

**FROM TEXT TO SELF: THE INTERPLAY OF
CRITICISM AND RESPONSE IN THE
HISTORY OF PARAPSYCHOLOGY**

Nancy L. Zingrone

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DEDICATION

To honour the memory of:

Janet K. and Anthony N. Zingrone

who raised me to believe that with persistence and
hard work anything is possible;

and

**Dorothy H. Pope, Charles Honorton,
and Marcello Truzzi**

who, in different ways and from different perspectives,
led me to value the work we all do;

and for

Robert Lyle Morris

to whom I owe a debt of intellectual and personal gratitude
that can never be repaid.

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ABSTRACT

The thesis examines the history of criticism and response in scientific parapsychology by bringing together the tools of history, rhetoric of science, and discursive psychology to examine texts generated in the heat of controversy. Previous analyses of the controversy at hand have been conducted by historians and sociologists of science, focusing on the professionalisation of the discipline, its philosophical and religious underpinnings, efforts of individual actors in the history of the community, and on the social forces which constrict and restrict both the internal substantive progress of the field and its external relations with the wider scientific community. The present study narrows the problem domain from the English-language literature — an extensive database of over 1500 books and articles — to the following: (1) a brief history of the development of the field in the U. K. and the U. S. that includes a survey of previous reviews of the controversy; (2) a specific controversy that extended over a 10-year period in the mid-twentieth century; and (3) a solicited debate on parapsychology with two target articles, 48 commentaries, and 3 responses published in *Behavioral and Brain Sciences*.

The thesis is comprised of eight chapters. In Chapter 1, the goals and methods of the thesis are described, previous considerations of controversy and closure in science studies are reviewed, the notion of closure is discussed, and the thesis content is described. In Chapter 2, a brief history of the field is provided which emphasises the broad structure and content of the field rather than specific methodology, results, or theory. In Chapter 3, previous reviews of the controversy are examined to provide a sense of the controversy terrain and to examine the extent to which what Gilbert and Mulkay (1984) have called “contingent” and “empiricist” repertoires have been used in criticisms and response. In Chapter 4, case studies on parapsychology that appeared in the science studies literature are reviewed. Rhetoric of science is introduced as a domain from which analytic tools for the present research are drawn. In Chapter 5, a case study tests the hypothesis that differences in style and structure in the two volumes that bracket the most important controversy in the history of American experimental parapsychology may have contributed to the scope and persistence of the controversy. The controversy extended from 1934 to 1944, beginning with the publication of the monograph *Extra-sensory Perception* (Rhine, 1934) and ending with the publication of *Extrasensory Perception After Sixty Years* (Pratt, Rhine, Smith, Stuart & Greenwood, 1940). In Chapter 6, I justify a turn towards the methodology of discourse analysis by reviewing both the antecedents of modern discursive psychology, and methods that are currently in use. I also review Mulkay’s (1985) *The Word and The World* as a prelude to the case study in the next chapter. In Chapter 7, a subset of the methods available in discourse analysis, particularly the concepts of formulation, category entitlement and footing are used to analyse a target article, 48 commentaries and two responses to the commentaries that center on James Alcock’s contentions that parapsychology is the search for the soul and that dualism as a philosophical position is incommensurate with science. I show how Alcock’s use of the contingent repertoire in characterising science

practise in parapsychology undermines his authority as a scientific interlocutor, and obscures, to some extent, the substantive message he intended his target article to carry. Chapter 8 concludes the thesis by restating the findings of the three methods used, examining the limited use of the methods in this thesis and outlining what a more extended study with the same and/or related materials would look like, while describing other potentially fruitful research that might be done. How these methods should and may contribute to science practise in parapsychology is also discussed with a particular emphasis on the multidisciplinary nature of the discipline and the need for a more complete reflexivity.

DECLARATION

This thesis has been composed by myself and the work is my own.

The following papers based on my thesis research have been published or presented at conferences:

Zingrone, N. L. (in press). Complicating the conversation: Rhetoric, substance and controversy in parapsychology. *Journal of Parapsychology*. (Revised version of convention paper.)

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Nancy L. Zingrone

CHAPTER ONE

INTRODUCTION

In this Chapter I will attempt to characterise the problem domain — scientific parapsychology — within science as a whole. Close examination of controversy in the sociology of scientific knowledge, and in the wider set of disciplines that make up science studies has allowed us to see in base relief the complicated dance of the substantive and the social that occur in the establishment of both ‘facts’ and disciplines in mainstream and marginal sciences. As will be seen in Chapter 4, some analysts have focused on parapsychology, as, at best, a hotly-contested marginal science, and at worst, a pseudoscience. There is merit in the gaze of ‘outsiders’ in this type of work as will be seen in the review of methods below. But, I would argue, there is also merit in an ‘insider’ attempting to adopt a reflexive stance towards the persistence of controversy in parapsychology. Further, such a stance should be an integral part of our discipline, a legitimate method for ‘doing’ parapsychology, if for no other reason than that the field — in both a substantive and a social sense — is situated precariously, even ephemerally, on the map of science. This thesis will attempt not only to establish the usefulness of a science studies-based approach in general, but also to illustrate the contribution some of its specific methods may make in a reconfigured parapsychology.

In this chapter then, I will first characterise scientific parapsychology as a profession, then provide some background into the study of controversy from the perspectives of science studies. I will also describe the structure of this document.

It will become apparent as I move along this route that I have chosen two less-travelled paths. Firstly, as mentioned above, I believe that work from this perspective should be considered parapsychological. Parapsychology has largely been an experimental, laboratory-based interdisciplinary endeavour, relying mainly on the conventional methods of social and cognitive psychology with forays into physics and engineering. To a lesser extent it has also included essentially social psychological and anthropological field investigations as well as a form of survey-based differential psychology. Running through these investigatory strands has been a kind of binary focus on the reported phenomena itself, and on the psychology of those who manifest it in the laboratory or report it in life.

Parapsychology has not heretofore formally attempted to move its focus beyond its own cognitive boundaries and examine the social factors that determine its contested status in science. Whilst the field has welcomed in its midst sociologists and anthropologists who examined such social factors by participant observation and other methods, no ‘insider’ before myself has argued that such work is properly part of parapsychology’s remit.

Secondly, most research reports in psychology have been organised in an expository structure that is common in the natural and physical sciences, that is, in a textual progression from introduction to method to results to discussion. Parapsychology research reports have also conformed to these conventions. Whilst I have analysed the putative impact of departure from such structure on the ‘hearability’ of the details and interpretations of research results in parapsychology in Chapter 5, I have chosen to organise my own thesis along lines more familiar to an historian or rhetorician of science. It seems to me that a less rigid style of presentation is important here both because it is a more comfortable method for incorporating historical and sociological detail into the thesis, and because I have conceived of this project as one that is, although empirical, self-consciously interpretative and speculative.

Parapsychology as a Science

Scientific parapsychology has followed a trajectory of professionalisation similar to that of other more conventional fields, albeit with less ‘success’. That is, the discipline developed from the research interests of individuals who operated in isolation, to loosely-organised groups of educated amateurs (which have not entirely disappeared), to the current small community of psychologists, physicists, and other academics and scientists. Over its history, scientific parapsychology has sought to focus attention on a class of anomalous phenomena, the experience and exploration of which are complicated by misattribution and misperception as well as by a variety of spiritual, religious and cultural meanings attached both to the phenomena and to the field itself. Parapsychology today is only partially professionalised, its integration into mainstream science complicated by multiple understandings of its core, its canon, and its goals.

In other disciplines, the scientific study of complex problem areas have borne more fruit whilst following a similar path towards professionalisation. Such natural sciences as geology have moved from descriptive, exploratory, and even amateur avocational interest in experimentation, through the efforts of gentlemen scholars, clerics and university professors in other disciplines, to an organised international community of well-trained and well-paid scientists and scholars, who share common educational experiences and similar careers within well-established and well-funded research institutes and university departments (e.g., Morrell & Thackray, 1981; Rudwick, 1988; Shapin, 1991). The similarities between the rise of these disciplines and the history of scientific parapsychology are many (Mauskopf, 1989) but the levels of professionalisation that have been reached are very different. Given that these similarities are structural, procedural *and* methodological, the differences between the development of scientific parapsychology and that of more ‘normal’ sciences seem profound.

An international scientific society for parapsychological research was founded in 1957.¹ This society, the Parapsychological Association (PA), was meant to move self-consciously beyond the mix of amateurs and professionals who participate in such 19th-century style organisations as the Society for Psychical Research (SPR) founded in London in 1882 (Gauld, 1968), and the American Society for Psychical Research founded in Boston in 1884 (Berger, 1985).² After 48 years of existence, however, the membership of the Parapsychological Association remains small and essentially segregated from mainstream academe, the affiliation to the American Association for

¹ International scientific bodies for most sciences were founded from the 17th to the 19th centuries, by comparison.

² As will be seen in Chapter 2, the earliest version of the ASPR was meant to be largely a scientific society, a status it did not attain. Today, in contrast to the SPR and the ASPR, the Parapsychological Association has stringent membership requirements, reserving full participation only for those with advanced degrees and a track record of publishing serious scholarly or scientific work in the field. An argument can be made that further professionalisation is needed, however, because a PhD is *not* required for full membership, and it is possible to rise to the highest echelons of the organisation with a mid-level degree coupled with publications and research experience. My own career is an example of this.

the Advancement of Science in 1969 notwithstanding (Dean, 1994).³ As evidence of this marginality within the wider scientific community, the percentage of PA membership employed even part-time in paid academic or scientific positions in parapsychology is easily less than 10%. Although the number of academic units devoted to the scientific study of paranormal belief and experience in Great Britain have increased in recent years,⁴ only one such unit exists in the United States.⁵ If one adds to that number, the universities with individual faculty members who regularly supervise students at the undergraduate and postgraduate level or who conduct occasional research on paranormal phenomena, the total number of scientific parapsychology research and teaching sites increases perhaps to a dozen or so.⁶ Very few private institutions devoted solely to scientific research in parapsychology exist in the entire Anglo-American world.⁷

³ As of May 2005, there 207 members and 35 regular and student affiliates drawn from 31 countries. Twenty years ago in 1986, there were 274 members drawn from 26 countries. (There were no affiliate categories at that time.) Although the percent of PhDs and MDs amongst the current membership (71%) is greater today than it was in 1986 (59%), the overall number of full and associate members has declined by 25%.

⁴ There is the Koestler Parapsychology Unit here at Edinburgh; the Anomalies Study Unit at Goldsmith's College at the University of London; the Perrott-Warrick Research Units at Universities of Northampton and Hertfordshire; a parapsychology and transpersonal psychology unit at Coventry University, and a research consortium comprised of faculty from Liverpool Hope and Liverpool John Moores Universities.

⁵ The only university-based unit remaining in the United States is the Division of Perceptual Studies at the University of Virginia. Unlike the departments in the U.K., the Division does not supervise graduate students and thus has a significantly different potential for influence on the future of the field than do the academic units in the United Kingdom. In addition to which the Division has a time limit: the founding endowment, according to its terms, may be diverted to other uses in the university in 2022.

⁶ Regular supervision of parapsychologically-relevant theses only occurs in the United States at such non-traditional graduate schools as the Institute for Transpersonal Psychology and Saybrook Graduate School of Psychology in California. In Australia this occurs in the Department of Psychology at the University of Adelaide. Outside the English-speaking world post-graduate supervision and research in the field have occurred at the University of Amsterdam, Freiburg University, the University of Gothenburg, the Catholic Pontifical University of São Paulo and at the University of São Paulo.

⁷ I am personally unaware of such research centres in Great Britain although there may be some. In the United States, there is currently only the Rhine Research Center in North Carolina, and the Institute for Noetic Sciences in California, both of which focus more heavily on membership activities for the general public than on research. One-person laboratories are not plentiful either. At the moment in the United States there are only two or three that regularly produce research. In the non-English speaking world, research centres exist only in Argentina, Brazil, Holland and Japan.

Dedicated funding sources are also limited. Only the Perrott-Warrick Fund at Cambridge University and the Society for Psychical Research in Great Britain, the Parapsychology Foundation in the United States, and the Fundação Bial in Portugal regularly fund students and researchers in the field. The Institut für Grenzgebiete der Psychologie und Psychohygiene (IGPP) in Freiburg im Breslau provided similar funding for a decade (my own project amongst them), but now only funds a few research sites in Germany. Whilst some parapsychologists have managed to obtain grant money from conventional sources,⁸ it is, in fact, highly unusual for ‘normal’ funding agencies to support the field.⁹

The amount of financial support that *is* available is wholly inadequate to the task at hand, in any case. In 1982, on the occasion of the centenary of the Society for Psychical Research in London, a Dutch psychologist Sybo Schouten (1983), then at the University of Utrecht, roughly calculated the amount of money and person-hours spent on scientific parapsychology since 1882. He estimated that the budget of experimental psychology in the United States for two months equalled the entire expenditure in psychical research and parapsychology over that 100-year period. Using Schouten’s metric,¹⁰ in the twenty-two years since 1982, the field’s expenditures on research have

⁸ The Koestler Parapsychology Unit at Edinburgh and the Perrott-Warrick Units at the University of Hertfordshire and at Northampton University are particularly successful at obtaining conventional grants. Very few centres of research in the U.S. have been able to obtain conventional grants on a regular basis, with the possible exceptions of the Princeton Engineering Anomalies Laboratory (in the process of closing after nearly 30 years of operation), and the remote viewing unit at Science Applications International Corporation (SAIC) in Palo Alto, California, which for many years was funded by the U.S. Department of Defense but which, to my knowledge, no longer conducts parapsychological research.

⁹ If one expands the definition of parapsychology to include energy medicine, alternative healing and so on, the funding situation is somewhat different, but my focus here is on what I believe constitutes the “core” of the field, that is, the laboratory and field investigation of seemingly psychic phenomena.

¹⁰ If Schouten’s estimates were correct and 2 months of American experimental psychology’s funding and hours of labour is equivalent to 100 years of our funding and hours of labour, then it is possible to estimate the ratio for the twenty years that have elapsed since Schouten published his speculation. This can be done by setting up a simple equivalence ratio and then solving it by converting 100 years into months (1200) and 22 years into months (264). The formula becomes $2/1200 \sim X/264$. In this equation X solves to .44 of a month. Using 30.417 as the average number of days in a month, .44 of a month = 13.38 days. Some have argued that the presence of the IGPP and the Fundação Bial in the mix has increased the level of funding overall in recent years, but over the same period in which these funding sources became important, the Department of Defense in the U.S. ceased to fund the field. Consequently, in my opinion, using Schouten’s metric is probably a conservative estimate of the current funding situation.

added a little more than an additional dozen days of ‘normal’ funding to that total. Whilst scientific parapsychologists, myself amongst them, like to say that we have made an enormous amount of scientific progress in just under two and a half of Schouten’s ‘months’, the barriers to a ‘normal’ rate of progress for us often appear insurmountable.

A variety of factors other than funding complicate the status of parapsychology within science and the academy. Many parapsychologists, even those who have achieved academic positions, have complained of various forms of overt and covert discrimination (Hess, 1992). Examples have also been described of initial acceptance into academic circles followed by a collapsing of these opportunities through mounting opposition of faculty and administration colleagues, or through the death of a single individual with enough power to provide protection for the parapsychologists on campus (McClenon, 1985).

Invoking the controversial nature of the underlying claimed phenomena and of the scientific study of that phenomena, or indeed the controversial nature of daring to espouse a serious scientific interest in the claimed phenomena in mainstream science, is not enough to explain why parapsychology has neither been integrated into mainstream science nor faded away. As will be seen in later chapters, parapsychologists have worked hard to structure the field as a science, to adopt appropriate methodologies, to attack systematically the underlying questions of the field, and to answer criticisms raised by further modifications in science practise and theory, all done in the hope of carrying the discipline across the boundary into mainstream science, but without the intended result.

A variety of questions may be raised about this lack of ‘progress’ towards integration into mainstream science. Some have focused on data and results, asking whether the field has amassed sufficient amounts of evidence to bring about such ‘progress’ (e.g., Child, 1987). This thesis will not do that. The reality of the purported phenomena and the persuasive quality of the evidence — indeed, what counts as evidence — will not be debated here. The appropriateness of current theory — or even whether such ‘theory’ exists — will not be discussed here. Nor will I review the accuracy or applicability of the criticisms that have been raised. Others — whose work I will review in Chapter 3 — have attempted to do this, and whilst it will be important to have a sense of these materials so as to contextualise the specific controversies on

which, and the specific individuals on whom I have focused, I will not emphasise accuracy or applicability here.

Rather than reviewing the cognitive substance of the field, I am interested instead in both parapsychology's social surround and in the discourse of controversy by which and through which parapsychology attempts to negotiate its place within science. In the sociology of scientific knowledge in particular, and in science studies in general, it has been argued that scientific disciplines are *situated* within the scientific mainstream on the basis of a variety of contested factors, amongst which the substantive is only one. This is as true of disciplines that are seen as unproblematically 'scientific' as it is of sciences — such as parapsychology — that are seen as, at best, problematically scientific and, at worst, as pseudoscientific. The study of controversy in the former sort of sciences has raised issues important to an understanding of science as a social institution and of science-as-practise. It is my belief that the study of controversy in disciplines that lie beyond the margin — as parapsychology most surely does — can be even more illuminating.

Science Studies

Science studies as a set of related disciplines can trace its beginnings back to the founding of the Edinburgh University Science Studies Unit in 1966 by David Edge, its first director.¹¹ In its early days, science studies analysts saw themselves as doing the sociology of scientific knowledge (SSK). Hess (1997, p. 52)¹² has contended that the sociology of knowledge in general, and by inference, SSK in particular, arose in

¹¹ David Edge died in January of 2002. Details of his life and his profound influence on the sociology of scientific knowledge (SSK) and science and technology studies (STS) are available in a recent obituary written by his colleague at the Edinburgh Science Studies Unit, David Bloor (2003).

¹² I rely on Hess at various points in this chapter, not only because his is one of the few advanced textbooks that attempts to cover the wide range of science studies, but also because, in this textbook, Hess dismisses both rhetoric of science and psychology of science as not useful to science studies in general. One of my motivations for using the tools of rhetoric of science and for attempting to extend the analytical repertoire available to psychology of science by including discursive psychology comes from my sense that Hess's dismissal of the rhetoric of science was unjustified and that his dismissal of the psychology of science ought to be.

opposition to both philosophy of science in its many varieties,¹³ and to the sociology of science best exemplified by the work of Robert K. Merton (e.g., 1973) in which science was viewed as a ‘relatively just institution that worked well’ (Hess, 1997, p. 53).¹⁴ In its infancy, Hess argued, the sociology of knowledge being developed at Edinburgh did not influence the wider sociology of science in a significant way, primarily because Mertonian work on institutional structure, norms, productivity, and other variables was carried out mainly in the United States (pp. 52-80), whilst the version of the discipline which began to look at the ‘theories, methods, design choices, and other technical aspects’ (p. 81) that constituted the content of science and its ‘constructed’ nature (p. 82) developed largely in Great Britain and in Europe.

The Edinburgh approach came to be known as ‘the strong programme’ in the sociology of knowledge and was exemplified by the works of David Edge, David Bloor, and Barry Barnes, amongst others (p. 86).¹⁵ The strong programme emphasised the four tenets of causality, impartiality, symmetry, and reflexivity. Hess described causality as the search for an understanding of ‘beliefs or states of knowledge’, impartiality as the taking of an agnostic position towards ‘truth or falsity, rationality or irrationality, or success or failure of knowledge’, symmetry as the importance of using the same explanatory principles for ‘true and false beliefs’, and reflexivity as the understanding that ‘the same explanations that apply to science would also apply to the social studies of science’ (p. 86-87). The Edinburgh school also developed an approach called

¹³ Hess (1997) listed a number of positions within philosophy of science which science studies found lacking. In the beginning of the influence of the Edinburgh School, these would have included the positivistic philosophy of Rudolph Carnap (e.g., 1995) (Hess, 1997, pp. 13-14), the work of Karl Popper which focused on the falsification of hypotheses as the principle activity of science (e.g., 1959) (Hess, 1997, pp. 19-22), and Thomas Kuhn’s idea of paradigm shift (e.g., 1970) which characterized science as moving through three stages: (1) growing controversy over the presence of anomalies under an existing theory; (2) paradigm shift in which a new theory was proposed and accepted into which the anomalies could be incorporated, and (3) a period of normal science in which the new theory was explored experimentally, during which time more anomalies might be found that, if left unresolved, would ultimately force a reiteration of the three-stage cycle (Hess, 1997, pp. 22-27).

¹⁴ The roots of science studies, in particular of the social constructivist variety, also lay in the seminal work by Peter Berger and Thomas Luckman (1966), *The Social Construction of Reality*.

¹⁵ Amongst the canonical texts of this approach are Barnes (1974), Barnes and Edge (1982), Bloor (1976/1991), and Barnes and Shapin (1979).

‘interests analysis’ exemplified by Barnes and his colleagues’ largely historical studies of science (Barnes, 1977; Barnes & Shapin, 1979) which examined the influence of such macrosociological forces as class on the content of science and on the course of scientific controversy (e.g., Barnes & MacKenzie, 1979).¹⁶

Another school that developed within the SSK tradition in the U. K. was the ‘Bath School’. As Hess rightly notes (Hess, 1997, pp. 94-95), the ‘Bath School’ was largely the work of Harry Collins and his student, Trevor Pinch, who specialised in empirical, observational, microsocial studies of the construction of scientific content under conditions of controversy.¹⁷

As the British and European brand of science studies began to flourish in the 1980s and 1990s, it became the dominant force in the remapping of the sociology of science. What emerged, on both sides of the Atlantic and elsewhere, was a largely social constructivist enterprise which both flowed from, and to some extent misunderstood David Edge’s original intent.¹⁸ The ‘strong programme’ of the Edinburgh school and the reflexive methodology of the Bath school became synonymous with social constructionism which, in turn, was taken by many to mean a negation of the power of science to describe nature, and even, to mean the setting aside or denial of the notion that the natural world is ‘real’ and ‘out there’ in some objective sense. Whilst Latour and others have disputed this misinterpretation of their work (e.g., Latour, 1999, pp. 1-23), David Edge put it in the strongest terms in the invited address he gave on the occasion of the 25th anniversary of the founding of the Society for Social Studies of Science (4S):

There is one STS (Science and Technology Studies) idea that seems particularly difficult to communicate — namely, the idea that ‘social

¹⁶ Thomas Gieryn has also contributed to this literature in the context of his articles on boundary-work (e.g., Gieryn, 1983), as has Andrew Pickering (e.g., 1982), who in more recent years has shifted his focus largely to science practise (e.g., Pickering, 1992, 1995, 1999). This is, due to space limitations, a decidedly simplified description of the varieties of science studies.

¹⁷ The canonical texts for this approach are Collins (1974, 1975, 1981, 1982a, 1982b, 1982c, 1983, 1985), and Collins & Pinch (1979, 1982).

¹⁸ The canonical texts of this approach include the writings of Michael Lynch (e.g., 1985), Steven Woolgar (e.g., 1976, 1983, 1988), Bruno Latour (e.g., 1983, 1987, Latour & Woolgar, 1979, 1982), Michel Callon (e.g., 1986), Karin Knorr-Cetina and others (e.g., 1981, Knorr-Cetina & Mulkay, 1983).

constructs'; 'social institutions'; far from being 'soft' and 'pliable,' are as hard as nails. To claim that science is 'social to its core' is not to deny the robust, sharp reality of its facts and theories. It is not to say that 'anything goes' — that claims can be established by ungrounded fiat — quite the reverse. And, of course, to advance social 'explanations' and analysis of scientific work is not to demean and discredit 'science.' (Edge, 2003, p. 161)

Not only have scientists found science studies — especially the strong programme — alarming (e.g., Gross & Levitt, 1997; Hacking, 2000; Koertge, 2000), but more recent generations of science analysts have made the strong programme even stronger, especially those trained in literary criticism. For example, the journal *Configurations*, which focuses on science as literature, often publishes articles in which the 'reality' of the natural world is questioned quite seriously (e.g., Ashmore, Edwards & Potter, 1994), as do journals which focus on rhetoric (e.g., Graves, 1995). A great deal of literature has also appeared in 'mainstream' science studies journals in which the strong programme and its variants are taken at face value. Latour himself, whatever his protestations to the contrary, has contributed to this line of work (e.g., Latour & Woolgar, 1979) as has Knorr-Cetina (1981).¹⁹

Controversy in Science

For some decades in all the various subdisciplines of science studies, the deep examination of controversy has been a growth industry. At one point, in the pre-history of this collection of subdisciplines in science studies, controversy was believed to be an aberrant moment on the way to some grand consensus. This consensus was then

¹⁹ A good early review of constructivism was published by Tibbetts (1986). Examples of the more recent scholarly descriptions of the problem can be found in MacKenzie (1999a, 1999b), Koertge (1999) and Edge (1999), for example. The propensity for even a hint of constructionism to alarm working scientists is pervasive enough that in a paper in which I (Zingrone, 2002a) defended the usefulness of science studies, I felt compelled — à la Latour — to state clearly where I stood on the issue of the reality of the natural world in the first paragraphs of the paper (p. 3). I did this because, after a presentation of a previous version of the paper, a number of people accused me of abandoning the scientific method and defending an idealist rather than a realist philosophy. This misunderstanding of my message was, to me, an indication that, just as discussing parapsychology seriously alarms some sceptics, discussing science studies seriously alarms some scientists — even scientists who work primarily in parapsychology — and that this alarm is sufficient to alter significantly the 'hearability' of the message.

imagined to hold sway over all practitioners of ‘true’ science. As psychologist of science Ernan McMullin (1987), put it:

classical theories of science, whether of Aristotle, of Descartes, of Kant, or of the positivists, all took for granted two theses: *foundationalism* (that science must be built on a foundation of propositions, themselves unproblematically true), and *logicism* (that science possesses a logical method that will allow one to determine which of two theories is the better one in any given case. (p. 50, my italics)

As the various disciplines of science studies developed, this simplistic view of scientific practise was repeatedly challenged, replaced by the understanding that controversy is itself ‘continual and essential’ (p. 50) to the refinement of scientific methodology and to the development of scientific knowledge. Controversy can then be defined as a

publicly and persistently maintained dispute ... [in which] the difference is one of belief, of knowledge claim ... [that] is held to be determinable by scientific means, (p. 51) [and that] must seem to the community to be worth taking seriously. (p. 52)

For the positivists who held sway in the mid-twentieth century — Karl Popper (1959, 1970) amongst them — the contested questions that caused scientific controversy were ‘What constitutes good conjecture?’ ‘What constitutes a good test?’ ‘What counts as refutation, replication, falsification?’ Underlying this view was the notion that sciences contained what philosopher Larry Laudan (1983) called ‘epistemic invariants’ (p. 28), truths or facts that are ‘essential’ to any form of science, that underlie all sciences, that all sciences must contain to be known as ‘true science’.

Individual sciences and individual scientists might identify or understand these invariants incorrectly, at least at first, but ultimately the ‘facts’ and their meaning would be uncovered and understood correctly. The presence or absence of such truths or the methods by which they may be uncovered in the repertoire of a discipline could be used to sort out ‘real science’ from ‘pseudoscience’, good practise from bad, theoretical competence from incompetence. This self-correcting logically- and rationally-revelatory process in science, it was thought, distinguished it from all other forms of knowledge-gathering and knowledge-use, and thus, established its superiority.

But even twenty years ago, after a decade of fieldwork amongst the hard sciences by such sociologists of science as Harry Collins (1974, 1975), Laudan (1983) and others began to doubt the existence of such invariants, finding instead in their examinations of science and scientific controversy an ‘... epistemic heterogeneity of the activities and beliefs customarily classified as scientific’ (p. 28) that made the Popperian notion of ‘demarcation’ in science moot. Laudan and others began to suspect that the variation of method, interpretation, theory, and practise provoked continual controversy. In different fields and even in different schools within a single field, different answers existed to the questions of what was relevant in terms of instrumentation, what was an acceptable level of predictability, what was an acceptable range of values in measurement, when it was appropriate to engage in *ad hoc* hypothesizing and when not, and so on.

Having expressed doubts about the presence of Popperian epistemic invariants in science, Laudan did not deny, however, that there were ‘... crucial epistemic and methodological questions to be raised about knowledge claims’ (p. 29), nor did he de-emphasise the importance of arguing ‘that a certain piece of science is epistemically warranted and that a certain piece of pseudo-science is not’ (p. 29). Even though science contained wildly varying sets of methods, interpretational standards, and consensually-proclaimed truths, controversy still could occur appropriately over such important questions as ‘... when is a claim well-confirmed; when can we regard a theory as well-tested; what characterizes cognitive progress?’

Controversy in science has been described in a variety of ways. For Thomas Gieryn (1995), controversies are boundary disputes, negotiations over the territories of phenomena, method, training, funding, over what constitutes a ‘fact’, who is qualified to make that determination, and at what point along the way. Gieryn has painted science as a complex landscape of point and counter-point, an exercise in cultural cartography. Controversy in science, Gieryn contends, involves the drawing and re-drawing of existing ‘maps’, the moving of boundaries, the modification of features, and the reification (however temporarily) of research programs and disciplines into features of the scientific landscape, that is, into identifiable ‘repertoires of characteristics’ that are available for the next cartographer in line (pp. 405-407).

If the Popperian (and even the Kuhnian notion of science) was 'essentialist' (p. 407), for Collins, Laudan and Gieryn, in the less essentialist view of science, controversy is everywhere. At each of the myriad stages in science practise, there is room for dissent, for varying worldviews based on what seem to be, at first glance, unproblematic truths about the natural world. Add in the profound influence of such nonepistemic variables as historical, political, social, and psychological factors and controversy easily arises. Once established, it twists and turns towards resolution in exceedingly complex ways. Amongst the complicating non-epistemic determinants of controversy, its process and resolution, are the influence of disciplinary socialisation, the political status of disputants, the power and pervasiveness of networks of advocates and counter-advocates, as well as personal motivations that have little to do with the work at hand and more to do with the constraining impact of everyday life, whether it be everyday life in the laboratory, the department, the university, the corporation, or at home. This is not to mention, of course, individual differences in intellect, temperament, and experience.

So, for example, evidence of the profound influence of non-epistemic factors has been uncovered in such case studies as Collin's (1974) examination of the research groups who developed the tea laser, work on replication in physics (Collins, 1975), historical studies of the rise and fall of such specific medical practises as blood-letting (e.g., Warner, 1980) and the development of laboratory science (e.g., Shapin & Schaeffer, 1985) in 17th-century England. These, and many other studies, have underscored the frequency with which the dominance of a research school, technique or interpretation was accomplished through the influence of non-epistemic factors

Scientific Norms

As in all social groups, science has developed norms. First described in the 1940s by the sociologist of science Robert K. Merton (1973), scientific norms are both social and moral. The Mertonian norms of science are communism, universalism, disinterestedness, and organised scepticism.

Thomas Gieryn recently described Merton's norms in this way (1995):

Communism asks scientists to share their findings, and the institution promises “returns” only on “property” that is given away.

Universalism enjoins scientists to evaluate knowledge claims using “pre-established impersonal criteria” (say, prevailing theoretical or methodological assumptions), so that the allocation of rewards and resources should not be affected by the contributor’s race, gender, nationality, social class, or other functionally irrelevant causes. The norm of *disinterestedness* does not demand altruistic motivations of scientists, but channels their presumably diverse motivations away from merely self-interested behavior that would conflict with the institutional goal of science ([which is the] exten[sion of] ... certified knowledge).

Organized skepticism proscribes dogmatic acceptance of claims and instead urges suspension of judgement until sufficient evidence and argument are available (p. 398).

As Hess (1997, pp. 56-58) and others (e.g., Gieryn, 1995; Mulkey, 1975) have noted, these *norms* are used as ideals to which science aspires and *cannot* be construed as descriptive of science practise in any subtle way. Rather norms *prescribe*: They are important touchstones against which scientific behaviour can be measured, especially in the context of controversy.

The perceived violation of Merton’s norms can lead to controversy. In parapsychology in recent years, for example, controversies have arisen when colleagues have refused to share data (e. g., Blackmore, 1987; Sargent, 1987) or were perceived by peers to have misused such data (e. g., Berger, 1989; Blackmore, 1984; Spinelli, 1989; Markwick, 1990), when it seemed that personal criteria had been employed in the evaluation of knowledge claims (e.g., Beloff, 1968; Hansel, 1961a, 1961b, 1966, 1968; Eysenck, 1968; Honorton, 1967; Medhurst, 1968; Pratt & Woodruff, 1961; Rhine & Pratt, 1961; Shapiro, 1968; Slater, 1968; Stevenson, 1967, 1968; West, 1968), and when the consensual rules of evidence and argument seemed to have been purposely distorted in the service of politics rather than science (e.g., Bem, Palmer & Broughton, 2001; Storm & Ertel, 2001; Milton & Wiseman, 1999).

Gieryn (1995, p. 398) has argued that the prose of those who describe the breaking of norms often conveys a sense of moral indignation. Further, when norms are wielded for political and social purposes, it is often to do boundary work (Gieryn, p. 400), that either establishes a hierarchy of disciplines or separates ‘legitimate’ scientists from non-scientists or pseudoscientists.

Approaches to Controversy in Science Studies

Recently Martin and Richards (1995) have subsumed the literature dealing with controversy in sciences studies under four main approaches: the positivist approach, the group politics approach, the constructivist approach, and the social structural approach.

The Positivist Approach

The positivist approach was described as one in which:

the social scientist accepts the orthodox view ... and analyzes the interchanges of the disputants from the standpoint that there is a correct position and an incorrect one. The debate is held to be legitimate and the social scientist attempts to determine if the controversy has been caused by incomplete or contradictory evidence, and then looks for resolution. ... the problem then becomes how to explain continued dissent. Legitimate questions for sociological research on the controversy under this approach are "Why do the critics persist in the face of the evidence?" "Who are the critics and what do they gain from persisting in their views?" "How do they relate to the wider forces [at work in society], such as corporations, governments and groups of 'true believers'?" (p. 510)

Scientists who argue in defence of the orthodox scientific view of the knowledge claim underlying the controversy are not very interesting to the analyst who uses this approach because such scientists have simply adopted the 'correct' interpretation of the evidence. The really interesting actors in the controversy are the dissenters. Examining these disputants under the positivist approach leads to what Martin and Richards have called 'a sociology of error'. There is an asymmetry in the analysis in that those who hold to the accepted 'truth' are not studied, and the dissenters are examined using all the 'familiar social science tools ... [to analyze] individual psychology, belief systems, social roles, vested interest groups, and the like' (p. 510). The analyst is, in effect, asking why the dissenters are so determined to be wrong and stay wrong.

The Group Politics Approach

Martin and Richards characterised the second approach used by science studies analysts as the 'the group politics approach' (p. 511). This approach, pioneered by

Dorothy Nelkin (e.g., 1982, 1992, 1995), ‘focuses on the groups involved in the controversy (governments, laboratories, disciplines)’. From this approach the resolution of controversy is seen as ‘a process of conflict and compromise involving various groups contending in a political marketplace’. Approaching controversy from this point of view allows the analyst to adopt any of a number of ‘theoretical frameworks’ such as the notion of ‘resource mobilization, in which the focus is on how different groups mobilize and use a range of “resources”, including money, political power, supporters, status, belief systems, and scientific authority’ (p. 511). In the group politics approach, the epistemic content of a scientific controversy is merely one more tool used by the combatants to bring closure to the controversy and to restore or overturn the balance of power, retaining or reallocating resources.

Analysts who use this approach seem to take for granted that the average scientist is fundamentally disinterested, and therefore ‘objective’. When specific interests are identified as operating in the controversy at hand, the group politics analyst will talk about the disputants as having been drawn into the ‘politicization of expertise’ (p. 511). Studies of this sort usually focus on scientific disputes that occur in the realm of public policy (e.g., Nelkin, 1995) where politics and resource allocation may be paramount. Martin and Richards (1995) argued that, applied to a specific scientific controversy occurring within a discipline or across local boundaries of related disciplines, the group politics approach loses its utility through the narrowness of its focus, especially if it is used to the exclusion of other approaches (p. 511).

The Constructivist Approach

The third approach to the study of scientific controversies, the constructivist approach, is, as noted above, the most misunderstood, both by scientists and by the public at large. For the purpose of this thesis it is sufficient to reiterate that the constructivist approach to scientific controversy allows for the influence of a variety of social forces and processes on the development of scientific knowledge. This approach takes as a given that a natural world exists (Latour, 1999, especially pp. 1-23) but that the shape and movement of the natural world — its dimensions, its causes, its laws — must always be interpreted imprecisely. Further, this imprecision arises, at least partly,

from the state of the art of current-day science, that is, from present-day limitations in theory, method, mode of observation and measurement. But — and this is the key point that the constructivist analyst makes — the imprecision also arises from the sometimes profound influence of social, political, and personal variables on the scientist herself at a variety of such points as the moment of measurement, or during the process of interpretation.

To put it more simply, sometimes the shape of the natural world and the social-psychological-political surround of the scientist combine in equal measure to determine what is taken as a scientific ‘fact’. Sometimes when method, theory and knowledge are more developed, the contour of the natural world is more obvious and something akin to ‘pure’ knowledge determines the production and application of new ‘facts’. But when method, theory and knowledge are not so developed, or when the social-personal-political surround is overwhelming, the contour of the natural world becomes lost and extra-scientific, non-epistemic factors determine the production of knowledge. Essentially then, at different levels of what is *already known*, epistemic and non-epistemic factors vary as determinants in the production of what is coming *to be known*. That is, as Martin and Richards (1995) have argued:

accounts are not directly given by nature but may be approached as the products of social processes and negotiations that mediate scientists’ accounts of the natural world. [The study of] ... [c]ontroversies have the ... advantage that these social processes, which ordinarily are not visible to outsiders, are confronted and made overt by the contending disputants. (p. 512)

The Social Structural Approach

The fourth approach to controversy Martin and Richards (1995) described is the social structural approach (p. 514) which looks at scientific controversy from the point of view of such macro-social structures as class, the state, and patriarchy. Marxist and feminist sociologists of science have used this approach with varying degrees of success. Amongst the most important of these types of analyses, to my mind, are those that have been done on gender and science (Haraway, 1991; Harding, 1986; Keller, 1985), a topic

that has found some resonance in parapsychology as well (e.g., Coly & White, 1992; Hess, 1988; Zingrone, 1994).

A Multi-Method Approach

Martin and Richards (1995) maintained that a method that integrates one or more of these four approaches is needed to properly understand scientific controversy. Such a fusion of perspectives, they argued, has a significantly better chance of providing really useful answers to such questions as ‘Why do specific scientific controversies erupt?’ ‘Why do some controversies persist?’ ‘What counts as closure in a scientific controversy?’ and ‘How does closure occur?’

I agree. It is important to acknowledge the centrality of the cognitive underpinnings of scientific debate, to recognise that there are always cognitive winners and losers whose relative positions in the debate are meaningful and must not be set aside.²⁰ But it is also important to understand the social and political surround in which the cognitive debate evolves and persists. Without such an understanding, the analyst may forget that the attributions which divide winners from losers may be resource-based, for example, and not representative of the strength, utility, or ‘trueness’ of the underlying knowledge claims. An integrated approach to science studies requires the analyst to remember that knowledge claims themselves, and the process by which a controversy erupts and persists, are multiply-determined and complex, arising from a symphony (or a cacophony) of forces, processes, interests and positions, with the contours of the natural world more or less obscured. In such an environment, Martin and Richards (1995) have argued, science analysts must be careful about what voice, what observation, what depiction is privileged as their analysis proceeds.

²⁰ This relatively politically-incorrect emphasis on what may be ‘real’ has a resonance in parapsychology in which paranormal theorists remind those of us who are more conventional theorists that it does make a difference to an understanding of both the phenomenology and the psychology of the report of an ESP experience, say, if the report contains specific information that can, in fact, be verified.

Seeking Closure

Psychologist of science Ernan McMullin (1987) identified three methods by which scientific controversies cease (p. 6): resolution, closure, and abandonment.

For McMullin, resolution is a kind of closure that flows from rational argument, a closure based on merit and on fact. Simple closure, on the other hand, flows from social, political and psychological considerations. Abandonment is simply that, the setting aside of the research problem. McMullin warned that controversies that achieve closure through the application of non-epistemic factors are inevitably reopened on rational grounds.

McMullin, like many current psychologists of science, oversimplifies science and its attributes. Closure, like controversy, is a complex and varied terrain. In McMullin's defence, however, even Thomas Kuhn has been accused of 'black-boxing' closure in his discussions of the related concept of consensus, an essential element in his concept of paradigm shift (Gieryn, 1995). For Kuhn — at least the early Kuhn — there was an inevitable movement in normal science towards the conversion experience, the paradigm shift, in which the consensus as to what constituted the fundamentals of science changed profoundly (1970, 1977). This conversion experience then rippled through science, altering the boundaries of disciplines, reallocating resources, and so on. The paradigm shift was followed in Kuhn's scenario by another long walk through normal science towards the next sea change, the next paradigm shift.

Thomas Gieryn (1995) felt that Kuhn used the notion of consensus as a magical point of accord that was reached when the paradigm shifted. Consensus, Gieryn noted, is far more problematic than that. Three 'interpretative problems' needed to be solved before consensus could be established: (1) scientists would have to decide who belonged to the community and who could determine what consensus meant and whether it had been achieved; (2) scientists would have to make judgements on the 'changing beliefs of other scientists in regard to [their subject matter] ... Who accepts it, and when did their conversion to the new framework occur?'; and (3) the cognitive content of the consensus would need to be articulated and accepted, which in itself presumes the presence of consensually validated modes of articulation and criteria by which to define acceptance

(p. 404). The opportunities for variation in the resolution of these interpretative problems are endless, underscoring the insightfulness of Gieryn's view that science is 'a kind of spatial "marker" for cognitive authority, empty until its insides get filled and its borders drawn amidst context-bound negotiations over who and what is "scientific"' (p. 405).

Using Science Studies from an 'Insider' Point of View

As a parapsychologist, I am well aware that attempting to adopt an analyst's gaze at my own discipline is an exercise in reflexivity. I am firmly entrenched in what Barry Markovsky (1997) has called a social network. In his work and in the work of his students (e.g., Eisenberg, 2002), a case has been made that the network of social relationships in which scientists participate is an important factor that keeps them on one side of a disciplinary line, along with their mentors, colleagues and students. As a scientist moves through his or her career, these social networks solidify, making the individual resistant to the findings and methods of networks to which he or she does not belong. Social network theory is a powerful sociological tool for understanding group processes and their influence on the actions and beliefs of individuals.²¹

So, as an insider in parapsychology with an affinity for the methods and approaches of various subdisciplines in science studies, I was keenly aware that I was going into this project with competing points of view, that I am to some extent an embodiment of the conflict between the social and the cognitive, between the positivist and the constructivist/political approaches, that I am both operating within, and attempting to move beyond, my own social network. In choosing specific methods through which to examine the persistence of controversy in parapsychology more closely, I was aware that I needed to find a stance that both incorporated, and insofar as possible, protected against my 'insiderness'. Adopting a multi-method approach that is largely constructivist but also maintains an eye on the political and psychological, and

²¹ Further work by Anne Eisenberg (2002) has shown that the relationship of social power and group identity both engages with, and constructs, scientific legitimacy within and across disciplinary boundaries.

by choosing published texts as the focus of my research, I self-consciously sought to acquire both a stance that underscored reflexivity, and a 'subject' for study that, by virtue of its structure, form and style, was not as fluid or as open to the imposition of my personal biases as primary archival materials or 'talk' might be.

The primary questions in this thesis then are methodological and substantive. Methodologically, I wanted to identify and test some selected methods in science studies that might be used to understand controversy in parapsychology more deeply. Substantively, I wanted to know what aspects of the texts I choose contributed to, or inhibited, closure.

As for the methods, having settled on text, I chose three distinct avenues by which I might study controversy in parapsychology. The first was drawn from history of science. I took what Kragh (1987) has called the 'long view' of the field's history and attempted to account for the persistence of controversy by examining briefly the evolution of its various institutions, and by reviewing the principle texts of controversy that were published in the English-language literature in parapsychology. Materials were drawn from the *Proceedings of the Society for Psychical Research*, the five main English-language journals extant today — the *European Journal of Parapsychology*, the *International Journal of Parapsychology*, the *Journal of the Society for Psychical Research*, the *Journal of the American Society for Psychical Research*, or the *Journal of Parapsychology* —, as well as from periodicals published in other disciplines from 1882 to 2002. To some extent, the brief historical overviews in Chapters 2 and 3 provide a superficial first attempt at drawing lessons from this material, with the former chapter focusing on the history of the field as an institution, and the later chapter focusing on the history of published responses to criticism.

In both history and rhetoric of science, a number of useful case studies have been produced that narrow in on an individual scientist (e.g., Gruber, 1974), or on a community of scholars and scientists and the impact of their inter-relationships on the cognitive consensus at which they ultimately arrived (e.g., Rudwick, 1988; Shapin & Shaffer, 1985). To some extent, the review of the controversy over ESP that occupies Chapter 5 may be considered one of these.

The persuasive structure of scientific texts is a topic that has been dealt with by rhetoricians of science (e.g., Cecarelli, 2000; Gross, 1996; Myers, 1990), by literary critics of various types and orientations (e.g., Bazerman, 1988, 1995), by psychologists interested in argument (e.g., Billig, 1991) or discourse (e.g., McKinlay & Potter, 1987), and by science analysts (e.g., Gilbert & Mulkay, 1984). These works, from different disciplinary starting points, were able to uncover some important features of scientific expression. The organising principle that runs through Chapter 3 and the case studies that appear in Chapters 5 and 7 have drawn from these approaches.

The Thesis that Follows

At this point it is important to describe the structure of the rest of this document. As I have mentioned above, the thesis is not written in the style normally associated with a scientific report but rather presents a more humanities-based organisation of contextualising background followed by case study, interpretation and speculation.

Chapter 2 provides a brief history of the field which emphasises the broad structure and content of the field rather than specific methodology, results, or theory. It is hoped that this chapter will give the reader not only a sense of the shape of the field as it has unfolded over the 124 years since the founding of the Society for Psychical Research, but also a sense of the centrality of controversy in the history of the field.

In Chapter 3, I introduce an organising principle used by Gilbert and Mulkay (1985) in which arguments are divided between the ‘contingent’ and the ‘empirical’. Analogous to the notions of ‘social’ and ‘cognitive’, Gilbert and Mulkay found that these two repertoires not only identified different types of arguments in controversy but also were deployed differently as political and substantive aspects of the institutions, individuals and cognitive content of the dispute varied. Previous reviews of criticism of parapsychology are organised according to whether they focused on contingent or empirical aspects of the controversy at hand, or combined these elements in a broader argument. By organising this considerable material in this way, I intended not just to describe it for its own sake but also to provide a context for the later, more narrowed case studies.

In Chapter 4, I return to a survey of science studies that focuses on the use to which science analysts have put parapsychological materials in the past — attempts to deal with the demarcation of science from pseudoscience amongst them — and on the utility of rhetoric of science as an analytic approach.

In Chapter 5, I test the hypothesis that differences in style and structure of the published materials of critics and proponents in parapsychology may have contributed to the persistence of the controversy. The style and structure of two books by proponents are examined as representative of the controversy, *Extra-sensory Perception* (Rhine, 1934) and *Extra-sensory Perception After Sixty Years* (Pratt, Rhine, Smith, Stuart & Greenwood, 1940). I attempt to contextualise these volumes within the historical and political surround of their inception and development as well as to give a flavour of the controversy itself as it played out in the pages of both parapsychological and mainstream periodicals.

In Chapter 6, I review the history and traditions of discourse analysis as it has been done in a variety of disciplines, including psychology. I provide some examples of the concerns and findings of discourse analysts, discussing these in relation to ‘fact’ construction, controversy, conflict in discourse, and to scientific talk and tests.

In Chapter 7, using some of the concepts of discourse analysis in psychology, I examine how two specific claims made by a critic in a target article in the invited debate which appeared in the journal *Behavioral and Brain Sciences* in 1987 are received and responded to by interlocutors who are either critics themselves, or proponents.

The conclusion of the thesis, Chapter 8, compares and contrasts the insights gained into the persistence of controversy when using a multi-method approach that focuses on history, rhetoric, and discourse. In addition to outlining research that remains to be done, I speculate on the possible shape and content of a reconfigured parapsychology that includes science studies.

CHAPTER TWO

A BRIEF HISTORY OF PARAPSYCHOLOGY

In this chapter I will briefly review the history of the field so as to provide the reader with a sense of the context out of which parapsychology developed, as well as a sketch of the general outline of the institutional state of the field today.²²

The Early Origins of Psychical Research

It is not uncommon for histories of scientific parapsychology to begin with the allegedly mediumistic happenings that surrounded four little girls in New York state in the mid-nineteenth century. But neither the phenomena, nor the effort to understand the phenomena using whatever tools of science then extant began in Hydesville in 1848 (Podmore, 1902, pp. 3-43; Dodds, 1971; Inglis, 1992). Not only have the phenomena usually subsumed under the heading of parapsychology been reported since antiquity but there were, in fact, quite a number of attempts that predated the modern era in which authors sought to systematise tales of, or test hypotheses about, those phenomena a “modern” reader would now see as ostensibly paranormal.²³ Amongst these were:

- the Greek philosopher Democritus’ (460-370 BCE) attempts to conduct an ‘experimental study of images (whether divine or ghostly in origin), sometimes isolating himself for the purpose in desert places and cemeteries’ (Dodds, 1971, p. 195);
- ‘the famous story of the test applied by Croesus, King of Lydia in the mid-sixth century BCE to the Delphic and other oracles — the earliest example of what would today be called an experiment in long-distance telepathy’ recounted in Herodotus’ *History* (Dodds, 1971, p. 198; Inglis, 1992, p. 55);

²² In this chapter I have not ‘done history’ in the sense of providing primary research or a critical review of primary sources. What follows is a mixture of secondary and primary sources meant only to provide a rough context for future chapters. In Chapter 8, in the critique of my use of this method, I will outline some of the historical projects that remain to be done.

²³ A case can be made, of course, that those experiences we recognise as belonging to the problem domain of parapsychology — that is, as ostensibly paranormal — may not have been considered anomalous in the times in which they occurred. Therefore, I am aware that their inclusion here to some extent constitutes an exercise in ‘Whig history’. The list is important, however, for establishing a context.

- Cicero's treatise, *De Divinatione*, in which he logically examined the evidence for divination and prophecy and that Inglis (1992) characterised as the 'first sceptical manifesto' (p. 58);
- the writings of St. Augustine (354-430 CE) characterised by Dobbs as 'The most careful and sober descriptions of supernormal occurrences which have come down to us from antiquity' written by a man 'who [Dobbs says] deserves a more honourable place in the history of psychical research than any other thinker between Aristotle and Kant' (p. 205), presumably at least partly because of Augustine's systematic examination of the accuracy of prophecy in Biblical sources (e.g., Augustine, 1950, pp. 522-523, 545-548, 572-579, 587-598);
- Joseph Glanvill's (1636-1680) much-reprinted book, *Saduccismus Triumphans* (1668) in which he recounted his investigation of the Mompesson poltergeist case (Inglis, 1992, pp. 120-122), as well as described a questionnaire which he developed to gather and organise the details of the seemingly-paranormal experiences he investigated (Ebon, 1974, pp. 58), amongst other things;
- *De Beatificatione et Beatorum Canonizatione* (1730) by Prospero Lambertini (1675-1758) who later became Pope Benedict XIV, in which seemingly-paranormal cases were critically examined for the purposes of standardising the procedures by which a miracle was established during the canonisation process (Haynes, 1970; Nickell, 1993, pp. 10; Rogo, 1975, pp. 35-36);
- the French Royal Commission on Mesmerism of which American Ambassador Benjamin Franklin (1706-1790) was a part, and which, in 1784, investigated, in the context of Mesmerism, a wide variety of phenomena now considered to be the province of scientific parapsychology (Gauld, 1995, pp. 26-29; Inglis, 1992, pp. 142-143; Rogo, 1975, pp. 36-39);²⁴
- Samuel Hibbert's (1782-1848) *Sketches of the philosophy of apparitions, or an attempt to trace such illusions to their physical causes*, first published in Edinburgh and London in 1824, and then enlarged for a second edition in 1825;

²⁴ The preparation of the ground for Spiritualism by Mesmerism has been the topic of a number of scholarly treatments (e.g., Crabtree, 1993; Dingwall, 1968, Vol. 4, p. 32).

- a pamphlet by the prolific Scottish novelist, Sir Walter Scott (1771-1832) entitled *The existence of evil spirits proved; and their agency, particularly in relation to the human race, explained and illustrated*, published by Jackson & Walford in London in 1843;²⁵
- and *Night-Side of Nature: On Ghosts and Ghost-Seers*, the popular compendium of German apparitions, poltergeists and other seemingly paranormal cases written by British novelist Catherine Crowe (1800-1876) and published in 1848.²⁶

Some of these early studies, such as those of Democritus, Crowe, and Scott, were made up of local legends and the experiences of people known to the authors. Occasionally, cases were investigated or critically analysed such as in the works by Augustine, Glanvill, and Lambertini. Many of these early works also contained what we would now consider to be theoretical discussions and critical commentary on the origin of the phenomena and on the plausibility of the tales retold.

Although the use of March 1848 as the origin of modern Spiritualism and psychical research in the United States has been correctly criticised (e.g., Braude, 1989; Hyslop, 1919; Podmore, 1902), the Fox sisters' seemingly mediumistic phenomena did catch the attention of a number of American scientists, amongst them eminent chemist Robert Hare (1781-1858) who published a treatise on the topic in 1855. Mediumistic phenomena, especially those surrounding Daniel Dunglas Home (1833-1886), who arrived in England in 1855, sparked a great deal of interest amongst both the general population and the intellectual and social elite (e.g., Gauld, 1968, pp. 69-82; Lamont, 2002). The growth of Spiritualism in England was as rapid and socially visible as in the United States (e.g., Barrow, 1986; Oppenheim, 1985; Owen, 1990). Beloff (1977) has noted that, although the phenomena of Spiritualism spurred research, it did not constitute an advance but was rather:

²⁵ One of Scott's biographers notes that he was long a recipient of letters from readers and friends carrying tales of local legends and various personal experiences, including those of seeing ghosts (Bucham, 1932, p. 215).

²⁶ Gauld cites Crowe's (1846/1848) book as one of the landmark events in psychical research in Great Britain in the mid-19th century; the founding of the Cambridge University Ghost Club in 1851 being another (Gauld, 1968, pp. 66-67).

a regression to a cruder and more outlandish conception of the paranormal. Its key idea, that of communicating with the spirits of the deceased, stems from a venerable occult tradition — shamans and witch-doctors were forerunners of the medium. What was new ... was its prosaic matter-of-factness and its cosy conception of the relationship between the two worlds. (p. 5)

In addition, Beloff wrote, the exposure of obviously fraudulent mediums and the scandals that accompanied their uncovering brought about a ‘debasement of ... [Spiritualism that] was indeed more rapid and shameless than the similar debasement of the earlier mesmeric movement, for there was more money to be gained by successful imposture in the case of Spiritualism’ (p. 7, see also Pearsall, 1973).

But research on séance room phenomena attempted on both sides of the Atlantic drew both accolades and heavy criticism. Hare’s own volume was answered in print by John Lord in a pamphlet published in 1856, the title of which characterised Hare’s work as a ‘mendacious humbug’. Stanford University professor and recipient of funds originally intended solely for psychological research, John E. Coover (1872-1938), in a lengthy treatment of the evidence against the Fox sisters, noted that a number of scientific investigators had exposed the fraudulent basis of the Fox sisters’ supposed phenomena (Coover, 1927, pp. 236-237). Amongst them were: Professor Page of the Smithsonian who published his findings in the United States in 1854; and the naturalist Louis Agassiz who, with his Harvard colleagues Benjamin Peirce and Eben Horsford, published a pamphlet in 1859 which detailed the results of the work of an ‘investigating committee’ to which a prize of \$500 had been attached if real phenomena were witnessed.²⁷

The Society for Psychological Research

In the early 1850s, the American medium Mrs Hayden arrived in London and sparked general and sustained interest in séance room phenomena. By 1853, a related phenomena of table-tilting arrived from the continent, and ‘spread across the country’

²⁷ Coover claimed that the book was published by George Lunt of the Boston Courier in 1859. I was unable to verify the details of either the Page or the Agassiz et al. reference in the collections of the Library of Congress, Harvard University Library or the New York Public Library.

(Gauld, 1968, p. 68) An example of the interest of educated men in these phenomena can be found in the writings of Michael Faraday (1791-1867). Following on a suggestion made by W. B. Carpenter, Faraday (1853a, 1853b) designed and executed a series of ingenious experiments that showed how unconscious muscular action could produce table-tilting when the hands of sitters were in contact with the table.

As the reports of the performances of Daniel Dunglas Home and other mediums grew, the London Dialectical Society set up committees to investigate the phenomena being reported (London Dialectical Society, 1871). Convened in 1868 (Gauld, 1968, pp. 83-84; Lamont, 2002, p. 32), a number of prominent men of science of the era were invited to conduct research. Many declined. The physicist and chemist William Crookes (1874) was amongst those who investigated Home.

Gauld (1968) has said:

By the mid-eighteen-seventies the main issues had become pretty clear-cut. Either one had to accept the occurrence of astonishing and incredible physical phenomena, of a kind which had hitherto escaped detection; or one had to admit that the senses or the memories of seemingly sane people could deceive them in preposterous and unprecedented ways. (p. 83)

Although such men as Cambridge philosopher Henry Sidgwick (1838-1900) were laying the investigative groundwork for later psychical research in their mediumship studies in the 1870s, William F. Barrett (1844-1925) is usually given the credit for laying the organisational groundwork. Barrett, a physicist from the Royal College of Science in Dublin, had submitted a paper in 1876 to the British Association for the Advancement of Science on what Inglis called (1992, p. 321) 'the range of perception' exhibited during mesmeric trance by a child Barrett was investigating. The naturalist and Spiritualist Alfred Russell Wallace (1823-1913) was the chairman of the committee that determined which papers would be read at the meeting, and through his influence, Barrett's submission was included in the program (p. 321). Although the paper was not published in the BAAS proceedings for that year (Lodge, 1927, p. 3), it did appear in the *Spiritualist Newspaper* (Barrett, 1876). In response both to the verbal presentation and the published article, Barrett received a flood of correspondence detailing experiences. The act of instigation for which Barrett has been given credit was

this: It was said that Barrett was the first to press for a society for the scientific study of seemingly-psychic phenomena, and that his influence, in effect, ‘caused’ the founding of the Society for Psychical Research (SPR) in London (Gauld, 1968, p. 147; Murphy, 1961, p. 2).²⁸

Alan Gauld’s ‘biography’ of the origin and early years of the Society charts the genesis of interest in psychic phenomena in the minds of its founders as well as the goals and early research efforts of the SPR. It is interesting to note that essential members of the core group came together not only because of their fascination with the phenomena of the séance room but also because of their interest in the equalisation of educational opportunities for women.²⁹ Both topics were of primary importance to Henry Sidgwick (Thomson, 1937, pp. 297-298), a professor of moral philosophy at Cambridge University who was also the first president of the Society; to Eleanor Balfour Sidgwick (1845-1936), his wife, whom Sidgwick met whilst campaigning for women’s education at Cambridge, and to Frederic W. H. Myers (1843-1901), a classical scholar who resigned his university position in 1869 to work full-time in the movement (Gauld, 1968, p. 94).

Both Sidgwick and Myers had also been involved with the Cambridge University Ghost Club prior to the early 1870s,³⁰ at which point first Myers and then both men began to investigate séance room phenomena. By 1874 Sidgwick and Myers had organised an “informal association” (Gauld, 1968, p. 104) which included, amongst others, the individuals who would later become the most productive workers of the early

²⁸ There is some controversy over whether or not Barrett was really the person who deserved credit for the initial idea, or whether journalist and Spiritualist Edmund Dawson Rogers was in fact the first to argue for such a society (Alvarado, 1983, pp. 147-148).

²⁹ Both the founding of Newnham College — one of two colleges at Cambridge to offer education to young women in the late 19th century — and its early educational rigour have been attributed to the efforts of Henry Sidgwick (Havard, 1959, pp. 16-20). Eleanor Sidgwick was also to serve Newnham College through-out her life, acting as Newnham’s Treasurer, Vice-Principal (1880-1882), and Principal (1892-1910).

³⁰ The Cambridge University Ghost Club was founded in 1851 by Archbishop Edward White Benson (Benson, 1899, p. 98; Berger, 1985, p. 42), who was a second cousin to Henry Sidgwick (Benson, 1899, p. 145). In his autobiographical sketches of Cambridge, physicist J. J. Thomson (1937) noted that Sidgwick had joined the Ghost Club as an undergraduate. He characterised the goal of the Society as the ‘... the investigation of ghost stories. Accounts of abnormal experiences such as hallucinations, premonitions, phantasms of the dead and living and those occurring at spiritualistic seances were published from time to time, but no one troubled to test the evidence in support of them’ (p. 298).

Society for Psychical Research: Edmund Gurney (1847-1888), who read classics at Cambridge, graduating in 1871; another Cambridge classical scholar Walter Leaf (1852-1927); John Strutt, Lord Rayleigh (1842-1919), a Cambridge professor, physicist and Nobel Laureate (1904), and his wife Evelyn Balfour Strutt, Lady Rayleigh; Arthur Balfour (1848-1930), also Cambridge-educated, who served as Prime Minister of Great Britain from 1902 to 1905; and of course, Eleanor Balfour Sidgwick. Myers, Sidgwick and their group joined with such prominent Spiritualists as the Reverend W. Stainton Moses (1839-1892), the linguist Hensleigh Wedgewood (1803-1891), and the journalist Edmund Dawson Rogers (1823-1910), amongst others, to found the Society for Psychical Research on February 20th, 1882.

The work of the society was to be carried out by six committees with special areas of emphasis. The overall purpose of the society was to bring a scientific gaze to a variety of phenomena (Gauld, 1968, p. 137; Haynes, 1982, p. xiii). The six committees focused on telepathy (which they called ‘thought-reading’), mesmerism, haunted houses and apparitions, séance room phenomena, the Reichenbach phenomena,³¹ and a ‘literary’ committee whose remit included gathering historical evidence on all the phenomena at hand from bibliographic sources (*Objects*, 1882, pp. 3-4).³²

Over the 124 years that separate us from the founders of the SPR, the society has remained one of the primary venues in Britain for the dissemination of the findings of the scientific study of psychic phenomena, if not frequently the primary locus of research through its grant recipients, committees, and publications. Amongst these have been: the findings of mediumship studies, both physical and mental, and the re-analysis of these (e.g., Crookes, 1889; Hyslop, 1901; Keen, 2002; Munves, 1995, 1997; Myers, Lodge, Leaf & James, 1890; Rayleigh, 1933; Thomas, 1933; Thouless, 1937; Schwartz, Russek & Barentsen, 2002; Schwartz, Russek, Nelson & Barentsen, 2001; Wiseman, 1992); the results of spontaneous case collections and surveys (e.g., Alvarado, 1986; Alvarado & Zingrone, 1995; Beloff, 1973; Besterman, 1933; Clarke, 1991; Cornell &

³¹ Reichenbach phenomenon were the visual perception of luminous phenomena around crystals, magnets and to a lesser extent, the human body (Reichenbach, 1968).

Gauld, 1961, 1969; de Pablos, 1998; Houran & Thalbourne, 2001; Lambert, 1964; Sidgwick & Committee, 1894; Stevenson, 1970; Stevenson & Chadha, 1990); laboratory research (e.g., Chauvin, 1988; Jephson, Soal & Besterman, 1931; Medhurst, Stark & Thompson, 1965; Pallikari-Viras, 1997; Randall, 1972; Robertson, 1957; Scofield & Hodges, 1991; Thouless, 1951; West, 1954); and theoretical and philosophical essays (e.g., Alvarado, 2003a; Dobbs, 1965; Ellison, 1978; Taylor, 1999; Thouless & Weisner, 1947; Thouless, 1984). The modern research published in the journal and proceedings of the Society for Psychical Research no longer includes the original emphases on mesmerism (except in the guise of hypnotism or altered states) nor does it include Reichenbach phenomena. Telepathy, apparitions, haunted houses, and séance room phenomena are still very much in evidence, however.

American Psychical Research in the 19th Century

If in Britain the history of parapsychology can be said to have centred primarily on a single organisation with a relatively unbroken line of leadership and a largely unaltered purpose, the history of parapsychology in the United States has been more Kuhnian. The dominant paradigm in the field has shifted repeatedly, accompanied by competing paradigms of method and theory lurching to the fore and then receding. What is the core and what is the periphery of parapsychology in the United States has changed along with these paradigm shifts, between organisations and geographical locations, and within organisations.³³

Amongst the elements shared between the Society for Psychical Research and its American counterpart is the myth surrounding the importance of William F. Barrett's role. Internal historians of American psychical research tend to lay the primary impetus for the founding of the American Society for Psychical Research (and thus all psychical research and parapsychology in the United States) squarely at Barrett's feet (e.g.,

³² Nicol (1972) noted that of the committees 'All produced significant reports except the Physical Phenomena Committee, whose activities ended in disputes amongst its members' (p. 350).

