

B R A I N H E M I S P H E R E D I F F E R E N C E S

I N P A R A N O R M A L A B I L I T I E S :

W I T H S P E C I A L R E F E R E N C E T O T H E I N F L U E N C E
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D E C L A R A T I O N

I declare that this thesis is my own work

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A B S T R A C T

The lack of repeatable experimental results has been identified as a major impediment to practical scientific acceptance of parapsychology. An attempt was made in this research to devise a repeatable experimental method. It was based on historical clues and contemporary evidence suggesting that the hemispheres of the brain may have different roles with regard to the processing of paranormally acquired information.

Using techniques adapted from non-parapsychological investigations of hemisphere asymmetries of function several experiments were carried out and the results of these supported the hypothesis of asymmetrical hemisphere involvement in ESP.

Parallel investigations by the author on other topics suggested a possible role of the experimenter in using paranormal ability to obtain results in parapsychological experiments regardless of the hypothesis. Such evidence was found by recent reviewers to permeate the history of experimental parapsychology and recent independent theoretical proposals implicated the experimenter in obtaining experimental results by a psychokinetic effect on the random statistical fluctuations in subject-produced data.

An experiment was conducted to investigate the indications of psi-based experimenter effect and the results suggested that the author himself was capable of affecting even highly automated experiments. Careful replications of the earlier successful experiments on hemisphere differences and ESP were undertaken to determine if the effects noted then were due to the subjects or to the experimenter. The results provided no evidence to indicate that the earlier findings were due to the subjects or to hemisphere differences.

In conclusion the argument is advanced that much of the findings of experimental parapsychology may consist of similar psi-based experimenter effects and that it is necessary to begin investigations of the psi-ability of the experimenter in achieving his results. When the psi component of the experimenter can be assessed a large portion of the variability in experimental results may be accounted for.

CHAPTER I

I N T R O D U C T I O N

CHAPTER I

I N T R O D U C T I O N

If there is one research development which, more than anything else, could bring about the general acceptance of parapsychology by the scientific community and the integration of its work with that of the more traditional scientific disciplines it is the discovery of a reasonably repeatable experimental paradigm. Accordingly, to develop such an experiment should be considered one of the primary goals of experimental parapsychology.

To the critic of parapsychology such a position would seem obvious since for him it has not been established that paranormal abilities even exist. Very early in the history of experimental parapsychology R.A. Fisher (1938) made the point that, rather than very high odds against chance for any individual experiment, what is relevant to the establishment of the facts of nature would be the demonstration of the reliable reproducibility of the phenomena. It has been noted by Ransom (1971) that the most frequently voiced criticism of parapsychological findings is that they are not generally replicable. In the absence of this repeatability critics such as Scott (1976) can claim that parapsychology is not so much a science dealing with lawful regularities but merely a collection of historical events. Even West, a parapsychologist with a long-standing interest in the field, has admitted, "*In fact,*

parapsychology consists of a series of historic demonstrations of miracles rather than a body of scientific knowledge" (West, 1971, p.156). Beloff (1973a, p.288) has echoed this point. Each parapsychological finding or demonstration of ESP cannot be separated from the particular personalities and circumstances of the occurrence. Certain critics (Price, 1955; Hansel, 1966; Scott, 1974) take this to mean that it must be a case of fraud on the part of one or more of the individuals concerned if no other explanation can be found since they feel that ESP is a priori impossible.

The need for repeatability is not so obvious to many parapsychologists. Indeed, a survey taken of persons attending the 1971 convention of the Parapsychological Association (Schmeidler, 1971) indicated that almost 90% of the respondents thought that ESP was so well established that any further work which asks the question 'Does ESP occur?' would be uninteresting. It could be inferred that many would therefore regard the repeatable experiment necessary to answer once and for all the question 'Does ESP exist?' as being not worthy of pursuit, probably because they feel that goal has already been achieved. These findings prompted Beloff to comment "*Had we now become such a tight little in-group . . . that we had lost all touch with reality?*" (Beloff, 1972, p.189).

Surveying the writings of parapsychologists over the past couple of decades indicates that among this group there are three schools of thought regarding the question of repeatability. The first school believes that repeatability of psi phenomena has long

since been achieved and it is only prejudiced critics who say that it has not. The second school admits that repeatability has not yet been achieved but argues from the point of view of the philosophy of science that it is not strictly necessary to have it. The third school states flatly that we have not yet managed to develop the repeatable experiment and we desperately need to do so before any real progress can be made.¹

It has been argued by parapsychologists of the first school that parapsychological findings are well replicated and that there already exists such a thing as a repeatable experiment. Randall (1975, p.178) claims, "*Everyone of the major findings of parapsychology has been replicated many times*". Thouless (1971, p.222) makes a similar statement, "*. . . the basic parapsychological findings are being constantly confirmed by repetition*". Louisa Rhine (1967, p.25) makes the curious comment, "*As a matter of fact, each time an experiment in ESP achieved a firm statistically significant level, the repeatability of an ESP test was affirmed*". When it comes to providing for the reader an example of the repeatable experiment it is usually the Sheep-goats effect (Randall, 1975, p.100; Thouless, 1972, p.53), although Pratt (1973) rather weakly puts forward the repeatable subject, i.e. Stepanek.

Can these rather extravagant claims of the first school be supported? It should be noted that few parapsychologists

¹ What follows is not intended to be an exhaustive review of all those parapsychologists who have commented on the repeatability problem but merely a representative sample of the more recent writers on the subject.

would openly make such claims since there is little agreement on what constitutes the 'basic findings' of parapsychology. With few exceptions the findings in parapsychology are inextricably linked to a few personalities - either experimenters or subjects. The pitfalls of such a situation have often been noted by critics of parapsychology and recently were dramatically and sadly illustrated by the discovery that the leading figure in what appeared to be the most promising line of research of the decade had been faking his results (Rhine, 1974b). Although Pratt (1973) wonders why parapsychologists alone among scientists should be expected to defend themselves against charges and suspicions of fraud it seems obvious that such a state of affairs will continue until the 'findings' of parapsychology can be extricated from the particular personalities or local conditions to which they now seem bound.

The proffered candidate for the repeatable experiment is the Sheep-goats effect first noted by Schmeidler (1945) and a subject of research for over three decades. A careful review of the literature, however, indicates that this is hardly a strong candidacy. Palmer (1971), in a paper reviewing the published Sheep-goats findings with an eye to demonstrating the strength of the effect, was forced to list eleven failures to replicate in comparison with only six confirmations in experiments claiming to tap the Sheep-goats variable and using standard methods and analyses. While he goes to some length to demonstrate that this is all part of an overall pattern he at least acknowledges that on top of all the reported failures to replicate he knows of further unpublished ones and he grants that the question of self-censorship of negative findings can be raised

to provide suspicion of yet more failures. With repeatability of this order is there any wonder that all of parapsychology's critics and even some parapsychologists fail to notice it?

Of all the first school adherents the most carefully researched arguments for the existence of repeatable experiments can be found in Honorton's 1975 Presidential Address to the P.A. (Honorton, 1976). Here Honorton tabulates the reported experiments in a few areas of parapsychology (not including the Sheep-goats work) and argues on an ad hoc basis that parapsychology's replication rate is really quite good. Yet despite his seemingly generous allowances for 'unreported' failures to replicate in his calculations his conclusions that certain areas of parapsychology are highly replicable fail to ring true to more critical workers in the field for several reasons. His allowances for unreported failures are not particularly generous when one considers that the major parapsychological journal has followed, since its inception in 1937, the deliberate policy of publishing only results of experiments which turn out positively or at least provide evidence for ESP (Rhine, 1975a).² His tabulations seem a bit suspect since when a quick check was made of the 'microdynamic PK' situation

² One particular example may be of interest. The Journal of Parapsychology published a marginally significant anpsi experiment by a diligent Dutch researcher (Schouten, 1972) yet the Journal consistently refused to publish eleven similar studies done by the same investigator at about the same time which failed to yield significant results (Schouten, pers.comm.). These further studies belatedly saw the light of day in 1976 (Schouten, 1976) though they would have been of considerable use to those trying to evaluate the status of the anpsi work prior to the Levy affair (see Appendix B).

(being the most recent) it was clear that Honorton had overlooked at least two published failures to replicate³ which appeared in ample time to be included. Finally, even if one is willing to accept Honorton's 'replication rates' not even the most incautious parapsychological experimenter would credit these figures with any predictive value. Only a few years ago even cautious observers were impressed by the very high replication rate for Animal psi and psi studies but since it became apparent that the results may have been fraudulent (Rhine, 1974b) not even Rhine's own investigators can get the animals to perform (Terry, 1976). There is, in fact, no way of predicting whether a psi experiment, even when of the 'high replication rate' type, will work even on a percentage basis. This is particularly apparent in the Edinburgh Parapsychology Laboratory where a number of investigators have consistently failed to get results in all of Honorton's chosen areas.

From the second school on the repeatability question comes arguments that experimental repeatability is not necessary. Basing their arguments on theoretical propositions of philosophers of science, frequently Kuhn (1970), parapsychologists such as LeShan (1966) and Pratt (1974) would argue that the demand for repeatable experiments is a carryover from an overthrown scientific paradigm, namely Newtonian physics, and should not be forced onto our field of inquiry. They would argue that it is necessary to create a new paradigm in which the unrepeatability of psi phenomena can be incorporated. Needless to say no suggestions for the

³ Thouless, 1971; Randall, 1974.

framework of that paradigm are forthcoming. Pratt (1974, p.144) suggests ". . . we should drastically shift our emphasis and no longer insist upon achieving strict predictability and repeatability in parapsychology". Pratt claims he is not abandoning the concept of lawfulness in parapsychology and suggests as an alternative to repeatability parapsychologists adopt the 'law of recurrence'. It would seem that if any attempt was made to define this law or make it a working hypothesis (which Pratt does not attempt) then one would quickly return to repeatability. Similarly, LeShan (1966) after complaining that the repeatability argument is a product of an outdated world view, proposes a redefinition of a repeatable experiment to limit it to work with a single subject. LeShan's repeatable experiment might be acceptable but for the fact that he wishes it to be linked to a single subject, a situation which consistently puts parapsychologists in a weak position regarding critics.

Brier (1973) has argued convincingly that the concept of repeatability is largely irrelevant to the question of whether or not parapsychology may be considered a scientific endeavour. He makes the point that the distinguishing quality of a science is its method. While parapsychology to a large degree does adhere to a scientific method, the ultimate test of its scientific status for Brier is the meeting of the Popperian (1966) criterion of refutability. In this sense parapsychology could have all the replications it wishes and still not be acceptable from Popper's (or Brier's) point of view if it is unable to formulate its hypotheses in a way that is capable of falsification.

It is quite possible that parapsychologists of the third school, such as Beloff (1973a, 1973b, 1974), the most persistent advocate of repeatability, Johnson (1976) and Murphy (1971), would be willing to concede Brier's point yet in no way mitigate their calls for the repeatable experiment. This is largely because such calls for repeatability appear to stem from the pragmatic view that the repeatable experiment would be a cure-all for parapsychology's ills. Whether or not on a philosophical level parapsychology can be considered a science is of little consequence when one considers the following facts: Virtually no major scientific journal accepts papers on parapsychological topics, except perhaps to 'debunk' parapsychological claims,⁴ and, on occasion, they can be shown to be prejudiced against such reports (Honorton, Ramsey and Cabbibo, 1975). None of the major sources of scientific research funds in the English-speaking world have opened their coffers for parapsychologists, and, perhaps as a result of that fact, comparatively few scientists are actively working in the field. Such a situation continues despite the affiliation in 1969 of the Parapsychological Association with the American Association for the Advancement of Science and a number of polls which indicate an openness toward psi phenomena among scientists (e.g. New Scientist, 1973). Obviously something is missing and Beloff (1974), p.9) makes it clear what that is,

"Before any claim, great or small, can be accepted definitively as a fact at least one or other of the following conditions must be met. Either we must be in a position to explain the phenomenon in question to an extent where we can predict when it should and when

⁴ A recent exception was the grudging publication in Nature of the paper by Targ and Puthoff (1974) on their work with Uri Geller. Of some interest also is the editorial, pp.559-560.

it should not occur or, failing any such theoretical understanding, we need overwhelming inductive grounds for believing that such and such procedures can be relied upon to produce such and such effects even though no one can say why this should be so. Only then can the particular observations and experiments upon which the claim is founded cease to have more than an historic interest and disputes about the honesty or competence of experimenters cease to be relevant. For, at that point it is open to any critic or doubter to try replicating the findings for himself."

Murphy (1971, p.4) makes a similar point in discussing the means of attracting scientific attention for the 'unclassified' events of parapsychology,

"This is my point then: the joint necessity for replication and rationality, and the weaker the one leg on which to stand, the more important it is that the other can bear the weight to be borne."

In addition to the obvious advantage of silencing forever those critics who feel it necessary to accuse parapsychologists of fraud and obviating the necessity for the elaborate anti-fraud procedures advocated by Rhine (1974a,b) the development of a repeatable psi experiment would have the following practical advantages. It would provide a reasonably stable source of psi with which experimenters could test hypotheses not merely through statistical tests of significance but against the ultimate test of scientific acceptance, at least according to Popper, that of falsifiability. It would overcome one of the major obstacles to involvement by other scientists which is that of having tried and failed to find psi phenomena. Finally, it would have specified some of the variables which are relevant to experimentally produced psi phenomena.

Those calling for repeatability in parapsychology are at pains to point out that they are not after an absolute 100% success rate but merely a 'reasonable' rate of repeatability which would carry with it a certain degree of predictability. Beloff (1973, p.198) is willing to settle for 50% repeatability if it is a predictive 50%.

Of course it remains a possibility that not even a reasonable degree of repeatability can be achieved and then it may be time to reconsider the methodology being applied. However, it is far too soon to even pretend that parapsychologists have exhausted all the possibilities available to them within the existing scientific framework. Indeed, parapsychologists have hardly started when compared with other sciences. On the other hand, Rhine's (1971) complementary argument that it is too soon to expect parapsychologists to have the means to develop a repeatable experiment seems particularly counterproductive if it is meant to dissuade parapsychologists from attempting this goal.

For this investigator the arguments for an all-out effort to develop a repeatable psi experiment far outweigh the arguments put forward to say that it is too early to expect such a development or that since we have not managed it yet we are never likely to do so. Thus the underlying motivation behind the research contained herein was the hope that application of recent findings from an area of psychology and the employment of a novel methodology to parapsychological investigations might go some distance toward the elusive goal of the repeatable experiment.

A LIKELY CANDIDATE FOR A REPEATABLE METHODOLOGY

In the recent history of parapsychology there have been several occasions when a technique or idea borrowed from psychology or developed within parapsychology has held out the hope of becoming a repeatable experiment. In addition to the Sheep-goats design of experiment some recent candidates for repeatability honours have been the animal ESP experiments, alpha rhythm and relaxation studies, ganzfeld techniques and, of course, dream telepathy. As we have seen, none of these ideas have lived up to their early promise and some are never likely to.

Through the latter half of the 1960's right up to the present one of the most active areas of research in psychology has been that of brain hemisphere specialization. Getting much of its impetus, though not its inception, from the 'split-brain' patients studied by Sperry and his colleagues, the idea that the left and right halves of the brain tended to be specialized for certain types of cognitive activity fostered hundreds of experiments aimed at finding out what the various specialities were. The particular constellation of right hemisphere abilities which were emerging from the research suggested to a few investigators that this hemisphere was the seat of the intuitive, non-rational side of man's consciousness. On the basis of this rather superficial characterization of right hemisphere activity Ornstein in 1972 suggested that paranormal communication may take place in the mode of consciousness which he would associate with the right hemisphere.

Despite the large amount of research in the area of hemisphere specialization and hemisphere differences virtually none of this found its way into parapsychology. Even the considerable popular impact of Ornstein's book failed to engender much interest among parapsychologists.⁵ Upon more detailed examination the case for a relationship between paranormal abilities and the differing specialities of the halves of the brain turned out to be rather more substantial than the suggestions inherent in Ornstein's somewhat inadequate conceptualization. Indeed, an examination of the parapsychological literature in conjunction with the accumulating findings of brain hemisphere research revealed not only that paranormal phenomena had been linked to the right hemisphere in the very early days of hemisphere research but also that contemporary parapsychological research has provided a good deal of evidence suggesting a relationship with hemisphere specialization.

There was in existence, then, a considerable body of evidence which at least suggested that the hemispheres were not equally involved with perception of the extra-sensory sort and, in fact, a case could be made for the right hemisphere being better at it. Yet, through the entire history of experimental parapsychology virtually all ESP experiments made use of responses necessarily controlled by the dominant left hemisphere. What if paranormal communication is regularly apprehended by our not quite verbal right hemisphere but unable to force its way into verbal consciousness? Could this be a clue on the way to curing the repeatability problem? Obviously it was worth a try.

⁵ An exception is Braud's (1974) use of Ornstein's left and right hemisphere 'cognitive modes' as a predictor variable in an ESP experiment.

By this time psychological research into hemisphere specialization had provided a number of experimental techniques which, at least to some extent, separated the responses given to various cognitive tasks by each hemisphere. Through such methodologies researchers were able to determine whether the hemispheres differed in their ability to perform certain cognitive tasks and if they did, which of the two seemed better at the kind of processing which the task represented. Taking into account the specific requirements of psi research it seemed feasible that one or more of these techniques could be adapted to investigate whether or not paranormal abilities were lateralized.

The first series of experiments in this direction got off to a shaky start, but in the end provided quite dramatic evidence of unequal hemisphere involvement in ESP. The technique used in this series was a relatively simple adaptation of the traditional five-choice guessing test, but instead of using verbal responses the subject indicated his choice by feeling and lifting one of five three-dimensional wooden shapes. A rough access to each hemisphere was thus achieved since control and sensation in the fingertips is primarily within the opposite brain hemisphere. The effective 'separating' of the hemispheres was done by having the subject perform a task, reading, which is known to occupy only one hemisphere, the left. Not only did the results suggest that the right hemisphere was better at dealing with ESP but they came complete with one of Rhine's cherished 'Signs of psi' (Rhine, 1974a), namely, an unanticipated sex difference conforming to the

latest hemisphere specialization findings and discovered over a year after the original experiment had been completed.

One of the main features of the method used in the first series, one that was, in fact, a prime consideration from the beginning, was that it was simple enough for any suitable laboratory to carry out. Preliminary reports on the work were published in the hope, never particularly realized, that other laboratories would undertake replications. In the meantime work was undertaken to see if the effect could be demonstrated in an experimental paradigm which eliminated two of the possible weak-points in the first series. These were the fact that responses were observed and manually recorded by the experimenter and that the shapes had verbal names which could have 'confused' the hemisphere differences approach in a way which would have worked against the hypothesis.

The second series, though employing a higher order of technology, also made use of a basically simple design. The experiment looked for a psi influence on a subject's response time caused by an agent receiving an advance warning of the stimulus. The subject's responses were obtained from both hands in order to access each hemisphere as in the first series. In the first experiment of this series the reading task was not employed and it turned out that there were no differences between the hands. When the reading task was employed, in the second experiment, there was a significant psi effect which was apparently manifested differently in the responses of each hand. The findings nicely corroborated the earlier series and also displayed the characteristic sex difference in the psi effect.

With the obviously encouraging results achieved to that point plans were set in motion to extend both methodologies in the expectation of refining the psi effect and the hemisphere differences effects. From the point of view of having a completed and tidy research adventure to report, one which boldly sets forth a hypothesis and then provides a series of experiments which conclusively proves it, it would have been nice had the story ended here. In earlier days perhaps it would have done, and no one would have been the wiser. J.B. Rhine, certainly influential in parapsychological circles has argued, "*There is no way to explain away a successful experiment by a failure with an ever-so-careful chance replication*" (Rhine, 1975a, p.141). Presumably Rhine could have forgiven (or perhaps even applauded) the experimenter who would have chosen to ignore the remaining part of this research. The motivation behind this work was not, however, simply to add another unexplained and unconfirmed 'effect' to the many already found in the literature. To ignore the fact that the effect quite suddenly evaporated for the very same investigator who 'discovered' it and to fail to investigate the reasons why this happened would be to do a disservice to the motivation which was the root of the research in the first place, the search for repeatability.

PARALLEL DEVELOPMENTS

By 1975 John Beloff's Edinburgh Parapsychological Unit had achieved a world-wide reputation for its consistent inability to replicate any of parapsychology's 'major findings', and, indeed,

this state of affairs was often a source of amusement to fellow parapsychologists.⁶ It was certainly not for want of trying, however, and this investigator had the good fortune to collaborate several times with an exceptionally able colleague in attempting to replicate some of the more promising lines of research. These attempts included replications of animal psi experiments (before the Levy affair), Schmidt-type random number generator work, and Stanford's release-of-effort effect. All such replications brought null results, but to the inquisitive eyes of the two investigators there were subtle signs in the data which suggested that something other than the obvious was involved. None of these 'hints' even approached significance but they stuck like unresolved chords in the minds of the investigators.

It appeared to the investigators that they somehow influenced the results of the experiments despite their deliberate attempt to avoid the pitfalls of the Rosenthal (1966) type of experimenter effect. The problem seemed to go beyond that but it was difficult to pinpoint how. However, about this time two theoretical proposals filtered into the thinking of the researchers at the Edinburgh Unit. Both models of psi phenomena, one mathematical and the other quantum mechanical, focus on the same aspect

⁶ As an example, after Beloff read (in absentia) this investigator's report of the shapes series (see Chapter III), K.R. Rao rose to ask a question to the effect, "*How is it that now you have finally tarnished your reputation by reporting a successful psi experiment?*" which provoked general laughter. Beloff hastily pointed out that the work had been done by an American, provoking even more laughter.

of the psi situation - the feedback of the event which has occurred. This is the crucial factor because without feedback of what happens the subject could not be said to exert any psi effect. The models further predict that not only does the psi effect reside solely with those who see the results of their psi efforts (feedback) but that possibly anyone who also receives this feedback may have a paranormal influence.

Quite apart from these theoretical models of psi two excellent review papers appeared (Kennedy and Taddonio, 1976; White, 1976a,b) presenting a large body of suggestive evidence for a psi-based experimenter effect. Unfortunately both papers failed to realize the implication of the feedback oriented models of psi for the role of the experimenter in parapsychological research and fell short of drawing the obvious conclusion: that one person who always receives feedback in psi experiments, and, indeed, the person perhaps most motivated to see psi effects in the experiment is none other than the experimenter himself. If either of the models is correct, or at least if the role of feedback has been correctly assessed, then it is probable that the observed psi effects in experiments with unselected subjects are at least as much, if not more, due to the experimenter than the subjects.

Though Schmidt has outlined this possibility as early as 1972, such ideas did not gain much currency among parapsychologists. This is, no doubt, due to the realization of the subversive nature of such proposals regarding the traditional interpretation of all

of parapsychology's 'effects'. These feedback models of psi require nothing less than a radical reinterpretation of all parapsychological findings taking into account the degree of the experimenter(s) involvement both in terms of feedback received, expectations held and perhaps emotional involvement during the experiment and the analysis of the data.

