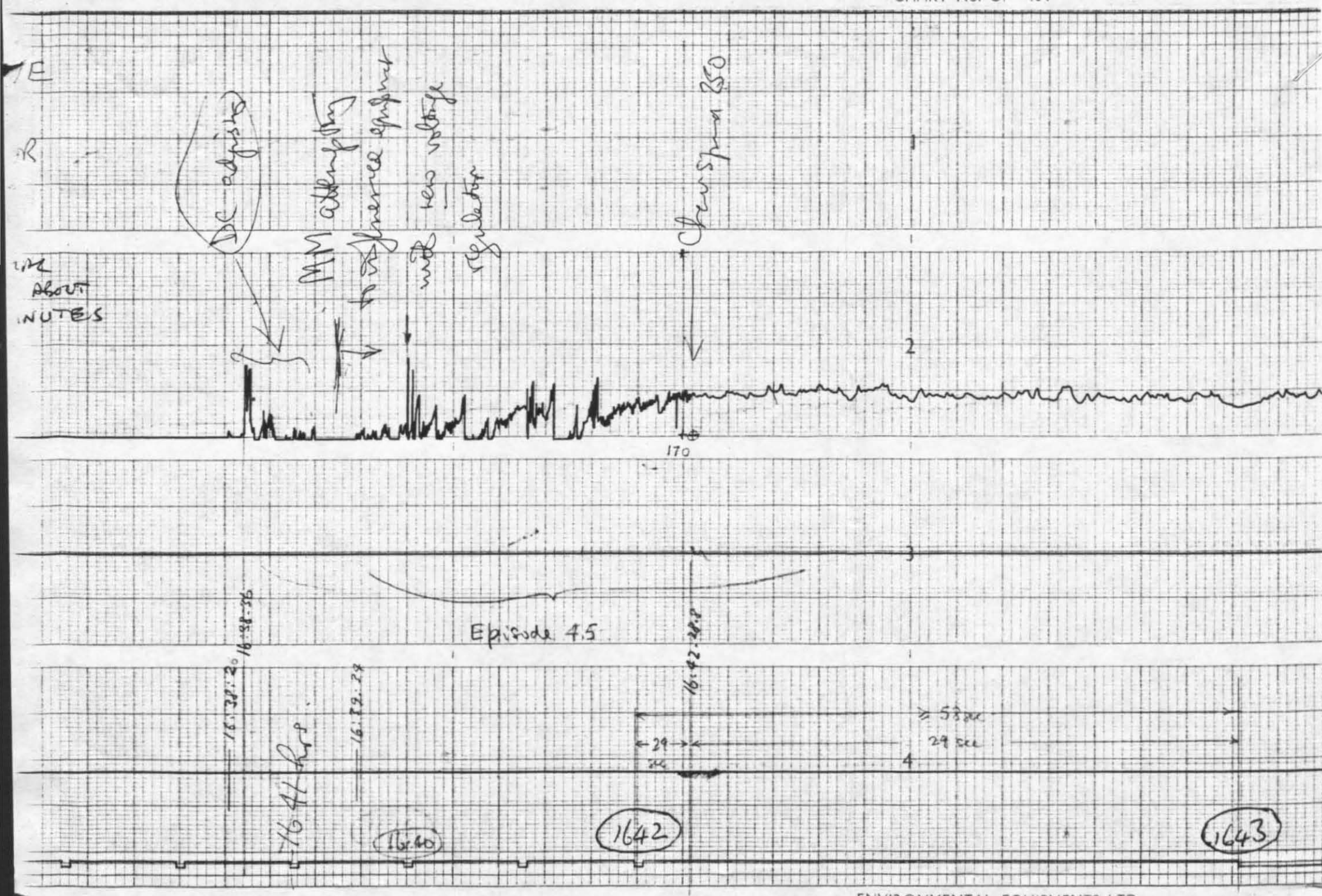
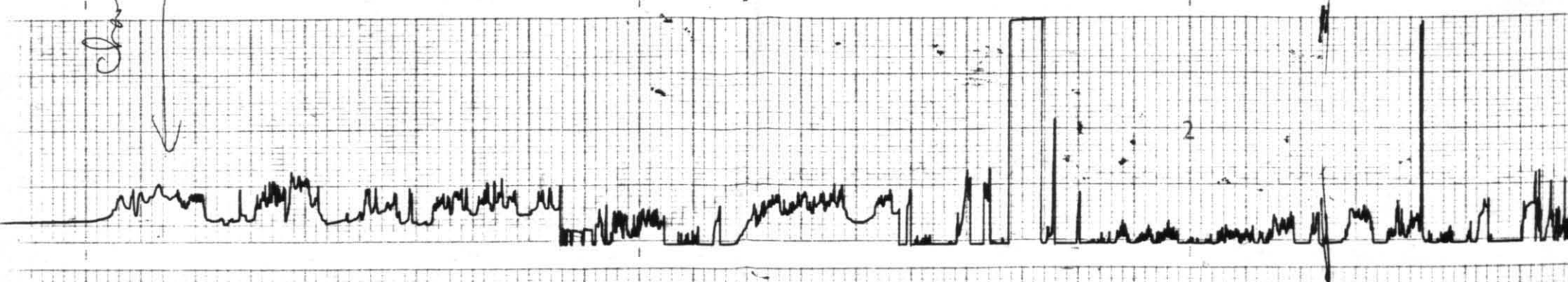