³³ On the concepts of paradigm shift see Kuhn (1970), and on the economic/political concept of core and periphery see Wallerstein (1974, 1980).

Murphy, 1977, p. 51; Berger, 1985, p. 44).³⁴ Such authors also place equal emphasis on William James (1842-1910), who long had a personal interest in the phenomena, partly because of his father's adherence to Swedenborgianism (e.g., Murphy & Ballou, 1960; Murphy, 1977). In actuality, the story is much more complicated than that.³⁵

In fact, the two co-secretaries of the American Association for the Advancement of Science, Carville Lewis of the American Academy of Science in Philadelphia, and Harvard medical school professor Charles Sedgwick Minot (1852-1914), met William Barrett at the 1884 meeting of the British Association which was being held in Montreal. They induced Barrett to give a talk on psychical research in the rooms of the American Academy of Science in Philadelphia after the BAAS meetings. When Barrett accepted their invitation, he had also made arrangements to talk to a group of Bostonians who were interested in establishing a society for psychical research (Taylor, 1985). The Boston group included N. D. C. Hodges, then acting editor of *Science*,³⁶ and the Harvard astronomer Edward C. Pickering (1846-1919). Ultimately two meetings were held in Boston in which the first volumes of the proceedings of the SPR were reviewed. It was actually Charles Minot, and not Barrett or James, who proposed the organisation of the American Society for Psychical Research to the group. The Society came into formal

³⁴ Alan Gauld (1968) takes the more conservative tack of labelling Barrett's participation in the founding of the ASPR as 'instrumental' (p. 147).

³⁵ In fact, the full story of the founding of the early ASPR and the interests and motivations of its early membership has never been told. Molly Noonan's (1977) doctoral thesis comes close, but was written before the wave of history and sociology of professionalisation, pseudoscience, marginality and controversy in science and is thus, not a work of historical subtlety. Two authors have dealt more closely with this history to some degree: Eugene Taylor, a Jamesian scholar who has a particular interest in a variety of late nineteenth- and early twentieth-century movements amongst which he counts psychical research (e.g., Taylor, 1993, 1996, 1999); and Deborah J. Coon, an historian of psychology who is interested in the professionalisation of American psychology (e.g., Coon & Sprenger, 1998). Coon received financial support from Harvard's Hodgson Fund (named after ASPR secretary Richard Hodgson) to complete the study she has done that is most relevant to the history of the ASPR (Coon, 1992).

³⁶ *Science* was not at the time affiliated with the AAAS but was a private magazine founded in 1880 by Thomas Edison, who had personal interests in survival and telepathy. At the time *Science* had recently been purchased from Edison by Gardner Greene and his son-in-law, the inventor Alexander Graham Bell (1847-1922). Under Edison, and also under Green and Bell, *Science* had a positive, even enthusiastic outlook on psychical research (Taylor, 1985, pp. 328-329) which continued until it was bought by psychologist James McKeen Cattell (1860-1944) in 1893, who, unlike his predecessors, was highly critical of psychical research throughout his career (e.g., Cattell, 1938). *Science* adopted Cattell's attitude towards the subject.

being on December 19, 1884 (Berger, 1985, p. 47). At that second meeting of the Boston group, the ‘Committee of Nine’, which did include William James, was established to solicit membership from amongst the American social and scientific elite.³⁷

The group had decided that ‘the American scientific community could no longer remain agnostic regarding certain psychic or spiritualistic phenomena’ (Noonan, 1977, pp. 63-64). The initial list of invitees was crafted for the credibility such prominent men could bring to the enterprise, should the program of research lead to results supportive of telepathy and other psychic phenomena (Taylor, 1985, p. 327).

The first president was mathematician and astronomer Simon Newcomb (1835-1909) of Johns Hopkins University.³⁸ The first vice-presidents were Edward C. Pickering, Minot, Henry P. Bowditch (1840-1911) also of Harvard Medical School, psychologist G. Stanley Hall (1846-1924), then at Johns Hopkins University and later President of Clark University, and philosopher George S. Fullerton (1859-1925) of the University of Pennsylvania. William James was also a member of the wider council which, including the officers, numbered 21 individuals (*Proceedings of the Society for Psychical Research*, 1885, pp. 1-2). The American society was founded with an express purpose that speaks of the highest sense of scientific duty (*Proceedings of the American Society for Psychical Research*, 1885):

The Council of the American society ... feels that the duty can be no longer postponed of systematically repeating observations similar to those made in England, with a view to confirming them if true, to definitely pointing out the sources of error in them if false. If true, they

³⁷ In Gardner Murphy’s work on the history of the American Society, William James is characterised as the American equivalent of the SPR ‘engine room’ (e.g., Murphy & Ballou, 1960, Murphy, 1961, Murphy, 1977) which, in my opinion, overstates James’s importance. It is true, however, that as the society aged and the most active of its initial membership began to resign, James’s continued interest and financial support *became* as crucial for its survival (e.g., Noonan, 1977, p. 71; Taylor, 1985, p. 329-330,) as did the London group’s financial support of the American Society when Richard Hodgson was the Research Director. Further, unlike his British counterparts, James’s research and writing never narrowed to psychical research only but remained wide-ranging, touching on nearly every aspect of psychology, psychotherapy and the like.

³⁸ William James characterised the election of Newcomb and the other officers as a ‘matter of policy’. There was a particular understanding of the persuasive power of having a man of Newcomb’s stature at the helm, should he become convinced of the reality of the phenomena (e.g., Noonan, 1977, pp. 74, 79-80; Taylor, 1985, p. 328), which, sadly, he did not.

are of value, and the tracing of their limits becomes a scientific duty. If false, no time should be lost in publishing their refutation; for, if allowed long to stand uncontradicted, their only effect will be to re-enforce powerfully the popular drift toward superstition. (pp. 1-2)

The ASPR was thus formed for the purpose of replication, and if none were forthcoming, for refutation of sufficient force to have an impact on popular beliefs.³⁹ Thus, a number of important differences between the ASPR and its predecessor, the SPR, were apparent from the beginning. Berger (1985) notes that whilst Henry Sidgwick's presidential address 'emphasised that the dispute concerning the reality of the phenomena the SPR had been established to investigate was nothing less than a scandal', Simon Newcomb's presidential address 'placed the investigation of telepathy on the same level with looking for some different kind of gold: It would be a waste of time' (p. 47).⁴⁰ In Noonan's (1977) more positive portrayal of Newcomb, he and many members of the early ASPR, distinguished themselves from their English counterparts by a willingness to accept alternate and more conventional explanatory models. As Mauskopf (1989) has noted:

... the governance of the ASPR was placed in the hands of the scientific elite. ... Unlike the SPR which undoubtedly had as many persons of prominence in its membership and leadership as the ASPR but whose leadership was committed to the sympathetic pursuit of psychical research through the concern to stem the tide of scientific materialism, the ASPR leadership was much more tough-minded, much more interested in psychical research phenomena as puzzles to be solved. (p. 12)

Another difference was that, unlike the SPR, the ASPR did not have amongst them a core group of individuals with driving personal interests or sufficient personal

³⁹ Noonan (1977) speculates in her conclusion that the prospect of uncovering facts of psychological import motivated many of the medical men, neurologists and philosophers amongst the group, men who otherwise had no particular commitment to whether or not the facts might be supportive of the kind of metaphysical questions to which the London group seemed devoted.

⁴⁰ Berger's use of this quote to characterise Newcomb as a thorough-going sceptic is probably unfair. In the early days of the ASPR Newcomb himself admitted only to being an 'unconfirmed skeptic' (Noonan, 1977, pp. 62-63), that is, willing to be convinced by evidence one way or another. Whilst he held out no hope for telepathy by the time he wrote his Presidential Address, he was interested, at least for awhile, in investigating such phenomena to see where they might lead.

funds to devote themselves full-time to psychical research. It was not that the academics involved were disingenuous about their initial interest in the problem area. Nor were they inexperienced in confronting the claims of Spiritualism — even G. Stanley Hall and Simon Newcomb had spent their ‘boyish credulity ... [visiting] every professional medium in Philadelphia’ (Moore, 1977, p. 143). It was that they did not carry with them *either* the pressing need to answer the wider questions that engaged the London group *or* the financial wherewithal to give up the other scientific avenues upon which they made their livings.

On the topic of passion for the subject matter, James put it bluntly in a letter to Thomas Davidson written in February of 1885 which Taylor (1985) has quoted:

... As for any anti-spiritualist bias of our Society, no theoretic bias, nor *bias* of any sort whatever, so far as I can make out, exists in it. The one thing that has struck me all along in the men who have had to do with it, is their complete colourlessness philosophically. They seem to have no preferences for any general-ism whatever. ... (p. 328)

The ASPR organised themselves into research committees modelled on the committees of the SPR and some quite interesting research was accomplished (Berger, 1985, pp. 48-50). But for most of them, as the years went on, the findings were not sufficiently supportive of paranormal explanations (insofar as they would define them) to make worthwhile the maintenance of a specific sub-discipline devoted solely to the topic. The fall-off amongst those who were willing to carry out research was such that just under two years after the society was founded, on October 4, 1886, James was writing to a friend that ‘There is no one in the Society who can give any time to it, and I suspect it will die by the new year’ (Murphy & Ballou, 1960, p. 66),⁴¹ a sentiment which echoed a fear he had first voiced in 1884, a month before the ASPR was formally founded (Taylor, 1985):

⁴¹ The exodus of the prominent men of science also meant an exodus of the social elite, and with them access to funding for infrastructure and research (Noonan, 1977). Financial difficulties were to remain a perennial problem for the ASPR under whatever leadership the society might have (e.g., Berger, 1985, Osis, 1985), from its beginnings until the 1960s when the inventor of Xerox, Chester Carlson, funded an active research program. In more recent decades, however, the ASPR is once again struggling to survive.

... The [American] Society for Psychical Research isn't out of the egg yet and its success will wholly depend on whether any individuals be forthcoming who will give their time to it as Gurney, et al., have done in England. We know of none such yet, and without these the society will be 'simply ridiculous'. (p. 326)

In 1887, the Society for Psychical Research in London sent the Australian psychical researcher Richard Hodgson (1855-1905) to Boston to take over the reins of the organisation as its secretary, receiving funds for the purpose from the SPR. Hodgson has been described as a capable, energetic, intelligent, and experienced researcher, who, as the membership of the ASPR declined, coupled managing 'the correspondence, circulars, and other work' (Berger, 1985, p. 51) with an active research program (Berger, 1988, pp. 11-33). Although some research was conducted by other members,⁴² the most important body of work was that which Hodgson and James did with the medium Mrs Piper (Moore, 1977, pp.143-149). When Hodgson died suddenly in 1905, the future of the American Society was again in doubt.

Psychical Research to Parapsychology in the 20th Century

James Hervey Hyslop (1854-1920) was a Columbia University philosopher who had resigned his academic position for health reasons and was in the process of establishing a research institute in New York City. He stepped in and filled the void, negotiating the American Society away from the Society in London and reconstituting it in New York City as a section of his independent institute.⁴³ Hyslop's 14-year tenure at the helm of the ASPR was largely a one-man / one-woman operation in which virtually all functions were carried out by Hyslop and his secretary, Gertrude Tubby. The pages of

⁴² Minot (e.g., 1886, 1887, 1889) and others conducted screening tests to detect individuals with telepathic abilities, investigated hauntings and apparition cases (e.g., Royce, 1888, 1889a, 1889c) and so on (e.g., Royce, 1889b), but did not find evidence of any psychic functioning.

⁴³ See Berger, 1988, pp. 64-94 for a more complete biography of Hyslop and a detailed description of the ASPR under his stewardship.

both the journal and the proceedings of the Society during the Hyslop period were also largely a result of his personal industry.⁴⁴

After Hyslop's death, the ASPR enjoyed a brief time during which it seemed to be re-orientating itself as a scientific society for psychical researchers. British psychologist and Harvard professor William McDougall (1871-1938) was brought in as a long-distance President, remaining at Harvard whilst the ASPR itself continued in its offices in New York City. British psychical researcher and sceptic Eric Dingwall (1895-1986) was put in charge of research into physical phenomena, a post in which he served although he remained in London. Walter Franklin Prince (1863-1934), an American psychical researcher who had been the editor of the Society's publications before, and Acting Director of the ASPR, after Hyslop's death (Mauskopf & McVaugh, 1980, pp. 16-17), was made the manager of the general research functions of the Society. A 'Research Advisory Council' was established that included academic scientists, amongst them psychologist John Coover of Stanford University who had conducted some early card-guessing experiments, and psychologist Joseph Jastrow (1846-1935) who had been a member of the early ASPR (Berger, 1985, pp. 65-66; Mauskopf, 1980, pp. 16-17).⁴⁵

The mediumship of a highly controversial Boston woman, Mina Crandon, also known as 'Margery', was already causing tension in 1921 between the scientific and the non-scientific members. In 1923, much to his surprise, a powerful group of non-scientific members dismissed McDougall and allowed the Scientific Research Council to lapse (Mauskopf & McVaugh, 1980, pp. 19-21). Gertrude Tubby was named editor of the Society's publications, and the Reverend Frederick Edwards was named President.⁴⁶ McDougall and Prince remained members of the Society and served briefly on the committee investigating 'Margery'. By 1925, however, both McDougall and Prince had resigned from the Society (pp. 22-24).

⁴⁴ In fact in the period from 1907 to 1920, 67% of the articles published in the *Journal of the American Society for Psychical Research* were authored by Hyslop (Alvarado, 2003b).

⁴⁵ Jastrow was involved in the early ASPR when he was a graduate student at Johns Hopkins University under G. Stanley Hall. He became the first American PhD in psychology (Noonan, 1977), and along with his professor, Stanley Hall, a vociferous critic of psychical research (e.g., Jastrow, 1900, 1910, 1927a).

⁴⁶ Mauskopf & McVaugh (1980) note that more than 100 members left in response to the 'coup' (p. 21).

Prince founded the Boston Society for Psychical Research — another largely one man / one woman operation; that of Dr Prince and his secretary Lydia Allison (1880-1959). The BSPR remained active until Prince's death in 1934 (Allison, 1956).⁴⁷

From 1923 until the 'Palace Revolution' of 1941, the ASPR was in the grips of individuals without scientific training, most of whom were Spiritualists, and many of whom were enthusiastic supporters of the 'Margery' mediumship, which later became regarded as totally fraudulent by academic researchers in the field (Prince, 1926; Rhine & Rhine, 1927). Both the quality of the research done by members of the ASPR in the period from 1923 to 1941, and the quality of its publications, declined precipitously.⁴⁸

The core of scientific parapsychology shifted away from New York City in the 1920s, first to Boston and the Boston Society, and then to Duke University in Durham, North Carolina. In 1927, William McDougall left Harvard University to take over the chairmanship of the Philosophy and Psychology Department at Duke University. Having begun to retrain themselves in psychology at Harvard, University of Chicago-trained botanists J. B. Rhine (1895-1980) and Louisa E. Rhine (1891-1983) followed McDougall to Duke. Initially J. B. Rhine worked analysing transcripts of mediumship sessions but soon focused on the task of operationalising the phenomena of psychical research into simple laboratory tasks involving cards and dice (Mauskopf & McVaugh, 1980, p. 80).⁴⁹

⁴⁷ Mauskopf & McVaugh (1980) wrote that William McDougall, after the ASPR, had 'in effect given up on societies as the vehicle for his concerns and was looking hopefully to the American university as a more promising context for scientific psychical research' (p. 23). McDougall's (1927) article on 'psychical research as a university study' and his later efforts to establish serious scientific psychical research at Duke University provide evidence of his change of focus. See Mauskopf & McVaugh, 1980, pp. 44-70, for a survey of early psychical research in the university context in the United States.

⁴⁸ It is interesting that the most recent 'coup' in the history of the American Society for Psychical Research that occurred in the mid-1980s, was once again a struggle between scientific members — this time identified mainly with the Parapsychological Association — and non-scientific members who were pushing for a renewed focus on the needs of the general public. In this particular struggle, the 'popular' faction gained dominance. The lines were not as clearly drawn as they had been in the 'Palace Revolution' as the President, Executive Director and some of the remaining board members of the ASPR were PA members, albeit with more popular and clinical interests than some of the scientific parapsychologists who resigned.

⁴⁹ A deep biographical account of J. B. and Louisa E. Rhine, as important as they are to the history of parapsychology, is well beyond the scope of this thesis. Mauskopf and McVaugh's (1980) volume, whilst ostensibly a 'biography' of experimental parapsychology up to 1940, serves also as an early biography of the Rhines. For other biographical and autobiographical material see Berger (1985, pp. 194-231, 251-260), Brian (1982), Rhine & Rhine (1978), and Rhine (1983).

A number of professionalising events issued from the Rhine group at Duke University, amongst them the publication of Rhine's (1934a) monograph, *Extra-sensory Perception*, the founding of the *Journal of Parapsychology* in 1937, and the publication of the Rhine team's reply to criticism, *Extrasensory Perception after Sixty Years* (Pratt, Rhine, Smith, Stuart & Greenwood, 1940).⁵⁰

In 1941, psychologist Gardner Murphy (1895-1979), clinical psychologists Montague Ullman and Jule Eisenbud, and others, were part of a group that accomplished the so-called 'Palace Revolution', returning the American society to the hands of the scientists (see, for example, Osis, 1985). In 1951, the Parapsychology Foundation was established in New York City by the Irish medium Eileen J. Garrett and the American Congresswoman Frances P. Bolton. The Foundation, which did not lack for funds during the lifetimes of its two founders, sponsored research in its facility, began a series of international scientific conferences, and established a research library (e.g., Ullman & Krippner 1970).⁵¹

A several-decades long period of 'normal science' in the Kuhnian sense (if seen from within the boundaries of the field) is apparent in the biographies of individuals who established laboratories or worked as researchers in one or more active sites in the U.S. from the 1940s through the 1980s (e.g., Berger, 1985; Krippner, 1975; Pilkington, 1987; Pratt, 1964; Rhine, 1983). Perhaps the most important of these was Charles Honorton, whose career included stays at the Foundation for Research on the Nature of Man (FRNM) (the successor to the Duke Parapsychology Laboratory, now called the Rhine Research Center), the Maimonides Dream Laboratory in Brooklyn, New York, at his

⁵⁰ Rather than characterise this output by samples of published papers and reports, the reader is directed to Chapter 5 of this thesis which focuses on the period from 1934 to 1944, and to the content of two commemorative volumes on the work of J. B. Rhine (Rao, 1983) and L. E. Rhine (Rao, 1986), as well as to my quantitative history of gender and publishing in American parapsychology (Zingrone, 1988).

⁵¹ The most important research to come out of this short-lived laboratory was that on dream telepathy conducted by Montague Ullman and his colleagues referenced above. Once the Parapsychology Foundation's laboratory closed, Ullman's dream telepathy research group moved into his sleep laboratory at the Maimonides Medical Center in Brooklyn.

own Psychophysical Research Laboratories in Princeton, New Jersey, and at the Koestler Parapsychology Unit at the University of Edinburgh until his death in 1992.⁵²

From the late 1930s through the early 1970s, J. B. Rhine's group remained at the centre of experimental parapsychology. Then, a power struggle between Rhine and his researchers over acceptable areas of experimentation caused an exodus of some key individuals from the laboratory, amongst them Charles Honorton, mentioned above, and the late Koestler Professor of Parapsychology here at Edinburgh, Robert L. Morris (Brian, 1982). Only a few years later a case of fraud involving a young man Rhine had put into the FRNM directorship caused another reorganisation (Rogo, 1985). Even with these set-backs, however, the laboratory continued to train new researchers and conduct significant research from the late 1970s through the mid-1990s.

In the late 1970s research laboratories sprang into being at Princeton University (e.g., Jahn & Dunne, 1987; Dunne & Nelson, 1991) and elsewhere. Existing laboratories such as Mind-Science and Science Unlimited Foundations in San Antonio, Texas added parapsychological research to their purview (e.g., Braud, 1990; Heseltine, 1985; Schmidt, 1985) as did Stanford Research Institute (SRI) in Palo Alto, California (e.g., Targ & Puthoff, 1977; Tart, Puthoff & Targ, 1980), either by actively pursuing a program of research, or by tolerating or even encouraging the personal interests of staff members employed to do other types of more conventional research.

Important research was coming out of the Division of Parapsychology at the University of Virginia (since called the Division of Personality Studies, and recently renamed the Division of Perceptual Studies) (e.g., Stevenson, 1983), the Communications Department at Syracuse University (e.g., Morris, Nanko & Phillips, 1982), the psychology departments of the City College of the City University of New York under the supervision of Gardner Murphy's protégé, the psychologist Gertrude R. Schmeidler (e.g., Maher, Peratsakis & Schmeidler, 1979), and from St. John's University in Jamaica, New York, (e.g., Stanford, Frank, Kass & Skoll, 1989a, 1989b), to name a few.

⁵² For more information on Honorton's career, see Rao (1993).

Research in these institutions was largely experimental, focusing on telepathy, clairvoyance, and psychokinesis. Reincarnation, out-of-body experiences, near-death experiences, and survival research were also studied. During the period, classified research into remote viewing was also being conducted at Stanford Research Institute, and at other locations on behalf of the U. S. Department of Defense (e.g., Broughton, 2003).

Over the same decades publishing outlets in the United States expanded from the journal and proceedings of the American Society for Psychical Research and the *Journal of Parapsychology* to the *International Journal of Parapsychology* and the Parapsychological Monographs series produced by the Parapsychology Foundation.⁵³ Its more popular periodical, *Parapsychology Review*, was also being published by the Foundation as well as the proceedings of its international academic and scientific conferences. A variety of other speciality journals such as *Psi Research*,⁵⁴ and the *Journal of Scientific Exploration* also appeared. The Parapsychological Association⁵⁵ published annual volumes of convention abstracts in its series, *Research in Parapsychology*.⁵⁶ An edited series of review volumes was also established called *Advances in Parapsychological Research*, of which eight have appeared and a ninth is in

⁵³ The *International Journal* was published for a decade only, from 1959 to 1968. It recently reappeared in 2000 but has fallen behind schedule because of the financial burdens under which the Foundation currently labours.

⁵⁴ *Parapsychology Review* was published from 1970 to 1990, and *Psi Research* from 1982 to 1986.

⁵⁵ As mentioned in Chapter 1, the Parapsychological Association (PA), the international 'union' of scientific parapsychology was founded during this period, in 1957. Its bid for affiliation with the American Association for the Advancement of Science was accepted in 1969, a fact which is often offered as an indication of the scientific legitimacy of the PA but which, in my opinion, has had little or no impact on the status of the science in the United States.

⁵⁶ *Research in Parapsychology* was published from 1977 to 1993. Due to financial constraints, the proceedings from 1994 through 1997 will be published on the PA's website and not produced in print. Abstracts and invited papers from the 1998 convention through the 2003 convention have been published in the *Journal of Parapsychology*. Because of financial problems at the Rhine Research Center, however, the future of the *Journal of Parapsychology* is also in doubt.

preparation. Finally, an informal, but very important debate journal, *Zetetic Scholar*, was in circulation.⁵⁷

By the mid- to late-1990s, however, the situation in the United States began to contract with the closing of Psychophysical Research Laboratories when funding was lost. Mind Science and Science Research Unlimited both shifted research programs away from parapsychology to other areas of science. Gertrude Schmeidler retired from City College and the Princeton Engineering Anomalies Laboratory at Princeton University began the process of closing as its director, engineer Robert Jahn, moved into retirement. Finally, the Rhine Research Center suffered a new series of restructurings of the laboratory's focus and staff from the mid-1990s through 2005.⁵⁸

The only academic unit remaining in the U.S. today is the Division of Perceptual Studies at the University of Virginia, but like the Rhine Research Center, the American Society for Psychical Research, and the Parapsychology Foundation, it is struggling with diminished financial circumstances. The only institutionally-based research laboratory left in the United States is that run by Dr Dean Radin at the Institute for Noetic Sciences, a general membership organisation founded by the American astronaut, Edgar D. Mitchell, and devoted to a wide variety of New Age causes.

The American Society for Psychical Research has also undergone new shifts in focus and management in recent years, during which some of the more scientific members resigned. On-site research at the ASPR has come to a halt as has the

⁵⁷ The *Zetetic Scholar* was a unique debate journal, informally produced by sociologist Marcello Truzzi who provided parapsychology and other marginal areas of science with a venue for productive confrontation in the 1980s. Eleven one-issue volumes appeared until it ceased publication, mainly due to the press of Truzzi's other interests and commitments.

⁵⁸ The Rhine Center's management changed in 1994 through the firing of K. Ramakrishna Rao as director and again in the late 1990s, when then-director Richard S. Broughton and researcher Cheryl Alexander resigned. Disputes with the Center's board of directors over proper fiscal and research management led to the former change. The latter revolved around disputes over the laboratory's research direction. Since then the Rhine Center has suffered through several more changes in management and direction, owing to financial difficulties and a clash of agendas between scientific and non-scientific staff and volunteers. In fact, until recently the Rhine Center had only one paid staff member, the British psychologist Dr Christine Simmonds. Since she returned to the U. K. in September of 2005, the Rhine Center has ceased to conduct research, is run wholly by volunteers, and has refocused itself as a general membership organisation dedicated to public education.

publication of the *Proceedings* and the future publications of its journal is also in doubt.⁵⁹ The few independent researchers who are still active in the United States have continued to conduct studies, however, and are well-represented in the annual conventions and in the pages of the journals that are still operating, even if the financial situations of these researchers are also precarious.⁶⁰

One should not take away from this brief outline of the history of Anglo-American parapsychology the idea that there is something very different about its varying organisational structures or the waxing and waning of its tides of research and publications output from the rest of science. In some branches of science, discovery and domination has shifted from society to laboratory to university and back again in a dance of influences, interests and substantive contributions (e.g., Kusch, 1995; Mayr, 1982). If the histories and sociologies of science teach us anything, it is that there are some very pronounced commonalities between scientific parapsychology and other more mainstream branches of science (e.g., Collins & Pinch, 1979).⁶¹ But, to be blunt, parapsychology also shares a great deal of common ground – especially in the United States at the moment – with branches of science which have disappeared or which suffer still under the heavy burden of hardened scepticism and criticism.⁶²

⁵⁹ The American Society is currently doing spontaneous case research on ostensibly precognitive experiences related to the World Trade Center attack in 2001, as well as scanning its extensive archives for future preservation. It does hopes to continues to publish its journal, albeit on a delayed schedule (Keane, personal communication, June 2002).

⁶⁰ I am not providing bibliographic support of these assertions. As a two-time president of the Parapsychological Association (2000-2001, 2003-2004), and as a member of the American scientific parapsychology research community since 1974, I have a great deal of personal familiarity with the situation in the field in the United States.

⁶¹ This particular article, however, has been criticised in the science studies literature because Collins and Pinch use the categories ‘parapsychologist’ and ‘orthodox scientist’ uncritically, failing not only to analyse the use of these terms by their informants but also failing to be reflexive about their own adoption of these rather vague and multiply-nuanced categories (Mulkay, Potter & Yearley, 1983, pp. 185-188).

⁶² A number of good historiographies of superseded, displaced, ‘premature’ or pseudosciences exist. See, for example, the anthology edited by Hanen, Osler & Weyant (1980). Sociologists of science Harry Collins and Trevor Pinch have also dealt with a number of these struggling disciplines and sub-specialties in their ‘Golem’ series (1993, 1998).

Conclusion

In this chapter I have outlined briefly the intertwined histories of psychical research and parapsychology in Great Britain and the United States. Two different patterns have emerged. In Great Britain, the field's primary institution, the Society for Psychical Research in London, was founded by a coalition of academics and spiritualists for the purpose of research. Although the tension between the competing agendas of scientists and the general public remain a part of the SPR's identity, the Society is active in providing significant support for the scientific side of the field through funding, its research library and publications, and its annual conference, amongst other things. The Perrott-Warrick Fund at Cambridge University, and the efforts of both John Beloff, and the late Professor Robert L. Morris at the University of Edinburgh from 1970 through 2004 have helped to expand greatly the presence of active academic and scientific units in British universities.⁶³ Such units not only continue to grow, gain funding, produce research, and mentor new scientists, but they are also dedicated to the study of putative psychic functioning from both the paranormal point of view, and from a more conventional point of view, thus incorporating the work of both proponents and critics into a single problem domain.

In the United States, on the other hand, although the origins of the field lay in efforts to establish a scientific discipline, the tension between the general public and the scientific side of the field has erupted many times in dramatic shifts in social and organisational power. The American Society for Psychical Research, for example, suffered repeated "coups" in which the dominance of the general public alternated with that of scientists and academics.

⁶³ Beloff trained a number of scientists in parapsychology who have worked in the United States, Europe and elsewhere such as: Richard Broughton and Deborah Delanoy, who are now both senior faculty members at the University of Northampton in England; Adrian Parker, at the University of Gothenburg in Sweden; and Michael Thalbourne at the University of Adelaide in Australia. Some of Morris's students — although the majority are employed in Great Britain — have also taken positions in other countries, such as Carlos S. Alvarado and I in the United States, Robin Taylor in Fiji, and Ricardo Eppinger in Brazil. So although Beloff and Morris largely built scientific parapsychology in Great Britain, their reach has extended elsewhere as well.

University-based research in the United States began a period of intense activity in the 1930s and continued through the mid-1990s, with, at its height, research being conducted at a half dozen university sites around the country. Private research facilities were also in operation, especially from the early 1980s through the mid-1990s. Since the mid-1990s, however, the field has been in a precipitous decline. University-based research is all but gone. Very few private research laboratories still exist, and those that do are shifting their focus away from research.

The core of English-speaking scientific parapsychology, then, began in 1882 in London, shifted to Durham, North Carolina in the mid-1930s, and back to Great Britain in the late 1980s. The cognitive content of the field has contracted somewhat over the duration of this history, with Reichenbach phenomena no longer investigated, and the mesmeric tradition continued through studies of hypnosis and other altered states and their relationship to putative psychic functioning. The importance of mediumship and field research has diminished since the last decades of the 19th and early decades of the 20th century. The field became largely experimental in the United States from the 1930s forward and in Great Britain from the 1970s onward. In recent decades, spontaneous case research, field investigations and mediumship research have had a resurgence, but, on both sides of the Atlantic, parapsychology is still largely an experimental science.⁶⁴

However few in number the core group of researchers has been over the duration of the field's history, there has always been an overt interest — as can be seen in the published literature — in refining methodology and theory, and in responding substantively to criticisms raised by mainstream scientists: that is, in scientific 'progress'. This interest, and its concomitant points of controversy, will be contextualised in Chapter 3 through a review of those authors across the history of the field who have attempted to survey criticism and response in a systematic way. Through this review I hope not only to make visible a sense of the content of the many controversies that have beset the field, but also to narrow the focus from the wider map

⁶⁴ Again, I am not providing specific references here, although a number of review chapters published in the *Advances* series (e.g., Palmer, 1979) as well as the content of the *Proceedings of Presented Papers*, from the annual Parapsychological Association, illustrate my points.

of the historical context to the more persistent features of the argumentative terrain in parapsychology.

CHAPTER THREE

PREVIOUS REVIEWS OF CRITICISM

In this chapter I survey a selection of reviews of criticism that have appeared in the English-language literature from 1926 to 1998. Because my purpose is contextual, I will not provide evaluations of the accuracy of the criticism that underlies the reviews. I hope to provide the reader not only with a sense of what specific criticisms have been raised over the history of the field, but also to show how the controversies, in general, have been characterised. Therefore, the reviews function here as broad geographical features on the controversy landscape, with specific points of criticism and response raised within them providing a closer glimpse of the argumentative map.

It seems to me that, in broad outline, criticisms and counter-criticisms⁶⁵ have fallen into two general categories: the cognitive/logical/scientific and the social/psychological/religious. By the former I mean criticisms of methodology, modes of analysis, the plausibility of the existence of the phenomena under study, and the persuasiveness of proposed explanatory models. By the latter I mean characterisations of methodological and intellectual competence, personal motivations, emotional stability, and the ability to set aside personal biases and foibles so as to attain the scientific ideal of 'objectivity'.

These two broad outlines are similar to what Gilbert & Mulkay (1984), in a more general study of scientific prose, have called the 'empiricist repertoire' and the 'contingent repertoire', respectively.⁶⁶ However, over the history of criticism and response in the parapsychological literature, in some instances the 'heat' of the exchanges seemed to signal that something else exists in these controversies that is much deeper than the 'mere' deployment of argumentative strategies. There is something here which signals that the interlocutors enjoy widely differing levels of social power and that the argumentative 'stake' proponents would attribute to critics and vice versa is very different.

⁶⁵ Criticism is levelled against parapsychology by sceptics and critics. Counter-criticism is levelled against sceptics and critics by parapsychologists, or 'proponents'.

⁶⁶ A more in-depth discussion of that work and their conception of these two repertoires is presented later in this chapter.

The Terrain of Criticism and Response in Parapsychology

Over the course of the history of parapsychology there have been a multitude of major and minor controversies.⁶⁷ In the English-language literature alone, nearly every issue of the field's publications contains at least a single entry in some controversy, whether it is research designed to refute or incorporate substantive or methodological criticism,⁶⁸ disputes over methodology and interpretation in field investigations and experiments,⁶⁹ or debates on statistical issues,⁷⁰ to name but a few.

In addition to internal publication outlets, a wide variety of scientific journals outside the field have been the site of some very important controversies, amongst them *Science* magazine⁷¹ and its British counterpart, *Nature*.⁷²

⁶⁷ I will not be dealing in this chapter with the compilation of criticism which is Pratt et al.'s *Extrasensory Perception after Sixty Years*, nor with any of the review papers (e.g., Pope & Pratt, 1942) that were incorporated into that text, because these form part of the case study in Chapter 5. Neither will I be dealing with John Palmer's commentaries on criticism (e.g., Palmer, 1986a, 1986b, 1987b, 1987c, 1987d) which are theoretical statements on criticism and controversy. In addition two other articles were excluded, one because it focused solely on a single type of experiment (Akers, 1984), and another because it did not treat criticism or response in the wider sense (Child, 1985).

⁶⁸ See, for example, Dunne & Bisaha, 1979; Pratt et al., 1940; Sidgwick & Committee, 1894.

⁶⁹ For example, controversies over: an experiment in 'psychography' (e.g., Hodgson & Davey, 1887; Massey 1887); the Nellie Morris mediumship (e.g., Jenkins, 1888 Sidgwick, Mrs H. [E. M.] 1888a, 1888b, 1888c, 1888d, Wallace, A. R., 1888a, 1888b, 1888c); the Kern city poltergeist (e.g., Betty, 1984, 1985a, 1985b, 1985c, Hövelmann & Zorab, 1985, Stevenson, 1985); and ganzfeld methodology (e.g., Alcock, 1986, Child, 1986, Honorton, 1985, Hövelmann, 1986, Hyman, 1985a, Hyman & Honorton, 1986, McClenon, 1986, Palmer, 1986c, Rosenthal, 1986, Stanford, 1986, Stokes, 1986, Utts, 1986).

⁷⁰ See, for example, Burdick, 1979; Child, 1977; Feller, 1940; Greenwood, 1938; Greville, 1941; Kreidler & Kreidler, 1977; Leuba, 1938; and Stanford & Palmer, 1972.

⁷¹ Amongst these were controversies over: the statistics used by the Rhine group in the 1930s (e.g., Huntington, 1937; Sterne, 1937); a suggested methodology for testing ESP (e.g., Smith & Canon, 1954; Murphy, 1954; Nash, 1954; Rhine, 1955g; Smith, 1956); and the putative ability to 'read' through the skin, that is, 'dermo-optics' (e.g., Brewer, 1966; Buckhout, 1966; Gardner, 1966; Makous, 1966; Weintraub, 1966; Zubin, 1966).

⁷² Amongst these were controversies over: a letter on 'Science and Psychical Research' published by R. J. Tillyard's (1926a) which started a very active exchange of letters to the editor (e.g., Dingwall, 1926; Donkin, 1926a, 1926b; Doyle, 1926a, 1926b, 1926c; Editor, 1926a, 1926b; French, 1926; Lodge, 1926; Lotsy, 1926; Rayleigh, 1926; Richet, 1926, Swinton, 1926a, 1926b, 1926c, 1926d, 1926e; Tillyard, 1926b, 1926c, 1926d, 1926e); sceptic C. E. M. Hansel's critique of the work of S. G. Soal and through Soal, experimental parapsychology in general (e.g., Hansel, 1959a, 1959b, 1960a, 1960b, 1960c, Soal, 1960a, 1960b), which work has since been generally discredited inside the field (e.g., Barrington, 1974; Markwick,

The reviewers of published criticism sought to: obtain substantive closure on some particular point of theory or methodology; examine the quality and purpose of criticism rendered; or understand why parapsychology, its phenomena and findings, are so hotly contested. The reviewers I have chosen are persons who were, or are, considered to be members of the parapsychological community. By that I mean that they have — through their writing, their employment, and/or their participation in various avocational and professional organisations in the field — shown themselves over the course of their careers to be ‘insiders’. In the main, they are ‘proponents’ although some amongst them, such as the Stanford Professor of Psychical Research, John E. Coover, were generally sceptical about the existence of the phenomena, whilst others, such as the late sociologist of deviance Marcello Truzzi, considered themselves to be neutral.⁷³ The conclusions presented here should not be taken as ‘unbiased’ or ‘true’ but rather as examples of the attempts that have been made to deal with criticisms raised over the years, and of the breadth and depth of the argumentative terrain.

In Table 1 the reviews are presented with the community and orientation of the reviewers, the focus of the review, the primary type of criticism and response covered, and the primary thesis of the review noted. More information on the specific classifications follow the table.

1974; Mundle, 1973, 1974; Pratt, 1974; Scott & Haskell, 1973, 1974, 1975; Smythies, 1974; Stevenson, 1974; Thouless, 1974) with some few exceptions (e.g., Beloff, 1974b); the methodology used in remote viewing research (e.g., Marks, 1981; Marks & Kammann, 1978; Puthoff & Targ, 1981; Tart, Puthoff & Targ, 1980); and general issues surrounding the field which were discussed in correspondence titled both ‘Investigating the paranormal’ and ‘On paranormal theories’ (e.g., Couch, 1986; Elitzur, 1986; Marks, 1986; Morris, 1986; Stevenson, 1986).

⁷³ As will be seen later in this chapter, Truzzi is identified in the overall description of the chapter as a member of the parapsychological community although in the table describing Child’s review he is identified as an ‘external’ critic. In the former, I have classified him as an ‘insider’ because of his participation in the parapsychological community over the last two decades. At the time Child’s review was published, however, Truzzi was still mainly identified with the sceptical community.

Table 1.

Overview of the Reviews of Criticism and Response

Source	Community /Orientation	Focus of Review	Primary Type of Criticism/Response	Primary Criticism of Critics/Proponents
Sudre, 1926	Psychical research, proponent	Published criticism of séance room investigations	Contingent: motivations, beliefs	Critics compromised by materialist worldview
Coover, 1927	Psychical research, sceptical	Same as above	Empiricist: appropriateness of methodology	Séance room phenomena cannot be investigated scientifically because of conditions of occurrence
Prince, 1927	Psychical research, proponent	Published criticism	Contingent: motivations, beliefs	That psychological and intellectual 'failings' critics attribute to proponents are more true of critics, i.e., 'Enchanted Boundary theory'
Prince, 1930	Psychical research, proponent	Same as above	Contingent: motivations, beliefs	Same as above
Prince, 1933a & b	Psychical research, proponent	Same as above	Contingent: rhetorical choices of critics	Listing 'illegitimate' criticisms
Tyrell, 1947	Psychical research, proponent	Same as above	Contingent: motivations, beliefs	That critics are compromised by materialist worldview, i.e., 'Enchanted Boundary theory'
Nicol, 1956	Parapsychology, proponent	Rhinean School research program	Empiricist: methodology, quality of evidence	Why experimental parapsychology is not persuasive
Stevenson & Roll, 1966	Parapsychology, proponent	Published criticism and response	Contingent/Empiricist: rhetorical construction of response, use of evidence in response	Rhetorical choices of proponents contribute to persistence of controversy
Ransom, 1971	Parapsychology, proponent	Same as above	Contingent/Empiricist: source and applicability of criticism	'Errors' made by both critics and proponents
Thouless, 1971	Parapsychology, proponent	Published criticism	Same as above	Critical evaluations when not 'accurate' are result of worldview
Rogo, 1975	Parapsychology, proponent	Previous reviews of criticism	Contingent: motivations, beliefs	Persistence of criticism can only be explained by critics' worldview
Honorton, 1976	Parapsychology, proponent	Published criticism	Empiricist: methodology, findings, theory	Persistence of criticism explained by substantive failings in parapsychology
McConnell, 1976	Parapsychology, proponent	Critical arguments	Contingent: motivations, beliefs of both critics and proponents	Persistence of criticism explained by competing worldviews

Table 1. continued

Source	Community /Orientation	Focus of Review	Primary Type of Criticism/Response	Primary Criticism of Critics/Proponents
McConnell, 1977	Parapsychology, proponent	Published criticism	Contingent/Empiricist: source and applicability of specific criticisms	Persistence of criticism explained by differing <i>a priori</i> probability assessments of the likelihood that the phenomena exist
Rockwell, Rockwell, & Rockwell, 1978	Parapsychology, proponent	Published criticism from <i>Free Inquiry/The Humanist</i>	Contingent: rhetorical construction of criticism	Rhetorical strategies used by critics in these periodicals inappropriate for scientific debate
Bauer, 1984	Parapsychology, proponent	Previous reviews of criticism	Contingent/Empiricist: source and applicability of specific criticisms	Commonalities and differences of criticisms raised over the history of the field; motivations, beliefs involved in persistence of criticism
Stokes, 1985	Parapsychology, sceptical proponent	Published criticism	Contingent/Empiricist: source and applicability of specific criticism	That criticism may be categorised as either 'rational' or 'extra-rational'
Child, 1987	Parapsychology, proponent	Published criticism	Contingent: source of specific criticisms	Descriptive review of both 'internal' and 'external' criticism
Ellison, 1989	Parapsychology, proponent	Critical argument	Contingent: motivations and beliefs	To deal effectively with critics, proponents must understand critical 'mentality'
Honorton, 1993	Parapsychology, proponent	Published criticism	Contingent/Empiricist: source and applicability of specific criticisms	Critical review of criticisms raised
Keen, 1997	Psychical research, proponent	Published criticism	Contingent: motivations, beliefs	Persistence of controversy explained by sceptics' motives and beliefs
Radin, 1998	Parapsychology, proponent	Specific criticism	Contingent: motivations and beliefs	Rhetorical choices made by critics influenced by inadequate understanding of parapsychology, reliance on media over-simplification instead of published literature, personal motivations and beliefs
Truzzi, 1998	Parapsychology, self-characterised as neutral	Critical arguments	Contingent: rhetorical elements of critical arguments	Rhetorical choices made by critics and proponents contribute to the persistence of controversy

As can be seen two communities are listed: psychical research and parapsychology. Psychical research is defined here as the pre-Rhinean paradigm, a key

element of which being séance room investigation. Sudre, Coover and Prince are categorised as psychical researchers, for example, both because their reviews focused mainly on literature that deals with séance room phenomena and because their work occurred before experimental parapsychology became established as the dominant paradigm in the field (e.g., Nilsson, 1975, 1976). In addition, although Coover also contributed experimental studies to the literature (e.g., Coover, 1917) and Prince was generally supportive of the experimental turn that occurred with the work of J. B. Rhine in the 1930s,⁷⁴ both considered themselves to be psychical researchers. I categorised Montague Keen as ‘psychical research’ because his research interest in paranormal phenomena was avocational and focused on séance room phenomena.

Four types of orientations to the field are also listed on Table 1: ‘proponent’, ‘sceptical’, ‘sceptical proponent’ and ‘neutral’. Proponents are individuals who have a positive view of the phenomena, the methods, and findings of parapsychology, and have generally been workers in, and/or defenders of, the research done in the field. John Coover is listed as having a ‘sceptical’ orientation because he concluded that his experimentation disproved the existence of clairvoyance and telepathy (Coover, 1917), an interpretation that has been hotly contested by others (e.g., Rhine, 1934a; Thouless, 1935). Douglas Stokes has been categorised as a ‘sceptical proponent’ because, although he is critical of much of the experimental and theoretical literature of parapsychology (e.g., Stokes, 1986), he has published mainly in parapsychological journals. Marcello Truzzi, on the other hand, is categorised as ‘neutral’ on the basis of his self-professed orientation to the field. However, he is considered to have been a member of the parapsychological community because of his regular participation at Parapsychological Association conventions until his recent death.

The column ‘Focus of Review’ characterises the content of each review. As can be seen on Table 1, nine reviewers focused on published criticism in a general way, five reviewers focused on critical arguments, two focused on criticisms of investigations of séance room phenomena, two focused on previous reviews of criticisms, two focused

⁷⁴ That this was so can be seen in his general support of J. B. and Louisa E. Rhine, and in his specific support of the publication of Rhine’s (1934) monograph, *Extra-Sensory Perception*, as will be seen in Chapter 5.

both on published criticism and published response, one focused only on criticism that had been published in the secular humanist periodical *Free Inquiry/The Humanist*, and one focused only on criticism of the Rhinean School's experimental parapsychology research program.

The column 'Primary Type of Criticism/Response' classifies the reviews by whether they rely on a 'contingent' or an 'empiricist' repertoire when analysing criticism or response. As can be seen on Table 1, seven individuals relied heavily on characterisations of critics' and/or proponents' motivations and beliefs as the source of criticisms raised, three focused mainly on the rhetorical choices made by critics and/or proponents in the published works reviewed, four focused on methodology, findings and theory, and seven combined analyses of the accuracy and applicability of criticism with speculations on the motivations and beliefs of the critics.

The column 'Primary Criticism of Critics/Proponents' gives slightly more detail about the overall thesis of the reviews. These were rather more variable and as can be seen from Table 1 ranged from the simple thesis that some critics allow their *a priori* commitment to the materialist worldview influence their evaluation of parapsychological research and theory, to the more complex thesis that rhetorical choices made by critics are influenced by an inadequate understanding of the field's substantive content, by a reliance on media over-simplification as a source of their knowledge about the field, and/or by personal motivations and beliefs.

The Contingent and Empiricist Repertoires

In 1984 Gilbert and Mulkey proposed that the discourse of scientists provided evidence that '... [scientific] accounts are organised to portray ... [the] actions and beliefs [of scientists] in contextually appropriate ways' (p. 14). They divided the discourse of scientists into two repertoires, the contingent and the empiricist. By empiricist, they meant a form of accounting for actions or beliefs that relied on a depiction of underlying objectivity such as one would find in the methods section of experimental reports. That is:

... the form of accounting used to depict scientists' actions in methods

sections [that] seems to be more or less explicitly an attempt to extract certain invariant dimensions from the unique specific actions carried out by particular researchers in particular laboratories and to embody these dimensions of action in general, impersonal rules which can be followed by any competent researcher. (p. 52)

Accounting that was empiricist served to render the personal agency of the individual scientist 'less visible' whilst giving the impression that the arguments raised were 'derived neutrally from the facts' (McKinlay & Potter, 1987, p. 446).⁷⁵ In Gilbert and Mulkey's (1984) case studies of the 'talk' of scientists, they found that scientists were much more likely to use the empiricist repertoire when they were describing their own experimental work and/or their own theoretical positions. That is, it was typical of scientists to 'speak as if their own position is an unproblematic and unmediated representation of the natural world' (p. 68) and as if their 'voice and that of the natural world are one and the same' (p. 89).

Accounting that was contingent, on the other hand, focused on those aspects of science practise that did not appear in an experimental report (p. 41), that is, personal and social influences that contribute to one's competence as a scientist, one's ability to experiment, to see the 'facts'; in short, the 'intrus[ive] ... non-scientific influences' (Mulkey & Gilbert, 1982, p. 165). The contingent repertoire seemed to them to be a creative one, a process in which the attributions to social and psychological influences were fluid and able to accommodate new views of the 'facts' (pp. 83). Further, attributing such contingent motivations to one's opponents was a kind of 'logical necessity' that followed on an individual's belief that if they are right about the scientific facts at hand, then their arguments must be correct, their science practise must be competent, and their opponent obviously wrong. The scientist who saw him- or herself as 'right' thus spun a contingent account to explain how a colleague had gone wrong, how the opponent's competence as a scientist had been compromised (p. 79).

⁷⁵ McKinlay and Potter's (1987) extension of Gilbert and Mulkey's (1984) work examined psychologists' discourse when dealing with 'top down' and 'bottom up' theoretical approaches. The discourse examined was talk drawn from public exchanges in scientific conventions. In their study they found that, in addition to accounting for the error of others, the interlocutors also showed a need to 'maintain the coherence of their own positions. ... [and] to avoid potentially disruptive blamings' (p. 457). The latter was accomplished

When Gilbert and Mulkay expanded their study to include an examination of how consensus is formed, the contingent repertoires that were employed under conditions of negotiation included issues related to community membership (pp. 135-140), and to credibility and authority (p. 125).

McKinlay and Potter (1987) also found that depictions of an individual's own position were likely to include assertions that he or she could be described as a scientist and as *doing* science 'objectively' and competently, whilst depictions of the opponent's position were likely to include the assertion that even if the opponent could be characterised as a scientist, his or her scientific practise must have been influenced by social motives, by politics, or by other social or psychological factors simply because they *were* opponents (p. 447).

In reading over the texts I had gathered in the history of criticism and response in parapsychology, it seemed to me that these two repertoires were clearly in evidence. I disagree then with Myers (1990) who found Gilbert and Mulkay's notion of empiricist and contingent repertoires to be 'a cumbersome analytical tool', one which Myers felt was not useful, especially for a reception study of a particular text (p. 28):

... these two categories seem to owe their existence to a polemic against the idea that anything lies behind the text ... [and that the method forced the] selection of some features [to fit one interpretation or the other]... . (p. 29)

Although the analysis of ways in which scientists account for error can, and has been moved to a deeper level (e.g., Michael & Birke, 1994), it seems to me that at the descriptive level, the notion is not only useful but elegant, and certainly, in my opinion, applicable to the texts at hand.⁷⁶

largely by making critical statements sufficiently vague as to avoid naming an individual present who might be inspired to reply.

⁷⁶ To some extent, I think Myers' criticism is born of a fairly common reaction to texts seen as part of the social constructionist literature of science studies. That is, there is the need amongst some analysts to see any attempt to deal with scientific text *qua* text as a denial of the existence of the cognitive content of science or, in the extreme formulation of the criticism, as a denial of the existence of the natural world (see, for example, Koertge, 2000). In Myers' (1990) case, this concern is to some extent paradoxical as his work focuses on the rhetoric of certain scientific documents and could be perceived by some analysts as belonging to the same tradition as that of Gilbert and Mulkay.

Accounting for Error in the History of Criticism and Response

In this section of the chapter I will describe briefly each of the reviews grouped together by the dominant repertoire available in the text. Following this, I will give some examples of accounting for error drawn from the specific repertoire under discussion.

Reviews of Criticism Dominated by the Empiricist Repertoire

John E. Coover

Coover's (1927) review was built on an argument against the phenomena of psychical research that stressed, as the title indicated, 'Metapsychics and the incredulity of psychical researchers' (p. 239). Coover was responding to a counter-criticism that had appeared in the *Journal of the American Society for Psychical Research* (Gerhardt, 1926) in which the author had asked why psychologists had arrayed themselves so strongly against psychical research. Coover replied:

The opposition of the psychologist is probably stronger than that of his fellow scientists because much of the detail in his particular field of knowledge has an especial pertinence to the evidence and methods of metapsychics. (p. 229)

In addition, Coover was reacting to Sudre's (1926) article discussed in the next section which complained that the reception séance room investigators and their findings had received from mainstream science stemmed, Sudre felt, from ignorance of the substantial improvements in methodology that had made 'metapsychics' the equivalent of any science.

To answer these counter-criticisms, Coover focused on descriptions of the methods and results of mediumship research available in the writings of such proponents as Charles Richet (1850-1935),⁷⁷ the published literature on the Fox Sisters (e.g., Austin, 1850; Capron & Barron, 1850; Flint, Lee & Coventry, 1851), and, to illustrate how

⁷⁷ Specifically Coover focused on *Thirty Years of Psychical Research* (Richet, 1923) and, he claimed, *Traite de Métapsychique* (Richet, 1922) which was the second edition of the work. I am using the term 'claim' because *Thirty Years* was an English translation of *Traite de Métapsychique* and not a different book, which is how Coover uses it.

deliberate deception and the notorious inaccuracies of eyewitness testimony might confound the evidentiality of séance room investigation, a number of books and articles on fraudulent mediums (e.g., Sidgwick, 1886; Truesdale 1892).⁷⁸

Amongst his specific comments, Coover set aside Richet's 'faggot theory' argument (Coover, 1927, p. 232) that too much evidence for the reality of séance room phenomena has accumulated over time to dismiss the entire set of findings on the basis of any new results, even if they were shown to have been fraudulently-produced.⁷⁹ The investigations into the Fox sisters' putative phenomena was especially damning, he thought:

The stream of negative evidence warns official science that all metapsychic phenomena may be illusory; may be but physiological, psychological or simple legerdemain. (p. 245)⁸⁰

In addition, Coover compared two lists of methodological elements in mediumship investigations and experimental science, respectively, to underscore his point that it was not possible to conduct 'science' in the séance room.

J. Fraser Nicol

Nicol's (1956) article was an invited presentation in the CIBA Foundation conference on extrasensory perception held in 1955. Like Coover, Nicol was presenting an argument of counter-criticism, that, in effect, accepted criticisms raised and extended

⁷⁸ It is interesting, however, that Coover chose to critique a psychical research that included nothing outside of the séance room, even though the materials he cited indicated that he was conversant with that wider literature to which he had himself contributed (i.e., Coover, 1917).

⁷⁹ The argument here is that mere quantity of data was sufficient to provide evidence, especially if the data can be shown to have avoided such systematic errors as those which arise from eyewitness testimony and/or from a self-selection reporting bias which could be expected to impact both the content and structure of case details (e.g., Rollo, 1967; West 1948). The 'faggot theory' — also called 'the bundle of sticks theory' — is generally used, however, without a sense of whether or not the database in question is, in fact, free of systematic errors. The controversy over theory underlies disputes over the methodology and interpretation of case collection aggregation (e.g., Rhine, 1969, Rhine, 1970a-b) and meta-analysis in parapsychology (e.g., Bem, Palmer & Broughton, 2001; Errata, 2001; Milton & Wiseman, 1999, 2001; Storm, 2000; Storm & Ertel, 2001; Storm & Thalbourne, 2000).

⁸⁰ Psychical researchers would certainly not argue that 'metapsychic' phenomena is *never* illusory, but would assume that the proper stance is to investigate things on a case by case basis.

them, calling for ‘insiders’ to refine their science in order to meet the requirements of critics. The four criticisms raised by wider science that Nicol felt entirely justified were: (1) the seeming irrelevance of parapsychology to other sciences; (2) the lack of any repeatable experiment; (3) disagreement amongst psychical researchers as to the quality of the evidence; and (4) claims made by ‘insiders’ that were unsupported by the published evidence as Nicol evaluated it (p. 26).

On the first point, Nicol noted that whilst Lodge (e.g., 1933, Chapter 20) had tried to tie the findings of psychical research to physics and Rhine the findings of experimental parapsychology to psychology (e.g., Rhine, 1934a, 1934b, 1934c), little else had been done to connect the field with the interests of mainstream science. On the second point, Nicol felt that methodological progress had been made over the history of the field, yet ‘psychical researchers had failed to produce one repeatable experiment’ (p. 28). The lack of repeatability, for Nicol, was a major impediment to scientific recognition.⁸¹ On the third point, Nicol felt that there was still sufficient disagreement amongst ‘insiders’ as to what constituted the best evidence that it was not possible to assume a consensus had been reached. To make this point he compared the ‘best evidence’ offered in Pratt et al.’s (1940) *Extra-sensory Perception after Sixty Years (ESP-60)* to the ‘best evidence’ offered in Soal and Bateman’s (1954) *Modern Experiments in Telepathy* and found that of the six studies endorsed in *ESP-60*, Soal and Bateman entirely ignored two, found one to be merely ‘fairly good’ (p. 30), felt that fraud was a plausible alternate hypothesis for another (p. 31), and considered an additional one to be ‘questionable’ (p. 32), leaving only one of the six as providing ‘best evidence’ (p. 31) in both sources. Nicol concluded, ‘Clearly, there is no unity of opinion amongst leading psychical researchers as to what constitutes valid evidence’ (p. 32).⁸²

⁸¹ Nicol’s contention that psychical research had no replicability was debatable even in 1956, in my opinion. However, as will be seen below, it is a criticism that has endured.

⁸² Modern evidence that a lack of consensus still exists is provided by the controversy over ‘seven evidential experiments’ which was published in the pages of the *Zetetic Scholar* in the early 1980s (target article: Beloff, 1980a; commentaries: Alcock, 1980; Beloff, 1980b; Child, 1980; Cohen, 1980; Collins, 1980; Morris, 1980; Musso & Granero, 1980; Nicol, 1980; Palmer, 1980; Randi, 1980a; Rao, 1980; Schouten, 1980; Scott, 1980a-b; Stanford, 1980), and by the on-line Ganzfeld debate of the late 1990s (Milton, 1999). It is debatable, however, whether consensus is really necessary for scientific recognition. Many modern science studies scholars focus on the *process* of achieving consensus — that is the continual

On the fourth point, Nicol noted J. B. Rhine's propensity to make claims for his data that went beyond previously published research reports, and/or seriously distorted the details found in those reports (pp. 34-36).

Charles Honorton

Charles Honorton's (1976) article, 'Has science developed the competence to confront the claims of the paranormal?', was the published form of his 1975 Preseidential Address. The question he sought to answer was posed originally by the editor of *Nature* in an introduction to an article by physicists Russell Targ and Harold Puthoff (1974a) on experiments they conducted with the controversial self-proclaimed psychic Uri Geller.⁸³ Altering the question somewhat to 'Has parapsychology developed the competence to confront the claims of the paranormal?', Honorton reviewed the ESP controversy that raged from the publication of Rhine's (1934) monograph (*ESP*) to the publication of Pratt et al.'s (1940) *Extrasensory Perception after Sixty Years (ESP-60)*.⁸⁴ Honorton also surveyed the controversy over *ESP-60* that appeared in *Science* in 1955 (e.g., Bridgman, 1955; Meehl & Scriven, 1955a; Price, 1955a, 1955b; Rhine, 1955a, 1955b, 1955c), and the statistical debate in *Nature* (e.g., Brown, 1953; Soal, Stratten & Thouless, 1953), commenting on what he saw as the differing policies towards publication of the two journals: that is, that *Science* was prone to publish experiments that had negative results only and that *Nature* seemed willing to publish experiments with either positive and negative results (p. 201-202).

negotiation of points of agreement and disagreement — as business-as-usual for normal science (e.g., Gooding, 1986; Lynne & Howe, 1997; Rosenswein, 1994).

⁸³ The article was part of a wider controversy about research conducted with Uri Geller that played out in the pages of *New Scientist* (Acker, 1974; Bastin, 1974; Beloff, 1974a, Bohm, 1974; Creighton, 1974; Dixon, 1974; Ellison, 1974; Evans, 1974; Faili, 1974; Gooch, 1974; Hanlon, 1974a-e; Hasted, 1974; Hazell, 1974; Honorton, 1974; Mitchell, 1974; Mott, 1974; O'Regan, 1974; Otis, 1974; Playfair, 1974; Scott, 1974; Sladek, 1974; Targ & Puthoff, 1974b), *Philosophy* (Bambrough, 1974), the *Journal of the Society for Psychical Research* (Berendt, 1974), the *Journal of Parapsychology* (Cox, 1974), *Science News* (Sarfatti, 1974), *Human Behavior* (Trotter & Shawvrey, 1974) as well as in *Nature* (Anonymous, 1974; Raff, 1974; Targ & Puthoff, 1974a-b).

⁸⁴ Details will not be given here as the controversy is the focus of Chapter 5.

Honorton focused on the claim that parapsychology lacks replication which he criticised as erroneous given that he could produce reviews of three areas of experimental research for which he felt replication had been achieved.⁸⁵ Although Honorton's arguments occasionally drew on the contingent repertoire, the dominant focus was empiricist: presenting the data, especially in tabular form, and arguing that all that was needed was more data of sufficient quality to force mainstream science to meet its 'obligation to assess, without prejudice, the serious research in this area' (p. 215).⁸⁶

Examples of Texts that Employ the Empiricist Repertoire

Before moving on to the reviews of criticism that are dominated by the contingent repertoire, it is useful here to include a few examples of the kind of text that appeared in the three reviews described above. These excerpts are focused on substantive statements about the nature of science and on specific findings and methods in parapsychology either in response to, or as part of, criticism.

From Coover (1927) I have taken the following:

... the philosophy [of science] is materialistic, regarding mind as epiphenomenal, and the laws of material science as inviolate and alone competent to explain all the phenomena of the universe ... (pp. 230-231).

... Another cause for the incredulity of 'official science' is to be found in the prevalent methods of metapsychic investigation, and this cause perhaps has much greater weight than the stream of negative evidence. (p. 245)

⁸⁵ These were laboratory experiments of ESP beginning with those conducted during the ESP controversy from 1934 to 1940, dream telepathy experiments (e.g., Ullman & Krippner, 1970) and other studies of 'internal awareness states' and ESP (e.g., Stanford, 1974; Stanford, Zenhausern, Taylor & Dwyer, 1975), and what Honorton called 'microdynamic psychokinesis' (e.g., Andre, 1972; Honorton & Barksdale, 1972; Matas & Pantas, 1971; Schmidt, 1970, 1973, 1975; Schmidt & Pantas, 1971; Stanford et al., 1975) (pp. 205-214).

⁸⁶ Amongst the factors drawn from the contingent repertoire that Honorton included was the tendency of *Science* to 'suppress' positive data (p. 201-202), and the general difficulty of publishing in mainstream journals of any kind. These factors were both caused, Honorton thought, by the 'prejudice' of mainstream science and were causally operative in the inability of parapsychologists to make more substantive progress (p. 215).

... Metapsychic investigations are not experiments, they are seances. The phenomena come unexpectedly, not just at the moment the observer is prepared to examine them carefully. ... Even when scientific instruments are used in metapsychic investigation, the control of the conditions of experiment remains in the medium's hands. (p. 249).

The first excerpt provides Coover's definition of science, and the second and third excerpts focus on what Coover sees as the methodological uncontrollability of séance room investigations.

From Nicol (1956) I have taken the following:

... *Qualitative experiments*. These differ ... in the fact that the *material* for study or transmission is prepared by the researcher in advance of the experiment. The weakness of such investigations is that the material, generally drawings, solid objects, verses of poetry and the like, is inaccessible to statistical evaluation. (p. 25)

... The second advantage, characteristic of all scientific work in which quantitative methods are used, is the opportunity they give to create repeatable experimentation. By this is meant the designing of an experimental set-up which, found in practice to produce a significant effect, can be repeated by any competent person at any time in the foreseeable future with approximately similar significant results. (p. 28)

... With regard to PK work comparing the effect of throwing different numbers of dice at a time, the author of the book endeavours to present evidence (here and elsewhere) that PK is more effective on many dice thrown together than on one or two. For example, in the book it is observed that "the tests with six dice scored higher than those with two dice. The rate with two dice was not much above 'chance', but the results with six were highly significant." This finding is obviously not what one would expect from a physical viewpoint. / These statements are at variance with the experimental facts. In the PK literature, two experimental papers cite comparisons of this type. One is the work of Frank Smith (Rhine, 1944), and the other is part of the first E. P. Gibson research (Gibson & Rhine, 1943). The results were ... [a table of results was inserted here] / The results for both Smith and Gibson are plainly the exact opposite of those so confidently announced in the book. (p. 34)

In the first excerpt Nicol focuses on the methodological weaknesses of a particular type of experiment. In the second excerpt, the emphasis is on the importance of replication to science, and in the third, Nicol points out a discrepancy between the

results of two experiments as presented in the original publications and as presented in a later review.

I have taken the following from Honorton's (1976) review:

... During this period there were approximately 60 critical articles by 40 authors, published primarily in the American psychological literature. Fifty experimental studies were reported during this period, two-thirds of which represented independent replication efforts by other laboratories of the Duke University work. The critical issues raised during this period were, for the most part, legitimate ones, and the experimentalists were quick to modify their procedures to accommodate valid criticism. (p. 200)

... The work involved a data base of approximately 3.3 million individual trials. As Table 1 indicates, 61 percent of the independent replications of the Duke work were statistically significant. This is 60 times the proportion of significant studies we would expect if the significant results were due to chance error. Of course, there is also experimental error and some of these studies left much to be desired in terms of methodology. Yet on the basis of my own study of this literature, I concluded that at least 33 of these 50 studies were methodologically adequate on the basis of the experimental reports. (pp. 203-204)

... These detection criteria can account for some of the most prominent features of spontaneous paranormal experiences. The high incidence of spontaneous psi experiences occurring in dreams and other internal attention states would be expected, inasmuch as such states are associated with deafferentation — sensorisomatic noise-reduction — and deployment of attention inward, toward mentation processes such as thoughts and images which may serve to carry psi information, thus increasing the likelihood of *detection*. The utilization of imagery and other forms of mentation in the processing of environmental information has been demonstrated in studies of subliminal stimulation — which, incidentally, is also facilitated by internal attention states (Dixon, 1971). (p. 208)

In the first excerpt Honorton summarises the literature of the 'ESP Controversy' that will be featured in Chapter 5, noting that not only were independent replications obtained but that proponent researchers were also responsive to criticisms raised in their attempts to refine methodology further. In the second excerpt, Honorton provides more

detail about the replication rate and the adequacy of the methodology he reviewed. In the third excerpt he discusses one of the three areas he used as exemplars of progressive lines of research, and relates aspects of this particular theoretical formulation to another research program — albeit a controversial one — in psychology.