To an investigator like myself with a quite unheard of record of success in psi experiments for the Edinburgh Unit such proposals were a cause for concern, to say the least. Clearly the issue could not be avoided. An experiment was designed to examine the possibility that subjects could influence results of an ESP test taken at one time in accordance with expectations they were given after the test was completed. In some ways this was a miniature of the experimenter's situation except that he does not enjoy the luxury of having no expectations until after the experiment.

This experiment involved the giving of each subject two identical runs of a disguised ESP test. After the tests were completed the subjects were given reason to expect one of the runs (randomly assigned by the computer) to be higher than the other and then they were given their results. The surprising finding was that their results conformed to their expectancies to a statistically significant degree. Even more disconcerting for the experimenter was the fact that data which were neither seen nor even known about by the subjects, and which were only known to the experimenter as a final t-test result, displayed a highly significant deviation from chance.

According to the models, as these data were unknown to the subjects, the effect must reside with the experimenter who had as his feedback the final test result. This seemingly unlikely situation was rendered considerably more plausible by a paper published at the very time the experiment was being run. Walker (1976) provided equations derived from his quantum mechanical model of psi to show that, in fact, a higher score can be obtained when the feedback provided is only a final result. This prediction was remarkably like what actually did happen.

These rather dramatic findings suggesting a major role for the experimenter in the production of psi effects in the experimental data warranted a confirmation study. A precise replication of the experiment was undertaken with all conditions kept exactly as they had been for the original several months earlier; all things except for one - the experimenter. No longer could the experimenter avoid the very strong suggestions that he was a potent psi source in his experiments. During the replication and the examination of the results the experimenter was very conscious of this possibility and, as so often happens in parapsychology, the 'effects' completely disappeared. The replication study produced not a hint of a significant effect in any of the conditions which provided significant effects in the original.

What did this say about the experimenter's psi involvement in his experiments? Could one afford to ignore the whole study since the replication failed to produce significant effects? To this experimenter it seemed that one could do that only at his scientific peril. This particular research episode seemed much

like a miniature of parapsychological research with the 'effects' mysteriously disappearing when the pressure was on to replicate them. With the recent reviews of experimenter effect, the new models of psi, an increasing awareness by experimenters of their possible psi role in their experiments,⁷ and experiments such as the one just discussed, a new view was beginning to take hold in some quarters of the parapsychological community. This view was that the real clues for solving the repeatability problem may lie with the experimenter and his psi abilities.

UNFINISHED BUSINESS

Where did all this evidence for the experimenter's involvement via psi leave the findings for hemisphere lateralization? Unfortunately no other laboratory had made an attempt to replicate the findings of the shapes series of experiments so the question was still open.⁸ Obviously, before more time was invested in this particular line of inquiry the effect would have to be reconfirmed to at least the investigator's satisfaction.

The hemisphere specialization work was carried out by an experimenter naive to the possibility that he might be the source

⁷ At the 1976 Parapsychological Association Convention at least five well-known parapsychologists were willing to admit, off the record, that they had come to suspect a large factor in their getting experimental results was their own psi ability.

⁸ One study claiming to extend this work has been reported (Maher and Schmeidler, 1977) but whether it was even related to the Shapes series had to be questioned in a letter to the editor (Appendix C).

of the observed effects. The experimenter was no longer so. If he succeeded in replicating his earlier, successful work then there would be further support for the lateralization effect being a real one. If, as apparently happened in the expectancy manipulation experiment, the experimenter had 'frightened' himself out of producing effects, a situation observed with some special subjects, then the lateralization effects would probably disappear as well.

In fact, the effects did disappear. An attempt was made to replicate the highly automated reaction time experiment. The results were not even reminiscent of the first version. Subsequently an attempt was made to replicate the most successful of the shapes series. Though, again, every possible care was taken to insure this was a faithful replication the results in this case as well did not even resemble those of the experiment it was meant to replicate.

So what follows is the story of a promising hypothesis gone sour, but from the ruins of that hypothesis emerge the most hopeful clues yet to the solution of the parapsychologists' problem - repeatability.

CHAPTER II

HEMISPHERE DIFFERENCES

AND PSI PHENOMENA

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HEMISPHERE DIFFERENCES AND PSI PHENOMENA¹

(Why it seemed like a good idea)

Hemisphere specialization, or hemisphere differences, depending on one's point of view, could easily be said to head the list of 'most popular research or discussion topics' in psychology for the past fifteen or so years. Yet for all but the last few years this important area of research has been generally ignored by parapsychologists. At the inception of this research seemingly no parapsychologist had concerned himself with the implications which brain hemisphere specialization might hold for parapsychology.

One of the most striking features of the human brain is the fact that the greater part of it is neatly divided into two halves, the temporal lobes, with their mirror image cerebral cortices. Cadwallader has related that despite the gross similarity of the cerebral hemispheres our ancestors as early as 3000 B.C. were aware of the fact that there were functional differences between them. (Cadwallader et al., 1971). Contemporary hemisphere research traces its roots back to the later half of the 19th century when investigators such as Broca, Wernicke and Dejerine had established quite clearly the dominant role of the left hemisphere for language processing. The right hemisphere received little attention at this time although by

¹ With the permission of my supervisor parts of this chapter have been published (Broughton, 1975).

1864 John Hughlings Jackson had ascribed 'retino-ocular' functions subserving visual perception to the right hemisphere (Taylor, 1958). For the most part, however, investigators of that era and through the early part of this century were not sure what the special functions, if any, of the right hemisphere were since there appeared to be nothing to parallel the clear demonstration of the left hemisphere's superiority for language functions.

In the early 1940's the first known commissurotomy operations (disconnecting the cerebral hemispheres by surgical section of the corpus callosum and the other cerebral commissures) were performed by A.J. Akelaitis but his and others' subsequent investigations of the patients suggested that the operation had produced no consistent deficits in their psychological functioning. These muted conclusions naturally prompted no big upsurge in interest in hemisphere specialization. Interest did continue, however, and through the next two decades a considerable amount of research into the effects of focal lesions of the cerebral hemispheres provided reports of various intellectual and perceptual impairments from such injuries. For the most part these findings fit the pattern established by the early workers in the area.

The veritable explosion of interest in hemisphere specialization in evidence today can be said to have had its beginnings in the reports of the now famous work of R.W. Sperry and his colleagues with the 'split-brain' patients. About 20 years after Akelaitis began his work the commissurotomy operation was reintroduced by P.J. Vogel as a means of relieving severe and chronic epilepsy in

a number of patients who were subsequently exhaustively studied by the Sperry team. As with the earlier commissurotomy patients the investigators did not detect any gross changes in the general behaviour of these patients. However, using a number of clever yet simple techniques Sperry's team was able to demonstrate that the hemispheres were capable of operating quite independently of one another (Sperry, Gazzaniga and Bogen, 1969). Furthermore the accruing evidence for the relative specialization of the left and right hemispheres received dramatic confirmation in their experiments.

HEMISPHERE SPECIALIZATION FINDINGS OF POSSIBLE RELEVANCE TO PARAPSYCHOLOGY

It would be redundant to attempt here a review of the great bulk of research which has been amassed on the topic of hemisphere differences since several good volumes are already available.² To make a long story short the picture of hemisphere specialization which has emerged from the many studies of both split-brain patients and normal subjects is that the left hemisphere specializes in discrete information which is processed sequentially (language being the obvious example) while the right hemisphere is better at more diffuse, holistic, gestalt information which is processed simultaneously (such as visuospatial ability).³ This simplification must inevitably

² Gazzaniga, 1970; Dimond, 1972; Dimond and Beaumont, 1974; Kinsbourne and Smith, 1974.

³ Generalizations such as this refer to right handers. For left handers, who constitute between 5% and 15% of the general population depending on the manner of classification (Annett, 1970), the picture is not the same. Apparently non-familial left handers perform as right handers in dominance tests while familial left handers show much less marked laterality effects (Zurif and Bryden, 1969). Most laterality studies simply exclude left handers unless they are of particular interest in the investigation.

obscure the many more subtle differences observed within and between the hemispheres but it is a widely accepted working classification.

Hemisphere specialization research has made a number of interesting observations on the brain's processing of information which have relevance for the parapsychologist and which will be examined here.

INDEPENDENT PROCESSING BY THE DISCONNECTED HEMISPHERES

One of the most surprising findings emerging from the research on the human commissurotomy patients is that the two halves of the brain, under appropriate conditions, can function independently of one another. They are not, however, equipotential with respect to all cognitive abilities. The right hemisphere is all but incapable of using language to express itself. When a tachistoscope was used to flash a stimulus in the left visual field thus directing the input to the right hemisphere the patient was unable to verbally report what had been seen although the hand corresponding to the visual field could correctly point to the stimulus or retrieve an object as a response with no difficulty (Gazzaniga, 1967). Likewise if two stimuli were flashed, one to each hemisphere, a verbal report would reveal the stimulus which went to the left hemisphere, but a drawing made with the left hand (out of sight) would show the other stimulus which had gone to the right hemisphere (Sperry, 1968). Tests using 'Chimeric stimuli' (two contrasting stimuli halves joined at the midline as if one, e.g. the left half

of a female face and the right half of a male) tachistoscopically presented to the split brain patients indicated that they can effectively perceive two things in the same place at the same time. Each hemisphere was quite sure that it had seen a complete face, although whether it was a male or female face depended on which hemisphere was being interrogated (Levy, Trevarthen and Sperry, 1972). Numerous other experiments have demonstrated the capacity for independent processing by the disconnected hemispheres and these findings have implications for the theoretical interpretations of hemisphere differences treated below.

RIGHT HEMISPHERE FUNCTION

Of the findings relating to the specific aptitudes of each hemisphere that of most interest for parapsychology is the right hemisphere's clear superiority for visuo-spatial abilities. It was the split brain patients who provided dramatic confirmation of slowly accumulating findings. Shortly after the operation each patient displayed a marked inability to copy simple line drawings of shapes with the right hand but could do this reasonably well with the left. The left hand, however, could no longer write at all whereas the right hand had little difficulty (Bogen, 1969a). In experiments using the chimeric stimuli where the patients were allowed to make their responses by pointing to the stimulus in a multiple choice arrangement they showed a marked preference for the right hemisphere's input (Levy, Trevarthen and Sperry, 1972) indicating that this hemisphere prefers to process this visual material.

These consistent findings regarding the right hemisphere's superiority for processing visuo-spatial material and the gestalt-like manner of its information uptake have been repeatedly confirmed in experiments using normal subjects. Using tachistoscopic presentation Levy (1974) has found that while verbal materials are recognized more easily in the right visual field (left hemisphere) meaningful forms (like faces) as well as meaningless forms are better recognized in the left visual field (right hemisphere). Durnford and Kimura (1971), and Trevarthen and Levy (1973) have found the right hemisphere superior in such activities as depth perception, line orientation, scanning, and visual point location. Galin and Ornstein (1972) have reported that in encephalographic studies the amount of alpha frequency, generally an indication of a relatively quiescent state in the brain, tends to increase in the hemisphere opposite to the one which is being engaged by a task in which it has relative superiority. Thus during monitoring of normally conducted cognitive tasks (reading contrasted with Koh's Blocks) they found a greater percentage of alpha wave activity in the right hemisphere for the language task and in the left hemisphere for the visuo-spatial task.

Several other findings regarding the cognitive abilities of the right hemisphere are of interest to parapsychology. Dreaming appears to be largely a function of the non-verbal hemisphere. Bogen (1969a) reports that the commissurotomy patients have noted the absence of dreams after the operation and suggests that this is due to the disconnection of the dream area from the verbal output area. Humphrey and Zangwill (1953) reported on several patients

who spontaneously reported that they had ceased to dream since suffering brain injury. In all cases the lesion was right sided or (in one case) bilateral. They suggest, ". . . just as the aphasic is unable to express his thought in propositional form, so the agnosic patient may fail to express his ideation at the lower level of fantasy and dreams" (p.325).

Dimond and Beaumont claim to have experimental evidence demonstrating that the right hemisphere is more creative than the left. They used a word association test in which the stimuli were presented to each hemisphere using a tachistoscope and found that while the response latency was the same for each hemisphere, words directed to the left hemisphere resulted in responses which were significantly more common than the responses which were elicited from words to the right hemisphere. They interpret their findings as indicating a greater participation of the right hemisphere in the creative aspects of thought. They see the role of the right hemisphere in this respect as "*concerned with the more inventive, exploratory, improvisatory aspects of mental activity*" (Dimond and Beaumont, 1974, p.75).

ANATOMICAL DIFFERENCES

Although many parapsychologists do not seem to concern themselves with the role which the ordinary physical brain may play in paranormal perception it should be of some interest to parapsychologists that there are anatomical differences between

the cerebral hemispheres. Their possible relevance to parapsychology will be discussed later.

Until fairly recently it was thought that the hemispheres were roughly identical, though in mirror image. Various gross measurements of size, specific gravity, weight, failed to demonstrate any significant differences (von Bonin, 1962) though many investigators might have expected them owing to the prevailing conception of one major and one minor hemisphere. The observed differences seemed to be of a purely functional type with no apparent structural reason why language should be lateralized to the left. This view was further supported by the demonstrably great plasticity of the child's brain in which either hemisphere can assume the function of the other should it be damaged.

Geshwind and Levitsky (1968), however, have demonstrated that there are in fact large structural differences between the two hemispheres and they are in precisely the area one would expect them to be. Specifically, they examined inside the Sylvian fissure, an area concealed from superficial examination, and found that the Planum Temporale in the central portion of the posterior speech area of Wernicke was considerably enlarged in the dominant speech hemisphere. In other words there is considerable enlargement of the speech area in the hemisphere which handles speech processing. At the present moment there is discussion as to whether the percentages of left larger, right larger, and equal cases represent what should be expected on the basis of findings

from other studies. Further studies confirmed the anatomical findings and Wada (1969) has reported that these differences can be observed even in a foetus indicating that such differences are present at birth and thus must be genetically programmed.

A more inferential conclusion regarding anatomical differences between the hemispheres comes from the work of Semmes (1968). In extensive studies of brain injured war veterans she and her associates have found that in the left hemisphere deficits in the performance of a specific task could be identified with a specific lesion, while there was no such localization in the right hemisphere. A lesion of the left hemisphere might interfere with the performance of a particular task but a lesion of the right hemisphere, if small, might have little or no observable effect, but if large, might affect a whole range of tasks. They conclude that the left hemisphere is anatomically specialized for discrete, focal information processing and the right hemisphere is more diffusely organized for tasks requiring simultaneous processing of information, such as spatial perception.

Thus two independent lines of investigation suggest that there are actual structural differences which may at least guide, if not determine the lateralization of cognitive functions to the areas which can 'service' them best.

THEORIES OF CORTICAL FUNCTION AS
RELATED TO HEMISPHERE LATERALIZATION

A brief attempt has been made to summarize some of the facts of hemisphere specialization which are obviously pertinent to parapsychology and the links will be made more explicit later. Because of the vast scope of the research in this area it is of particular interest to parapsychologists to know what are the prevailing theories of hemispheric operation, as put forth by the leading investigators, so they may be able to relate these to what seems to be known about psi phenomena both in the lab and from the study of spontaneous cases. At the inevitable risk of oversimplification it can be said that for the most part theorists fall into two camps, one which stresses the independence of the two hemispheres and takes an almost dualistic approach to the problem, and the other which stresses the integral cooperation of the hemispheres for normal consciousness.

As might be expected the approach which emphasizes the 'two minds' of man comes from the investigators most closely associated with the commissurotomy patients of Bogen and Vogel. However, this approach is not new and can be traced, according to Bogen (1969b), to A.L. Wigan who in 1844 stated,

"I believe myself then able to prove - 1. That each cerebrum is a distinct and perfect whole as an organ of thought. 2. That a separate and distinct process of thinking or ratiocination may be carried on in each cerebrum simultaneously."

(Wigan, 1844)

He had developed this conviction from a series of autopsies in which persons who to all intents and purposes appeared perfectly normal unto death were found to have one entire hemisphere missing. Bogen continues tracing this line of thought through Brown-Sequard who in 1877 expressed a conviction similar to Wigan's but Bogen points out that the dualist position eventually took a back seat to the dominant hemisphere hypothesis which followed the work of Hughlings Jackson and others in the latter part of the 19th century.

Recently Zangwill (1976) has added the name of Fechner to the ranks of the early dualists. Fechner strongly believed in the equipotentiality of the hemispheres and argued that their anatomical continuity was essential for the unity of consciousness. In the event that the hemispheres were divided, an entirely hypothetical situation for Fechner, there would be a duplication of consciousness.

"The two cerebral hemispheres, while beginning with the same moods, predispositions, knowledge and memories, indeed the same consciousness generally, will thereafter develop differently according to the external relations into which each other will enter."

(Fechner 1860, cited in Zangwill, 1976)

The best known of the modern exponents of this dualist view of the cerebral hemispheres is Roger Sperry, leader of the original team of psychologists who investigated the famous split-brain patients who underwent precisely that operation which Fechner thought impossible. Sperry has firmly and frequently stated his interpretation of the evidence from his investigations of the split-brain patients as demonstrating the existence of two separately functioning minds within one brain. Not long after the first operations had been performed Sperry reported,

"Everything we have seen so far indicates that the surgery has left these people with two separate spheres of consciousness. What is experienced in the right hemisphere seems to lie entirely outside the realm of awareness of the left hemisphere. This mental division has been demonstrated in regard to perception, cognition, volition, learning and memory."

(Sperry, 1966, p.299)

This conclusion is undoubtedly based on the many observations made on the split-brain patients which indicated that often one hemisphere could carry out intelligent, purposeful activity while the opposite hemisphere was totally unaware of what was happening. These findings closely paralleled those of Myers and Sperry (1953) in which it was noted that chiasm and callosum sectioned cats could learn one discrimination task with one eye-hemisphere pair and an entirely different discrimination task with the other eye-hemisphere pair and apparently never be bothered by the discrepancy.

The second major proponent of the dualist position is Joseph Bogen, a member of the original investigating team and an assisting doctor for many of the operations. He draws both from the animal and the human split-brain patient experiments to support his view *"that each of us has two minds in one person"* (1969b, p.151). In arguing the case for his own particular characterization of hemisphere laterality Bogen goes on to say, *"We may then conclude that the individual with two intact hemispheres has the capacity for two distinct minds."*

Bogen's views have been embellished and popularized by Ornstein (1972) who, while not having a large impact on other

investigators (see for example Zangwill, 1976, p.309) has had considerable influence on the public.

Michael Gazzaniga, also a member of the original team, to some extent shares Bogen's position.

"All of the evidence indicates that separation of the hemispheres creates two independent spheres of consciousness within a single cranium, that is to say, within a single organism."

(Gazzaniga, 1967)

While there is little doubt that experiments with the split-brain patients have demonstrated complete independence of operation regarding certain perceptual tasks and the associated cognitive processing these quite obviously represent a 'special case' and it is questionable as to how far the concept of 'two minds' can be applied to normal, un-split brains although this is clearly implied in some of the foregoing examples.

That the concept of two minds in one brain becomes blurred when applied to normal humans can be seen in the interpretation offered for the normal operation of the brain in which each 'mind' seems to refer to no more than the demonstrated special processing capabilities of that hemisphere.

While Sperry has not particularly extended his views on the consciousness of split-brain patients to include that of normal persons Bogen and Gazzaniga have.

Bogen (1969b) argues for a view of the normal brain in which the differing capacities of the hemispheres function as exact complements of one another. The left hemisphere specializes in propositional thought while the right hemisphere specializes in what Bogen has named 'appositional' thought. He treats the ontogenesis of the complementary halves only cursorily and he avoids altogether any detailed consideration of the manner in which the hemispheres work together thus presenting a view which is largely descriptive rather than explanatory.

Gazzaniga has attempted to provide some of the missing parts for the complementary function hypothesis. He has proposed (Gazzaniga, 1970) that the ontogenesis of lateralization proceeds as follows: There is evidence that infants are effectively split-brained because of the late development of callosal fibres therefore manipulations of objects in one hand lay down their associated engrams in the opposite hemisphere. Naturally there is considerable duplication of engrams in the two hemispheres. Gazzaniga 'predicts', although he does not say why, that the right hand will take the lead in manipulating objects and since soon its left hemisphere would be in a position of having more knowledge it could ask more questions thus creating a circularly reinforcing situation favouring one hemisphere. As the callosum develops and the hemispheres become connected there is no longer a need for the 'less competent' hemisphere to store the duplicate information so this is suppressed by the dominant hemisphere.

A later version of Gazzaniga's theory (Gazzaniga, 1974) incorporates the recent findings regarding structural asymmetries in the speech area which might give an innate advantage to one hand for tactual exploration thereby removing the awkward necessity of having to make the a priori predictions of the earlier version. The important feature of this version is his more elaborate consideration of the means by which the hemispheres work. He presents evidence suggesting that one of the main functions of the callosal linkages is to permit the left hemisphere to inhibit the duplicate and potentially conflicting cognitive processes in the right hemisphere.

Other investigators have preferred to stress not the obvious duplication of consciousness in the special cases which the split-brain patients represent but the manner in which the hemispheres cooperate in normals to generate unitary consciousness. Kinsbourne (1974a), like Gazzaniga, views the primary means of hemisphere interaction as being dynamic inhibition. He suggests a developmental sequence similar to Gazzaniga's in which one hemisphere, having taken the lead in a type of processing, finds it more efficient to actively suppress duplicate activity in its opposite number via the callosal fibres. Marshalling an impressive amount of evidence Kinsbourne continues that the observed asymmetries in cerebral function are not the result of one hemisphere being totally incapable of performing the various functions in which its partner excels but rather due to the fact the hemisphere dominant for certain functions holds the opposite one in check while it does the necessary processing. The mutual inhibition hypothesis of

hemisphere functioning has received support from his own work with gaze shift (Kinsbourne, 1972) as well as that by Trevarthen and Sperry (1973).

Kinsbourne further develops (1974b) an earlier proposal (Kinsbourne, 1970) that attention is 'time-shared' between the hemispheres. Only one hemisphere is fully activated at any instant and when a task demands the speciality of the other hemisphere the first must be deactivated and the second activated in turn. Kinsbourne (1970) interprets the delay in reaction time studies where a stimulus is presented to the non-competent hemisphere as representing the time taken to arouse the competent hemisphere to process the stimulus rather than it representing merely a less efficient processing by the non-competent hemisphere. Ledlow, Swanson and Levy (1973), report evidence to support this view. Galin and Ornstein (1972), on the basis of EEG studies also suggest that rather than integrating functioning of the hemispheres a person rapidly shifts back and forth between them.

It appears that the main value of Kinsbourne's interpretation of the evidence lies in his exposition of the means in which the two hemispheres work together to form an efficient, unified consciousness through the mutual inhibition of unwanted duplicate functioning.