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RESET
179



16:40:23

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38 sec



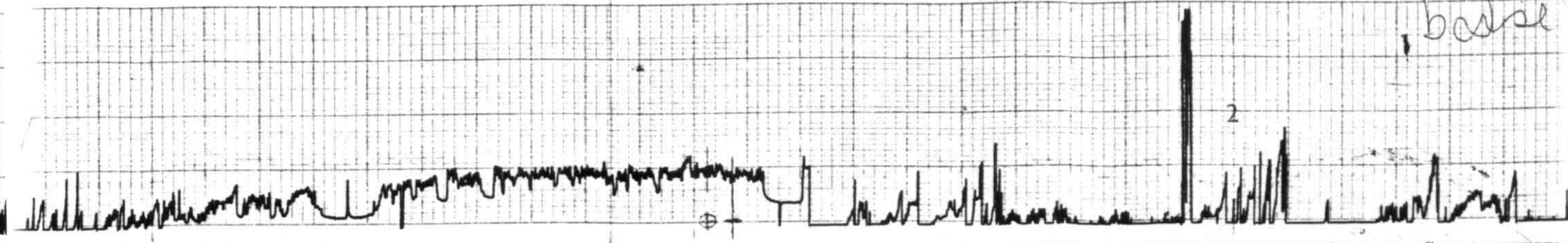
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at 16:59:48