Reviews of Criticism Dominated by the Contingent Repertoire

René Sudre

In his 1926 review, René Sudre (1880-1968) reviewed what seemed to him to be the investigative failures of sceptical scientific men who came to the séance room, not only because of their preconceptions about the plausibility of the phenomena under study, but also because of their lack of experience in mediumship studies and their *a priori* dismissal of the methodological knowledge of psychical researchers. Sudre noted that, by cutting themselves off from the specialist knowledge of the psychical researcher, sceptics almost guaranteed their lack of success.⁸⁷ Sudre said: ‘Their incredulity is a systematic one for metapsychics ... [which] disturbs their conception of their world and therefore they will have none of it’ (p. 335).

Walter Franklin Prince

Four of Walter Franklin Prince’s reviews of criticism are included here. The first (Prince, 1927) was published in Carl Murchison’s (1927) *The Case For and Against Psychical Research*. The second was Prince’s (1930) book-length examination of criticism of the phenomena and the discipline of psychical research published from the mid-19th century through 1929 coupled with Prince’s commentary on the results of a survey of selected individuals from amongst the social and scientific elite. The third and

⁸⁷ A lack of respect for the methodological literature of psychical research and parapsychology is not found only amongst sceptical scientists. A number of modern mediumship researchers (e.g., Keen, Ellison & Fontana, 1999; Schwartz, Russek, Nelson & Barentsen, 2001; Schwartz, Russek & Barentsen, 2002) has been criticised by sceptical members of the parapsychological community for not showing familiarity with the published literature of mediumship. For criticism of Keen et al. (1999) see Christie-Murray (2001), Cornell (1999), Gauld (1999), and West (1999). For Keen’s replies, see Keen (2001a-b). For criticism of Schwartz et al. (2001, 2002) see Wiseman & O’Keefe (2001). For Schwartz’s reply see Schwartz (2001).

fourth (Prince, 1933a-b) were published anonymously in the *Bulletin of the Boston Society for Psychical Research*, of which Prince was the editor.

The thesis that runs through the first two of these reviews was that otherwise intelligent and competent individuals lost their ability to be ‘objective’ when they turned their attention to the phenomena of spiritualism and psychical research. This ‘Enchanted Boundary’ between the normal and the paranormal served, Prince thought, to confuse individuals to the extent that they became prey to a wide variety of social, psychological and religious factors when making what should otherwise have been scientific judgements. Critics and proponents alike were susceptible to the effects of this boundary.

In the first review (Prince, 1927), because he felt that no scientist should make up his mind without a personal examination of the ‘facts’, Prince organised sections to answer a variety of contextual questions that should guide such a person through the materials. Amongst these were: (1) What were the causes which led to the foundation of the Societies for Psychical Research? (2) Have the methods of psychical researchers to outward appearances, been cautious, logical and painstaking, or otherwise? (3) How far have opponents shown themselves qualified by experience or by study? (4) On which side, amongst the most scientific leaders, is there the greater appearance of dealing with facts rather than dogmas, with logic rather than appeals to authority? (5) What are some of the arguments against psychical research, and to what extent are other branches of scientific inquiry also liable to the weight of them? and (6) Has psychical research made, aside from the category of the supernormal, any worthy contributions to knowledge? (p. 179).

To answer the questions he raised, Prince provided twenty points of response ranging from a statement about the persistence of the phenomena and its prevalence in the ‘modern’ world, to seemingly common structural aspects of the experiences reported, to the influence of the ‘will to believe’ and the ‘will to disbelieve’ both in critics (pp. 183-184) and proponents, and in scientists in general (p. 185). After discussing how sceptics approached the facts of the field — which he felt provided evidence that sceptics were routinely compromised by emotionality (p. 186) — Prince listed such ‘deficiencies’ in critical publications as: the tendency of critics to choose reports that

had been acknowledged as weak evidence by proponents but which critics handled as if they represented the best evidence; the errors critics made in representing the content of the documents they criticised; and ‘how in juvenile awe of scientific assumptions which are continually altering and enlarging they undertake to demolish facts by dogmatic *pronunciamentos*’ (p. 187, italics in the original).

Prince then reviewed a number of points he felt underscored the conformance of psychical research to science and the critics’ lack of understanding of what science was seen to be by its practitioners, such as: the willingness of psychical researchers to be critical of their own research (pp. 187-189, 191); that the presence of controversy and consensus-building within psychical research were signs of science-as-usual and not signs of an illegitimate enterprise (pp. 192-193); and the willingness of psychical researchers to conduct studies that ultimately benefited mainstream science, including ‘the psychology of hypnotic trance, mental therapeutics and multiple personality’ (p. 197).

In the second review, the book *The Enchanted Boundary*, Prince (1930) provided the results of a comprehensive survey of all published criticism from 1820 to 1930. Prince conceived of the enterprise as following in the footsteps of such mainstream scientists as John Tyndall (1820-1893) and T. H. Huxley (1825-1895) because, by ‘clearing out the obstacles in the way of scientific understanding’, he was ‘removing prejudices and misconceptions, paving with the logic of science’ (p. vii).

The volume was divided into two parts. In Part I, Prince dealt with 40 individuals who authored books, articles, and letters to the editor, a set of texts he claimed was exhaustive of what was then available in the English-language literature.⁸⁸ After reviewing the individual items and evaluating the substantive criticisms for their

⁸⁸ Unfortunately Prince’s style of citing references was incomplete and it was impossible to identify all the works he discussed, thus impossible to check his claim that his list was exhaustive. It can be said, however, that his list was at least extensive and included what seemed to me to be the most important criticism published in the period he studied. Not only did he include popular books debunking or criticising spiritualism and psychical research, but also materials that appeared in the scientific periodical literature. A complete list of the works he reviewed will not be reproduced here because of space constraints. But suffice it to say that *Enchanted Boundary* itself is worthy of a detailed study from both the historical and discourse analytic perspectives.

accuracy and applicability, Prince did a sort of rhetorical analysis, testing sections of text against the kind of prose one would expect from a scientist.

In Part II, Prince reported on the results of a survey he had conducted in which a question purposely crafted to be ‘provocative’ had been sent to a selection of the authors he surveyed. The responses included ‘many expressions of scepticism in all the tones from mild compassion to acid contempt’ (p. x).⁸⁹ Whilst his survey was presented to his participants as seeking seemingly psychic experiences, he was, in actuality, hoping to elicit a range of responses to psychical research and its phenomena in general, with the goal of amassing ‘a considerable collection and analyses of testimonies in opposition to the existence of psychic facts’ (p. ix). Consequently, Prince made an effort to give respondents who expressed opposition a chance to ‘develop the logic of opposition further to state more explicitly their grounds of the negative certainty expressed in further correspondence (p. ix). Some were willing to do this, others were not’ (p. ix). Seventy-one individuals responded.⁹⁰

In summarising and presenting their responses, Prince only published names when respondents had given explicit permission to him to do so. Although some letters were edited for printing, the majority were printed verbatim. Prince argued that his presentation of the responses was not intended to harm any of the respondents, and whilst he found it personally difficult to be fair to those whose views were so different from his own, he believed that the cause of opposition had a lot to do with ‘The Zeitgeist, particularly in America’ (p. ix). His estimation of the arguments he found in the published criticism and those he received in response to his survey is evident in the title of the volume, which was chosen, Prince noted, to convey only that the respondents did not, ‘in the discussion of matters relating to psychic research, seem to display all the intelligence which they understandably possess’ (p. x).

The third and fourth reviews of criticism Prince (1933a-b) provided focused on specific arguments raised by critics that Prince felt were ‘illegitimate’. For Prince,

⁸⁹ Unfortunately he does not include the text of the question in the book.

⁹⁰ Prince excluded only two respondents, Arthur Conan Doyle, who only mentioned his controversy with Harry Houdini, and a Dr Head, who had restricted his responses to apparitions.

legitimate controversy, that is, the ‘best debates’ were ‘disfigured by no ascriptions of mental weakness or moral disability directed by either speaker against the other, no brutal ridicule, no insinuations, no innuendos and no rakings of gossip’ (p. 1). Using this prescription, Prince saw a lot of illegitimate controversy around him especially concerning the phenomena of physical mediumship (p. 2) in which the typical prose was marred with ‘... [r]idicule, mere burlesquing and joke-cracking at the expense of a forensic adversary [which] is about the cheapest and lowest form which controversy can take’ (p. 2). The materials from which Prince derived his examples of illegitimate criticism were actually counter-criticisms written by proponents of the ‘Margery’ mediumship in response to the attitudes and actions of Prince himself, William McDougall, and Eric Dingwall, amongst other psychical researchers who suspected that the ‘Margery’ mediumship was fraudulent.⁹¹

G. N. M. Tyrrell

G. N. M. Tyrrell’s (1879-1952) book, *The Personality of Man*, published in 1947, included two short chapters on criticism. They were: ‘Attitude towards the Subject. Psychical Research: Are Men of Science Impersonal about Facts?’ (pp. 226-239), and ‘Attitude to Psychical Research: Still More Evidence on this Question. Its Fundamental Importance’ (pp. 240-247). Tyrrell followed Prince’s lead (1930) in examining the texts of critics of psychical research and showing how individuals, who were otherwise intelligent, lost their ability to function competently when confronted with the content of psychical research. (pp. 227-228). Like Prince, Tyrrell organised his chapters around a series of questions about the legitimacy and findings of psychical research. Unlike Prince, however, Tyrrell focused on the writings of such psychologists as Joseph Jastrow (e.g., Jastrow, 1900, 1910, 1912, 1927a, 1927b) and Amy Tanner (1910), and historian Joseph McCabe (1920), amongst others.

⁹¹ This particular controversy was set against the backdrop of the ‘coup’ at the American Society for Psychical Research which ousted McDougall, Prince and Dingwall and left the Society in the hands of the ‘Margery’ apologists as mentioned in Chapter 2. The principle document from which Prince draws his examples was *Margery the Medium* (Bird, 1925).

For Tyrrell, the willingness to dismiss psychical research out-of-hand, without evidence or with statements based on serious distortions of the evidence displayed by the examples he recounted, was nothing short of amazing. He asked ‘What is the matter with all these people, one wonders?’ To which he suggested that they were ‘wandering in some enchanted wood’ (p. 239). The epitome of this attitude, Tyrrell thought, was represented by Charles Kellogg (1937b) who decried the diversion of graduate students from important areas of psychology into parapsychology, an area Kellogg saw as unworthy of either funds or personnel (p. 239).

In addition to dealing with published materials, Tyrrell also commented on newspaper articles that announced the Perrott Studentship in Psychical Research at Trinity College, Cambridge, in 1940 (pp. 242-243). Referred to in the press as the ‘Ghost Scholarship’, Tyrrell felt that the derisive titles of newspaper articles indicated clearly ‘the attitude of the public towards psychical research; for the press reflects public opinion. The general opinion evidently is that the study of human personality is not a matter to be taken seriously. Something *psychological* is at work under the conscious surface of the critic’s mind which spurs him on to reject facts without testing them, if they depart too far from what is familiar’ (p. 246).⁹²

D. Scott Rogo

In Rogo’s (1975) textbook, *Parapsychology: A Century of Inquiry*, the author included a chapter called ‘Parapsychology and the ESP Controversy’ (pp. 11-27), in which he provided a review of criticism from 1882 to 1975. Unlike other reviewers I have analysed, Rogo felt that parapsychology had become accepted by an overwhelming number of scientists (p. 102) based on his description of a survey he conducted with members of the American Psychological Association and on a poll conducted by the

⁹² The Perrott Studentship became the Perrott-Warwick Research Grants which fund a number of psychical research-related research units around Great Britain but which does not provide a ‘home’ for such research on the grounds of Cambridge University itself. For the continued ambivalent attitude towards psychical research and parapsychology of that University as represented by the committee which manages the grants, see Carr (2001), Parker (2001a-b), and Wiseman (2001).

New Scientist (Rogo, 1973).⁹³ Rogo went on to provide a brief taxonomy of ESP and PK, to define science, and then to comment that the persistence of the ESP controversy was a mystery to him because he felt that, by 1974, parapsychology had ‘achieved scientific credibility and recognition’ (p. 15).⁹⁴

In an effort to understand this disjuncture between his perception of the scientific status of parapsychology and the continued controversy over its findings, he summarised, amongst other items, Ransom’s (1971) review of criticism, Hansel’s (1966) critic of the Pearce-Pratt experiments, McConnell’s (1969) article on extrasensory perception and credibility, George R. Price’s (1972) retraction of his (1955) article in *Science*, and Prince’s (1930) *The Enchanted Boundary*.

Robert A. McConnell

McConnell’s (1976) article began as a lecture to an ‘anti-parapsychology course’ in which McConnell sought to outline the points on which scientific parapsychologists and critics agreed. This particular strategy was a result of an agreement he made with his critical colleague who was the instructor for the course in which McConnell gave his talk. Amongst the points he covered were: the critical claim that if ESP was proven to be ‘real’, then our view of the world and our behaviour would be affected more profoundly by such a fact than by any other discovery in history, an

⁹³ In the text of the chapter at hand, Rogo claimed that 90% of the respondents to an APA poll had characterised an ESP study as ‘scientifically valid’ (p. 102) and that 97% of the respondents to a poll conducted by *New Scientist* had endorsed statements that ESP was either a possibility or had been proven. Because Rogo provided no references for either of these two studies, and because I can not confirm that they exist, these particular statements should be taken as unsupported. In addition Rogo reported in the text that 80% of respondents to his poll of psychology department chairpersons had expressed the opinion that ESP should be covered in course content. When I checked Rogo’s (1973) article, however, it can be seen that he found that 62.4% of his respondents (psychology department chairpersons in approximately 235 colleges or universities in the United States) thought that parapsychology should be covered in undergraduate psychology courses. Of these, approximately 88% of the departments that had a clinical emphasis were amenable to the idea, as were 67% of the departments that had no particular emphasis, and 50% of the experimentally-oriented departments (p. 21). In addition, in response to the question of whether or not a regular undergraduate course in parapsychology should be adopted, just under 30% of all the respondents said yes. Of these, 50% of the clinically-oriented departments surveyed answered in the affirmative, as well as 30% of the departments that had no particular emphasis and 22% of the experimental departments (p. 22).

⁹⁴ Then, and now, this is a debatable claim.

argument with which McConnell agreed (p. 303); that the evidence accumulated so far was not sufficient to ‘favor the reality of ESP’, a point on which critics and parapsychologists diverged (p. 303); and that a consensus on the quality of the evidence had not been reached either amongst critics or parapsychologists (p. 304).⁹⁵ To explain why divergences existed in the evaluation of the evidence for ESP, McConnell noted that scientists preferred their own beliefs (p. 305), and that parapsychology was in what Thomas Kuhn (1970) would call a ‘pre-theoretical period’ (p. 307). The course instructor who had invited McConnell felt that those individuals were not only wasting research time and resources but that they were also spending too much time attempting to draw the attention of mainstream science. McConnell, on the other hand, felt that questions being asked in parapsychology were too important to go uninvestigated and that, in any case, the cost of that research was insignificant, perhaps ‘not more than a penny or two for every citizen in the USA’ (p. 308).⁹⁶

T. Rockwell, R. Rockwell and W. T. Rockwell

In 1978, a father and two sons reviewed the rhetoric of criticism as displayed in articles which appeared in *The Humanist* (now called *Free Inquiry*), a publication of the Secular Humanist Society.⁹⁷ The Rockwells grew alarmed by the rhetorical treatment

⁹⁵ This opinion would be disputed by a number of critics who would claim that a consensus had been reached in mainstream science, that is, that extrasensory perception and psychokinesis do *not* exist. What was left was to explain why a community of otherwise perfectly competent individuals seemed to believe that the two phenomena *did* exist (e.g., Alcock, 1984, 1985).

⁹⁶ The sceptical instructor was social psychologist Daryl Bem (D. Bem, personal communication, 2004) who in mid-1980s became involved in Charles Honorton’s research, joined the Parapsychological Association around the same time, and is currently both a Board member of the PA and an active researcher.

⁹⁷ The Secular Humanistic Society publishes *Free Inquiry/The Humanist*. Their stated goals are to promote moral conduct and rational thinking that is based on secular values and not on superstition, religion or pseudo-science. One the driving forces behind the Secular Humanist Society is University of Buffalo philosopher, Paul Kurtz, who is also one of the founding members of the Committee for the Scientific Investigation of the Claims of the Paranormal (CSICOP), and an editor of its magazine, *Skeptical Inquirer*, as well as being involved in *Free Inquiry/The Humanist*. In addition, Dr Kurtz also founded Prometheus Books which publishes titles in conformance with the aims of secular humanism, that is, books that debunk religion, spirituality, feminism, deconstruction and post-modern literary criticism, and the purported pseudosciences, amongst which parapsychology is numbered. Prometheus also publishes surveys of critical thinking for college classrooms, and reprints classic texts in philosophy and science. This complex of societies/publications present themselves as avenues for fair and balanced examination of a wide variety of

afforded scientific parapsychology in the pages of that magazine, because they had understood the magazine to be ‘...for those who would rationally evaluate the bewildering barrage of claims associated with the term “paranormal”’ (p. 24). In their estimation such a balanced forum was needed because ‘universities are generally uninformed on the subject; the press typically contributes to the problem; the public is confused; and, except for those directly involved in the research, the scientific community will not face up to the issue’ (p. 24).

Over time, however, the Rockwells began to feel that they detected an editorial policy designed to push the debate to its lowest point rhetorically. To illustrate these failings, they provided a number of specific examples.⁹⁸ Amongst these were: *ad hominem* attacks (p. 25); instances of defamation of individuals such that ‘Sometimes the attack is upon imagined or irrelevant personal characteristics of the individual investigator’ (p. 26); examples of ‘*in loco rationis*’ in which the ‘criticism relies heavily on vague, sweeping charges and the general imputations of base motivations’ (p. 27)⁹⁹; ‘*non sequiturs*’ in which writers in *The Humanist* charged that investigators in parapsychology can not be trusted because they have ‘worked long in the field’ (p. 29)¹⁰⁰; the use of ‘rumour and innuendo’ (p. 30); ‘Apocalyptic Rhetoric’ (p. 33) in which the critic claimed that parapsychologists are ‘part of a larger movement to subvert the minds of the young and destroy civilisation’ (p. 33); amongst others.

topics, whilst the content and rhetorical style of many of their publications focus mainly on the suppression of any point of view they feel threatens the ‘modern’ worldview as they have defined it. For this reason, amongst other, further research into their methods and rhetoric is warranted.

⁹⁸ Because the Rockwells believed that the rhetoric reflected the editorial policy of the magazine they refrained from citing specific authors, instead citing volumes and page numbers as they listed their examples.

⁹⁹ One of the examples they give is from volume 5, page 3, from which they quote: ‘many of the positive parapsychology results being published are fraudulent, the result of data-tampering or improperly controlled experiments’ (p. 27).

¹⁰⁰ Taken from Volume 9, page 29.

Arthur Ellison

Ellison's (1989) contribution to the literature provided guidelines to 'insiders' on how to handle criticism. After discussing how sensation becomes perception in individuals and how scientists build theories from their own personal or experimental experiences, Ellison argued that sceptics are inherently distrustful of experiences they have not shared, and that proponents needed to frame counter-arguments with the understanding that any one who experiences dissonant information or observations usually responds first with anger.

Montague Keen

Keen's (1997) article, 'A Sceptical View of Parapsychology', summarised the history of scepticism aimed at psychical research in general, and at spontaneous phenomena in particular. Whilst Keen noted that healthy scepticism was a necessary ingredient for scientific progress, scepticism that had rarefied into 'a fixed posture' – that is, an unassailable belief system – worried him (p. 289). Amongst the individual sceptics whose work he reviewed were: Nicholas Humphrey (1995) who had published a book called *Soul Searching*; Richard Dawkins (1996) who had published a critique in the *Sunday Times* aimed at those television producers whom he considered to be gullible about claims of the paranormal; the debunking work of the magician James Randi (1982, 1995); and two publications by psychologist and sceptic Ray Hyman (1985b, 1996), amongst others.

Keen noted that for mainstream science the sanctions against any science that appeared to contradict the perceived consensus are extremely severe. As examples, he discussed the experiences of Jacques Benveniste, the French chemist who claimed to have found results supportive of some of the underlying principles of homeopathy, and those of the chemists Stanley Pons and Martin Fleischman who claimed to have found evidence for cold fusion (p. 291).

In examining the work of critics of psychical research and parapsychology, Keen found a frequent lack of knowledge of the relevant literature — as in the case of Dawkins and Randi (p. 294). He highlighted Hyman's erroneous claim that every new

generation of parapsychologists disavowed the work of previous generations. This was a rhetorical device that Hyman used, Keen felt, to sweep aside all previous work and focus only on recent experimental studies, without the necessity of reviewing the evidential and methodological context out of which they arose (pp. 299-300). After commenting on the claim that the field lacks replication, Keen pointed to materialism (pp. 301-302)¹⁰¹ and ‘hubris’ (p. 302) as complicating factors in critics’ efforts to examine the available evidence.

Dean Radin

In his (1998) book, *The Conscious Universe*, Dean Radin published a chapter called ‘A Field Guide to Skepticism’ (pp. 205-228). In the initial paragraph Radin identified his exercise as one that focused on the contingent repertoire:

... We will see that many of the skeptical arguments commonly levelled at psi experiments have been motivated by non-scientific factors, such as arrogance, advocacy, and ideology. (p. 205).

After making a case that science requires scepticism, Radin talked about the effects of what he called ‘extreme skepticism’ on the ability of parapsychologists to do research, arguing that ‘The professional skeptics’ aggressive public labelling of parapsychology as a “pseudoscience,” implying fraud or incompetence on the part of the researchers has been instrumental in preventing this research from taking place at all’ (p. 208).

Radin relied on Honorton’s (1993) and Child’s (1985) articles on scepticism to guide the review of critical materials. The main point Radin derived from these articles and books was that ‘virtually all the skeptical arguments used to explain away psi over the years had been resolved through new experimental designs’ (p. 208). Radin then argued that ‘the few remaining hard-core skeptics’ merely recycled old arguments,

¹⁰¹ Materialism, whilst considered by such ‘insiders’ as Sudre and Keen to be a barrier to the ‘unbiased’ evaluation of the findings of psychical research and parapsychology, is not considered to be so by all modern experimental parapsychologists. Such individuals — myself amongst them — assume that explanations for the phenomena under study will someday fit into a materialist framework, that is, be accommodated by modern physics and not set aside as something irreducibly transcendent (e.g., Edwin C. May, personal communication, 1997; Robert L. Morris, personal communication, 2001).

especially the claim that ‘after one hundred years of research, parapsychology has failed to provide convincing evidence for psi phenomena’ (p. 210). In addition, ‘extreme skeptics’ could be counted on to use a number of questionable argumentative strategies either to counter any positive claims made by the parapsychological community or to inoculate readers against them (pp. 218-219). Amongst these were: the notion that if any results were confirmed by mainstream science, the impact of such results on science as a whole would be trivial; and that ‘three centuries of established science’ (Begley, 1996) had failed to find evidence of psychic functioning when scientific parapsychology had been in existence for much less time than that, only psychical researchers and parapsychologists had done research, and no independent body of disconfirmatory data existed. In addition, Radin noted that:

Skeptics are fond of claiming that believers in psi are afflicted with some sort of abnormal mental condition that prohibits them from seeing the truth ... [such as] psi researchers’ hidden desires to justify some form of spiritual belief ... (p. 224)¹⁰²

But, Radin asserted, such critics’ own ‘... feelings toward organised religion and ... [their] fears about genuine psi ...’ (p. 225) may compromise their own ability to deal with the evidence scientific parapsychologists offered.

Marcello Truzzi

Over the years, Truzzi had been an important voice for a moderate view of the controversy between the critical and proponent communities. He neither believed nor disbelieved in the paranormal, and endeavoured to maintain an open-minded and ‘truly’ sceptical point of view. In his (1998) article, Truzzi’s stated purpose was not to promote paranormality but rather to attempt to provide a ‘more level playing field’ for parapsychologists and their critics. Truzzi began by arguing for the necessity of establishing ‘equilibrium’ in science, a balance-point between the ‘vested interests’ of

¹⁰² Radin was referring to James Alcock’s contention in the background article he prepared for the National Research Council’s committee on parapsychology in the 1980s. Evidence for Radin’s point is certainly visible in his *Behavioral and Brain Sciences* target article (Alcock, 1987) which will be analysed in Chapter 7.

‘institutionalized Big Science’ and the need for science to remain ‘a tentative and open system, both fallible and probabilistic’.¹⁰³ In Truzzi’s estimation, the critical community was leaning dangerously in the direction of ‘a new and quasi-religious dogmatism, usually termed Scientism’ in which the open-ended method of science had become corrupted into a closed-system.

After a discussion of the notion of impossibility, Truzzi paraphrased 19th-century pragmatic philosopher Charles Sanders Peirce who held that the first ‘obligation [of the scientist] must be to do nothing that might block inquiry’. Yet, many critics, Truzzi claimed, demanded that all research be cut off for a variety of disciplines and that such individuals routinely used ridicule and sarcasm — both violations of what was considered to be normal scientific discourse — to obtain their ends, engaging in which Collins and Pinch had called ‘scientific “vigilantism”’. This was especially true of the way in which the paranormal was ‘discredited’, Truzzi argued, noting that ‘excessive zeal’ was often found amongst those who considered anomalies to be threats to the scientific order. These individuals, Truzzi went on, ‘have even been characterised as a “New Inquisition” seeking to stamp out the heresies against an orthodoxy of Scientism’.

Such critics used a number of rhetorical tactics, Truzzi claimed, amongst them characterising the phenomena under study in parapsychology as ‘miracles’, ‘magical’ or ‘supernatural.’ The urge to discredit — a social goal — rather than to disprove — a scientific goal — resulted in personal attacks on proponents rather than scientific investigations of the claims of the paranormal. These individuals did not, Truzzi believed, warrant the term sceptic but were rather ‘scoffers’:

The true skeptic (a doubter) asserts no claim, so has no burden of proof. However, the scoffer (denier) asserts a *negative* claim, so the burden of proof science places on any claimant must apply. When scoffers misrepresent their position as a form of “hard-line” skepticism, they really seek escape from their burden to prove a negative position.

¹⁰³ There are no page numbers listed in this section because Truzzi’s article was published first on a website on one long scrollable page and later in an artistic compilation of illustrations and text in a printed volume entirely without page numbers.

Truzzi felt that scoffers had a tendency to confuse weak evidence with *no* evidence, a practise that would be damning for a variety of disciplines. Once weak evidence had been made stronger by further research, rather than reconsidering their conclusions, scoffers tended to demand even stronger evidence. Critics who adopted this behaviour, then, Truzzi felt, espoused unfalsifiable positions. In addition, Truzzi argued scoffers misunderstood the concept of replication,¹⁰⁴ the history of science, and current science practise.

Amongst his final points, Truzzi focused on the notion that individuals who propose what seem to extraordinary claims are often saddled with the unfair burden of being required to provide proof beyond a reasonable doubt.

Examples of Texts that Employ the Contingent Repertoire

In this section I have surveyed the reviews of criticism in which the contingent repertoire dominated. A number of examples of this type of argument are available, especially in the works of Walter Franklin Prince. For example, Prince claimed that it was possible to identify those individuals whose emotional predilections over-influenced their intellectual judgements because they:

... who have had and set forth some evidence, ... mix with it so much indiscrimination, incaution and intemperate zeal as also to make them ineffectual except with the unthinking and as marks for their adversaries. (Prince, 1927, p. 185)

Prince characterised individuals who opposed the existence of the discipline of psychical research in the following way:

Every one of them, by the application of the test is shown to belong, *when he enters the field of Psychical Research with general, hostile intent*, to the third class, that composed of persons whose conclusions are actuated mainly by their emotions, by manifest bias and prejudice, rather than by calm reasoning on the basis of careful study; persons who *react irrationally to particular subjects which for some reason are obnoxious to them*, and evidence the fact by generalities, *a priori*

¹⁰⁴ Truzzi cited Harry Collins without a reference. I assume that he is thinking of Collins' work on 'experimenter's regress' (e.g., Collins 1974, 1975, 1982a).

assumptions, refusal to face squarely and discuss calmly main issues, attacks on men of straw, weird logic which they would deride were it employed in their own special field, indulgence in wild and unsupported hypotheses in regard to the intellects of all their opponents, exhibitions of ignorance of their subject matter by frequent blunders of fact, exclamations of disgust and sundry marks of emotionalism. (p. 186)

It can be seen from the above that Prince was not at all shy about attributing motives to the detractors of psychical research. Another example: those who based their negative view of the field on the writings of others accepted what they read, Prince (1930) said, without:

... thinking it necessary to ascertain whether [the evidence] ... had been met squarely and analysed fairly, or [had] ... been garbled, nibbled about the edges, and treated with an evasion, sophistry and persiflage which would be deemed unworthy in any other field of discussion (p. viii).

And he asked:

Then what induces these writers to shun real acquaintance with the matters which they discuss, to misquote or reverse the meaning of sentences before their eyes, to misspell familiar names, to rely without misgiving on secondary and unreliable sources, to misstate facts easy of reference, to employ schoolboy logic, to yield to emotion and boast of it, to parrot materialistic dogmas instead of discussing evidence, and to parrot dogmas regarding their opponents' intellects instead of meeting their arguments. Surely my hypothesis of the enchanted boundary is the most charitable one, and it is quite sufficient to explain the phenomena. (pp. 132-133)

Prince (1933a) was not above 'naming names' and in his review of Malcolm Bird's (1925) book on 'Margery', Prince noted that Bird's own propensity to excoriate all of his detractors made it difficult to take his arguments seriously:

There well might be in any considerable group one or more persons whose motives, morals, conduct or mentalities are justly amenable to attack, though better with few adjectives and much evidence (not mere word-chopping or fact-juggling). But that *all* of a certain man's express forensic opponents, irrespective of previous rank, station and reputation, should be subject to condemnation, and that it should be possible to determine that all others who murmur dissent from his opinion bear the like stamp of malignity or mental impotence, is quite incredible. (p. 49)

From Keen (1997) comes this example:

... the philosophic stance of the sceptic is no less deeply grounded in faith than is that of the most devout deist. His faith is in the indisputable dominion of the laws which have been established to explain the working of the universe apparent to our five senses and operating within the three dimensions bounded by time. Beyond that there is nothing, nothing but the projections of human desire, the flights of imagination, the rich diet of illusion and fantasy. But because he is conscious that this philosophy has been seriously battered, the tenacity with which he defends his citadel is the more ferocious. (pp. 291-292)

Amongst the motivations that compromised sceptics' ability to deal with the evidence, Keen claimed, was also:

... Hubris ... a perilous destiny for those proclaiming the absolute impossibility of evidence undermining their belief system. (p. 302)

In Radin's (1998) treatment of the motivations of critics he provides some evidence for his assertions that strong emotions can motivate critics as well as it can motivate proponents:

We may now turn the tables on Alcock and ask what motivates sceptics to spend so much time trying to dismiss the results of another scientific discipline. For Alcock, it seems that his feelings toward organized religion and his fears about genuine psi are motivations. For example, Alcock has written:

In the name of religion human beings have committed genocide, toppled thrones, built gargantuan shrines, practised ritual murder, forced others to conform to their way of life, eschewed the pleasures of the flesh, flagellated themselves, or given away all their possessions and become martyrs.¹⁰⁵

And,

There would, of course be no privacy, since by

¹⁰⁵ Radin is quoting from Alcock (1981), p. 7.

extrasensory perception one could see even into people's minds. Dictators would no longer have to trust the words of their followers; they could 'know' their feelings. ... What would happen when two adversaries tried to harm the other via PK?¹⁰⁶

... In other words, religious faith can motivate scientists both toward or against psi research. (pp. 225-226)

In the next section, I will survey the reviews of criticism and response that provided arguments drawn both from the empiricist and the contingent repertoires.

Reviews of Criticism in Which Both the Empiricist And Contingent Repertoires were Used

Ian Stevenson and William G. Roll

Stevenson and Roll (1966) reviewed a variety of works of criticism as a prelude to producing some guidelines for those who engaged in the debate. Amongst these the controversy that followed the publication of Price's (1955a) article in *Science*,¹⁰⁷ which played out both in the pages of *Science* and the *Journal of Parapsychology* in 1955 and 1956.¹⁰⁸ Stevenson and Roll also reviewed controversies published: in *Psychiatric Quarterly* (Szasz, 1957; Unger, 1957); *Nature* and *New Scientist* (e.g., Hansel, 1959a, 1959b, 1960a, 1960b; Soal, 1960a, 1960b) and *Psychological Bulletin* (Girden, 1962a, 1962b, Murphy, 1962); as well as criticisms that appeared in the pages of the field's journals (e.g., Hansel, 1961a, 1961b; Pratt & Woodruff, 1961; Rhine & Pratt, 1961). In

¹⁰⁶ Radin is quoting from Alcock (1981), p. 191.

¹⁰⁷ The 'Science and Supernature' controversy was published in *Science* in 1955 (Bridgman, 1955a; Meehl & Scriven, 1955a; Price, 1955a, 1955b, 1955c; Rhine, 1955a, 1955e; Soal, 1955a; Wolfle, 1955) with a retraction by the original author published in 1972 (Price, 1972).

¹⁰⁸ Letters that were published in *Science* were also abstracted in the *Journal of Parapsychology (JP)* (Bridgeman, 1955b; Meehl & Scriven, 1955b; Price, 1955b; Rhine, 1955b, 1955c, 1955f; Soal, 1955b) and letters that were refused publication in *Science* were published in full in the *JP* (Crumbaugh, 1955; Erickson, 1955; Gardiner, 1955; Gibson, 1955; Kapchan, 1955; McConnell, 1955; Ozanne, 1955; Smith, 1955).

addition, they also reviewed a book published by D. H. Rawcliffe (1952) that combined parapsychology with the occult in order to dismiss both.

In reaction to the content and tone of the materials they surveyed, Stevenson and Roll made four recommendations for future critics: (1) that they should understand the difference between spontaneous and experimental research and between exploratory and confirmatory research; (2) that they should 'restrict themselves to known facts'; (3) that they should provide *evidence* of any allegations made, particularly when fraud was charged or even when it was 'merely implied'; and (4) that they should not select evidence but present everything that was confirmatory or disconfirmatory (p. 350). As regards recommendation four, Stevenson and Roll also urged: 'It is the critic's duty to bring out *all* the evidence relevant to his criticisms and not only the material supporting them. If the critic disregards evidence that is unfavourable to his views, he displays an ignorance of scientific method that disqualifies his work from serious consideration' (p. 352).

Finally, they argued that the act of engaging in criticism carried with it a moral responsibility to be fair, if for no other reason than that criticism was easier to publish:

The fact that there is a market for adverse reviews is a powerful temptation for the critic. If nothing else restrains him, he would do well to realize that in the history of science he will be dealt with severely if it is found that his evidence, and not the criticized research, is wanting (p. 352).

Champe C. Ransom

In 1966, C. E. M. Hansel's speculative criticism found a new outlet in his book (1966) *Extrasensory Perception: A Critical Evaluation*. Heavily criticised by parapsychologists for a variety of problems, (e.g., Beloff, 1968; Eysenck, 1968; Honorton, 1967; Medhurst, 1968; Shapiro, 1968; Slater, 1968; Stevenson, 1967, 1968; West, 1968) to which Hansel (1968) replied, the book nonetheless went on to become a classic of criticism.¹⁰⁹ By 1970 published criticisms of parapsychology were again on the

¹⁰⁹ It was published in two more editions, one in 1980 and another in 1989. Altered slightly from edition to edition, the errors of fact pointed out by early reviewers have remained uncorrected.

rise. The Parapsychology Foundation approached Ransom with the idea that he review the published materials so as to determine what were the most common criticisms raised to that point.¹¹⁰ Ransom identified what he believed to be the nine most common criticisms. These are listed on Table 2 with exemplars of each of these criticisms and a brief restatement of Ransom's reply.

Table 2.

Ransom's Nine Most Common Criticisms

Criticism	Exemplar of Criticism	Ransom's Response
Successful experiments are not repeatable	Hansel (1966)	Lack of repeatability over-estimated by critics; when failures to replicate occur they can be caused by a variety of factors from 'mismanagement' of the experimental design to unknown confounding variables (pp. 292-293)
If fraud is possible, ESP is not	Hansel (1966); Price, (1955)	Raising the spectre of fraud is not enough; evidence must be provided that fraud has taken place (pp. 294-295)
Parapsychology uses improper statistics	Brown (1953)	That not all critics agreed with this criticism (e.g., Rawcliffe, 1952) and that such technicalities should be left to the specialists (p. 295)
Psi phenomena are <i>a priori</i> impossible	T. R. Willis quoted in Burt (1967)	Based on the notion that everything that <i>could</i> be learned about the universe had <i>already</i> been learned (pp. 296-297)
Parapsychology draws unwarranted conclusions	Nicol (1956)	Specifically focused on those who drew spiritual meaning from experimental results

¹¹⁰ The Foundation provided Ransom with a grant for the purpose (Lisette Coly, personal communication, 2003).

Table 2 continued

Criticism	Exemplar of Criticism	Ransom's Response
Science cannot investigate the paranormal	Nicol (1956)	This criticism was related to the notion that the paranormal was uninteresting to mainstream science, but also to the notion that psi was 'elusive' and not amenable to scientific testing
Refinement in methodology will eliminate evidence for psi	None given	A form of 'all-or-none' criticism that both underestimated the quality of early experiments and set aside the possibility that there might be 'real' reasons for the imposition of particular methodologies to inhibit positive scoring in psi experiments
No consensus exists on the quality of the evidence	Nicol (1956)	A 'reasonable' criticism that underscored how difficult it was for the layman to evaluate parapsychological findings when parapsychologists could not agree on a 'conclusive' experiment (p. 302) ¹¹¹
Parapsychologists are biased because they believe in psi	Rawcliffe (1952)	That parapsychology was not beset with more bias than any other area of science (p. 302)

The second most common criticism, 'if fraud is possible, ESP is not' was a complex charge, Ransom thought. Critics were not only speculating that fraud might have occurred but they were making the case that if a scenario by which fraud might have been committed could be envisioned, then the experiment could not be accepted as evidence.¹¹² Further, once made, the criticism was, Ransom said, 'insurmountable ... since the critic can always claim that everyone involved in the experiment in question was lying about any or all of the details. Even if an experiment was repeated, it could be claimed that it is *possible* that all the experimenters were fraudulent. This impasse

¹¹¹ This criticism assumes, however, that there is such a thing as a 'crucial' experiment. Belief in the potential existence of a 'crucial' experiment has been set aside by some critics (e.g., Hyman, 1985b), not to mention sociologists of science (e.g., Collins & Pinch, 1993, pp. 128-129).

¹¹² This was essentially Hansel's point in his criticism of the Pearce-Pratt experiments (e.g., Hansel, 1961a).

shows, again, the importance of dealing with the question of direct evidence of fraud rather than the possibility of fraud' (pp. 294-295).¹¹³

The fourth most common criticism, that 'psi phenomena were *a priori* impossible' was, Ransom thought, particularly hard to answer. This was especially so when it was recast in its 'pure' form, that is: 'no amount of evidence can prove something that conflicts with everything else we have learned' (p. 297).¹¹⁴

The fifth most common criticism — that 'parapsychology draws unwarranted conclusions' — was one with which Ransom agreed. The thesis of the exemplar, Nicol's (1956) critique of the Rhinean paradigm, was, in Ransom's opinion, a point well-taken. For Ransom, there were enough unanswered questions to render experimentalists' emphasis on the spiritual, at best, premature, and at worst, a distraction from the work that still needed to be done.¹¹⁵

Ransom ended his review with four problems he saw in critics and their published writings: (1) some critics suffer from a strong will to disbelieve; (2) some critics focus on out-dated research when they develop their criticisms, ignoring modern research which is usually strong evidentially or, at least, better designed; (3) critics disagree with one another about the content of their criticism; and (4) some critics 'praise' some areas of research whilst ignoring or complaining about other areas (p. 305).

Robert H. Thouless

Thouless's (1971) review of criticism constituted a chapter in his book, *From Anecdote to Experiment in Psychological Research*. Amongst his conclusions were that: 'some critics fall far short of this ideal', working from the weakest evidence the field

¹¹³ This particular type of criticism is still raised (e.g., Wiseman & Milton, 1998), the crucial point of whether or not fraud *actually* occurred being set aside as immaterial by the critics.

¹¹⁴ This is also a criticism that is still raised (e.g., Bunge, 1987; Tobacyk, 1987).

¹¹⁵ Alcock's (1987a) target article in the *Behavioral and Brain Sciences* exchange is a more modern example of this type of criticism. There are those in the field who agree that such speculation is not appropriate (e.g., Palmer, 1987), and those who believe that queries about spirituality and the soul are completely appropriate (e.g., Tart, 1987).

provided and not from the best; some critics crafted their criticisms rhetorically to raise the emotional tone of the debate rather than providing a logical assessment of the research; other critics were not above ‘... digging up or inventing discreditable episodes in the ... personal lives ... [of their opponents] which have no connection with their experimental researches’ (p. 94).

Thouless counted C. E. M. Hansel (e.g., 1966) and George R. Price (1955) as two critics who could not be faulted for promoting unproductive criticism, even though they ‘sometimes seem[ed] to fall short of the standards of the ideal critic’ (pp. 94-95). Thouless was worried, however, that criticism might force an ‘extreme preoccupation with experimental precautions’ (p. 95), wasting hours that might have been used for ‘the more profitable task of finding out about the nature of ESP’ (p. 95). In reviewing Price’s article, and C. E. M. Hansel’s book, Thouless found Hansel to be ‘less emphatic than Price in his rejection of the possibility of ESP’ (p. 96), although Hansel obviously found the ‘intrinsic improbability ... [to be] too great for the existing experimental evidence to be sufficient support for its reality’ (p. 96).

The spectre of fraud Hansel raised concerning the Pearce-Pratt series of experiments was reasonable, Thouless thought, given that the subject had been left unsupervised, but Thouless also found useful Stevenson’s (1967) argument that Hansel’s fraud scenario was improbable. The only way to rule out fraud, Thouless thought, was to require critics to do research themselves. To promote this idea, Thouless provided guidelines for designing an experiment, although he thought critics might find it difficult to recruit willing participants if their beliefs were known.

Finally, Thouless cautioned parapsychologists to remember that the phenomena were improbable, although he admitted he had little patience for critics who claimed that parapsychology represented ‘an incompatibility with current scientific theory [that was] ... equivalent to a breach of nature’, a view he saw as ‘... a somewhat superstitious view of natural law’. For Thouless, natural law was ‘... not a pre-existing system of rules which phenomena have to obey; it ... [was] a system of rules that the scientist puts forward to account for observed regularities. If an unexpected event occurs, it is not a breach of the law; it is an indication that the law, as at present enunciated, must be altered’ (p. 100).

Robert A. McConnell

McConnell (1977) reviewed criticism in parapsychology in a general way, focusing firstly on deriving a list of the most common criticisms, and secondly, on examining how assessments of *a priori* probability and pre-existing beliefs would effect attitudes towards parapsychological research. McConnell's list of the sixteen most common criticisms are presented on Table 3 with McConnell's interpretations of the criticisms and his estimation of whether or not some consensus on the point might be reached.

Table 3.

McConnell's Sixteen Most Common Criticisms

Common Criticism	McConnell's Interpretation	McConnell's Response
ESP is theoretically impossible	The critic who uses this argument means that ESP conflicts with the scientific worldview as he or she sees it (p. 202)	<i>A priori</i> assessment, unlikely that agreement between proponents and critics can be reached on this point
ESP is contrary both to common sense and to practical experience	Same as above	Same as above
A theoretical explanation must be offered before ESP can be seriously entertained by mainstream scientists	Same as above	Hope for eventual agreement if acceptable theory can be developed
ESP experiments with statistically significant results must be considered to represent the selection of a chance fluctuation from amongst many unsuccessful, unreported experiments	The critic who uses this argument might be satisfied by higher levels of significance and/or by the integration of the findings into a coherent theory (p. 203)	Hope for eventual agreement if accumulation of results rules out statistical artefact
Statistical significance proves nothing as any individual significant result may have happened solely by chance	Same as above	Same as above
ESP must be more reproducible to be proven	Proponents also want repeatability but need funds to continue to refine experimentation (pp. 204-205)	Hope for eventual agreement if relevant methodological weaknesses can be found and corrected

Table 3 continued

Common Criticism	McConnell's Interpretation	McConnell's Response
Better controls against fraud are needed	Proponents should maintain personal responsibility for experiments and encourage better <u>methodological training</u>	Hope for eventual agreement if relevant methodological weaknesses can be found and corrected
Fraud is always a better explanation because ESP tests attract deviant individuals	Proponents should reject the fraud hypothesis when used as an unfalsifiable claim	Same as above
Sensory leakage may have caused results even if only minimally present	Used by critics as a blanket <i>a priori</i> rejection (p. 206)	Same as above
Procedural errors may have caused results	Same as above	Same as above
Written reports are never complete so unknown but crucial weaknesses in the design may have caused results	Same as above	Same as above
Flaws have been found in most ESP experiments therefore undiscovered flaws may have caused the results	Used by critics as an <i>a priori</i> blanket rejection (p. 206)	Same as above
Any set of observations can be explained by an indefinitely large number of imaginable ordinary mechanisms therefore ESP results may have been caused by an undiscovered ordinary mechanism	Same as above	Same as above
Any ESP experiment with a weakness must be discarded from the dataset and no single experiment can prove anything. Therefore no single ESP experiment exists that can provide the reality of the phenomena.	Same as above	Unfalsifiable and thus unlikely that agreement between proponents and critics can be reached if critics hold to this point
Automated ESP experiments may suffer from undiscovered temporary technical flaws or systematic biases that provide spurious ESP effects. Fraud cannot be ruled out in automated experiments unless independent electrical engineers have tested the equipment in use.	Proponents also concerned with technical factors in automated tests but not communicating that fact to critics (pp. 206-207)	Hope for eventual agreement if relevant methodological weaknesses could be found and corrected

The final section of McConnell's paper was devoted to two types of probability assessments he felt any scientist made when dealing with any evidence. These were 'subjective counter explanatory probability' (p. 207) and 'subjective antecedent probability of the reality of ESP' (p. 208). The notion of 'subjective counter explanatory

probability' (SCEP) required that an experiment 'add to the calculated chance probability the subjective probabilities of all other possible counter explanations such as fraud, equipment failure ... the oversights that bedevil us all, and mistakes rising from incompetence' (p. 207). Once calculated, SCEP became 'an estimate of an upper limit for the direct evidence "probability" of the non-occurrence of ESP in nature' (p. 208). The notion of 'subjective antecedent probability of the reality of ESP' (SAP), on the other hand, was determined by an investigator's familiarity with the current factual knowledge in other sciences into which ESP must be integrated, and with the general history and method of science. These two sets of 'knowledge' were moderated by the investigator's degree of enculturation and respect for authority (p. 209). Effecting both the subjective counter explanatory probability (SCEP) and the subject antecedent probability of ESP (SAP) were 'intelligence, curiosity, empathy, and a grasp of current and historical social reality' (p. 209). Closure of the controversy raging between critics and proponents, then, was complicated by both SCEP and SAP, especially because both sets of judgements were influenced by 'emotional elements' (p. 210), and could be expected to be contested within individual investigators, no matter what his or her stance on ESP, as well as between them.¹¹⁶

Eberhard Bauer

Bauer (1984) published an article in which he not only reviewed what he considered to be important criticisms in the history of the field, but also the possible factors influencing the attitudes of critics and proponents towards the phenomena and research of parapsychology. Bauer noted that the history of parapsychology could be seen as the history of controversy. In his estimation, there were four main points of contention: (1) no consensus existed over who was 'entitled to be considered as a "parapsychologist" [and] ... who is allowed to act as a "critic"' (p. 142); no consensus existed on content, methodology or theory; and the issue of belief was more complex than normally thought in that some proponents were also critics — albeit 'internal'

¹¹⁶ It is interesting that SCEP seemed to be based on empiricist factors — craft knowledge, assessments of methodological quality, the accuracy or applicability of alternate hypotheses and so on — and SAP seemed to be based on contingent factors — personal abilities, temperamental variables and social skills.

critics¹¹⁷ — and that one could not infer from a proponent's lack of a critical stance that they were 'believers', nor was it possible to assume solely from any individual's critical stance that he or she was a 'dis-believer' (p. 142). Further, it could not be assumed that those who adopted a critical stance were, in fact, knowledgeable about the field (p. 143). A further complication, Bauer thought, was the fact that there was no consensus on the 'boundaries of the field' (p. 143).¹¹⁸

Like Honorton (1975) reviewed above, Bauer felt that the period between 1934 and 1940 was an important one in which the criticisms focused profitably on statistical and experimental methodology as well as on the logic of published interpretations of experimental results (p. 145). Bauer also believed that Pratt et al.'s (1940) book silenced criticism after its publication. He set the date for the re-emergence of controversy later than other reviewers had, as 1962 when Edward Girden (1962a, 1962b) began a sustained critique of psychokinesis research. Paradoxically, Bauer went on to discuss both the statistical controversy that appeared in *Nature* in 1953 (Brown, 1953) and Price's (1955) article in *Science*, the latter of which carried the date more commonly nominated as the end of the period of 'silence'. Bauer saw Price's article as a radical reframing of the criticism of ESP research from methodological and statistical error to either deliberate fraud or to abnormal mental states (e.g. Price, 1955, p. 160).

C. E. M. Hansel's (1966) critique of ESP research was considered, Bauer claimed, by 'the non-parapsychological world ... as the final word to be wasted on the subject' (p. 147).¹¹⁹ Bauer characterised Hansel's as an extreme version of the fraud hypothesis: That is, that all successful ESP experiments had to have been compromised by fraud because ESP was so unlikely. The notion that parapsychology lacked

¹¹⁷ Irvin Child (1987a), whose review I will describe next, made use of 'internal' and 'external' critics as an organising principle.

¹¹⁸ Bauer cited Nilsson's (1975, 1976) assessment of the impact of the Rhinean School on the list of phenomena considered to be 'solvable' and thus appropriate for inclusion, Louisa Rhine's (1969) argument for the narrowing of the purpose of spontaneous case research to include only hypothesis generation for the laboratory, and Thouless's (1973) complaint that the Rhinean School had attempted to eliminate from the field too many of the traditional phenomena, amongst them the out-of-body experience and survival research.

¹¹⁹ Bauer cited Slater (1968) as the bestower of the attribution of the 'final word'.

replication, as raised by Hansel, was, in fact, very important to parapsychology, Bauer thought, and was a point — especially as regards ‘replication on demand’ — with which many proponents agreed (e.g., Beloff, 1972; Crumbaugh, 1966; and Donnemeyer, 1966) (p. 148).

Bauer also claimed that the “‘erosion of evidence” [was] ... one of the most stable traits in the history of parapsychology’ (p. 150). By this he meant a number of experiments thought at one time to be evidential had been recast as weak evidence or as non-evidential because of changes in methodology, the lack of replication, or worse, because the results of the original experiments *were found* to have been caused by fraud (p. 150).

Bauer then asked the question whether personal emotional reasons could be found to explain the continued commitment of proponents and critics to their positions (pp. 152-153). He reviewed a number of speculations as to the psychology of critics (e.g., Eisenbud, 1963; LeShan, 1966) but did not review literature that speculated on the motivations of proponents.

Irvin L. Child

Child’s (1987) review of criticism focused both on ‘internal’ criticism — that is, criticism of parapsychological research published by proponents — and ‘external’ criticism — that is, criticism published by sceptics. Written for the fifth volume in the *Advances in Parapsychological Research* series (Krippner, 1987a), Child’s purpose was ‘to provide a guide to the reading of recent criticism’ (p. 192). He organised his review by author, described the published work of the individual and evaluated it from an ‘insider’ perspective. To present the material Child reviewed I have re-organised it into Table 4. In the first column I have entered the references Child examined. In the second column I have identified the critic as either ‘internal’ or ‘external’. In the third column I have briefly restated Child’s comments on the content of the individual’s criticisms, and in the fourth column I have characterised Child’s evaluation of the criticism, which in some cases focused on the accuracy or utility of the criticisms raised and sometimes on the motivations or beliefs that Child believed lay behind them.

Table 4.

Child's List of 'Internal' and 'External' Critics

Citations to Critics	'Internal' or 'External'	Criticism Raised	Evaluation of Criticism
Akers (1984)	Internal	Methodological criticism of ganzfeld research	Specific points of methodology useful; notion that experiments must be flawless to be evidential seen as counter-productive
Blackmore (1985)	External	Treatment of replication in the context of conference on the topic	Notes that Blackmore herself has failed to obtain positive results in her experiments
Diaconis (1978, 1979, 1980)	External	Mathematical and statistical knowledge and experience as a magician used to critique special subject research	Claims wide knowledge of parapsychological literature but does not show such familiarity in his criticism
Gardner (1981, 1983)	External	General criticism of various areas of parapsychological research	Although having a reputation for exceedingly derogatory prose, some substantive criticisms useful
Girden (1962a, 1962b, 1978) Girden & Girden (1985)	External	Substantive critique of PK research	Compromised by belief that ESP and PK are <i>a priori</i> impossible
Hansel (1959a, 1959b, 1960a, 1960b, 1960c, 1961a, 1961b, 1966, 1980, 1985, 1987)	External	Substantive critique of ESP research with a particular emphasis on fraud scenarios	Compromised by belief that ESP and PK are <i>a priori</i> impossible and that all studies must be evaluated singly and if flawed can not be combined with other studies as evidence

Table 4 continued

Citations to Critics	'Internal' or 'External'	Criticism Raised	Evaluation of Criticism
Honorton (1983, 1985)	Internal	Methodological, statistical	Productive criticism, introduction of meta-analytical methods important to substantive argument, engaged in efforts to collaborate with external critics
Hövelmann (1983) ¹²⁰	Internal	Boundary issues: that is, which phenomena to include or exclude to ensure substantive progress	Introduced profitable discussions of discourse of criticism, as well as profitable debates on professional issues
Hyman (1985b)	External	Methodological issues	Critical of external critics who held unfalsifiable positions such as Hansel; familiar with primary sources in the field; willing to work with internal critics
Kennedy (1980)	Internal	Methodological issues of free response experiments	Productive criticism based on familiarity with primary sources
Marks & Kammann (1980)	External	Methodological criticisms and motivations, beliefs of proponents	Uncritical use of Hansel, productive criticism of remote viewing research

¹²⁰ Hövelmann's seven recommendations for the future of parapsychology inspired a variety of responses from proponents and critics which were published in Marcello Truzzi's informal debate journal, *Zetetic Scholar*. Amongst these were comments by proponents (Beloff, 1983; Eysenck, 1983; Inglis, 1983; Keil, 1983; Lucadou, 1983; Morris, 1983; Nash, 1983; Palmer, 1983c; Rosen, 1983; Schmeidler, 1983a; Stokes, 1983; Timm, 1983), critics (Blackmore, 1983; Hoebens, 1983; Scott, 1983; Tobacyk, 1983; Zusne, 1983), and others (Leeds, 1983; Mertens, 1983; Pinch, 1983).

Table 4 continued

Citations to Critics	'Internal' or 'External'	Criticism Raised	Evaluation of Criticism
McConnell (1982, 1983)	Internal	Criticises proponents for 'right-wing and left-wing appeasement' ¹²¹ ; Criticises journals for failing to support his specific criticisms	Self-published volumes of wide-ranging criticism not solely devoted to parapsychology ¹²²
Morris (e.g., 1976)	Internal	Methodological, statistical and theoretical criticism	Even-handed balance of criticism and praise; effective restatement of external criticisms
Moss & Butler (1978a, 1978b) ¹²³	External	Blanket characterisations of parapsychological research without examples ¹²⁴	Rhetorical context complicated reception of useful substantive points by proponents; blanket statements, stated purpose of article, and errors of fact counter-productive
Neher (1980)	External	General criticisms	Lack of familiarity with primary sources; emphasis on qualitative material only

¹²¹ By right-wing appeasement, McConnell meant an excessively soft response by parapsychologists to critics who attempted to block parapsychology's integration into the mainstream. By left-wing appeasement, McConnell meant excessively tolerant behaviour towards occult practitioners by parapsychologists who wanted to encourage psi phenomena but who seemed to be unaware of the damage such individuals caused the field (p. 210).

¹²² McConnell's (1982, 1983) self-published books are a strange mixture of counter-criticism and McConnell's very personal philosophies on virtually every topic in the modern intellectual world from feminism to deconstruction. The mixture, and McConnell's propensity to circulate his critical papers amongst a wide list of generally uninterested elite scientists, has decreased his credibility in the field, although he has produced, over the years, important internal criticism (e.g., McConnell, 1949).

¹²³ Moss and Butler's (1978a) original article was followed by a critique by McConnell (1978) to which they replied (Moss & Butler, 1978b). K. R. Rao (1979) contributed comments in a later volume of the same journal.

¹²⁴ Child said they raised four general points: (1) that no fool-proof 'recipe' existed for replication; (2) parapsychological experiments never controlled for intervening or confounding variables; (3) no independent variable with a consistent impact on results had been found; and (4) too many people sold their services as psychics (p. 210).

Table 4 continued

Citations to Critics	'Internal' or 'External'	Criticism Raised	Evaluation of Criticism
Palmer (e.g., 1983a, 1983b, 1986d)	Internal	Methodological and theoretical criticisms; need for experiments to test conventional hypotheses; need for more process-oriented research	Balanced and productive criticism; research on conventional hypotheses productive
Randi (1975, 1980b)	External	Magic-based critiques of special subject research; blanket rejections of all experimental research in field	Lack of applicability of main line of criticism to experimental research; lack of familiarity with scientific method in general and primary sources in parapsychology in particular; extreme rhetorical strategies render few useful substantive points unhearable to proponents
Schechter (1984)	Internal	Specific methodological criticisms for experiments using hypnosis	Productive criticism for narrow line of research
Stanford (1981)	Internal	Methodological and theoretical criticisms of experimental research	Productive and balanced criticism
Stokes (e.g., 1985)	Internal	Methodological and theoretical criticisms of experimental research	Productive and balanced praise and criticism of points raised by both critics and proponents
Truzzi (e.g., 1982)	External	Criticism of theoretical and rhetorical aspects of parapsychology; focus on necessity to separate the anomaly from its explanation	Balanced criticism of both critics and proponents based on an emphasis on wider scientific goals; provided through <i>Zetetic Scholar</i> important forum for debate
Zusne & Jones (1982)	External	Criticism of interpretation of experimental results based on a wider view of the context of anomalistic psychology	Focus on conventional explanations for seemingly-paranormal events

In his survey Child categorised ten of the individuals whose work he reviewed as ‘internal’ critics and thirteen as ‘external’ critics. His stance towards one of the individuals he reviewed was relatively neutral (Blackmore), whilst he characterised eleven individuals as providing productive criticisms, and ten individuals as offering counter-productive criticism. One individual was described in such a way, it seemed to me, that Child felt the person’s work was particularly ‘blameworthy’ (Randi), and five were described in such a way, it seemed to me, that Child felt the work of these individuals was particularly ‘praiseworthy’ (Morris, Palmer, Stanford, Stokes, and Truzzi).

Of the eleven individuals who were characterised as providing productive criticism — that is, criticism that Child classified as useful to researchers and/or which showed knowledge of the primary literature of the field — all but four (Hyman, Truzzi, and Zusne and Jones) were internal critics. Of the ten individuals who are characterised as providing counter-productive criticism — that is, criticism that Child classified as unfalsifiable, too general, or revealing ignorance of the primary literature of parapsychology — all but one (McConnell) were external critics. In addition to ignorance of the primary literature of the field, Child found blanket rejections of the research, *a priori* denial of the existence of the phenomena or of research worth examining, and overly emotional or condemnatory prose particularly problematic. On the other hand, doubt and the willingness to deal with the details of primary sources both competently and — in the rhetorical sense — respectfully led Child to characterise the critical content as productive.

Child’s review also included a reader’s guide to the periodicals in which the criticism he summarised had been published.

Examples of Text drawn from Authors who Employed Both the Empiricist and the Contingent Repertoire

The authors surveyed in the preceding section of this chapter reviewed the materials using relatively equal portions of the empiricist and the contingent repertoires. That is, these reviewers attempted not only to deal with the scientific content of the criticisms they examined but also with the motivations and beliefs that might, they

thought, lie behind both the content of the criticisms raised and the manner in which they were presented. Below I will provide some brief examples of this mixture of repertoires.

From Ransom (1971):

... If the alleged phenomenon depends on mood, for example, we would *expect* the experimental results to be exactly as erratic as they have been, until the proper mood was discovered and was able to be evoked in subject and experimenter. But the search for unknown (and perhaps non-existent) factors could go on forever — one critic says it has gone on long enough: ‘... while protagonists of ESP could reasonably plead for breathing space to identify the elusive variables which lie at the root of this unreliability, most scientists now feel that they have had their chances and failed to deliver the goods’.¹²⁵ / I am not certain how Dr. Evans knows that this is what most scientists think about parapsychology, or when in science one closes the door and says, ‘time’s up’, but it is true that researchers have worked for many years and failed to come up with enough knowledge of the alleged phenomenon to produce the necessary repeatable experiment. Can the small number of researchers and the limited amount of funds be blamed? (p. 293)

Ransom does a number of things with this paragraph which is part of a general comment in the section devoted to ‘non-repeatability’. He first offers an empiricist reason for the lack of repeatability by evoking the notion of undiscovered confounding or intervening variables. He then quotes critic Christopher Evans who argued in a *New Scientist* article in 1969 that parapsychologists had already had sufficient time to prove their case through research. Ransom agrees that the repeatability problem has not been solved but he appeals to the possibility that such contingent factors as the lack of funding and personnel may, in fact, be behind the persistence of the problem.

In another example of this mixture, Ransom implies the possibility that the critic is being influenced by something extra-scientific rather than claiming that influence does *in fact* exist. In the section ‘Conclusive Evidence vs. the Possibility of Fraud’ (pp. 293-295), Ransom first quoted Hansel (1966):

It cannot be stated categorically that a trick was responsible for the results of these experiments, but so long as the possibility is present, the

¹²⁵ Taken from Evans (1969), p. 640.

experiments cannot be regarded as satisfying the aims of their originators or as supplying conclusive evidence for ESP. (p. 241)

Ransom goes on to review briefly both Hansel's and Price's (1955) use of the fraud hypothesis as condemnatory of an experiment even if only the possibility of fraud existed and not evidence that fraud had *actually* occurred. Ransom then commented on this type of criticism which he saw as '... not really a denial of the claim that there is evidence for the existence of ESP but rather it is a rejection of the claim that there is conclusive proof for its existence' (p. 294). Ransom then comments:

To me there is something unsatisfactory in leaving the matter in limbo like this. The important question is not whether cheating was possible in a certain experiment, but whether or not someone actually cheated. If cheating did not, in fact occur, the fact that the experimental design made cheating possible is of no concern. It is only because we may have no way of knowing whether someone actually cheated that we have to adopt the next best standard, that regarding the possibility of cheating. But, though, we cannot have *proof* of whether someone cheated or not, we can have *evidence* one way or the other. Is there any direct evidence of fraud? Is there any direct evidence of an honestly conducted experiment? These questions must be dealt with by the person who is not merely trying to prove or disprove the existence of psi phenomena. In short, if you have a situation where fraud or ESP are the only explanations for an experimental result, the result is evidence for (not proof of) ESP to the degree that the evidence for an honestly conducted experiment outweighs the evidence for fraud; and it is evidence for (not proof of) fraud to the degree that the evidence for fraud outweighs the evidence for an honestly conducted experiment. (p. 294)

In the quote above, Ransom sets the fraud hypothesis on the empirical grounds one would expect a scientist to desire before making a decision. Then he characterises Hansel's and Price's approach to the problem:

These comments are prompted by the fact that Hansel and Price, after correctly pointing out that if two explanations are possible neither one is proved, seem to be uninterested in the question of where the weight of the evidence lies. (p. 294)

In juxtaposition of the previous paragraph and this comment, Ransom implies that neither Hansel nor Price are interested in the evidential question their charge of fraud raises, but only in providing a condemnatory argument. To set aside the empirical

goals to which a scientist might aspire, the reader is left to wonder what extra-scientific factors lie behind Hansel's and Price's own arguments.