Of the various theories of hemispheric operation the most generalizable is that offered by Trevarthen. His theory draws not

only on the many findings from the studies with the split-brain patients and normals but also on his work which has demonstrated that at least one important aspect of visual perception remains unified even when the hemispheres are split (Trevarthen and Sperry, 1973). Trevarthen argues that there are two principal strategies for information uptake which are employed by virtually all of the human perceptual systems and it is in the differences between these strategies that the clues to hemisphere specialization may be found. The two strategies of information uptake can best be illustrated by vision. Trevarthen points out that there are two types of vision. What he has termed 'ambient vision' refers to a highly proprioceptive type of seeing (similar in sensory-motor function to the subconscious perception of space relations in orientation and locomotion) which serves to perceive overall structure and detect changes in that structure in the peripheral area of vision in which behavioural acts may take place. In contrast to this there is 'focal vision' in which a very small part of the visual field is held fixated for a short time, normally a fraction of a second, thus isolating perceptual objects for detailed scrutiny. Ambient vision then serves to scan the environment and derive the next focus of attention by reacting to the appearance of a source of information. Focal vision strategy, on the other hand, invents foci according to the structure of a mental image and checks to see if it is supported by appropriate stimuli present in the field. *"Focal vision is more concerned with assimilating information according to our goals, and is less likely to accommodate to unexpected events in the environment"*, he points out (Trevarthen, 1973). At another time he draws the parallel

between ambient vision and the overall cognitive process which Neisser (1967) calls the 'preattentive process' (Trevarthen, 1974).⁴

As his analysis of these two forms of information uptake relate to the hemispheres Trevarthen says:

"I interpret what we know at present to indicate that the right hemisphere is more concerned with establishing intelligent priorities in the pre-focal field, and with an assessment of the composition of the field in relation to the sum total of the contents of immediate awareness. We may deduce that its memory functions are organized to assimilate and retrieve a record of personal or egocentric experience in its fullest and least rationalized or categorized form. The left hemisphere is more selective within the field of experience seeking to establish and use categorical universals, especially those more related to the semantic categorizations of speech, and to apply them in solving problems with thought, and in communicating."

(Trevarthen, 1973)

THE POSSIBILITY OF LATERALIZED PSI ABILITIES

When a parapsychologist confronts the findings on hemisphere lateralization two things seem apparent: (1) If paranormal information, an extrasensory signal, somehow gets into the nervous system of a human being then, for it to come out in any of the familiar ways, this signal will have to pass through the cerebral cortex, if it is not there from the start. Therefore, might it not be subject to the same sort of laterality effects as normal cognition? (2) ESP as it appears in the laboratory is at best flighty and unstable. Is there a possibility

⁴ Kinsbourne (1974b) has also proposed a very similar view of hemisphere functioning but as it is largely derived from Trevarthen's it will not be treated separately.

that this may be related to the fact that virtually all parapsychological tests expect the response to come from the dominant hemisphere which simply may not fully share the paranormal information?

Considering parapsychology's past record regarding repeatable experiments the above considerations alone might be sufficient to initiate some investigations in the matter but certainly at the time this project was conceived there had been no attempts to examine ESP in relation to the observed asymmetries of the brain. This of course means there were no direct forerunners upon which this project could build. However there was certainly no lack of evidence strongly suggesting a link between paranormal abilities and cerebral asymmetries.

It is fair to say that, prior to this work, no parapsychologist had seen a link between psi phenomena and hemisphere differences. This is not, however, true of the earlier investigators of paranormal phenomena who went by the name of psychical researchers. Several references dating to the early days of hemisphere asymmetry research explicitly link psi phenomena to one half of the brain.

EARLY REFERENCES TO BRAIN HEMISPHERES AND PSI

As early as 1855, in a periodical called the North American Review, a journal more literary than scientific although the boundaries were less clearly drawn in those days, an article entitled

"Modern Necromancy" reviewed two books concerned with the spiritualist movement of the day. The authors⁵ dismiss pointedly the spiritualist hypothesis⁶ and argue that there must be a more reasonable, scientific explanation of the phenomena associated with the seance room. The authors introduce the idea that the phenomena are probably due to the medium rather than discarnate entities. They then go on to discuss the 'duality of the brain' with sufficient competence to suggest that they may have been influenced by Wigan, to whom Bogen has referred. Among other things the authors argue that dreaming could be a state of consciousness experienced in one half of the brain whereas normal consciousness is in the other, thus explaining why the immediate affairs of one state rarely continue through to the next.

The authors consider cases of dual consciousness in which one person alternates between two personalities and identities. They suggest that these cases may be due to an abnormal alternation between the halves of the brain with the 'foreign' personality being a realistic and extended dream state. Mediums in trance may be in a similar state they claim. The cause of the cases of dual consciousness they believe is due to "*some peculiar condition of animal electricity, or sensitivity to electromagnetic influences*". They

⁵ The author(s) were not identified by name.

⁶ Part of their declamation is at least as well put as T.H. Huxley's (1871) memorable reply, "*The only good that I can see in a demonstration of the truth of 'Spiritualism' is to furnish an additional argument against suicide. Better live a crossing-sweeper than die and be made to talk twaddle by a 'medium' hired at a guinea a seance.*", and should be recorded here. Commenting on the mercenary character of Spiritualism the authors declare: "*What kind of an immortality - how dignified, how happy - can that be which can be disquieted by the incantations of hireling women, and subjected to the impertinent teasing of any idler who has more money than brains.*", (North American Review, 1855, p.514).

then make an analogy between the human body and a galvanic battery and suggest that ordinarily the body produces enough power to keep one hemisphere active. In exceptional cases an excessive charge might build up and activate the second hemisphere which would account for the spirit personality and the paranormal phenomena of the seance room which they feel resemble various displays of electrical phenomena. As far as the 'communications' go they suggest,

"It is conceivable that an electromagnetic communication may be established between the intensely stimulated brain of the medium and the brains of those comprising the circle . . ."

(p.522)

As an historical illustration of the speculations of learned men this is interesting and though in a sense 'quaintly naive' the authors touch upon points which remain relevant. A more directly useful analysis of cerebral localization and psi phenomena was to come in 1885 in a paper by one of the well known investigators of the early Society for Psychical Research, Frederic W.H. Myers.

Myers had made a detailed study of graphic automatism or trance writing using the planchette, a sort of writing ouija board popular with the Victorians. To use the device an operator, in a trance, would place his or her fingertips (of both hands) on it and when a question was asked the planchette would write out the answer. Graphic automatism, while regarded as a means of communication with the spirits of the dead by the spiritualists, was by the more enlightened psychical researchers of the time thought to be due to

the unconscious mind of the operator. In cases where the communication gave evidence which could not have been known normally by the operator the early investigators preferred telepathy between living persons (usually the sitters) as the explanation rather than the idea of spirit communication.

Drawing upon the 'latest' work of Charcot, Bernard and Hughlings Jackson, Myers presents a detailed analysis of the striking similarities between the types of utterances produced by the planchette and the efforts to communicate made by persons who have suffered injury or disease in the language hemisphere of the brain.

One of the more amusing characteristics of the planchette utterance is particularly interesting in light of the well documented evidence regarding the verbal proclivities of the minor hemisphere in aphasic patients.

"There is another peculiarity of the early stages of automatic writing which it has somewhat embarrassed Spiritualists to explain. 'Planchette', automatists often testify, 'is sadly given to swear'. Especially when the hand is exhausted by a long and somewhat barren effort the word devil will sometimes be written over and over again with an energy which shocks the unsuspecting writer."

(Myers, 1885, p.44)

Other similarities to communication attempts by aphasics which he cites include the repetitive or recurrent nature of trance writings and the fact that frequently the operator seems 'word-blind' to what is being written by his own hand in the trance state and often has to ask his 'hands' to clarify illegible words.

Myers proposes that what emerges in the planchette communications is a picture of a 'secondary self' of the operator. This secondary self is a mixture of the unconscious remembrances of the operator and, in some cases, telepathically obtained thoughts of others. Graphic automatism in the cases which he has studied is of course the product of "*sane and healthy persons for experimental purposes*" and has more the character of an accomplishment than the onset of a disease. Thus Myers suggests that for the automatist this difficult communication via the right hemisphere represents an advance by which the secondary self makes itself known whereas for the aphasic the right hemisphere route is the only one "*still open for the primary self*".

"I urge that if automatic writing . . . be originated, not by the half-insane cunning of the self familiar to us, but by the rudimentary efforts of a secondary self to emerge into objective activity; - then it is likely that there will be some order discernible among the manifestations; - some 'seat of election' among the cerebral faculties, in which this secondary self will be found to establish itself most perceptibly, - some 'path of least resistance' by which its externalisation will be most commonly effected.

"And what I am at present maintaining is that in cases where automatic writing occurs during the waking consciousness of the primary self, then the right hemisphere is, to a certain extent, the 'seat of election' of the secondary self, and the word-seeing and word-writing centres of that hemisphere form, to a certain extent, the readiest path of externalisation for its inward activity."

(Myers, 1885, p.57)

Myers implies that when this secondary self is privy to telepathic communications (and he cites several examples) these may find their expression through the right hemisphere.

Another reference from the days of psychical research comes from Nandor Fodor's monumental Encyclopaedia of Psychic Science. In a discussion of the famous medium Eusapia Paladino (1854-1918) under the heading "Trance" Fodor relates a point rather reminiscent of the point being made by the editors of the North American Review:

"Lombroso called attention to the fact that Eusapia Paladino, who was usually left handed in sittings, became right handed in one seance and Dr. Morselli himself became left handed. This confirms Dr. Audenino's hypothesis of transitory left handedness in the abnormal state . . . The left handedness seems to indicate the increased participation of the right lobe of the brain in mediumistic states."

(Fodor, 1966, p.390)

From these three instances it can be seen that the possibility of a disproportionate influence on the part of the right hemisphere in psychic phenomena was certainly in the current thinking of some of the investigators of the early days. Unfortunately this awareness did not continue through to the birth, if one may call it that, of parapsychology in the 30's. Since the early work just reviewed there has been little or no reference to hemisphere differences in parapsychological research.

CONTEMPORARY SUGGESTIONS OF CEREBRAL ASYMMETRIES IN PSI

Recent research does, in fact, have much evidence which at least suggests that there may be hemisphere asymmetries, however, the contemporary case for unequal hemisphere involvement in ESP

arises not from any particular line of research but from the convergence of a number of experimental and theoretical approaches to the problem of explaining psi phenomena.

VISUAL CHARACTER OF ESP

One element common both to the many cases of spontaneous ESP as well as to the most successful experimental approaches has been the explicitly visual nature of the paranormal experience. Louisa Rhine (1962) has pointed out that most cases of spontaneous ESP experiences are either 'intuitive', in which the percipient has a feeling regarding a person or event, or hallucinatory and/or dreamlike with the majority of these being visual in nature. The preponderance of dreamlike and hallucinatory type visual experiences in the large number of cases which she catalogued in 1953 led her to suggest then that ESP seems to favour the more 'primitive' sensory functions as against the highly schematized rational thinking of normal waking consciousness (L. Rhine, 1953).

Paralleling Louisa Rhine's efforts in the United States the Society for Psychical Research in Britain (Green, 1960) reported a census of paranormal hallucinations. Of the 1500 replies 300 were sufficiently detailed for analysis. Using the classification of Rhine they also found that the majority of the hallucinatory or dream experiences were visual in nature. A large proportion of these were alleged to have 'come true' as well. Lest the large proportion of hallucinatory experiences be dismissed as pathological it should be noted that D.J. West who is both a parapsychologist and

a psychiatrist has detailed the differences between ESP hallucinations and pathological ones. He concludes,

" . . . I still find little difficulty in placing most experiences squarely where they belong, either in the realm of psychotic disorder, or among the curiosities of interest to parapsychologists."

(West, 1960, p.96)

Sannwald (1963) in Germany and Stevenson (Prasad and Stevenson, 1968) in India have also examined spontaneous cases in these other countries and have likewise found that the vast majority of psychic experiences involve 'seeing' the event either in a waking hallucination or in a dream. Their findings closely parallel those of the English-speaking countries.

White (1964) has provided a detailed review of the experimental methods used in the earlier days of parapsychology. Almost all of the quite successful subjects which she reviews report that their correct responses are visually mediated. Among the many reports she quotes are the following examples (p.40):

"The thing which stands out is that whenever anything was perceived in full color - it being the image of the thing drawn rather than the drawing itself, it was always correct . . ."

(Carlson)

"When most successful I have distinctly seemed to see the card."

(Rawson)

". . . but more frequently I seemed to see the picture . . ."

(Mlle. Jane)

Another interesting point emphasized by White is that with these subjects there is a deliberate effort to disengage the 'conscious mind' in the technique of these early high-scoring subjects. It appears that the visual imagery comes only with difficulty and great effort is required to keep the mind blank and receptive. She quotes Rush on the point:

"As soon as the suggestion of a form begins to take shape on the subjective screen, the rational faculty leaps in to impose a plausible interpretation and thus to distort or entirely misconstrue what might otherwise have become an accurate response."

(p.41)

Of the high scoring subjects who have emerged in recent years one of the most spectacular has been Bill Delmore, a young law student extensively studied at the Institute for Parapsychology. Kelly, Kanthamani and Child (1974) reported the results of part of their investigations at the 1973 P.A. Convention. Having noted that their subject, *"like many other 'sensitives' . . . strenuously insists on the quasi-visual character of his ESP experiences."*, they go on to describe a study in which the errors in the ESP data were compared with normal visual errors. It turned out that the errors made by Delmore in ESP target recognition were very similar to those made under conditions of weak visual stimulation. The investigators conclude,

*"We tentatively interpret this result as consistent with the subject's introspections in suggesting, for him at least, ESP information is regularly encoded in the form of fleeting visual imagery; errors appear at a secondary stage when he attempts to identify the images."*⁷

(p.94)

⁷ For what it is worth, while on the topic of special subjects, Uri Geller has several times described his psychic impressions as appearing as an image on a television screen in his mind, and he almost always draws his responses in a test.

In the early 1970's a shift from the traditional card guessing to what were called 'free response' techniques was evident in parapsychology.⁸ These frequently involved using complex pictures and allowing the subject a 'free response' in which he described his images and the descriptions were blind matched against the possible targets in the pool. The usual targets for such experiments were art prints, travelogue or thematic slides, etc. generally chosen for their differing visual characteristics. Subjects were encouraged to give reports of their internal visual imagery during and after the sessions and these reports were used as protocols which the judges used in their blind matching. The increasing preference among parapsychologists for this technique reflected a growing awareness of the role of visual imagery in mediating ESP responses as well as a desire to use experimental methods requiring the subject to shift his attention away from the external world. In the hands of certain investigators this technique has proved to be highly successful and Honorton (1976) regards it as one of the most replicable of all parapsychological experimental designs. For a while at least the free response technique using art prints worked even in the Edinburgh laboratory (Parker, 1977).

Targ and Puthoff, working at the Stanford Research Institute took the free response method one step further. If it is the visual characteristics of the targets which make them more

⁸ Rao (1966) of course reports several early cases of free response testing but concluded at the time of writing, "*Hardly any parapsychologist now is doing any serious research with such materials.*".

accessible by ESP, they reasoned, natural or man-made real life locations may prove more potent as targets for free response techniques. Thus was devised what they called 'Remote Viewing' (Targ and Puthoff, 1975), a technique in which the agent went out to visit a scene at some location randomly chosen from a pool of possible locations within 30 minutes drive of the test centre. This improved free response technique proved remarkably successful (and incidentally constituted a substantial part of the famous Targ and Puthoff paper in Nature, see Ch.I, p.8n), but more interesting than simply its success was an observation they made regarding the subjects:

"In our recent work, we have encouraged subjects to make drawings of anything they visualize and associate with the remote location. We have found . . . that the drawings made by the subjects are in general more accurate than the subject's verbal descriptions."

(Targ and Puthoff, 1975)

Thus from both the spontaneous and the experimental data throughout the history of parapsychology and most obviously in the recent work there are continuing suggestions that the most successfully received ESP information seems to come in the form of visual imagery. An interesting sidelight to the spontaneous cases is that when the ESP sensation is not visual - which frequently occurs in the waking state - it is most commonly in the form of a non-verbalizable 'hunch' about something. Targ and Puthoff's comments noted above almost seem to point directly to a hemisphere bias favouring the right in the processing if not reception of parapsychologically acquired information. It should be noted that the general feeling among parapsychologists that ESP information is best in a

visual modality grew up quite independently of both Myers' hypothesis regarding right hemisphere involvement and hemisphere lateralization findings in general.

DREAMING AND ESP

A closely related and largely overlapping line of evidence comes from the frequently observed connection between ESP and dreaming. In all of the reviews of spontaneous cases mentioned above the most common means of experiencing the paranormal communication was through realistic dreaming (i.e. a dream which closely resembled the actual events rather than indicating them symbolically). It has been one of the most persistent observations that the most dramatic instances of ESP occur during dreams.

Of the studies of dreaming and ESP the most thoroughgoing and successful have been the extensive investigations carried out at the Maimonides Medical Center in New York City by Montague Ullman and Stanley Krippner along with their colleagues. The story of the progress of their work and dramatic examples of some of the successes have been well documented in numerous papers and two books (Ullman and Krippner, 1970; Ullman, Krippner and Vaughan, 1973). Beloff (1974) has commented on this work, "*By any standards, this work deserves to rank among the most outstanding achievements of parapsychology during the past decade.*", (p.39).⁹

⁹ Lest it be thought that this work is the looked for repeatable paradigm it should be noted that unpublished reports coming from the Maimonides lab indicate that a major attempt to replicate the earlier findings has failed to produce the same sort of results.



The general method for these experiments was to have a subject go to sleep for the night while being monitored for the onset of dreaming. During the dream periods an agent, elsewhere of course, would look at a target, usually an art print, and occasionally act out associated scenarios. As the subject ended his dream period he would be awakened and asked to report what he had been dreaming about. These reports were tape recorded and transcribed. The results were analysed using independent judges to rank the transcripts according to their correspondence with each of the targets in the appropriate pool. In most of the experimental series the results were statistically significant, sometimes very highly so. Beyond this though, examples of some of the responses indicated correspondences between target and dream so strikingly close as to be almost unbelievable.

Considering the findings from hemisphere research suggesting a link between dreaming and the right hemisphere the exceptional success of the Maimonides work along with the long history of anecdotal reports constitute another line of evidence pointing to the right hemisphere. Bogen's conjecture that the reason why the split-brain patients no longer dream is because their verbal processing area is no longer in contact with the dream area seemed particularly relevant to the issue. At the time it was easy to speculate that the right hemisphere was in some way more amenable to telepathic information and that this would occasionally show up dramatically during the particular time in a person's day when the verbal hemisphere had a certain amount of reportable contact with the more esoteric activities of the right hemisphere,

namely, during dreaming. Even in the dream experiments, however, the investigators were expecting the ESP evidence to come via the dominant hemisphere which provided the verbal report. Comments made by the Maimonides researchers suggest the difficulties in this approach. Discussing the fact that in a number of cases the blind judges, working from the subjects' verbal reports, gave lower ratings than the subjects did when they were given a chance to rate the targets, Ullman et al. (1973) remark,

"The difference in judging ability may be related to non-verbalized dream imagery and feelings. 'Something - I don't know what - about this target reminds me of my dreams' is a comment we have heard often from subjects who give a higher rank for the target picture than do the judges."

(p.109)

COGNITIVE STYLE AND ESP

It must be admitted that one of the early influences on this research work was Bogen's (1969b) view, popularised by Ornstein (1972), that the two minds which he believes each person has tend to externalize themselves in characteristic cognitive styles. In brief it is thought that a relative reliance on the left hemisphere is associated with a rational, scientific attitude whereas a greater reliance on the right hemisphere is associated with creative and artistic attitudes. It is of course assumed that these styles are relative and subject to continuous adjustment within any individual. While this particular view has to some extent been replaced by more sophisticated theories discussed earlier it is not entirely without merit and on that basis several items of research were worth noting.

Austin (1971) administered Hudson's (1966)

'converger-diverger' tests to a group of subjects to separate them according to cognitive style: One group with an intellectual bias toward the rational and scientific and another group who were more prone to imaginative and artistic endeavours. The former are called convergers and the latter divergers. The dichotomy closely parallels Bogen's descriptions of left and right hemisphere preference respectively. Austin then conducted a dream lab study with these subjects and found that the divergers were significantly likely to recall their dreams more frequently and in greater detail than convergers. Divergers recalled almost 100% of their dreams while convergers managed only 60% (on being awakened at the end of an REM dream period). Holmes (1973) followed up this line of research and interpreted the differences in recall between the two groups as reflecting different strategies of defence against the possibly threatening material of the dream. These latter findings are of interest in relation to the 'Psi filter' hypothesis discussed below.

It is interesting to note that Honorton (1972) has carried out a study indicating that those subjects who reported frequent dreaming showed a significant advantage in ESP ability in a standard card guessing test over those who reported only occasional dreaming. This confirmed earlier findings by Johnson (1968) which indicated that frequent dreamers did better in a clairvoyant and precognitive card guessing test than infrequent dreamers. The implication of these findings fits in with the speculation made above in that they may indicate that those who are more generally

in touch with their dreams, by virtue of their cognitive style, are also more likely to be in touch with their ESP, even in the laboratory setting.

THE POSSIBLE RELEVANCE OF ANATOMICAL DIFFERENCES

Several lines of evidence and speculation converge to point to the possible relevance of the anatomical differences existing between the two hemispheres as yet another reason for investigations into differential hemispheric involvement.

One of the major theoretical contributions to parapsychology in recent years has been Stanford's (1974a,b) Psi-Mediated Instrumental Response (PMIR) model of psi. This is a wide ranging proposal with many interrelated hypotheses which take account of much of what has been observed in both laboratory ESP and spontaneous cases. Stanford argues that the dramatic examples of ESP which are noted are the exception rather than the rule where the operation of psi is concerned. There is ample evidence to suggest that psi also operates via subtle, barely noticeable changes or modifications of ongoing mental or behavioural processes. An example of what is meant by this is the case of a man who, on his way to the airport discovers he has forgotten his ticket, and as a result of going back for it, misses his plane; the plane subsequently crashes.¹⁰ Stanford argues from several cases of the sort that these

¹⁰ While this example is almost a cliché in fact a very similar experience was had by a former member of the Edinburgh Psychology Department.

may represent a need-serving psi modification of ongoing behaviour to effect the beneficial result, i.e. in the case above forgetting the ticket having an obviously beneficial effect. Two of the proposals which he puts forward are,

" (6) PMIR occurs in part through psi-mediated facilitation or triggering of otherwise ready or available responses (including actual behaviour, thoughts, memories, or feelings). (7) PMIR tends to be accomplished in the most economical way possible."

(p.312)

In other words, the psi influence makes only the smallest change necessary in ongoing brain activity to achieve the desired outcome. Several investigations by Stanford and others have provided evidence supporting the unconscious and need-serving aspects of the psi process.¹¹

Stanford further argues that telepathy, in many cases if not all, has an 'active-agent' component, similar to the 'Kappa telepathy' of Thouless and Wiesner (1946), and that it is the agent's causing of psychokinetic changes in the brain activity of the subject which is responsible for the information transfer or behavioural change in the subject. To support this contention he cites experiments indicating a PK influence on living tissue as well as evidence to be discussed in the second of the converging lines of this section.

The second line of evidence in this case comes from what is generally regarded as one of the most important developments in recent

¹¹ Stanford and Thompson, 1974; Stanford and Associates, 1975; Stanford and Stio, 1976.

parapsychological research and that is the work of Helmut Schmidt. In the late '60s Schmidt devised a four-choice guessing machine which had as its source of randomness the emission of alpha particles through the decay of atomic nuclei, a process which is theoretically purely random. Extensive testing of the machine indicated that it was one of the best sources of random targets available and dual automatic recording devices minimized any possibility of recording errors.