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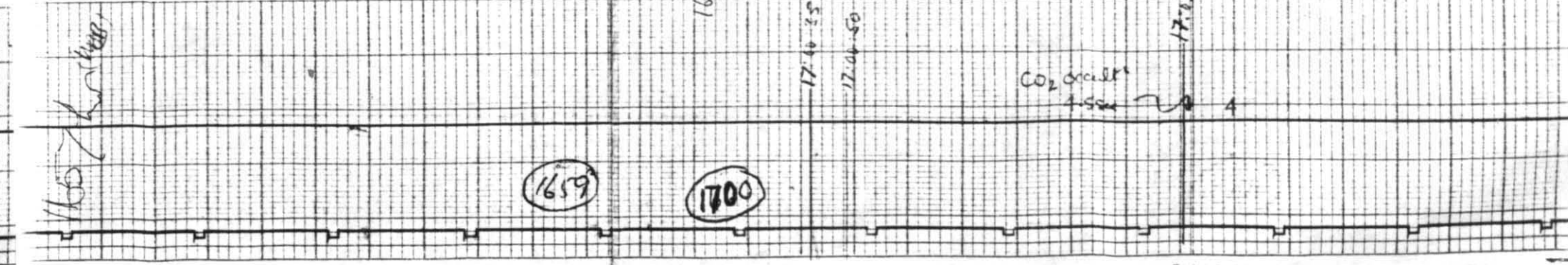
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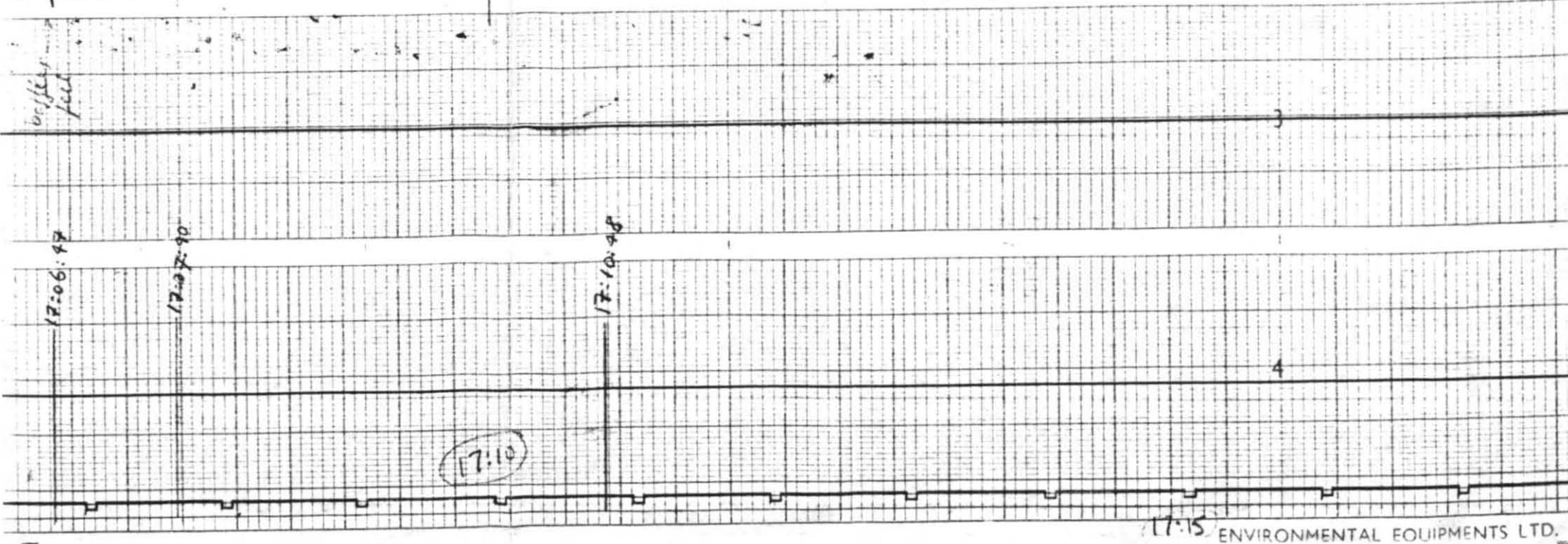
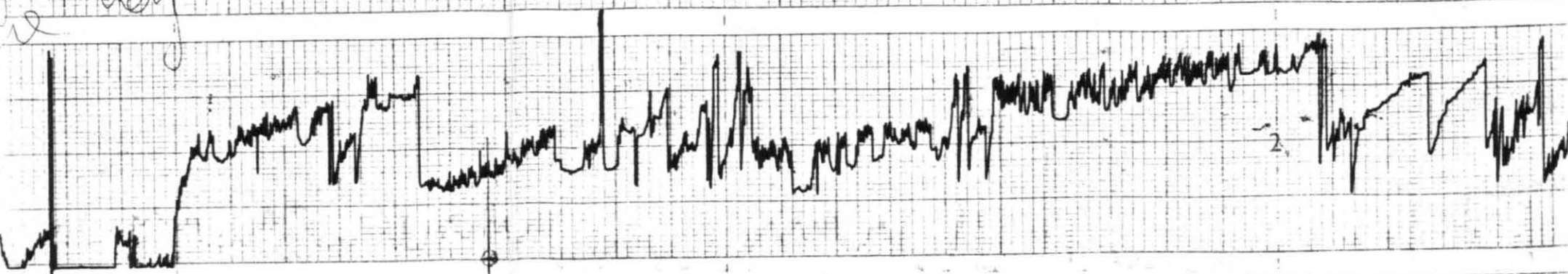
1700