In Bauer's (1984) review, he does not combine empiricist and contingent repertoires in his examinations of the accuracy or applicability of criticism but rather presents sets of criticisms that he identifies as dealing with the content of parapsychological research, along side other sets of criticisms that are wholly contingent. Amongst the counter-criticism that focus on the latter, Bauer presented the following:

Already W. F. Prince (1930) observed that even when scientifically educated persons enter the field of parapsychology and pass the 'enchanted boundary' they suddenly appear to become one-sided in the information they collect and to ignore arguments. In short, they react so irrational[ly] in their opposition as would be unthinkable inside their own field. Apparently firmly rooted defences against the acceptance of the paranormal lie behind the rational discussions. Servadio (1958) when interpreting this defence proposes a psycho-dynamically based 'disbelieve reaction' to parapsychological phenomena. In Eisenbud's (1963, 1966) speculations the defence against psi is part of nature itself, and even parapsychologists are prevented from gaining experimental control over these powers by an 'unconscious sabotage' directed against their own efforts. (p. 153)

In other sections of the same review, however, Bauer focuses on the methodological content of criticisms (e.g., pp. 147-148).

Child's willingness to deal with both the empirical content of the criticisms he reviewed and with the motivations that might lie behind them was established in the first paragraphs of his review (e.g., pp. 191-192). For example, after dividing all criticism into the two broad categories of 'denial' and 'doubt', he wrote:

It is amongst outside critics, however, that denial and doubt appear most regularly and conspicuously as the motivational source of criticism of particular studies and of parapsychological research in general. Doubt is a reasonable position in general, and especially called for in a scientist. But where outright denial of the possibility of psi is found, a religious origin (in the broader sense of that term) seems likely, and three kinds of firm religious belief appear at times to be involved. (p. 191)

These were, Child believed:

... the humanistic religious movement, that, in its eagerness to reject the supernatural, rejects the paranormal ... A second, whose adherents would not like it to be classified as religious belief, yet which is closely related to the first, is the effort to adopt a world view based entirely on established scientific knowledge, attempting to reject all other sources of influence. To adherents of this view, evidence of paranormality is likewise threatening because it suggests that substantial segments of reality may be missing from their conception of reality. The third ... are those of some traditional religions, whose adherents sometimes see a scientific approach to the paranormal as a threat to their particular dogma about the supernatural. (p. 191-192)

As he worked through his descriptions of the individuals he reviewed, Child's *modus operandi* was to present first the empirical details of each critics' criticisms, and then to assess the accuracy and applicability of these criticisms. Further, the assessment phase of his treatment of each critic frequently included speculations as to the motivations and beliefs that led a critic to make the claims he or she offered in their articles. For example:

Hyman (1977) gives as the fundamental reason that other scientists need not attend to parapsychology, the fact that this field lacks any 'phenomena for which it can spell out conditions sufficient to guarantee their occurrence' (p. 49). The same thesis is presented in his most recent article (1985a), which is particularly thorough in its critique of 19th century efforts at controlled study of possible ESP and PK in psychics. It also reviews recent developments in experimental parapsychology, showing how a doubting critic can reasonably justify the view that paranormality is not clearly established. He does not confront head-on the question of how the evidence should be evaluated by someone who considers method more fundamental than world view to the scientific tradition. (p. 205)

Whilst Child gives Hyman credit for reviewing 'recent developments in experimental parapsychology', that is, for outlining the available evidence, Child also implies in the final sentence that Hyman has couched his arguments not in terms of such empirical considerations as method but rather in terms of the contingent variable of 'world view'.

In addition, Child reviewed, as Bauer did, both the empiricist and contingent repertoires in the prose of the critics he surveyed:

... Alcock (1985) continues to state that what parapsychology lacks is dependably replicable evidence, and to imply that its scientific status would change radically if dependable replicability should appear. But his attack seems to be on religious grounds. He feels sure that dependable replicability will not be found because it would be possible only if reality had the dualistic character that early psychic researchers hoped to establish, and because scientists can be confident that materialistic monism is the true belief. He seems to ascribe to everybody interested in parapsychological research a religious motivation, which may be frequent but is surely not universal, and which to me seems irrelevant to evaluation of research (unless it can be shown to provide reasonable grounds for suspecting fraud or careless error). (p. 197)

Conclusion

By surveying the reviews of criticisms that have preceded me, I have tried to give the reader a sense of the geographical landscape of controversy in parapsychology. A number of landmarks should be visible: (1) that across the decades there have always been critics who believe that ESP and PK are impossible and that because of that *any* explanations for positive results are preferable even if those explanations are based on bold speculation or innuendo; (2) that whilst the charge that parapsychology lacks replication may be a myth, it is true that parapsychology lacks a consistently repeatable experiment; and (3) that whilst critics may be able to gain ground with contingent arguments, proponents may well need to keep to the empiricist repertoire in order to be heard. In terms of the latter, there are certainly things to say such as: (1) sceptics who publish criticisms based either on ignorance or a misrepresentation of the facts of the primary published literature of the field can be strongly criticised; (2) sceptics who make wholly contingent arguments and do not raise empiricist issues can be strongly criticised; and (3) sceptics who do not do research can be strongly criticised.

In Chapter 4, I will return to the science studies literature and provide some examples of how science studies analysts have viewed the controversies over parapsychology. I will discuss generally some approaches that may be taken to the problem, focusing primarily on the potential usefulness of the rhetoric of science as a tool to account for the persistence of controversy. My rhetorical 'turn' in this thesis is

motivated partly by my understanding of rhetoric of science as another discipline's approach to scientific texts and scientific discourse and therefore, of potential use to a recasting of methodology in the psychology of science and in parapsychology. Equally, this turn is partly motivated by my reading of the reviews and criticisms that followed the publication of Rhine's (1934) monograph. It seemed to me that there might well be a relationship between the elements of structure and style in proponents' texts that influences the reception of those texts amongst critics. In addition to being a change in focus in terms of the methodologies used to frame the material, Chapter 4 also represents a narrowing of the focus from the wider terrain of criticism and response in the history of parapsychology to a specific landscape on which the 'ESP controversy' was contested.

CHAPTER FOUR

TAKING A TURN TOWARDS TEXT

Before I introduce and review studies in the rhetoric of science that may be useful to an analysis of the ‘ESP Controversy’, I will first survey the approaches to parapsychology that some science analysts have taken. This review will not be exhaustive of the material dealing with parapsychology that may be found in science studies, but is presented as a context for the case study in Chapter 5.

The Demarcation Problem

Mario Bunge

Amongst the most important treatments of parapsychology to have appeared in the science studies literature have been those which have focussed on the demarcation problem. This problem can be defined as the attempt to distinguish between mainstream and marginal science, or between mainstream and pseudoscience. Not only have philosophers of science tackled this problem (e.g., Carnap, 1995; Laudan, 1983, Popper, 1959, 1963), but so also have sociologists and historians of science (e.g., Gieryn, 1983; Good, 1983; Mausekopf, 1983). One prominent sceptic and philosopher, Mario Bunge (1982), also turned his attention to demarcation, as did a prominent parapsychologist (Morris, 1987).¹²⁶

Morris (1987) described the demarcation problem as asking the question of ‘whether we can demarcate between those areas of endeavour that represent productive scientific practice ... versus those that merely caricature the sciences and are actually bogus endeavors of no scientific value, the pseudo-sciences’ (p. 241). Morris noted that the application of the demarcation problem to a discipline claiming to be scientific might occur in one of three ways: (1) as applied to the discipline as a whole in a straight, declarative way such as ‘Parapsychology is a science/pseudoscience’; (2) as applied to

¹²⁶ Morris’s article appeared in a thematic issue of the philosophical journal, *Inquiry*, devoted to parapsychology. The other articles included in the *Inquiry* issue were by Beloff (1987), Braude (1987), Churchland (1987), and Dybvig (1987a).

the discipline as a whole in a way which allowed a more fluid classification such as ‘Parapsychology falls within the category of science/pseudo-science’; and (3) as applied to the workbench of individuals who labour within a parapsychological problem domain such as ‘Parapsychology often practices science/pseudo-science’ (p. 244).

To declare that parapsychology is a science or pseudo-science at the ‘level of the whole endeavour’, Morris felt, required that the social and cognitive content of the discipline be ‘fairly well-organised, cohesive, integrated, and definable ... both institutionally and in its practice’ (p. 244). Because of the diversity of training, beliefs, career trajectories, attitudes towards the phenomena, interpretations of the findings and so on, such an either/or determination of the scientific status of parapsychology as an enterprise would be impossible, Morris thought (pp. 245-246). An easier task would be to determine in a more general way whether parapsychology could be judged to be more scientific than pseudo-scientific or vice versa, or could be said to be proto-scientific (that is, in a kind of pre-scientific state). Such a fluid categorisation would not require strict ‘organisational cohesiveness’ (p. 247) but could assess how well the methodological practises of parapsychology were consistent with those practises normally thought of as scientific (p. 247). The third level of application of demarcation criteria, to the question of whether ‘parapsychology often (or primarily practises) science/pseudo-science’ (p. 248), clearly ‘focussed attention on the specification and evaluation of individual practises’ (p. 249). This application was more useful, Morris thought, because it examined the practises that are employed within the discipline, which could vary from those developed specifically for the field to those which borrowed from, and or were in use in, other fields. Demarcating at the level of practises could generate fruitful research questions, such as how specific scientific or pseudoscientific practises function to query the phenomena, or to develop theory.

Mario Bunge (1982), who has been an active and vociferous critic of parapsychology,¹²⁷ developed a list of ten criteria ‘to supply an accurate diagnosis of

¹²⁷ For other articles or comments dealing with parapsychology by this author, see Bunge (1984, 1987, 1991a-b).

pseudoscience' (p. 370). Bunge first set aside six criteria which had been offered by others as able to demarcate science from pseudoscience. They were:

- the consensus view
- the empirical content doctrine
- the success view
- the formalist doctrine
- refutationism
- methodism (p. 373).

Bunge rejected 'the consensus view' because it could also be characterised as the notion that 'science is uncontroversial' and clearly, Bunge thought, controversy was ubiquitous in all sciences. 'The empirical content doctrine' was also rejected because it granted legitimacy only on data that had been gathered empirically and the 'inductive synthesis thereof' which would leave out all theoretical sciences. The 'success view' conflicted with the proper goal of science, Bunge thought, which was 'truth' rather than 'success'. Bunge rejected the 'formalist doctrine' because it focused on the mathematisation of a science, which might leave out both 'experimental science and young science'. He also objected to 'refutationism' because pseudo-sciences, if they posted hypotheses that could be falsified, might then be deemed scientific. Finally, Bunge rejected 'methodism' which it meant that any discipline that used a version of the scientific method could be considered a science. One of Bunge's fears: that 'trying to catch ghosts with special nets' might be deemed scientific under 'methodism'.

Bunge provided his own list of characteristics by which one could demarcate a 'real' science from a pseudo-science. They were:

- that each of the following requirements could change in content over 'the course of time as a result of inquiry in the same field and in related fields'
- that the 'philosophical background' of the cognitive field in question 'consists of an ontology of changing things ... a realistic (but critical, not naïve) epistemology, [and] the ethos of the free search for

truth' as opposed to 'the ethos of a bound quest for utility or for consensus'

- that the 'formal background' of the cognitive field is comprised of 'up to date and mathematical theories'
- that the 'domain' of the cognitive field is 'composed exclusively of (certified or putatively) real entities ... past, present or future'
- that the 'specific background' of the cognitive field is 'a collection of up to date and reasonably well confirmed (yet not incorrigible) data, hypotheses, and theories obtained in other fields of [related] inquiry'
- that the 'problematic' of the cognitive field 'consists exclusively of cognitive problems concerning the nature (in particular the laws) of the [domains] ... as well as problems concerning other components' of the cognitive field
- that the 'fund of knowledge' of the cognitive field 'is a collection of up to date and testable (though not final) theories, hypotheses, and data compatible with those in [the specific background of the cognitive field] ... and obtained in [the cognitive field] ... at previous times'
- that the 'objectives or goals' emphasise 'discovering or using the laws of the ... [relevant] domains, systematising (into theories) hypotheses ... and refining methods'
- and finally that the methods 'consist exclusively of scrutable (checkable, analysable, criticisable) and justifiable (explainable) procedures' (p. 376).

This list of elements necessary for a cognitive field to be called a science led Bunge to posit yet another list of attitudes and activities which one could expect scientists either to display or engage in, and which pseudo-scientists could be expected to avoid. A scientist, Bunge argued:

- admits to ignorance and calls for further research
- has an appreciation of the weaknesses of his or her own field

- experiments, in the sense that problems are proposed and then attempts are made to solve the problems
- is open to ‘new ideas and attitudes’
- tests hypotheses
- seeks to discover lawfulness of the phenomena at hand, or to test the applicability of laws developed for related phenomena or fields
- seeks, believes in, ‘cherishes’ the notion that science is ‘unified’
- strives to be logical
- strives to be mathematical
- gathers quantitative data
- seeks to refute its own ideas through ‘counter-examples’
- checks data through methods already in use, or devises new methods
- seeks and attempts to eliminate ‘systematic errors’
- uses other disciplines, seeks to integrate its own work with other disciplines
- admits he or she might be wrong about theory or methods
- ‘settles disputes by experiment or computation’
- does not appeal to authority
- ‘[does not] suppress ... or distort ... unfavourable data’
- ‘updates [his or her] own information’
- seeks criticism
- writes for specialist audiences and not lay audiences
- and ‘is not likely to achieve ... celebrity’ (p. 380).

Whilst Bunge's system of nine requirements specified what a science may be in an ideal sense, and his 22 attitudes and behaviours can be considered an expanded checklist derived from Mertonian norms (some of which are redundant and others may well be seen as problematic descriptions of science practise in the eyes of historians *and* sociologists of science), the complexity of the system made it cumbersome to use.¹²⁸ Further, the philosophical specificity of the basic criteria and the accompanying list of attitudes and behaviours did not guarantee that objective assessments could be made of problematic disciplines.

In trying to apply such criteria to parapsychology, not only did Alcock's (1981) attempt to prove that parapsychology met all eight criteria for a pseudo-science fail in Morris's (1982) estimation due to Alcock's lack of familiarity with the underlying literature of the field, but Bunge (1982) himself categorised parapsychology as a pseudo-science by appealing to a litany of blatant inaccuracies and mischaracterisation of the research practises of the field (pp. 380-382).¹²⁹ Amongst these were: 'The typical parapsychologist is not very good at handling formal tools, in particular statistics' (p. 381); 'Parapsychology makes no use of any knowledge gained in other fields, such as physics or physiological psychology' (p. 381); and 'Parapsychology is an isolated field; it does not overlap with any other field of inquiry' (p. 382). Even a cursory glance at such canonical texts in scientific parapsychology as Pratt et al.'s (1940) *Extra-Sensory Perception after Sixty Years*, Wolman et al.'s (1977) *Handbook of Parapsychology*, the textbooks of Edge, Morris, Rush & Palmer (1986) and Irwin (2003), Beloff's (1993) history, and such recent books as Cardeña, Lynn and Krippner's (2000) anthology and

¹²⁸ Hess (1997) does not mention Bunge's article in his brief review of the philosophical discussion of the demarcation problem (pp. 21-22). Morris (1987) notes, after a brief description of an earlier, somewhat shorter list of criteria, that 'Such lists of criteria have also failed to win general approval, with disagreement over the applicability of the individual items, availability of counter-examples to each, and concern that such a complex list is not effective in sorting major endeavours into the distinct categories of science and pseudo-science' (p. 242).

¹²⁹ Bunge (1992) is opposed to a laundry-list of disciplines and approaches that, presumably, conflict with what seems to be his commitment to a naively positivistic view of science. Amongst the more consensually accepted approaches Bunge decries are: relativism (pp. 46-51), 'cryptobehaviorism' (pp. 51-59) and other aspects of what he sees as the 'new sociology of science'. Like Gross and Levitt, he characterises the work of the 'NSS' as having been produced by 'science-hating' analysts who are responsible for 'an utterly grotesque picture of science' (p. 73).

Radin's (1998) survey of research findings, provide ample evidence that whilst Bunge may be adept at *making* lists of demarcation criteria, he may not be so adept at *applying* them.

Trevor Pinch

Trevor Pinch's (1979) approach to the demarcation problem and parapsychology was decidedly more creative than Bunge's. Amongst other requirements, Pinch used the more widely-known Popperian demarcation criteria of conjecture and refutation (Popper, 1963). That is, the ability to falsify hypotheses proposed for testing was seen to be a key ingredient of science, and problem domains that proposed and tested hypotheses could be considered scientific. But in a strategy that foreshadowed the potential of science studies analyses for delivering intellectual surprise, Pinch turned the notion of falsification on its head. Where Bunge would have applied it only to the findings of experimental parapsychology if he had not already rejected the field as pseudo-scientific, Pinch applied the notion instead to the sceptic's frequent cry of fraud.

In the persistent controversy over parapsychology, as was seen in Chapter 3, the fraud hypothesis has been used to set aside the entire discipline when a single instance of fraudulent behaviour is uncovered or supposed by a number of critics.¹³⁰ Pinch contended that such a practice was not typical of science in general:

No one considers that one case of fraud at the Sloan-Kettering Institute means that the whole of cancer research can be explained away, but to show that one parapsychology experiment *might* have involved fraud, is apparently, often enough to dismiss the whole enterprise. (p. 331)

Pinch went on to argue that the consequence of the use of fraud as a universal explanation was that 'parapsychologists have been subjected to probing scrutiny by methodologists hoping to expose the "mistake" which they "must" have made ... [whilst] the alternative hypothesis of fraud has, apparently, so far escaped any such exhaustive examination' (p. 331).

¹³⁰ For example, see Child on Akers and Hansel, amongst others, on Table 4 in Chapter 3 above.

Pinch went on to provide just such an examination, analysing the scientific status of the fraud hypothesis by using four criteria that had been raised in sceptical evaluations of the evidence for the paranormal principle. These were: repeatability (pp. 336-338); ‘metaphysical bias’ (p. 339); the Popperian demarcation criteria of falsification (pp. 339-340); and ‘theoretical inadequacy’ (pp. 340-341).

For Pinch, evoking the Humean argument against miracles — that extraordinary claims required extraordinary proof — in essence involved the assertion that parapsychological claims were in some sense miraculous, that is, that they contradicted known scientific knowledge. Pinch argued that such an assertion rested on a cultural bias for calling ‘our present knowledge’ (p. 332) scientific, and demarcating as spurious anything which did not appear to be consistent with the analyst’s cultural definition. Instead, Pinch said, ‘...[i]t would seem that, if the problem of demarcation of genuine scientific knowledge from spurious knowledge is to be solved, it must be approached by the delineation of characteristics of science that are independent of the content of particular knowledge claims’ (p. 332).

In applying the criteria of repeatability to the fraud hypothesis, Pinch made the novel criticism that, for a fraud to be replicated, the original result obtained through fraud must first have appeared to genuinely support the paranormal hypothesis, preferably through successful publication of the study in which a convincing paranormal explanation seemed to account for the results. Once so accepted, then the fraud could be revealed. Further claims of fraud would then seem reasonable as a counter-explanation for other previously-reported results, thus ‘replicating’ the original fraud-caused result. That is:

It is not good enough to perform an experiment that is only convincing to the experimenter; others must also judge it so. If those claiming fraud do not get their initial (apparently) paranormal result published, then the critic can say that fraud has not been unequivocally demonstrated because the paranormal interpretation of the results was unconvincing. It is as though a magician pulls a rabbit out of a hat without showing us first that the hat is empty. No replication of fraud which meets this condition has yet been reported — at least none which warrants scientific attention. (p. 336)

Pinch footnotes these comments with the description of a demonstration of purported psi that James Randi once undertook which was published as a ‘real’ psychic event in *Psychic News*, a popular newspaper devoted to occultist theories of psychic phenomena and the like. Once published, Randi revealed that the demonstration had been accomplished through fraud. Whilst Pinch would characterise this as an attempted replication, he felt that the publication outlet, *Psychic News*, was hardly acceptable as an appropriate scientific venue in which the initial claim would have been critically examined as it might have been in a scientific journal. He argued that a scientific publication outlet would have been more likely to dismiss something as fraud, thus rendering the demonstration unconvincing and therefore not admissible as a ‘real’ replication of subject fraud.

Pinch concluded that the fraud hypothesis was usually raised only as a possibility with no proof that fraud had actually occurred.¹³¹ That is, rather than providing evidence for the fraud hypothesis, raising the spectre of fraud was merely an instance of ‘emphasis[ing] the original paranormal experiment, and ... produc[ing] a radically different interpretation of it’ (p. 337). This, Pinch argued, was a ‘hazardous’ enterprise in that ‘experimental variables which are crucial to establishing the fraud hypothesis are usually irrelevant to the paranormal hypothesis, and may not even be included in the original experimental report’ (p. 337). By not confining the argument to ‘details actually present in published accounts of experiments’ Pinch felt the debate would be ‘reduce[d] ... to the consideration of hearsay’, hence ‘independent replications are the only way to make progress in this area’ (p. 337).

Pinch wondered whether, ‘since repeatable evidence for fraud is lacking, it is worth investigating whether those believing in it have some metaphysical predilection for that hypothesis’ (p. 338). In his review of the critical literature, Pinch felt he had indeed found some basis for the notion that persons who favour the fraud hypothesis

¹³¹ The example drawn from Ransom’s review in Chapter 3 above in which he focused on the ‘limbo’ that resulted from Hansel’s and Price’s claim that fraud *might* have occurred supports Pinch’s argument.

without evidence do so because of pre-existing biases.¹³² Pinch reminded his readers that ‘There is, of course, more to science than experimental evidence, but other considerations (especially the appeal to explicitly philosophical arguments) are not of more importance than the facts’ (p. 339). In cases in which critics evoked *a priori* biases against the paranormal hypotheses, Pinch speculated, ‘metaphysics has acted as a substitute for solid evidence’.

Pinch next turned towards the Popperian demarcation principle of falsification:

Even if the fraud hypothesis had a firm empirical base it would not necessarily meet the criteria for being scientific. After all, many pseudo-sciences make empirical claims. Scientific methods demand other strictures — in particular, on the type of theorising we engage in to explain the facts. Here the fraud hypothesis falls well short of meeting one of the basic canons. ... The logic of the fraud hypothesis entails that it can never be refuted; it is inevitably true because it mows down all empirical data. Supporters of fraud who are not ingenious enough to find a ‘normal’ explanation to account for the parapsychological results on the basis of subject fraud alone can ‘extend the conspiracy’ to include investigators and independent observers. There is always a ‘normal’ explanation to be found, and such explanations are as open to imaginative innovation as science itself. (pp. 339-340)

So, Pinch concluded, in Popperian terms, ‘the fraud hypothesis is unfalsifiable’ (p. 340).

Finally Pinch reviewed the criteria of theoretical adequacy, the lack of which was often used to characterise parapsychology as a pseudo-science. Pinch asked whether it was reasonable to expect theoretical adequacy of the fraud hypothesis before it could be called scientific. He argued:

There has not (to my knowledge) been a successful and agreed theory of this kind in all the time since the fraud hypothesis was first advanced. It has been suggested that financial rewards provide an incentive: this may account for the cheating of some subjects, but it does not explain why scientists are fooled — or why, if they are not, they should attempt to

¹³² Pinch provides a quote from Hansel (1960d) in which Hansel wrote ‘In my view *a priori* arguments determine our attitude towards an experiment, and may save time and effort in scrutinising every experiment’ (p. 176).

deceive their fellow scientists. ... [It has been suggested] ... that academic rewards might also motivate some scientists, but it is clear that parapsychologists are severely disadvantaged in terms of both financial rewards ... and status; indeed, many young scientists thinking of a career in parapsychology are warned off by elder statesmen of the field, to save them wasting their time. (p. 340)

Pinch concluded that the fraud hypothesis failed to meet minimum criteria to be considered scientific, and yet fraud hypotheses was always preferred to the paranormal hypothesis. Such a disjuncture raised questions for Pinch about the 'the role of demarcation criteria in science' (p. 341). After briefly reviewing philosophical attempts to develop such demarcation criteria, Pinch rightly noted that a perhaps more productive locus for such analyses was in the case-study literature of science studies.¹³³ He argued that these studies had led science analysts to conclude that 'the repeatability of an experimental finding alone rarely produces an unambiguous "yes or no" verdict on the scientific validity of a knowledge claim' (p. 341). A variety of interpretative disagreements could arise which blocked the awarding of the term 'replication' to a study's results based on differing interpretations of the meaning of replication from scientific context to context. In practise, analysts had found that 'scientists can produce endless reasons to label certain experimental replications as "incompetent"' (p. 341), reasons which rested on 'judgements of plausibility, which are inevitably culture-bound' (p. 342).

Thus, Pinch contended, 'demarcation criteria (if we can generalise from the case of replication) do not appear to contribute to an explanation of why a particular belief is aberrant: they merely emphasise its aberrance' (p. 342) Demarcation criteria are thus 'open to negotiation', he felt, and the content and conditions of that negotiation become important not only in understanding a particular scientific conflict, but also in understanding how scientific knowledge is made consensual in a general sense.

¹³³ Pinch cited Harry Collins's early studies of replication (Collins, 1974, 1975) which eventually became solidified in Collin's conception of 'experimenter's regress' (Collins, 1985) in which results could not be interpreted as evidence without fitting into pre-existing expectations of what those results should be. Brian Wynne's (1976) article on 'deviance in physics' was also cited.

Ultimately, Pinch's case study of demarcation and the fraud hypothesis in parapsychology led Pinch to conclude that 'demarcation criteria do not provide us with independent access to the scientific validity of beliefs' (p. 343).¹³⁴

David J. Hess

In addition to studies of parapsychology done from the point of view of the sociology of science, work has also been done from the perspective of the cultural anthropology of science. David Hess's (1993) *Science in the New Age: The Paranormal, Its Defenders and Debunkers, and American Culture* is one such case study.¹³⁵

Hess's book contrasted three communities revolving around the paranormal: members of the Parapsychological Association (PA); members of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP); and members of the American New Age movement.¹³⁶

Hess adopted a 'cultural perspective' which, he claimed, made:

... it possible to interpret skepticism skeptically — or better, socially and culturally. Either in the form of the antagonistic CSICOP — or in the form of what could be called the kinder, gentler skepticism of the academic parapsychologists ... [a]t one extreme ... members of

¹³⁴ It is interesting that Pinch ended his article by cautioning his readers that 'It would ... be a mistake to read [the article] ... as in any way supporting the claims of the parapsychologists. My interest is *not* in the *validity* of either hypothesis, but in the types of argument made for and against them' (p. 344). Pinch's comments are, perhaps, a foreshadowing of the contribution of the principle of symmetry to the recent 'science wars' (see Edge, 1999, for a useful summary of these debates) as well as of recent critical assessments of symmetry's methodological usefulness (e.g., Scott, Richards & Martin, 1990). Even Collins and Pinch have since commented that their neutrality as analysts has, in essence, allowed proponents of generally-rejected lines of research to gain some rhetorical advantage over proponents of other, opposing lines of research that had otherwise seemed to have had 'consensual' science behind them (e.g., Collins & Pinch 1993, 1998).

¹³⁵ Hess, who works in the anthropology of science and technology, did a masters degree in parapsychology at John F. Kennedy University in the 1980s. His doctoral work at Cornell University focused on the interplay between parapsychology and spiritism in Brazil, and he supported himself during his time in Brazil partly through teaching statistics at an unaccredited spiritist university that gave degrees in parapsychology (Hess, personal communication, 1988, 1990). Although his intellectual interests have moved beyond parapsychology to the extent that he has not been considered an 'insider' for many decades, he is well versed in both the social and the cognitive aspects of the field.

¹³⁶ Hess's book also included an analysis of the portrayal of the paranormal in Hollywood movies as well as an appendix that discussed the methodological and theoretical concerns of an anthropology of science.

CSICOP portray New Agers as fools bound to dogmatic superstitions who would lead the country towards an apocalypse of unreason. At the other extreme, New Agers see skeptics as bound to their own superstition of dogmatic materialism that could result in environmental Armageddon. From this perspective, the discourses of CSICOP and the New Age movement can be seen as two variants — or better, as polar opposites — with parapsychology as the mediating discourse. In this “chiasmus” all the actors nonetheless view themselves as “skeptical” in their own way. (pp. 14-15)

For Hess, the cultural struggling between these three communities could be seen as ‘the varieties of skeptical experience’ (p. 15), a novel and compelling assessment of the different perspectives on the paranormal. To this mix, Hess also added what he called the ‘fourth voice’ (p. 15) which was the overlay of ‘anthropologists, sociologists, historians, cultural critics and various other students from the humanities and social sciences’ who, whilst not ‘aligned’ with any of the three communities, were, Hess claimed — and amongst these individuals he included himself — ‘by no means neutral and impassive observers in these debates’ (p. 15). He characterised his work as ‘deconstruct[ing] boundaries among the three cultures ... [and showing] how this shared “paraculture” is itself part of the broader culture’ (p. 15).

Hess first provided a brief history of the New Age movement, beginning with Mesmer and Swedenborg and moving through the influence of the European Spiritist Allan Kardec into the British and American Spiritualist movements. He traced the splitting off from these movements of the individuals who founded the academic psychical research and parapsychological communities in Great Britain and the United States whilst critically examining the importance of Spiritualism and spiritualists in the founding of the Society for Psychical Research in London, and of the American Society for Psychical Research.

Hess’s history of the sceptical movement began in the 19th century with the reaction of the medical establishment to spiritualism, dealing with such individuals as Joseph Jastrow and Amy Tanner, amongst others. The founding of CSICOP, the early disputes over the direction of its journal (e.g., Pinch & Collins, 1984, Truzzi, 1982), and

the founding of the Society for Scientific Exploration (SSE) were also reviewed.¹³⁷ The SSE, Hess noted, took a wider view of anomalies and sought, by its conventions and journal, to provide a venue in which paradigm-contradicting phenomena, “facts” and theories could be discussed openly and productively by a scientific elite.¹³⁸

Hess attempted to situate each of the three groups within the wider post-modern cultural context in the United States, drawing on such disparate elements of culture as advertising, art, evangelicalism, the popular press and publishing, and the media (pp. 37-40).

In the second part of his book, Hess turned his attention to the ‘cultural construction of skeptical and paranormal discourse’ (p. 41). He examined: differing understandings of ‘self’ and ‘other’ amongst the three groups; the cultural meaning of the characterisation of such individuals as J. B. Rhine (for academic parapsychologists) and Paul Kurtz (for sceptics) as heroic; and the differing impact of such variables as gender and hierarchy. Regarding the latter, Hess noticed: (1) the importance of female leadership in the New Age movement (pp. 95-98); (2) the bifurcation of academic parapsychology into ‘life and lab’ in which spontaneous case research and the discourse of experience became associated with both a feminine perspective and a subordinate position in the field, and experimental, laboratory-based research, physics- and engineering-based theoretical work became associated with a masculine perspective and a dominant position (pp. 98-105); and (3) in the sceptical movement, the tendency to

¹³⁷ The Society was founded in 1982 to provide a forum for scientists from all branches of science to present and discuss anomalies. Founders included such individuals as astronomers Charles Tolbert and Lawrence Fredericks of the University of Virginia and Peter Sturrock of Stanford University, physicists Harold Puthoff of the Institute for Advanced Studies in Austin, Texas, zoologist Roy Mackal from the University of Chicago, sociologist Marcello Truzzi from Eastern Michigan University, and a number of parapsychologists including the late Koestler Professor Robert Morris of the University of Edinburgh and Ian Stevenson of the University of Virginia. Membership was limited to individuals who held doctoral or medical degrees. The Society publishes the *Journal of Scientific Exploration*.

¹³⁸ Hess made an interesting comment in his description of the SSE, that whilst it was ‘ostensibly more acceptable to orthodox scientists’ (p. 34) and thus could be seen as ‘more scientific’ than the Parapsychological Association, because of the willingness of the SSE to entertain studies of UFOs and astrology, and because of the tolerance SSE members showed to field investigation, most members of the Parapsychological Association, a largely experimental body, dismissed the SSE as ‘less scientific’ than the PA. Hess noted that ‘the debate over which group is more or less “scientific” depends on the multiple and complex criteria that can be invoked to construct the boundary between science and non-science’ (p. 35).

characterise the paranormal ‘other’ as feminine and as associated with uncontrolled nature or uncontrollable (once unleashed) consequences, whilst debunking — the principle discourse of sceptics — came to be seen as ‘very masculine, even a macho art’ (pp. 106-108). Hess’s conclusion on this topic is worth repeating here:

Distinctions marked for hierarchy and gender therefore play themselves out at a variety of levels. Among the cultures, skepticism is relatively masculine, the New Age movement relatively feminine, and parapsychology somewhere in between. Within these cultures, the internal Others may be marked for gender as well. The gendering of internal Others is clearest in parapsychology, but there are suggestions of it in the other two cultures. Within the New Age movement, goddess religion is much more explicitly feminist and women-oriented than Ferguson’s [1987] Aquarian conspiracy, and in some way the martial metaphors of debunking mark it as more masculine than the erudite critiques that Kurtz calls “neutrality.”¹³⁹ Finally, each culture conceives of its paranormal Other (the sceptics’ world of the paranormal, the parapsychologists’ paranormal phenomena, and the New Agers’ goddess) as feminine. All sides agree that orthodox science and skepticism, no matter how they view it, is a relatively more masculine category. (p. 115)

For Hess, one of his important contributions was the uncovering of ‘internal discursive boundaries’ in all three of the communities he examined: “‘neutral’ skepticism versus debunking, experimental parapsychology versus spontaneous case research, and the relatively scientific and erudite writing of Marilyn Ferguson versus the mystical discourse of channelers, goddess worshippers, and crystal healers’ (p. 143). The presence of these internal boundaries served to underscore the point that Hess’s analysis had provided ‘an expansion of the concept of boundary-work by examining how in a concrete case study it can operate in complex and multiple ways’ (p. 145). Hess believed that he had shown the following:

... scientific boundaries are recursive, nested, and multiple; there are layers of scientificity that become clearer as one unfolds levels of skepticism and “pseudoscientificity” both within and across discursive boundaries. Boundary-work therefore is going on in all directions, not just in the direction of orthodox science towards religion and

¹³⁹ The discussion appears in Hess (1993), pp. 106-107.

“pseudoscience” (pp. 144-145).

Lawrence J. Prelli

There is something very satisfying about the application of science studies methodology to the complexities of scientific parapsychology for an ‘insider’. It is not, of course, appropriate for such studies to determine the evidentiality of the findings, or to assess the goodness-of-fit of parapsychological models and theories to the shape of the natural world. The evaluation of the ‘success’ of scientific work of parapsychology lies along a different path. Nor is it possible, using the tools of science studies, to make clear sense of the reliability and validity of sceptical analyses of the scientific content of parapsychology in some global, controversy-killing fashion. But it *is* possible, using a science studies analysis, to uncover a deeper understanding of parapsychology as a social organism, and as a meaning-making enterprise — albeit on the margins of mainstream science, and as one amongst many variously-situated knowledge-building endeavours.

Lawrence Prelli’s (1996) use of parapsychology in a science studies context is in actuality a work of rhetoric of science, a segment of science studies that will be introduced more fully in the next section. I decided to include it here, however, to represent those works in which some aspect of parapsychology itself, or of the discourse that surrounds parapsychology, is used to make a more general point.¹⁴⁰

Prelli’s article focused on the rhetorical *topoi* used to construct scientific *ethos* in general and on the use of norms (Merton, 1973) and counter-norms (e.g., Cole & Cole, 1973, Gaston, 1978, Mitroff, 1974, Mitroff & Mason, 1981) in a specific controversy over whether Koko the Gorilla was able to use language in a meaningful and independent way.

Prelli noted that the adherence to norms and the avoidance of counter-norms varied in importance depending on the rhetorical situation in which the individual writer or speaker found themselves. In the situation in which the speaker or writer was

¹⁴⁰ Another example of this type of work is Charles Taylor’s (1996), *Defining science: A rhetoric of demarcation*, which looks at rhetoric in the contexts of creationism and parapsychology.

identified with ‘such “non-scientific” pursuits as securing personal celebrity with lay audiences, achieving political or religious aspirations or perpetuating beliefs that have occult or supernatural implications’, the establishment of a ‘professional *ethos*’ was especially difficult.

Using parapsychology and creationism as examples, Prelli noted that when orthodox scientists ‘attack’ the work of others whom they wish to characterise as pseudo-scientific, the discourse pivots on descriptions of such individuals’ failure to either understand or exhibit Mertonian norms. That is, the pseudo-scientific other is depicted as either *incapable of, or unwilling* to do ‘good science’ (p. 89, italics in the original).

Such efforts serve what Gieryn (1983) ‘boundary-work’ which, Prelli argued, has both an epistemological and a practical purpose in the sense that orthodox scientists who were able to draw such a distinction between themselves and non- or pseudo-scientists, were also able through the use of rhetorical *topoi* to ‘insulate scientific research from political interference’, preserve their intellectual status, and block access to those who would compete for resources and rewards. The scientists on the receiving end of this boundary-work would also ‘compose rhetoric about scientific *ethos*’ but in doing so there would be efforts to expand the boundaries of orthodox science or make them more permeable to new ideas. By describing their conformance to norms and their avoidance of counter-norms, unorthodox scientists were ‘attempting to show that they, too, were scientists and that their claims should also be taken seriously as reasonable scientific contributions’ (p. 91). Prelli argued that rhetorical case studies on the margins of orthodox science could produce a deeper understanding of the construction of *ethos*.

The Rhetorical Turn

Unlike the field work he (Hess, 1991) had done in a previous, related study of parapsychologists and spiritists in Brazil, the work described above (Hess, 1993) focused on the analysis of text. Even so, Hess’s methodology owed more to his anthropological training, to his understanding of Gieryn’s theory of boundary work, and

to a sociologically-informed analysis of discourse than it did to rhetoric of science.¹⁴¹ Prelli's (1996) article, on the other hand, seemed indicative to me of the usefulness of such an approach. For example, amongst other points, Prelli noted that critics accuse parapsychologists of not '...act[ing] like "real" scientists' and instead, of wilfully violating Mertonian norms. That is, he found, parapsychologists were characterised as being:

- ... openly defiant of both the 'universal' consensus on accepted and rejected knowledge and the need for empirical confirmation of technical claims (*universality*);
- ... pursuing extra-scientific motives including advancement of beliefs in the supernatural (*disinterestedness*);
- ... dogmatically attached to their allegedly 'scientific' claims (*organised skepticism*); and
- ... incapable of participating in the 'real' scientific community as indicated by their inability to secure visible positions, 'legitimate' research funds, and publications in orthodox journals (*communalism*). (p. 103, n. 47)¹⁴²

To some extent because of Prelli's comments, I began to suspect that borrowing methodologies from the rhetoric of science to examine parapsychological texts would provide useful insights into the persistence of the substantive controversy in parapsychology and suggest practical methods by which the persuasiveness of parapsychological texts might be enhanced.

Privileging the Text

In the preface to the second edition of his seminal work, *The Rhetoric of Science*, Alan Gross (1996) noted that his text was intended to create a disciplinary

¹⁴¹ Although Hess includes a few titles from rhetoric of science in his description of his approach to the communities in his 1991 book (p. 183) just as he did in his 1997 textbook (p. 159), he characterised his own work as broadly interpretative rather than relying on any specific method he associated with rhetoric of science.

¹⁴² Bunge's discussions of the attitudes and behaviours of pseudo-scientists, as reviewed above in the section on demarcation, seem especially consistent with rhetorician Lawrence Prelli's description of attempts to discredit the scientific *ethos* of parapsychologists.

space (p. viii). Gross not only delineated his goals for rhetoric of science, but also situated them in the wider discussions of science studies. He felt it was important to chart a cohesive methodological and theoretical territory which future rhetoricians of science might profitably explore.¹⁴³

To do this, Gross not only provided examples of work which ‘reconfigured’ (p. xxi) classical rhetoric studies but also presented a series of his own case studies that showed the utility of his approach. Amongst the examples were: Gaonkar’s (1993) study of rhetoric within scientific texts in general; and Boyd’s (1979) analysis of the use of metaphor in scientific theory, a topic that has also been examined in the context of parapsychology (e.g., Williams & Dutton, 1998). These and other such studies took as their inspiration Perelman and Olbrechts-Tyteca’s (1971) restatement of classical rhetoric, *The New Rhetoric: A Treatise on Argumentation*. Other landmarks in the re-involution of rhetoric include: Vickers’s (1993) reviews of the reinstatement of rhetoric at various points in history, including the more recent ‘recovery’ sparked by Perelman and Olbrechts-Tyteca; and Fahnestock’s (1998) detailed introduction to both the history, and the current usefulness of classical rhetorical analyses of science texts. Amongst the research topics Fahnestock focused on were: (1) examinations of the structure of scientific records and their relationship to the published documents which were dependent upon them; (2) studies of the persuasive importance of rhetorical features of scientific texts in the early stages of theory-change, when convincing evidence may not be available; and (3) analyses of style and arrangement of rhetorical features in documents designed to be self-persuasive. Other fruitful areas she identified were: the growth of the scientific article as a genre; and the rhetorical features of texts produced in the context of controversy.

Studies of the relationship of records to documents have ranged widely over the history of science. For example, Gross analysed Boyle’s records of the experiments that led to the framing of ‘Boyle’s law’ (pp. 85-91) as well as Einstein’s personal records on relativity as they related to the published version of the theory (pp. 92-96). Gross concluded that published scientific papers instantiated a myth of logically-developed

¹⁴³ Gross is, of course, not without his detractors. See, for example, McGuire & Melia (1991).

scientific progress modelled on Baconian induction, in contrast to the much more complex trajectory from experiment to theory that was revealed by the actual work records. Historians of science have also found this disjuncture between the written depiction of a scientific work in published documents and the underlying laboratory records (e.g., Holmes, 1987, 1991).

A number of examinations of the relationship of rhetorical features to the stage of theory development have been published, amongst them Gross's (1996) study of the rhetoric inherent in Copernican texts (pp. 95-110) in which he found support for Feyerabend's notion that, in the early stages of a theory when evidence is scanty, such rhetorical features as 'style, elegance of expression, simplicity of presentation, tension of plot and narrative, and seductiveness of content' (quoted from Feyerabend, 1978, p. 157) were essential to the persuasiveness of the texts, and, in this specific instance, to their ability to provoke a paradigm shift. In a related study, Gross (1996) examined Newton's *Optiks* (pp. 111-128) so as to illuminate the rhetorical qualities of a document which, essentially, forestalled such a shift.

Studies related to the development of the scientific article itself have included: the growth of the scientific journal as a genre (e.g., Browman, 1991); the repackaging of 'anecdotes and experiments' into coherent scientific reports in the 17th-century (e.g., Dear, 1991b); the use of argument in scientific reporting (Holmes, 1991); and the impact of referees on the construction of science report-writing conventions (Hunt, 1991). Done by historians of science, rather than rhetoricians, the studies were part of the 'literary turn' in intellectual history, and as such had similar goals to those outlined by Gross in that they illustrated the evolution of narrative form in science (Dear, 1991a).

Similar examinations of the evolution of the journal article as a literary form have appeared in communications, linguistics and science studies. Amongst these are: Bazerman's (1988) analysis of the development of the scientific journal using articles published in the *Philosophical Transactions of the Royal Society* between 1665 and 1880; and a variety of studies of the style, structure and rhetorical content in scientific articles (e.g., Bazerman, 1995; Bazerman & Paradis, 1991; Latour & Woolgar, 1979; Woolgar, 1976, 1980).

In terms of the rhetorical features of documents designed to be self-persuasive, Gross examined the content of Charles Darwin's *Red Notebooks* in which Darwin gathered and commented on facts he considered potentially important to his emerging theory, which were then re-presented in a more cohesive form in the *Origin of the Species* (Darwin, 1859) (Gross, 1996, pp. 144-159). For Gross, the content of the *Notebooks* showed that 'Darwin's most creative phase is appropriately described as a rhetorical transaction within the self' (p. 159), a conclusion somewhat at odds with Kohn's (1980) notion of the *Notebooks* as a site in which theories were tested and rejected before the final theory was formed.¹⁴⁴

Finally, rhetorical studies of controversies have included: Bazerman's (1988) examination of the controversy over Newton's optics (pp. 80-127); Fahnestock's (1997) treatment of the early date/late date migration controversy amongst archaeologists; and Lynne and Howe's (1997) study of 'punctuated equilibria' in evolutionary theory.

The synthesis Gross (1996) wanted to forge also entailed a commitment to a complex and *complete* analysis of science as rhetorical, as constructed by, and existing in, language and its uses. One of the ways in which he accomplished this was to question the bestowment of 'high esteem' (p. 21) on the scientific enterprise by other analysts who had come before him (e.g., Overington, 1977; Ziman, 1968). He analysed sample text from three arenas — 'political oratory, scholarly argument, and scientific reports' (p. 21) — to look for rhetorical characteristics that would warrant privileging scientific writing over other forms of rhetorical persuasion, choosing the use of analogy as a primary focus of the study. Gross concluded that science writing had taken a form which nurtured what Gross called the 'useful illusion ... [that] the results of science depend[ed] not on argument but on nature herself' (p. 22). In a close analysis of the texts that announced Watson and Crick's 'discovery' of DNA's double helix (pp. 54-65), Gross stated that his intention was not '... merely to rehearse, to deepen, or to extend the claim ... that Watson and Crick use persuasive devices to convince scientists of the correctness of their structure; rather, [he wanted] ... to suggest a more radical claim: that the sense that a molecule of this structure exists at all, the sense of its reality, is an effect

¹⁴⁴ For other examinations of Darwin's rhetoric see, for example, Campbell (1997) and Young (1986).

only of words, numbers, and pictures judiciously used with persuasive intent' (p. 54).¹⁴⁵ For Gross, the rhetoric of science was thus both 'a discipline and a perspective from which disciplines can be viewed' (p. 52).

Since the publication of Gross's discipline-building effort in 1990 and the publication of its second edition in 1996, a number of important works in the rhetoric of science have been issued. The topic of demarcation was revisited, albeit from the rhetorical point of view, in Charles Taylor's (1996) study of the constructive contribution of rhetoric to attempts to demarcate creation science and cold fusion from mainstream science.

Another useful case study was Myers' (1990) study of 'writing biology' in which the author examined the narrative structure of a variety of scientific texts. Included were: grant proposals; eventually-published articles studied with submission versions, their referee reports, correspondence with editors, and pre-publication revisions; and the text and reception of E. O. Wilson's (1975) controversial book, *Socio-biology*.

In 2001, Ceccarelli, examined the notion of interdisciplinarity using three texts that had had varying levels of success at forging interdisciplinary research programmes. Her methodology illuminated the impact of very different rhetorical strategies on the reception of the works. More than that, Ceccarelli provided a successful example of interdisciplinarity in and of itself in that classical rhetoric, rhetoric of science, and the history of science were woven together in her insightful analysis.

One of the most recent rhetorical studies — and one of the most ambitious to date — was Gross, Harmon and Reidy's (2002) comprehensive history of the development of the scientific article, both as text and as an embodiment of argument, in the natural and physical sciences literature from the seventeenth century to the modern era. Gross and his colleagues conceived of the project as proceeding in 'three acts: the creation of arguments for and against knowledge claims about the natural world, the artful deployment of these arguments in a text, and their representation in the syntax and

¹⁴⁵ In this section (pp. 54-65), Gross worked from a subset of Watson and Crick's scientific papers (e.g., Watson & Crick, 1953a, 1953b, 1953c) as well as from Watson's biography (1966) and his more recent history of scientific research on DNA (Watson & Tooze, 1981).

semantics of natural languages' (p. vii). Using a method both analytic and quantitative, they digested a wide variety of materials in English, French and German. Writing for an interdisciplinary audience they hoped would include historians and analysts drawn from science studies, linguistics, literary criticism, communications, and classical rhetoric, as well as from amongst rhetoricians of science, the authors adopted a version of 'selection theory' as a framing mechanism so as to determine how and why the modern scientific article had evolved to its present form. Based on their research, they concluded that 'the current scientific article is, on the whole, an accurate reflection of the world as science conceives it, an effective means of securing the claims of science, and an efficient medium for communicating the knowledge it creates' (p. ix). That is:

Translated into evolutionary language, selection pressures favor a style that represents science as an objective enterprise, foster more efficient communication, and produce stronger, more flexible argumentative strategies. These result in either a gradual or continuous change in some feature over time — as in the general decline of personal pronouns and [the] corresponding rise of passive voice — or a relatively abrupt change — as in the emergence of a heading abstract ... (p. 231).

More will be said about Gross, Harmon and Reidy's work in the next chapter. Suffice it to say, however, that examinations of the rhetorical elements of texts have provided useful glimpses into the features of scientific texts that facilitate and inhibit their ability to communicate the scientific content they embody, and to contribute to the building of scientific knowledge and consensus.

Conclusion

In this chapter I have reviewed a selection of works within science studies in which parapsychology was either featured in the analysis or used as an example to illustrate a particular point. From Pinch's ingenious discussion of the fraud hypothesis to Bunge's attempt to establish demarcation criteria that would preclude parapsychology from the workshop of science to Hess's cross-community examination of critics, parapsychologists, and New Age adherents and Prelli's brief examination of the use of norms and counter-norms as rhetorical *topoi*, I hope I have shown that an examination of both critical and proponent texts in parapsychology can profitably occur. I also hope that

the brief review of the type of case study being done by rhetoricians of science has at least suggested that there is room for such work on the field's canonical texts.

In Chapter 5, I will describe the case study that I have attempted using such methodology. One of the questions that will be asked is whether the scientific article in parapsychology, as it developed between the publication of *Extra-Sensory Perception* in 1934 and the publication of *Extrasensory Perception After Sixty Years* in 1940 could be characterised as following that movement towards objectivity and efficiency in communication that Gross, Harmon and Reidy (2002) found in scientific articles in general. I will draw on some of the quantitative methodology they developed, as well as on convergent work done by others (e.g., Bazerman, 1988; Montgomery, 1996). Although I analysed the rhetorical elements of the complete set of the published materials that comprise the ESP controversy that took place from 1934 to 1944, for the sake of brevity, the case study presented in Chapter 5 will focus mainly on the two books that bracketed this period. It was my hope that I would uncover some of the elements of style and structure that complicated the reception of the experimental work Rhine and his colleagues presented to mainstream psychology in the mid-decades of the 20th century.

CHAPTER FIVE

THE ESP CONTROVERSY

In this chapter, I will examine the two most important documents in the controversy that began with the publication of J. B. Rhine's (1934) monograph, *Extra-sensory Perception* and effectively ended four years after the publication of Pratt et al.'s (1940) *Extrasensory Perception After Sixty Years*.¹⁴⁶ Although more than 100 articles directly related to this controversy were published from 1934 to 1944, the need to present additional case study materials in subsequent chapters prohibits me from including any of the other materials.

The principle question being asked in this chapter is whether differences in style and structure of the published materials of proponents in parapsychology may have contributed to the persistence of the controversy. The two books provide a useful comparison, if for no other reason than that *Extra-sensory Perception* appeared at the beginning of Rhine's career, and *Extrasensory Perception after Sixty Years* was a team-written, self-conscious effort to defend an on-going and hotly contested research programme. The first section of this chapter will deal with the former, and the second section will deal with the latter. I have used tools from the rhetoric of science throughout this chapter. They will be introduced as they are needed.

Extra-Sensory Perception

Before I examine the style and structure of this document, I think it is important to describe the context in which it was published as well as its content.

J. B. and Louisa Rhine came to Duke University in the fall of 1927 on a six-month grant that required J. B. to analyse mediumship transcripts obtained by the donor.¹⁴⁷ Six months turned into a lifetime, first under the auspices of Duke University,

¹⁴⁶ By this I mean this phase of the controversy. It re-emerged in 1955 with the publication of Price's article in *Science*.

¹⁴⁷ The donor was John F. Thomas who later obtained a PhD from Duke based on doctoral research that included an extended analysis of the transcripts. For more information of their early years in Durham see Rhine (1983, pp. 115-117, 122-154) and Mauskopf and McVaugh (1980, pp. 79-88). Mauskopf and McVaugh's (1980) volume is in essence an examination of psychical research up to 1940 with an emphasis on Rhine's work. Their work was based not only on a series of extensive interviews with the Rhines and

and later in their own private institute. From 1928 through the end of the 1929/1930 academic year, J. B. Rhine worked as William McDougall's assistant in a series of controversial Lamarckian experiments. He also taught undergraduate and graduate courses, first in philosophy — before the philosophy and psychology departments became formally separated — and later in psychology. Thanks to funding provided by the University Research Fund in the summer of 1930, Rhine was able to begin a sustained research programme designed to test two of the traditional phenomena of psychical research, clairvoyance and telepathy. This research, once begun, continued to take up more and more of Rhine's time, until it became his primary activity at Duke.

Published by the Boston Society for Psychical Research, Rhine's (1934) monograph, *Extra-Sensory Perception (ESP)*, provided a summary of the work conducted by the small team Rhine had assembled around him from 1930 to 1934. The volume began with a foreword by McDougall, in which he introduced the reader to Rhine as a scientist and as an individual, vouching for the character and integrity of Rhine's collaborators and the students who participated in the experiments. For McDougall, Rhine's research constituted the first step towards the ‘‘naturalisation’’ of psychical research within the universities’ (p. xiii). McDougall noted that the content of Rhine's monograph showed the importance of having a university environment in which to do such research, in that considerable progress had been made; and that the process of this experimentation served the goals of a liberal education as well, providing practical familiarity with science for the students who were involved as subjects and for those who acted as assistant experimenters.

McDougall commented that he had, at times, become acquainted with a research report which seemed, *prima facie*, to be sound, but which became suspect upon meeting the scientist who had conducted the research. McDougall believed he was not alone in using personal knowledge to temper his evaluation of experimental reports.

with male laboratory members as well as with collaborators and subjects who had survived to the 1970s, but also on an extensive review of archival sources both at Duke and elsewhere. Their decision not to interview female members of the laboratory staff other than Louisa Rhine was unfortunate, but fairly typical of historians of that era (see Alvarado, 1989). Alan Gauld's (1968) history of the SPR also suffers from this androcentric bias (see Zingrone, 1994b).

Consequently, he felt it was important to impart his personal knowledge of Rhine and his team so as to add credibility to the claims made in the monograph that followed.

To that end, McDougall provided a brief biographical sketch of both J. B. and Louisa Rhine, emphasising that they had given up promising careers in biology to take up training in psychology and psychical research.¹⁴⁸ McDougall characterised the Rhines as ‘working scientists’ who were not, as many of their predecessors in psychical research had been, ‘moneyed amateurs’ (p. xiv). The act of giving up what could have been productive, mainstream scientific careers, was to McDougall’s mind ‘magnificently rash’ (p. xiv).¹⁴⁹ More importantly, he noted, the Rhines had not changed disciplines out of a personal motivation to contact some dead relative, but rather ‘... as far as I could and still can judge, [out of] the desire to work in the field that seemed to contain the most promise of discoveries conducive to human welfare’ (p. xiv).¹⁵⁰

McDougall admitted he felt somewhat responsible for the Rhines’ movement into the riskier science of psychical research because they had credited McDougall’s (1911) book, *Body and Mind*, and his 1927 article on the university study of psychical research as bringing them into the field. He described how the Rhines had arrived on his doorstep in Cambridge, Massachusetts when he was leaving the United States for a year,

¹⁴⁸ J. B. and Louisa Rhine had obtained doctorates in plant physiology at the University of Chicago in 1925 and 1923, respectively. In 1925, they began to set aside that discipline in order to study psychical research. Once at Duke, J. B. Rhine taught courses in history of science, and studied psychology and philosophy with McDougall. Louisa Rhine had a part-time teaching appointment in botany but collaborated with her husband on parapsychological research before she resigned to have children. She (1983) does not, nor do Mauskopf and McVaugh (1980) mention, that she had any formal training in psychology at Duke, although she attended psychology classes with J. B. at Harvard. During her childrearing years, in so far as she could, and after she returned full-time to the Duke Parapsychology laboratory in 1955, she focused only on parapsychology (Rhine, L. E., 1983, pp. 129-152, 209-211, 257-258). See also Mauskopf & McVaugh (1980, p. 326, note 14) for their assessment of the Rhines’ research collaboration.

¹⁴⁹ J. B. Rhine had had an appointment at a small West Virginia state university teaching botany before he and Louisa decided to take up psychical research full-time. McDougall was, perhaps, over-estimating the appeal the Rhines’ alternate future had, even for them.

¹⁵⁰ McDougall also commented in the sentence immediately following this that ‘botanical research’ did not seem to him to be a discipline for which one could ‘retain enthusiasm ... unless one is a scientist of the peculiarly inhuman type’ (p. xiv.). Rhine may have not had the usual personal motivation for entering the study of psychical research, but he had another one: the need to be a ‘crusader’, to have a ‘cause’. Louisa Rhine (1983) felt this was a salient fact about her husband, saying that he had determined that psychical research was a discipline in which he could fulfil that need (e.g., p. 183).

and how Rhine and his wife had taken courses at Harvard and become involved with the Boston Society for Psychical Research in McDougall's absence, so as to prepare themselves for their future research.¹⁵¹ When McDougall returned to the United States, the Rhines followed him to Duke University.

McDougall described J. B. Rhine as a 'fanatical devotee of science, a radical believer in the adequacy of its methods and in their unlimited possibilities' (p. xv). In addition, McDougall also saw considerable social gifts in Rhine, necessary, McDougall said to '... overcome the initial difficulty of inducing students to participate in and to give time and effort to research of a kind which is looked at askance by the world in general and by the scientific world especially' (p. xvi). Rhine's enthusiasm served, McDougall thought, to inspire confidence in his team of collaborators, who, McDougall said, were students of the highest calibre '... in respect of training and ability, of scientific devotion and personal integrity' (p. xvi).

Finally, McDougall addressed the issue of whether or not Rhine's collaborators could have deceived him. For McDougall, not only did his assessment of the personal characters of the individuals he knew preclude such a thing, but given the sheer number of experimenters and students involved, McDougall believed that a conspiracy of such proportions could have occurred was 'wildly improbable' (p. xvii).

Dr Walter Franklin Prince's introduction focused instead on the research. Prince gave background to the reader, noting that the experiments described in the monograph had been conducted over a period of three years, that a large group of individuals had taken part in the work, that it had been conducted with the cooperation of all the members of the Duke University psychology department,¹⁵² with 'waxing'

¹⁵¹ For more information on this period see Mauskopf and McVaugh (1980, pp. 74-79) and Rhine (1983, pp. 98-110).

¹⁵² Prince exaggerated the harmony in McDougall's department. In the early years of Rhine's experimentation, 1930 to 1933, some department members were involved in the research. But by the spring of 1934, before *ESP* was published, Rhine's departmental colleagues had become sufficiently alarmed by the appeal of Rhine's research to students that they wrote to McDougall, who was spending six months in England, asking that he curtail Rhine's activities somewhat. Their principle objections were that Rhine was acquiring resources for his group at the cost of the other faculty members, and that incoming graduate students were getting the idea that they had to be pro-psychical research in order to study psychology at Duke. McDougall wrote to Rhine suggesting that he should, indeed, reign in his activities. McDougall also recommended that psychical research not be taught to undergraduates and perhaps not to women students

methodological constraints (rather than waning), and so on. Prince found the findings of process-oriented research conducted by Rhine's team to be an argument against the chance hypothesis, amongst which were the influence on results of the introduction of novelty in the experimental situation, and of the personal health and states of consciousness in which the participants completed their experimental tasks.

Prince finished his introduction by providing a reading guide to the monograph which urged the reader to bookmark the table of definitions of the methodological abbreviations, to read through the sections on sensory cueing, deception and other normal hypotheses, as well as Chapter 7 in which participant Hubert Pearce's overall results appeared, prior to reading the chapters in which Rhine described the historical development of the programme and other specific experimental results.¹⁵³

In Rhine's preface, he began by noting that the research *ESP* summarised had been conducted for three years prior to the writing of the monograph, and after the second year, seemed to be producing results of sufficient quality and strength '... to move some of my more interested friends to urge publication' but that he delayed for another year so as to be sure that the conclusions he was going to propose had been bolstered 'beyond any reasonable doubt' (p. xxvii).

Rhine portrayed the context in which his work was conducted as having benefited from such indicators of a growing popular acceptance of the phenomena of telepathy and clairvoyance as the publication of Upton Sinclair's (1930) *Mental Radio* with its introductions by McDougall and Albert Einstein, and ESP tests conducted by *Scientific American*. The climate led, Rhine felt, to a context in which there was 'much

(Mauskopf & McVaugh, 1980, pp. 135-136). When Rhine discovered that a letter to McDougall from his colleagues had initiated the exchange, he was understandably shaken by the indirect method they had chosen to air their grievances. Rhine became determined to separate his parapsychology laboratory from the department and set it up as an independent unit (e.g., pp. 137-138). Outside funding was secured and by the late 1930s, Rhine's group enjoyed an autonomy on campus that was bolstered by a level of funding equal to just under a tenth of the total research budget of the entire university (pp. 138-139), a fact that could not have increased his popularity in the department. The principle donation amounted to \$25,000.00, which in today's dollars is equal to \$835,000.00, using the unskilled labour calculation. (This calculation is considered to be a more consistent method of comparing income over time than calculations based on the Consumer Price Index. See <http://eh.net/hmit/> for the justification of this calculation.)

¹⁵³ There is an implied criticism of the structure of the monograph in Prince's 'reader's guide' which was echoed in some of the reviews the volume received.

more natural inquiry as a consequence and less of the older blind intolerant credulity — for or against’ (p. xxviii).

Rhine next described what he hoped to accomplish with the monograph:

The work reported here is motivated largely by what may be termed an interest in its philosophical bearing — by what it can teach us of the place of human personality in nature and what the natural capacities are that determine that place. ... but it is a “philosophy for use” that these studies are meant to serve. The need felt for more definite knowledge of our place in nature is no mere academic one. Rather it seems to me the great fundamental question lying so tragically unrecognized behind our declining religious system, our floundering ethical orders and our unguided social philosophies. (p. xxviii)

After this lofty goal was stated, Rhine moved to the particulars:

... that the more general purpose behind this work is to push on with caution and proper systematization into all the other seriously alleged but strange phenomena of the human mind. By proceeding always from already organized territory out into the phenomena on trial, never lowering the standards of caution in the face of the desire to discover or the need to generalize, the field of these unrecognized mental occurrences can and will ultimately be organized and internally systematized to a degree that will simply compel recognition. How long this may require one cannot estimate; but it is the only truly scientific course to take. (p. xxvii-xxix)

Rhine then discussed the term ‘extra-sensory perception’, which he saw as an improvement over other more theory-laden terms in use elsewhere.¹⁵⁴ He ended the preface with a justification of the structure of the volume and a series of acknowledgements for personal and financial support.¹⁵⁵

¹⁵⁴ Rhine felt very strongly about the importance of having terminology that both grew out of, and fed into the experimental work, especially that which was conducted in his own laboratory (e.g., Zingrone & Alvarado, 1987, pp. 51-52, 56-59).

¹⁵⁵ Rhine thanked his colleagues in the Duke University Psychology Department, his own staff, his student assistants and his mentors, and acknowledged the financial assistance provided by both the Psychology Department and by the University Research Fund. The initial grant to conduct research provided by the University Research Fund was \$400.00 which Louisa Rhine (1983) said was allocated for the purpose of psychical research (p. 154). This is equivalent to \$13,800.00 in today’s dollars using the ‘unskilled wage’ calculation mentioned in Footnote 152. Interestingly, Mauskopf and McVaugh (1980) claim that the grant was given to support a survey of orphanages and reform schools (p. 88). It is unclear from their comments

The monograph was divided into three parts. Part I contained two chapters. Chapter 1, 'The Clarification of the Problem' (pp. 3-15) began with the following description: 'the phenomena of this field are not only radical in their aspect of escaping some accepted basic law of our science of nature ... [but they suggest] personal agency in some form' (p. 4). Psychological research, Rhine said, although a 'branch of psychology'¹⁵⁶ appeared to harbour phenomena that violated various 'common physical law[s]' (p. 4). The phenomena were categorised under the headings of 'physical' and 'mental', with luminous phenomena, levitation, physical mediumship and 'psychic healing' (p. 5) classified as 'physical' phenomena. Telepathy and clairvoyance — the phenomena on which the monograph focused — were classified under the heading of 'mental' phenomena. Whether 'physical' or 'mental', however, Rhine felt that 'all the phenomena of the field are "psychical" in some degree' (p. 7). Using this schema, he set up a listing of 5 areas of parapsychology classified according to 'the other fields most involved in the laws seeming [to have been] evaded or transcended' (p. 7)¹⁵⁷ (See Table 5.)

Rhine next factored the perceived agency behind the purported effects into his schema, subdividing phenomena on the 'basis of the state of the personalities supposed to be involved — chiefly as to corporeality' (p. 9).¹⁵⁸ Corporeal agency was further divided into 'simple corporeal agency' and 'inter-corporeal agency', and incorporeal

whether the University knew what type of research the money actually supported. An additional proposal for \$200 submitted in 1931 was turned down (p. 92).