Schmidt found that there were a few subjects who could consistently guess correctly which of the lamps would be lit by the quantum process to a degree far exceeding what would be expected by chance alone (Schmidt, 1969). In principal there is no known way in which subjects could predict which lamp could be activated by the decaying atoms yet some subjects could and could do so to a significantly above chance extent consistently over thousands of trials. Schmidt went one step further, however; instead of asking subjects to guess which lamp will come on next he asked them to make one of the lamps come on more (or less) frequently than would be predicted by chance. In other words subjects were being asked to influence by PK the rate of decay of an atomic nucleus, a process which physicists with the most intense electric or magnetic fields are virtually unable to alter.

The results of Schmidt's early experiments are well known and this work is continuing with similar success at his hands. Selected subjects proved capable of influencing the quantum process in such a way as to make a given target lamp come on more than would

be expected by chance. Over the thousands of trials used these experiments have produced some of the highest odds against chance ever noted in PK experimentation (Schmidt, 1971; Schmidt and Pantas, 1972). More recently Schmidt (1973) has developed machines which use the electronic noise of a noise diode as a source of quantum randomness and has had similar striking results.

What the Schmidt experiments, and those like them, indicate is that some subjects are able to cause the otherwise random fluctuations of atomic decay or electronic noise to cohere into some form of order. In other words the noise somehow has information imparted to it, and that somehow appears to be by the mental influence of certain persons. The human brain, of course, consists of billions of electro-chemical connections in the form of the synapses and there is indeed a good deal of quantum 'noise' associated with the background neural activity of the brain. In a very, very coarse way might not the randomness of the Schmidt machines be analogous to the random activity of the brain and might not the effect which Schmidt's subjects have on these machines be analogous to telepathy? Certainly Stanford thinks these possibilities should be taken seriously, and with his model it is only a very slight influence which may be necessary to effect the desired change.

This is, it must be admitted, quite a leap of speculation, bold some might say, foolish, others, but it was in fact made quite a few years ago by no less a brain researcher than Sir John Eccles. In a rather daring paper in Nature (Eccles, 1951) he proposed that

'mind' interacted with the human brain through psychokinesis, or at least something of that order. Whether or not one shares the dualistic interpretation of mind and brain which underlies his hypothesis does not alter the value of some of his observations on the operation of the brain and the way in which PK may influence cortical activity. On a neurological level his views seem a direct antecedent of Stanford's ideas.

"Suppose some small 'influence' were exerted at a node that would make a neurone discharge an impulse at a level of synaptic excitation which would otherwise have been just ineffective, that is, in general to raise the probability of its discharge. Such a discharged impulse would in turn have an excitatory effect on all the other nodes on which it impinges raising the probability of their discharge, and so on. If we assume, as above, that the spread from node to node occupies 1 msec., then even on the two-dimensional net of the illustration a spread to a large number of neurones is possible in, say, 20 msec., a time that is chosen because it is at the lower limit duration of discrete mental events. It is to be remembered that the neurones of the network are at a high level of excitation, as there is initially a large amount of circulating activity (impulses) in the network."

(p.55)

Not all the neurones in the network would fire necessarily but only those which were poised at a critical level of excitability as a result of ongoing activity. Eccles continues,

"Thus, within 20 msec. the pattern of discharge of even hundreds of thousands of neurones would be modified as a result of an 'influence' that initially caused the discharge of merely one neurone. But further, if we assume that this 'influence' is exerted not only at one node of the active network, but also over the whole field of nodes in some sort of spatio-temporal patterning, then it will be evident that potentially the network is capable of integrating the whole aggregate of 'influences' to cause modification of its patterned activity that otherwise would be determined by the pattern of afferent input and its own inherent structural and functional properties."

(p.55)

The foregoing lines of thought thus converge on the idea, albeit quite hypothetical, that telepathic or psi influences in general occur through some direct psychokinetic action on the brain. While there is no pretense that this particular hypothesis is 'airtight' in comparison with the various suggestions offered through years this one seems to have an inherent attractiveness which makes it worth being taken seriously for investigative purposes. This then leads to the question which shows the relevance of this rather long digression to hemisphere asymmetries and that is,

"In the light of what has been shown regarding both the cognitive abilities and, more importantly, the anatomical differences both demonstrated and inferred (i.e. Semmes (1968) conclusions regarding the more diffuse organization of the right hemisphere) might it not be possible that one hemisphere is neurologically organized in such a way as to be more susceptible to psychokinetic intervention?"

To this investigator the possibility is sufficiently strong to be added to the other possibilities pointing to hemisphere asymmetries in ESP.

PSI AND DEVELOPMENT

Gazzaniga's (1970,1974) theory of the development of hemisphere specialization, if applied to the evidence suggesting that the right hemisphere is more successful at ESP but such information is only sporadically available to the verbal hemisphere, would predict that quite young children should at least be more able to express psi communication. Gazzaniga's theory holds that lateralization takes place during the early years of a child's development

with the anatomical asymmetries giving an edge to one or the other side of the brain for various cognitive functions. The increasing superiority of one side then inhibits the duplicate activity on the opposite side. If, as in the young child, the lateralization and accompanying inhibition is not fully developed it might be expected that ESP would be more readily available to the child's expressive faculties.

While there exists considerable anecdotal material concerning child telepathy (Schwarz, 1961; Ehrenwald, 1971) only a few experiments with relatively young children have been reported and even in these the children were at least of school age. Work at the University of Surrey by Ernesto Spinelli (1977, pers.comm.), which started about the same time as this research, sought to examine the psi abilities of children younger than had ever been examined up to that time. The findings were of considerable relevance to the question of lateralization. Working with large groups of subjects from just over 3 years of age and up Spinelli found not only that the very young children scored exceptionally well in suitably adapted ESP tests but also that there was a very significant decline in scoring ability as the age of the subjects increased. In two series of experiments (the first having some flaws which were corrected for the second) with about 1500 subjects Spinelli found overwhelming evidence indicating that ESP ability decreased as age increased with chance level scoring occurring with groups from age 5 and up. Spinelli interprets his findings as demonstrating the existence of a pre-verbal 'primary language' but it seemed at the time¹² that his findings were

¹² Unfortunately within the past year two as yet unpublished studies, one of which was done in Edinburgh, have completely failed to replicate Spinelli's findings.

entirely compatible with the idea that the decline of ESP is proceeding along with the hypothesized increasing inhibition of the right hemisphere emanating from the left. Thus the suggestion, and while only a suggestion seemingly one too good to pass by, is that ESP may be considered incompatible with verbal communication and thus may be inhibited out of normal consciousness as the dominant hemisphere gradually assumes control.

LATERALIZATION THEORIES AND PSI THEORIES

The question of why psi should be lateralized to one hemisphere of the brain cannot be answered without reference to the larger question of why psi should be in the first place. Among the most persistent ideas has been the suggestion that ESP was more prevalent at an earlier stage of man's evolutionary development where it served an entirely useful function but has since declined (Rhine and Pratt, 1957; Thouless and Wiesner, 1946). The arguments generally run along the lines that psi would have been useful for group hunting or individual survival but as socialization increased and linguistic communication developed psi communication could have waned in importance, perhaps even becoming socially undesirable as survival of the group became more important than that of the individual.

If it be the case that psi has developed along the line just suggested then it could be expected that what is left of possibly, though not necessarily, more extensive psi abilities has followed the same evolutionary course that the more readily identifiable cognitive abilities have done. Recalling Trevarthen's description of

the differences in function of the hemispheres as reflecting different methods of getting information from the environment, the left specializing in focal and discrete object perception while the right, having more of an environmental scan, is attracted by events of possible importance to the person, it is not unreasonable to think that the sort of processing which a survival-related psi ability might require would be more akin to the ambient environmental scan of the right hemisphere than the focalized processing of the left. A final speculative note on this theme; if psi is an obsolescent survival-related ability one might expect to find the last vestiges of it during man's most vulnerable time, when he is sleeping, and indeed, it is during sleep, or at least during dreaming, that the most accurate and useful psi experiences occur.

The second major theoretical position shared by many parapsychologists is rather a supplement to the previous in that it focuses on what is seen of psi today. Essentially it is an adaptation to parapsychology of Bergson's (1911, 1913) thoughts on the function of the brain as a filter which insures that attention to the details of getting along in the world is not distracted by in-essentials. Thus, in the field of parapsychology it has been argued by Beloff (1972, 1973a) and Rushton (1971) that it may be desirable and even necessary to have a means of restricting or inhibiting psi information from entering normal consciousness because the bulk of it may be as unimportant and distracting as the welter of unimportant sensory information which impinges on the body and is filtered out. Brain mechanisms of attention may have evolved in such a way that except in the most exceptional circumstances psi

information is filtered at a sub-conscious level. Stanford would argue, of course, that psi may still be there, influencing behaviour, but under normal circumstances is prevented from entering our awareness.

A certain amount of inferential support for this theoretical position comes from the noticeably above average success of the psi in Altered States of Consciousness research. Such work has dealt with ESP in various abnormal states of consciousness resulting from hypnosis, ganzfeld techniques, sensory bombardment or deprivation, and in some cases just simple but deliberate relaxation. Parker (1975a) and Honorton (1974) have reviewed the high degree of success this work is having and at least Honorton (1976) believes that this work is the key to the repeatable experiment problem. The common feature shared by all of the altered states work is the attempt to "*circumvent the brain's normal defense mechanism*" (Beloff, 1973a) in order to allow some psi to show itself.

If the weak and variable evidence of psi in normal consciousness is due to some mechanism which filters it, perhaps along with a good deal of other material, out of the general stream of sensory information reaching consciousness it is not at all far-fetched to suppose that the filtering may be done by or at least have some relationship with the mechanisms of verbal expression which are so intimately a part of consciousness.¹³ The implication of

¹³ This wording is a deliberate effort to avoid becoming embroiled in the controversy regarding whether or not consciousness is necessarily verbal, a battle in which Zangwill (1974, 1976) and Eccles (1965, 1973) are the principal participants.

this line of reasoning is that if not psi itself at least the inhibition of it may be lateralized along with the verbal processing aspects of consciousness.

These two highly speculative theoretical positions regarding psi phenomena both could be taken to suggest possible laterality effects in psi ability; the evolutionary theory because psi information seems of the type best handled by the right hemisphere, and the filter theory because it seems reasonable to expect the proposed filter to be closely allied with verbal consciousness.

SUMMARY

The extensive findings and considerable theorizing on cerebral asymmetry of function intersect with parapsychological findings in a number of areas to provide reasonable grounds for suspecting asymmetrical involvement of the brain hemispheres in the operation of ESP. The main points are:

1. Hemisphere lateralization research indicates that the hemispheres of the brain are capable of operating independently and experimentally supported theories suggest that duplication of function is minimized by cross callosal inhibition. Furthermore, the type of information most amenable to right hemisphere processing is that obtained by ambient or pre-attentive sensory processes which generally do not in themselves reach consciousness.

Parapsychological theories, with some experimental support, suggest that ESP may be largely unconscious when present, and under normal circumstances unable to reach consciousness (or at least the verbal apparatus by which we describe it). Long standing but more speculative parapsychological theories have the type of information represented by ESP more compatible with the right hemisphere's pre-attentive scan and less compatible with verbal consciousness.

2. The particular abilities so far identified as best processed by the right hemisphere include tasks of a visuo-spatial nature and some evidence indicates that the right hemisphere predominates in dreaming. Spontaneous ESP almost exclusively occurs as hallucinatory visual experiences or dreams and the most successful of recent parapsychological work has been that using dreams or explicitly visual target and response combinations. Very old evidence suggests that some verbally presented evidence of ESP resembles the limited verbal ability of the right hemisphere when the left is damaged.

3. Certain lateralization theories, with some experimental support, suggest that lateralization develops over a relatively long time, several years at least, and that very young children are effectively 'split-brained' with duplicate activity going on until one hemisphere takes the lead and suppresses the opposite hemisphere. In parapsychology considerable anecdotal and some very recent experimental evidence indicates that very young children may be better at ESP, or at least more able to communicate it, than adults, and that

this ability seems to decline rapidly until about age 5 when chance results are the norm.

4. Studies have demonstrated differences in the neurological organization of the hemispheres. Certain parapsychological theorists have argued that telepathy is a form of active psychokinesis on the brain. It seems a possibility worth examining that one half of the brain is more suitable for PK intervention.

The evidence and speculation suggesting asymmetrical hemisphere involvement in ESP, with the general tendency to favour the right hemisphere, is considerable but there is no attempt here to imply that it is unequivocal. Sadly, there is little in parapsychology for which that epithet would be inappropriate. Parapsychology is not yet at the stage where an investigator can safely follow a narrowly defined research path since the fundamental questions remain unanswered. In consideration of parapsychology's mediocre replication record and the fact that up to this time there was no reason for experimenters to require responses by other than the dominant hemisphere route, the question addressed in this chapter has been, "Is there a sufficient case for examining the ESP abilities of the hemispheres, particularly the right, separately?" To this investigator the answer was clear.

CHAPTER III

EXPERIMENTS ON HEMISPHERE

DIFFERENCES AND ESP - I

CHAPTER III

EXPERIMENTS ON HEMISPHERE DIFFERENCES AND ESP - I

THE SHAPES SERIES¹

Motor control and somatosensory representation in the cerebral cortices is primarily contralateral, that is, each half of the brain receives sense impressions from and exercises motor control over the opposite side of the body. This connectivity is not exclusive as there is ipsilateral, or same-side, representation as well, but this is of a particular kind. While the bulk of the detailed information is relayed to the contralateral hemisphere a certain limited amount of information, mostly of a quantitative and less specific kind, is conducted ipsilaterally. However the contralaterality of cortical somatosensory representation becomes increasingly exclusive for the more distal parts of the body, such as fingers, so, except for fairly coarse features such as size, weight, etc. it can be said that when the right hand palpates objects only the left hemisphere is privy to the stereognostic information until it is communicated to its opposite number via the corpus callosum (Sperry, 1968; Gazzaniga, 1970).

¹ With permission of my supervisor the findings in this chapter have been published in part, (Broughton, 1976a).

This feature of neurological organization figures prominently in the early testing of the split-brain patients. With the callosum cut and the hemispheres unable to communicate with one another it was quite literally a case of the left hand not knowing what the right hand was doing. When tachistoscopic visual presentation was used to send certain information to each hemisphere the split-brain patient would see two different objects, or more accurately, each hemisphere would see one of the two objects. From a sack containing a number of objects the right hand would easily retrieve what the left hemisphere had seen and the left hand would fetch the different object which the right hemisphere had seen. If questioned, "What did you see?" the patient naturally reported the left hemisphere object. Similar results were obtained in tests requiring drawing or simple pointing responses (Gazzaniga, Bogen and Sperry, 1965). Also split-brain patients were unable to reproduce in one hand a hand position arranged by the experimenter on the other (Sperry, 1968).

It is possible to see in these findings the basis for a simple ESP test which might bypass the dominant hemisphere which has been in command of most ESP tests. Since the traditional tool of the parapsychologist, the ESP card guessing test, can be viewed as an ESP mediated visual recognition task it would be an easy matter to create an analogous task for tactual recognition which could be performed by each hand. Five three-dimensional shapes could serve as the counterparts to the five symbols of the ESP deck. In place of shuffling the cards one set of shapes would be used in conjunction with a target randomizer. For the guessing the subject would make

his selection by feeling about the second set of shapes and selecting one of them as his guess, registering the fact by lifting it slightly rather than making a verbal response. In a rough fashion the guessing would be controlled by the hemisphere opposite the hand in use and, importantly for an experiment seeking to tackle the repeatability problem, the experiment would remain sufficiently simple to enable easy replication by other laboratories.

Because an experiment like the one just outlined would necessarily have to be conducted with normal subjects, as opposed to split-brain patients, the use of the hands alone most likely would not be sufficient for detecting hemisphere effects. The two hemispheres of normal persons are in constant communication via the corpus callosum and during the time taken to make the manual selection of a shape there would be ample time for the hemisphere not under test to get in on the act. If there is anything to the suggestions noted in the previous chapter that verbal consciousness filters out the 'unwanted' paranormal information then simply using the left hand would not solve the problem unless it was possible to create a situation in which the unconcerned hemisphere was rendered unable or partially unable to interfere with its counterpart's guessing.

The study of skilled performance has demonstrated that requiring a subject to perform two competing tasks simultaneously causes performance in one or both to decline (Welford, 1968). Since with right handers verbal processing is known to take place in the left hemisphere verbal tasks have been used in many hemisphere

asymmetry experiments to activate that hemisphere in contrast with the right. Spatial tasks are commonly employed as a comparison task for the right hemisphere.² Kinsbourne has made use of the competing skill situation to demonstrate hemisphere asymmetries (Kinsbourne and Cook, 1971). In his experiment subjects attempted to balance a dowel on the index finger of each hand, one hand at a time. Relative to control performance the addition of a verbal task caused a decrease in balancing ability on the right hand (competing situation) and an increase in balancing ability on the left hand (non-competing situation).

In the possible ESP experiment previously described the responding is non-verbal so it would be possible to use a verbal or other left hemisphere task to occupy that hemisphere and set up a competing situation in only one hemisphere against which the bi-manual ESP guessing could be compared. The obvious situation would be to use the bi-manual guessing test with and without a left hemisphere task similar to those used in hemisphere specialization studies: counting or reading.

If paranormal perception, or the processing of it, is a function of the entire brain, then the loading of a competing task on one hemisphere could be expected to depress the scoring by the corresponding hand if there is general above chance scoring by the subject. This situation would be analogous to allowing half of

² For examples: as related to eye movement, Bakan, 1969; Kinsbourne, 1972; as related to cortical electrical activity, Galin and Ornstein, 1972.

the brain to relax,³ which has been demonstrated to be a psi-conducive state,⁴ while keeping the other half busy.

If paranormal perception is a function of the left hemisphere, which seems unlikely or we should see it more frequently than we do, then the control, or non-competing condition of the experiment should demonstrate higher scoring for the right hand relative to the left. A possibility exists in such a case that the left hemisphere's ESP abilities are inhibited by the right hemisphere, a possibility for which no suggestion has been found in the literature, but it must be admitted that the ideal experiment would have a right hemisphere counterpart to the left hemisphere's competing task. In practice, however, this is quite difficult, since the particular abilities of the right hemisphere do not lend themselves to tasks of a continuous nature which could be employed in the experimental set-up. At the present stage of investigation, in view of the absence of any suggestion of left hemisphere superiority in ESP or right hemisphere inhibition of ESP it seemed justifiable to use only the left hemisphere competing task.

The final possibility is that the right hemisphere is better at the processing of paranormal information. If that is

³ Galin and Ornstein (1972) have demonstrated that alpha rhythm in the brain, generally an indicator of quiescence, increases in the right hemisphere relative to the left when the subject engages in a verbal task.

⁴ For a recent review see Braud and Braud (1973, 1974).

the case then it could be expected that the left hand would display higher scoring relative to the right. If, additionally, the verbal, left hemisphere exerts an inhibitory influence on the par-normal perception in the right hemisphere then such a left hand superiority might only occur when the left hemisphere is occupied with a competing task.

Since the idea of the experimenter as an objective observer is largely a myth and as Popper never tires of pointing out an experimenter's statements about his results are always interpretations of the facts observed, and in fact, interpretations in the light of his theories (Popper, 1963, p.107n), this investigator wishes to put on record that his interpretation of the literature, as well as his intuition, if that may be permitted, led him to favour the final possibility, i.e. that the right hemisphere is better at ESP but normally this may be inhibited by some function of the left hemisphere.

An experiment was thus designed to test the various possibilities outlined above. It was expected that the experiment would effectively separate on a functional basis the ESP responses of the two hemispheres while at the same time possessing a simplicity that would permit easy replication by other laboratories. Subjects would perform runs of a five-choice twenty-five trial ESP guessing test using their fingers to select their choice from among five three-dimensional shapes made of wood. This would represent a non-verbal equivalent of the traditional card guessing experiments. One run would be done on each hand in a relaxed condition and again

while the subject was additionally performing a left hemisphere task constituting a two-factor experiment yielding four conditions:

1. Left hemisphere control of guessing with no competing task.
2. Right hemisphere control of guessing with no competing task for the left hemisphere.

These two conditions allow full normal operation of the hemispheres and whatever inter- or intra-hemispheric inhibition may be present. After loading the competing task on the left hemisphere the following conditions obtain:

3. Left hemisphere control of guessing while additionally occupied with a competing task.
4. Right hemisphere control of guessing while the left hemisphere is occupied with another task.

Because there were no experimental precursors to suggest a possible outcome and the initial experiments themselves were rather exploratory it seemed that the best hypothesis which could be formulated at this time was the rather general one, "The hemispheres of the brain do display differing abilities with regard to at least some forms of ESP". Predictions as to the nature and the direction of the differences would have been premature although subsequent experiments could hope to be more specific.

GENERAL FEATURES OF THE SHAPES SERIES

Equipment

The shapes used in place of ESP cards in this series were pyramid, cylinder, ball, cube and cone.⁵ They were constructed from light softwood and averaged 6 x 6 x 6 cm (i.e. the size of a cube into which each could fit). Average weight of the shapes was approximately 30 gms. The shapes are shown in Plate I.

Targets for the series were generated by the Edinburgh Electronic ESP tester (Beloff and Regan, 1969), a five-choice random target generator. The main unit of the machine was located with the Agent in a room 15 m. distant from the laboratory connected by a corridor and having two offices intervening. The Experimenter operated the main unit by using the satellite unit (ordinarily the subject's console) as a remote control. Pilot tests on the machine indicated satisfactory randomness and during each experiment records were kept of the targets generated for randomness tests. The main unit of the E.E.E.T. was fitted with a shelf made of aluminium on which the target shapes rested. The machine's selection was indicated by the illumination of a light under the shape. Plate II shows the Agent's target selecting unit.

⁵ It has been pointed out by Schmeidler (pers.comm., 1974) that the shapes used in this experiment lent themselves to verbal encoding if the subject is so inclined. If this should be the case it might tend to blur hemisphere differences and the use of shapes less prone to verbal encoding should be tried in future experiments of this type. Some of the shapes used by Gibson (1962) may be feasible, although the final outcome of this investigation makes the question academic.

The physical arrangement of the laboratory for the experiment was as follows: The subject sat in a simulated leather reclining chair (kept in upright position). The set of shapes from which the subject selected his guess was contained in a small tray on a low table which put them in a convenient position with respect to his hand. The tray was used to prevent the shapes from becoming too dispersed. The Experimenter sat at a small table opposite the subject so as to be able to see the subject's selections. The remote console was located on the small table and placed within a cardboard carton to prevent the subject from seeing the target indicators which were lit (feedback indicators) after the subject's guess was recorded. For experiments II and III the subject's chair was fitted with a reading table. A typical arrangement for the subject is shown in Plate III.