1657



CO₂
TESTS
adv

DC test



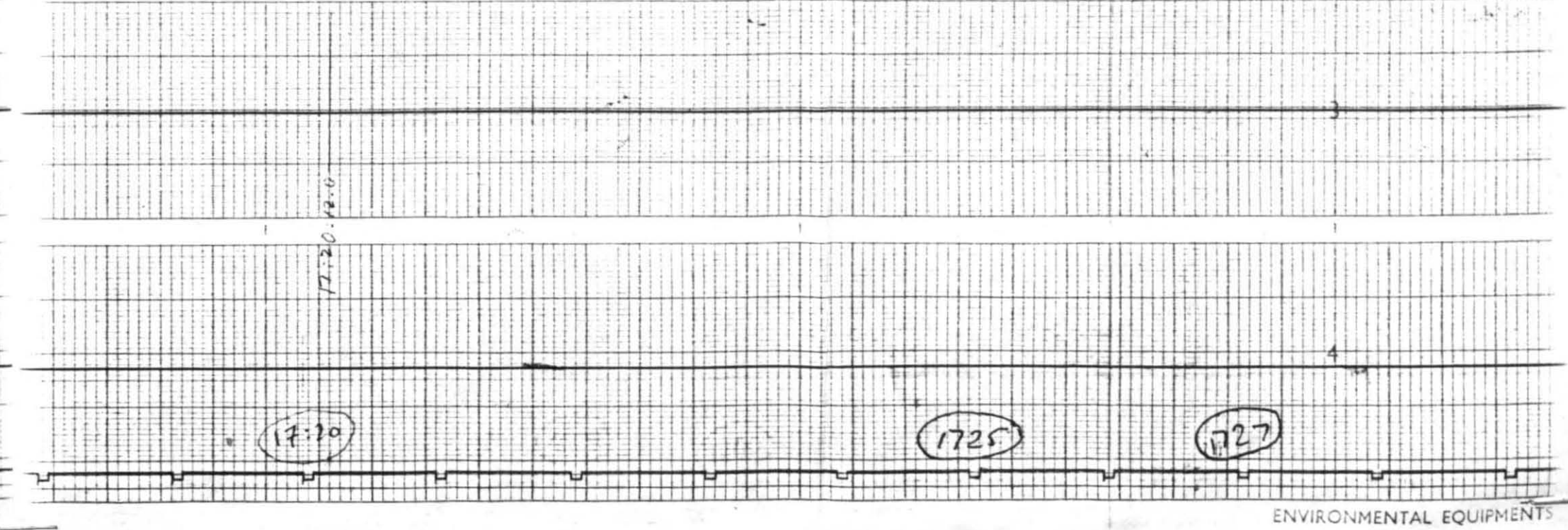
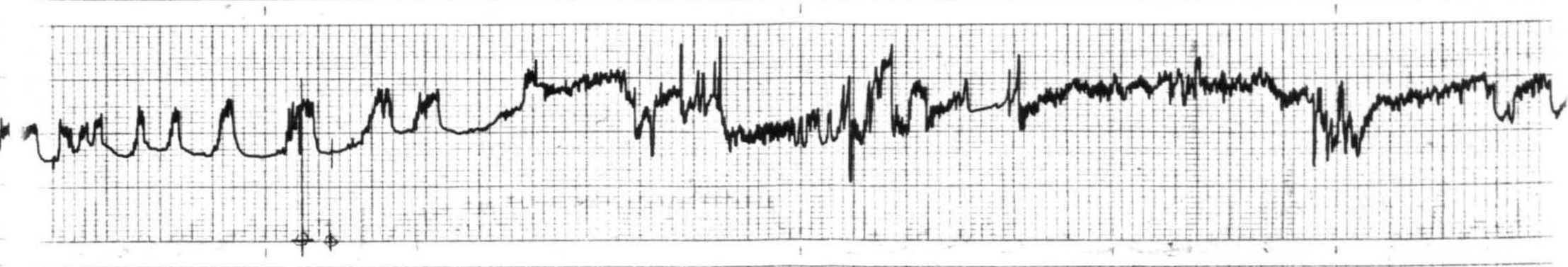
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17:15 ENVIRONMENTAL EQUIPMENTS LTD.



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ENVIRONMENTAL EQUIPMENTS