¹⁵⁶ He said 'Like any other branch of Psychology' in the 1934 edition, and changed this phrase to 'Like any other branch of Experimental Psychology' in the 1964 edition. Given his general attitude towards psychology, which will be discussed below, it is at least somewhat surprising that he identified his research so clearly with the discipline, especially in the 1964 edition.

¹⁵⁷ Rhine's classification system was quite complex at this stage and alluded to the psychological literature of mediumship and other phenomena. As Rhine and his team built the discipline of experimental parapsychology over the decades that followed, the data of psychological research, especially that of mediumship, mattered less to resulting classifications. See Alvarado & Zingrone (1984), for a more complete discussion of the substantive and social purposes of Rhine's classification system, and the place of classification in the development of new sciences.

¹⁵⁸ The continued influence of the origins of psychological research, and Rhine's own interest in the study of mediumship is apparent in this further classification, in that by corporeality Rhine meant 'in the body'. Agency that was corporeal was produced by living individuals. Agency that was classified as incorporeal appeared to have originated from individuals who were no longer living, that is, from 'spirits'. One wonders why this classification which, one can assume, would have been highly unacceptable to many psychologists of the time, was never mentioned in the published criticisms of the volume.

agency was divided into ‘intercorporeality through corporeal agency’ (presumably mediated phenomena such as that which was observed in the séance room) and ‘simple incorporeal agency’ (presumably directly-perceived phenomena such as apparitions of the dead). The coupling of this system with the previous one (minus the parapsycho-literary/artistic category) resulted in a sixteen-cell table with cells 1 through 4 representing those parapsychical, parapsycho-physical, parapsycho-physiological and parapsycho-pathological phenomena seemingly to have arisen from a single corporeal agency, cells 5 through 8 representing the same delineation of phenomena but seemingly to have arisen from inter-corporeal agency, and so on.

Table 5.

Rhine’s Preliminary Classification Schema

Parapsychical	Telepathy and clairvoyance, experimental and spontaneous; dowsing; previsionary and monitory dreams or hallucinations; ‘psychometry’, ¹⁵⁹ ‘spirit’ communication, etc.
Parapsycho-physical	Telekinesis, levitation, ‘psychic lights’, temperature changes, ‘apports’, etc.
Parapsycho-physiological	Materialisations, ‘extrusions’, elongations, stigmatisation, extreme body-temperature changes, etc.
Parapsycho-pathological	‘Possession-pathology’; ‘psychic healing’ of organic disease, beyond effect of suggestion
Parapsycho-literary / artistic	Creative writing or other art, clearly ‘impossible’ as result of natural training

Once having set up this schema, Rhine noted that the work of his team was limited solely to cells 1 and 5, that is to parapsychical phenomena which seemed to arise from a single living individual such as clairvoyance or to take place ‘between’ two or more living individuals such as telepathy. Within this segment, ‘Corporeal Parapsychical’, Rhine felt that the important questions were: (1) whether or not it was possible to ‘find persons able to demonstrate the more commonly reported sort of apparent exception to psychological laws — mainly, cognition of events without the usual sensory or rational experience required by our habitual concepts for the knowing act?’; and (2) ‘Is this an actual principle of nature that such extra-sensory cognition can be done by normal individuals, as is so often reported?’ (p. 11). Rhine then noted that the

¹⁵⁹ In experimental parapsychology the term ‘psychometry’ is used to denote a method of obtaining seemingly psychic information about an individual or a place by coming into physical contact with an object belonging to that individual or place.

historical chapter which followed would provide evidence for the existence of the phenomena, so that the rest of the monograph could focus on the confirmation of that evidence in the laboratory and on the further discovery of lawful relationships between methodological and psychological variables and the ability of experimental subjects to produce the phenomena in the test conditions.

In Chapter 2, Rhine reviewed research conducted in Europe, Britain and the United States in the late 19th- and in early 20th-century. After a brief description, he set aside both mediumship and spontaneous case research. He found that the general impression the readers took away from the main collections of spontaneous experiences (e.g., Gurney, Myers & Podmore, 1886; Myers, 1903; Osty, 1923) could be ‘quite impressive in emphasizing the frequency and generality of distribution of such occurrences among the population’ (p. 17) and, when coupled with mediumship reports and other surveys and studies, could be very convincing to some. But, Rhine noted, ‘There are, however, those more skeptical minds that demand some measure of experimental manipulation and even some artificial control of the phenomena in question before they venture credence’ (p. 20).

Rhine then briefly described experiments conducted in Europe, by members of the Society for Psychical Research and the early American Society for Psychical Research, and by his predecessors in universities, especially John E. Coover of Stanford University, and G. H. Estabrooks of Harvard. Rhine concluded:

... the evidence is (to one who labors through it all) overwhelmingly convincing of some extra-sensory mode of perception. That this includes the perception of mental states of a wide range of variety is also clear. That the hypnotic trance is not necessary, but is a possible “telepathic” working condition, seems also proved. ... [that] loss of ability with lapse of time are recorded ... suggestion is made that certain drugs may help ... the agent’s greater tendency to fatigue and headache is referred to ... [and that there is a] general need for passivity and serenity on the part of the percipient. (p. 31)

Rhine noted that few attempts were made to differentiate ‘true’ telepathy from ‘true’ clairvoyance.¹⁶⁰ After criticising theories proposed prior to the appearance of his group,¹⁶¹ he concluded:

For a summary of the chapter, one may say that the evidence for general E.S.P. is good but the theories are bad; and our knowledge of the phenomena needs refinement through variation and improvement of conditions. We need tests for pure telepathy and more of them for pure clairvoyance, made under conditions that enable easy evaluation of significance, provide safe exclusion of other modes of cognition, and introduce variation enough to suggest the relation of E. S. P. to other processes ... (p. 39)

In the appendix to Chapter 2, Rhine described various computational systems introduced to evaluate the results of specific experiments in the 19th-century, beginning with Richet’s introduction of probability in his article on telepathy.¹⁶² Rhine presented a method of obtaining the probable error as a way of evaluating deviation from chance in his experiments. He ended the appendix with a brief letter from the well-known statistician R. A. Fisher endorsing his calculations.

In Part II, Rhine began with an overall survey of the experiments conducted at Duke. Rhine described his search for human subjects which included testing groups of children at a summer camp, and how he later developed, with a Duke colleague, ESP tests using sealed envelopes. After discussing the results of these tests, he referred the reader to a later chapter for more detail. Rhine then provided a kind of personal history, describing the development of various methodologies, the discovery of several ‘good subjects’, the testing of friends by his graduate students, the testing of his family members, and more formal tests with Duke students in which attempts were made to alter their state of mind or to alter the physical parameters of the experiments themselves. Organising the chapter as an ‘autobiography’ of the laboratory, Rhine

¹⁶⁰ This is something of a distortion of the interest in the two types of phenomena. See Mauskopf and McVaugh (1980, pp. 29-36) for a discussion of the differentiation of telepathy from clairvoyance.

¹⁶¹ These were the theories proposed by Crookes (1897) and Tischner (1925), amongst others.

¹⁶² Rhine used Gurney’s (1884) review of Richet’s experiment (pp. 239-256), the original report of which was published in French.

introduced the problems, shift in interests, and the cast of characters. In some instances, he referred the reader to later chapters for more information.

In Chapter 4 Rhine described studies he characterised as ‘early and minor’ (p. 62). In the style of the preceding chapter, studies were described in the order in which they were done, accompanied by a minimum of methodological detail and tables of results.¹⁶³ Results presented in this chapter and later chapters were organised ‘around the personalities of the major individual subjects’, a technique Rhine justified because ‘so much irregularity of conditions, procedure, and results is inevitable because of the great factor of human variability’ which, in turn, made it ‘hard to generalize over the whole range of subjects in a detailed fashion’ (p. 62). Chapter 4 also included the development of what would become the standard Rhinean card-guessing technique. He said: ‘[I]n work of this kind it is necessary to proceed as explorers, ready to adjust plans at every turn, flexible as to methods and conditions. Only the general objectives need be kept fixed, and the means and criteria of interpretation’ (p. 63). Before the ESP cards were settled on, the early tests included number guessing, raising one’s hand at a signal as a guess as to which hand the experimenter would raise, and guessing letters of the alphabet.

By the winter of 1931, the tests had standardised around the ESP cards, but results were reported (in Table III on page 69) by providing data on the individual high scoring subjects alone. More formal experiments conducted by graduate students, and informal experiments conducted ‘from time to time’ by family members, were also summarised. In addition, the miscellaneous experiments of 1932 and 1933 were included so as to provide the reader with the assurance that all the data was being reported. Even as Rhine claimed this, however, he described a procedure that contradicted this statement:

¹⁶³ In several instances, especially in the description of the first envelope clairvoyance tests, a great deal of emphasis is placed on the description of how the sealed envelopes were constructed. Whilst this kind of detail was helpful to readers in terms of providing an understanding of the difficulties students would have if they wished to cheat on the tests, other procedural aspects were not as carefully described. This was so even though Rhine claimed, at the beginning of the chapter, that ‘all the available information that seems helpful to thorough understanding will be included’ (p. 62).

... we have followed the policy of giving a new subject a preliminary test, the results not to be taken into the record no matter what they are. When the subjects gets 3 hits in 10 or better, the record can be started on the next trial following but must be so designated at the time. If, during the performance for record, the score drops below 6 in 25, it is legitimate to quit scoring for the time. These preliminary test data have been rejected. My estimate of them, from memory and my own experience, is that they were on the whole above chance average anyhow, and probably represent only a few hundred trials with those subjects who later came into good scoring. But there have been a few subjects who have "practiced" for thousands of trials without getting above the chance expectation (*np*). No conclusion of this report would be changed or appreciably weakened by including these practice data. For that matter, no amount of failing to score above chance by any number of other individuals can seriously affect our judgment of the results of those who succeed, since an individual ability is in question. (pp. 76-77)

Rhine ended the chapter by noting 'I have finally a number of scraps of data for record that do not fit in anywhere. Some of them are very good and some are poor. I cannot be sure, of course, that to-morrow or next year I shall not find a sheet of data stuck away absent-mindedly in a book I was reading or holding at the time', but Rhine stressed 'I am fully confident that there is no batch of forgotten and unreported data that would alter the final "anti-chance" value (*D/pe*) by so much as half a unit' (p. 77).

Following this chapter are four chapters which focused on specific individuals and their results: undergraduate student A. J. Linzmayer; Charles Stuart who was already then a staff member of the laboratory; Divinity School student Hubert Pearce; and in the fourth chapter, undergraduate students May Frances Turner, June Bailey, T. Coleman Cooper, Sara Ownbey, and George Zirkle. The structure of each chapter mirrored the others in that Rhine first described the temperament of the individuals on whom the chapter focused, then noted whether they reported having had psychic experiences of their own, and whether their immediate family members had reported experiences. All of these 'star subjects' were described as intelligent, capable Duke University students. If they reported personal psychic experiences, they were presented as not inordinately focused on them. Each was described as having either interest in, or talent for, some

form of artistic endeavour (usually music), and as out-going and more interested ‘in people than in things or causes’ (p. 115).¹⁶⁴

After this, tests were described in chronological order and tables of results given which also mentioned variations of conditions, such as whether the tests were of ‘pure’ telepathy or clairvoyance, or ‘undifferentiated’ ESP, whether the cards were screened or not, what method of guessing was used, whether the cards were observed or handled by the subject or not, whether the subject was tired or ill, whether the experiment was conducted with the subject under the influence of caffeine, alcohol, sodium amytal (Rhine administered this drug to both Linzmayer and Pearce), or hypnosis, whether tests were given in the laboratory with observers present, or were administered off campus such as the series Rhine conducted with Linzmayer in Rhine’s car, or whether distance was introduced as a variable between the subject and the agent if the test was one of telepathy or general ESP.¹⁶⁵ The final statement for significance in each chapter rested on pooling the results across all conditions for each individual. Interspersed with the brief descriptions of the experiments and their varying of conditions were Rhine’s interpretations of each subject’s scoring behaviour, his personal assessment of the reliability of the testing and of the security procedures used, as well as descriptions of various circumstances in the subjects’ lives which Rhine felt might have effected results in a systematic way.

The final part of the monograph provided an over-arching discussion of the findings and their meaning. In the first of these chapters, Rhine dealt with five alternate

¹⁶⁴ It is interesting that some of the attributes of Rhine’s star subjects fit well with modern experimental results which suggest that participants who have a family history of psychic experiences, claim their own experiences, and are extraverted and artistic can be expected to do well in ESP tests. Without any objective measures of these personality traits or states or the details of the personal experiences claimed by his high-scoring subjects, and without knowing what the characteristics were of the rejected subjects in Rhine’s period of testing, it is impossible to tell if this apparent goodness-of-fit to modern findings is coincidental or not.

¹⁶⁵ This is not to say that the tables were structured similarly over the chapters in the sense that the same variables were noted, if relevant, from experiment to experiment. Rather each table had its own focus, and whilst some were comparable to tables constructed for other subjects, most were one-off presentations of variables important, in Rhine’s mind, to the particular subject on whom the chapter focused. The difficulty in reconstructing the data into a coherent form was something that reviewers and later critics commented on. The style of construction was also incorporated, albeit to a lesser extent, in *Extrasensory Perception after Sixty Years*, inspiring some readers to complain again.

hypotheses for his results: chance (pp. 145-147), fraud (pp. 147-149), incompetence (pp. 149-150), unconscious sensory perception (pp. 150-153) and rational inference (pp. 153-155). Rhine described the chance hypothesis as follows:

According to the Chance Hypothesis, we should be as likely to go below chance average, if we ran 90,000 more trials as we should be to go above. All the positive deviation we have accumulated has just been one grand, persistent accident, stretching through three years of varied conditions and over a wide range of subjects[?]. ... What, then, can one say to this? (pp. 145-146)

Rhine gave three answers: firstly, that the statistical evaluation of the results had adequately ruled out the chance hypothesis; secondly, that cross-checking (cutting new decks of cards and matching their order against calls made by high scoring subjects in other tests) produced chance scores; and thirdly, that a large number of witnessed and unwitnessed trials in which runs of hits of 19 or more in succession were obtained argued against chance operating, because of the astronomical odds against such long successful strings being produced by chance alone.¹⁶⁶

Rhine described the second alternative hypothesis, the fraud hypothesis, as asking ‘Are we dealing with real facts of actual occurrence or are they fictitious?’ (p. 147). After rejecting ‘long hours’ over ‘many years’ (p. 147) as an argument against a single individual’s potential act of fraud,¹⁶⁷ Rhine argued that it was highly implausible that so many experimenters, witnesses and subjects could have colluded together over such a long period in tests with such varying conditions to produce results which, when aggregated, seemed to tell a coherent psychological picture. Further, Rhine felt that results indicative of ESP obtained in distance series and in series done when cards were

¹⁶⁶ This argument is only good for the witnessed trials, and even then it is impossible at this remove to determine whether or not the witnessed trials with this magnitude of success were also trials in which sensory cueing or fraud were irrefutably ruled out by test conditions.

¹⁶⁷ Essentially this argument was that no one would be willing to spend long hours over many years just for the purpose of perpetrating fraud, which Rhine dismissed, given, for example, (from his perspective) the lengths to which fraudulent mediums regularly went to perpetrate fraud. See the Rhines’ (Rhine & Rhine, 1927) report on their sittings with “Margery” (Mina Crandon) in which they felt they had witnessed blatant fraud.

shielded or screened argued against fraud not only in those extraordinary series, but in other less well-controlled experiments as well.¹⁶⁸

The third alternative hypothesis, ‘incompetence’, was based on the premise that investigators had made systematic errors in the test preparation, data collection and data recording. Rhine felt that whilst some data errors may have occurred, precautions had been taken to mitigate against these, and that, in any case, what he perceived as the inherent lawfulness of the results over the entire period of testing argued against both incompetence and an overall explanatory principle.¹⁶⁹

The fourth alternative hypothesis, ‘unconscious sensory perception’ included the possibility that sensory cues were available for conscious use.¹⁷⁰ To counter this as an overarching explanatory principle, Rhine argued that at least some data had been gathered in test conditions in which sensory cues were not available to the subject (when cards were screened, or covered in opaque envelopes, or were ‘new’, that is when the seal on the packet was unbroken at the beginning of the experiment, and the deck, once liberated from the packet, was used unshuffled, thus making it unlikely, Rhine thought, that the subject could know the order of the cards), or when the cards themselves were distant from the subject, even to the point of being in another room. Further, the pattern

¹⁶⁸ These points would not, of course, have argued against fraud committed *by* Rhine at some other stage of the experiments or in the presentation of the data but charges of fraud were seldom raised overtly against Rhine. One notable exception was behaviourist B. F. Skinner (1937), in his review of Rhine’s (1937) popular book, *New Frontiers of the Mind*. Skinner found sensory cues in the backs of the ESP cards included by the publisher with the book he received for review, and inferred, wrongly, that the commercial cards were indicative of the quality of the experimental cards. Not only were the experimental cards generally constructed more carefully but in many experiments cards were sealed into envelopes or, as Rhine noted in his monograph, hidden from the subjects’ view by screens and other devices. Hansel (1960c, 1966, 1989) and Price (1955) also raised the spectre of fraud but focused on the possibility that other experimenters in the laboratory had perpetrated it rather than Rhine himself.

¹⁶⁹ Willoughby (1935a) speculated that the ‘lawfulness’ Rhine claimed to have uncovered could have as easily been lawfulness connected to a subject’s ability to detect and utilise sensory cues to obtain significant hit rates, whether or not such an ability was used consciously or unconsciously.

¹⁷⁰ By splitting out ‘unconscious sensory perception’ from ‘fraud’ *per se*, Rhine seemed to be drawing a distinction between fraud committed by the entire cast of characters involved in the research from 1931 to 1933 as well as premeditated subject fraud (controlled by witnessing and other controls), and individual fraud that was, perhaps, accomplished in an unpremeditated and opportunistic way when it became apparent that sensory cues were available.

of the results obtained in the conditions which Rhine felt precluded sensory cueing were consistent with results obtained under other conditions.

The final section dealt with the notion that the subject could produce scores indicative of ESP by using rational inference to guide their guessing behaviour. Rhine saw this possibility as only available to the subject in the pure telepathy experiment when a long series of guesses would be made and then recounted for analysis. He did not believe that the normal testing situation allowed the subject to make such inferences, but he did not address the question of trial-by-trial versus end-of-run feedback methods, a topic which would figure in the controversy that followed the publication of the monograph. Nor did he discuss the composition of the target decks or whether or not the order of target cards coupled with certain types of feedback would have provided an opportunity for producing artifactual hitting through rational inference.

Having eliminated the counter-explanations in his own mind, Rhine felt sanguine about claiming that 'For those, then, who can accept proof before explanation is arrived at (i.e., for the scientifically mature) ESP is a natural fact and principle, puzzling as its explanation may be.' Rhine then speculated on its 'nature and functioning' (p. 155).¹⁷¹

The next five chapters dealt with the interface of ESP research with various other disciplines. Rhine dealt with the 'physical conditions of ESP' (p. 156). By physical, Rhine meant 'demonstrably energetic', that is, capable of 'doing work' in the physics sense of the word (p. 157). Rhine felt that a number of findings obviated against a physical explanation for the operation of ESP, amongst them the exclusion of visual cues in experiments in which screens were used.

Next Rhine dealt with the 'radiation theory' because he claimed that the only energy that might be capable of providing the information necessary for the subject to correctly call the cards would be a 'radiation of extremely short and very penetrative waves' (p. 158), and that such radiation would need to be present in an inert deck of

¹⁷¹ Although critics did not comment on Rhine's propensity to label those who did not accept his conclusions as 'scientifically *immature*' through indirect evaluative remarks such as these, such characterisations of his opponents may well have impacted negatively on the 'hearability' of his experimental results.

cards, perhaps in the ink. But, Rhine argued, because no discernible patterns in the results had ever been found which related to the age of, or to the amount of use a deck of ESP cards had received, one would be forced to assume that whatever ‘radiation’ the ink on the cards might emit did not alter with handling or with the passage of time. This seemed unreasonable to Rhine. In addition, distance tests added the requirement that whatever the cards emitted must be perceivable over long distances, and also distinguishable from other likely target material in closer proximity to the subject. On these grounds, Rhine said, ‘I can see no hope for a radiation theory of E.S.P.’ (p. 163).

In the next chapter physiological conditions were discussed. Whilst Rhine felt there was no evidence for a receptor organ in the body of the subject which was responsible for the acquisition of ESP information, he thought it was premature to rule out the possibility that one existed. One thing that he felt might argue against the presence of such a specific receptor was that Stuart and Linzmayer both preferred different types of physical orientations to the target deck of cards, one preferring to sit with his back to the cards, and the other facing them (p. 169). However, Rhine felt, there was evidence that specific brain states might interfere with, or enhance ESP scoring. Not only did some drugs, sleepiness and illness seem to dampen the success of Rhine’s star subjects, but other drugs, such as caffeine, seemed to enhance their performance. Further, his high-scoring subjects seemed to have similar personality traits such as creativity. Another possible piece of evidence for the interaction of ESP with brain states was the finding in the spontaneous literature that ESP information tended to come to consciousness in dreams or trance states, and that certainly, the best mediums of Rhine’s day operated solely in trance (p. 172). But, Rhine concluded that:

First, the E.S.P. experience seems rather to be that of a more complex level, one that is readily broken up by sodium amytal and fatigue while the senses are still functioning. Second, the experience of the percipient is one of cognition or “knowing”, not a “sensing” in the strict psychological meaning of the word. That is, he knows but cannot tell “how he knows” ... Third, there is no consciousness of localization of the basis of the cognition, as is possible in sensory perception. Fourth, ... there seems to be no special orientation required for success. Fifth, ... there is the further basic difference also that the known energy forms seemed inadequate as a physical basis. (p. 174).

In Chapter 12, Rhine reviewed psychological correlates of ESP scoring. Using his star subjects he illustrated the point that some subjects felt it was necessary to go into 'a state of detachment, abstraction' (p. 175) to make a correct call. The fact that the arrival of visitors mid-test was normally followed by a depression in scoring and a subsequent recovery argued for the need for detachment for 'hitting' to occur. That is, the disturbance caused by a new person in the room could, Rhine thought, bring the subject back from that point of 'abstraction', thus breaking whatever state of consciousness into which they had fallen whilst successfully calling the cards (p. 176).

Attention to the task also seemed to be necessary to produce a successful result, and anything that interfered with the ability of subjects to concentrate — such as illness, sleepiness, or the influence of drugs — also interfered with their ESP ability (p. 179-181). Patterns in hitting and missing which became typical for individual subjects might be interpreted, Rhine thought, as evidence for this characteristic waxing and waning of attention (pp. 182-189). Equally, Rhine felt his team had observed the influence of psychological states on scoring, that is, that scoring might fall when subjects felt self-doubt, or when they disliked or were sceptical of the conditions of the test (pp. 190-191). In the final sections of the chapter, Rhine speculated on whether or not it could be said that his experiments provided evidence that mind was somehow independent of the 'material world' (pp. 192-196).

In the chapter on 'E.S.P. and General Parapsychology', Rhine reiterated the point that his research had sought to delineate clairvoyance from telepathy. Instead, he felt he had proved that the phenomena were fundamentally similar to one another given that similar psychological states effected them in similar ways. That is, clairvoyance and telepathy were one process for Rhine and the distinctions that were drawn between them were merely experimental operationalisms, differences in the methodology used to elicit results.

In the chapter on 'biological considerations', Rhine speculated that ESP ability was inheritable, based primarily on the fact that all of the star subjects and five of his seven other high-scoring subjects could name members of their immediate family who had shown evidence of ESP ability in their lives. Further, in each of these cases, if more than one family member was named, these 'psychic' individuals were 'blood relations'

to one another (p. 207). Rhine concluded the chapter with speculations on what the evolutionary status of ESP might be. He argued from the scoring decline curves of his higher scoring subjects and from the fact that so few individuals seemed to possess the ability, that ESP's apparent potential for usefulness in an evolutionary sense was limited.

In the final chapter, Rhine collated the results of all the trials that had been presented in the monograph, providing a table in which these results were pooled and evaluated statistically. After restating many of the specific findings he had described, he offered the following hypotheses: (1) that high scoring in the laboratory was related to psychic experiences in life; (2) that the results supported a 'non-physical nature of mind'; (3) that success in ESP tests suggested that ESP involved "conation" rather than cognition'; (4) that ESP was 'energetic' even though no known energy had been identified; (5) that there was no specific receptor for ESP information; (6) that ESP might be interfering in conventional psychological experiments and in clinical encounters; (7) that the ability was probably innate rather than learned; (8) that there was probably a 'species level' of ESP that even high-scoring subjects could not exceed; and (9) that because ESP-like experiences seemed to figure in religion and mysticism, religious adepts might be found to be high in ESP ability (pp. 222-223).

The volume ended with two appendices, one a list of hints for successful experimentation and the other introducing a table of significances which new experimenters might use to evaluate their data.

On Rhine's Rhetoric in *ESP*

Establishing Credibility

Rhine expected some controversy to surround the publication of his book. He was well aware that the work he was doing was unprecedented in the American academy:

It is to be expected, I suppose, that these experiments will meet with a considerable measure of incredulity and, perhaps, even hostility from those who presume to know, without experiment, that such things as

they indicate simply cannot be! (p. xxxvii)

The potentially controversial nature of the phenomena and its findings were emphasised elsewhere in the volume as well. For example:

... But, in outlining the field in which we are finding our problem, we are regarding it very tentatively. Since many claims in that field do not at present warrant great confidence, we are giving a minimum of credence at every point and are proceeding with extreme caution. (p. 3)

... Curiously enough, however, the facts seem to require proof over and over — many, many times. ... This will, I predict, be one of the more amazing facts for the future historian of science. And after reading Bruck and Warcollier and Coover and Estabrooks and Sinclair, as well as the more numerous and varied series that preceded, still the students who would work in the field to-day must set out first to prove it all over again! Scientific method and systematic observation have meant so little that we dare not lean on them heavily unless we are already prepared, by *a priori* mental attitude, to accept their findings. (p. 25)

Such an expectation was reasonable given the history of criticism of psychical research up to that point.

The initial response to *ESP* was positive, especially in the popular press.¹⁷² The response from psychology departments was relatively low-key from 1934 to 1936. Mauskopf and McVaugh (1980) noted that, initially at least, ‘a sizeable number of psychologists began to try to replicate his findings’ and ‘[i]n spite of Rhine’s own initial tendency to keep his distance from the psychological community, many psychologists seem to have come to feel a certain tolerance for parapsychology’ (p. 241). Whilst Rhine

¹⁷² Positive reviews appeared in the *New York Times*, the *Herald Tribune* and elsewhere. Mauskopf and McVaugh (1980, pp. 153-154) noted that Waldemar Kaempffert, the science editor at the *New York Times* chose to review the book as if nothing sensational was included in it and as if the results of Rhine’s tests had proved ESP in a sober manner under the auspices of a serious department of psychology, an example of science and — and in a characterisation that would matter more a year or so later — of psychology ‘as usual’ (Kaempffert, 1940a). Kaempffert continued to include news items on parapsychology in his ‘This Week in Science’ and ‘Science in the News’ columns throughout his tenure as editor (e.g., Kaempffert, 1934, 1937b, 1939a, 1939b, 1940b, 1941a, 1941b). The column routinely reported on interesting findings in the natural and social sciences, on the development of new technologies and the installation of new scientific equipment in university laboratories of science. Including parapsychology routinely in such a publication may have helped to sustain the fury of those who believed that parapsychology was a pseudoscience as well as those who would have liked to have seen more mention of advances in mainstream psychology instead.

had claimed that parapsychology was a ‘branch of psychology’ (1934a, p. 4), he was also later to claim that he wrote the book for psychical researchers and not for psychologists. His ambivalence towards what could be considered his own community was especially apparent in the summary article he wrote for the *Journal of Abnormal and Social Psychology* (Rhine, 1934b). Once psychologists were forced by the public uproar over the publication of his popular book (Rhine, 1937) to respond to Rhine’s claims, the ambivalence of his relationship to psychology proper may have contributed to the heat of the controversy that followed.

Then, as now, a psychologist reading a monograph on any ‘branch of psychology’ would have expected to find indications in the text that the author identified with the discipline and its preoccupations, that acceptable science practise was apparent in the descriptions provided, that the author was conservative and tentative in his conclusions, and further, that the work existed within what Gross (1996) has called ‘a network of authority’. That is, the document needed to show that ‘behind that publication [was] a series of grants given to scientists connected with a well-respected research institution, [and that there were] within the text, a trail of citations highlighting the [work] ... as the latest result of a vital and on-going research program. Without this authoritative scaffolding, the innovative core of [the work] ... would be devoid of significance’ (p. 13).

McDougall’s foreword used the rhetorical device of *ethos* (Bazerman, 1988, p. 141), that is, he attempted to endow Rhine with credibility as well as to locate Rhine’s research within such a ‘network of authority’ by declaration and description alone. Specifically, McDougall’s text situated Rhine squarely in the newly-formed Department of Psychology at Duke University, having come there by a prestigious route, that is, by way of two highly-regarded institutions, the University of Chicago and Harvard University:

... Both have taken their doctorates at the University of Chicago, both had begun promising careers as university teachers of biology, and both have resigned these ... The Rhines, in pondering the question — What is most worth doing? To what cause can we give ourselves? — had come upon my *Body and Mind* and upon others of my writings, especially my plea for *Psychical Research as a University Study*, and

had determined to join me at Harvard ... the Rhines spent the year at Harvard studying psychology and philosophy ... And in the autumn of 1927 they turned up at Duke University, as determined as ever ... (McDougall, 1934, p. xv)

The details were correct, of course, but no mention of the other academic facts of their lives were made; not that they had published scientific articles in plant physiology,¹⁷³ nor that J. B. Rhine's 'promising career' was in fact a faculty position in the intellectual 'backwater' that was the University of West Virginia in Morgantown. Neither of these facts could have added the *gravitas* that mentions of the University of Chicago and Harvard University brought to the narrative, and, in fact, might have detracted from McDougall's depiction.

Walter Franklin Prince, in his introduction, also situated the research in a prestigious locale:

... we find the co-operation, observation and critical judgement of many persons both within and without the teaching staff of the psychological department of Duke University ... (Prince, 1934, p. xix)

Rhine, himself, followed through by placing his work well within its authoritative context, in the acknowledgements at the end of his own preface:

... I wish to give the strongest utterance to an expression of gratitude that these experiments have been permitted in a Psychological Laboratory of an American University. I am doubtful if there is any other Psychological Department on this side of the Atlantic or even, perhaps, in the world, where they would even have been permitted, much less encouraged and supported ... (Rhine, 1934, p. xxx-xxxii)

Whilst McDougall's and Prince's depiction situated Rhine and his work unproblematically in places of prestige, Rhine, himself, raised the spectre that something about his enterprise would have been unacceptable in other similarly-prestigious institutions, undermining the impression that here was, as Kaempffert underscored in his *New York Times* review, 'science as usual'. But Rhine also depicted his work as having been supported financially by conventional university sources, which could reasonably

¹⁷³ These were Rhine, J. B., 1924, and 1926a-b, and Rhine, L. E., 1924.

be expected to indicate to the reader that the research plan had been ‘endorsed’ by the granting of university funds:

... The financial assistance given me from the Department Budget and the University Research Fund is also gratefully acknowledged. ...
(Rhine, 1934, p. xxxi)

Whether or not grants were received for the purpose of conducting psychical research, Duke University *did* pay Rhine’s salary and the salaries of his colleagues, and departmental resources *were* involved in the conducting of his experiments. So it appeared that Rhine’s work enjoyed both the moral and financial blessings of Duke University, and therefore, by association, the impression may have been created that the monograph itself both warranted conventional approval and had earned conventional prestige.

If citation use in scientific writing establishes a sense of community (Montgomery, 1996, p. 39), then what Gross (1996) called ‘the trail of citations’ (p. 13) in *ESP*, provided evidence that Rhine’s work was more properly situated in psychical research than in psychology. Of the 87 citations made in the volume, 82 were to articles or books that dealt with aspects of psychical research, a few of which had been published in the general academic or psychological literature. Three citations were references to statistical textbooks. One citation referred to McDougall’s (1926) *Outline of Abnormal Psychology*, the only strictly psychological citation in the volume, and one additional psychology-related citation referred to Carl Murchison’s (1930) compilation in which Pierre Janet’s autobiography appeared (p. 125).

Clearly Rhine’s own work was built almost entirely on the psychical research tradition and the absence of a textual connection to psychology was noted by some of his reviewers (e.g., Willoughby, 1935a). Even at this remove, it is not difficult to think of literatures extant at the time that would have been relevant to Rhine’s work. For example, psychology journals and books of the time contained: discussions of the use of the probable error to evaluate performance (e.g., Edgerton & Patterson, 1926; Huntington, 1927; Odell, 1926; Yasukawa, 1927); examinations of the acquisition and loss of learned skills (e.g., Drury, 1930), potentially relevant to understanding the decline in ESP scoring over time; as well as experimentation on the influence of drugs

(e.g., Cattell, 1930) and the impact of being observed (e.g., Burri, 1931) on task performance in experiments, amongst others. Rhine, however, included no citations to the existing psychological literature save the two mentioned above.

Connections to psychology proper rested only on: (1) Rhine's declaration that his brand of psychical research was a 'branch of psychology' (p. 3); (2) the fact that he conducted his research within a psychology department and that his subjects and assistants were drawn from the faculty, students, and staff of that psychology department; (3) the citation of McDougall's work on abnormal states of consciousness; and finally, (4) Rhine's attempt to characterise the personalities and life experiences of his subjects in a general way so as to speculate on which of these loosely-determined psychological variables might have effected ESP card test performance.

The lack of intertextuality with psychology in *ESP* may have prompted such negative reactions to the more speculative chapters as Willoughby's (1935a) comment that, until the research itself could be considered credible '... we shall not regard the concoction of hypotheses of the mechanism of ESP as a profitable investment of energy' (p. 207).

Credibility and Style

How important are stylistic choices to the credibility of a scientific document? For psychical researchers who already had a positive attitude towards the underlying phenomena, Rhine's literature survey of previous experimentation in psychical research was probably more important to the establishment of credibility and authority amongst that audience than the style in which his results were presented. Whilst Dingwall (Anonymous, 1934) and Thouless (1935) were more interested in specifics, and other critics drawn from the psychical research community would surface as the controversy over Rhine's work unfolded, in general *ESP* caused a great deal of positive excitement in Great Britain and on the continent amongst the 'convinced'.¹⁷⁴

¹⁷⁴ Mauskopf and McVaugh (1980, pp. 103-132) reviewed the correspondence Rhine received from European and British colleagues. In general, *ESP* not only inspired replication, but also methodological innovation in Britain and on the Continent. G. N. M. Tyrrell's (1938) efforts to devise an easy-to-use and easy-to-analyse apparatus to test clairvoyance and telepathy was an example of this (pp. 174-175).

On the other hand, amongst the psychologists who had begun to attempt replications, Rhine's general inattention to specific methodological description undermined further his attempts to establish authority and credibility, especially amongst 'those inclined to be sceptical' (Thouless, 1935, p. 37). Similarly, neither McDougall's nor Prince's evocation of *ethos* would have carried much weight with the psychological community because McDougall, whilst prominent, was a well-known opponent of the behaviourism and neo-behaviourism then sweeping over psychology (e.g., Bazerman, 1988, p. 268; O'Donnell, 1985; Robinson, 1986, pp. 361-367, 404-413, 445-452).

There were other problems with the rhetorical style of *ESP* that may have undermined its credibility and authority as a scientific document. A number of rhetoricians of science have outlined, some impressionistically (e.g., Bazerman, 1988; Gross, 1996; Montgomery, 1996) and others empirically (e.g., Gross, Harmon & Reidy, 2002), the changes in the conventions of scientific writing from the 17th century to the present day. These authors found a progression from the personal and subjective to the abstract and 'objective' in science writing. For example, the typical 17th-century report, in which the identity of witnesses was as important as the description of the experiments' methodological detail, gradually evolved into the typical 20th-century report in which scientists strove to establish the 'presence' (Perelman & Olbrechts-Tyteca, 1971, p. 116-117, 142, 191) of nature as 'the only real agent ... a reality independent of its linguistic formation' (Gross, 1996, p. 17).

Achieving this 'abstraction' has come through a variety of structural and stylistic changes. One of the most important of the stylistic changes has been the evocation of a kind of 'death of self' (Montgomery, 1996, p. 21) in which the scientist strives to be a blank space upon which nature writes its facts. Most research, rhetoricians have noted, is itself highly personal, a first-hand activity that the scientist shapes and experiences. But when research is written up, there is a sense that credibility can not be evoked unless there has been a 'banishment of one's personal experience'; unless, in the narrative, the narrator — the scientist-as-person — 'is lost' (p. 31).

This is not to say that hyperbole and personal bravado are absent from 20th-century science writing. James Watson's and Frances Crick's accounts of the discovery

of the double helix, even in the originating scientific articles, were very much narratives of scientist-as-person (Gross, 1996, pp. 54-65) as was John B. Watson's monograph (1913) and article (Watson & Raynor, 1920) on behaviourism (Bazerman, 1988, p. 269-270).¹⁷⁵ But 'the literary nullification of the self' was well underway by the end of the 18th century and well-established by the beginning of the 20th century (Montgomery, 1996, p. 106). As such it was a primary ingredient in the establishment of the scientist as a humble observer of nature, as someone who held a 'special relationship to an objectified truth' (p. 14).¹⁷⁶

There were a variety of ways in which scientists could signal their personal distance from nature in their prose. Gross and his colleagues (Gross, Harmon & Reidy, 2002) noted a number of these, amongst them: (1) the avoidance of the use of personal pronouns or names; (2) the avoidance of the use of poetic or evaluative expressions; (3) the inclusion of such 'hedge' phrases as 'it seems to' or 'it appears to be'; and (4) the use of passive voice in verb construction (p. 215). By adopting these conventions the activity that is conveyed in the report becomes the activity of the phenomena itself, the agency of nature, and not of the scientist-observer.

Gross and his colleagues developed these and other markers of change in science conventions over a decade-long study of the evolution of scientific report writing in three languages. Expressions of interest were counted in randomly chosen, pre-specified segments of the documents at hand, and then compared across quarter-centuries from the 17th century to 1995. They found, over the centuries, a continuous drop in the use of evaluative expressions and personal pronouns, with personal pronouns stabilising at modern levels during the period of 1901 to 1925, and evaluative expressions dropping to modern levels in the period 1926 to 1950 (p. 166), that is, that

¹⁷⁵ Bazerman has argued that Watson's writing was extremely polemical, written as a kind of 'short story' in which the scientist and the subjects became characters in the narrative, with the scientist as the 'reasoner', the 'persuader' and, ultimately, especially in Watson & Raynor (1920), with the subject as 'victim'.

¹⁷⁶ Sociologists of science, Gilbert and Mulkay (1984) also make this point, when they commented that 'style ... tends to make the author's personal involvement less visible' (p. 47).

each of these elements were used in the modern period less often than once per 100 words on average.

They concluded that from the 17th century forward, scientific language had ‘evolved’ to distance the scientist from ‘nature’ in the narrative through a kind of ‘natural selection’ in which such paring away of the personal from the report seemed to increase both its social credibility and the efficiency with which the scientific content was conveyed (p. 167).¹⁷⁷ Scientists who returned to a personalised 18th-/19th-century style of writing have suffered the consequences, Gross and his colleagues claimed:

[The] infusion of personal, descriptive style [has come to be seen as] ... “bad” scientific prose, or in less pejorative terms, “science on holiday”, a style which is not only unpersuasive but which does not communicat[e] ... science effectively. (p. 167)

As Gross (1996) noted elsewhere: ‘[S]cientific prose generally [excludes] ... any device that shifts the reader’s attention from the world that language creates to language itself as a resource for creating worlds’ (p. 43). That is, nature and the knowledge a scientist can glean from nature must be privileged in the narrative, not the narrative itself, and not the scientist who authored the narrative.

Rather than conforming to what would have been, even in 1934, modern norms in science writing, Rhine’s text in *ESP* is what Montgomery (1996) would call ‘fervid’ or ‘sermonising’ (p. 108), a style more typical of the 18th and 19th centuries than the 20th, shot through with names and personal references and active, rather than passive, verb constructions. For example:

... We seldom ran over 20 trials per day per subject. Mr. McLarty did; as did also Mr. Mann ... among this group were 100 trials by Dr. William McDougall ... 150 by D. K. Adams ... our greatest gain was

¹⁷⁷ Gross and his colleagues proposed a kind of ‘survival of the fittest’ to account for the changes in style. That is, articles that became more impersonal, more passive, adopted the distanced relationship of the scientist from nature in style and structure, were also more influential scientifically, were cited more often, and were otherwise more ‘successful’ in establishing the ‘factness’ of the scientific content being conveyed. As this occurred, more and more scientists adopted the style and structure of the ‘successful’ articles, until conventions were established and communicated through various style manuals and style guidelines of academic journals. The evolution was then, like natural selection, a process through which the style that ‘worked’, survived.

the discovery of Cooper, who got 38 correct in 90 trials ... I must note that in these trials I did not myself supervise Cooper but asked another student, a friend of his, Mr Harriman, to do it. Mr. Harriman, himself, got only 1 correct in 10, with the reverse arrangement. But if there were any doubt of Cooper's and Harriman's honesty, the further work of Cooper under supervision, reported later in this chapter, would adequately satisfy it. ... (Rhine, 1934, p. 70)

If the text of *ESP* is examined empirically with the methods used by Gross and his colleagues, the deviation of Rhine's prose from the norm is even more pronounced. For example, in the case of personal pronouns and names, as mentioned above, Gross and his colleagues found that modern scientists used personal pronouns and names an average of once per every hundred words during the period 1926 to 1950. In my analysis of Rhine's monograph the use of personal pronouns and names was just over three times the average for the era, at 3.3 usages per 100 words.¹⁷⁸

Credibility and Structure

There is more to the modern conventions of scientific writing than pronouns, evaluative expressions, and the use of various rhetorical devices, however. Structure is also both an essential and an evolving element in the communication of science practise and its 'facts' (Montgomery, 1996):

Science, in great measure, is a matter of language. It is much else besides, of course: people, labor, equipment, instructions, capital, education and so forth. But as knowledge, as a collection of formal understandings that aim at communality and communal power, science must begin and end in words and images, for it is here that literate

¹⁷⁸ Gross and his colleagues (Gross, Harmon & Reidy, 2002) sampled 10-line passages from over 500 articles drawn from highly-cited journals published during the 20th century (p. 241). The presence of personal pronouns and names were counted and an average usage per 100 words was found. Using their method as a guide, I counted the total number of words on the first page of the monograph (Rhine, 1934, p. 3), and on every 10th page after that to page 221 in the conclusion section of the monograph. In addition, I counted the number of times personal pronouns or names were used on each of these pages. (Words appearing in footnotes or tables were not counted). Once completed I calculated the total number of words in my sample (7,153), and then number of times personal pronouns or names were mentioned over all my sample pages (234). I calculated the average citation per 100 words by using the simple equivalence formula $234 \text{ over } 7153 \text{ is equivalent to } X \text{ over } 100$, and solved for X. The average obtained was 3.3 per 100 words, with a range per sample page from 0 citations to 9.9, and a standard deviation of 2.6 citations per 100 words.

societies demand all effort and thought, and find their material embodiments. (p. 430)

A set of conventions regarding the description of methodological detail and the positioning of structural elements in scientific reports came into being over the modern history of science. Gross (1996) argued that in order for theory and conjecture — that is, that which experimental evidence seeks to support — to be persuasive, the ‘elements of a scientific paper must strive for abstraction, separating the “fact” from the methods that produced them’ (p. 91). In their empirical study of scientific documents, Gross and his colleagues (Gross et al., 2002, pp. 189-190) found that, over time, experimental, observational and theoretical sections became separated from one another in an evolving structure designed to be more communicative, more persuasive. The evolution of type, content, and position of elements in a scientific report in the physical and natural sciences fluctuated from the 17th century to the early 20th, but by the second half of the 20th century, they had essentially standardised:

In all disciplines and in all three languages covered by our sample of 20th-century complete articles,¹⁷⁹ the scientific article has grown an abstract that immediately follows the title and by-line, developed a routine three-step introduction, become increasingly concerned with setting the intellectual context by referencing, added a list of citations and acknowledgements as a ready means of crediting others, and evolved a sophisticated finding system that employs headings and different font sizes, graphic legends and numbers, numbered references and equations, and so forth. ... Overall, these measures have helped improve communicative efficiency, in partial compensation for the growing conceptual and semantic complexities of the subject matter and the purposeful narrowing of the intended audience. (p. 172)¹⁸⁰

¹⁷⁹ For this segment of their analysis, Gross et al. (2002) were using primary documents in English, French and German.

¹⁸⁰ The research I am citing is dependent mainly on scientific articles, although in Bazerman (1988), John B. Watson’s behaviourist manifesto, a monograph, is also analysed. This emphasis on scientific articles in my sources, rather than monographs, does not negate the relevance of these points to longer, deeper forms of scientific reporting. Such monographs as *Extra-Sensory Perception* are normally conceived of as a vehicle through which to describe a series of experiments or a research programme in some depth. Because the intended audience is still a specialist one, the structure of such monographs frequently mirror that of a single-experiment scientific report (e.g., Mangan, 1958, Schmeidler, 1960, Osis, 1961, Ullman & Krippner, 1970).

The elements in scientific documents are analogous to ‘arrangement’ in ‘speech’s gross anatomy ... [and] concerns the order of the components of the author’s argument’ (Gross et al., 2002). Arrangement serves to orient the reader to the document, that is: ‘Guided by this order, and the logical links amongst the different components, the readers infer the strength and uncover the weaknesses of the author’s key claims’ (p. 184).

In the Aristotelian system, a speech had four parts: an introductory section, a general statement of the problem, the persuasive argument, and an ‘epilogue’. Gross and his colleagues found that, on the other hand, modern-day scientific articles:

... possess a somewhat different basic structure: introduction, methodology, results and discussion, and conclusion. In this arrangement, Aristotle’s statement or claim appears as part of the introduction, and the middle two parts ... communicate the author’s argument or proof. Ancillary to these main parts are ... front matter and back matter. (p. 184)¹⁸¹

In such modern-day scientific reports the argument is typically conveyed in sections which focus on the methodology used and the results, either combined with, or leading into, the discussion. In Gross et al. (2002), they found that even the content of the elements of a scientific article’s arrangement had been standardised. That is, the introduction normally described and justified the research domain, carving out the ‘niche within that territory’ (p. 184), which was, in turn, followed by an explanation of how the specific experimentation featured in the article fitted into the niche. The methodological section of a scientific article typically included descriptions of planned procedures, ‘materials used in carrying out the procedures’ and a theoretical justification for methodological choices. The results section normally contained the results presented textually and visually and any relevant comparisons of specific segments of the results to other segments. The discussion section then interpreted the results by comparing them to other research or to some theoretical standard, made evaluative statements about the significance of the results, tied the results to previous research, refuted anticipated

¹⁸¹ Front matter is simply the title, the author’s name and an abstract. Back matter refers to lists of references, and acknowledgements and other footnotes.

criticisms, provided conclusions which would restate the relationship of the results to the original claims, commented on the ‘wider significance of those claims to research territory’ and made ‘suggestions for future work to validate or expand upon claims’ (p. 184-185).

Similarly, Bazerman (1988, pp. 257-277) focused on the ‘codification of structure’ in psychological research reports. Just as psychology and other social sciences, over the course of their development, ‘... have been moved to adopt (and adapt) what they perceive to be the methods of the physical and biological sciences’ (p. 257), they had also adopted, Bazerman argued, the structure of scientific reports in the same sciences. That is:

Central to the reorganization of these knowledge-creating communities [that is, the social sciences, behavioural sciences, cognitive sciences, or human sciences] has been an imitation of the forms of argument development within the natural sciences. The compelling force of these arguments, the consensus developed over the aggregate results of these statements, and the power over natural forces achieved through the understanding constructed from these texts, seem to remove them from the traditional realm of rhetoric ... By arguing without seeming to argue and compelling without apparently urging, the scientific manner of formulating knowledge seems to offer a way out of the deep divisions of belief and imponderable conundrums that ... pervade psychological, social, moral and cultural questions. (pp. 257-258)

Writers in psychology and other social sciences, painfully aware of the difficulties faced in experimentation *on* human beings and *with* human beings, sought what seemed to them to be the ‘objectivity’ and ‘certainty’ available in the natural sciences. Just as this positivistic characterisation of the physical sciences has been shown to mask complexities not necessarily visible on the surface, the act of ‘embracing a single, correct, and absolute way of writing science, any model of science’ Bazerman argued, ‘embeds [underlying] rhetorical assumptions’ in the document. Understanding what these assumptions are and how they guide the writing, Bazerman hoped, would allow the scientist to better control the structure and the style in present use, as well as to re-evaluate structure and style ‘as the human world changes’ (p. 258).

Importing structure and style from other disciplines, however, can be problematic. That is:

Attempts to transplant rhetorical forms from one community to another engage basic issues of what these communities are doing and how they go about it. The form will either be changed by the soil and climate of the new disciplinary community or it will struggle with maladaptation. (p. 259)

In psychology's case, the behaviourist tradition and its assumptions have been grafted onto the assumptions of the physical sciences, influencing the expression of both style and structure (p. 257-258). Conventions flowing from these traditions were defined first on experimental psychology, and then influenced the development of structural prescriptions for all areas of psychology. Eventually these were codified in the American psychological community in the *Publication Manual* of the American Psychological Association. The *Manual* itself evolved from a set of guidelines for authors which appeared in the February 1929 issue of *Psychological Bulletin* (p. 259), to a separate supplement of the journal, and finally to an independent handbook of publication conventions which is now in its fifth edition (American Psychological Association, 2003).

Bazerman (1988) described the 1929 guidelines as being short and somewhat general in their advice. Two areas were emphasised, the 'Subdivision and Articulation of Topics' (p. 261) and the presentation of sufficient detail so that readers could criticise the methodology and attempt replications. Some early psychological research reports appearing in the *Psychological Review* in the late 19th- and early 20th-centuries, before the codification in *Psychological Bulletin*, foreshadowed the later guidelines quite closely in their organisation. For example:

... [There was an] opening theoretical discussion ... [that] argues that a new kind of measurement is needed. The experimental design then provides the desired measurements. ... each aspect of the experimental method is justified and explained in terms of current knowledge ... The specific parameters for measurement refer back to the theoretical problem, and the actual results follow immediately as a response to the specific parameters. Discussion of the consequences of the results ... follow naturally as part of the thematic continuity of the whole essay. (Bazerman, 1988, p. 264)

Articles which would be recognisable in even the structural and stylistic terms of 1929 were not in the majority in those early decades, however. Widespread structural

standardisation didn't happen immediately with the turn of the 20th-century in psychology, but rather evolved over the decades. As late as the 1920s, many articles published in the psychological literature still followed what Bazerman identified as a 19th-century style. That is, they began with common everyday problems, and the resulting scientific examination read as 'continuously reasoned arguments', written in a philosophical style. The audience for whom such early articles were intended also varied. Rather than being aimed always at a specialist audience, quite a number of articles published in the psychology literature of the first two decades of the 20th century were intended for 'a wide range of people interested in the workings of the mind' (p. 268).

By the 1930s, however, the psychology article was becoming more standardised in structure, with a set of typical sections usually included in research reports. Unlike Gross et al. (2002), however, who found that methodology sections became somewhat *more* important as the 20th century progressed (pp. 184-185). Bazerman felt that, in psychology, methodology sections were becoming *less* important in the sense of being a bridge between the literature survey (in which the context of the experiment was justified) and the discussion section (in which the significance of the results were interpreted and future research was planned). Instead, in psychology, Bazerman argued, the methods section became the position in the scientific report in which the researcher assured his audience that his experiment had been conducted properly, establishing the reliability and validity of the results.

Both Bazerman's study and that of Gross and his colleagues were, in effect, emphasising the persuasive role methodology sections had to play in the research report with subtle differences in their arguments. For Gross *et al.*, the 'factness' of the underlying natural phenomena, the contact with 'objective truth' being displayed in a scientific report rested on the plausibility of the method-as-described as a proper vehicle for obtaining the presented results. For Bazerman, the methodology section was rather more personal: it was the credibility of the scientist and his or her ability to follow the rules that was at issue. Method, for both Bazerman and Gross and his colleagues, was the vehicle by which science was communicated, with the scientist-competently-doing-

method at the forefront in psychology, and an objectified method-as-depersonalised-science-practise in the forefront in the physical sciences.

The results section itself, whilst occupying the same relative position in the report, acquired a new importance as a context for the arguments offered in the discussion section (pp. 272-273). These structural changes, Bazerman argued, were also a consequence of the shift to behaviourism in psychology in the early 20th century:

With the article primarily presenting results, constrained and formatted by prescription, the author becomes a follower of rules to gain the reward of acceptance of his results and to avoid the punishment of non-publication. Accepting the role, he subordinates himself to the group endeavor of gathering more facts toward an ultimately complete description of behavior ... (p. 273)

Under behaviourism, Bazerman contended, psychology became an exercise in ‘incremental encyclopedism’ (p. 273), which, in turn, had more structural consequences for the acceptable scientific report. The hypothesis moved from a place in the discussion to the introduction so as to set an agenda for the article’s arguments, with frequent restatements as the article moved on to the conclusion. As Bazerman said, ‘... the “problem” [came] ... to mean the test of the hypothesis and the “discussion” the confirmation of the hypothesis’. This shift seemed to recast the audience from a somewhat passive community of readers who were interested in the problem area to a more active community of readers whose duty it was to find ‘such faults ... [as would] disqualify the experimental report as a valid increment to the descriptive encyclopaedia’ (p. 274). The new emphasis on a hypothesis-based structure made it more important, Bazerman argued, for the author to display competence rather than to be *merely* persuasive (pp. 274-275).

When the initial hypothesis was controversial, as Rhine’s defence of extrasensory perception most surely was, an author needed to be more careful in their conformance to the ‘rules’. Gross et al. (2002) argued that such authors needed to be mindful of that which was potentially controversial in their reports, taking care to justify such elements by ‘presenting and “impeaching” any plausible weaknesses’ that the reader might find in the report (pp. 207-208). As Gross (1996) argued elsewhere, any speculation needed to be argued in an exceedingly careful fashion, using inductive

means that were classically Baconian, moving from the most conservative points that were ‘closest to the facts’ to the points that were more conjectural (p. 96).

Whether Rhine was conversant with the structural elements that were necessary to make a persuasive scientific case in the natural and physical sciences, or whether he was in agreement with, or in opposition to, the evolving conventions in report writing in psychology — and the different requirements for potentially controversial research — is a matter for speculation.¹⁸² Whilst it is obvious from a great deal of his writing that he was not a behaviourist *per se*, and whilst the department of psychology in which he functioned was set up from the beginning as a haven for anti-behaviourist psychologists, the methodologies that Rhine developed were in some sense so simple operationally that they were more like the classical conditioning experiments of the behaviourists than they were not.¹⁸³ How unusual Rhine’s structural choices in *ESP* were in the context of the whole of research-based literature in American psychology in the 1930s is a matter for empirical study that is beyond the scope of this thesis. Suffice it to say, however, that even in the psychical research literature and especially in the experimental psychology literature, the structures described above were at least apparent, if not common. That is, research was normally reported by sections in the document that first stated the problem area and/or hypothesis, next reviewed past literature relevant to the research problem, then described the methodology used to test the hypothesis, next presented the results obtained, and finally summarised the results in a discussion which also reflected on the disconfirmation or confirmation of the original hypothesis, interpreted the significance of the results, speculated on the relationship of the results to the wider problem area, and set future research agendas.

¹⁸² Archival research might be able to answer this question to some extent. Rhine left an enormous amount of correspondence, some of it with his principle critics, and since their criticisms were at times structural, whatever personal philosophy that lay behind the structure of the monograph may be available in his correspondence.

¹⁸³ A commonly-heard tale in experimental parapsychology circles, especially from individuals who were connected to the Duke Parapsychology Laboratory from 1940 to the early 1960s (such as *Journal of Parapsychology* editor Dorothy H. Pope, and the researcher Karlis Osis), was that Rhine specifically developed his methodology to investigate the problems of psychical research by using the methods of behaviourism, so as to ‘beat the behaviorists at their own game’ (Dorothy H. Pope, personal communication 1983; Karlis Osis, personal communication, 1986).

Following Bazerman, the presentation of the methodology used would serve to instil confidence in the reader that the experimenter had conducted the experiment ‘cleanly and correctly’ (Bazerman, 1988, p. 271). Following Gross et al. (2002), the presentation of the methodology used would serve to reinforce the potential for obtaining ‘objective’ measurements of an underlying natural ‘truth’. Following both sets of analysts, the report itself would attempt to be persuasive, to convey the scientific content effectively, and to do so in a manner that established both the ‘propriety’ of the methodology and the ‘factness’ of the findings.

How does *ESP* fare when examined in light of these conventions? The structure of Rhine’s monograph is readily apparent in the description of its content above. Rhine (1934) began with a general introduction in which he covered the relevant psychical research literature, focusing mainly on the experimental studies of clairvoyance and telepathy which preceded him. For example:

The question or problem is a rather broad one, not limited to the perception, extra-sensorially, of mere objects or states, but is unlimited. It includes the perception of the mental states of other individuals, the facts of the past and of distant scenes, of sealed questions or of the “waters under the earth”. The future, too, and its scrutability are within the scope of the general problems ... The manner of the operation of such parapsychic perception, too, must be broadly viewed in clarifying the problem; it might be in hypnotic trance or under the influence of a drug, with the aid of an “object of reference” ... by the use of a crystal ball, a cup of tea-leaves, the ouija board or a divining-rod. So far as the generalized problem goes, these are all included in the broad question, Is there a human function of extra-sensory perception? (p. 12)

The issues he covered in Part I were well within the standards of the day, with the scientific claims he hoped to test described and evaluated, the appropriateness of the context in which he was conducting his research, his personal interests in the operational separation of clairvoyance from telepathy, and in the justification of his innovative simplification of the methodology used and so on. For example:

I refer to the results of systematic observation of clairvoyance mainly in its various forms of private and professional practice: dowsing, or clairvoyance with the use of the divining-rod; “psychometry,” or clairvoyance with the use of an object of fixation connected with the situation in question: crystal-gazing, card-clairvoyance and the like. If

in such practice there are given facts not known by the recognized means, as many studies claim to show is true, we have in them somewhat better material for study than in spontaneous cases, due to the fact that precautions can be taken and conditions imposed that permit systematic observation and to some degree approach true experimentation. (p. 18)

We need tests for pure telepathy and more of them for pure clairvoyance, made under conditions that enable easy evaluation of significance, provide safe exclusion of other modes of cognition, and introduce variation enough to suggest the relation of E.S.P. to other processes and lead to its natural explanation. (p. 39)

The issues Rhine covered in Part III were also well within the standards of the day. In Chapter 9, 'Elimination of Negative Hypotheses', Rhine identified possible weaknesses in his experiments and attempted to persuade his readers that he had counteracted them, or that they could be persuasively argued against on the basis of his experiments. For example:

Logically, the first alternative suggestion that is evoked to explain unusual results such as these high scores in card guessing, is that they "just happened." That is, that no special principle of causation is responsible; rather, that a number of unimportant circumstances contributed the peculiar results. This general absence of a special causal principle we can call the Chance Hypothesis. ... there is the mathematical evaluative principle of probability, by which we may be sure of the odds against an event occurring by chance alone. ... What "chance" then, has the Chance Hypothesis, when from chapter to chapter in Part II the value of X rises by leaps and bounds ... The relative certainty herein established for the Extra-Sensory Perception principle thus goes far beyond the highest standards and requirements we have for any phase of inquiry. (pp. 145-146)

Chapters 10 to 14 comprised his interpretation of his results and his evaluation of their meaning for various other branches of science as well as for psychology. For example:

One conclusion that seems fairly clear is that E.S.P. depends upon the higher functions of the nervous system. It requires a degree of control by the higher functions that permits a certain amount of "concentration"; i.e., attention to one thing and exclusion of others. This depends upon a certain degree of integration of the nervous system.

Dissociative drugs, sleepiness and certain illnesses work to lower this integration and self-control; whereas drugs that antagonize dissociative drugs help to recover normal control. And in our results the data show plainly that dissociative factors likewise lower E.S.P. ability, whilst counter-active factors help to restore it. (p. 169)

In Chapter 15, his concluding chapter, Rhine both set the agenda for future research and included additional information he felt would be useful to those who would attempt replication. For example (drawn from a list of 8 specific conclusions):

... 2. The distance data, along with the general facts, suggest the freedom of mind in E.S.P. from the common material relations of extension or distance. It would argue for the non-physical nature of mind if it can operate under these conditions. This is psychologically important as bearing upon the question of the body-mind relation, upon personality-survival and some of the other questions in the natural philosophy of mind. (p. 222)

... 7. There seems to be in this work thus far a “species level” of E.S.P. ability reached by most subjects and not much exceeded, on the average, over large numbers of trials. The evolutionary origin and the biological survival value of E.S.P. are problems at which we have only hinted possible answers. (p. 223).

The middle part of Rhine’s monograph is where he deviated from what would have been the expected structure for scientific reporting. Instead of producing a chapter on methodology and following it with chapters on results organised by type of experiments, Rhine chose to combine methodology and results in the same chapters. An overview of the research programme and the development of various methodologies were combined in the first chapter in Part II. Another chapter was devoted to ‘earlier and minor experiments’, three chapters were devoted to the results of individual high-scoring subjects, and one chapter to the results of five other subjects. Basic information about specific types of experiments were thus distributed over the chapters in service of the arguments Rhine was trying to make, and especially, in service of the autobiographical and biographical nature of the narrative. Rather than presenting evidence that the experiments had all been conducted ‘cleanly and correctly’ (Bazerman, 1988, p. 71), so as to provide the authoritative context necessary for acceptance of the results as well as the speculative discussion based upon them, rather than ‘separating the “facts” from the

methods used to establish them' (Gross, 1990, p. 91), Rhine chose, in the middle chapters, to mix methodology with results as well as with personal commentary, evaluative statements, *and* speculation.

If we take the D.T. (Down Through)¹⁸⁴ experimental procedure as an example of the way in which methodological details were distributed across the middle of the monograph, we find the first mention of the method in the chapter on high-scoring subject Hubert Pearce:

And finally, he did very well under the remarkable D. T. condition, in which the pack is left unbroken on the table while the subject makes the 25 calls in succession for the cards before him. (p. 99)

No mention of which type of cards were used in this particular experiment is made at this point, however, nor is there a specific mention of the room in which the test occurred nor were the experimenters or observers, if any, identified. The results of the series appear in a table on page 100. Two pages later we are told that Pearce suggested the method himself in a sentence in which other innovations Pearce proposed are also listed:

A few changes he has taken without a considerable drop, those apparently in which he has taken part in the planning and in which he felt sure of success. among these were the use of very small figures on the cards (about 2 mm. high) which he suggested, the D. T. procedure which he partly originated himself and the calling for low scoring, voluntarily proposed half playfully. These all succeeded at once. (p. 103)

D.T. next appears on page 111 where the distance between the card deck and the subject are varied in experiments. How specifically this was done, where and in what sequence is not mentioned. (We know who the subject was because we are still in the chapter on Hubert Pearce.) Here the reference to the procedure, the distances and the

¹⁸⁴ “Down-through” is a method by which cards are shuffled and placed face down on a desk, behind a screen or in a box, and the subject’s task is to guess the face identity of the cards from the card on the top of the face-down deck *down through* the deck to the card on the bottom of the face-down deck. Once the subject’s guesses for the entire deck have been recorded, the cards are turned over, one at a time, and entered into the record. ‘Hitting’ is determined by the number of matches between the subject’s guess and the face identity of the appropriate target card.

results are combined together in two paragraphs, and the results themselves are also presented in tabular form on page 112. There is also the following ambiguous statement:

Now, at the distances used, 8-12 feet and 28-30 feet, both D.T. and B.T. together do not show enough positive deviation to reach mathematical significance ... [w]hereas the P.T., which in the same room yielded less than the D.T. and about the same as the B.T., yielded at the shorter distances (8-12 feet) a positive deviation over 5 times the p.e. ... (p. 111)¹⁸⁵

Once again it is not possible to tell several crucial details, such as: How were the distances varied and in what order? How were the methods varied and in what order? How were they combined in order to make the claim that ‘together [they] do not show enough positive deviation’? and so on. Other crucial questions are also left unanswered, such as: What was the method of cutting and shuffling the cards? On what were the cards and calls recorded?¹⁸⁶ Who were the experimenters? Were these experiments also observed? and so forth.

The D.T. method is mentioned again on pages 112 and 113: on 112 in a comparison of Pearce’s scores on various methods, and on page 113 to characterise the D.T. method as one in which, Rhine claimed, again ambiguously, ‘the subject is most independent of his surroundings’.