Subjects

The majority of the subjects used in this series were university students who responded to appeals for volunteers. All were unpaid and naive with regard to the aims of the experiment. Only right-handed subjects were used in the experiment as it is only with right-handed subjects that one can be reasonably sure that language is lateralized to the left hemisphere. Handedness was assessed by self-report, i.e. subjects were asked which hand they commonly use for writing, etc. In general, in order to avoid emphasising this aspect of the experiment subjects were not asked about handedness prior to their arrival at the laboratory. On the rare occasions that neither the subject nor his partner (when used)

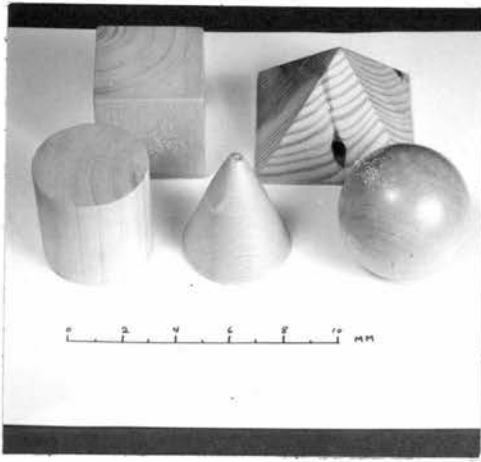


PLATE I : The five shapes used in the manual guessing arrangement.

PLATE II : Agent's console with duplicate set of shapes mounted on shelf.

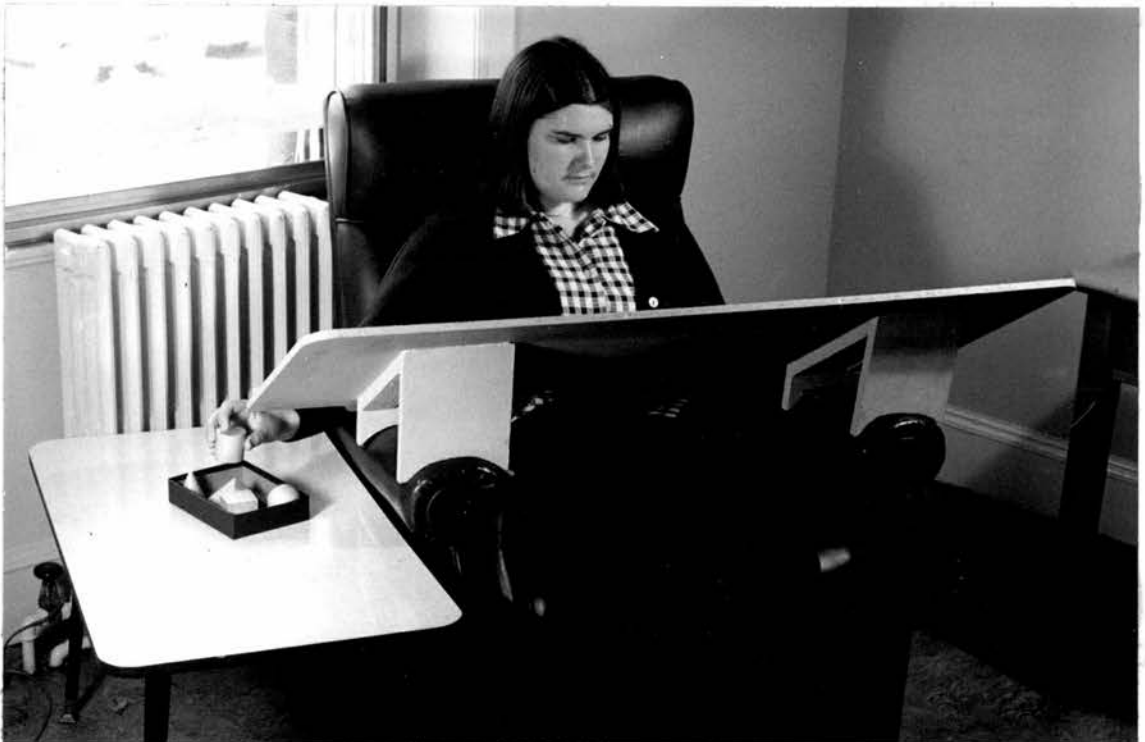
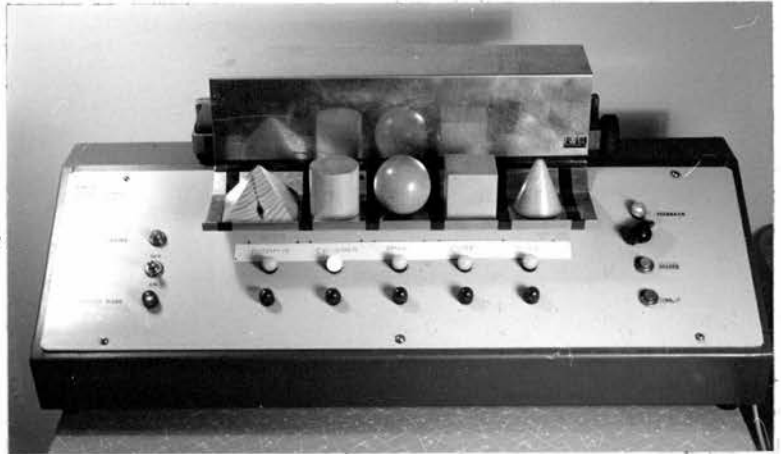


PLATE III : A subject (posed) in position and 'making a guess' while reading.

were right handed they were run as any other subject but the data were not included in the analysis.

For the first experiment subjects came with a partner of their own choosing and the only criterion for the partner was that it was someone whom the subject knew and with whom he 'felt comfortable'. Part of the reason for using the ostensibly telepathic arrangement was because subjects seemed to prefer this to a clairvoyant one. Additionally, it was thought that if subjects brought along a friend they would have less anxiety about participating in the experiment. For reasons explained later the partner was not required for the second experiment but was brought back for the third. Only the subject needed to be right-handed; no restriction was placed on the agent.

Method

Upon their arrival at the laboratory the subject and his partner were greeted by the experimenter. The basic aspects of the experiment were explained, i.e. that they would be doing a variation on the traditional ESP guessing tests with one of them serving as agent and the other as subject; that the subject would be making guesses non-verbally (demonstrated) and the experimenter would be recording them; and that the targets were being selected randomly for the agent by a machine ('randomly' described as 'having no discernable order or pattern'). After questioning each regarding handedness one person was given (or allowed to choose if both were right-handed) the role of subject and the other became the agent.

Both subject and agent accompanied the experimenter to the agent's room where the experimenter explained the agent's role. The agent was told that the machine did the choosing (not the experimenter - a popular misconception) and that it operated automatically on the experimenter's command. The target shape would be the one under which the lamp was lit. It was also pointed out that the experimenter did not know the target until after the subject's guess had been recorded (another common misconception). Regarding his telepathic efforts the agent was advised that simply his being conscious of the target was probably sufficient and that it was not necessary to attempt to emulate a radio transmitter. "A relaxed awareness of the situation, namely that (partner's name) is trying to guess the target and you are the only person who knows what it is", was suggested as the best strategy to adopt. The agent was also encouraged to pick up and handle the shape being indicated as well as entertain thoughts and associations about it. After the agent was asked if he had any questions about his role he was left in the room with the target generator. During the experiment no feedback of his partner's performance was provided to the agent.

The subject and experimenter returned to the laboratory where the different conditions pertaining to his role were explained. The subject was told that he would perform the guessing test in four runs of 25 trials each, one run on each hand while sitting quietly (conditions 1 and 2) and one run on each hand while reading⁶

⁶ For the very first experiment the left hemisphere task was counting backwards by three's or four's. However, about halfway through the experiment the investigator learned that this particular technique for left hemisphere loading seemed to be less successful than originally thought (Bradshaw, pers.comm. 1974). The original reason for choosing counting was that it was closer to the relaxed condition and that it could be done without the additional aspect of visual attention required for reading. Nonetheless, with such evidence that

(conditions 3 and 4 above). It was stressed that, while under the reading condition, the subject should concentrate on the reading task and not interrupt it or pause while making the guess with his hand. The explanation given to the subject for this seemingly bizarre behaviour was that it served as a means of forcibly taking his attention away from his guessing behaviour so that his guessing became 'unconscious and in the background'. No mention of hemisphere differences was made to either the subject or the agent until after the completion of their participation. The reading material used in the experiment were selections from the Law Reports of The Times, material chosen to be sufficiently complex to require the subject's attention yet contain a minimum of emotion. The assignment of conditions was counterbalanced.

The sequence of operations was as follows: After the agent and the subject were in position and both knew their tasks the subject was asked to put on a light blindfold (to aid relaxation rather than prevent vision). If reading was indicated the subject would begin this, otherwise he would just sit quietly. The experimenter would press the 'start' button on his console which would cause a target to be generated at the agent's end. After a ten-second pause (a somewhat arbitrary 'incubation' period)

⁶ (cont.) counting was not as well lateralized as thought (e.g. Diamond and Beaumont, 1972) this task was discarded in favour of reading for all subsequent experiments. A possible consequence of this change may be noted in the data. For the subject's part the tasks were treated the same and in this section 'counting' may generally be substituted for 'reading' for the first experiment.

the experimenter would say to the subject, 'Choose', and the subject would feel about the shapes and lift one a few inches. The experimenter would record this choice on the record sheet and then enter the choice on the console. At this point the experimenter's console would indicate which shape had been the target. This kept the experimenter blind to the target for each trial and provided double recording of hits. The experimenter then initiated the next trial by pressing 'start' and this continued for the 25 trials of the run. A short rest break was given after two runs and the entire session averaged just over one hour. At the close of each half of the session the score sheet was checked against the machine record for accuracy.

Data Analysis

The general analysis for the effectiveness of the left hemisphere task treatment and the left-right hands treatment was carried out using analysis of variance for two factors with repeated measures (Ferguson, 1971, p.264). It is a mixed model with Rows and Columns (treatments) fixed and layers (subjects) random. There is one score per cell. A model of the analysis of variance table is given in Figure 1. When the interaction was non-significant the sums of squares representing the Within Subjects Error were pooled and divided by the combined degrees of freedom to yield an F ratio with a greater number of degrees of freedom associated with the denominator (see Binder, 1955). A model of the pooled version, more frequently used in the Shapes series is given in Figure 2.

Source	Error Term	Degrees of Freedom
Rows (Hands)	RS	(R-1)
Columns (Condition)	CS	(C-1)
Subjects	-	(N-1)
R x C (Interaction)	RCS	(R-1)(C-1)
R x S	-	(R-1)(N-1)
C x S	-	(C-1)(N-1)
R x C x S	-	(R-1)(C-1)(N-1)

Fig. 1: Model of an Analysis of Variance Table for two factor with repeated measures design (Error sums of squares partitioned). Error term refers to the appropriate SS divided by its df.

Source	Error Term	Degrees of Freedom
Rows (Hands)	Within SS	(R-1)
Columns (Condition)	Within SS	(C-1)
Subjects	-	(N-1)
R x C (Interaction)	Within SS	(R-1)(N-1)
Within SS (RxS + CxS + RxCxS)	-	(N-1)((R-1)+(C-1)+(R-1)(C-1))

Fig. 2: Model of an Analysis of Variance Table for a two factor with repeated measure design with Error SS pooled.

To assess the scoring in the individual conditions, i.e. to see if any single condition provided evidence of above chance scoring, the appropriate test was the t-test for a single mean (see Stanford and Palmer, 1972). As this study was concerned with the behaviour of subjects in the test situation the more common C.R. would have been inappropriate.

Recording of Data

The data were recorded manually by the experimenter on specially prepared record sheets. The experimenter, sitting opposite the subject, would record the selection using a two-letter code (py,cy,ba,cu,co) and then enter the selection on the remote console of the E.E.E.T. At this point the console indicated what the target had been and this was recorded manually as well. Hits were noted both on the record sheet and automatically on the E.E.E.T.'s counter. Additionally, the machine maintained totals for the generated targets which were transcribed on to the record sheet at the end of each half session.

EXPERIMENT I

In the first experiment the procedure diverged from the general design outlined above in two ways which shall be noted. In this experiment the investigator was not sure that a single run of 25 trials per subject per condition would provide sufficient data for testing treatment effects. Subjects were therefore required to come on two occasions resulting in two runs per

condition which were to be pooled for the analysis as it was assumed that the treatment effects would remain the same on the two visits.

The second divergence was the result of the investigator 'hedging his bets'. Since a considerable amount of time was to be invested in collecting a large amount of ESP guessing data it seemed worthwhile to take a measure of the subject's 'cognitive mode' which one author (Ornstein, 1972) had indicated might be related to relative preferences for one hemisphere over the other. The measure was Hudson's (1966) Converger - Diverger Test which Austin had found was related to dream recall (see Chapter II, p.54). It was thought that if the hand and reading treatments failed to provide any differences then the data could be divided into convergers and divergers to see if there was any general scoring difference between these groups.

The Converger - Diverger Test consists of the A.H.5, a high-level intelligence test (verbal part only) and the "Object - Pattern Book".⁷ These were administered to subjects only who returned at a time when the experimenter could schedule small groups of subjects. Because of the intelligence test, in order to avoid worrying the subjects, it was arranged that the records would remain anonymous since the experimenter was not interested in individuals in any case. The procedure was to place the completed

⁷ This is a test devised by the Centre for Research in the Educational Sciences, University of Edinburgh. The idea is to think up as many as possible uses for five everyday objects and to give as many as possible things that five abstract patterns could represent.

ESP score sheet (without any identifying information) in an identified, sealed envelope. When the Converger - Diverger Tests were completed the two ESP score envelopes for that person were opened and the scores explained to the subject. Then all four unidentified records (two ESP, A.H.5, Obj.-Pat. Book) were placed in a single brown envelope which remained unidentified.

The Object - Pattern Book was scored by counting the number of responses. The A.H.5 was scored in the usual way. The Converger - Diverger scores were obtained, according to the method of Hudson, by calculating a Z-score for each subject on the A.H.5 and the Object - Pattern Book. The sign of the Object - Pattern Z-score was changed and added to that of the A.H.5. Thus a high positive score (high A.H.5, low OPB) yields a converger and a high negative score (high OPB, low A.H.5) yields a diverger. For purposes of comparison the top 30% (convergers) are compared with the bottom 30% (divergers) of the whole group.

As mentioned in the note, p.79, counting was used instead of reading but the explanations to the subject remained the same.

A further statistical test was incorporated in this series. Since each subject was to provide two sets of scores in each condition it was decided to take Eysenck's (1967) suggestion that one way of examining for extra-chance effects is to correlate ESP tests with one another as a reliability measure to determine whether the tests were, in fact, measuring the same extra-chance effect. Accordingly, a correlation test (Pearson's r) on the first and second visit results was planned for each condition.

Subjects

There were 20 right-handed subjects, 14 female and 6 male.

Results

A total of 160 runs were made, 40 in each condition representing the two visits by 20 subjects. Tests on the first order randomness for the targets generated by the E.E.E.T. indicated satisfactory randomness, $\chi^2 = 1.43$, 4 df, n.s.

The overall results in the first experiment gave no evidence of any departure from chance expectancy with none of the condition totals departing by more than nine points from the MCE of 200. Naturally, the analysis of variance failed to detect any difference between the conditions. The condition totals are given in Table 1a and the analysis of variance summary in Table 1b.

TABLE 1a: Totals and summary statistics for each condition of Experiment I, combined sessions.

	Left Hand Counting	Left Hand Relaxed	Right Hand Counting	Right Hand Relaxed
MCE	200	200	200	200
Score	203	209	197	191
Dev.	3	9	-3	-9
Mean	10.15	10.45	9.85	9.55
t-test	0.30	0.79	-0.21	-0.71

TABLE 1b: Analysis of variance summary for combined sessions of Experiment I. (Pooled Sums of Squares)

Source	SS	df	MS	F
Hands	7.2	1	7.2	1.108
Condition	0.0	1	0.0	0.0
Subjects	192.5	19	10.13	
H x C	1.8	1	1.8	0.277
Error	370.49	57	6.5	

The subjects were separated into Converger and Diverger groups of six subjects each but neither group displayed any above chance scoring in overall score or by condition. There were no significant differences between groups. As a result of the lack of any significant scoring or differences and because of the small groups no further analyses were undertaken on the Converger - Diverger question.

Despite the disappointing overall results the correlation tests were performed on the data with one unexpected finding. While near chance correlations were obtained for three conditions the left hand counting condition yielded a rather strong negative correlation of $-.494$ ($p = .03$,⁸ 18 df). This experimental condition represented the right hemisphere motor control of the guessing while the left hemisphere was otherwise occupied with the counting task. The negative direction of the correlation indicates that there was a strong tendency for subjects to score in opposite directions for the two visits, at least under the one condition. The results of the correlation are in Table 2.

TABLE 2: Correlation coefficients (Pearson's r) by condition for the two sessions of Experiment I

	Left Hand Counting	Left Hand Relaxed	Right Hand Counting	Right Hand Relaxed
$r =$	$-.494$ ($p=.03$, 18 <u>df</u>)	$.034$	$.144$	$.154$

⁸ p values throughout the paper are two-tailed unless specifically indicated as one-tailed.

This finding indicated that a post hoc analysis of the two sessions was necessary. The procedure used to ensure the anonymity of the subjects contributed to an oversight in that the actual order of sessions was known definitely only for the last ten subjects. Therefore the post hoc session analysis was of necessity confined to the latter half of the sample. The summary statistics for the two sessions are given in Table 3a. On inspection there is a clear difference between the two visits in at least one condition. The right hand conditions for both sessions yielded only chance results. For the first session the analysis of variance (Table 3b) indicated a significant difference between hands, $F = 4.037$ (df 1,27; $p < .05$) with the left hand producing a higher score than the right. The left hand while counting condition produced nearly significant above chance scoring, $t = 2.176$ ($p = .056$, 9 df), representing the right hemisphere guessing while the left hemisphere was occupied. The second session analysis of variance (Table 3c) also indicated a significant effect of hands, $F = 5.236$ (df 1,27; $p = .03$), but in this case the left hand produced lower scores than the right. The left hand counting condition, which produced near significant above chance scoring in the first session, produced almost significant below chance scoring in the second session, $t = -2.092$ ($p = .064$, 9 df). The difference in the left hand counting condition between the first and second session results was significant, $t = 2.496$ ($p < .04$, 9 df).

TABLE 3a: Totals and summary statistics for each condition of the two sessions of Experiment I, last 10 subjects only.

Session One				
	Left Hand Counting	Left Hand Relaxed	Right Hand Counting	Right Hand Relaxed
MCE	50	50	50	50
Score	68	64	55	47
Dev.	18	14	5	-3
Mean	6.8	6.4	5.5	4.7
t-test	2.176 (<u>p</u> =.056, 9df)	2.409 (<u>p</u> =.038, 9df)	0.542	-0.635
Session Two				
MCE	50	50	50	50
Score	38	47	52	55
Dev.	-12	-3	2	5
Mean	3.8	4.7	5.2	5.5
t-test	-2.092 (<u>p</u> =.064, 9df)	-0.537	0.429	0.921

TABLE 3b: Analysis of variance summary for the first session of Experiment I, last 10 subjects only.

Source	SS	df	MS	F	
Hands	22.5	1	22.5	4.037	<u>p</u> = < .05
Condition	3.6	1	3.6	0.646	
Subjects	38.1	9	4.23		
H x C	0.4	1	0.4	0.072	
Error	150.5	27	5.574		

TABLE 3c: Analysis of variance summary for the second session of Experiment I, last 10 subjects only.

Source	SS	df	ms	F	
Hands	12.1	1	12.5	5.236	<u>p</u> = .03
Condition	3.6	1	3.6	1.558	
Subjects	41.4	9	4.6		
H x C	0.9	1	0.9	0.389	
Error	62.4	27	2.31		

Discussion

There is little possibility of disguising the fact that the particular configuration of results which emerged from the first experiment was not at all expected. The investigator's interpretation of the available evidence would have made the first session results precisely those which he would have wished for the experiment as a whole. The mirror image results of the second session could not have been anticipated. What is important to note, however, is that all the significant scoring, positive and negative, was concentrated in one condition, the one representing right hemisphere control of the guessing while the left hemisphere was occupied.

It is admittedly difficult to explain the curious reversal of scoring direction between the two sessions of the experiment. Certain observations which were made may suggest some explanations but these are all far from conclusive. As with many guessing tests, the tasks involved in this experiment were on the tedious side. The subject's role was at least more active than the agent's and no subject expressed any negative feelings about his participation. Nonetheless, it is still possible that this was a straightforward case of psi-missing on the part of the subject. On the other hand, Stanford (1974b) has emphasised the role of the active-agent in such ESP situations and it is worth noting some features of the agent's activities in the experiment. In most cases the agent was recruited by the subject and in a sense was doing a favour for him. Of the two roles in the experiment that of the agent was decidedly less exciting and was frequently

characterized by agents as 'boring'. While there may have been an initial desire to do well on the part of the agent, by the time the second visit came around the agent may have been dismayed when he realized that he had to sit through that boring task again. There was some informal evidence that at least a few agents were less than enthusiastic on the second occasion. Perhaps there was even a bit of unconscious resentment directed at the subject for having dragged him into this situation or toward the experimenter for insisting that they work in the same roles that they had in the first session. Such resentment could have resulted in the communication of more psi 'misinformation' than correct targets, and such 'misinformation' is most apparent in the conditions favourable to ESP in the first place. This was entirely speculative of course, but perhaps useful to guide future work.⁹

If the magnitude, and not the direction, of the scoring is considered then the analyses of variance for both sessions revealed at least one fact that was in accordance with the hypothesis, namely that the hand by hand treatment indicated a greater effect on one hand than on the other. This could be taken as differing hemispheric involvement in ESP. The counting treatment seemed to have little effect. The interaction terms were somewhat lower than would be expected by chance indicating that the hands treatment and the counting treatment produced quite independent effects.

⁹ Stanford's paper which prompted the speculations on active-agent telepathy came too late to influence the second experiment which did not use an agent in the design.

The interesting findings of this experiment were largely the result of post hoc analyses and as such are subject to the usual cautions. Nonetheless it was encouraging to note that effects seemed confined to one condition.

EXPERIMENT II

A second experiment was carried out to confirm certain aspects of the post hoc findings of Experiment I. Several modifications were introduced to improve the experimental set-up. The first modification was largely one of expediency; subjects were recruited singly and the task was represented as one of clairvoyance. The first experiment had demonstrated that recruiting sufficient numbers of unpaid pairs of volunteers to spend over an hour in the laboratory was an exceptionally troublesome task, a problem not unknown to the Edinburgh University Psychology Department as noted by Drever (Murchison, 1932, p.25). Since it has traditionally been difficult to demonstrate meaningful differences between telepathy and clairvoyance or that there are cases where only one mode of ESP can be successful it was decided to use the clairvoyance mode in the interests of a smooth-running experiment. Secondly, because of the unpromising results of the Converger - Diverger analysis in the first experiment these tests were not used in the second. Finally, since it was not possible to pinpoint the cause of the reversed scoring in the second visit it was thought best to avoid whatever may have led to that situation and require subjects to come for only one visit. The only other

change was the one discussed earlier, namely that reading aloud replaced the counting task for the reasons already given.

The result of the various changes meant that Experiment II was to be a replication of the "First Session Effect" of Experiment I using clairvoyance instead of telepathy.

Except for the parts referring to the agent the method was exactly as outlined in the general description of the Experiments. To accommodate the clairvoyance mode the instructions were altered slightly and the subject was shown the room and the target generating device by way of giving him something to 'focus' upon.

Subjects

Twenty right handed subjects were used, 15 females and five males.

Results

A total of 80 runs were made, 20 in each condition. First order randomness of the targets was satisfactory, $\chi^2 = 2.63$, 4 df, n.s.

The overall results are presented in Table 4a. As can be seen there was only chance performance under all conditions and the analysis of variance (Table 4b) failed to detect any significant differences although there was a tendency for the reading condition to yield better scores than the non-reading condition.

Discussion

It seemed in hindsight that the switch to clairvoyance was a mistake. The only other change which seemingly would have been related to the scoring was the use of reading in place of counting and, if anything, that change showed a beneficial effect.

TABLE 4a: Totals and summary statistics for each condition of Experiment II.

	Left Hand Reading	Left Hand Relaxed	Right Hand Reading	Right Hand Relaxed
MCE	100	100	100	100
Score	105	107	115	88
Dev.	5	7	15	-12
Mean	5.25	5.35	5.75	4.4
t-test	0.45	1.02	1.44	-1.37

TABLE 4b: Analysis of variance summary for Experiment II.

Score	SS	df	MS	F
Hands	1.01	1	1.01	0.28
Condition	7.81	1	7.81	2.157
Subjects	132.44	19	6.97	
H x C	10.51	1	10.51	2.903
Error	206.41	57	3.62	

Shortly after the completion of this experiment the first of Stanford's two papers (Stanford, 1974a,b) detailing his psi-Mediated Instrumental Response theory arrived at the Edinburgh lab.¹⁰ In this paper he proposed that telepathy should be looked

¹⁰ In the October 1974 issue of the J. Amer. Soc. Psychic Res. which arrived in December 1974.

at as a product of an active agent and could be subsumed under a general model of PK. Though he provides a number of instances supporting his hypothesis, it remains a theory, but a theory which could have some bearing on the results of the experiment. If telepathy was an active agent phenomenon then perhaps what the first experiment demonstrated was a hemisphere difference in PK susceptibility, and by eliminating the agent the experiment might have eliminated an important component of the effect.

EXPERIMENT III

It was worth pursuing the question in a third experiment of the same basic design which might serve to clarify the results obtained in the first two experiments. The agent's role was reinstated and the subjects came with partners as they had in the first experiment. Reading remained the left hemisphere task and only one visit was required. It was hoped that this combination of conditions would approximately duplicate those of the first session of Experiment I.

Subjects

Again 20 right handed subjects were used. There were 11 females and nine males.

Results

As before there were 80 runs, 20 in each condition. First order target randomness was satisfactory, $\chi^2 = 2.11$ (4 df, n.s.).

The overall results are presented in Table 5a. As expected in the left hand while reading condition (right hemisphere guessing while the left is busy) subjects scored significantly above chance, $t = 2.939$ ($p = .008$, $19df$). A noteworthy, though non-significant negative score was obtained in the right hand relaxed condition (left hemisphere guessing while not otherwise occupied).

The analysis of variance indicated a significant effect in the relaxed vs. reading treatment, $F = 4.265$ (df 1,57; $p = .04$). The hands treatment yielded a weaker, non-significant effect $F = 2.962$ (df 1,57; $p = .09$). An unusually small interaction was obtained, $F = 0.013$, indicating that the effects of the two treatments were independent.

TABLE 5a: Totals and summary statistics for each condition of Experiment III.

	Left Hand Reading	Left Hand Relaxed	Right Hand Reading	Right Hand Relaxed
MCE	100	100	100	100
Score	120	101	104	87
Dev.	20	1	4	-13
Mean	6	5.05	5.2	4.35
t-test	2.939 $p = .008$, $19df$	0.127	0.525	-1.174

TABLE 5b: Analysis of variance summary for Experiment III

Source	SS	df	MS	F	
Hands	11.25	1	11.25	2.962	
Condition	16.2	1	16.2	4.265	($p = .04$)
Subjects	58.2	19	3.06		
H x C	0.05	1	0.05	.013	
Error	216.49	57	3.798		

Discussion

This experiment represents a clear-cut confirmation of the overall results obtained in the post hoc analysis of the first session of Experiment I. The simultaneous reading, i.e. the loading of the left hemisphere with an additional task, caused a significant increase in the subject's scoring ability. There was also a strong suggestion that the left hand has an advantage over the right in the manual guessing test, paralleling that found in the previous results. The analysis shows these two effects to be independent and when they are combined, as this experiment was designed to do, the result was significant above chance scoring in the left hand while reading condition.

GENERAL DISCUSSION OF THE SHAPES SERIES

The aim of this experimental series was to examine the possibility that some forms of ESP may exhibit brain hemisphere laterality effects when tested in an appropriate manner. The results of Experiment III, in which a significant above chance score was obtained with the left hand while the subject was reading, represent the type of scoring pattern which could be said to demonstrate that a hemisphere difference for ESP does exist. Unfortunately the overall results of the first two experiments did not provide a particularly consistent trend for Experiment III to confirm, although, as already mentioned, confounding variables may have brought about this situation.

The important point made by this series rests in the fact that in the post hoc analysis of the first session scores of Experiment I it was found that in the left hand while counting condition a noticeably high, almost significant, rate of scoring was achieved. The third experiment was designed to replicate closely the first session and did result in a confirmation of the effect. Performance with the left hand while additionally carrying out a left hemisphere task was significantly above chance while the other conditions were near chance or below. Since the left hand guessing represents right hemisphere control of the guessing, or at least a closer association of the right hemisphere with the guessing, and the reading task serves to occupy the left hemisphere, then this would indicate an apparent right hemisphere advantage for ESP when it is possible to test it more directly and reduce the possibility of left hemisphere interference at the same time.

In Experiment I the comparison between the first and second sessions was post hoc and less than satisfactory in that it included only the second half of the total number of subjects. However, the analysis of Experiment III revealed that the scoring patterns were remarkably similar. In the two halves of the first experiment left hemisphere task and the hand used effect were independent with the hand effect being the only one which was significant. In Experiment III both main effects were also independent with the left hemisphere task being the significant treatment and the hand used treatment falling slightly short of significance. These results are presented graphically in Figure 3. The failure of the left hemisphere task treatment in the first experiment to achieve a

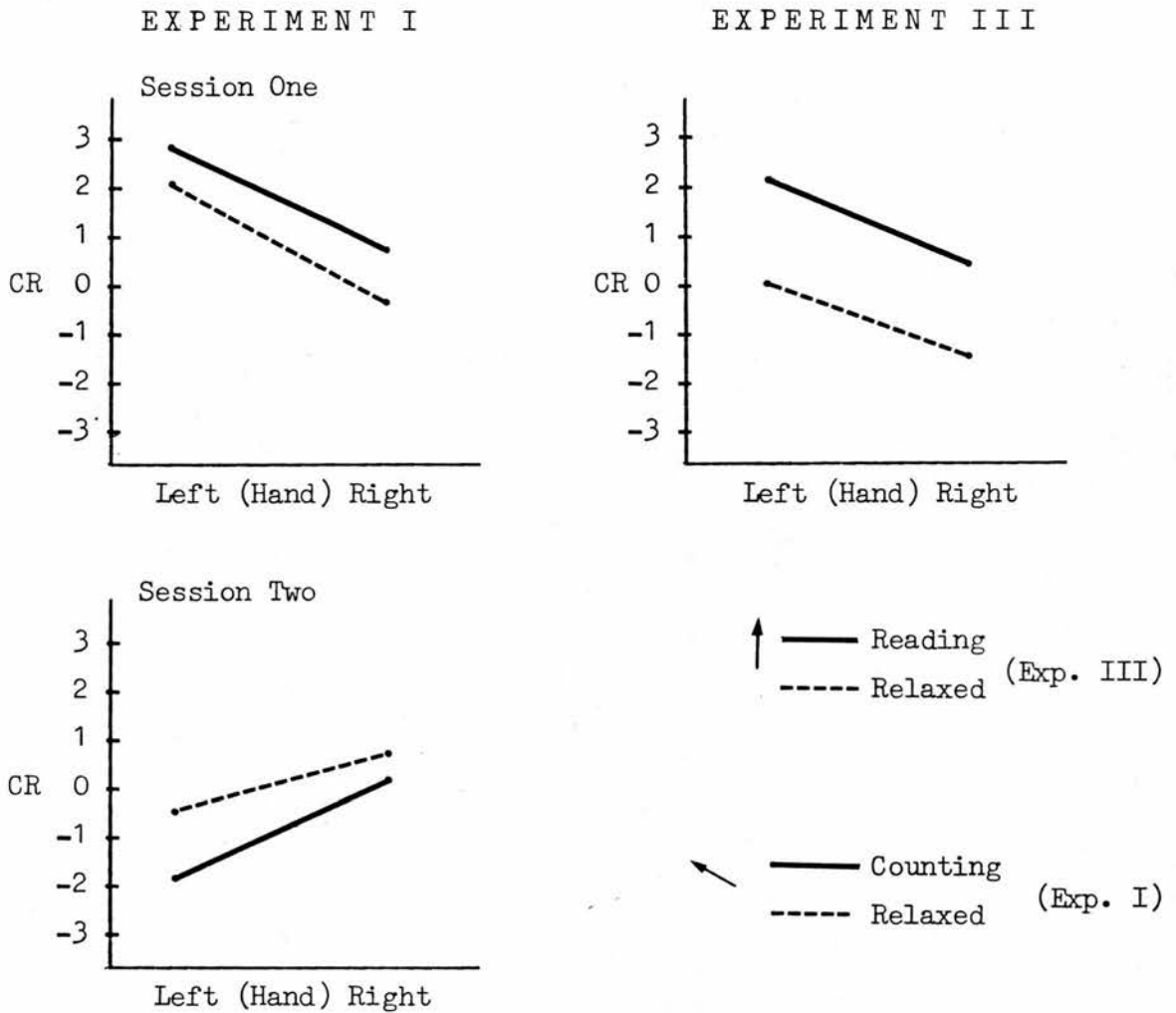


Fig. 3: Graphic representation of the scoring patterns for Experiments I and III. Data are normalized by C.R.

significant effect could have been due to the belatedly learned fact, mentioned earlier, that arithmetic tasks do not seem as well lateralized as previously thought. The reading task which replaced it in Experiment III was obviously more successful.

SEX DIFFERENCES IN THE DATA

These findings are presented last to preserve the chronology of the original discovery which may be of some interest for its interpretation. J.B. Rhine (1974a) maintains that one of the surest indications of ESP and one of the best guards against experimenter fraud is the belated discovery of 'signs of psi', incidental but significant effects noted in the data which were not anticipated by the investigator at the time of the experiments and original analyses. The results of the shapes series contained one such sign of psi.

The literature of parapsychology has provided no evidence to suggest stable sex differences in the operation of psi. Rao's (1966) review of the findings led him to conclude, "*All these findings show that neither sex has a monopoly on psi.*". With one at first unrecognised exception discussed later the occasional references to sex differences in the literature since the publication of his book indicate that the situation has not changed since then. At the time the shapes series was in progress the situation regarding sex differences in hemisphere specialization was equally non-committal. Some studies, e.g. McGlone and Davidson (1973), had suggested that males were more lateralized than females, particularly for some visuo-spatial tasks, while others, e.g. Buffery and Gray (1972) cited evidence to support the opposite view that males are less lateralized than females. For the shapes series, and even for the reaction time series (Chapter IV), there seemed no reason to incur the additional burden of recruiting groups of

subjects balanced by sex as this variable appeared to have no systematic relationship to the phenomena in question.

Sex of subject, therefore, was a variable distributed at hazard through the earlier experiments. In an unpublished seminar held in the Department of Psychology, University of Edinburgh, in January 1976 a noted Italian researcher into hemisphere differences, Carlo Umiltà provided evidence showing that for some time their research team had been obtaining significant laterality effects in various experiments only with male subjects. This led him to conclude that males are more lateralized than females, i.e. that the cognitive functions tested in his experiments are, in male subjects, more clearly divided between the hemispheres whereas those of females may be shared between the hemispheres. To this investigator's knowledge these claims have not yet appeared in print, only a curious shift in the procedure used by Umiltà and his colleagues: now they only use male subjects in their laterality experiments.

A recent paper by Bradshaw, Gates and Nettleton (1977) addresses the question more directly. In an experiment requiring lexical decisions to laterally presented words, illegal consonant strings, and legal non-words right handers displayed the usual left hemisphere superiority for such activities but on examination the effect was found to be concentrated in the data of the male subjects. The females provided minimal field differences suggesting to the authors an apparently greater hemispheric equipotentiality for lexical decisions. They further suggest that this finding could

account for females overall verbal superiority as well as their visuo-spatial inferiority.¹¹

The discovery of sex differences in hemisphere specialization obviously has wide implications for many areas of psychology but what is of interest to the present study is simply the fact that early evidence showed verbal manipulations in hemisphere investigations to be more effective for males.

The Umiltà seminar came about a year after the last of the shapes series was finished. The shapes material was considered finalized and a publication was in preparation. At the time, however, the reaction time series (Chapter IV) had just been completed. It was in these data that a difference in the apparent effectiveness of the reading treatment between the males and the females was first noticed. In the reaction time data the males appeared to bear the major responsibility for the effect. This curious and unexpected development prompted the investigator to look at the data from the shapes series again to see if such a difference existed there as well.

Because sex of subject was not controlled in the experiments none of the three experiments had balanced groups of males and females. Experiment III was reasonably close in that respect so these data were examined first. This was done

¹¹ This general claim regarding female abilities has been made in several studies and an overview can be found in Maccoby and Jacklin (1974).

informally in the first instance by subjecting separately the group of nine males and the group of 11 females to the same analysis of variance used for the experiment as a whole. The results were dramatic; the reading/relaxed treatment, which was found to be significant overall, yielded an $F = 11.035$ (df 1,24; $p < .005$) for the male subjects but only $F = 0.006$ (df 1,30) for the female subjects.¹²

The data from Experiment II were examined next. An equally curious result was found here, especially in light of the fact that originally this experiment had been judged unsuccessful. The reading/relaxed treatment yielded a significant effect of $F = 4.998$ (df 1,12; $p < .05$) for the five males but no such effect for the 15 females, $F = 0.277$ (df 1,42). This result raised a question regarding the 'failure' of the second experiment. Did Experiment II fail to elicit the desired effects because of the change to clairvoyance, as previously thought, or was it due to the low number of males in the subject pool? It appeared that either answer was now likely with the balance tilted a bit to the low number of males, but clearly further experiments were thought necessary to confirm this.

The combined session data for Experiment I yielded no difference between the sexes. For the sessions individually the subject pool was too small (only three males) to allow meaningful interpretation of the data. There was a suggestion that the hand

¹² Because of the informal nature of these tests the summary tables are not given here.

treatment favoured the females but it was not thought wise to pursue this for the reason mentioned.

To formalize the sex differences finding in the one set of data in which this was possible, Experiment III, a three-way analysis of variance with repeated measures (nested in sex) was performed on the data after randomly discarding the data of two female subjects to balance the groups, nine males and nine females. The results, presented in Table 6, showed clearly the difference between males and females for the effectiveness of the reading/relaxed condition. The effect of this condition was significant, $F = 6.527$ (df 1,16; $p < .02$) with a significant sex by condition interaction, $F = 5.192$ (df 1,16; $p < .03$). The difference between the hands was not significant, though in the expected direction, $F = 3.048$ (df 1,16; $p = .10$).

TABLE 6a: Condition means according to sex of subject for Experiment III with two females excluded to balance groups.

	Left Hand Reading	Left Hand Relaxed	Right Hand Reading	Right Hand Relaxed
Males	6.22	4.22	5.44	3.56
Females	6.0	6.11	5.22	4.89

TABLE 6b: Summary table for three-way analysis of variance with repeated measures, subjects nested in sex. Experiment III.

Source	SS	df	MS	Error ^a	F	
Sex	8.86	1	8.86	s(S)	2.958	
Hands	13.35	1	13.35	sH(S)	3.048	<u>p</u> = .10
Condition	19.01	1	19.01	sC(S)	6.527	<u>p</u> < .02
subjects	46.94	16	2.93			
S x H	0.35	1	0.35	sH(S)	0.079	
S x C	15.13	1	15.13	sC(S)	5.192	<u>p</u> < .03
H x C	0.12	1	0.12	sHC(S)	0.039	
s x H (S)	70.05	16	4.38			
s x C (S)	46.61	16	2.91			
S x H x C	0.35	1	0.35	sHC(S)	0.108	
s x H x C (S)	51.28	16	3.2			

^a Refers to the source of the Error variance for the F Ratio calculation.

SOME CAUTIOUS CONCLUSIONS

With the belated discovery of sex differences in the data the original interpretation of the results had to be modified slightly. It seemed that the series of experiments justified some cautious conclusions. The first, and most tenuous, was that there exists a general tendency for improved scoring in the manual ESP test when using the left hand. This must be treated cautiously because significant differences appeared only in the somewhat confused first experiment and thereafter the results, while showing the same trend, were not significant. The second, rather stronger conclusion, was that the loading of a competing task on the left hemisphere significantly improved ESP scoring in the manual guessing test, but only for male subjects.

This second finding is clearly of interest because it suggests that there is some function of the left hemisphere, perhaps related to verbal consciousness, which must be neutralized for psi to show itself in the general population. The particular task chosen in the second and third experiments seemed effective in this disruption of the anti-psi function only with male subjects, thus fitting in closely with an emerging pattern of results from hemisphere specialization studies.

While it had been noted earlier that most of the parapsychological observations on sex differences had been inconclusive, in the light of the findings reported above one of the studies, originally thought to be another 'one off' finding unrelated to a general trend, was seen in a new light. This was an observation by Krippner (1970) which noted that in the 74 dream studies to that time done at the Maimonides Medical Center it was the male dreamers who were providing the significant hits. When the results were divided according to sex the male subjects provided quite significant hitting while female subjects did not. In view of the connection between the right hemisphere and dreaming mentioned in Chapter II and the apparently more lateralized hemispheres of males this finding was seen to be of some interest, although at the time the exact nature of the relationship was not clear.

While the experimental results certainly provided differences of a statistical kind it seemed risky to assert that

the hypothesis set forth at the start, i.e. that the hemispheres do display differing abilities with respect to ESP, had been confirmed. The data appeared to point less to lateralized psi abilities and more to a lateralized psi-inhibiting mechanism which could be related to verbal consciousness.

No firm conclusions on this question could be drawn until it was possible to examine the effects of a right hemisphere task on guessing behaviour as well. Methodologically this presented difficulties because there was no immediately apparent equivalent to the reading, i.e. a continuous operation which does not require the other hemisphere to assist in making responses or otherwise indicate that the task was being properly carried out, and which could be incorporated into the ESP part of the test.

Pilot work was under way to devise an experiment which would incorporate all the relevant variables however it seemed that progress could also be made by designing another experiment that would answer certain questions which could be raised about the shapes series. These were questions as to whether or not shapes guessing favoured the right hemisphere generally, although this seemed unlikely considering the grossly different shapes in use, or if the possibility of verbal encoding of the shapes could have interfered in some way. It seemed particularly of interest to see if hemisphere differences could be found in an entirely different experimental paradigm, one which would not be subject to the possible cognitive constraints that guessing is, and which would eliminate the need for manual recording of data, a weak point of the first series. This work is the subject of the next chapter.

CHAPTER IV

EXPERIMENTS ON HEMISPHERE
DIFFERENCES AND ESP - II

CHAPTER IV

EXPERIMENTS ON HEMISPHERE DIFFERENCES AND ESP - II

THE REACTION-TIME SERIES¹

One of the first, and seemingly one of the more popular means of obtaining measures of laterality for various cognitive functions in normal subjects, as opposed to split-brain patients, is to measure the subject's reaction time. Typically such studies will employ unilateral presentations of the stimulus material (e.g. verbal material contrasted with physiognomical material) and measure the response time for recognition, decision, etc. by the subject. The response time may be measured unimanually or vocally depending on the particular investigation.

Poffenberger (1912) was among the first to notice that reactions to visually presented stimuli were about 6 msec. longer for crossed responses, i.e. the ipsilateral hand, than for uncrossed ones. In the next half-century several papers appeared either challenging or supporting these findings, but it was in the sixties that real interest in this technique developed, spurred largely by the generally increased interest in hemisphere specialization. During the last decade a large number of studies have confirmed the

¹ With the permission of my supervisor the findings in this chapter were presented in part at the 1976 P.A. Convention as a research brief (Broughton, 1977).

usefulness of reaction time as an indicator of functional laterality for a variety of tasks. Filbey and Gazzaniga (1969) used choice reaction time with both verbal and manual responses to the presence of a dot in one or the other visual field. Their findings demonstrated that there was a delay of between 30 to 40 msec. for information to cross the corpus callosum. In a similar experiment Moscovitch and Catlin (1970) obtained a smaller transmission delay time and found this to be consistent with available electrophysiological findings. Bradshaw and Perriment (1970) found a delay of about 20 msec. for responses requiring trans-commissural transfer of information. Berlucchi et al. (1971) studied simple reaction times for ipsilateral and contralateral responses to stimuli presented with varying degrees of eccentricity from the midline. Their findings confirmed the effect that responses which require interhemispheric transfer of information take longer.

The early studies merely noted the time taken for one hemisphere to communicate the presence or absence of a simple stimulus to the other hemisphere. More recent studies have employed the response time measure to demonstrate differences in the actual hemispheric processing of various types of material. Thus, Rizzolatti et al. (1971), using contralateral and ipsilateral manual responding, found a right visual field superiority for speed of response to letters and a left visual field superiority for facial recognition. They interpreted their findings as reflecting stimulus dependent hemisphere differences favouring the left hemisphere for recognition of letters and the right hemisphere for

recognition of faces. In a series of experiments Geffen et al. (1971) have obtained results which support these findings.

The usefulness of reaction time as a measure of laterality in other than visual tasks has been demonstrated by numerous experiments using auditory stimulation, particularly dichotic listening tasks. Kimura (1967), Kliz and Parsons (1975) and a number of other experimenters have reported successful use of response time to auditory stimuli in laterality tests with results analagous to those found with visual tasks.

In parapsychological investigations Stanford and his colleagues have pioneered the use of reaction time as the dependent variable in ESP tests, though in a situation in no way related to hemisphere effects.² It was found in several experiments that the verbal response time in a word association task could be influenced in a predicted direction by paranormal means thus lending support for the associative-mediation hypothesis of Stanford's Psi-Mediated Instrumental Response model. With this evidence that response time is sensitive to psi influence it seemed feasible to adapt a simple response time task with an ESP component to a hemisphere lateralization paradigm. Differences between the hemispheres in their responsiveness to a paranormal influence on reaction time would yield further information regarding the lateralization of psi abilities. Such a test could be carried out with a dependent variable which was relatively equal for each hemisphere

² Stanford and Thompson, 1974; Stanford and Stic, 1976; Stanford and associates, 1976.

and not subject to the cognitive complications of shape recognition. There was a certain amount of risk involved in that the efficacy of reaction time as a measure of psi, while amply demonstrated in Stanford's work, had never been tested in the particular experimental paradigm to be used in this research. On the other hand, it was likely that, in the absence of a paradigm which controlled hemisphere influence, no overall psi influence might appear in any case.