D.T. appears again on pages 117 and 120 as a passing mention of tests done with George Zirkle and as a passing mention on 121 in discussions of Sarah Ownbey’s pattern of results within experiments and across different methods. On page 121 it is also noted that Turner and Bailey had done D.T. work (no description of the experiments or results are given on the page) and that Cooper had not yet tried the D.T. method.

¹⁸⁵ B.T. is the ‘Basic Technique’ in which the deck was shuffled and cut and set face down on the table in front of the subject. The subject called a card, the card was removed still face down and set aside, and then the subject called the next card and so on down through the deck. The difference between B. T. and D. T., was that in D.T., the cards were not touched by *either* experimenter or subject until all the calls had been made. P.T. was the pure telepathy method in which the experimenter thought of a card, and the subject made a guess and the experimenter noted whether or not the subject had been correct.

¹⁸⁶ Standard card/call comparison record sheets were in use by the 1950s but when they were developed and how they were used in the early ESP tests is a matter for archival research. Whether or not the early data is preserved there can not be said without investigating the holdings fairly closely, a project for which I was unable to obtain funding and had to abandon.

D. T. is next mentioned on page 149 in which results are used as an argument against the fraud hypothesis — evoked generally and not specifically reiterated — because they were obtained when observers were in the experimental room and no sensory contact with the cards allowed to the subject. D.T. next appears on pages 159 and 161-163 when the position of the pack on the laboratory table is used as an argument against the radiation hypothesis of E.S.P. information acquisition. On pages 164-167, the pattern of scoring curves obtained with the D.T. method, in which there is a decline in correct guessing in the middle portion of the 25 calls, is used as an argument for the ‘physical difficulty’ (p. 164) of perceiving the centre of the pack when the deck is placed face down on the table and called straight through before feedback. Again, in this section, the conditions under which the results were obtained are not specified, although the scoring patterns are represented by curves on graphs which are in turn identified by subject name.

The development of the decks and the use of other types of symbols on the decks was information that was also distributed throughout the monograph. For example, the term ‘E.S.P. cards’ is first mentioned in Prince’s introduction on page xxii without any description. The term next appears on page 67 when the design of the deck is described and on page 68 when Rhine mentions ‘935 tests on the E.S.P. cards’ and the use of the cards to perform another 800 trials with various students in his search for high-scoring subjects. The construction of the cards themselves is not described (such as thickness of card stock or dimensions), nor is there any description of the way in which the symbols were stamped on the cards (such as the orientation of the image to the card edges, the size or colour of symbols). On pages 72 and 75 the cards were again mentioned in passing. What follows is an example of how this mention was made:

Mr. Lecrone, a student in my class during the summer of 1931, become [sic] deeply interested in my results, yet was courteously but frankly skeptical. He therefore (as one could only wish all skeptics would be spurred to do) set to work to give the question a fair test. He used the E.S.P. cards and following the procedure of having the agent look at the card while the subject attempted to perceive it. Mr. Lecrone’s conditions were not perfect but they served after 1,710 trials to convince him of the reality of extra-sensory perception. (p. 75).

Although we know from the paragraph that the methodology used was one in which an agent attempted to send the identity of the image on the ESP card to Mr Lecrone, we know absolutely nothing about the room in which the experiments took place, the distance of the agent from Mr Lecrone, whether Lecrone could see the backs of the cards, how the cards were cut and shuffled, which method of feedback was used, how the guesses were recorded and matched against the order of cards in the deck, whether there were witnesses, whether the 1,710 trials were accomplished consecutively or whether there were breaks in the procedure, and to what exactly the statement ‘Mr. Lecrone’s conditions were not perfect’ alluded.

It should be obvious from this brief examination of the way in which a key method, D. T., and the key target material, the ESP cards, were handled in the monograph that the level of specific detail the reader might be able to find in any methodological description was rather worse than one would expect, even if one read the monograph after having read the most critical of the published reviews.¹⁸⁷ In fact, the description that Rhine provided was so sketchy it is nearly impossible to get a picture of what his methodology *really* was, and *absolutely impossible* to reconstruct any specific experiment in all its details.

Far from separating the ‘fact’ from the method by which the ‘fact’ was established, Rhine embedded his facts in his methods, passing lightly over his methods to emphasise his personal, and oftentimes speculative, evaluation of the results, sacrificing even the barest of methodological or procedural detail for a breezy, biographical ‘glimpse’ of the subject, or a lyrical, but terse historical depiction of the research. As the monograph began to get serious scrutiny, this very atypical presentation of methodology and results had its consequences in assessments of Rhine’s competence, the credibility of the report, and whether or not the field Rhine hoped to establish was, in fact, scientific.

¹⁸⁷ Amongst the reviews and early critical comments were: Anonymous (1934); Dearborn (1934); Holroyd (1936); Kellogg (1936) Murphy (1934); Thouless (1935), and Willoughby, (1935a, 1935b, 1935c).

Extra-Sensory Perception after Sixty Years

From 1934 to the 1944, one hundred and fifty three items of criticism and response were published (see Table 6). In 1934, the first three reviews of *ESP* appeared and from 1935 to 1938, the number of articles increased dramatically, with the single exception of 1936. From the peak of 1938 when 46 items of criticism and response were published, the number per year declined, until 1944 when only seven appeared. The level of items characterisable as criticism or response would stay below 10 per year until 1955 when the Price controversy sparked renewed interest in the ESP controversy in the wider world and 33 items were published in the Anglo-American literature.

The period between the publication of Rhine's (1934) monograph and the spring of 1939 when Rhine's staff members began working on *Extrasensory Perception after Sixty Years (ESP-60)* included a number of important experiments, exchanges of criticism and response, and social 'events' of the magnitude of the ESP symposium for the Annual Convention of the American Psychological Association, organised by Stanford Professor of Psychical Research and critic, John Kennedy, and held in the fall of 1938. Space constraints prohibit me from including more detail about this era — except that which will be perceivable through the description and rhetorical analysis of *ESP-60* — but suffice it to say that by 1939, Rhine and his staff were feeling positive about the

Table 6.

Number of Items of Criticism and Response, 1934 to 1944

Year	Critical Items	Responses to Critical Items	Totals
1934	3	0	3
1935	6	5	11
1936	2	3	5
1937	17	12	29
1938	29	17	46
1939	16	12	28
1940	10	13	23
1941	5	7	12
1942	0	6	6
1943	1	2	3
1944	0	7	7

prospects for their research, having survived the 1938 APA meeting, and having received the news that Rhine had been admitted to membership in the American Psychological Association by a committee of five of whom only the psychologist Gardner Murphy was positive towards ESP *per se*. In this same year, a friend of Rhine's who worked for the Henry Holt publishing company in New York City suggested that the ESP controversy be drawn together in a 'technical book on ESP that would systematically explain and justify the methods and conclusions of his laboratory' (Mauskopf & McVaugh, 1980, p. 292). In response to that request, for six months, from mid-spring through the summer, Rhine's staff worked collectively on the manuscript that would become *ESP-60* (Pratt et al., 1940).¹⁸⁸

In the introduction to the volume, which was published on March 15, 1940 by Henry Holt,¹⁸⁹ J. Gaither Pratt characterised the work done to produce *ESP-60* as a 'strenuous ... period of compilation' (Pratt et al., 1940, p. ix). Conceived of as a 'complete review of the recent research in extra-sensory perception, in the light of all of the criticisms that it has drawn' (p. v), the systematic preparation of *ESP-60* not only kept its five co-authors busy but also three other full-time members of the Duke University Parapsychology Laboratory staff, and a host of laboratory 'friends' and critics, both at Duke and elsewhere.

¹⁸⁸ Whilst researching an article I wrote with a colleague (Zingrone & Alvarado, 1987) in the Rhine archives at Duke University in 1986 and 1987, I came across some of the laboratory records of this project. Rhine put together a master list of the topics to be covered in each chapter and assigned these to Stuart, Greenwood, Pratt and Smith, their tasks to be accomplished with the aid of laboratory clerical staff members, amongst whom was Dorothy Pope. Records were kept of the critics to whom Rhine wrote for comments to be included in the volume, as well as records of who responded and with what. Dorothy Pope remembered that whilst Rhine was in charge of the process and had final say on what was included in the final draft, Pratt did most of the writing, knitting together the contributions of the group into a cohesive whole (Dorothy Pope, Personal communication, 1986). For this reason Pratt was given first authorship on the title page, although the publisher created a confusion about the order of authorship that has lasted for decades by listing Rhine as first author on the spine, and by listing the authors in alphabetical order on the original dust jacket.

¹⁸⁹ Holt's advance publicity touted the volume as 'Rhine's sequel to *New Frontiers of the Mind*' (Books, Authors, 1940) which belied its very pronounced rhetorical and substantive differences from Rhine's (1937) popular book.

What was produced became the ‘central classic of experimental parapsychology’ (Honorton, 1993, p. 195). At the time, and in the years following, the text positioned the collective point of view of the writing team and their collaborators between the best of the psychical research that preceded the publication of *ESP-60* and future generations of researchers.

Three types of inclusions in the text helped to ensure that *ESP-60* took this central position. The first was the set of six chapters devoted solely to a comprehensive review of substantive criticisms of ESP research, responses to those criticisms, further commentary by some of the most active critics, and responses to that commentary (Pratt et al., 1940, pp. 70-242). The second inclusion took the form of 21 appendices devoted to statistical methods and to a comprehensive listing of studies included (Pratt et al. 1940, pp. 363-420). The third was a glossary of terms.

Although some have claimed that *ESP-60* was reviewed widely in the scientific press (Broughton, 1991, p. 72), reviews actually appeared only in the popular press (e.g., Anonymous, 1940b; Kaempffert, 1940a; Moulton, 1940; Skinner, 1940), in a compilation of brief reviews of books received in *Philosophy of Science* (M., 1940), and in five psychological journals — reviews which ranged from favourable (Garrett, 1941; Snyder, 1940) to mixed (Anonymous, 1940a; Ellson, 1940) to hostile (Anonymous, 1941). Two additional reviews appeared in the psychical research literature (Carrington, 1940; Taves, 1940).

A number of psychologists who received complimentary copies of the book took time to respond to Rhine’s request for a detailed critical commentary, some of which was quite favourable.¹⁹⁰ There were indications that the book was received in some quarters with a great deal of approval. For example, not only had the Chairperson of the Harvard University Psychology Department, social psychologist Gordon Allport, and Harvard faculty member, the experimental psychologist Edwin G. Boring, written Rhine with congratulations on the book (Mauskopf & McVaugh, 1980, p. 295), but chapters 1

¹⁹⁰ Mauskopf and McVaugh (1980) noted that Rhine sent 200 copies to any psychologist who was interested enough to request a copy. The only thing he asked was that they corresponded with him once they received their copies. About ten percent did so, and when they disagreed with the conclusions reached in the volume, they did so without the kind of polarising rhetoric that had characterised some of the critics of Rhine’s earlier volume (1934) (pp. 294-295).

through 6 and 8 through 10 were assigned to psychology undergraduates at Harvard during the 1940-1941 academic year. Similarly, Goodwin Watson of Teachers College, Columbia University, assigned it to his introductory psychology students (p. 357, notes 62, 63).

On Style in *ESP-60*

Before I discuss the style and structure of *ESP-60*, it is useful to take a look at the differences between this volume and Rhine's (1934) monograph (*ESP*). There are two ways in which these two books differ somewhat from one another: in intertextuality, and in conformance to scientific conventions regarding the use of personal pronouns and proper names.

Reference Citations in ESP-60

As mentioned in the discussion of *ESP*, Alan Gross (1996, p. 13) argued that a 'network of authority' was necessary to anchor a scientific text in the community for which it was intended. One of the primary ways in which such a network was evoked in a work was the 'trail of citations' to relevant literature. As was seen above, Rhine's 1934 monograph embedded itself in psychical research through its citations but barely attempted to relate any aspect of the methodology or the findings to psychology even though its author claimed that parapsychology was a branch of psychology. At first glance, the network of authority established by citation in *ESP-60* was somewhat different. Table 7 shows the percentage breakdown by discipline of the journal in which the references first appeared.¹⁹¹

Eighty-two out of 87 citations (94%) in the Rhine's (1934) monograph were to psychical research references, whilst only one was to a psychological source, and then only to a general book by McDougall. (The other citations were to statistical texts.) In *ESP-60*, on the other hand, three hundred and sixty-seven articles, books, and other

¹⁹¹ The citational style of *ESP-60* was analyzed by entering all the references into a Stat Paq Gold statistical database, and coding them for language of original, discipline to which the published item contributed, and the type of items cited. As this was conceived of as a descriptive exercise, only frequencies per variable were calculated.

published items were referenced. Of these, 230 (62.7%) were drawn from the psychical research and parapsychological literature. Of the remaining 137 references, 26 (7.1%) were drawn from such popular sources as *Harper's Monthly Magazine* and *The New York Times*. Historical and religious publications accounted for three more references (0.8%). Seven (1.9%) were drawn from general academic journals and magazines such as *American Scholar*, and eight from such general science journals and magazines (2.2%) as *Popular Science Monthly*.

Table 7.

Breakdown of Reference Citations in *Extrasensory Perception after Sixty Years* by Discipline of Publication

Discipline	Number	Percent
Psychical Research/Parapsychology	230	62.7%
Psychology	51	13.9%
Popular Literature	26	7.1%
Statistics/Mathematics	18	4.9%
Philosophy	13	3.5%
General Science	8	2.2%
General Academic	7	1.9%
Psychiatry	3	0.8%
Psychoanalysis	2	0.5%
Anthropology	2	0.5%
Education	1	0.3%
Philosophy of Science	1	0.3%
Eugenics	1	0.3%
Social History	1	0.3%
Sociology	1	0.3%
Spiritualism	1	0.3%
Religion	1	0.3%
Total	367	100%

The social sciences — anthropology, education and sociology but excluding psychology — accounted for 4 (0.9%) publications. Fifty-six articles were drawn from the psychological, psychiatric, and psychoanalytic literatures, with psychology contributing 51 of the fifty-six (13.9% of the total references). Statistics and mathematics accounted for 18 of the references (4.9%) and the remaining references were drawn from philosophy journals (13, or 3.5%), philosophy of science (1 or 0.3%), and eugenics (1 or 0.3%).

Another citational habit which signals conformance to scientific style is the exclusive or near-exclusive limitation of reference materials to scientific articles

published in refereed journals, technical reports, and chapters in conference proceedings. Although the majority of the references cited in *ESP* were to items published in journals and proceedings, these were generally the publications of psychical research and not of mainstream science.

As can be seen on Table 8, in *ESP-60*, 63.9% of the articles cited were published in academic or scientific journals or proceedings, but only 30% of these were published in *mainstream* academic or scientific journals. The team who wrote *ESP-60* had endeavoured not only to represent the breadth of the controversy that had surrounded their work but also to reiterate the justification of their methodological and mathematical choices. So articles published in the disciplines other than psychical research or parapsychology touched specifically on some methodological problem or other raised by the Rhine work, and/or speculated on its meaning, or otherwise examined problems of relevance to those other disciplines that had arisen from the ESP work such as the mathematical discussion of issues related to Rhine's use of probability theory.

Unlike *Extra-Sensory Perception* (Rhine, 1934), *ESP-60* operated as a text within a larger scientific debate that took place largely *outside* of psychical research and parapsychology proper. But where *ESP-60* did not differ from *ESP* was in the fact that the problems and methods considered in the wider literature *centred solely* around the problems raised by the 'special branch of psychology' Rhine and his colleagues sought to establish. Like *ESP*, the authors of *ESP-60* did not try to embed their work in the wider concerns of experimental psychology or any other science, with the exception of mathematics and probability theory. *ESP-60* lacked a grounding in psychology proper just as *ESP* did.

Of the types of publications listed in *ESP-60*, only 16 of them (4.4%) could be characterised as outside scientific publishing, even though fully 31.7% could be considered informal scientific reporting because they were items which varied in formality from conference proceedings to notes and editorials.

Table 8.

Breakdown of Reference Citations in *Extrasensory Perception after Sixty Years* by Type of Publication

Reference Type	Number	Percent
Journal/Published Convention Proceedings Article	234	63.9%
Book	56	15.3%
Newspaper Article	15	4.1%
Unpublished Manuscript	13	3.6%
Letter to the Editor	10	2.7%
Committee Report	8	2.2%
Research Review	6	1.6%
Journal Note	5	1.1%
Unpublished Masters Thesis	3	0.8%
Technical Report	3	0.8%
Book Review	2	0.5%
Appendix	2	0.5%
Editorial	1	0.3%
Magazine Article	1	0.3%
Book chapter	1	0.3%
Unpresented conference paper	1	0.3%
Conference report	1	0.3%
Conference presentation	1	0.3%
Total	367	100%

What can be assumed from this examination of the citation style of *ESP-60*, however? Was the work of the Duke laboratory — whilst perhaps more intertextually connected — really located within the wider scientific community? Perhaps all that can be said is that there was movement towards the scientific norm from *Extra-Sensory Perception* to *Extrasensory Perception After Sixty Years*, but the journey was not, by any means, complete.

The Use of Language in ESP-60

As mentioned earlier in this chapter, in addition to citational intertextuality, the use of language in a scientific report can signal whether or not the content ‘belongs’ to science in the sense of being ‘objective’, ‘data-driven’, or ‘fact-oriented’; that is, whether or not the scientific reader is moved to an assessment of reliability, validity, and scientific value by the prose with which he or she is presented. As was seen earlier in this chapter, Rhine’s use of language in his monograph differed markedly from what was usual in science writing of the day.

To examine whether or not *ESP-60*, which was team-written, differed in use of language of *ESP*, I conducted an analysis on *ESP-60* which was essentially similar to that conducted on *ESP*. That is, I examined every 10th page (when that page was not a table), counting the instances of the uses of personal pronouns and personal names, and obtained a ratio of usage per every 100 words for each of the pages reviewed. As will be remembered from the earlier analysis, Gross, Harmon and Reidy (2002) had found that the instance of personal pronouns and personal names had stabilised in scientific writing to a average instance of about one per 100 words. Unlike *ESP* which had more than three times that many, *ESP-60* had a mean of 1.78 per 100 words. When the analyses of the two volumes were compared using a Mann-Whitney U statistic, the difference was statistically significant (*ESP* Median = 2.68, *ESP-60* Median = 1.53, U = 600, z = 2.65, p[2t] < .008).

The prose in *ESP-60* clearly conformed more closely to the scientific norm in terms of instances of personal pronouns and personal names than Rhine's earlier work had done. But it should be noted that in *ESP-60*, the mean number of instances per 100 words still approached twice the number Gross, Harmon and Reidy had found. Thus, similar to the analysis of the references cited in *ESP-60*, there had been movement towards the norm in science writing in the style of *ESP-60* when *ESP* was taken as the starting point, but the journey was far from complete.

The Structure of *ESP-60*

J. Gaither Pratt and his colleagues (Pratt et al., 1940) stated the goals of *ESP-60* in its preface as follows:

[T]he authors have attempted to condense into a reasonably compact form: (a) all the experimental and evaluative methods by which the research has been done and by which its adequacy must be judged; (b) all of the results obtained — grouped, classified, and analyzed so as to enable them to be assayed critically from the point of view of all possible alternatives; (c) a thorough digest of the criticism, both constructive and otherwise; and (d) all of these as they bear upon the clarified question about which the research is concerned, with as much an answer to that question as the assembled materials permit. (p. vii)

The plan of the volume followed three parts. What follows is a description of the content.

Part I: The Question of the Occurrence of ESP

The first chapter in Part I was designed to set the stage for the research work, and to make the case that the methodology Rhine developed was truly derived from a body of experience reported for centuries prior to the formal beginnings of psychical research. As Pratt put it, the research programme of the Duke parapsychology group was motivated by ‘a need to disentangle the real problems from a welter of claims and ... [to find] methods to explore where psychology has seldom before ventured ...’ (p. 3). The text went on to describe briefly the place of ESP in the traditions of magic, the world’s religions, and in philosophy, both in the positive and the negative sense. Following this, the authors noted the presence of ESP-like phenomena in mesmerism and in dowsing, amongst other areas. They next reviewed the rise of Spiritualism and the founding of the Society for Psychical Research.

Pratt and his colleagues (1940) felt that pre-experimental and early experimental research had defined the research problem only vaguely. Experimental research, they argued, required a ‘sharply clarified statement of the object of inquiry’. The classification of the research problem as psychological arose because they believed that ESP experiences were ‘spontaneous experiences of a cognitive nature’. Further, because ‘the reported experiences are supposed to represent the apprehension of events external to the organism, there can be no doubt that, if it is what it seems to be, the occurrence is perceptual’ (p. 14), hence the term ‘extra-sensory perception’.

In the effort to derive an operationalism for experimental testing, the task at hand was reframed so as to provide the ‘most conservative formulation of the problem’:

Is it possible repeatedly to obtain results that are statistically significant when subjects are tested for knowledge of (or reaction to) external stimuli (unknown and uninferable to the subject) under conditions that safely exclude the recognized sensory processes? (p. 15)

A successful research programme required a variety of elements, amongst them ‘good faith, precautions against error, understanding of scientific method, correct

evaluation of results, and a long list of minor but general considerations assumed for all scientific experiments' (p. 15). But even as they reviewed these more obvious requirements of a scientific programme, Pratt et al. acknowledged that '[b]ecause of the strong opposition which the ESP hypothesis meets from modern psychologists, uncommonly high standards of evidence are required'. In addition to which, experiments in ESP research needed to concern themselves with the competence of the experimenters and the honesty of both experimenters and subjects, something which, they argued, was normally irrelevant in scientific research.¹⁹²

Pratt et al. reserved for themselves the right to reject 'additional demands' that were 'inconsistent' with the aims of the research or which set up conditions which were likely to inhibit the phenomena (p. 19). Finally, they argued that whilst a conservative formulation of the experimental problem might limit its usefulness as a basis for speculation, solving such a problem would lead to other questions of interest which could have a deeper meaning (pp. 20-21).

In the second chapter of Part I, 'The Mathematical Methods', Pratt et al. reviewed probability theory, the sample problem, and specific formulas available to researchers with a brief description of their development. The chapter also included a review of efforts to analyse statistically experimental research published before Rhine's (1934) monograph.

Chapter 3, 'The Experimental Methods', included a description of all the main elements of the then-standard ESP test, from the interpersonal atmosphere in the experimental room to the development and use of ESP cards, obtaining subjects, testing for different types of ESP, the materials necessary to safeguard against sensory cueing and so on.

Chapter 4, 'A Survey of the Results of ESP Tests', summarised the research up to 1939. Six reasons were given for compiling the results into one review: (1) '... a

¹⁹² It is unlikely that they meant that such concerns were *in fact* irrelevant, rather, honesty and competence were normally assumed, and those assumptions not discarded unless experimental results were not replicable. It is an interesting point from this remove, especially because we are more aware now of how the impression of competency and honesty is constructed textually, and what the cost can be when an author chooses not to conform to scientific conventions of construction, as Rhine chose not to do in *Extra-sensory Perception* (Rhine, 1934).

summary of all the evidence is advantageous in getting a general perspective for a decision about a particular research ...'; (2) '... valuable insights often arise from the comparison of large bodies of data of independent origin, and important trends are thus discovered ...'; (3) '... [s]omething in the way of appreciation of the scope of the research ... is to be gained from even a shallow survey of numbers of reports, experimenters, and subjects represented ...'; (4) the survey also provided readers with a review of the experiments which failed to confirm the ESP hypothesis, with attention to identifying those conditions which might be inhibiting the phenomena; (5) that by summarising the disconfirmatory and confirmatory studies together, one could see that sufficient replication had been made that disconfirmatory studies could be seen as potentially useful in identifying which conditions were necessary and sufficient for the appearance of the ability;¹⁹³ and (6) that the treatment of disconfirmatory results and confirmatory results taken together did not render the 'whole mass of data ... insignificant' (p. 72) but rather provided the reader with an opportunity to decide whether or not the results argued for the existence or non-existence of ESP.

The rest of the chapter presented the results in tabular and textual form. One-hundred and forty-five studies were reviewed for this compilation. The tables included summaries of the number of reports treated, the number of subjects tested, the laboratories in which the experiments were done, the statistical analyses of the results using the Critical Ratio, a compilation and analysis of studies which precluded sensory cues, with a breakdown by the type of precaution taken and the associated results.¹⁹⁴ Four conclusions were drawn from the review of the results:

¹⁹³ This point and the previous one only served to underscore the authors' belief that ESP *had been* proven and *did* exist and so disconfirmatory results could be used as indicators of methodological failure. This inability to discuss disconfirmatory results as truly *disconfirmatory* and to entertain the notion that ESP did *not* exist set apart the use of failed replications by Pratt et al. from the use critics made of them. If there had been movement in the style and structure of documents that defended the ESP hypothesis towards what was the norm in science, there had not been movement towards the predominant valuation of that hypothesis as it existed in the wider scientific community. Rhine's group never argued from a position of neutrality on this issue. On the other hand, such a commitment to one's own competence and the phenomena in which one is interested, is normal science practise and tends only to be questioned when the individual scientist's interpretation of their results conflicts with some competing tradition (e.g., Fahnestock, 1997; Myers, 1990)

¹⁹⁴ One worrisome inclusion in the review were the papers published by MacFarland and George (1937) and by MacFarland alone (1938) which should have been set aside because of the strong suspicion that the

- that ‘the majority of reported series are extra-chance in nature’
- that evidence supporting the ESP hypothesis was also apparent in series in which sensory cues had been excluded;
- that evidence supporting the ESP hypothesis had also been found in experiments in which ‘possible clerical errors’ were excluded; and
- given the first three points, the evidence surveyed to that point had to be considered supportive of the reality of extra-sensory perception (p 105).

In Chapters 5 and 6, Pratt et al. summarised the counter-hypotheses and the evidence for them. By counter-hypotheses they meant anything that had been postulated as an alternative explanation of the results obtained, that is, something *other* than extra-sensory perception. Chapter 5 took each of these individually and Chapter 6 examined them in combination. Their two goals in the first of these two chapters were to: ‘list all the recognisable alternative hypotheses, without regard to what anyone may think of them or how well they may have been answered and dismissed in the past’; and to answer the question ‘Does any *one* of these hypotheses fit *all* of the recorded research?’ (p. 107).

In Chapter 5, Pratt et al. first listed these counter-hypotheses in turn, and then evaluated the evidence in light of them. They divided all of the counter-hypotheses offered into seven subsections: those dealing with chance; with ‘selection’; with the ‘practices of the subject’; with ‘shuffling defects’; with record-keeping; with ‘sensory

experimenter (MacFarland) had faked the results by altering record sheets. Instead the articles appeared on a table which compiled results of experiments thought to have included ‘special safeguards against error, with exclusions of sensory cues’. There may have been methods used in these experiments which obviated the sensory cues to the subjects but fraud on the part of the experimenters was strongly suspected if not proved given both the analysis of the records sheets done by the Rhine group and the article on experimenter recording errors which focused on MacFarland’s studies by Kennedy (1939). Rhine’s daughter, Dr Sally Feather, has claimed that MacFarland was the only person ever suspected of fraud by the Rhine team who did not admit to fraud when confronted. Consequently, it was her belief, that her father had decided, because the evidence was not incontrovertible, that it would be improper to identify these experiments as fraudulent (Sally Feather, Personal communication, 2004). Unfortunately, the consequence of this decision is that the two studies remain in the literature and are occasionally cited by reviewers of ESP research as providing an example of a methodological refinement.

leakage'; with the competence or honesty of the experimenter; and finally, 'hypotheses of general speculative character'.

Under the chance section, Pratt et al. reviewed challenges to the statistical significance of the results. Amongst these were: the appropriateness of the specific mathematical and statistical methods used; the notion that extra-chance results were due to 'luck', with the implication that luck was something that could not, or did not need to be explained in any deeper way; and the general charge that either nothing or anything could be proved by statistical analysis, with the implication that statistics were thus useless in scientific research.

Under selection, Pratt et al. surveyed a number of specific criticisms, amongst them that subsets of the data had been excised from the complete data set and illegitimately analysed separately, whether this had been done on the basis of the characteristics of the data, or on the whim of the experimenter. Optional stopping, the cessation of a session due to an understanding on the part of the experimenter that a significantly positive statistical outcome had been reached, was also included in this section.

Under the counter-hypotheses that were related to the behaviour of subjects in the experimental situations, Pratt et al. reviewed the charge that subjects had kept mental track of their responses so as to maximise their scoring in later trials, as well as the argument that response biases of some subjects artefactually matched non-random sequences in the target card order. Response biases were further divided into subjects' non-random sequencing of calls within a run, and their possible preferences for specific symbols.

Under 'shuffling defects', three possible problems were reviewed: (1) pre-existing defects in the cards or defects which resulted from frequent use, which could then provide clues to the identity of the card face; (2) inadequate shuffling which left target orders from previous sessions relatively undisturbed, decreasing the randomness of the target order from session to session, and thus increasing the possibility for artefactual matching; and (3) the potential for subjects to come into contact with the cards prior to the experimental test, obtaining sufficient information about the identity of the cards so as to identify them later, or to mark them deliberately for that purpose.

Seven counter-hypotheses of the recording process were reviewed. The following were proposed as capable of accounting for all positive results: (1) errors in recording either the target order, or the subject's calls, or both; (2) errors in matching target order and subject's calls; (3) errors made when hits were counted; (4) computing errors at various stages combined; (5) inadvertent loss of disconfirmatory data; (6) motivated loss of disconfirmatory data; and (7) tampering with data records to produce spurious positive results by subjects or experimenters or others.¹⁹⁵

The evidence for five counter-hypotheses which involved sensory cues were reviewed next: (1) that experimental procedures did not preclude the subject from glimpsing the identity of the cards as the tests proceeded; (2) that tactile and visual cues were available to the subject during the experiments; (3) that subjects used marks on the cards which they themselves had made to identify the card faces; (4) that auditory cues had either inadvertently or intentionally conveyed information about the identity of the card face to the subjects; and (5) that cues due to faulty printing communicated the identity of the card face to subjects.

Four counter-hypotheses under the general heading of experimenter incompetence were then reviewed. These were: (1) 'loose conditions and poor observation' (p. 143) on the part of an incompetent experimenter which allowed errors of all kinds to artefactually inflate the results; (2) that ESP reports were in the main too sketchy methodologically to identify the confounding variable or artefact that must have caused the results;¹⁹⁶ (3) personal biases or enthusiasms predisposing experimenters to make errors in procedure or interpretation that would promote spurious positive results;

¹⁹⁵ Rhine's group argued that tampering as an explanation was unreasonable when the positive data was taken in aggregate, given that 100 confirmatory studies had been reviewed. Further, they felt a small number of experiments existed in which such tampering was ruled out, and these experiments had yielded similar results to other, less well-controlled experiments. It should be noted that even if the MacFarland studies had not been included, the ratio of confirmatory to disconfirmatory studies was still better than the 2:1 (e.g., 98 confirmatory studies to 45 disconfirmatory studies).

¹⁹⁶ This particular counter-hypothesis implied that incompetence or fraud *must* have occurred in every positive study, and evidence as to the presence or absence of a causal artefact could be found if a critical experimenter wished to dig deeper into the experimental details, although the argument did not *require* the critical experimenter to do so. This argument is the polar opposite — and equally indefensible — of the assumption that no experiment with negative results was ever *in fact* disconfirmatory, but rather only indicative of some inhibitive element in the study.

and (4) ‘moral or pathological’ failings of the experimenters which would account for honest error or dishonesty and fraud.

Finally, Pratt et al. considered five ‘speculative’ counter-hypotheses: (1) that positive results could not *prove* the existence of ESP because ESP itself had been negatively defined within the context of the experiments; (2) that previous indications of ESP should have been uncovered over the history of experimental psychology and because they had not, ESP did not exist; (3) that if ESP occurred it should have some ‘practical’ use and because such a use had not been demonstrated, it did not exist; (4) if ESP existed then the experiments designed to provide evidence would have been replicable; and (5) the mere notion of ESP conflicted with the philosophy of science and therefore must be assumed to be an *a priori* impossibility.

As each counter-hypothesis was discussed, Pratt et al. presented evidence and arguments against them, leading to their conclusion that:

None of the thirty-five hypotheses considered has been found capable of accounting for ESP results in their entirety, and it remains now to consider these hypotheses in combination. (p. 153)¹⁹⁷

In Chapter 6, Pratt et al. turned to a presentation of the best experiments, that is, of experiments they felt answered the most reasonable of the counter-hypotheses through the implementation of strict methodological procedures. Four experimental series were described in detail and presented as answering all of the important criticisms and still providing positive results. They were: the Pratt-Woodruff Series; the Pearce-Pratt Series; the Warner ‘Test Case’; and the Owney-Zirkle distance experiments. The Reiss

¹⁹⁷ Whilst evidence supporting many of these counter-hypotheses was weak, the argument that Pratt et al. made appeared to be built on a logical fallacy. They seemed to assume that for *any* counter-hypothesis to be condemnatory of positive results it must account for *all* positive results. Such an assumption took the law of parsimony to its breaking point, given that each experiment had its own environmental, methodological, and interpersonal variables, all of which could be expected to vary from trial to trial, session to session, and experiment to experiment, not to mention from laboratory to laboratory. More persuasive was the emphasis in the following chapters on those experiments which obviated the more important counter-hypotheses and still provided positive results, although critics could not be expected to agree that even a single study that survived this particular kind of flaw-analysis *proved* the ESP hypothesis.

experiment and the Murphy and Taves experiment were also offered as lesser, but nearly equal examples of unassailable experiments.¹⁹⁸

In dealing with these experiments Rhine's group felt that it was not legitimate to dismiss research on the basis of possible errors that had not yet been identified, a counter-hypothesis which could be used to demolish their experimental exemplars, nor was it appropriate to hold that once seemingly unassailable experiments had been found, any experiment with similar results, whether flawed or not, should be considered positive evidence of ESP. Rather, the 'correct position ... [was] to regard the type of research series under discussion as serving only a secondary purpose, although still one of great value: namely, that of narrowing the issues and facilitating judgement' (p. 174). However, once such exemplars had been identified, it was possible to use the larger group of less well-controlled studies as supportive of the conclusions of the evidential studies.¹⁹⁹

Part II: The Criticism and The Evidence

In Chapter 7, the first chapter of Part II, Rhine's group intended to review the published criticisms and to provide brief arguments against their applicability. In the introductory paragraphs, the authors claimed that they were taking a conservative approach to the criticism and response that followed. Pratt et al. organised the criticisms

¹⁹⁸ All of these experiments have been taken to task since the publication of *ESP-60* (e.g., Hansel, 1961a, 1961b, 1966, Beloff, 1980a, 1980b). For example, in the Pearce-Pratt experiment, whilst extraordinary precautions were taken with the record-keeping, and the target deck was kept well away from the subject, the subject himself was not observed whilst the experiments were taking place. Hansel proposed an elaborate scenario in which the subject might have gone to great lengths to fraudulently obtain the target card identities. Although this scenario has been largely rejected as impractical given the layout of the Duke University campus (Stevenson, 1967), the fact remains that the subject was not observed and thus the evidential value of the study is compromised.

¹⁹⁹ The distinction is a reasonable one, but there is a pragmatic difficulty that arises in that the reader remembers not that the exemplar studies provided evidence for a specific conclusion, but that the entire body of literature, whether flawed or not, provided positive evidence. As a qualitative supposition this conclusion could cause a reader to overestimate the overall evidentiality of a database. Quantitative compilations of data, on the other hand, such as meta-analysis, whilst providing a clearer depiction of the strength of a database overall — especially if study quality is entered into the equation — can be just as prone to misrepresentation due to the subjectivity that can creep into coding, blocking, and inclusion/exclusion criteria. See the criticism of the Milton and Wiseman meta-analysis of the Ganzfeld, for an example of a meta-analysis that has been heavily criticised on these grounds (Zingrone, 2002b).

into four levels of usefulness, the first being criticisms that were made on specific methodological points. The second most useful class of criticisms involved ideas that could be ‘tested by experiment or demonstration’ (p. 184). Less useful were value judgements issued by individuals with a different area of expertise or a different belief system. Whilst these could be operationalised into useful criticisms, Pratt et al thought these types of criticisms were more susceptible to bias or prejudice. The least useful type of criticism were ‘vague allusions, untestable contentions and expressions of personal beliefs’ (p. 184).

The next section of the chapter provided a brief historical overview of the criticism which followed the publication of *ESP* (Rhine, 1934), and which ended, the authors contended with the APA Symposium in 1938 at which ‘... [e]ssential agreement was reached on methods, and the problem was generally recognised as coming within the scope of academic psychology’ (p. 185).²⁰⁰ Following this, the authors reviewed first the mathematical and statistical criticisms and then the criticisms which had focused on methodology. Finally, Pratt et al. reviewed the more general criticisms on the *a priori* likelihood of psychic phenomena and the ability of science to deal with the question.

A brief section was included in which the authors claimed that the understandable interest of the general public in the topic was taken, incorrectly, as an indication that Rhine’s group had sought publicity in a fashion that was unseemly for scientists. In a footnote to this section they offered to send a copy of the Laboratory’s official policy regarding publicity to anyone who requested it.²⁰¹

²⁰⁰ ‘Essential agreement’ was a kind of gloss over the distances that still existed between the critics and Rhine’s staff and collaborators. Further, whilst there were certainly some who felt that ESP research could be classified within experimental psychology, as some of the surveys (e.g., Warner & Clark, 1938) showed, there was still a significant number of psychologists who felt very strongly that parapsychology was not then, and should not ever be, considered part of psychology (e.g., Rogosin, 1938a, 1939; Wolffe, 1937, 1938).

²⁰¹ A great deal of publicity was generated on Rhine’s behalf when the original monograph was published. Those who promoted *ESP*, such as the Boston Society for Psychic Research and others, most likely did so because of what they felt Rhine’s work could do for the scientific status of psychical research. Certainly Waldemar Kaempffert almost single-handedly kept Rhine’s work in the forefront of science news as covered in the pages of the *New York Times* (e.g., Kaempffert, 1937a, 1937b, 1938, 1939a, 1939b, 1940a, 1940b, 1941a, 1941b), although one gets the idea from Kaempffert’s prose that part of his motivation was to denigrate mainstream experimental psychology at every opportunity. It can be said that, to some extent, the publicity Rhine’s work got was not always Rhine’s doing. On the other hand, this section can also be

Following another brief section in which the results of the opinion surveys were reviewed (e.g., Warner & Clark, 1938), Pratt et al. listed constructive criticisms that they had found particularly useful (e.g., Lemmon, 1937; Hertzmann, 1938; Willoughby, 1935a, 1935b, 1935c, 1937). The next section reviewed a survey they had done themselves, when, in the summer of 1938, Rhine sent the following letter to forty-five individuals who had been publicly critical of the ESP research:

In the interest of taking every advantage of the critical judgment of all those interested, from whatever point of view, in the research in extra-sensory perception, I am writing to ask you if you are willing to draw up a brief statement as to what you would like to see done (beyond what has already been done) in the interests of what may appear to you a crucial testing of the hypothesis of extra-sensory perception. You are doubtless familiar with conditions used to exclude sensory cues and with the methods now in use for checking and statistically handling the results, as these have been described in the *Journal of Parapsychology*.

It is important to determine just what standards of evidence are generally acceptable in the interests of guiding future research. Any statement you make will be used only in staff discussions in the Parapsychology Laboratory. (p. 208)

Twenty-one replies were received, which Rhine's group thought, could reasonably be included in an aggregate and anonymous way in *ESP-60* without violating the conditions under which the comments were solicited. In their review, they grouped the responses under two headings: replies which focused on weaknesses that had been uncovered in past experimentation; and replies which focused on future methodological modifications which would enhance the evidentiality of any results obtained. Out of these, Pratt et al. identified a number of constructive criticisms that they felt should be adopted in future studies. Amongst these were: separating subjects and target materials

seen as somewhat disingenuous considering that Rhine chose to write a popular book (e.g., Rhine, 1937) in between his monograph and *ESP-60* rather than concentrating on scholarly articles. In addition, a number of reviewers of his popular book noted his tendency to misrepresent the details of his research whilst inflating the philosophical and scientific significance of the work of his laboratory (e.g., [Davis], 1937; Skinner, 1937), a fact which underscores Rhine's willingness to simplify to the point of obfuscating the scientific research he and his team had done. A further bit of countervailing evidence is Louisa Rhine's description of a change in the laboratory policy which occurred in 1939, in which she characterised her husband as deciding for a 'period of quietude' during which the laboratory would seek less publicity rather than more (Rhine, 1983, p. 190), a direct contradiction to Pratt et al.'s claim that the publicity Rhine's work had received before the publication of *ESP-60* was entirely unsought.

or senders by keeping each in different rooms; adopting methods of automatic recording; publishing complete tables of run scores; conducting experiments with subjects in darkened rooms; having an independent individual shuffle cards and record target card orders before the experiments were conducted; and having the experimenter who handled the cards use gloves. Comments on the constructive criticisms were generally positive: Pratt et al. indicated their willingness to adopt further controls.

Chapter 8 included comments that had been solicited from prominent critics specifically for inclusion in the *ESP-60*. Seven critics — Gulliksen, Kellogg, Kennedy, Lemmon, Thouless, Willoughby, Wolfle — were invited to provide comments for the volume.²⁰² Pratt et al. reprinted Rhine's letter in full because only three of the seven invitees agreed to participate and they worried that sceptical readers would think that there was something about the invitation that had kept some of the most important critics — e.g., Gulliksen, Wolfle, Kennedy and Willoughby — from taking up the challenge. The wording of the letter was respectfully done. The seven invitees were sent three chapters on which to comment at length: Chapter 4 which summarised all the research that had been done up to that point, and Chapters 5 and 6 which examined the research results in light of all of the named counter-hypotheses. They were also sent Appendix 17 which contained all the published reports in tabular form and Appendix 18 which included the relevant references.

The letter was sent to the invitees on the 17th of August in 1939 and the respondents were given until October 1st to produce a manuscript of 2,000 to 4,000 words which would then be included in the volume verbatim. Gulliksen declined because his academic schedule did not permit him to provide something either within the stated time frame, or within a 30-day extension past the deadline which Rhine had offered in hopes

²⁰² Both Gulliksen (e.g., 1938a, 1938b) and Wolfle (e.g., 1938a, 1938b) could be described as severe critics, although willing to present arguments that focused on the substantive content of the ESP reports and not only on the *a priori* perceived goodness-of-fit of the subject matter to the wider concerns of science. The rest were moderate critics who had always focused on substantive issues and whilst they might not have been convinced by any of the counter-arguments offered by the Rhine group, were at least willing to engage them seriously.

that Gulliksen could participate.²⁰³ Wolfle also declined the invitation due to time constraints. Kennedy declined — by return mail — for other, rather more interesting reasons:

August 22, 1939

Dear Dr. Rhine:

I must beg off your request for critical reading of Chapters IV and V of your new monograph. As you know, I reviewed the literature of Extra-Sensory Perception for the same period and came to the conclusion that little information useful to critics can be obtained by past experimental reports.

Frankly, I do not think that either past or modern ESP warrants serious attention by psychologists until you have obtained extra-chance results by methods which you have already advocated as desirable. A year ago at Columbus, you spoke at length on the desirability of a fraud-proof recording and selecting device for future ESP work. You recently wrote me that no data have been collected with such a device. Surely the problem of design and use of the machine is not so complex that you have been unable to collect data under a single condition with your best subject. Where are the fraud-proof ESP data promised by you a year ago? I sincerely commend this hiatus in your experimental proof to your immediate research attention before you publish another monograph.

You have my permission to publish this letter.

Sincerely yours,

(Signed) John L. Kennedy

²⁰³ One wonders why Rhine's team could not have set a more generous response timeframe from the beginning given that they were asking academics to take on a considerable load of extracurricular work at the beginning of the academic school year when it could safely be assumed that time would be extremely limited. In addition, one would have thought that any publisher who knew the terrain would have understood the importance of having the full participation of as many productive critics as possible, and thus agreeing to a more flexible production schedule. The only crucial element was that the book should appear sometime in between 1940 and 1942, given that the volume was entitled *Extrasensory Perception after Sixty Years* and had taken 1882 — the year in which the Society for Psychical Research had been founded — as its starting date.

Kennedy clearly felt that *ESP-60* was being published prematurely. Equally, Kennedy decried that fact that the only type of evidence he would consider worth re-focusing his time — the results obtained from a fraud-proof automated machine — were nowhere apparent in the chapters he had been sent.²⁰⁴ Rhine wrote back and invited Kennedy, amongst other things, to comment on G. N. M. Tyrrell's experiments with his testing device, the only such device in existence at the time, but a reply was never received.

Willoughby also had strong objections to getting involved with the new manuscript, partly because he felt that further belabouring of points already made would not be useful, and partly because he himself had never been able to obtain positive results in the card experiments he had tried. Rhine also wrote to Willoughby, hoping to get him to reconsider but in his reply Willoughby merely reiterated his points and declined again. Lemmon, Kellogg and Thouless, on the other hand, contributed critical comments which were printed in full, followed by Pratt et al.'s replies.

Lemmon's comments began with an indictment of his fellow critics who were raising issues in then recent publications that had already been settled, or were out of date in the sense that more recent experiments had made the earlier criticisms moot. His primary concerns included the use of the database as a whole by Rhine and his colleagues when it was clear to him that early studies were not of sufficient methodological rigour to be included in an evidential review. Lemmon felt that conducting experiments in which the probability of a hit was $\frac{1}{2}$ would obviate a lot of the mathematical controversy and would allow researchers to base the interpretation of their findings on firmer ground. He also felt ESP research should include experiments in which the point was to allow subjects to learn the ability, by giving trial by trial feedback. Then if ESP was like any other ability and was something that could be learned, subjects might be trained to the point at which their consistent positive scoring allowed testing of other hypotheses. Lemmon also provided an extended discussion of optional stopping and took issue with Greville's (1939) comment, insisting that, up to

²⁰⁴ This particular demand would have probably been disheartening to Rhine's group given the fact that there had been 'obstacles that [had] ... kept an ESP machine from being perfected' (Rhine, 1983, p. 190) of sufficient gravity that no data had ever been gathered under the conditions Kennedy requested.

that point, the mathematicians who had discussed the problem had not really grasped the criticisms that had been raised.

Pratt et al. applauded the criticism Lemmon levelled against his sceptical colleagues regarding the quality and timeliness of their criticism. The authors defended their use of methodologically inferior studies in their discussions by maintaining that they had made a distinction in the text between studies that were evidential — that is, methodologically sound with positive outcomes — and merely favourable — that is, methodologically inferior but with positive outcomes. However, in a footnote, Lemmon responded that whilst Rhine's group had made that distinction, they had not kept to the distinction in their prose, leading the reader to think of the entire dataset as evidential, a practise that was, in his estimation, clearly unwarranted. Pratt et al. acknowledged the comments on probability and the 'sporadic nature of ESP' (p. 227) and referred readers to later chapters. They continued to argue the point on the problem of optional stopping whilst noting that a correction had been developed to use in such cases, the latter comment Lemmon found to be 'very satisfactory' (p. 228).

In his criticisms, Kellogg objected to the use of the term 'extra-chance' as synonymous with ESP. He rejected the arguments that were made by Rhine's group on the use of probability theory and referred again to Wolfle (1938a, 1938b), Zubin (1937a, 1937b), Becknell (1938) and the Heinleins (1938) as having provided the reasons why Rhine's use of it was illegitimate.²⁰⁵ Kellogg raised again the notion that a subject of Pratt's whose scores had declined to a zero deviation from the mean chance expectation had merely experienced a 'run of luck' (p. 231) in her previous trials and could not be

²⁰⁵ This is evidence that the understanding of probability held by the subset of the critics who were involved in the development of measurement in psychological testing (known as psychometrics in psychology proper) was incommensurate with the understanding held by the ESP researchers, their more moderate critics in psychology, and by the mathematicians. I am not a mathematician but my own reading of Becknell and the Heinleins led me to believe that none of these writers understood what the ESP researchers were in fact doing with probability theory. In addition, Becknell, the Heinleins, Kellogg and Rogosin seemed to be arguing against the use of the normal curve and significance testing in any social science, something which was not only against the grain at the time, but which was a point of view that would become less and less widely accepted in mainstream psychology as time went on. This may be a misreading of the psychometricians and their arguments on my part, of course. In any case, Kellogg conceded nothing on this point, nor indeed on any other points regarding the work of the Rhine laboratory.

said to have once had the ability and, over time, lost it.²⁰⁶ Kellogg made the very cogent argument that discussions of how many negative results must be available to counteract positive results should not revolve around the idea that all positive results were ‘purely’ extra-chance. However, as Kellogg rightly noted, the criticisms raised had identified a number of artifactual and fraudulent means by which positive results might have been obtained, leaving the pool of positive results that could be seen as ‘purely’ extra-chance at a much lower number than was generally assumed, thus requiring a much lower number of artefact- or fraud-free negative results to counteract them.²⁰⁷ Kellogg’s final argument was that as methodological rigour had increased, levels of extra-chance scoring had decreased which made it likely that further identification of errors or artefact would erase the phenomena entirely.

Pratt et al. agreed with Kellogg that other variables — including the methodological details of an experiment — allowed one to attribute — or not — the source of the extra-chance scoring to ESP. They contended that their use of extra-chance was merely as a synonym for statistical significance and not as a synonym for ESP (p. 234).²⁰⁸ Kellogg’s reiteration of the mathematical arguments against the use of probability theory were rejected and the statement made by Burton Camp (1937) of the

²⁰⁶ I agree that ESP researchers were (and are) too quick to assume that failure to replicate previous results in retests of the same individuals is an indication that there had been ESP in the data originally but now the subject had lost his or her ability instead of assuming that the initial test was extra-chance purely by chance and that no ‘ability’ had been present in the earlier test. Characterising this as a mere ‘run of luck’, however, is itself illegitimate, a type of argumentation in which critics use ‘luck’ as a ‘universal container’ (Weiner & Geller, 1984) that does not have to be further analysed.

²⁰⁷ A further problem was identified by Pratt et al. in their discussion of what we would now call the ‘file-drawer problem’. This was that whilst a number of critics (e.g., Adams, 1938; Lemmon, 1937, 1939) published their negative results in full, a larger number, such as Gulliksen, merely made claims that they had obtained negative results without publishing them. Unless negative experiments are published in full, it is impossible to tell whether or not they are, themselves, artefact- or fraud-free, and thus would constitute studies of sufficient quality to counteract positive studies of comparable quality. The critical community still tends to make claims that thousands of negative experiments have been conducted and thus the case for ESP has been sufficiently counteracted, when *no published* evidence of these thousands of experiments exists.

²⁰⁸ Like the distinction between evidential and favourable, their use — in practise — of extra-chance seemed to require the interpretation that extra-chance results must necessarily be attributable to ESP, however.

American Mathematical Institute used as proof that this argument had been settled.²⁰⁹ Pratt et al. also took issue with Kellogg's sense that the decline in ESP scoring was related to the increased rigour in methodology, but they ended their review of his comments by giving Kellogg credit for the substantive value of his contributions to the controversy:

Dr. Kellogg has been the most penetrative and thorough of the critics of the ESP research, and has been by far the most widely read and influential of its opponents ... To him may be credited the first expression in print of the question as to whether the ESP test scores represent a true binomial distribution. He set to work at the difficult task of determining what difference lay between the binomial and the matching hypothesis ... believed the correct one for treating ESP scores, and he approximated the frequency distribution for the matching hypothesis ... [which] led to actual mathematical research on related points in at least five different universities. Though it was not a crucial question for the ESP research, the problem ... was one of considerable mathematical interest. Its solution has led to distinct contributions both to mathematics of probability and to the evaluative side of ESP research. (pp. 237-238)²¹⁰

Thouless found the chapters he received to be 'convincing' (p. 238). He felt that Rhine's group could be faulted for giving too much space to criticisms that were, Thouless thought, based on logical and mathematical fallacies. He lauded them for presenting tables of data gathered under more stringent conditions, and found those to be more convincing than the 'astronomical numbers obtained under mixed conditions' (p. 239). Thouless felt, however, that in tables which combined experimental results, the data should have been divided so that it was obvious which results were obtained with more rigorous methodology and which were not. Thouless objected to a footnote on the

²⁰⁹ This response to Kellogg's mathematical argument is yet another indication that the two worldviews on this point were incommensurate. Kellogg was essentially arguing for a rejection of the mathematical community's authority to settle the argument (or even to understand it fully) and Pratt et al. rejected Kellogg and the other psychometricians who wished to paint probability theory as controversial in general and useless for the purpose to which Rhine's group had put it in particular. For Rhine's group the mathematicians and statisticians they consulted were the only authorities on this point, and therefore they continued to reject the psychometricians' claim to competency.

²¹⁰ It could not have been lost on the readers that Pratt et al. praised Kellogg for inspiring the solution to the problem with which they were satisfied but which Kellogg categorically rejected.

publication of positive versus negative results in which the authors claimed that some individuals who had obtained positive results had been reluctant to publish them whilst others who had obtained negative results had rushed to make them public before they had been published in full. Because Thouless's own negative experiments were publicised without his permission before their formal publication, he felt that the language of the footnote was a bit strong. Finally he had faith that a properly designed experiment could militate perfectly against the intrusion of the experimenter's biases whilst he disagreed that ESP should have appeared in psychological experiments because he thought any influence of the ability would have been virtually undetectable.

The authors of *ESP-60* took heart that the critics who had contributed comments had not raised any new issues, and that only two of the points discussed were of interest to future researchers: how to deal methodologically and mathematically with optional stopping, and whether perfecting ESP research methodology further would eventually eliminate all positive results.

In Chapter 9 the authors summarised the points raised in Part II and reiterated their conclusions.

Part III. The Nature of ESP

The third part of *ESP-60* comprised five chapters in which the methodological and substantive findings of ESP research were reviewed. It is clear from the tone of these chapters that, at this stage in the document, the authors believed that the case for ESP had been made. They felt free to treat the experimental studies accumulated over the entire history of laboratory research on the topic as grist for the mill. The question 'Does ESP occur?' had been settled for them, and now they could safely turn to the question 'What is the nature of ESP?'. They were so sure of this conclusion that they felt it necessary to include the following footnote so as to provide encouragement to future researchers who might want to contribute to the evidence supporting the existence of ESP and who might be led by the authors' firm conclusion to feel that the question had already been settled:

... this position [should not be] ... construed as implying that any further work contributing merely to the strengthening of the ESP

hypothesis would be without value. (p. 249)

Chapter 10 provided the reader with the argument for including not only the well-conducted recent experiments, but earlier, less methodologically-rigorous experiments in the search for answers to the question of which specific variables were related to positive results in ESP tests. To take into account the varying quality of the studies used, they developed a three-point rating scale to characterise the strength of their conviction that an influence they described was supported by acceptable evidence, either in quantity or quality: (1) *established relation* in which at least two studies could be found to support a particular relationship and which were not, in and of themselves, susceptible to the counter-hypotheses raised by critics;²¹¹ (2) *indicated relation* in which a single, well-conducted experiment provided evidence for the relationship under consideration;²¹² and (3) *suggested relation* in which studies that were not considered to be sufficiently rigorous were used, and of which the ratio of 'favourable findings to ... adverse' was at least 2:1.²¹³

Chapter 11 focused on the relationships Rhine's group believed they perceived between ESP ability and the 'psychological, biological, anthropological and social character' of their subjects. They reviewed studies that spoke to the question of whether or not ESP ability was related to sex, age, presence of physical or mental handicap or illness, and hypnotisability.

²¹¹ This meant for the authors that at least one of the six studies identified as being the best evidentially needed to support the relationship in question, and that the second study used, if not also one of the best six studies, should at least have excluded visual cues. Further, if the relationship being put forth was counter-intuitive or in some way contradicted scientific knowledge or mainstream scientific beliefs, both studies must have been drawn from the more stringently-conducted studies.

²¹² This criteria were also modified should the relationship being postulated run against scientific beliefs, then at least two studies of sufficient quality were said to be needed to provide evidence, or three studies of lesser quality.

²¹³ This rating scale provided another indication of Rhine's unwillingness to set aside any study as beyond the pale, no matter what level of quality had been attributed to it. Presumably this tendency to be over-inclusive flowed from Rhine's belief in the appropriateness of his own research and his frustration at the imposition of methodological rigour which he saw as so much 'red tape and safeguards beyond reason' (Rhine, 1983, p. 190). Whether this frustration flowed from a lack of understanding of the methodological points that had been raised, disdain for experimental control, or just the inability to set aside so much hard work, or whether the frustration lived only in J. B. Rhine or was shared by the rest of his team is difficult to determine at this remove. Mauskopf and McVaugh (1980) are silent on this point.

Chapter 12 reviewed the methodological conditions that seemed to affect ESP scoring rates such as: the state of consciousness of the subject during testing, whether such a state was self-induced or brought on by drugs or alcohol; such elements of task complexity as number of targets and methods of response; the social psychological variables of the testing situations under which they included classroom testing with adults or children; the presence of visitors and other kinds of observers during the testing situation; and the social relationship enjoyed prior to the experiments by the subjects and experimenters. They also reviewed the evidence for differing motivational states in subjects as they were influenced by such test variables as the novelty of the target material or whether or not rewards had been built into the test, whether subjects worked alone or in competition with one another, the impact of the timing of feedback on the subjects' interest in the test, whether or not the experiments were conducted formally or informally, whether frustration had an impact on subjects' performances, and whether the subjects had control over the pace of the experiments or instead were required to keep to a particular schedule or tempo as imposed by the experimenter.

Chapter 13 focused on what Pratt et al. called 'physical relations'. In this review they looked at the 'range of stimulus' used in the experiments (p. 292), whether experiments were set up as pure telepathy or pure clairvoyance, or were conditions in which any type of ESP could be combined to produce a result. The size, visibility, and physical proximity of the target cards on results were also reviewed, as was the use of barriers, screens, or the introduction of distance between the targets and the subjects. A brief summary of this section included an argument against the importance of physical variables to the outcome of the experiments, a foreshadowing of stronger arguments Rhine would later make for the non-physicality of ESP,²¹⁴ and comments on time, in the sense of whether or not target materials were prepared before or after the subjects completed their guessing.

Chapter 14 examined the psychological nature of ESP, whether ESP was 'an unconscious process' (p. 311), 'erratic' (p. 312) or 'stable' (p. 313), and whether or not

²¹⁴ Rhine claimed to use the term 'non-physicality' to mean as yet unknown physical laws relating to the use of ESP in the test situation, although in practise, he used the term as a synonymous for 'spiritual' or even, 'beyond science' (see Zingrone, 1984).

the ability could be learned. The notion that ESP was an either/or kind of ability, and its relationship to the will were also considered.

For those who believed that ESP had been established experimentally, these chapters provided the groundwork for what would later be a number of seemingly-lawful relationships between ESP scoring and methodological, social and psychological variables (e.g., Radin, 1998). For those who did not, these chapters were wholly premature and the suppositions were only slightly more systematic than the speculations published in *ESP*.

Part IV: The Present Situation

The last part of the volume contained four chapters. Chapter 15 reviewed the problems that Rhine's group considered to be 'unsolved' (p. 329). Amongst these were: how to account for individual differences in ESP ability amongst subjects; how to uncover and then implement test conditions which would be conducive to positive scoring;²¹⁵ the need to further investigate possible physical variables related to ESP performance;²¹⁶ and the relationship of ESP ability, if demonstrated, to the individual psychological characteristics of high-scoring subjects.

Chapter 16 focused on methodology under development at the time *ESP-60* was prepared. Amongst these were methods by which ESP experimentation could use 'normal situations' or 'natural beliefs of the subject' (p. 339), or by which one could capitalise on 'a state of anticipation' in the subject (p. 340). The idea of testing ESP through the use of various apparatuses was also included, and various elements necessary for producing such devices were examined, as were the development of

²¹⁵ A great deal of modern research has revolved around or been built on conditions that were considered to be psi-conducive such as the induction of an inward-turning altered state in subjects (e.g., Bem & Honorton, 1994; Honorton, Berger, Varvogliss & Quant *et al.*, 1990; Krippner, 1993), the identification of personality characteristics such as absorption or dissociation (e.g., Zingrone, Alvarado, & Dalton, 1997-1998) or even of occupational categories such as artists and musicians, individual representatives of which (e.g., Dalton, 1997; Schlitz & Honorton, 1992) have produced consistently positive overall scoring rates as compared to unselected subjects.

²¹⁶ Research conducted at Edinburgh has contributed to this line (e.g., Dalton & Stevens, 1996).

methodologies to test for precognition, and to further automate the shuffling of target card decks.

Chapter 17 identified statistical problems that the authors claimed had been recently ‘solved’ or which still posed a problem. Amongst these were the proper ‘evaluation of blocks of data’ (p. 349), and ‘covariation’, that is, testing the dependence of the results of tests completed by individual subjects and determining whether or not lawful patterns existed between certain types of tests or certain types of individuals.

Chapter 18 presented a final summary of experimental parapsychology as it existed when the manuscript was finished. Pratt et al. concluded that scientific progress had been made in ESP research and that there were sufficient grounds to assume that further research would be valuable. They justified their optimism partly because of the number of psychologists outside of the Duke University laboratory who had taken up the research, the number of courses that included parapsychology as a topic area within the course or which focused on parapsychology itself, the number of textbooks that included mention of ESP research, the number of graduate schools at which theses on parapsychology were being prepared, and the growth of favourable attitudes towards ESP research as evidenced by the Warner and Clark (1938) survey. They did not believe that parapsychology had been accepted widely amongst psychologists, however. Pratt et al. were well aware of the problems that still faced the field and its workers in terms of acceptance in the normal social environment of academia.²¹⁷

The Appendices and Other Back Matter

The back matter of *ESP-60* included a set of twenty-one appendices, a glossary, reference list, and an index. Seventeen appendices were devoted to detailed information concerning various useful statistical techniques. Formulae and tables for evaluation of significance were included in many of these. Interestingly, amongst the statistical

²¹⁷ In producing their lists, however, Pratt et al. did not provide names of the universities at which experimental parapsychology research was being conducted, nor names of individual scientists involved, nor any real specifics about the claims they were making as to the social progress of the field. Whilst this would have slowed the pace of the text considerably, such information would have been invaluable to future historians of the era. Without archival research, it is impossible to tell whether or not their optimistic picture of the field in 1939 was accurate.

appendices was one which outlined the 'ESP Quotient' (pp. 419-420), a kind of early effect size. One appendix was devoted to a tabular summary of all experimental tests of ESP from 1882 through 1939, with the names of the first authors, the methods used, the number of subjects and total trials completed, expected probabilities for the target material, deviations from chance expectation, and Critical Ratios. This appendix was followed by another in which the references for each of the studies in the table were given. Another appendix provided the bibliographic sources for data in all the other tables in the text. Still another itemised all the known published criticisms, giving the name of the critic, and the counter-hypotheses they proposed, broken down by type of criticism. Raw data from the Ownbey-Zirkle 'pure telepathy' experiments were given in yet another appendix.

The Reception of *ESP-60*

Reviews of *ESP-60* appeared in the popular press (e.g., Anonymous, 1940a; Skinner, 1940), in psychological journals (e.g., Anonymous, 1940b; Anonymous, 1941; Ellson, 1940; Snyder, 1940) and in the journals of psychical research (e.g., Carrington, W., 1940; Taves, 1940). Amongst the better treatments of the book in the psychological literature was Henry J. Garrett's (1941) review in the *American Journal of Psychology*. Garrett took the time to review the methodology and arguments, making the reader aware of the advances that had been made in control and evaluation since Rhine's (1934) original monograph. Ultimately though, Garrett was not convinced that the persistence of extra-chance scoring in the experiments reviewed provided evidence of ESP. He agreed with criticisms Kellogg raised, that is, that the scoring level declined as the methodological rigour increased. The tone of the review, however, at least signalled that it was appropriate for psychologists to take the research seriously.

Rhine also received quite a lot of correspondence in response to the volume, which was, as mentioned earlier, distributed to psychologists and psychical researchers. Amongst the psychical researchers who responded was S.G. Soal, who felt that the book would become the principal textbook in the field for decades to come (Mauskopf & McVaugh, p. 297) and Sir Oliver Lodge, an elder statesman in psychical research who had been particularly important to Rhine in the early days of his interest in the field.

Lodge wrote to say that ‘the subject is now on its way to becoming respectable, treated in a handsome volume’ (p. 297).

On the Tone and Content of *ESP-60*

To what extent did the rhetorical elements of *ESP-60* contribute to what seemed to be a more positive reception amongst psychologists than *ESP* had enjoyed? In addition to conforming to a structure that seemed more scientific than literary, *ESP-60*'s tone, unlike *ESP*, was not autobiographical or conversational. As was seen in an earlier section of this chapter, the style of the prose used in *ESP-60* more closely matched that of mainstream scientific articles of the time. Language was conservative, largely devoid of personal references, and tended to highlight the authors' intention to treat their subject as ‘objectively’ as possible. For example:

It is advantageous in any exploratory work to undertake to solve the problem first in its most modest formulation and to be very clear as to what that formulation is. This is especially important in a sphere in which dispute and confusion are likely to result; for it is obvious that differences in conception of the problem will grossly affect the view of the results. (p. 15)

The six series just reviewed as inexplicable by the thirty-five counter-hypotheses probably represent a large enough body of evidence for a judgement by those who follow the procedure outlined at the beginning of the chapter as the logical one. But there will doubtless be some students who, at this juncture, experience apprehension that some possibilities may have been overlooked, and that some counter-hypotheses may not have been thought of. Whether or not this concern is logically sustained, some further comments on items of the surveyed ESP results not included in the two groups of six series dealt with above will be of interest. / From this point on, however, the discussion is not intended to represent other research reports to be discussed as being fully beyond all question, Instead, the argument runs as follows: There are several extra-chance series which are clearly subject to the bearing of one or possibly two of the listed hypotheses, but which in other considerations are adequately strong. (p. 173).²¹⁸

²¹⁸ It is safe to say that *ESP-60* used the empiricist repertoire more extensively and the contingent repertoire much more sparingly than *ESP* had done.