In experiments studying the lateralization of normal cognitive functions the desired information is obtained in situations where particular stimuli are directed to one or the other hemisphere with the expectation that one hemisphere will prove more competent in processing those stimuli than the other. In studies of psi function the situation is different. Obviously the experimenter is unable to direct the ESP 'stimulus' to one or the other hemisphere so it is not possible to employ precisely the same paradigm for ESP studies. However, by making the response simply one of speed, thus reducing the possibility of inter-hemispheric conflict, and obtaining the responses from each hand it might be possible to separate the hemispheres' responses to a non-localized or undirected ESP signal. If, using a common ESP influenced task, there is a difference between hands for the ESP component of that task, then it should be possible to draw inferences concerning the nature of the laterality effects noted.

Method

As with the shapes series and in common with all the bimanual reaction time studies reported this experiment makes use of the fact that the fingers are known to have almost exclusive sensorimotor representation in the contralateral hemisphere (Sperry, 1968). Such an arrangement would mean that the response was at least more closely associated with one hemisphere, even though it would not be possible to assume that the response originated exclusively in that hemisphere.

The task for the subject was to respond, by pressing a button, to the onset of a simple 1 kHz tone presented bilaterally to him via earphones. The subject would, of course, make his response with one or the other hand as required. The ESP component of the task involved the fact that an agent, a friend of the subject, on roughly half of the trials, chosen randomly, would also be performing a similar reaction time task but he would receive the tone 250 msec. before the onset of the subject's tone. These trials represented the 'advance' condition, that in which it was hoped the agent's prior response would, by ESP, influence the time taken by the subject to make his response. The subject and agent were not told of this fact before the experiment but were told that the study hoped to find a relationship between their scores. It was emphasized, particularly to the subject, that the responses should be made as quickly as possible. This was done to further motivate, if possible, the subject to make use of the available 'ESP advance warning'. The agent was free to respond with the preferred hand but the subject was required to respond with one or the other hand in turn by blocks of trials. The actual experiment consisted of 160 trials grouped into eight blocks of 20 trials.

The two manipulations of the experiment were, therefore, whether the trial was an advance or control trial and whether the subject used his right or left hand. The advance/control condition would provide a measure of psi influence and the hands condition might provide an indication of whether psi was more effective using one hand or the other. The shapes series indicated that the left hand seemed to have an advantage over the right, at least in correctly choosing targets, especially when a competing task was loaded on the left hemisphere. Because the reaction time situation was so entirely different from the forced choice guessing test it was not thought advisable to assume that the evidence from the shapes series would predict shorter reaction times for the advance condition on the left hand. The possible effect of the advance condition was not predictable. For one thing, the advance condition could have either a facilitating effect, i.e. the advance warning to the agent having the effect of priming by ESP the response of the subject, or an inhibiting effect if the advance ESP warning conflicts with the subject's awareness that he has no tone with the result that the subject produces longer reaction times.

While the shapes series indicated that competitive loading of the left hemisphere was necessary for significant above chance scoring in the guessing test it was hoped that the reaction time paradigm might elicit hemisphere differences without the loading task. Because the measure was speed of response it seemed reasonable to expect that the ESP influence might be felt before the opposite hemisphere could manage to inhibit it. Thus, in the first instance the experiment was conducted without the additional verbal task.

Method of Analysis

The experimental design was one of two factors, each with two levels, in a repeated measures design so it was decided to analyse the data using the same analysis of variance as in the shapes series. In this case the reading/relaxed treatment was replaced by the advance/control treatment. Because the experiment specifically expected an interaction in the event of hemisphere differences being demonstrated the sums of squares were not pooled for the analysis. One decision regarding the measure to be used per subject was taken in advance of the study and should be noted here. Since it seemed that there was a reasonable possibility that the ESP effect may not influence each and every trial but cause a substantial effect on a few it was decided that the measure to be taken would be the mean, rather than the median, of all the reaction times in each condition for each subject. The mean is sensitive to occasional large influences while the median is not and although many investigators specifically wish to avoid the occasional large departure from the average, and thus use the median, this study deliberately wished to include them as possible psi-influenced trials. All mean reaction times were to be graphed to examine the normality of the distribution of the scores before using the analysis of variance.

Since the actual direction of the effect could not be predicted in advance further tests would have to be on a post hoc basis. The main effects that were looked for were, (1) a systematic effect in the advance/control condition indicating the presence of a paranormal effect, and (2) a possible interaction with the

hand condition indicating a difference in the effectiveness of the ESP condition between the hemispheres. It was anticipated that there might be a simple difference between the hands revealed by the analysis of variance but as this would reflect normal sensorimotor differences in reaction time it was of no particular interest in this experiment.

Apparatus

The experiment was controlled and monitored by a Digital Equipment Corporation Linc-8 laboratory computer located about 50m from the parapsychology laboratory and connected to it by screened cables. The computer incorporates facilities for sensing signals (such as the press of a button), closing relays (to produce stimuli), and timing events. Computer monitored reaction times were accurate to 1 msec.

Binary random events were produced by a software routine sampling a squared white noise generator and the system is described in greater detail in Appendix A. The system is capable of producing high quality random numbers but as this experiment made use of only a 'low grade' randomness to assort the conditions and govern the pseudo-random intertrial interval no ongoing checks of its randomness were necessary.

Stimuli were generated by 1 kHz tone generators constructed in the departmental electronics workshop. These were controlled by signals from the computer. The tones were delivered via stereo

earphones jumpered for monaural presentation. Two separate local tone generators were used so there was no possibility of 'cross talk'³ on the signals to the subject and agent nor were the tones transmitted the length of the cable connecting the computer and the laboratory.

The response units for the subject and agent each consisted of a modified keyboard type button mounted centrally on an aluminium base designed for comfortable positioning of the hand. For the subject the response unit fit on the reading table used in the shapes series and could be positioned for easy and comfortable access by either hand. This is shown in Plate IV. The agent's unit additionally had a green lamp which was lit to indicate the pause between halves of the session. For the experimenter there was a two-button unit to issue commands to the controlling program.

Computer Program⁴

The program which controlled the experiment operated as follows: On a command from the experimenter indicating that a block of 20 trials was starting the program entered a pseudo-random timer sequence to give a delay of two to eight seconds. It then sampled the binary random number generator to determine if this was to be an advance or control trial. If it was an advance trial the tone was delivered to the agent followed 250 msec later by a

³ Cross talk is signal leakage between parallel wires in a cable. An example is the voices encountered between ringing tones while waiting for a party to answer the telephone.

⁴ American spelling used as this appears to be the accepted manner when referring to computer operations.



PLATE IV: Subject (posed) in position for reaction time experiment.

tone to the subject. A control trial resulted in a tone to the subject alone. The computer began timing at the onset of the subject's tone and then awaited a response from the subject. A response from either the subject or agent stopped their respective tones but only the subject's response was timed and recorded. If it had been a control trial a tone was delivered to the agent after the subject had responded to keep the agent equally busy. After both responses had been registered the machine returned to the pseudo-random timer. At the conclusion of 20 such trials the experimenter was informed by the lighting of a light and the computer awaited another 'begin block' signal. Trials on which the subject had his button depressed at the tone onset were counted as anticipations. Trials lasting over one second were counted as mistrials. Both were recorded separately but neither were counted in the 20 trials of the block, the computer re-running any such trials. After four blocks of 20 there was a pause of about five minutes during which the experimenter chatted with the subject. At the conclusion of the eight blocks the program recorded the stored data on magnetic tape and halted.

The 'begin block' signal to the computer from the experimenter also indicated which hand was being used for the following block of trials. The order of hands was determined for each subject by sequences previously prepared by a program using the binary RNG to produce four 'left' and four 'right' choices in random order.

Before the actual experiment there was a series of practice trials to insure that the participants were accustomed to responding.

Procedure

The subject and the agent were greeted by the experimenter and were questioned regarding handedness. The experiment was explained as one in which the interest lay in seeing how ESP might influence ongoing activities. The activity which had been selected was the response to a tone. They were led to believe that they were performing the same reaction time task but in separate rooms and that the experiment was to look for a 'relationship between their responding'. Neither the advance condition nor the fact that hemisphere differences were being studied was discussed prior to the experiment. The subject and the agent were shown both rooms used in the experiment (about 15 m apart - the same rooms as in the shapes series) and the procedure for responding was explained. The subject and the agent were told to make their responses as quickly as possible in all cases. The agent was left in one room and the subject and the experimenter went to the other where the experimenter explained the additional task for the subject, namely that he would be responding with one or the other hand by turns.

The experimenter then proceeded to the computer room and entered the subject's name, the date and time into the computer record. On returning to the subject's room the experimenter initiated a series of demonstration trials and checked that both the subject and the agent had mastered the simple task. The experimenter then left the agent in his room and joined the subject for the main part of the experiment.

When the subject was ready the main part of the experiment began. The experimenter consulted the hand order sequence and positioned the response unit. Then he pressed the appropriate button causing the computer to conduct a block of trials. When the computer signalled the completion of a block the experimenter repositioned the response unit, if required, and started another block. After four blocks, about ten minutes, there was a five-minute pause during which the experimenter chatted with the subject. The agent was left alone but the pause was indicated to him by a light on his response unit. The second half consisted of another four blocks just as the first half. While the subject was making his responses the experimenter sat quietly at his nearby desk.

At the conclusion of the experiment the actual nature of the study was explained to the participants. As the raw scores would have been relatively meaningless for the subject and agent this fact was explained and no feedback of the results was given.

When all the subjects had been run and the entire experiment completed another computer program printed all the data. Prior to this stage no data had been seen by the experimenter. Reaction times for each of the 160 trials were printed as well as whether they had been in the advance or control condition and the hand groups they were in. There were approximately 40 trials in each condition for each hand though this was not a fixed number due to the random allocation of advance and control trials. The number of anticipations for each condition and hand were printed as well as the number of mistrials. Means for all four conditions were printed and these were used as the subject's scores.

Subjects

The method of recruiting subjects was similar to that used in the Shapes series. Most of the subjects were university students who responded to appeals for volunteers. Only right-handed subjects were used and this was assessed by self-report as in the previous work. Subjects came with a partner of their own choosing according to the same criteria as in the shapes series. Only the person who served as subject was required to be right-handed.

EXPERIMENT I

The first experiment was run as described and proved remarkably easy to carry out. The computer control of the experiment, one of the first of its type in parapsychology, added a new dimension to the experimental situation. The experimenter, freed from the necessity to record data and operate equipment, was able to make the subjects feel generally at ease in the unfamiliar situation.

Subjects

Twenty right handed subjects were used. There were 15 females and five males.

Results

The mean reaction times for each of the four conditions for each of the 20 subjects were graphed and the distribution was found to be normal. The overall means for the experiment as a

whole were very close to one another and are shown in Table 7a.

The two-factor analysis of variance with repeated measures failed to detect any significant differences between conditions. The summary is given in Table 7b.

TABLE 7a: Mean reaction times for the four conditions in the first reaction time experiment.

	Left Hand Control	Left Hand Advance	Right Hand Control	Right Hand Advance
msec.	298.7	300.15	304.75	305.7

TABLE 7b: Analysis of variance summary for the first reaction time experiment. Error variance is partitioned.

Source	SS	df	MS	F
Hands	672.8	1	672.8	1.362
Condition	28.8	1	28.8	0.654
Subjects	142677.9	19	7509.4	
H x C	1.2	1	1.2	0.016
H x S	9384.0	19	493.9	
C x S	836.4	19	44.0	
H x C x S	1481.3	19	77.9	

Discussion

The hope that reaction time by itself would prove a useful paradigm for investigating ESP and possible hemisphere differences in ESP was not fulfilled in this experiment. The one-quarter second advance warning provided to the agent did not give rise to

any systematic paranormal influence on the reaction times of the subject. As with any novel paradigm there were a great many parameters which could be varied (e.g. advance time, subject set, type of stimulus, etc.) before the potential of the paradigm was considered exhausted.

The findings of this experiment were not thought to have contradicted Stanford's findings regarding the use of reaction time as a measure of ESP influence since the experimental paradigms of the two investigations were so substantially different.

While it was disappointing that reaction time in its simplest form seemed a less than promising investigative technique there remained the possibility that, as the shapes series had demonstrated, the loading of a competing task may be necessary for the psi influence to come through.

EXPERIMENT II

The shapes series demonstrated that ESP in a guessing situation seemed to be more effective in the right hemisphere when the left hemisphere was distracted with a verbal task. Because of the potential usefulness of the reaction time technique for investigating hemisphere differences in ESP it seemed worthwhile to try the technique combined with a verbal task to load the left hemisphere in the hope of teasing out a paranormal effect. The verbal task might be expected to cause differences between the hands, but, as mentioned above, this was a normal phenomenon not

particularly of interest in this experiment. The effects looked for were in the advance/control treatment and a possible interaction with the hand treatment.

Subjects

Twenty right handed subjects were used. There were 11 females and nine males.

Procedure

The procedure was in all aspects but one precisely the same as in Experiment I. The only change involved the fact that the subject was asked to read aloud extracts from the law reports of The Times. The explanation to the subject was similar to that used in the previous work, namely that it was a means of distracting his attention from the button-pressing response. When the experiment proper was about to start the subject began reading and continued, except for the rest break, for all eight blocks of trials.

Results

The mean reaction times of the conditions for each of the 20 subjects were graphed and found to be normally distributed. The overall means for the four conditions are given in Table 8a. Surprisingly, the analysis of variance indicated that there was no significant effect of hands, nor was there a difference between the advance and control trials. The analysis did indicate a very significant interaction between the hands and the condition, $F = 9.774$ (df 1,19; $p = .006$). Table 8b presents the summary of the analysis of variance. Inspection of the means revealed that the

essence of the interaction lay in the fact that the advance condition for the left hand resulted in a shorter mean reaction time and the very same condition for the right hand resulted in a longer mean reaction time. A correlated t-test for each hand yielded a significant difference between the control and the advance conditions for the right hand, t = **2.46** (p = .022, 19 df). The difference for the left hand did not reach significance.

TABLE 8a: Mean reaction times for the four conditions in the second reaction time experiment.

	Left Hand Control	Left Hand Advance	Right Hand Control	Right Hand Advance
msec.	368.75	363.85	362.5	371.05

TABLE 8b: Analysis of variance summary for the second reaction time experiment. Error variance is partitioned:

Source	SS	<u>df</u>	MS	<u>F</u>
Hands	4.5	1	4.5	0.011
Condition	66.6	1	66.6	0.466
Subjects	108242.3	19	5697.0	
H x C	904.5	1	904.5	9.774 <u>p</u> = .006
H x S	7720.4	19	406.3	
C x S	2713.6	19	142.8	
H x C x S	1758.2	19	92.5	

Figure 4 represents the Experiment II scoring graphically to show the interaction more clearly.

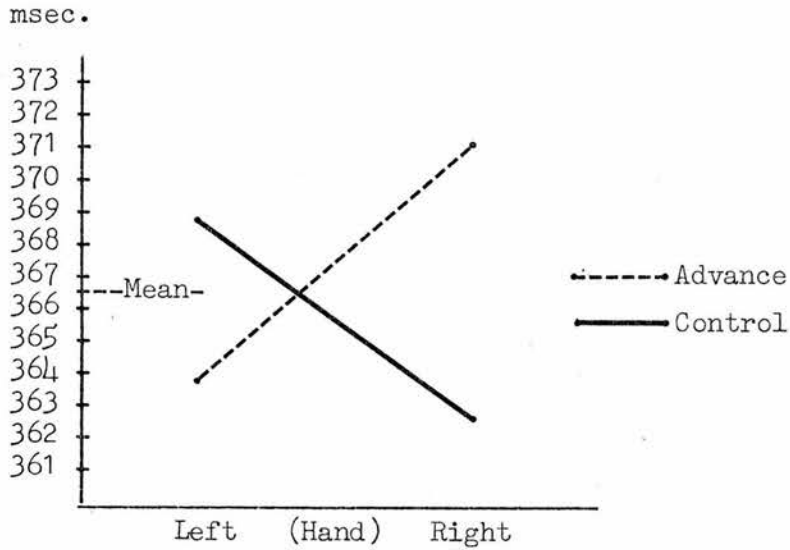


FIGURE 4: Graphic representation of the scoring pattern for the mean reaction times of the second experiment.

GENERAL DISCUSSION OF THE REACTION TIME SERIES

The interesting finding of this series lies in the apparent necessity for the loading of the reading task on the left hemisphere for a psi effect to be registered in this type of experimental paradigm. This interpretation is suggested by the failure of the first experiment, in which no reading was used and to a large degree this development corroborates the findings of the shapes series which indicated the necessity to disrupt some activity of the left hemisphere for psi to show itself. Interesting as these findings were in themselves it was still necessary to wait until a comparable method for loading the right hemisphere could be devised before more definite conclusions could be drawn.

The particular effect which the advance condition warning signal for the agent had on the responses of the subject was not

exactly anticipated in the particular form which was observed. It had been expected that there might be an effect in one hemisphere and not the other or a similar effect in both hemispheres, but not an almost equally strong opposite effect in each hemisphere. Nonetheless the effect is not inconsistent with the earlier evidence. A likely interpretation is that the advance condition had in the right hemisphere (left hand) a direct effect of priming the response and shortening the response time. In the left hemisphere, where the previous experiments indicate a suggestion of psi inhibition, the advance condition responses met with this inhibition and were slightly delayed as a result. It is also possible that the ESP component of the advance warning for the left hemisphere interacted in some as yet undetermined manner with the concurrent verbal task to produce the significantly longer responses. Only the advance trials could have interacted in this way since there was no significant difference between the hands for control trials (in fact the right hand was faster). Such interpretations are tenuous at best, based as they are on a single experiment, but they were sufficiently encouraging in their continued suggestion of hemisphere differences to warrant further development of the technique. Clearly the advance condition effect was paranormal in nature since sensory leakage had been eliminated and the non-reading Experiment I could be considered a control for this. Whether the interaction shown in the analysis can ultimately be attributed to hemisphere differences remains for further work to determine.

Sex Differences

In the shapes series one of the very interesting findings which supported the arguments that the effects in the data represent real hemisphere differences in the processing of ESP was the belated finding of sex differences in the data in precisely the manner predicted by non-ESP studies of hemisphere lateralization. The response time data were in the process of being analysed at the time the question of sex differences was first raised. As a quick check the data from the males and the females for Experiment II were separately subjected to the same analysis of variance which had been used for the combined data. The results showed the very strong presence of the interaction effect in the data from the males, $F = 13.10$ (df 1,8; $p < .01$), but not at all in the data for the females, $F = 1.67$ (df 1,10; n.s.).⁵ This dramatic corroboration of the sex difference in the previous series required a more rigorous test so the data of two of the female subjects were randomly discarded to balance the groups and the remaining data were subjected to a three-way analysis of variance with repeated measures and subjects nested in the sex condition. The results, along with the means are presented in Table 9. The hands by condition interaction is clearly significant, $F = 7.786$ (df 1,16; $p < .02$) reflecting the opposite effects of the advance condition discussed above, and the three-way interaction of sex, hands, and condition was also significant, $F = 4.607$ (df 1,16; $p < .05$) demonstrating the significant difference in the effectiveness of advance condition (with its interaction) between the male and female subjects.

⁵ Because of the informal nature of these tests the tables are not presented here.

TABLE 9a: Condition means (in msec.) according to sex of subject for Experiment II of the Reaction time series with two females excluded to balance the groups.

	Left Hand Control	Left Hand Advance	Right Hand Control	Right Hand Advance
Males	375.7	369.7	364.7	379.1
Females	358.3	357.3	351.4	353.1

TABLE 9b: Summary table for three-way analysis of variance with repeated measures, subjects nested in sex for Experiment II of the reaction time series.

Source	SS	df	MS	Error ^a	F	
Sex	5338.8	1	5338.8	s(S)	0.883	
Hands	180.5	1	180.5	sH(S)	0.571	
Condition	93.4	1	93.4	sC(S)	0.594	
subjects	96738.3	16	6046.1			
S x H	102.7	1	102.7	sH(S)	0.325	
S x C	68.0	1	68.0	sC(S)	0.432	
H x C	600.9	1	600.9	sHC(S)	7.786	p < .02
s x H (S)	5059.6	16	316.2			
s x C (S)	2517.5	16	157.3			
S x H x C	355.6	1	355.6	sHC(S)	4.607	p < .05
s x H x C (S)	1234.8	16	88.2			

^a Refers to the source of the error variance for the F ratio calculation.

The data from Experiment I were examined as well but no sex differences were noted. This is, of course, to be expected since the sex difference appears to be related to the effectiveness of the reading task which was not used in that experiment.

The sex differences in the data of Experiment II are strongly in accord with the findings of the shapes series. In the absence of the reading task no psi effect was noted. With the reading a clear psi effect was found and it was stronger for the male subjects than the female ones. The interpretation of this finding for the shapes series was that the reading managed to disrupt sufficiently some psi-inhibitory function of the left hemisphere so that the normally 'censored' psi managed to emerge in the response. The sex differences provide further support for this interpretation in that this demonstrates that the reading task, known to be more effective as a disrupter for males, also is more effective in disrupting the psi-inhibitor. For females, who appear to be less lateralized for language, the reading task is less disruptive for the psi inhibitor either because the reading task is not sufficiently concentrated in the appropriate area or because the psi-inhibitor is not lateralized either but more diffusely organized.

At first sight this sort of interpretation might seem applicable to the reaction time findings as well, but there is one problem. The more significant difference between the advance and control conditions occurs for the right hand (left hemisphere) and it occurs in such a way that the advance condition yields longer response times. If the reading task is effective in disrupting the inhibition of psi why is the advance condition so much longer in response? A possible solution to the problem might be to argue that there are two kinds of inhibition in operation here. The first kind, the psi inhibition, is knocked out by the reading task

and the psi 'message' becomes available. Thereafter, however, in the left hemisphere, the more focal (in Trevarthen's terminology) or logical one, the psi message conflicts with the obvious fact that there is no tone and there is thus an inhibition of any premature response on that hemisphere's part. This situation continuing to the point when the tone finally does come results in a generally retarded response. In the right hemisphere, unconcerned with such details, the advance warning gets through in a more straightforward, albeit weaker, manner.

In summary, the findings of the reaction time, in their own peculiar way, provide support for the findings of the shapes series and thus strengthen the case for hemisphere asymmetries in the processing of paranormal information.

CHAPTER V

THE GROWING AWARENESS OF A
PSI - BASED EXPERIMENTER EFFECT

CHAPTER V

THE GROWING AWARENESS OF A PSI-BASED EXPERIMENTER EFFECT

Since, as was stated at the outset, the main goal of this investigator's researches was to find a repeatable experimental paradigm for parapsychology it was a good strategy to remain alert for useful clues not only in the main work at hand, hemisphere differences in ESP, but also in the other research activities of the Edinburgh Unit and in developments in the field in general. As is the case in most major research programmes there are considerable gaps between actual experiments due to the need to develop new procedures and equipment, and to thoroughly test them, not to mention the time taken for background literature searches. This investigator was fortunate enough to be able to fill in some of those gaps by collaborating with a colleague, Brian Millar, in the testing and following up of other promising hypotheses and experimental leads. None of these collaborative efforts yielded any psi in the conventional sense but in some there were curious features in the data which began to suggest a pattern. This work is discussed here because certain clues to the problem of repeatability emerged only after some time when a relationship became apparent between this pattern and the investigator's main line of work on hemisphere differences.