In the first excerpt Pratt et al. established that they wished to take a conservative approach, to ‘solve the problem first in its most modest formulation’, creating the impression that the team proceeded carefully when they did their work, not rushing to conclusions but taking the methodical steps expected of scientists.

In the second excerpt, they acknowledged that the criticisms raised by the time they completed their review, might not have, in fact, included all the possible artefacts that could, at some later point, be found to impact on the results of their exemplar experiments. Nonetheless, they made the case that they had to proceed with what they had, and that, in addition, some few other studies which came close to, but did not duplicate the rigour of the exemplars, could also be usefully included their discussion.

In each of these examples the language in which Pratt et al. couched their arguments seemed to signal that value judgements and the prose constructed to convey them had been worked through carefully by a team of researchers mindful, not only of the requirements of science, but also of the possible pitfalls and problems inherent in their temperaments, their methodology, and in the evaluation of their results. The authors of *ESP-60* were not enthusiastically describing their own experience as scientists as Rhine had done in *ESP*, but were instead presenting and interpreting the data derived from experiments in which they were interested, that they and others conducted, experiments that had evolved methodologically in response to criticism. The picture they built was not the chronological ride through personal experience that Rhine presented in *ESP* but rather a distanced depiction of the phenomena under study, in which the experiment and the data they uncovered were the central figures. Rhine was not the protagonist in *ESP-60* as he had been in *ESP*, nor were his laboratory staff and collaborators the primary cast of characters: ESP research itself was the protagonist in *ESP-60*, and if any specific group of individuals could be identified from the structure of the document as key supporting cast members, that group was composed equally of independent investigators and critics.

Whilst *ESP* had inspired a number of individuals to take up ESP research — such as Gertrude Schmeidler (1983b) — *ESP-60* became the ‘central classic of experimental parapsychology’ (Honorton, 1993, p. 195), not only inspiring individuals to conduct research but also providing a background context, an agenda and a set of

methodological and analytical tools. But how important was this document to experimental parapsychology? How had it staked its claim to lasting authority within the field? Did this rhetorical success also extend to the critical community? Was anyone outside of the community predisposed to share the conclusions reached by Rhine's group?

Although sociologists of science Harry Collins and Trevor Pinch (1982) followed Paul Allison's lead (1973) and claimed that there 'seems to be no particular reason to suppose ... that Rhine's work marked a watershed ... in the "world-view"' of parapsychologists, a number of other writers have focused on the sea change that resulted from the Duke Parapsychology Laboratory's research program. These individuals have argued, and rightly in my estimation, that the Duke Parapsychology Laboratory and its publications — especially *ESP-60* — had a profound impact on the structure and methods of experimental parapsychology (e.g., Beloff, 1993; Mauskopf & McVaugh, 1980; Nilsson, 1975; Nilsson, 1976) for decades after its publication and in a variety of countries.

Although it can be argued that a great deal of experimental research had been done on extrasensory perception in older studies of 'thought-transference', 'cryptesthesia' and 'telepathy' (Amadou, 1954; Beloff, 1993; Inglis, 1984; Mauskopf & McVaugh, 1980; Moore, 1977), Rhine's reductionism and almost scientific attitude towards the development of appropriate methodologies produced a very narrowed and specific research program that dominated Anglo-American experimental parapsychology, at least until the late 1960s (Mauskopf & McVaugh, 1982; Schmeidler, 1982). As its canonical text, *ESP-60* provided both the rhetorical justification for narrowing the parapsychology research programme as well as the outline of tools and terms necessary to carry out that programme. Besides a greater conformance to scientific style and structure, besides a self-presentation that conformed to the consensually-sanctioned depiction of scientific methodology that included the distancing of the experimenter from the phenomena, how was the persuasive power of *ESP* established?

In the first chapter of Part I, Rhine's team adopted a structure of argument that had long had been favoured amongst proponents of parapsychology and psychical research: the justification from antiquity of the persistent presence of seemingly

paranormal phenomena. Just as Hyslop had claimed in 1907 that there was ‘psychic research in the method of New Testament’ (Hyslop, 1907, p. 477), just as Richet had surveyed reports of similar phenomena in texts from antiquity to Mesmer as an introduction to his general treatise on ‘metapsychics’ (Richet, 1923), and just as I provided a list of more or less systematic treatments of the phenomena that predated the establishment of the Society for Psychical Research earlier in this thesis, Rhine and his colleagues set the context for *ESP-60* by claiming that examples of seemingly psychic phenomena could be found in Herodotus, Plato, ancient Hindu and Islamic texts, and in both the Old and New Testaments of the Christian Bible (Pratt et al., 1940, pp. 4-7.)

Unlike their predecessors, however, the *ESP-60* team also compiled a list — more brief and thus less completely described to be sure — of the equally ancient tradition of scepticism which dated back to Croesus of Persia and to Aristotle (pp. 7-8). Similarly, just as Mary Austin, in her talk before the Clark University symposium that resulted in Murchison’s (1927) *The Case for and Against Psychical Belief*, preceded her arguments for the importance the survival question with a description of experiences indicative of a belief in the afterlife from ‘amongst the least and the most intellectual tribes’ (Austin, 1927, p. 118), so did Rhine and his colleagues list ethnographic and anthropological evidence for extrasensory perception. Unlike some of their predecessors, however, they took care to note that often ‘these tales have been freely discounted’ (Pratt et al., 1940, p. 9) and when appropriate — such as in the discussion of dowsing — noted when a phenomena had provoked a ‘great deal of controversy’ (p. 10) amongst scientists.

What begins in the introductory chapter is a kind of dialectic that pervades the book: that is, whilst the *ESP-60* team provided evidence for extra-sensory perception from their own research and that of others, they were also careful to delineate the weaknesses of that same evidence, adding authority to their text by adopting a style that implied impartiality and the willingness to be self-critical. Similarly, the section headings set up an expectation of progress towards ‘disentangl[ing] ... real problems from a welter of claims ...’ as well as for ‘securing an impartial investigation of a field long regarded as superstition ...’ (p. 3). We, as readers, move from ‘ESP in Pre-Scientific Systems of Thought and Practice’ (the section heading on page 4) to ‘Incidental Appearance of ESP in Scientific Fields’ (the section heading on page 8) to

‘Direct, but not Experimental, Approaches to ESP’ (the section heading on page 11 in which the mediumship and fieldwork studies of the Society for Psychical Research are discussed) and so on.

Not only does the rhetoric they use appeal to the antiquity of both the phenomena and its investigation, but it portrays extrasensory perception as a phenomena long noted by other scientists, in other disciplines as well — a broadening of the pool of potential witnesses to the ‘reality’ of ESP as well as to its appropriateness as a topic of scientific study. The third section heading then hints at what will become a stronger argument for the progression of methodologies in parapsychology, a justification for Rhine’s team to position their own work as more modern and more systematic methodologically and thus more scientific.

The *ESP-60* team’s approach to field work in parapsychology in the third section differs from previous characterisations of the SPR’s spontaneous case work as the application of the best scientific methods to a persistent and seemingly inexplicable phenomena (e.g., Dreisch, 1933).²¹⁹ Rather than cast the work of the SPR as an exemplar of scientific investigation — of which the field work on spontaneous cases was an essential element — Rhine and his colleagues praised the work but took care to underscore the fact that the ‘founders’ understood quite well the limitations of their own methodology:

Gurney, Myers, and Podmore, of the Society for Psychical Research ... collected and reported 702 cases ... [but] these writers realized the difficulties and sources of error of conclusions from such evidence; ... errors of observation by the reporter of the case, errors of narration due to a natural tendency to unify an account, errors of memory, and the general unreliability of individual testimony (Pratt et al., 1940, p. 11)

The discussion of this research stressed the equivocal nature of results obtained even though the best methods of field research had been used. Such methodology, Rhine’s team argued, could yield no ‘crucial test’ (p. 13). The way was thus paved for

²¹⁹ The debate over how ‘scientific’ field work is as opposed to experimental work is still raging. Some recent historians (Beloff, 1977; Gauld, 1968) would agree with Driesch’s characterisations of the SPR work whilst others see spontaneous cases research as Rhine’s team did: a mere stop on the road to experimental parapsychology (e.g., Thouless, 1972).

the justification of the superior nature of experimental findings and, in the discussion of the SPR's early experimental work, Rhine's team added a new element — the positioning of experimental work within the academy. Whilst they praised the 'high intellectual and moral calibre of the distinguished founders of the S.P.R.' (p. 13) and '[t]he fortunate combination of able scholarship and of social and professional eminence in the early S.P.R.' (p. 13), Pratt et al. noted that the SPR's work took place largely outside the academy, that is, in the context of a scholarly society rather than in a university laboratory.

It is not trivial to note that Rhine's team were the key staff members of the Parapsychology Laboratory of the Department of Psychology at Duke University. Pratt et al. argued that the university was the only appropriate environment for scientific research. The text that followed situated the Duke group squarely in the only position from which credible research could be done. This positioning was evident in their evocation of the first chairperson of the Psychology Department at Duke, the psychologist William McDougall, under whose sponsorship and protection both the Laboratory and the *Journal* had been established (Mauskopf & McVaugh, 1980). That parapsychology belonged in a university setting had been McDougall's (1927) sincere belief and the authors of *ESP-60* promised to 'assemble and appraise the experimental work of the field ... [so that] experimentation in extra-sensory perception will continue to warrant the extensive university attention it has received in recent years' (p. 14).²²⁰

The subtext of the first chapter, then, locates experimental work on extra-sensory perception in an historical context as a persistent and hitherto inadequately-explained human experience, as well as justifies the inclusion of the topic as an area of legitimate intellectual, academic, and scientific study. The methodology of experimentation is identified as a progressive, modern methodology more able to deal with the problems of extra-sensory perception. Similarly, the university laboratory is identified as the appropriate setting for such experimentation. The arguments offered and

²²⁰ In 1964, Rhine, with the help of private donors, founded a private institute outside of the university and parapsychology left the Duke campus. Whether Rhine had changed his mind about the importance of being in a university context, or if it was simply that he was not ready to accept his looming mandatory retirement as some of the long-time laboratory employees speculated (Faye David, 1983, Personal communication; Dorothy Pope, 1983, Personal communication), is question for future research.

the examples given by the *ESP-60* team served to underscore the authority of the Parapsychology Laboratory under J. B. Rhine as well as to enhance the credibility of Rhine's and his colleagues' claims to 'objectivity' and 'fair-mindedness', both of which were necessary to position the text that followed as disinterested in the Mertonian sense, a text that occupied a mid-point between the old-time proponents and modern critics.

The Restatement of the Problem

In the remaining sections of Chapter 1 and in Chapters 2 and 3, the *ESP-60* team recast extrasensory perception from an unpredictable, variable phenomenon of the natural world to a simple, experimentally-testable hypothesis:

Is it possible repeatedly to obtain results that are statistically significant when subjects are tested for knowledge of (or reaction to) external stimuli (unknown and uninferable to the subject) under conditions that safely exclude the recognized sensory processes? (p. 15)

Rhine and his colleagues believed that experimental and statistical techniques could be developed and refined to the extent that it would be possible to obtain adequate evidence not only to settle the question but to probe the conditions (psychological and experimental) under which ESP manifested. Very specific statistical problems and experimental issues were outlined and resolved in Chapters 2 and 3. These ranged from identifying and recruiting gifted subjects, to which materials, techniques and laboratory environments were the most appropriate.

Chapter 3 also contained the compilation and analysis of all preceding ESP experiments. In modern terms, this section would be seen as a 'meta-analysis', that is, an aggregation of data across a series of studies of which the intent was not only to make some estimate of overall significance, but also to examine various aspects of the studies conducted, including such social variables as the prominence of experimenters, the identity of laboratories, methodological choices, and the like. In meta-analyses such variables are then examined for their relationship to the individual statistical outcomes and to the aggregate significance across the series of studies (e.g., Glass, McGaw & Smith, 1981; Rosenthal, 1991). Although I do not know whether this compilation of data

in *ESP-60* is the first ‘meta-analysis’ in psychology as some have claimed (Boesch, 2004), but it was certainly the first time in experimental parapsychology that such an aggregation of results was attempted, and in which such social variables as ‘total work and professional status of the experimenters’ (Pratt et al., 1940, p. 81) were considered.

The rhetorical impact of the detailed discussions and tabular presentation of all preceding data in Chapters 2 and 3 — and most especially Table 8 in Chapter 3 in which a subset of studies were reorganised and analysed according to their sensitivity to methodological errors and sensory cueing — was, again, to underscore the authority and credibility of Rhine and his team. Not only were their statistical and experimental procedures closely and clearly argued, but they presented what appeared to be ‘all’ the evidence to support their point of view. In addition, their analysis appeared to take into account any normal explanation or methodological failing that might have impacted on the data. In this way ‘exhaustive’ and ‘systematic’ could be added to the list of characteristics that they claimed for themselves.²²¹

Chapters 4 and 5 listed the counter-hypotheses ‘with which ... [the Rhine team were] familiar’ (p. 110). These were divided into counter-hypotheses related to: an understanding of ‘chance’ such as ‘Wrong measures are used to determine the probability of the results occurring by chance’ (p. 110); to ‘selection’ such as ‘There has been sufficient selection of subjects for participation in the tests (that is, dropping the low scorers and continuing with those making good averages) to provide the deviations obtained’ (p. 111); to subject behaviour such as ‘By keeping track of his calls in a given run, the subject can gain sufficient advantage in the later calls to give the extra-chance results obtained’ (p. 111); to methodological issues such as problems with the preparation of target materials (p. 112), the keeping of records (p. 112), and the exclusion of ‘sensory leakage’ to the subject (p. 113). The bulk of Chapter 4 dealt with each of these in turn, undermining the credibility of the counter-hypotheses.

²²¹ This is not to say that Rhine and his team constructed their evidential review with only an eye to its reception. As has been argued elsewhere in this chapter, Rhine’s group believed very strongly in the scientific method and in the power of science to answer all questions, provided the work was done systematically.

In Chapter 5, the *ESP-60* team described six series of studies in minute detail with diagrams and photographs of experimental conditions. The procedural descriptions of these studies were cross-referenced to the counter-hypotheses reviewed in Chapter 4, as applicable. Four of these studies had been conducted at the Parapsychology Laboratory at Duke. Two others, the Murphy and Taves series (1939) and the Reiss series (1937, 1939), had been independently conducted elsewhere. Although the *ESP-60* team concluded its review with the claim that '[t]he six series just reviewed as inexplicable by thirty-five counter-hypotheses probably represent a large enough body of evidence for a judgement' (Pratt et al., 1940, p. 173), further, more brief discussions of other studies considered to be of sufficient quality were also presented.

In the first reference to a critic by name — John L. Kennedy, Coover's successor at Stanford — the *ESP-60* team described 'three positions ... [that it took] with regard to this class of work' (p. 173). The first of these positions, which they attributed to Kennedy, was the rejection of the notion that if any error could have occurred, it must be assumed that it *had* occurred. The second position accepted the notion that if the likelihood of a potentially confounding counter-hypothesis amounted only to 'a mere possibility' (p. 174) in the light of other confirmatory series which had been categorised as beyond such criticism, then it was reasonable to accept such a study as providing evidence for ESP. The third position diminished the direct importance of the second series of studies by characterising the discussion as 'serving only a secondary purpose ... that of narrowing the issues and facilitating judgement' (p. 174). Thus it was possible to review the seven other studies as potentially evidential. Some of these were conducted by Rhine and his colleagues, and others were conducted by researchers at other universities (i.e., Martin & Stribic, 1938a, 1938b). Again, the rhetorical force of the chapter was to underscore the objectivity of the *ESP-60* team by making the case that they were able both to deal with counter-hypotheses and to offer criticism of confirmatory research conducted both by themselves and by independent colleagues.

This careful construction of a scientific story that included a favourable explication of both the motives and competencies of the Rhine team could not have been unself-conscious. Rather, as was the case with much of Rhine's other work, there was a specific rhetorical and instructional purpose behind the text. In addition to providing an

example of careful, measured, detached prose — that is, in keeping with the norms of objectivity and disinterestedness in science and thus adding credibility to the claim that experimental parapsychology was a legitimate scientific discipline — *ESP-60* also provided, albeit by example, a set of logical and procedural guidelines for students and other researchers. The *ESP-60* team would return to this instructive purpose in Parts III and IV and in the back matter of the volume, after they had completed the review of the commentary of their critics. Rhine's group, in building the elements of *ESP-60*, were clearly aiming at a text that would not only bring closure to the controversy but become an importance reference work for future students and researchers. Did they accomplish their goal?

Closure and Persuasion: After *ESP-60*

In order to understand the impact of *ESP-60* and to assess the usefulness of a rhetorical examination of the ESP controversy, it is necessary to briefly review the content of some of the reviews the book received.

Of the reviews that appeared in 1940 (Anonymous, 1940a-b; Carrington, 1940; Ellson, 1940; Moulton, 1940; Skinner, 1940; Snyder, 1940; Taves, 1940), I will focus only on two as examples of the range of critical reviews. The first (Skinner, 1940), appeared in the *Saturday Review of Books* on July 20, 1940. Skinner's brief review is worth quoting in full:

This book is not, as its title implies, a balanced account of the status of mental telepathy and clairvoyance today. For the most part Professor Rhine and his colleagues are concerned with summarizing and evaluating the criticisms which have been leveled against the Duke University experiments. In spite of much obviously earnest effort, the case for extra sensory [sic] perception is by no means clinched. In general, one may question the value of any review of early work at this time. If extra sensory perception [sic] is as readily available for study as the authors contend, then little is to be gained from quibbling over experiments performed several years ago under conditions which have not satisfied many qualified observers. The requirements of a crucial series of experiments (with respect of design, control, manner of recording, and method of analysis) are not fairly well agreed upon. Further work, which is apparently in progress, will be more to the point than historical research on the validity of part [sic] manifestations. The

authors only weaken their case by making so much of the early evidence. (p. 21).

Skinner's comments underscored the continued concern amongst critics that the quality of ESP research methodology was inadequate and that other types of evidence offered from the psychological research literature was not considered relevant to the debate. For such critics as Skinner, the 'reality' of extrasensory perception could only rest on laboratory experiments of sufficient methodological rigour, and because Rhine's team had not as yet, these critics believed, conducted such experiments, the discussion of old research was pointless. For Pratt et al., on the other hand, the Duke experiments and the results obtained by other ESP researchers, arose out of a context of anecdotal and pre-Duke experimentation, all of which pointed towards, if not confirmed the 'reality' of the phenomena under study. Whilst presentation of the movement towards a tighter methodological standard was clearly a goal of the volume, the preparation of future generations of researchers was, perhaps, a more important goal for Rhine's team.²²²

Ellson's (1940) more thorough and detailed review, whilst still dismissive of the 'reality' of the phenomena, was an example of the more scholarly treatment of *ESP-60*, in that it raised specific criticisms, some substantive and some rhetorical. Published in *Psychological Bulletin*, Ellson's major points were: (1) whilst the text showed improvement over *ESP* in the attempt to provide an exhaustive and conservative treatment of the results to that point, it was still 'extremely difficult, if not impossible, to reconstruct a single ESP experiment from the sketchy description of methods' (p. 823); (2) that even though five chapters had been devoted to summarising and evaluating the criticism to date, Ellson found the tone of the rest of the volume to be 'definitely uncritical' (p. 823) given that the criticisms that had been raised, were, in Ellson's opinion 'too easily dismissed' (p. 824); (3) there were, in Ellson's estimation, some 'significant omissions' such as the lack of a full discussion of experiments that

²²² The structure of the volume attests to this fact in that more pages are devoted to what future researchers would need than towards counteracting the criticisms that had been raised, e.g., the historical background of the problem, the comprehensive review of results to that point, the review of criticisms with an eye towards underscoring which methodological conditions were essential to producing an experiment that might be considered crucial, and the statistical methods and bibliographic references necessary to arm the future researchers with tools which to do the work.

supported the ESP hypothesis but which suffered from inadequate control, nor had there been a detailed comparison of well-controlled and badly-controlled experiments; and (4) the entire section discussing what could be made of the nature of ESP given the quality and strength of the results obtained was, in Ellson's opinion, unwarranted.

In the section in which Ellson reviewed Pratt et al.'s summary and response to the counter-hypotheses, Ellson argued that the authors had reviewed specific criticism raised about specific sets of experiments as if they were general criticisms raised about the entire database. By adopting this strategy, Ellson felt that Pratt et al. had made their job that much easier: once the criticisms were recast as general, it was simple enough to find individual experiments that were not subject to the criticisms raised and thus demolish the criticism. For Ellson, the purpose of the counter-hypotheses section was to set aside all criticism 'as irrelevant and [as having] ... no logical value as a refutation' (p. 824). Finally, Ellson rejected the idea that *ESP-60* might be used as a reference work for future researchers because of what he saw as the inadequate description of the methodology of any single experiment, especially of the crucial ones. The last sentence of the review raised the question of the volume's ability to function as a work of propaganda, but demurred from offering more complete comments on that point.

Conclusion

Over the course of this chapter, I have brought some of the tools of the rhetoric of science to two documents representative of the ESP controversy that raged from 1934 to 1944, Rhine's (1934) *Extra-Sensory Perception* and Pratt et al.'s (1940) *Extrasensory Perception after Sixty Years*. I have tried to show that rhetorical choices made by the authors of these two books had an impact on both the reception of their work amongst the wider scientific community, and on the utility of their research for those predisposed to take the notion of ESP seriously. In *ESP*, Rhine's style was not in accordance with existing conventions of science writing in its tone or structure, and there were consequences that undermined his stated goal of establishing experimental parapsychology as a scientific discipline in the American academy.

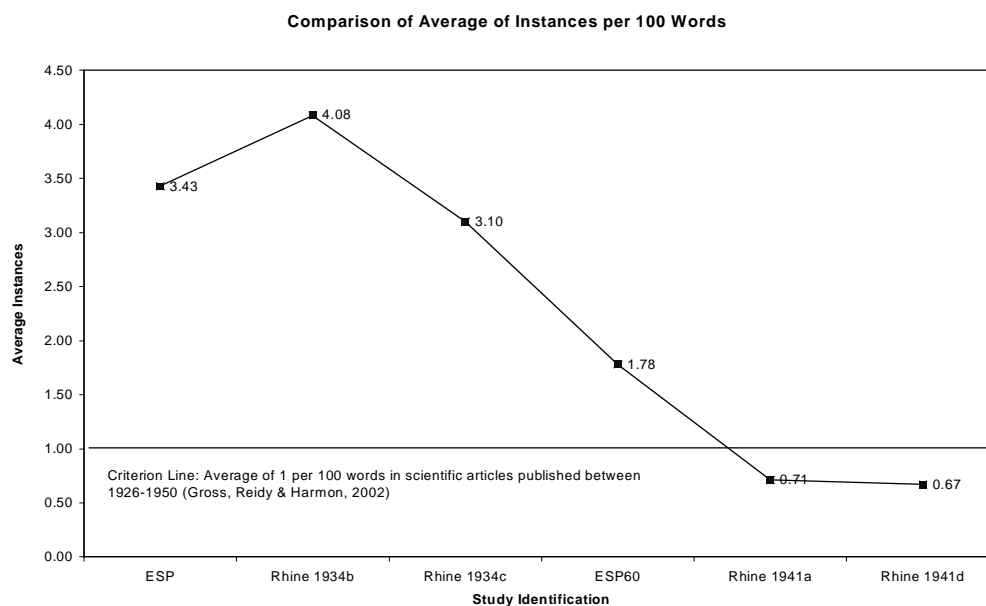
In terms of the rhetorical choices Rhine made, there was a gradual change towards more conventionally scientific prose over the period in his own writing. For

example, Figure 1 provides a graphical representation of the movement of Rhine's rhetorical style towards the scientific norm that Gross, Harmon and Reidy (2002) identified as characteristic of mid-20th-century scientific prose. As the reader will remember, using personal names and pronouns as an indicator, Gross, Harmon and Reidy found that scientific articles contained an average of one instance of such usages per 100 words. Rhine's prose in *ESP* included over three times as many instances of personal names and pronouns as Gross, Harmon and Reidy had found, as did the articles Rhine published in 1934. A similar analysis of the prose in *ESP-60*, which was largely written by Gaither Pratt showed greater conformance to scientific norms in that instances of personal pronouns and names had dropped to an average of 1.78 per 100 words. But Rhine's own personal scientific reports published in 1941 also showed a precipitous drop to 0.71 and 0.67 respectively, bringing the articles — at least in terms of this particular indicator — into conformance with scientific prose of the time.

Whether Rhine's shift to a more 'distanced' approach (e.g., Montgomery, 1996) to his narrative was a result of the reaction to the style of *ESP* is a matter for conjecture. But there is evidence that as he and his team engaged in the controversy, there was a decided movement towards a form of discourse that could more effectively convey their findings as scientific 'facts'.

Figure 1.

Comparison of Personal Pronouns and Proper Names in a Sample of Texts in which J.B. Rhine was a First or Second Author, from 1934 to 1941.



As for the understanding of certain points raised: a great deal of incommensurability was apparent in the exchanges. It is fair to say that Rhine resented the imposition of criticism and methodological constraints, even though he took pains to invite such critical scrutiny from the wider scientific world. It is also fair to say that many of the substantive points raised — from statistical issues to methodological ones — were lost on him. There is some evidence that he was careless in reporting his own research, in that some reviewers claimed his descriptions in his popular works contradicted the scientific reports he published. There were points in the controversy in which it appeared that Stuart and Pratt were better equipped to understand the concerns of the critics than Rhine was. But there were also points at which it was obvious that Rhine's group felt they had answered the criticisms raised in the appropriate way and yet criticisms that were no longer applicable were still being raised as condemnatory of their whole enterprise. Frustration was apparent in the exchanges towards the end of the period at the lack of closure, and yet, paradoxically Rhine's team was declaring the controversy settled in their favour. What Rhine's team saw as a problem well on its way to being solved, was to many critics an unfinished task at best, and an impossible task at worst.

One might speculate that some of the incommensurability came from Rhine's imperfect understanding of and/or often hostile attitude towards psychology. Even if historical research such as Mauskopf and McVaugh's treatment of the early years of parapsychology had not uncovered evidence that Rhine's identification with psychology was an uneasy one at best, the rhetorical choices he made — whether in how he characterised his controversial findings, how he tried to counteract the reactions he expected from psychologists, or in his citation practices — could still reasonably be interpreted as signalling a hostility towards the field in which he professed to work. This orientation to psychology may not only have had an impact on the substantive aspects of his research, but on the barriers to acceptance ESP research faced amongst psychologists.

The factors that are involved in the shape and course of controversy in science are complex.²²³ I think I have shown that it is possible to draw some insights into what makes communication across a controversy-boundary possible from the structure of the documents, choices in construction and tone, and conformance to wider standards of scientific writing. It is certainly true that the ESP controversy was saddled with a wide variety of issues complicating acceptance. These were plain in the hostile reactions of such critics as Rogosin (1938a, 1938b, 1938c) who were infuriated by the very existence of the field, to the consequences of the denial of authority of the psychometricians by the mathematicians and vice versa (e.g., amongst the psychometricians, Kellogg, 1937a, 1937b, 1938; amongst the mathematicians, Greenwood, 1939; Greville, 1939; Stuart & Greenwood, 1938), to the inability of critics to give credence to the context out of which laboratory testing of ESP arose, to the unwillingness of some critics to evaluate research until studies were published that were absolutely perfect, and so on.

But just as historical research can only illuminate some contours of the landscape of a controversy, rhetorical analyses only illuminate others. Historical research may give one a glimpse of the camps, locating them on socio-political or cognitive maps, but rhetorical research gives one only a glimpse of the methods by which the camps attempt to communicate, of how they attempt to establish their relationships to one another. Other, perhaps more personal meanings are only hinted at, seen from the outside, dependent on a reading that takes place at a level that is external to the individuals involved and to the language they use, and shot through with the interpretational context the analyst brings to the task.

In the chapters that follow, I will turn towards another methodology to examine whether deeper insights about the contours of controversy might be gleaned from the text and talk produced by scientists.

²²³ A good study of what some have called the ‘deliberative character of strategic ... debates’ is available in Czubaroff (1989).

CHAPTER SIX

TAKING A TURN TOWARDS SELF

In this chapter, I will take a turn towards the ‘self’ that produces the talk and text that discourse analysts examine, as a prelude to the case study in Chapter 7. Unlike historical analysis which takes a long view of the terrain, and unlike rhetorical analysis which takes an approach that is still somewhat impersonal, discourse analysis — even the text-based version of it that I will employ — has the potential of moving into the exchanges themselves, to illuminate a terrain that is both more personal and ‘in action’.

Before I survey the analytic traditions that fed into discourse analysis, it is important to review how I have dealt with discourse so far. In Chapter 3, I used a organisational frame I borrowed from Gilbert and Mulkay (1984) who postulated that scientists used two sets of rhetorical strategies, the empiricist and the contingent.²²⁴ The empiricist repertoire has been defined as ‘an integrated vocabulary of terms, explanatory moves and metaphors’ (Potter, 1996, p. 174) by which science practice and scientific ‘fact’ may be described. In the ‘empiricist repertoire’ the depiction of ‘nature’ that results is externalised, distant from the speaker. Those who use it claim to be data-driven, to be responding to something objective and ‘true’, something ‘out there’,²²⁵ unconstructed by the scientific ‘story’ being told, writing and talking as if their own assumptions, observations, and interpretations unproblematically mirrored the external world and were thus ‘truth’. In science, however, for every ‘truth’ that exists, there are opposing ‘truths’. To account for the ‘error’ of the other, Gilbert and Mulkay described the ‘contingent’ repertoire as one that relied on the attribution of personal motivations, and competencies, so as to explain away the ‘truths’ offered by one’s opponents. If, in the simplistic normative world that traditional sociology of science depicted, scientists were disinterested, ‘objective’ observers of ‘nature’ at large, then attributing contingent factors to the claims of one’s opponents solved what Potter (1996) called ‘the dilemma

²²⁴ Mulkay, Potter & Yearley (1983) note that the idea of interpretative repertoires in science text and talk were essentially what Halliday’s termed ‘linguistic registers’ (p. 197, from Halliday, 1978).

²²⁵ For an extended discussion of Gilbert and Mulkay’s (1985) contribution to the understanding of how “out-there-ness” is established in scientific discourse, see Edwards (1996, p. 150).

of stake'. A 'true' scientist would not allow her personal interests to influence her science practise, and thus the 'empiricist repertoire' could be 'deployed for truth' by casting the text in an impersonal light and the claims of one's opponents could, on the other hand, be set aside by appealing to the 'contingent, constructive account reserved for doubt and error' (Edwards & Potter, 1992, p. 70). That is, the 'mistakes' made by one's opponent could only have arisen from social, psychological, and political factors that compromised her 'objectivity'.

In my analysis I sorted the writings of reviewers of criticism by using these repertoires as global categories. I found thirteen reviewers from René Sudre (1926) to Marcello Truzzi (1998) who accounted for the 'errors' of the critics they reviewed by claiming that these individuals were compromised by: their motivations and beliefs; an unquestioning adherence to the materialistic worldview; a failure to separate their critical, intellectual selves from their emotional selves; a willingness to distort the research reports they criticised through inappropriate rhetorical devices; or simple incompetence.

I found three reviewers from Coover (1927) to Honorton (1976) who used the empiricist repertoire when they reviewed critics, noting that such individuals had evaluated the 'facts' and 'methods' of parapsychology and found them wanting, but that the conclusions the critics reached were at least partly 'true'.

Six reviewers, from Stevenson and Roll (1966) to Honorton (1993), used a mix of these repertoires, dealing with critics' evidential assessments and the rhetorical strategies they employed to describe or interpret these assessments. Whilst these reviewers characterised the critics they reviewed as, at times, influenced by extra-scientific beliefs, arguments were also found in their writings with which reviewers could deal empirically, as substance, as 'fact'.

Reviewers who accused critics of being compromised by contingent factors tended to dismiss specific criticisms raised as 'illegitimate'. Reviewers who evaluated critics as having focused on substantive issues met those criticisms in a similar vein.. In Ransom's (1971) review, for example, when a critic he surveyed used the contingent repertoire to account for parapsychologists' 'errors', Ransom also relied on contingent factors to explain away the critic's points. On the other hand, when the critic focused on

empirical disagreements, such as Hansel's (1966) claim that 'successful experiments [in parapsychology] were not repeatable', Ransom replied in kind. Such a view, he explained, was the consequence of an over-estimation of the lack of replicability in the field as well as a gloss over the various 'reasonable' causes for replicative failure. But when Rawcliffe (1952) claimed that parapsychologists are too biased to do parapsychological research because they believed in the phenomena they studied, Ransom shifted to a discussion of bias in science, making the point that mainstream scientists and perhaps Rawcliffe himself were equally compromised by personal bias.

The materials I reviewed seemed to show a relationship between the repertoire in which a criticism was couched and the repertoire used to counter the criticism. I therefore suspected that scientific prose could not always be categorised as simply as empiricist or contingent. Since Gilbert and Mulkay's system was published, a number of writers have either criticised or moved beyond it analytically. For example, Edwards (1996) argued it was perhaps more useful that repertoires were seen as

... having discrete uses with respect to practices of fact construction involving warranting and accountability ... [for example] the empiricist repertoire [could be] ... considered as a set of resources that may be drawn on when externalising facts by *divesting* agency from fact constructors and *investing* it in facts. (p. 158, italics in the original)

That Gilbert and Mulkay, and like them, McKinlay and Potter, would take their analytical schema to scientific documents was to be appreciated, however. As Potter and Wetherell (1987) noted, because science writing is '... an abstract, technical and precise realm', uncovering discursive processes in such a 'rarefied environment' assures the analyst that such processes can be found in less formal text. Because scientific texts were rule-bound, science writing could be thought of as '...a useful hard case where discourse analysis can hone its claims' (p. 64).

Further, not only did Gilbert and Mulkay introduce the notion of interpretative repertoires, their work provided a glimpse at '... participants' own understandings of what was involved in scientific work' (Eddy, 2001, p. 197). Interpretative repertoires were not mere 'discourse *per se*' but were:

... broadly discernible clusters of terms, descriptions and figures of

speech often assembled around metaphors or vivid images[,] pre-eminently a way of understanding the *content* of discourse and how that content is organised ... (Wetherell & Potter, 1992, pp. 90-91)

In this chapter I will review the analytical traditions that have shaped discourse analysis in science studies in general, providing a glimpse into some of the concerns of contemporary DA, especially as it has developed within psychology. The ‘self’ to which I turn in this chapter is the scientist who engages in talk or produces text that is constitutive of ‘fact’, doing so in such a way as to reinforce their own authority or credibility as legitimate ‘discoverers’ or ‘conveyers’ of scientific knowledge.

Discourse Analysis

On The Traditions of Discourse Analysis

Edwards and Potter

Edwards and Potter (1992) listed five traditions they believed contributed to the shape of discourse analysis: (1) the sociology of knowledge; (2) linguistic philosophy; (3) semiotics; (4) speech act theory; and (5) ethnomethodology (p. 27).

Ashmore’s (1989) work on reflexivity, Gilbert and Mulkay’s (1985) study of scientific text and talk, and Potter and Mulkay’s (1985) examination of interpretative repertoires in interviews with scientists were listed as examples of studies of discourse that arose within the context of the sociology of knowledge.²²⁶

Linguistic philosophy, on the other hand, was seen as a domain in which ‘problems of knowledge had been reworked as problems of language’ (Edwards & Potter, 1992, p. 27). The work that best exemplified this tradition was that done by such philosophers as John L. Austin (1911-1960) (e.g., 1962) and Ludwig Wittgenstein (1889-1951) (e.g., 1953). Potter (2001) wrote that Wittgenstein’s work was transformative in

²²⁶ Specific examples of this type of discourse analysis but as applied to problems of social psychology can be found in Potter (e.g., 1984, 1988) and in Potter and Wetherell (e.g., 1987).

that it first proposed the notion of ‘language as a toolkit’.²²⁷ For Wittgenstein, language was not as an ‘abstract system’ of symbols, but rather a means to accomplish a variety of practical and epistemic tasks, shaped socially, and carrying identity, meaning, and ‘thought’ (pp. 41-42). Austin shifted the focus to ‘speech acts’ (Potter, 1996, p. 45), a notion that was carried forward by his students, such as John Searle (1969).²²⁸

Amongst the exemplars of semiotics — a post-structuralist, post-modern approach that both drew from, and contributed to, literary criticism — were works by Jacques Derrida (1930-2004) (e.g., 1973), and Michael Shapiro (1988). Derrida focused on the socially-constructed nature of text, and Shapiro argued that social reality was an emergent property of text.

Ashmore, Myers and Potter

In a review of ‘Discourse, Rhetoric and Reflexivity’ in the *Handbook of Science and Technology*, Ashmore, Myers and Potter (1995) provided an ironic and reflexive picture of the context in which discourse analysis operates. They foregrounded their own analytic place in the narrative by presenting the review as a diary written by a female post-graduate who was considering changing her thesis project from biology/zoology to science studies. Their sense of the traditions out of which discourse analysis developed and with which DA shared interpretative space in science studies was presented by two devices: the contents of the ‘Canonical Footnote’ their mythical post-grad alter ego

²²⁷ I am sceptical about the claim that these ideas of mind, self and society emerging from linguistic communication were ‘new’ at the time they were proposed because they seem to me to mirror closely the work of turn-of-the-last-century pragmatic philosopher George Herbert Mead (1863-1931) who made the same points, although for him ‘linguistic communication’ was ‘social communication’. I have frankly found it surprising that Mead’s work is virtually ignored in the discourse analysis literature, except the occasional mention in lists of philosophers whose work was published only in compilations by their students (e.g., Potter & Wetherell, 1987, p. 81).

²²⁸ The shift from structure as it is dealt with in rhetorical analysis to function as it is dealt with in discourse analysis should be apparent. That is, rhetorical analysis examines the structure of a document, the tropes and devices that construct text or talk. It seeks to understand how that structure influences not only the content as it exists but the social and cognitive interpretations that it inspires. Discourse analysis zeros in on the discourse itself, on what it ‘does’, how it functions, argumentatively, socially, politically, globally, locally (Edwards & Potter, 1992, p. 27).

claimed to have uncovered, and the topics represented by the sections of the library she visited over a period of seven days. The ‘Canonical Footnote’ provided the organisational structure for her browse through related literatures:

²⁶See, for instance, Medawar (1964); Gusfield (1976); Woolgar (1976, 1980); Latour and Woolgar (1979); Knorr-Cetina (1981); Yearley (1981); Law and Williams (1982); Mulkay, Potter, and Yearley (Eds.) (1983); Gilbert & Mulkay (1984); Latour (1987); Lynch (1985a); Mulkay (1985); Shapin and Schaffer (1985); Potter and Wetherell (1987); Bazerman (1988); Ashmore (1989); (Myers, 1990).²²⁹

In preparing to write the thesis, I not only moved through this review, but I found myself experiencing some of the same affinities/confusions to which the mythical post-grad attested in her narrative. As I began to write this specific chapter, I also checked the books that constituted my core set of readings in discourse analysis to see whether or not the items listed in the ‘Canonical Footnote’ held such a central place.

The Canonical Footnote

Peter Medawar’s 1964 article ‘Is the Scientific Paper a Fraud’ represented the first time a scientist admitted in public or in print that research report structure did not present experiments as they happened but rather repackaged them in a stylised form emphasising rationality, objectivity and an orderly progress from hypothesis to conclusion.²³⁰ Medawar was particularly candid in his description:

Just consider for a moment the traditional form of a scientific paper ... [It] is something like this. First, there is a section called the “introduction” in which you merely describe the general field in which your scientific talents are going to be exercised, followed by a section called “previous work” in which you concede, more or less graciously,

²²⁹ The narrator (to maintain the constructed story-teller in the review) cited the following as the footnotes that inspired her to make this list: Gilbert and Mulkay (1984, p. 194, n. 27); Lynch (1985, p. 17, n. 2); and just to make the reflexivity loop that more pronounced, Ashmore, Myers and Potter (1995, n.1) which is, in fact, the footnote in the chapter under review in which ‘she’ listed her sources.

²³⁰ The Nobel Laureate Sir Peter Medawar first broached the topic of his article in a BBC radio broadcast on ‘how science was really done’ (Moss, 1999, p. 193). I used a 1996 compilation of Medawar’s essays.

that others have dimly groped towards the fundamental truths that you are now about to expound. Then a section on “methods” — that is OK. Then comes the section called “results”. The section called “results” consists of a stream of factual information in which it is considered extremely bad form to discuss the significance of the results you are getting. You have to pretend that your mind is, so to speak, a virgin receptacle, an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the “discussion” section, and in the discussion you adopt the ludicrous pretence of asking yourself if the information you have collected actually means anything; of asking yourself if any general truths are going to emerge from the contemplation of all the evidence you brandished in the section called “results”. (pp. 33-34)²³¹

Although Ashmore, Myers and Potter nominated this as a ‘must-read’, only Cole (1992), a traditional sociologist of science who is a critic of discourse analysis, actually mentioned this article, characterising it merely as a predecessor to the constructivist program of drawing a distinction between ‘doing science’ and ‘writing up science’ (p. 77).

Gusfield’s (1976) article, ‘The literary rhetoric of science: Comedy and pathos in drinking driver research’, situated itself as a project in the sociology of knowledge informed by both classical rhetoric and literary criticism. Gusfield made an early case for the usefulness of recasting scientific text as a literary form, by examining agency (e.g., ‘the pattern of rejection of personal terms ... [so as to establish] a reality outside the observer’, p. 20), purpose (e.g., ‘means to persuade, but only by presenting an external world to the audience and allowing that external reality to do the persuading’, p. 20), the ‘reduction to substance’ (e.g., establishing the ‘whatness’ of the object, p. 23), and producing the feeling of science (e.g., the use of non-emotive language that evokes emotions nonetheless, p. 30), amongst other things.²³²

²³¹ As was seen in Chapter 5, in Gross, Harmon and Reidy’s (2002) and Bazerman’s (1988) discussions of the function of ‘methods’ in scientific report, this section is hardly as unproblematic as Medawar depicted it.

²³² The place of this reference in the Canonical Footnote may signal its importance to competency in discourse and rhetoric, but in many of the core texts of discourse analysis, if it appears at all, it is cited only as an example of a rhetorical analysis of a single text (e.g., Potter & Wetherell, 1987, p. 161).

The next two references in the 'Canonical Footnote', Woolgar's articles (1976, 1980) on accounts of scientific discovery, were categorised as a constructivist form of sociology of knowledge. The former focused on the use of 'discovery accounts' in the intellectual history of science and the latter reviewed the use of 'logic' and 'sequence' in such accounts. In some of the core texts of discourse analysis, these two articles were cited as foundational to the enterprise when one discussed discovery *per se* (e.g., Callon, 1995, p. 39), or the methodological choices made in a single study of a 'complex worked-over text' (e.g., Potter & Wetherell, 1987, p. 161, 187). More importantly, Woolgar's articles were situated as points of origin for the discussion of 'action description' as an 'externalising device' (Edwards & Potter, 1992, p. 91), as well as for the entire deeper enterprise of the social construction of science (e.g., Mulkay, 1985, p. 173).

Latour and Woolgar's (1979) book, *Laboratory Life*, has a complex identity: as a work of interview- and observation-based laboratory bench ethnography; and as an example of a constructivist sociology of knowledge that shifts the focus to scientists' talk whilst calling for a better understanding of the 'scientific enterprise, the quality of the knowledge it produces, and its role in transforming our lives' (Potter & Wetherell, 1987, p. 159). Although valued both as an methodological first step and as a necessary rejection of traditional Mertonian sociology of science (Potter, 1996, p. 34), this approach set up an analytic paradox that later works on reflexivity sought to solve. That is, Latour and Woolgar 'often [attempted] ... to produce a unitary, realist version of how facts are manufactured out of idiosyncratic local resources ...' (Potter, 1996, p. 37) which led to what Potter called a 'hierarchy of modalisation' in which the analyst moved along a continuum from treating the talk of the speaker as 'suspect or provisional' to the point at which the analyst presented his own interpretative exercise as 'solid and unproblematic and quite separate from the speaker' (p. 112).

The next three references in the 'Canonical Footnote' share Latour and Woolgar's complex identity as further examples of a constructivist analysis that flowed from observation-based ethnographical studies of laboratories.

Knorr-Cetina's (1981) book provided both an analysis of discovery accounts and of the social construction of 'facts' in science that, instead of 'considering scientific

products as somehow capturing *what* is ... [considered] them as selectively carried out, transformed and constructed from *whatever* is' (p. 1, my italics). For Mulkay (1985), the volume was important because it served as a reference point in the understanding of how scientists attempt 'to remove themselves from a narrative so as to privilege "experimental facts"' (p. 33), progressively hiding 'the possible contingency of factual claims about the physical world ... from view [by adopting] ... increasingly empiricist formulations of their knowledge claims' (p. 175.)

With Yearley (1981), the focus shifted in the direction of discourse analysis in that he emphasised the function of textual elements in the construction of a 'persuasive' scientific argument. For Potter (1996), Yearley's article was an example of an early analyst who 'examined the role of formulation in legal, media and scientific contexts' (p. 49), 'formulation' being understood as restating or summarising an interlocutor's argument so as to move the interaction towards some persuasive goal. As Potter noted, 'Such formulations are not neutral, abstract summaries ... but are designed as they are, in order to have specific upshots relevant to future actions' (p. 48).

Classified as belonging to the tradition of a laboratory study in the style of Latour and Woolgar and Knorr-Cetina, Law and Williams (1982) narrowed in on the notion of 'action', illustrating how scientists handled citation, 'fact' construction, and self-distancing syntax so as to 'maximize the attractiveness of ... their papers' (p. 535).

Mulkay, Potter and Yearley's (1983) book chapter continued this movement towards action by critiquing two previous analyses of discourse in science, one by a trio of authors (White, Sullivan & Barboni, 1979) who employed citation analysis to 'establish' the interaction of theory and experiment, and the other by Collins and Pinch (1979) on parapsychologists' discourse. Mulkay, Potter and Yearley criticised both sets of authors, who, whilst conducting very different kinds of sociology, had allowed themselves to adopt the interpretative repertoire of the individuals 'under' study. What was needed instead was a discourse analysis whose methodology was more 'prior', more foundational, capable of providing a 'systematic investigation of the social production of scientific discourse'. Methodology should, Mulkay, Potter and Yearley thought, provide an understanding of '... how actors socially construct their accounts of action and ... constitute the character of their actions primarily through the use of language'.

Without such a shift in analytic gaze, they believed analysts would ‘... continue to fail ... to furnish satisfactory answers to long-standing questions about the nature of action and belief in science’ (pp. 195-196).²³³

Because I have Gilbert & Mulkay (1984) elsewhere in this thesis I will not discuss it here. In addition, because I plan to use Mulkay (1985) in more detail here, I will discuss this entry in the ‘Canonical Footnote’ later in this chapter. In between these two references were Latour’s (1987) *Science in Action* and Lynch’s (1985) *Art and Artifact in Laboratory Science*, which address the question of ‘how’ scientists both *account* for their actions and *constitute* their actions through their use of language. Edwards and Potter (1992) noted that Latour introduced the notion of ‘modalities of discourse’ in *Science in Action*, that is, the methods of discourse by which the reader’s reaction is both anticipated and directed in scientific texts (p. 69). Bazerman (1998) characterised the volume as a work of ‘power semantics’ in which scientists were depicted as ‘... powerful rhetorical actors enlisting others in networks ... creat[ing] webs of relationships so strong that certain ideas, objects, facts become black-boxed and are thereafter ... taken for granted as unproblematic’ (p. 16). Knorr-Cetina (1999) also emphasised the analysis of power, interests and social forces (p. 29) in her use of *Science in Action*, foregrounding Latour’s analysis of the discourse laboratory leaders used to stabilise the relationships of their laboratories to, and within, the wider scientific world (p. 223). Wetherell and Potter (1992) lauded Latour’s (1987) book, amongst other works of the same era (e.g., Gilbert & Mulkay, 1984), as demonstrating ‘... that what is counted as true and false changes regularly, suggesting that it would be ill-advised to take any current view as definitive and timeless’ (p. 66).

Lynch’s (1985) volume, *Art and Artifact in Laboratory Science*, was an example of the type of laboratory bench ethnographies that ‘... linked an intense interest in knowledge production to the pursuits of scientists and other actors ... to scientists’ rhetoric, their power strategies, their economic moves, their laboratory decisions, their

²³³ Both Gilbert and Mulkay (1984, Mulkay, 1985) cited this article as a direct predecessor to their own work.

communication, and above all their ... interpretations and negotiations ...' (Knorr-Cetina, 1999, p. 11). How so ever these social factors were related to the production of knowledge, Lynch did make a case that 'the distinction between "reality" and "fiction"' had an epistemic impact (p. 250).²³⁴

Shapin and Schaeffer (1985) seem to have been included as an example of a history of science case study that breaks with the Mertonian model (e.g., Potter, 1996, p. 18), focusing on power and interests in the style of Latour (1987) and Lynch (1985). That is, *Leviathan and the Air Pump* (Shapin & Schaeffer, 1985) followed the growth of the experiment and the 'experimental life' in seventeenth-century science, expanding its focus to the historical surround, but analysing text so as to emphasise 'the interweaving of scientific interest with social and political factors' (Knorr-Cetina, 1999, p. 29).

The last four references in the 'Canonical Footnote' represent three traditions: discourse analysis *per se* (Potter & Wetherell, 1987); rhetoric of science (Bazerman, 1988; Myers, 1990); and the interdisciplinary stance (Ashmore, 1989).

Potter and Wetherell's study both critiqued traditional social psychology and provided a discourse analysis of identity-building and identify-deconstructing (or racist) talk in New Zealand. Berman and Parker (1993) credited Potter and Wetherell (1987) for having 'popularised discourse analysis in social psychology', turning the DA gaze towards traditional methodology and theory-making in social psychology, directly critiquing what Berman and Parker called the 'spurious model [of] ... thinking as uniform, rational, and classifiable into equal-interval categories' (p. 4).

What Potter and Wetherell refuted was the simplistic picture of human traits and states that arose both out of social psychological experimentation and pen-and-pencil surveys in which contextualised task accomplishments or responses were taken to

²³⁴ Recent discourse work (e.g., Knorr-Cetina, 1999) shifts the focus to the notion that 'scientists and other experts [are] enfolded in construction machineries, in entire conjunctions of conventions and devices that are organised, dynamic, thought about (at least partially), but not governed by single actors' (p. 11). This separates the newer work from that which has evolved in the discourse analysis of social psychology such as that done by Potter, Edwards, Wetherell and others, in which the emphasis is still on the local action of discourse.

indicate something ‘true’ and ‘timeless’ about personality and behaviour.²³⁵ Edwards and Potter (1992) noted that Potter and Wetherell (1987) questioned traditional attitude research in particular by criticising the ‘idea that talk and text can be directly mapped onto underlying cognitive representations of knowledge and reasoning’ (pp. 15-16). They also questioned traditional social psychology’s reliance on social categorisation as a given rather than as an ‘object of study’ when social categorisation was more likely a ‘contingent, historically specific, [and] ideological’ basis for the notion of ‘cultural groups’ (Wetherell & Potter, 1992, pp. 146-167).

On the methodological side, for Moir (1995), the narrowing of the focus to discourse as action in Potter and Wetherell (1987) and the presentation of text with analysis allowed the reader to really evaluate the analysis that was being made (p. 32), thus opening to scrutiny Potter and Wetherell’s own interpretative work. Taylor (2001) felt that they took this invitation to readers a step further by arguing ‘for validation through reference to the coherence and *fruitfulness* of the findings, as well as with reference to participants’ orientation and to new research problems which are raised’ (p. 321).

Because Bazerman (1988) and Myers (1990) were discussed in Chapters 4 and 5, I will only briefly recharacterise them here. Bazerman studied the rhetorical structure of scientific documents largely from the disciplinary point of view of communications and language. Myers dealt with rhetorical aspects of controversy in biology, dealing both with texts and with visual representations. Bazerman (1998) himself noted that in his 1988 study he had emphasised ‘framing devices’, illustrating them by analysing ‘texts of recognised types, appearing in certain circumstances, ... [and] perceived to have particular force’ (p. 24).²³⁶ Myers (1990), on the other hand, was seen as dealing with the

²³⁵ Marshall and Raabe (1993) also focused on Potter and Wetherell’s rejection of the idea that ‘there is some enduring entity within individuals that can be measured’ and their claim that respondents and participants exhibited ‘specific linguistic formulations ... [that were] dependent on specific contexts’ when they completed experimental tasks (p. 36).

²³⁶ Lemke (1998) and Martin (1998) situated Bazerman (1988) as both going beyond the traditional emphasis on function in rhetoric (Martin, 1998, pp. 6, 11), and as establishing ‘talking science’ as a ‘specialised linguistic register’ in which structural analyses might be done (Lemke, 1998, p. 91).

empiricist repertoire (Edwards & Potter, 1992, p. 135), and as providing an analysis of the ‘virtues and pitfalls’ of ‘visual rhetoric’ (Potter, 1996, pp. 10, 123).

Ashmore’s book (1989) critiqued previous discourse analysis of scientists and their text and talk and struggled with methods by which analysts could foreground their own ‘presence’ in the narrative. Potter (1996) credited Ashmore (1989) with providing the ‘most developed discussion’ of reflexivity, or that ‘set of issues that arise when considering the relationship between the content of research and the writing and actions of researchers’. The *tu quoque* argument, central to the principle of reflexivity, holds that constructivist analysis of the ‘empiricist and objective tropes’ of scientific text should be turned inward on the analyst’s own work, which, more often than not, presented itself in realist terms (p. 228). Ashmore’s unique contribution to this debate — and there were those in science studies who dismissed it²³⁷ — included the particular chapter under review here, which is an example of Ashmore’s ability to foreground the analyst’s presence in the mix by taking the reader on a ‘creative adventure’ (Wetherell, 2001):

... The analyst presents not the facts or an objective summary of what is there to be found, rather, she or he more playfully, and certainly self-consciously, construct a reading or interpretation ... [such that] the analyst’s account is another story to be added to the participants’ accounts and stories [the result having been] ... *narrated* into being. (p. 396, the author’s italics).

Ashmore, Myers and Potter’s (1995) alter ego, the mythical post-grad, began her week of reading with discourse analysis, and after reviewing rhetorical and sociological studies of science, visual presentations and mathematics as text, examinations of gender in science and gendered depictions of science, SSK- and DA-based examinations of social sciences, and after pondering the importance of reflexivity, she returned to discourse analysis as a viable methodology. As she browsed through books from each section of the library, she discovered the roots of discourse analysis in ethnography and semiotics amongst other traditions, as well as the use to which discourse analytic work is put in social psychology which, she said, ‘seem[ed] to be mostly concerned with

²³⁷ Collins (1981) rejected criticism of the realist language he used in his case studies by maintaining that the *tu quoque* argument confuses constructionist *analysis* with realist *dismissal*.

effecting change in the discipline itself" (p. 337). Her travels led her to adopt a reflexive stance as she realised that 'writing about writing *has* to be a self-consciously circular process and its practitioners must learn to live with the (rhetorical) consequences' (p. 339).

Whilst not written in the style of a disciplinary introduction, as was Edwards and Potter (1992) or Wetherell (2001b), Ashmore, Myers and Potter's (1995) 'creative adventure' into the traditions that imbue and surround various efforts at 'writing about writing' communicated more of the 'feel' of working with discourse than either of these other treatments, possibly because the roots of DA were inferred rather than described, and, as such, their unfolding seemed more an action of reader than the authors.

Wetherell

Wetherell's editorial introduction (2001a) on the origins of discourse analysis and her chapter (2001b) on its themes are included in the first of two volumes that were written specifically for coursework on the subject (Wetherell, Taylor & Yates, 2001a; Wetherell, Taylor & Yates, 2001b).²³⁸ The following 'six more or less distinct discourse traditions' were identified:

1. conversation analysis and ethnomethodology;²³⁹
2. 'interactional sociolinguistics and the ethnography of communication';
3. discursive psychology;
4. critical discourse analysis and critical linguistics;
5. Bakhtinian research; and

²³⁸ One wonders how much reflexive discussion went into constructing the didactic form of these two volumes, that were created to 'provide a good, functional, working map of the field and a core reference point, designed for active researchers, and with the social scientist rather than the linguist in mind' (Wetherell, 2001a, p. 1), or whether the efficacy of such a form was black-boxed from the outset as a 'given'.

²³⁹ These seem to be to be two interacting but separate categories and Wetherell probably should not have combined them here. Ethnomethodology is an anthropologically-based method of participant observation at the laboratory bench (e.g., Knorr, Krohn & Whitley, 1980; Collins, 1974, 1975), that can involve the analysis of text as well as talk but that more often takes a wider view. CA, on the other hand, is complex, quasi-quantitative, most certainly atomistic analysis of talk and its patterns with a much narrower view of the context in which the talk occurs.

6. Foucauldian research. (p. 6)

The unifying tenet of all of these traditions is the notion that discourse is ‘social action’ (p. 10). This concept, in turn, has three aspects. The first is that discourse is ‘constitutive’ in that ‘language ... represents the world and people’s thoughts and opinions, [and it] ... can be faithful ... or ... unfaithful and misleading’ (Wetherell, 2001b, p. 15). In this view, the representations discourse constructs are accomplished through a complex interaction of the speaker, his or her use of language, and the social context in which the utterance comes into being. The second is that ‘discourse involves work’, that is, language resources and forms are marshalled to perform some specific function such as persuasion (p. 17). The third aspect of discourse is that it provides for the ‘co-production of meaning’ that is ‘normative’, ‘relational’, and ‘indexical’ (p. 18)

Conversation Analysis and Ethnomethodology

Conversation analysis focuses on ‘talk-in-interaction’ which has been called ‘the primordial site of human sociality’ (from Schegloff, 1992, in Heritage, 2001, p. 47). Developed initially from an interest in the minute analysis of ‘routine exchanges’ (e.g., Antaki, 1994, Sacks, 1992), conversation analysis works with finite sequences of dialogic speech, first transcribing the materials verbatim from recorded conversations, then coding them for various specific details such as pauses, emphases, overlap between one turn-taker and another, and non-linguistic but potentially meaningful elements that occur, such as grunts, laughs, stutters and other sounds.²⁴⁰

CA methodology as developed by Jefferson (e.g., Jefferson, 1985, 1988; Jefferson & Schenkein, 1978), has two aspects, one that pertains to the conversation analyst and one that pertains to the data that results. Firstly, the complexity and detail of the coding method and its anchoring in the ‘actual’ sounds, pace and other variables of utterance, requires of its practitioner a highly-evolved set of transcription, coding and

²⁴⁰ The use of the word ‘coding’ here sets me out self-consciously as someone who is sceptical about the assertion conversation analysts make that the transcriptive phase of CA is not ‘coding’ but *mere* transcription. Rather it seems to me more likely that the transcription regimen in use in CA includes latitude for the influence — whether subtle or gross — of the transcriber of the talk on the resulting transcription. This seems especially plausible given the emphasis conversation analysts themselves lay on importance of apprenticeships with ‘accomplished’ analysts (Wooffitt, Personal communication, 2004).

interpretative skills.²⁴¹ Secondly, the conversation-based data *per se* provides the analyst and the consumer of the analysis with finite sequences packed with potentially infinite interactional forms and structures of which one may ask deep questions about the nature of discourse as social action. The paradox here is that at the same time that the analyst *qua* analyst is distanced from the analysis by the application of highly systematised methodology, the competency of the analyst in utilising that methodology can moderate that distance, having a potentially profound effect both on the ‘quality’ of the available conversation-based data and on its subsequent interpretation by others.²⁴²

Ethnomethodology pulls out from finite sequences minutely analysed to a wider analytic that deals with ‘ways of speaking ... speech communities ... [and] native terms for talk’ (Fitch, 2001). Situated in an analytical soup with conversation analysis, micro-analysis, and the ethnography of speaking (Wieder, 1999), and derived from the work of Garfinkel (e.g., 1988, 1996), amongst others, in ethnomethodology there is a kind of positivistic tinge to the work in that ‘... actions, events, or objects are understood as *procedurally* encounterable by whomsoever witnesses them, and hence are, in the first place and always, objects within a field’ (Wieder, 1999, p. 166).

Socio-linguistics

Kress (2001) defines sociolinguistics as a form of discourse analysis that flowed from turn-of-the-last-century linguist, Ferdinand de Saussure. It led into what Kress calls a ‘structuralist’ analysis of language that focuses on ‘the signifier’ and the ‘signified’ and their relationships to each other (p. 31). Halliday (e.g., 1985) moved the focus to ‘social function’, that is, away from the ‘building-blocks’ of language, to the way in

²⁴¹ For a more detailed introduction to conversation analysis, see, for example, Markee (2000).

²⁴² Like discourse analysis itself, conversation analysis is significantly *craft-based*, by which I mean that apprenticeships with competent analysts are necessary to achieve what is considered to be proficiency in the method, at the same time that the process of such apprenticeships ensures that the craft knowledge that results is, to some extent, highly idiosyncratic to specific consensually-constructed and consensually-maintained communities of analysts. Awareness of this complexity and its inherent idiosyncrasy led me to avoid training with American analysts whilst working through these chapters so as not to complicate further my understanding of the materials at hand. It also led me to set as my goal in this portion of the thesis the *mere illustration* of the potential usefulness of such methodologies to an understanding of controversy in parapsychology. I did not attempt to actually *do* DA in this chapter in any deep sense of the term.

which words were strung together and nuanced so as to produce social meaning and accomplish social action (Kress, 2001, p. 35). Two key elements of Halliday's system were the 'choice' speakers made when using language, and the function of specific types of 'choice'. Kress listed three of these: (1) 'ideational function' which communicates something about the context in which utterances were made; (2) 'interpersonal function' which communicates something about the social interactions and relationships of the speakers if a dialogue was being analysed, or of the social context in which a single speaker considered himself to reside; and (3) 'textual function' which communicates something about the organisation of the speech itself 'as a message' (p. 34).

Critical discourse analysis and critical linguistics

Critical discourse analysis shifts the focus of the analysis to what the talk or text at hand can tell the analyst about '... power, dominance, social inequality' and, reflexively, 'the position of the discourse analyst in such social relationships' (van Dijk, 2001, p. 300). Unlike other socio-political analyses of discourse, critical discourse tends to be 'top-down', that is, it focuses on those who hold power and the ways in which they construct and maintain that power through discourse. This line of analysis developed out of Marxian and neo-Marxian sociological analysis with its traditional reworkings of the historical notions of modes of production (e.g., Gramsci, 1971), as well from the work of Foucault and other examples of '... sophisticated sociopolitical analyses' (van Dijk, 2001, p. 301) such as those done by Fairclough (1989), and Hodge and Kress (1988).²⁴³ Whilst this type of analysis seems more structural at some levels, within speech acts themselves subtle clues may be found which illustrate the way power functions to express or prohibit dominance (van Dijk, 2001, pp. 304-305).

²⁴³ It has been said that the work of this 20th-century Italian social theorist is no longer considered useful in his home country, although it has had a profound influence on 'cultural studies' in Britain (e.g., Verdicchio, 1995, p. 169). This influence, however, has been accomplished both by ignoring the historical Italian context in which Gramsci wrote and by reinterpreting his writings as related to a kind of universal 'nationhood' (p. 175) rather than to the specific political milieu to which he referred.

Bakhtian Research

Mikhail Bakhtin, an early 20th-century Russian theorist, provided another system to query how text and talk functions in the world (e.g., Maybin, 2001). Bakhtin's view of language revolved around a dialectic, that is, the notion that language always 'emerges from social conflict' and that there is always an ideological component to text and talk, an 'evaluative accent' (pp. 64-65).²⁴⁴

Bakhtin identified two types of social forces at work in language, 'centripetal' and 'centrifugal'. Centripetal force 'produce[d] the authoritative, fixed, inflexible discourse of religious dogma, scientific truth ... political and moral *status quo*', that is, a language constrained in both structure and function. Centrifugal force, on the other hand, opened language to the influence of a multitude of such 'particulars' as '... genres, professions, age-groups and historical periods', producing discourse that was '... open and provisional' and subject to the influence (p. 65).²⁴⁵ Bakhtin also dealt with the notions of: speech genres, that is, of such '... themes, constructions ... linguistic styles' as primary genres or simple '... unmediated speech'; and secondary genres, or that which was '... more culturally complex, artistic, sociopolitical, and scientific ...' and usually written (p. 66).²⁴⁶ Further, Bakhtin found that language is 'heteroglossic', constructed of a 'dynamic multiplicity of voices, genres and social languages' (p. 67).

²⁴⁴ Bakhtin has been depicted as having been forced to confront the sociolinguistic theory of such individuals as Saussure because of the political context in which Bakhtin found himself, that is, in the intellectual and artistic communities of 1920s Russia (e.g., Brandist, 1996, pp. 95-97). Like Gramsci, Bakhtin tied ideology to language in his work; and like Gramsci, British discourse analysts have taken up his work to what many believe is good effect (e.g., Maybin, 2001), but which — again, like Gramsci — is considered an illegitimate use by some Marxist and neo-Marxist intellectuals (e.g., Brandist, 1996, p. 108).

²⁴⁵ This dialectic is very much apparent in scientific texts if one characterises the search for 'scientific truth' — and the maintenance of 'found' truths through constraints on structure and content — as a centripetal force, and the constant pressure to expand science's 'truths' over broader and deeper territories of phenomena as a centrifugal force.

²⁴⁶ I am sceptical that there is such a thing as 'unmediated speech' even if the language being analysed is spoken or appears to be simple. DA has shown us that 'ordinary' first-hand utterances are in 'actuality' not simple, nor are they 'unmediated'. Although the concept has been criticised in other disciplines (e.g., Dorst, 1983), it has been used uncritically elsewhere (e.g., Simpson, 1997), such as to designate 'verbatim' or 'simple' dialogic speech between actors/characters in literature (e.g., Reed, 1993).

Foucauldian Analysis

Stuart Hall (2001) provided the chapter on Foucault's approach to discourse and language for the Wetherell et al. (2001) reader.²⁴⁷ Hall identified three themes in Foucault's writing (e.g., Foucault, 1972, 1973, 1980): (1) the 'concept of discourse'; (2) 'power and knowledge'; and (3) 'the question of the "subject"' (p. 72). For Foucault, in Hall's view, the shift of interest from language *per se* to the notion of discourse rested on the understanding that 'discourse [was] ... a system of representation' (p. 72). As such one might then examine the '... rules and practices that produced meaning statements', whether they were uttered or written.

Foucault distinguished himself from semioticians in that he was not interested in discourse in and of itself, but rather in how it functioned, and what it conveyed about the social, political and historical context in which it arose. To uncover this more contextualised construction of meaning, Foucauldian analysts needed to focus not only on the utterances or texts themselves, but rather on the 'rules' that existed for the production of such utterances or texts within their own time, the topics they covered, the ways they related to authority and power, and on the reciprocally-constitutive nature of discourse and history (Hall, 2001, p. 73).

As Foucault's career evolved, he became more concerned with '... how knowledge was put to work through discursive practices in specific institutional settings [so as to] ... to regulate the conduct of others ...' and on the interplay of knowledge with power, especially '...how power operated within ... an institutional *apparatus* and its *technologies...*' (p. 75, author's italics).

Discursive Psychology

In discussing Wetherell's approach to the genealogy of discourse analysis, I have left discursive psychology to last because the brief attempts at analysis I will provide in Chapter 7 follow on some concepts that have found their fullest expression in the collection of methods subsumed under this particular approach.

²⁴⁷ It is interesting that Hall is one of the British linguistic analysts criticised by Verdicchio (1996) as misusing Gramsci (p. 169).

As should be apparent by now, perhaps the most important contribution to the study of discourse that has been made by social psychologists is the recasting of discourse as social action. Wetherell (2001b) identified three aspects of this concept that are key to the enterprise:

1. That discourse is 'constitutive' (p. 15)
2. That discourse 'involves work' (p. 17)
3. That meaning is the product of 'co-production' (p. 17).

By 'constitutive' Wetherell meant that language is not a 'neutral' medium through which '... a description of the world, or the structure, form or outcome of a conversational exchange merely occurs. Rather discourse performs social work, constructing a version of social reality' (p. 17), moving a depiction or an interaction towards a particular end, whether intentionally or not, and thus, is not only structural but functional.

To say that meaning as it arises through discourse is the result of an act of 'co-production' is to say that as discourse occurs, there are 'complex social and historical' processes that are both 'conventional and normative' and it is through these processes that the meaning that is attached to the discourse is formed. Meaning-making, thus, is social in both the '...global sense ... [and in the] local sense'. Utterances may thus be cultural in a wider sense, and also 'indexical' in the sense that the meaning they co-produce depends very much on 'their contexts of use'. To put it more simply, discourse arises *out* of something but does not do so randomly or nonsensically. Rather, it arises *for* something, crafted in a social context, so as to perform a social action (p. 18).

When discourse is viewed in this way, it becomes possible for analysts to look for patterns in the discourse that can illuminate the practices by 'which people collectively ... organise their conduct' (p. 18). One such pattern, 'interaction order' (p. 2) was developed by the sociologist, Erving Goffman (1983). It is a concept that, at first glance, may have seemed to be a simple structural feature of discourse, but which yielded, upon analysis, a number of important insights into how 'speaking rights' (Wetherell, 2001b, p. 18) were established, what governs what Goffman called 'turn-taking', how discourse was constructed to be collaborative or disruptive and the interaction of such constructions with expected rituals of discourse, how categoric or

individual identifications were accomplished, and how certain face-to-face environments impacted the discourse that resulted (Goffman, 1983, p. 3), amongst other things.²⁴⁸

Discourse Analysis in Psychology and Its Application to Fact Construction and Controversy

Edwards and Potter (1992) identified the following five major concerns of discourse analysis as it is used in psychology. Firstly, DA ‘deals with naturally occurring talk and text’ (p. 28). This distinguishes DA from speech act theory, conversation analysis, and the traditional way in which experimental psychology deals with text.²⁴⁹ Secondly, DA in psychology is said to be concerned with the ‘content of talk, its subject matter’ and with its ‘social rather than linguistic organisation’ (Edwards & Potter, 1992, p. 28). This distinguishes it from the form of linguistic analysis in which language is dealt with as if it were content-free, as collections of grammar, syntax, phonemes and so on. Thirdly, DA in psychology focuses on ‘action, construction and variability’ (e.g., Potter & Wetherell, 1987), wherein action is defined as produced by talk, drawing on stylistic, linguistic and rhetorical resources, and variability is defined as that which becomes visible in the way in which talk and text are constructed and deployed. Through variability, the analyst is able to discern the ‘interactional contexts’ talk and text are ‘constructed to serve’ (Edwards & Potter, 1992, p. 28). Fourthly, DA in psychology is concerned with argument in text and talk in the sense that it seeks to understand how ‘everyday talk and thought’ are organised to serve communicative goals (e.g., Billig, 1987). Finally, DA in psychology is concerned with the ‘ostensibly “cognitive” issues

²⁴⁸ Whilst these ideas were expanded over Goffman’s career, they remain important not only to analysts who focus on Goffman’s work *per se* (e.g., Smith, 1999), but also to those who apply the concepts to specific social groups (e.g., Shakespeare, 1998, pp. 26-27), as well as to those who are seeking to expand their scope into more complex interactions (e.g., Misztal, 2000) or institutions (e.g., Jackson, 2003).

²⁴⁹ Just as one might be sceptical of the Bahktian notion of primary genres, one can be sceptical of the sources of ‘naturally-occurring speech’ that form the basis of some DA in psychology. For example, an interview with Princess Diana (e.g., Wetherell, 2001b), or commentaries on specific social topics solicited by psychologists (e.g., Potter, 1996) can hardly be called ‘naturally-occurring’.

of reality and mind ... how cognitive issues of knowledge and belief, fact and error, truth and explanation' function (Edwards & Potter, 1992, p. 28).

In naturally-occurring discourse, as in talk elicited for specific research purposes, or in text, 'speakers' seek to establish 'facts' in the dialogue. Edwards and Potter (1992) provide a summary of the construction of 'factual accounts'. Such establishment, they argue, is a kind of 'social accomplishment' and a number of devices have been identified that allow speakers to represent their depiction as being a 'feature of an "out-there" world, rather than [a] reflection of the actor's own desires or conscious' (p. 160). Part of an actor's ability to do this depends on their ability to martial 'category entitlements', that is, to be perceived as a person who 'knows'. Whilst such entitlements can be more a component of the social context in which an utterance is made, it can also be an action of the discourse itself. Actors may use 'vivid description', that is, a sufficient amount of 'contextual detail' so as to 'make' the perceptual event the speaker is describing more immediate to the hearer which, in turn, gives the impression that the speaker possesses 'particular skills of observation' (p. 161). Embedding a 'fact' depiction within a narrative that seems to require the appearance of the 'fact' or using 'vague, global formulations' (p. 162) can both establish the 'fact' in the exchange and protect it from undermining in a subsequent turn in the discourse.

Using 'empiricist accounting' can also help construct a 'fact'. Other devices include: embedding the 'fact' claim in what appears to be a structure drawn from formal logic; 'drawing on the extremes of relevant dimensions of judgement' (p. 162); claiming that the 'fact' has been consensually constructed or corroborated by individuals who seem to hold category entitlements; or embedding the 'fact' in 'lists, particularly three-part lists' (p. 163), the effect of which is to lead the hearer to consider the list as either 'complete' or 'representative'.

A key element of DA in psychology is that the interlocutors' intentions, motivations or cognitive competencies are not at issue. The point is to analyse the discourse itself, *sui generis*, giving primacy to the way in which talk and text *do* social action (Wetherell & Potter, 1992, p. 93). Through studies of this kind, a great deal has been learned about the 'standard discourse moves' that are used in specific discursive contexts. For example, when 'fact' construction or claims to category entitlement are

deflected or undermined, a number of standard methods of ‘coping with a negative evaluation’ have been uncovered (Potter & Wetherell, 1987), including:

1. ‘admit[ing] the offence but offer[ing] mitigations ...’
2. ‘deny[ing] the offence and claiming that one is wrongly accused’
3. ‘accept[ing] the blaming in its entirety and perhaps intensify[ing] or expand[ing] on it by giving other examples’
4. ‘undermin[ing] the accusation by renegotiating the nature of the offence, [or] recategoris[ing] it as something less negative and more excusable’, and/or
5. ‘redirect[ing] the accusation to another group of people, carefully separating or distancing oneself from the accusation’ (p. 212)

Mulkay’s *The Word and the World*

Scientific controversy is a species of discursive conflicts that can occur both in text and in talk. Although twenty years have passed since it was published, Mulkay’s (1985) treatment of controversy in science is still one of the best studies of this kind. In this volume, Mulkay used texts that had been gathered for Gilbert and Mulkay (1985), extending those materials through conversations and correspondence. Mulkay provided descriptions of ‘interpretative practises’ that could be found in ‘scientific discourse’, by which scientists ‘attribute[d] meaning to, and thereby constitute[d] their social world’ (p. 3). Like Ashmore, Myers and Potter (1995), Mulkay took his readers on a creative adventure that both illustrated his analyses and foregrounded his presence as an analyst.²⁵⁰

Mulkay’s volume is divided into four sections. The first section focused on a series of letters exchanged by two individuals who had been informants in Gilbert and Mulkay (1985). Although Mulkay identified them as Noble Laureate biochemist Peter Mitchell and one of his colleagues, Mulkay called them ‘Spencer’ and ‘Marks’, not only

²⁵⁰ Although this could be seen as a return to sociological analysis, and not an appropriate component of a chapter that has moved decisively towards DA in psychology, Mulkay’s volume is in keeping with the spirit, if not the methods and findings of DA in psychology in that his primary focus is on what social action talk and text could *do* under conditions of controversy.

to background their true identities and thus make them representative of scientists in general, but also to insert a playful element into the analysis. In the letters, Spencer attempted to convince Marks that his own view of a particular phenomena was fact-based, and that Marks' opposition to Spencer's view was a case of mistaken interpretation. At the same time, Marks attempted to persuade Spencer that Marks' view was the correct one.

In the second section, Mulkay examined a conversation he had with Spencer about Mulkay's previous analysis of the correspondence. He was concerned to show Spencer that correspondence could not be successful in persuading opponents because each participant brought a sense of their own superiority to the exchanges, and each attempted to persuade the other from that stance. Spencer's comments showed he was convinced not only that his approach to the phenomena was correct, and but that he could negotiate a consensus with Marks through correspondence. Elements of the correspondence itself, however, convinced Mulkay that, in fact, Spencer was not interested in having a 'real' dialogue in which his colleague's views might influence his own. Rather Spencer hoped Mulkay's analysis would provide him with new ways to force consensus on Marks. Mulkay believed, on the other hand, that because of the 'interpretative inequality' inherent in the exchanges, consensus was impossible.

In the third section, Mulkay used a literary example to illustrate how totally different interpretative scenarios might be woven around identical texts. Recasting his discussion of replication as a play, Mulkay incorporated the scientists' positions which moved from an idealised depiction of replication to a more relativistic depiction of the meaning and style of replication in practise. By using the play, Mulkay was also able to interweave reflexively into his analysis the positions of classic sociology of science, the ethnomethodological approach, and the paradoxical stance Collins (1983) has taken to his own analyses which they have been criticised as realist.

In Part B of the volume, Mulkay focused on discovery, using the interview transcripts and other texts, amongst them Mitchell's Nobel Laureate acceptance speech. In this way, Mulkay illustrated how scientific discoveries are depicted, from the folk notion of the 'Eureka' moment, to Mitchell's own acknowledgements of both the precursors to his work and the evolution of his own 'discovery'.

Mulkay followed this with two more playlets that illustrated the various positions he had taken, through which he brought the book to a close. Serving the same functions, albeit in a more reflexive manner, these playlets were offered in place of a conventionally-constructed ‘conclusions’ chapter.

For analysts interested in controversy, Mulkay derived a number of useful conclusions both from the materials and the methods he used. For example, he uncovered the presence of the empiricist repertoire in an epistolary debate, as well as showed that the failure of that debate to effect closure stemmed at least in part from both correspondents’ inability to adopt interpretative equality. Mulkay also found structural elements in the correspondence that were analogous to turn-taking in conversation analysis, in that there were stylised openings and closings that anchored letters in the chronology of the correspondence, both acknowledging previous turns and inviting succeeding ones. Mulkay was also able to examine the differing uses of self-preferential and self-deprecating statements.

There were some differences from previous the findings of CA and DA extant at the time Mulkay completed his own analyses. For example, ‘written turns appear[ed] to be much longer, more complex and less directly generated and constrained by the mechanisms of turn-taking’ (p. 100). He speculated that as discourse moved away from talk, the ‘basic generating mechanisms of turn-taking’ were no longer in force, and interlocutors ‘increasingly [relied] on alternative techniques, such as ... on the empiricist repertoire’ (p. 101).²⁵¹

Conclusion

In this chapter I reviewed the traditions that led to discourse analysis as well as presented some of the findings that pertain to ‘fact’ construction and controversy in scientific talk and text. I have not presented a general review of discourse analysis *per se*

²⁵¹ See Greatbach and Dingwall (1998), Whalen and Zimmerman (1990), Widdicombe and Wooffitt (1995), and Wooffitt (2001) for conversation analyses that illustrate a variety of elements of talk including turn-taking, and which are drawn from a wide variety of real-world contexts.

but rather focused on the evolution of the notion of discourse as social action. By characterising this review as ‘taking a turn to self’, I do not mean to minimise the importance discursive psychology lays on the primacy of discourse but to foreshadow the way in which the identity of the speaker in the texts I analyse forms a key context out of which scientific discourse occurs, if for no other reason than that the depiction of one’s personal identity — whether explicit or implicit — is a species of category entitlement and as such may ‘make’ or ‘break’ the establishment of ‘facts’ in scientific prose.

In Chapter 7, I will attempt to apply some of the concepts and findings of discourse analysis to a series of examples of scientific text which were produced for a published debate on parapsychology’s status as a science.

CHAPTER SEVEN

ANALYSING THE DEBATE IN *BEHAVIORAL AND BRAIN SCIENCES*

Introduction

In Chapter 6, I reviewed a variety of traditions that have attempted to analyse discourse so as to derive an understanding not only of the structure and form discourse exhibits, but also of the action discourse *does* in the social, political, historical and rhetorical contexts in which it arises. In this chapter I will present a case study in which I have attempted to use some of the methods of discourse analysis as they have evolved in psychology. Granted these exchanges are anything but ‘naturally-occurring’ speech. Rather they are scientific texts solicited by the editors of *Behavioral and Brain Sciences* as part of formal debate over the status of parapsychology as a science.

The debate comprised: two target articles, one by proponents K. Ramakrishna Rao and John Palmer (1987), ‘The Anomaly called Psi: Recent Research and Criticism’, and the other by critic James E. Alcock (1987a), ‘Parapsychology: Science of the Anomalous or Search for the Soul?’; 48 invited commentaries on the target articles,²⁵² including one by Alcock (1987b) and one by Palmer (1987a); and two closing statements, one by Alcock (1987c) and one by Palmer and Rao (1987).²⁵³ Although I will

²⁵² The invited commentaries were: Adamenko, 1987; Akers, 1987; Bauslaugh, 1987; Beloff, 1987b; Benassi, 1987; Beyerstein, 1987; Blackmore, 1987a; Braude, 1987b; Broch, 1987; Broughton, 1987; Bunge, 1987; Child, 1987b; Cichetti, 1987; Costa de Beauregard, 1987; Dawes, 1987; Donderi, 1987; Dybvig, 1987b; Eysenck, 1987; Feder, 1987; Flew, 1987; Gardner, 1987; Gergen, 1987; Gilmore, 1987; Glymour, 1987; Hansel, 1987; Hövelmann, 1987; Hyman, 1987; Josephson, 1987; Krippner, 1987b; Mackenzie, 1987; Nadon & Kihlstrom, 1987; Navon, 1987; Nelson & Radin, 1987; Palmer, 1987; Parker, 1987; Pinch, 1987; Railton, 1987; Sanders, 1987; Schmidt, 1987; Spanos & de Groot, 1987; Stanford, 1987; Tart, 1987; Tobacyk, 1987; Truzzi, 1987; Utts, 1987; Vassy, 1987; Wolins, 1987; and Woodward, 1987.

²⁵³ Alcock (1990a) reiterated his points in a ‘continuing commentary’ published in *BBS* in 1990, to which Rao and Palmer (1990) and three others responded (Hövelmann, 1990; Hubbard, 1990; Snow, 1990); and in another round of correspondence in 1998 (Alcock, 1998; Glicksohn, 1998; Palmer, 1998). I will not be dealing with texts from either the 1990 or the 1998 debate here.

not comment on this directly in this chapter, the crux of the cognitive argument that Rao and Palmer made and Alcock rejected was whether or not parapsychology has uncovered 'true' anomalies.

Method

In order to analyse the text in this debate, I compiled and read the complete set of articles, commentaries and statements listed above in the order in which they were published. As I read these materials for the first time, I noticed that Alcock's responses to the invited commentaries seemed to express surprise that a number of the commentators who belonged to Alcock's own community of sceptics/critics, had criticised him both for characterising parapsychology as a 'search for the soul' and for making the argument that 'dualism' and 'science' were incompatible. It occurred to me after that first reading that Alcock had not anticipated any criticism on these two points from members of his own community. Therefore I decided to focus on this aspect of the exchanges in this analysis.

I then went through the texts authored by Alcock, excerpting any passages in which parapsychology was characterised as the 'search for the soul', or in which 'dualism' was characterised as incompatible with 'science'. I also went through the commentaries and excerpted any passages that commented on either or both of these two assertions. Next I read through statements that pertained specifically to Alcock's use of contingent arguments, excerpting examples of these, and contrasting them with passages in Alcock's responses in which he formulated the arguments of those who criticised him. I next excerpted examples of passages in which commentators agreed with Alcock's characterisation of parapsychology and with his points on dualism and science, and contrasted these with passages in Alcock's responses in which he formulated these comments. For those who criticised Alcock, and for those agreed with him, I also compared commentators' formulations of Alcock's statements to the original texts in Alcock's target article. Finally, I rearranged the passages so that the excerpts taken from commentaries written by critics of parapsychology were in one group, and excerpts written by proponents of parapsychology were in another group.

Results

The First Turn: Alcock

The notion that parapsychology is not the ‘science of the anomalous’ but rather a metaphysical search for the ‘soul’ is first raised in Alcock’s title. The final line of the abstract also addressed the point directly:

It is argued in this paper that parapsychological inquiry reflects the attempt to establish the reality of a nonmaterial aspect of human existence, rather than a search for explanations for anomalous phenomena. (Alcock, 1987a, p. 553)

In the excerpts that follow, Alcock characterises parapsychology as founded on a rejected philosophical position — Cartesian dualism — that he sees not only as incommensurate with the modern scientific worldview but also as a kind of protective covering for what he argues is the ‘real’ purpose of parapsychology, that is, the search for a disembodied primary principle, for mind independent of brain, for ‘soul’.

The following excerpts were chosen because the search for the soul or dualism were directly mentioned. In section 1.2 of his target article, ‘A non-physical dimension of existence’, Alcock wrote that parapsychologists believe that paranormal phenomena exists independently of the brain. Using a quote from Beloff (1977, p. 21), Alcock made his point that ‘... parapsychology, using the methods of science, becomes a vindication of the essentially spiritual nature of man which might forever defy strict analysis’ (Alcock, 1987a, p. 555).²⁵⁴

Prior to this paragraph Alcock described another perspective within parapsychology which he called the ‘incompleteness of current science’ in which it was argued that, ‘Just as the scientific worldview changed to accept the extraterrestrial

²⁵⁴ It is typical of Alcock’s writing to use quotes from proponents to provide support for his points. Alcock’s critics have commented that he finds quotes that represent minority views amongst parapsychologists, but presents them as representative of majority views. This particular quote is a case in point: Beloff’s emphasis on dualism in parapsychology, and also on the necessity of setting aside some phenomena as being outside of the purview of science are, in my opinion, minority views in the field, the latter more so than the former.

source of meteorites ... it must ultimately accommodate psi'. On these two perspectives Alcock countered: '... the incompleteness approach would no doubt be more acceptable to most scientists. Yet, it does not really capture the *flavor* of the paranormal' (p. 555, Alcock's emphasis), which, Alcock asserts, if it does not overtly accept dualism, at least eschews materialism, as in his paraphrase of L. E. Rhine: '... Some parapsychologists might deny being mind-body dualists, but they would do well to consider just how they are going to define their subject matter without some reference to the independence of the mind from the materialistic realm ...' (Alcock, 1987a, p. 556).

Another passage in which Alcock describes the non-materialist/dualist basis of parapsychology is as follows:

... The dispute about psi reflects the clash of two fundamentally different views of reality. The first of these is the materialistic, monistic view that the human mind is some sort of emergent manifestation of brain processes, whereas the second is the dualistic position that maintains that the human mind/personality is something beyond the stuff of atoms and molecules. (p. 565).

For Alcock, parapsychology is 'the search for the soul ... Because, if the mind can operate separately from the physical brain, as the psi hypothesis would suggest, then it possesses much of what has been ascribed to the soul' (p. 565). Whilst he distances himself from the extreme formulation that parapsychology uses definitions drawn from religious texts, he argues that the parapsychology's discursive distance from religion is only rhetorical, a protective colouring that hides what is really at stake in the field:

Most religions teach that the Soul survives death in some form. The question of survival of the parapsychologists' 'soul' or 'mind' or 'personality' after death is, even many leading parapsychologists agree, an important question for parapsychology to consider. (p. 565)

Because survival is a 'fundamental question' in parapsychology, Alcock argues, it is proper to lift that protective colouring so as to confront the 'real' motivation behind parapsychology:

Thus, it is important in any debate about parapsychology to make clear just what is being debated. Is the debate about whether or not there exist 'natural' phenomena that science has so far failed to recognize, or is the

debate about whether or not dualism, as opposed to materialistic monism, is the correct view of nature and of mankind's place in nature? Or, is the first question very often the surface issue, while the hidden agenda is the question of dualism? (p. 565).²⁵⁵

In these passages and others like them, Alcock presents a formulation of the 'central' arguments of parapsychology that emphasises aspects of the history, theory and current concerns of the field that conform to Alcock's argument but may not be representative of what would count as consensual understanding of these same elements amongst parapsychologists. Alcock presents them as representative, however, as insights based on good, scientific observation. In warranting his formulations, he produces corroborating quotations or paraphrases from well-known parapsychologists. By doing this Alcock attempts to establish his footing as an animator of the 'facts'. Rather than being the author of his statements, in Goffman's (1983) sense, Alcock is presenting himself as someone *through whom* a depiction is being communicated. He sets up a distance between his personal beliefs and his formulation, saying in effect, 'it's not my fault: this is what the field is really about, and here are some insiders who say this exactly'.²⁵⁶ Secondly, Alcock uses this footing to accomplish an empiricist accounting, that is, he depicts himself as a *mere* recipient of this characterisation of the field, and that, in actuality, the writing of parapsychologists themselves has 'forced' these 'facts' upon him (e.g., Edwards & Potter, 1992, pp. 160-162).

In another passage Alcock produces what amounts to a stake inoculation in that he anticipates the empiricist accounting that Rao and Palmer use in their target article, and brackets that anticipated argument as providing a misleading picture of what is 'actually' going on in parapsychology:

Psi has been postulated not because normal psychology is incapable of

²⁵⁵ The notion that the survival question is fundamental to parapsychology is as disputed within the field as Beloff's emphasis on dualism. In fact, many in the field see Rhine's labelling of survival as an unsolvable problem as the first step towards 'real' scientific 'progress' in the field.

²⁵⁶ Wetherell (2001b) defined Goffman's notion of 'footing' as the idea that '... when people talk they can speak as either the *author* of what they say, as the *principal* (the one whom the words are about) or as the *animator* of someone else's words' (p. 19). Footing can be put to use in discourse designed to accomplish a number of social actions.

accounting for people's apparently psychic experiences ... Rather, the search for psi is now, as it has been since the formal beginning of empirical parapsychology over a century ago, the quest to establish the reality of a nonmaterial aspect of human existence — some form of secularized soul. (p. 565)

In these excerpts and others like them, Alcock sets himself up as a 'true reader' of the subtext of any seemingly scientifically-conservative assertions that Rao and Palmer may make, either for the presence of anomalies in human experience or the need for greater understanding of such anomalies than psychology has heretofore provided. Alcock does this by setting aside the argument he anticipates as a spurious, *merely* rhetorical formulation of the field's purpose.²⁵⁷

Further, the 'search for the soul' metaphor is not accompanied by specific textual support from the parapsychological literature, with the exception of a few general comments such as those mentioned above. Because this is so, Alcock's passages may also be classified as what Edwards and Potter (1992, p. 163) call 'systematic vagueness' in that Alcock's non-specific argument that parapsychologists' descriptions of their enterprise mask what 'really' motivates their work can serve to undermine any arguments to the contrary.

The Second Turn: The Commentators

The following presents an analysis of excerpts drawn from the responses to Alcock's (1987a) target article in *Behavioral and Brain Sciences*. The first section focuses only on those who are known to be critics of parapsychology and the second section only on those who are known to be proponents of parapsychology.²⁵⁸

²⁵⁷ Being an 'insider', I find it difficult to refrain from noting that Alcock's characterisation of the 'real' purpose of parapsychology is, from my perspective, unrepresentative of the field as a whole. That is, at least some of us are motivated to do research in this field *precisely* because we think phenomena to which paranormality is attributed are inadequately explained by conventional psychology. In addition, for many of us, whether or not these phenomena can still be called 'psychic' or 'paranormal' *when* they are adequately explained is beside the point.

²⁵⁸ The excerpts that follow are not drawn from every commentary. Amongst the critics, for example, there were those who agreed with Alcock that materialism was the only philosophical position a scientist could take if for no other reason than that scientific 'progress' was based wholly on materialism (e.g., Tobacyk, 1987). Such commentaries — which would be seen as

The Critics

In one commentary (Akers, 1987), Alcock's depiction of the 'real' parapsychology was extended to an explicit conclusion. 'Alcock', the commentator wrote, '... sees, as the fundamental problem, that parapsychologists are not really involved in scientific research'. Whilst this excerpt did not dispute this depiction of the field, it denied Alcock's footing as an animator, as an 'observer' of the 'facts'. Instead, the commentary cast Alcock as the author of his statements, reducing the distance Alcock attempted to place between himself and his 'facts', implying that his arguments were contingent, and as such, outside the bounds of acceptable scientific discourse: e.g., 'I doubt whether these motives, assuming that they can be identified, are relevant to the debate' (p. 567).

Even for those critics who agreed that such contingent factors as the 'search for the soul' lay behind science practise in parapsychology, they saw an equally contingent factor behind Alcock's formulation, that being a 'staunch belief in materialism' (e.g., (Benassi, 1987, pp. 570). Other critics formulated Alcock's argument as even more strongly contingent by using colloquial language to label the discourse Alcock used: e.g., 'Alcock quite obviously has a *pet peeve*. He is concerned with calling attention to the extent to which mind/body dualism may be a "hidden agenda" in much "parapsychological" work' (Sanders, 1987, p. 607, my italics). Like the author of the first excerpt, these critics undermined Alcock's footing which, in turn, undermined the persuasiveness of Alcock's points.

Other commentators did not set aside Alcock's claim that parapsychology is compromised by 'hidden' motivations, but rather made a more subtle argument that such contingent variables affect all scientists, and because of this, scientific prose should focus on the empirical. For example:

There is an implication in Alcock's piece that parapsychologists are driven by their metaphysical belief systems and that a considerable amount of variance in their behavior can be explained by these beliefs.

historically and sociologically naïve — merely applauded Alcock's perspective and did not either extend or expand it.

Thus, parapsychologists may be seen to conduct flawed experiments, to be taken in by tricksters, to fail to reject the psi hypothesis in the face of disconfirming evidence, and so forth, because their search is not for scientifically validated 'truth' but for the soul. All of this may be correct, but are not mainstream scientists also guided and biased by their beliefs ... The level of debate between skeptics and parapsychologists would be elevated if the critics would focus more attention on the quality of evidence parapsychologists present and less on their motives' (Benassi, 1987, pp. 570-571).

Alcock's footing was less severely undermined in this commentary than in the previous one, partially because colloquial language was not used (i.e., 'pet peeve'), and partially because the list both served to formulate his central point more strongly and was set off by a tentative but explicit agreement: 'All of this may be correct ...'.

Another commentary focused on the presence of 'hidden' motivation in all scientists and the ability of scientific methods to hold those motivations at bay so that what was 'true' could emerge from scientific research. Rather than being 'wrong', or being the 'author' of a personal and biased characterisation of parapsychology, it was implied that Alcock, perhaps, did not have sufficient faith in the power of science to level the playing field that such contingent variables as personality, beliefs, or historical context, amongst others, made uneven:

Alcock is surely right in arguing that much parapsychological research has stemmed from dissatisfaction with materialism as a worldview. However, from the title of his paper onward, he assumes that such a motivation on the part of parapsychologists *ipso facto* makes their endeavors scientifically suspect. In reality, much of the highly respected work of such eminent scientists as Kepler, Newton, Flourens, James, and Sherrington (the list could be easily extended) was motivated by dissatisfaction with materialism ... The empirical findings of these scientists have not stood or fallen on the basis of their beliefs about materialism, and neither should any empirical findings generated by parapsychologists. (Spanos & de Groot, 1987, pp. 609-610)

Other commentators objected to Alcock's perceived commitment to materialism as the only philosophical basis for science. For example:

Like Alcock, I find that the evidence for psi remains unconvincing, but I think Alcock goes beyond skepticism (doubt and non-belief) to disbelief and advocacy of the materialism/monism of the dominant (orthodox)

psychological outlook. And I think he assumes that that position does not need to bear any burden of proof ... Alcock insists that most parapsychologists are open or closet dualists, and he argues that there is a fundamental incommensurability between their views and those of the mainline (materialistic/monistic) psychology he represents. ... Alcock seems to view dualism as so fundamentally unreasonable that there is little possibility of eventual agreement based on future experiments. (Truzzi, 1987, p. 614)

There is a sense here, as in the previous commentary, that the philosophical underpinnings of empirical work can be seen, not as scientific 'content' but as a kind of contingent variable, that can be — and indeed, in the history of science, has been — set aside as empirical results mount. There is an undermining of Alcock's footing as distanced from his 'observations' because it is implied that Alcock's understanding of the history of science and of the importance of contingent variables to scientific 'progress' is in 'error' and/or erects barriers to future scientific research.

Finally, some commentators were not susceptible to Alcock's inoculation against Rao and Palmer's use of the empiricist repertoire in their target article. That is, Alcock's discussion of 'hidden' motivations did not dissuade these commentators from finding a disjuncture between Alcock's notion that all parapsychologists are compromised and the impression Rao and Palmer conveyed in their depiction of parapsychological research. For example:

... it is difficult to see how his arguments can be brought to bear against those who, like R&P, explicitly renounce such an agenda, hidden or otherwise. They want to treat psi phenomena as anomalies. The question for them is whether there are such anomalies. / Surely that is the right question. ... (Sanders, 1987, p. 607)

Alcock's comments on 'hidden' motivations can also be seen as an attempt to deny category entitlement to parapsychologists as scientists. For at least one commentator, this 'observation' of a 'hidden' agenda both contradicted his own experience with parapsychologists, and lacked persuasiveness because of the gradations of philosophical beliefs that exist in philosophy and science along the continuum from dualism to materialism:

Clearly, Alcock's attack is less on the data for psi than on the psi

researchers' "hidden agenda." The wide diversity of psi researcher's views on such philosophical matters simply contradicts Alcock's charge; clearly, such an agenda is not apparent in the R&P approach, and it is explicitly denied by many parapsychologists I know. (It should also be noted that Alcock neatly ignores the existence of a wide variety of dualistic and monistic philosophies of mind.) In any case, however, Alcock's attack on motivation is here quite irrelevant insofar as the data gathered by the psi researchers is uncontaminated by it. (Truzzi, 1987, pp. 614-615)²⁵⁹

It is important to remember that the excerpts I have discussed are from commentaries written by individuals with a critical and/or sceptical stance towards parapsychology. That is, individuals who basically agreed in whole or in part with Alcock's overall approach to parapsychology, still criticised Alcock for using arguments they saw as conflicting with the history of science, as illegitimate, or as unfortunate, impeding debate on the empirical, cognitive content of parapsychology, on the 'data'.

The Proponents

It is not surprising that those who have held a positive opinion of, or who have worked in the field would find Alcock's notion of parapsychology as 'the search for the secularised soul' wrong-headed, if not offensive. The following excerpt, written by a proponent who is not a working parapsychologist, provides an example of a response that ratcheted up the debate in a contingent sense:

Follow if you will, in these pages, the torments of a religious conscience as it confronts heresy. The heretic's thoughts and works appear to be steeped in blasphemy, and that the heretic and the orthodox believer might profess the same religion is an idea simply too painful for the orthodox imagination. / Alcock, the orthodox believer, anathematizes the parapsychological heretics and casts them out among the damned ... Should the heretics, anathematized, be excommunicated from Science? According to Alcock, yes ... But Alcock, a generous inquisitor, offers parapsychologists the opportunity to reaffirm their

²⁵⁹ The final sentence in this excerpt is sociologically-charged, especially the phrase 'insofar as the data gathered by the psi researchers is uncontaminated by it'. Truzzi was certainly aware that data is never *uncontaminated* by psychological and sociological variables that impinge on science. But I believe he was hopeful that contingent variables could be minimised, and *insofar* as that was possible, lasting results/lasting interpretations could be accumulated in science.

orthodoxy: They will be readmitted to the faith if they “focus on the anomalies while putting the concept of psi aside.” (Donderi, 1987, p. 582)

The commentator then characterised Alcock’s point of view as comprising a ‘cult’ within ‘the ecumenical’ context of science (p. 582). In this excerpt Alcock’s footing as a distanced, scientific observer, an animator of the ‘reality’ of parapsychology, its motivations and philosophical underpinnings, is undermined in the strongest terms. Rather Alcock has become a dogmatic religionist, and an ‘inquisitor’, a word with extremely negative cultural and historical connotations. In addition, as an ‘inquisitor’, the commentator situated Alcock, in an ironic turn, as a ‘generous’ inquisitor who would grant a reprieve if only parapsychologists eschewed the notion of ‘psi’.²⁶⁰

Another commentator stated: ‘Alcock’s opinion that what parapsychology is actually all about is the “search for the soul” (or maybe even the search for something *extra naturam*) is the unquestioned premise, not the result, of his investigation’ (Hövelmann, 1987, p. 593). Working from such an ‘unquestioned premise’, Alcock was ‘forced ... to present a picture of parapsychology that is both carefully curtailed and distorted in a very specific way, one that cannot in fairness be considered representative of the work and arguments of leading parapsychologists’. This commentator then reproduced a list of sentences from Alcock’s target article in which his characterisation of parapsychology and parapsychologists was reiterated. Making a ‘claim to credibility’, the commentator objected to Alcock’s ‘description [as] ... a caricature of the leading conservative, experimentalist circles in parapsychology’. The crux of this criticism was that Alcock’s statements not only constituted ‘unsupported beliefs about ... current and future scientific-political developments within parapsychology’ but the commentator stated a belief that Alcock seemed completely uninterested in reframing his contentions as empirical questions (p. 593). Alcock’s stance as a distanced, disinterested observer able to use an empiricist accounting to criticise parapsychology was very strongly

²⁶⁰ Part of the irony here, for an outside reader, is the fact that Alcock’s text rejected the notion that parapsychology *is* the search for anomalies, as well as rejected the notion that any *anomaly-focused* parapsychologist could have an impact on the field as a whole.

undermined by this commentator. Whilst not resorting to the highly contingent and emotionally- and culturally-charged language of the previous excerpt, Alcock's categorically entitlement was demolished.

The Third Turn: Alcock's Replies

Alcock's reaction to his respondents hinged on the following which he felt were the principle 'misunderstandings' in the commentaries:

- that only the title, abstract, and a few paragraphs at the end of his review have anything to do with 'mind-body dualism';
- when 'mind-body dualism' was raised it was only to explain the 'persistence' of parapsychology and not as a comment on the 'nature of the debate' (p. 627);
- that he never claimed that parapsychologists were searching for 'the existence of disembodied souls as such', but rather used 'search for the soul' and 'mind/body dualism' as a metaphor for the search for the independent influence of mind on matter; (p. 628)
- that he believes that parapsychology 'opposes the predominant materialist worldview' (p. 628);
- but that he does *not* believe that 'the dualistic hypothesis has to be wrong or that it is to eschewed by those who practice science' (p. 628);
- that he never said that parapsychologists are not trying to scientifically 'validate' their data, or that their data should be 'disregarded' because they hold a particular worldview;
- that he agrees that whilst motivations are 'irrelevant to the evaluation of their claims', motivations explain the persistence of parapsychological research *per se*
- that parapsychology has survived because of 'the quest to demonstrate that the materialistic worldview is incomplete' because it has not got the scientific substance to keep itself going otherwise (p. 634).

Before I comment on the devices Alcock used to cope with the 'negative evaluations' of his discourse (Potter & Wetherell, 1987, p. 212), I felt that it was

important to check the ‘facts’ that Alcock claimed had been misunderstood by the commentators who criticised him. On the first point, Alcock is correct that only his title, the last line of his abstract and a few paragraphs at the end of his article spoke directly either to the ‘search for the soul’ metaphor, or to mind/body dualism.

On the second point, in contradiction to Alcock’s claim, there were no sentences in his article in which he explicitly related either the ‘search for the soul’ or mind/body dualism to the persistence of parapsychology as a field. The point at which that became explicit was in his replies to the commentaries.

On the third point, he is correct that at no point in his original article did he accuse parapsychologists specifically of looking for evidence of a disembodied soul.

Although Alcock, in the fourth point, reiterated his belief that parapsychologists are against materialism, in points five, six and seven he argued that he did not believe a dualist perspective was incommensurate with science, nor did he explicitly state that parapsychologists’ data should be ‘disregarded’ because of the worldview they held. On these last points, he is also correct that nowhere in the original target article did he explicitly say that science and dualism were incompatible, nor did he explicitly argue that parapsychologist’s data should be set aside because of dualism or because their ‘hidden’ motivation influenced their judgements. However, in his target article, Alcock had expressed scepticism about those ‘modern parapsychologists [who] prefer to speak only of anomalies [because] ... if they are to be of continuing interest to parapsychology, [they] must ultimately involve some radically different relationship between consciousness and the physical world than that held to be possible by contemporary science’ (p. 556). Further, ‘Psi phenomena are defined implicitly in terms of their incompatibility with the contemporary scientific worldview’ (p. 556); ‘Indeed, if parapsychologists are right about psi, then the well-tested theories of physicists and neurologists are wrong ...’ (p. 562), and:

... finding explanations for ostensible anomalies is not what parapsychology is really about for most parapsychologists. If it were, much more effort would be made to try to find psychological and neuropsychological explanations for such experiences before even contemplating the radical psi hypothesis. ... If parapsychology is not primarily motivated to explore anomalies in an open-minded fashion,

what is its motivation? Why does parapsychology persist after a century of failing to produce compelling evidence of psi? Why does the psi hypothesis survive? (p. 564)

In his general comments, Alcock mainly denied the accusations his critics levelled against him. In most cases, his depiction of the explicit points made in his article can be confirmed in the text. But in some cases, such as the connection he drew between the ‘search for the soul’ and the ‘persistence of the field’, they could not be confirmed. From his perspective, Alcock’s critics over-reacted to what he felt were minor points in his target article whilst ignoring the empirical arguments that he did make. To some extent, this ‘misunderstanding’ might be explained by the fact that three structural elements that typically ‘frame’ scientific texts — that is, the title, the abstract, and the concluding statements — did feature this argument prominently.

As for the arguments of his critics, Alcock formulated these with varying degrees of accuracy. For example, he characterised one critic as describing him as ‘being “obsessed with dualism” to the extent that it is difficult to evaluate my arguments about the evidence for psi’ (Alcock, 1987a, p. 627). The critic had, in fact, said ‘Alcock’s special preoccupation with dualism (or spiritualism) makes it difficult, then, to know what to make of his claim that “parapsychologists have clearly failed to produce a single reliable demonstration of ‘paranormal,’ or ‘psi,’ phenomena”’. Later in the same commentary, Alcock’s points are depicted as ‘... right on the mark’ and ‘... clouded only a little by his excessive emphasis on spiritualism’ (Sanders, 1987, p. 607). Clearly the commentator felt that the language Alcock used complicated the reception of his empirical points. But ‘obsession’, Alcock’s formulation, lies at some emotional distance from the commentator’s term, ‘preoccupation’.

In responding to commentary in which he was depicted as a ‘generous inquisitor’, Alcock confused that commentator with another, attributing the specific points made by the latter to the former. Although he attributed one point correctly to that commentator ‘... [he] argues that I postulate *a priori* a materialistic universe that precludes the existence of paranormal phenomena’, he did not mention any of the more personal and condemnatory statements that that commentator had made (Alcock, 1987c, p. 628).

In other responses, Alcock described some criticisms correctly and responded to them. Other criticisms attributed to commentators can not be found anywhere in their published text. For example, Alcock correctly noted that one commentator accused him of making *ad hominem* arguments (i.e., Truzzi, 1987), and that another accused him of ‘...considering parapsychology to be a thinly disguised search for a metaphysical ideal and not really a science at all’ (i.e., Broughton, 1987). However, Alcock accused another two commentators of the *ad hominen* criticism when it was not mentioned anywhere in their text (e.g., Spanos & De Groot, 1987, p. 609-610).

In his specific responses, Alcock did not mention the comments on ‘the search for the soul’ or the dualism argument made by those critics who agreed with him (e.g., Broch, 1987; Tobacyk, 1987), nor by those critics who disagreed with him (e.g., Gergen, 1987) on the applicability of the argument to parapsychology. He did, however, mention a proponent’s acknowledgement (Tart, 1987) that parapsychology is a ‘search for the soul’ and ‘why not?’, another’s (Woodward, 1987) comment that scientific claims are possible from both the perspective of dualism and from that of materialism, characterisations of Alcock (Child, 1987) as a ‘theory truster’ and Rao and Palmer as ‘observation trusters’, an argument (Bauslaugh, 1987) that findings should be suspect if only ‘believers’ obtain them, and the criticism (Krippner, 1987) that Alcock had ‘... no hard data to support ... [his] speculation about the reasons for the persistence of parapsychology’. Finally, Alcock provided ‘data’ to support his characterisation of the parapsychological community as compromised by their anti-materialistic stance. That is, taking data from a study done in 1973, Alcock claimed that it had been found that 56% of the ‘Parapsychology [sic] Association membership’ are ‘already persuaded about a nonmaterial basis for life or thought’ (Alcock, 1987c, p. 629).²⁶¹

²⁶¹ Critics tend to use Allison (1973) and proponents do not. In parapsychology the study is not considered reliable, not only because it was a masters thesis supervised by an individual who is not known to have any familiarity with the field, but the results themselves have not been replicated and the author has no other publications of any kind.

Evaluating The Conversation

Prima facie, it is easy to see why individual respondents thought they perceived a strong bias against dualism, the use of a contingent repertoire to account for errors that Alcock believed parapsychologists have made, and why they thought the main point of Alcock's criticism was that parapsychology was the 'the search for the soul'. Paradoxically, it is also easy to see why Alcock felt he had been misunderstood, in that many of the specific points that were attributed to him by his critics were not explicit in his text.

If one looks at these texts in terms of the actions they perform (e.g., Horton-Salway, 2001), Alcock's use of 'the search for a soul' as a description of what parapsychologists 'do', also describes himself as someone who does not 'search for the soul'. There is a context in which the utterance operates — a consensual understanding of science as a meaning-making enterprise separate from religion — that serves to support Alcock's category entitlement as a scientist at the same time it seeks to deconstruct that same entitlement in parapsychologists.

Similarly, Alcock's use of 'mind/body dualism' and the attribution that parapsychology is in 'opposition' to the 'predominant materialistic worldview' serves to identify Alcock as someone who can be described as adhering to the 'predominant materialist worldview'. That he connects these two descriptions in more than one excerpt, as can be seen above, may indicate that he believes both descriptions are connected in a consensual understanding of science. The various ways in which these two points are reiterated across the text — as a subtitle, as a declarative description of the thesis of the paper in the last line of the abstract, as contained in both introductory segments and summary segments of the sections of the text — can be seen as 'versions' of these two actions. Alcock may want the discursive accomplishment of his text to be the act of sorting of himself and his audience into an undisputed category of 'scientist', and parapsychologists in an undisputed category of 'not-scientist', thus building his own authority as both an animator and a credible evaluator of the evidence whilst he undermines Rao and Palmer's authority as animators and as credible evaluators of the same evidence.

The individuals whose text I excerpted in the second turn ‘heard’ him, for example: ‘According to Alcock, parapsychology is really a thinly disguised search for a metaphysical ideal, and not really a science at all’ (Donderi, 1987, p. 582). But for some, the action was perhaps too extreme as they, in turn, formulated Alcock’s statements into something less strongly-put, such as the attribution to Alcock of the point of view that the ‘... fundamental problem [is] ... that parapsychologists are not really involved in scientific research’ (Akers, 1987, p. 567).

Others who ‘heard’ him, simply disagreed. One member of the critical community, for example, was not willing to characterise Rao and Palmer as compromised by dualism, rather he preferred to describe the ‘openness to dualism’ he perceived in Rao and Palmer’s article as ‘hints’ (Benassi, 1987, p. 570).

Commentators who decried the contingent arguments in Alcock’s target article, were in effect accusing him of violating the norm of disinterestedness in that Alcock was seen to hold ‘an unquestioned premise’ as he moved into ‘his investigation’. Further depictions of Alcock as someone who did not make a representative judgement and ignored data, in effect, deconstructed Alcock’s category entitlement, stripping him of his scientific identity (e.g., Hövelmann, 1987, p. 593). This particular formulation, it should be mentioned, was made by a proponent.

Alcock’s reactions to these responses can be seen as an attempt to regain the footing he meant to occupy in his target article (e.g., Goffman, 2001; Wood & Kroger, 2000, p. 102), that of an animator of ‘facts’ in the natural world, and not as the ‘author’ of an idiosyncratic, contingent attack on parapsychology. Alcock may have believed he had done a competent job of conveying his evaluation of parapsychology as an empirical judgement but clearly some of the commentators found his text less than convincing. He disavowed, in the strongest terms, a number of the formulations his critical commentators offered, e.g.: ‘Yet very little of my target article actually had anything to do with this subject’ (Alcock, 1987c, p. 627); ‘There is clearly some misunderstanding (and obviously I must take the blame for that) about just what it was that I was saying when I discussed a search for the soul’; and ‘Nowhere did I say — nor would I suggest — that parapsychologists are poor scientists simply because they take a dualistic or any other metaphysical position’. (p. 627).

What can be inferred from the fact that Alcock felt the need to initiate two more rounds of commentary in *Behavioral and Brain Sciences* in 1990 and in 1998 in which the titles make clear that the ‘search for the soul’ and the characterisation of dualism as incompatible with science were still salient and problematic for his critics? Without analysing that text, it may not be too much to claim here that he understood that the work he intended his original text to do had not been accomplished and perhaps, that the damage that failure had inflicted on his footing still needed repair.

Conclusion

In this chapter I have attempted a relatively brief and superficial DA of the solicited debate that appeared in *Behavioral and Brain Sciences* in 1987. I have focused on some aspects of James Alcock’s target article that provoked criticism amongst both his own colleagues — that is, members of the critical community — and amongst proponents of parapsychology. I also included some examples of Alcock’s attempts to cope with negative evaluations of his points. In these texts, as elsewhere, the use of contingent arguments in scientific texts were either generally rejected and/or had an impact on the form of at least some of the commentaries that followed.

If one looks at these exchanges with the analysis of the Spencer/Marks correspondence in Mulkey’s (1985) volume in mind, one might say that just as Spencer had done in the correspondence with Marks, Alcock attempted to establish an interpretative inequality in his characterisation of parapsychology as ‘the search for the soul’ that would allow him to inoculate successfully the wider scientific audience — the readers of *Behavioral and Brain Sciences* — against any empirical arguments his ‘opponents’ might make, thus establishing his view as an accurate, scientifically reasonable depiction of parapsychology. Perhaps some of the surprise I perceived in his responses to those commentators who criticised him — especially to those who were members of his own critical community — flowed from the realisation that his claim to a superior position in the debate was, at least to some extent, unwarranted, and that the metaphorical and philosophical points he took to be self-evident were, in fact, contested both as descriptions of the scientific enterprise in general, and of scientific practise in parapsychology.

Further, as difficult as these rejections of his ‘position’ and his ‘premises’ might have been, had they been published in the less visible critical venue, *Skeptical Inquirer*, or the almost *invisible* parapsychological venue, the *Journal of Parapsychology*, the undermining of Alcock’s entitlement might not have provoked him to attempt repair so strenuously. One wonders what fodder for the DA cannon could be gleaned from a conversation with Alcock about this exchange, in the style of the conversations Mulkey conducted with ‘Spencer’ on the subject of his correspondence with ‘Marks’.

This thesis draws to an end, and in Chapter 8, I will attempt to weave the threads of these disparate analyses back into whole cloth, as well as to speculate on the future of an expanded, multi-method, truly ‘herteroglossic’ parapsychology.

CHAPTER EIGHT

CONCLUSIONS

The Findings

In this thesis I have used three methodologies, more or less deeply, to investigate criticism and response in the English-language academic and scientific literature of parapsychology. Not only have I done this because I have enjoyed a thirty-year career in the field and thus have some stake in the outcome of such debates, but I am also a research psychologist with an interest in science studies, and it seemed to me that controversy in parapsychology could be a fruitful ground for analysis. If this had not been the first time that such an analysis was attempted, it might have been possible to focus only on a comparison of the methods of rhetoric of science with discourse analysis, or to narrow the enterprise further to only a rhetoric of science *or* discourse analysis. But because this is the first time that this vast and detailed terrain has been mapped, I felt that it was necessary to approach it from all three methodologies: (1) taking a historical long-view of the terrain; (2) analysing a key debate using rhetoric of science; and (3) testing the waters for future studies of text and talk using discourse analysis. I felt that each of these three forays were necessary to define both the scope and the depth of the materials as a prelude to future research. Further, these three disparate methods have served, I believe, to illustrate the usefulness of expanding the methodological and theoretical repertoire of parapsychology itself.

The historical analysis merely presented an overview of the development of psychical research and parapsychology in the Anglo-American world. Both a 'biography' of the field as it evolved in both contexts, and a historical overview of texts that reviewed criticism of the field, were presented. The historical analysis illustrated some surprising features of that evolution. For example, it can now be argued that psychical research and parapsychology in Great Britain was 'naturally occurring' in the sense that its organisations were founded by individuals with deep, personal interests in the field, and that the flowering of the research/teaching sites of the field happened there without a great deal of conflict or upheaval. In the U.S., on the other hand, the impetus

for the initial development of a society to investigate parapsychology seemed to arise out of a sense of civic duty amongst scientists. It did not seem to be propelled, at least initially, by deep personal interests in the topic. Further, the rise and fall of research/teaching sites in the U.S. have been marked by conflict, seemingly embodied in a wrenching progression from one ‘coup’ to another, whether the ‘revolutionaries’ were members of the general public, or scientists with a very different views of the proper future of the field than that held by their colleagues.

Further, the chronological survey of reviews of criticisms, especially because it was organised by the discursive notions of ‘contingent’ and ‘empiricist’ repertoires, not only illuminated recurring themes in the criticism, but also illustrated the ‘hearability’ inherent in empiricist arguments. That the ultimate divide between some critics and some proponents rested on contingent factors — differences in worldview being the most prominent amongst these — was not surprising. But that critics and proponents could elevate the level of discussion by keeping strictly to the empiricist repertoire was surprising.

The rhetorical analysis as presented here merely ‘represented’ what was in actuality a deeper analysis of all the published materials from 1934 to 1944, a text set of more than 100 articles, reviews, and letters to the editor, in addition to the two main books.²⁶² It was *not* surprising that, firstly, the analysis underscored the fact that, in this controversy at least, empiricist criticism dominated, and resolution of those criticisms not only refined scientific methodology in parapsychology but also contributed to wider scientific debates about probability theory and the use of statistics. It *was* surprising, however, that, secondly, both the style and the structure of the original document, as well as the early articles published by Rhine and his team, complicated the reception of the empirical points they attempted to make. This was so, I believe, because of the serious disjuncture between the style of the reports written by the Duke team and what was considered to be standard, effective scientific prose in psychology. Further, also surprising to me, both the historical and the rhetorical review of these texts showed that

²⁶² Because of the scope of the materials treated, the first version of Chapter 5 was over 150 pages long.

extra-parapsychological disciplinary issues complicated the debate, not the least of which was the utter lack of intertextual support for Rhine's claim that parapsychology was a 'branch of psychology'.

The discourse analysis showed that, even in the formal setting of a solicited debate in a prestigious scientific journal, it was possible to find discursive devices in text that have otherwise been discovered in talk. Not only did the primary texts attempt to set up category entitlements sufficient for persuasion in a scientific context by, amongst other things, constructing a footing that was distanced enough from the 'natural world' to attribute agency to that which exists 'out-there', but elements of the turn represented by the commentaries also illustrated how such a footing could be undermined if the contingent repertoire was used in service of it, instead of an empiricist one. Further, in the third turn, common devices used to repair footing or deflect undermining were apparent. Because I am new to the method, all of these findings were surprising to me.

As an 'insider' I found it interesting that the historical analysis gave me a different perspective both on the possible causes of the field's current decline in the U.S. and its growth in the U.K. than I had formed previously. The rhetorical analysis underscored for me a significant lack in our science practise as parapsychologists that I was not as keenly aware of before, that is, that whilst many of us claim that we are psychologists first, and parapsychologists second, not enough has been done to anchor our work in psychology proper, not to mention in other relevant branches of science. Further, the discourse analysis gave me a new perspective on specific critics and on the ways in which we should construct our 'identity' talk and text so as to be persuasive. Finally, the combination of these three perspectives have given me a glimpse of how the boundaries of parapsychology can, and must, be expanded if its future findings are to be useful to science as a whole.

The Methods

From the beginning this thesis was set up to be a 'three study' project. Most of the theses that my cohort at Edinburgh had either defended or were conducting whilst I did my research included three or more separate experiments. There was a sense that not

only did the experience of doing a doctoral degree deepen, but the usefulness of the results expanded, when an approach to a problem relied on convergent lines of research. For some this meant investigating a phenomena by conducting three experiments that each expanded their methodological and theoretical repertoire. For others this meant, finding three different theoretical perspectives, or three different methodological approaches to the same hypothesis.

Because of my background in history of science, because I had been an ‘insider’ for so many years, and because it had never been done before, Prof. Morris and I decided that I should choose three methods to approach the history of criticism and response. (Citation analysis and content analysis were tried on and discarded relatively early on.) Ultimately it was decided that I would: (1) use my training in history to organise the whole problem terrain so as to contextualise controversy in parapsychology; (2) use my interest in science studies, especially in the rhetoric of science, to analyse what we both felt was the most important controversy in the history of experimental parapsychology; and (3) using the resources that the Department presented coupled with my life-long avocational interest in language, try to discern whether or not discursive psychology could be profitably applied to selected texts in the parapsychological literature.

Each of these three methods might have been deepened and expanded to form the substance of a separate thesis in and of themselves, and each suggest what might be fruitful areas of future research.

History of Science

Chapters 2, 3 and 5 are imbued with some of the more superficial methods of the history of science. Each provide a sweeping analysis that rests on published documents. In Chapter 2, I relied on a variety of monographs, Mauskopf and McVaugh’s (1980) ‘biography’ of psychical research and parapsychology, autobiography and biography, and notes taken from private conversations with various individuals who were first-hand observers of historically-meaningful moments in the field. In addition, I relied on my own experience as a working parapsychologist. The historical picture that I was able to

construct from a juxtaposition of these sources was sufficient for the purpose of this thesis because history was a tool rather than an end in itself.²⁶³

To conduct a study that would be sufficiently deep to be called ‘history’, I would have had to first develop an historical hypothesis such as ‘What social and institutional forces account for the differences in the development of psychical research and parapsychology in the U.S. as opposed to the U.K.?’ or ‘Was the stated identification of parapsychology as psychology under Rhine merely a rhetorical device, an aspiration, an organisational principle or a source of contention amongst Rhine and his team, and did this identification impact on the social integration of the Duke Parapsychology Laboratory into the wider Duke community, and/or did it have an impact on the cognitive content of the science practice that was established at Rhine’s laboratory?’

Second, I would have needed to use such primary sources as manuscripts, correspondence, structured interviews with principles, organisational records, in the first instance to contextualise psychical research and parapsychology in their national contexts, and in the second instance, to flesh out as accurate a picture as possible of Rhine’s laboratory and its place in the Duke community and/or of its epistemic production and its role in its close social context and in the field as a whole.

Less ambitious historical hypotheses would also have been useful such as querying why Joseph Jastrow spent so much of his career criticising parapsychology, or why Rhine’s laboratory disengaged from Duke and how Duke ‘itself’ felt about that disengagement.

These are all interesting historical questions, and I personally would love to tackle those that deal with Rhine’s laboratory and its place in both the histories of science and the history of parapsychology.

²⁶³ In a real sense what I have done here is ‘Whig’ history: That is, I have set my hypotheses and gathered sources to answer questions that are of interest to practitioners in the discipline I examined. Whilst disciplinary history has its place, ‘real’ history is like ‘real’ rhetorical analysis or ‘real’ discourse analysis: It is done for its own sake.

Rhetoric of Science

Chapter 5 presents the results of the limited rhetoric of science study that I did on the texts of the ESP controversy. Rhetoric of science can be done from a qualitative point of view, or, like Gross, Harmon and Reidy's (2002) empirical study of textual elements, it can be highly quantitative. Because of the limitations of space, I was not able to include a great deal of the analysis that I did on the specific development of the controversy as it was reflected in the articles and letters to the editor that I surveyed. There were also questions that I could not answer. One such was: Did the structure and style of scientific reports in parapsychology really differ from those in psychology of the time? Because no one else has done the quantitative study on texts in psychology that Gross and his colleagues did on texts in the natural and physical sciences, this question remains unanswered. In addition, for the thesis chapter, I reported only on a few of the possible textual elements that might have been chosen for analysis, and I did not do a strictly rhetorical analysis in which the *topoi* themselves were the focus of the study.

In the future, I would very much like to see psychological texts analysed using the methods of Gross and his colleagues, as well as more close studies of other controversies in parapsychology. The way in which I used certain methods drawn from the rhetoric of science in this thesis were sufficient to make the point that rhetorical factors have complicated the debate along the way, and that rhetoric of science is a useful method by which to analyse controversy in parapsychology, both as a whole, and in other specific instances.

Discourse Analysis

Because the effective use of discourse analysis is very much a craft skill, and I was not planning on doing a discourse analytic thesis, I did not take the time to add a craft apprenticeship to my reading in discourse analysis. Consequently, my understanding of the method and its utility is at an early stage. However, because the thesis was meant to be a three-study project, and the goal in this section was to illustrate that discourse analysis can effectively and profitably be applied to documents produced in the context of controversy in parapsychology, I believe that I provided the illustration sufficient to my purpose. I am aware, of course, that I have *used* discourse analysis for

another purpose *besides* discourse analysis, which differs from what would have been presented had I been *doing* discourse analysis for its own sake. Not only would the results of my analysis of the documents at hand have been deeper had I taken a different tack, but they would also have been amplified, given that I dealt with only a small subset of the social action that played out in these texts.

In addition, during the course of my thesis, I compiled and, then for space reasons, discarded, texts written for other less formal venues than *Behavioral and Brain Sciences*, such as Alcock and Palmer's exchange in the conversational debate journal *Zetetic Scholar* as well as articles they wrote for their own communities. In addition to the works of Alcock and Palmer, CSICOP's magazine, *Skeptical Inquirer*, contains a number of types of discourse dealing with the controversy over parapsychology, from brief articles and essays to columns of news on the paranormal, and letters to the editor from general readers. Further, both CSICOP and the SPR have routinely taped the lectures given at their annual conventions for several decades. These are available for analysis, and would lend themselves readily to discourse and conversation analysis.

The Future of Parapsychology

Although I came to this thesis as an 'insider' in parapsychology with, I believed, a collateral identification with psychology and to a lesser extent, history, I have left this thesis with a wider view both of what parapsychology does and should entail, and of the shape and proper place of its problem domain. Parapsychology, especially in its experimental iteration, has built itself self-consciously on the example of the physical sciences. 'Physics envy' is certainly not the sole province of parapsychology but it has had an impact on the shape of the field. Over the decades the methodological challenges that spontaneous case reports present, for example, have been solved by setting aside these reports as uninteresting and *merely* anecdotal. Laboratory tests of supposed psychic functioning have been fruitful (e.g., Radin, 1998) but there is something to be said for a focus on experience (e.g., Alvarado, 1996; Irwin, 2003).

Over the course of my career I have done experimental work, but I have also focused on the psychological correlates of both 'success' in the laboratory and unverified pencil-and-paper reports of experiences. Focusing on psychological correlates is not only

inherently interesting to me, it is a career choice as well, because from that approach, the ontological reality of the phenomena being reported is not in question. There are those in the field who think any kind of parapsychology that does not, metaphorically-speaking, have 'ESP in the data' is illegitimate. But it seems to me that if the ontological status of the reported phenomena is fundamentally at issue — and it is — then it behooves us to know as much about those who claim to experience it as possible.

Through this thesis, I have come to a position on sociological and psychological approaches to parapsychology as a social institution, to its discourse whether it expresses experience or science practise, and to other aspects of its contextualised existence within the 'world', that is analogous to my position on the utility of psychological correlates. That is, at the same time the field needs to pursue its epistemological goals, it needs to be mindful that its productions are not devoid of context, nor are its institutions. The context is as important as the data, and each should be systematically examined, within whatever theoretical or methodological frame is useful. On a solely pragmatic level, I was pleasantly surprised at the result of a conversation analysis that was done on the interaction between experimenter and participant in ganzfeld experiments (Wooffitt, 2003). Just a small point amongst the findings such as discovering that participants provided more elaboration of the images they had 'seen' when experimenters preceded their questions with 'And you said' can have a profound effect on the level of detail available for independent judging and other analyses of the data amassed in the ganzfeld. Had conversation analysis not been brought to that interaction, the ability of some experimenters to elicit sufficient detail for successful independent judging would have continued in its epistemological limbo as an unexamined feature of that catch-all phenomena, the 'experimenter effect'.

My own work has taught me that reflexivity is a skill that all parapsychologists should learn. Our need to be mindful of the presence of artefact in our experiments and of the confounding influence of personal beliefs has made us, I think, more reflexive than many mainstream scientists. But our reflexivity is nowhere near complete. Further studies from the sociological, historical, rhetorical and discourse analytic perspectives can serve to make us more mindful of the subtle elements that help or hinder our

experiments, our interpretative skills, and our efforts at within- and cross-disciplinary persuasion.

I am forced to return to Prof. Morris who instilled in my doctoral cohort the idea that the central questions of parapsychology comprised a problem domain best attacked from a variety of interdisciplinary and multidisciplinary perspectives. To some extent it is 'only' a matter of embracing the 'heteroglossia' that is parapsychology, but how hard that can be when it is difficult enough to master a single disciplinary language!

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