By late 1973 the most hopeful sign that parapsychology would be able to develop a repeatable experiment was the rodent ESP work being carried out at Rhine's Institute for Parapsychology and elsewhere throughout the world. It had as its start a paper by Duval and Montredon (1968) which demonstrated that mice were able to avoid randomly distributed shocks to a very significant degree.¹ The successful avoidance of the shocks was found only in what were termed Random Behaviour Trials (RBTs) or trials in which the animal behaved apparently spontaneously², but that indicated that these trials were apparently ones in which the animal was urged to move by a psi-impulse. The Duval and Montredon work was seen as particularly important for two reasons. First, it addressed the long standing question of whether or not psi was an exclusively human ability not shared by the lower animals. Secondly, since the experiment could be done with rodents which presumably would not be subject to the vagaries of interpersonal relationships, alleged by some to have been at the root of failures to replicate psi effects, long series of automated experiments in many different laboratories could now be carried out.

This work was quickly followed up in the United States where W.J. Levy and his colleagues at the Institute for Parapsychology

¹ The experimental set-up for this sort of experiment involved a binary random number generator which governed the delivery of a mild shock to one or the other half of the cage floor. The animal's position was monitored to see if it had been on the 'safe' (hit) or 'shocked' (miss) side.

² Technically: If when not shocked on trial n-1 the animal changed sides for trial n or if when shocked on trial n-1 the animal remained on the same side for trial n .

conducted a number of experiments with mice, gerbils (jirds) and hamsters dramatically confirming the rodent's ability to 'precognize' which side of the cage would be shocked and so move safely away.³ Later work at the same institute featured a completely automated apparatus in which not only were the animals monitored automatically (as in the earlier work) but they were also automatically placed in and removed from the test cage thus allowing such experiments to go on in the absence of the experimenter.⁴

In Utrecht, Schouten (1972) used positive reinforcement and demonstrated that mice could correctly guess which of two levers would cause a drop of water to be administered.

The generally high rate of success for the animal work (no failures had yet been reported) led Randall (1975) to conclude:

" . . . there is only one fact which has, in the writer's opinion, been established beyond all reasonable doubt, namely the existence of precognition in rodents."

(p.92)

EDINBURGH GERBIL EXPERIMENTS

With various experiments demonstrating rodent precognition with aversive stimuli and at least one study confirming this effect with positive reinforcement there seemed reason to believe that

³ Levy, Mayo, Andre, and McRae, 1971; Levy and McRae, 1971; Levy, 1972.

⁴ Levy, Davis, and Mayo, 1973; Levy, Terry and Davis, 1973.

this paradigm would work in the Edinburgh Laboratory. In the summer of 1973 Adrian Parker, a parapsychology postgraduate, undertook a rodent ESP study in which the animals were given food rewards for correct responses. A description of this experiment follows.⁵

Procedure

Three gerbils were trained to press either of two operant keys to obtain a reward of a sunflower seed. Whether it was to be the right or the left key which produced the reward was determined by a random sequence coded onto punched paper tape. The ESP task for the gerbils was to guess correctly which of the two keys would produce the food reward on any given trial. Normal precautions against sensory leakage of the coming targets were taken. The logic apparatus only advanced and examined the target tape after the response had been made. The random sequences were prepared by the experimenter using published tables.

The animals were brought to a state of mild food deprivation and then trained in the testing cage. When all animals were accustomed to a roughly 50% payoff and were not favouring one or the other key the experiment proper started. Seven experimental sessions were run at a rate of roughly one every other day. A session was terminated when an animal produced no response for two minutes. The general method of conducting the experiment was, (1) place the paper tape in the reader at an

⁵ The experiments to follow have been reported in detail elsewhere (Parker, 1974; Broughton and Millar, 1975). The report here will contain only sufficient detail to convey the general idea of the project.



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arbitrary starting point, (2) place the hungry animal in the cage, (3) leave the room and wait for the sound of the apparatus to indicate that the end of the session had been reached. The sequence of targets and responses was recorded on a 12-channel event recorder and scored (some weeks later) by the experimenter and a colleague working independently.

RESULTS OF THE PARKER SERIES

The particular method of ending the session in this experiment meant that the animals performed varying numbers of trials. The overall results indicated a strong confirmation of the previous work. In this case however, not only were the Random Behaviour Trials (the source of the high scoring in the negative reinforcement work) significantly above chance expectation, $CR^6 = 2.03$ ($p < .05$, one tailed), but also the non-random behaviour trials, $CR = 1.94$ ($p < .05$, one tailed). The total scoring rate, which was not usually significant in the negative reinforcement work, was also significant in Parker's experiment, $CR = 2.26$ ($p < .005$), one tailed). Table 10a sets out Parker's results in detail.⁷

⁶ Critical Ratio: the observed deviation divided by the standard deviation (\sqrt{npq} , where n = trials, p = probability of a hit, q = probability of a miss per trial), and evaluated as a standard normal deviate.

⁷ A particular feature of the gerbils' behaviour toward the response key led to an ambiguity in scoring a small number of trials which in turn resulted in a slightly greater number of trials being recorded than the number of times that the animal actually caused the tape to advance. Parker reports both the uncorrected scores and a corrected version. The uncorrected scores, being more conservative are used in this paper.

THE BROUGHTON - MILLAR SERIES

This investigator and his colleague, Mr. Millar, arrived at the Edinburgh Parapsychology Unit only a few months after the completion of the Parker Experiment. All of the equipment used by Parker and even some of the same animals were available. It was thought by the unit's director, Dr. Beloff, that it would be useful to have these two new investigators start off with a replication of Parker's successful experiment. Using virtually the identical equipment as Parker and even two of the original animals, plus three new ones, a replication study was undertaken during the winter of 1973. The new animals were trained to the requisite standard and the veterans were given a refresher course.

The general procedure for conducting the experiment was the same as Parker had used with a few minor differences. The number of trials per session was to be fixed in advance and sessions were to be run every day. Additionally, a larger number of trials would be run and split into a pilot-confirmation design. Since there were two experimenters involved they would run the animals on alternate days and hold the data separately.

Results of the Pilot

The overall results bore no similarity to Parker's in any respect and totally failed to confirm his findings. Not even the Random Behaviour Trials provided any evidence of above chance scoring. These results are reproduced in Table 10b. Parker's

work had suggested a difference between animals so this study included a two-way analysis of variance on the total scores examining gerbils by days (in the sequence). The result did suggest a difference between animals with $F = 3.184$ (df 4,20; $p < .05$) although this was largely due to two of the animals. One finding emerged, however, and though not significant seemed rather curious to the investigators: the result obtained for the data collected by R.B. was noticeably better in terms of scoring than that of B.M. The analysis of variance used for the overall experiment was then applied to the data from each experimenter and this showed that the observed differences between animals was concentrated in R.B.'s data, $F = 4.500$ (df 4,8; $p < .05$) while completely absent from B.M.'s data, $F = 1.38$ (df 4,8; n.s.).

It was difficult to know how to interpret these findings. On the face of it the experiment was a dismal failure to replicate the Parker findings yet there were the differences between animals and suggestions of experimenter differences in what should be chance data. It was decided to go ahead with the main study.

The main study was conducted in precisely the same manner as the pilot except that the number of trials per day had been reduced to 50 with a total of 800 per animal. The study included only four animals, one having died. The housing of the animals was improved by providing an 'enriched' environment which Levy and McRae (1972) had indicated as having a beneficial effect on scoring.

Main Study Results

The overall results were as those of the pilot with chance scoring rates in both the RBT and the non-RBT trials. These data are summarized in Table 10c. The two way analysis of variance failed to show any significant differences either between animals or days.

What remained in the main study was the same tendency, noted in the pilot, for RB to have 'better' scoring than BM. Again the difference was not significant. The analysis of variance on each experimenter's data provided no significant differences for BM but a significant effect of days for RB, $F = 3.051$ (df 7,21; p < .05), though no effect of animals as noted in the pilot.

One of the early comments regarding the securing of repeatability still heard today (Honorton, 1976) is that it may be necessary for intending replicators to carry out the study in the very same lab in which the original work had been done, using the same equipment, etc. In the Broughton-Millar gerbil study that prescription could not have been followed more closely, yet the experiment was a total failure to replicate the previously successful work. All that could be gleaned from this study was that rodent ESP was not quite the simple matter that it had been made out to be, and the curious suggestion regarding the differences in the data from the two experimenters. This tendency for one experimenter to obtain higher scores on the days on which he ran the animals, coupled with the fact that the same experimenter

TABLE 10a: The results of Parker's gerbil study.

Animal	Random Behaviour Trials				Total			
	Trials	Hits	Rate	CR	Trials	Hits	Rate	CR
Gerbil 1	98	51	52.0%	0.30	737	398	54.0%	2.14*
Gerbil 4	162	94	57.0	1.96*	884	469	53.1	1.78*
Gerbil 5	169	91	53.6	0.93	910	465	51.1	0.63
Total	429	236	55.0	2.03*	2531	1332	52.6	2.62**

TABLE 10b: Results of the Broughton-Millar pilot study to replicate Parker's findings.

Animal	Random Behaviour Trials				Total			
	Trials	Hits	Rate	CR	Trials	Hits	Rate	CR
Gerbil 1	100	45	45.0%	-0.90	480	238	49.6%	-0.14
Gerbil 5	60	24	40.0	-1.42	480	228	47.5	-1.05
Gerbil 7	71	30	42.0	-1.19	480	229	47.8	-0.91
Gerbil 9	81	42	51.6	0.22	480	265	55.2	2.24*
Gerbil 10	121	65	53.7	0.73	480	250	52.2	0.91
Total	433	206	47.6	-0.96	2400	1210	50.5	0.43

TABLE 10c: Results of the Broughton-Millar Main study to replicate Parker's findings.

Animal	Random Behaviour Trials				Total			
	Trials	Hits	Rate	CR	Trials	Hits	Rate	CR
Gerbil 1	185	89	48.1%	-0.44	800	412	51.5%	0.81
Gerbil 5	174	90	51.7	0.38	800	405	50.6	0.32
Gerbil 7	157	90	57.3	1.76*	800	392	49.0	-0.53
Gerbil 10	185	92	49.7	0.00	800	405	50.6	0.32
Total	701	361	51.5	0.75	3200	1614	50.4	0.48

* $p < .05$, ** $p < .005$, one tailed.

managed two significant F tests in his data which did not seem related to anything in particular appeared to the investigators more than the chance coincidences others might have viewed it as. The investigators duly reported their observations in the paper related to this work:

"The only suggestion, and a very tenuous, post hoc one at that, which emerges from the data is that one experimenter tended to obtain scores with a lower overall variance for both animal and day differences than did the other."

(Broughton and Millar, 1975; p.27)

At this time this failure to replicate was rather disturbing both to the investigators who conducted the study and those who took the problem of repeatability in parapsychology seriously. It represented a single dissenting voice in a sea of studies claiming to show ESP in rodents. This changed very quickly when it developed that W.J. Levy, the principal investigator for most of the published American rodent work, was found to have been faking his data, albeit in a quite different experiment. Since that time no successful rodent ESP studies have been reported. (For a more detailed account of the events following the Broughton-Millar study see Appendix B.)

TABLE 11: Comparison of the two experimenters' scores in the Broughton-Millar gerbil study

<u>E</u>	Pilot				Main			
	Trials	Hits	Rate	CR	Trials	Hits	Rate	CR
RB	1200	625	52.1%	1.44	1600	817	51.1	0.83
BM	1200	585	48.8	-0.81	1600	797	49.8	-0.13

What remained with the investigators at the conclusion of the gerbil study was a feeling that R.B. had a tendency to get slightly better results and that either he had a tendency to increase the variance in the experiment or B.M. had a tendency to decrease it.

Variance as an indicator of paranormal influence on scoring had been under investigation by parapsychologists for some time. The rationale behind this, as pointed out by Thouless (1972), is that with ample data from various experiments demonstrating both psi-hitting and psi-missing there is always the possibility that both might be operating, for whatever reason, with a single subject or in a single experiment. The presence of a substantial but bi-polar psi effect could be reflected in data with an observed variance significantly greater than what would be expected by chance. Carpenter (1966) has found just this effect in analysing halves of ESP guessing runs. More puzzling is the phenomena of exceptionally low variance in ESP scores which has been found by some experimenters (Rogers, 1967; Stanford, 1966). Stanford suggests that very low variance may be the result of some internal cancelling mechanism which balances the hitting and missing quite precisely within any given run. Thouless (1972) has taken the problem of variance as an indicator of psi quite seriously and describes at length various ways of looking at the ESP scoring variance.

MULTIPLE RATE RNG EXPERIMENT⁸

The possibility of variance effects was on the minds of this investigator and Mr. Millar when there was an opportunity for collaboration on a Schmidt-type PK experiment on a random number generator. Schmidt's work is discussed in Chapter II and in this case the experiment was designed to extend Schmidt's (1973) experiment investigating the efficiency of the PK effect as related to the speed at which the RNG operated. Using rates of 30 and 300 trials per second Schmidt found that psi-efficiency with respect to time was similar under the two conditions, though there was a nonsignificant tendency toward lower efficiency at the higher rate.

It would be of great theoretical interest to know if there were upper or lower limits to the operation of PK in such devices as fast random number generators so an experiment by Millar and Broughton (1976) set out to extend the range of speeds used by Schmidt. Coupling a random switching unit with a Linc-8 computer provided a random number generator similar in principal to Schmidt's and capable of operating at speeds of up to 1000 trials per second.⁹

The task for the subject in this experiment, as in Schmidt's, was to cause a binary random number generator to deviate from its normal 50-50 output probability. In this case the trial-by-trial output of the RNG was displayed as a vertically moving dot

⁸ Details of this experiment have been published elsewhere (Millar and Broughton, 1976).

⁹ For a detailed description of this device see Appendix A which describes the hardware and software used.

on an oscilloscope screen and the subject was asked to push the dot as far away from the central position (representing MCE) as possible. Subjects were, of course, given a demonstration of the display and were aware that they were being asked to perform a PK task. They were, however, unaware of the fact that during the 400 seconds of their effort the RNG ran at four different rates, 1000, 100, 10 and 1 per second, each for 100 seconds in a counterbalanced arrangement. Thus for each subject there were four conditions with 100,000 trials at 1000 per second, 10,000 at 100 per second, 1,000 at 10 per second and 100 at 1 per second.

Twenty-four unselected subjects were run in this experiment. Unfortunately, in the Edinburgh tradition there was no evidence of any above chance scoring in any of the conditions. Unlike the gerbil experiment this was not considered an important failure to replicate as a number of substantial changes from Schmidt's original design had been introduced deliberately as an extension of his work and these could account for the lack of scoring.

It had been planned at the outset to examine the scoring variance in the different conditions and to examine the data collected by each experimenter separately. The examination of variance was done as follows: For each subject the total score and the four condition scores were converted into standard (Z) scores calculated against the theoretical mean. The mean Z-score and the best estimate of the variance about that mean was calculated. This procedure allowed the different trial rates and totals to give

comparable results. In this case the theoretical value of the variance is 1 with infinity degrees of freedom and the resulting empirical variances can be assessed by an F test. The overall results of the experiment indicated that there was a very significant departure from chance variation in the slowest of the trial rates, 1 per second. The results are given in Table 12.

TABLE 12: Variance measures for the Millar-Broughton multiple rate RNG experiment, combined data.

	All con's.	1000/sec	100/sec	10/sec	1/sec
Mean Z	0.11	0.05	0.15	0.12	0.12
Variance	1.28	1.24	0.69	1.19	1.78*
F (.05) = 1.52, F (.01) = 1.79, with 23, ∞ df.					

Because the experimenters were responsible for running the subjects which they recruited they ran unequal numbers of subjects, with R.B. accounting for nine and B.M., 15. When the data were split according to experimenter, in both the total score and each of the conditions R.B.'s variances were higher than B.M.'s with one of R.B.'s rates (1/sec) being significantly higher than the theoretical expectancy, one other being very nearly so ($p < .06$), and the total variance approaching significance as well. These data are set out in Table 13. Informally this evidence strongly suggested to the collaborating experimenters that when R.B. ran subjects and collected the data there was an increase in the variability of the scores, almost significantly above chance

expectation. When B.M. ran subjects there was no such increase in variances and with B.M. and R.B. working together there was no evidence of above chance scoring.

TABLE 13: Variance measures for the Millar-Broughton multiple rate RNG experiment split according to experimenter.

	All con's	1000/sec	100/sec	10/sec	1/sec
<u>B.M. (15 Ss)</u>					
Mean Z	0.07	0.04	0.04	0.17	0.25
Variance	1.06	0.95	0.66	1.02	1.32
F (.05) = 1.69, F (.01) = 2.07, with 14, ∞ df.					
<u>R.B. (9 Ss)</u>					
Mean Z	0.18	0.08	0.33	0.02	-0.12
Variance	1.80	1.91	0.78	1.62	1.96*
F (.05) = 1.94, F (.01) = 2.51, with 8, ∞ df.					

The 'impression' regarding the relative effects on scoring by the two experimenters was now supported by two quite different experiments, one with animals and the other with people. In both cases the experiments were highly automated with no chance of recording errors accounting for the differences between the experimenters. Though the obvious possible source of differences in subject performance, the experimenter-subject interaction, could not be as well controlled as the recording of results the two experimenters were well aware of this possibility and made every

attempt to minimize differences in this area. This was done by having standardized, almost ritualized, procedures for testing and handling the animals and by having written instructions to be read to the human subjects and standardized ways of dealing with them as well. Also, while experimenter differences in interaction had been known to increase or decrease subject performance there was no evidence that it affected the variance to any great degree.

In themselves the variances in the collaborative experiments might not have been that interesting were it not for the curious parallel picture which was emerging in experiments carried out by the two investigators separately. This investigator took a virtually untried idea and in a series of experiments obtained quite impressive evidence of ESP. Millar, on the other hand, spent much of his time trying to replicate studies in which other investigators had already found ESP evidence. When Millar turned his hand to these experiments he could find no evidence of ESP whatsoever.¹⁰

So two closely working experimenters in a single lab were faced with a perplexing paradox. One experimenter seemingly could take 'any old crazy idea' and get it to produce psi whereas the other could not even get previously successful experiments to work. Of course, there is a considerable literature, dating from the early days of Rhine's work at Duke University, which addresses this problem directly. There had always been experimenters such

¹⁰ See, for example, Millar, 1976a,b.

as Millar who could not produce psi no matter how hard they tried and the early parapsychologists were aware of this fact even if they were not sure why this should be.

McFarland (1938) is generally credited as one of the first parapsychologists to notice differences between experimenters in obtaining psi results. He conducted tests of five subjects who made over 15,000 calls at target cards which were being handled by one of two other experimenters. One of the assisting experimenters had obtained positive results in previous work and the other only chance results. Significant positive scoring for an astronomical CR of 11 was obtained with the first experimenter's targets but only chance results were obtained on the other experimenter's cards. Neither experimenter was in the room with the subject during testing.¹¹

J.G. Pratt was an early and highly enthusiastic parapsychologist although he found himself unable to find psi in his experiments. A colleague of Pratt's, M.M. Price generally managed to be successful in her psi experiments. These two researchers collaborated in one of the first investigations into what makes for successful psi experimenters (Pratt and Price, 1938). They did not systematically compare results but arranged the experiment so that Price handled the subjects under different conditions while Pratt attended to the recording and to securing

¹¹ Stanford has subsequently discovered a statistical error which should considerably reduce the high scores. Nonetheless the scoring is still highly significant.

the site against sensory leakage. When Price was required to treat the subjects 'unfavourably', which she found difficult to do, she was unable to elicit psi in either that condition or the corresponding 'favourable' one. In the second part of the experiment when she could deal with subjects without any restrictions her subjects scored at levels similar to her other successful experiments. The interpretation offered by Pratt and Price concentrates on the particular manner in which Price works with the subjects, a very supportive and playful manner, and contrasts that with Pratt's particularly businesslike approach.

Numerous other experiments in which two or more experimenters working under very similar conditions obtained very dissimilar results can be found in the literature. Nicol and Humphrey (1953) obtained strikingly different results between them although they used the same subjects and tests under very similar conditions. Sharp and Clark (1937) reported an experiment using four experimenters to conduct group tests. The results indicated that experimenters obtained scoring rates commensurate with their belief in ESP which ranged from highly positive to decidedly sceptical. Osis and Dean (1964) conducted an ESP test with lecture audiences. Both experimenters used the same lecture notes and test directions but there was a significant difference in the results they managed to get from their groups. Osis obtained significantly positive results while Dean managed only chance results. In all these cases, as in several not mentioned, the explanation of the differences was in terms of the psychological interaction between the experimenter and his subjects.

Some experimenters have the proper personality, the 'right touch' for getting subjects to score, while others simply do not.

Thus for decades the received opinion on experimenter differences was that it was all explicable by recourse to assets or deficits in the experimenter's personality, manner and enthusiasm for the experiment. J.B. Rhine, who has seen more psi experimenters come and go than any other parapsychologist and Pratt, an early recruit to Rhine's team, have observed:

". . . Those who never succeed at all may, of course, be suspected of not ever having felt some contagious or communicable interest as would help to create a favorable test environment for their test subject."

"A psi experimenter is one who, under conditions that insure he is not fooling himself, can get results. All others should do something they can do well."

(Rhine and Pratt, 1957; p.132)

As the number of papers accumulated which discovered differences between experimenters' ability to get results it became abundantly clear that an infinite number of such psychological explanations for the differences could be found. Indeed a certain 'lab lore' was built up regarding the knack for getting psi from subjects. It was tempting for the two Edinburgh investigators to use a similar explanation to account for R.B.'s ability to get psi and B.M.'s inability except for the fact that the observed differences in the experimenters' interpersonal styles was the exact opposite of what Rhine's theory and the lab lore would predict. In the experimental situation B.M. was the more informal, enthusiastic and outgoing whereas R.B. tended to be more formal, cool and 'professional' with subjects.

Though parapsychologists had from the earliest days been conscious of the experimenter effect in their research, and in this respect were well ahead of their psychological colleagues, in the mid 1960's they seized upon Rosenthal's (1966) work as 'proving' what many of them had been saying all along. Some parapsychologists accepted the Rosenthal hypothesis and used it either to generate parapsychological tests of the Rosenthal model or as a basis for a model of parapsychological experimenter differences. Honorton, Ramsey and Cabibbo (1975) used this model for an experimental series which demonstrated differences in scoring which varied as a function of the experimenter's attitude. Kennedy and Taddonio (1976) in an important paper to be discussed later, accepted the Rosenthal effect as being a model upon which the search for parapsychological experimenter effects should proceed. Parker (1975b), working in the Edinburgh Psychology Department conducted a parapsychological analogue to Rosenthal's work. A group of undergraduates were told that parapsychological effects had been 'proven' by science and they should get high scores in an experiment they were about to conduct. Another group were told that parapsychology was riddled with flaws and artifacts and they would obtain scores at chance levels. The scores obtained by the respective groups of experimenters did differ significantly in the expected direction.

However, there is a strong danger that in following the Rosenthal model parapsychology may be building a house of cards. Rosenthal's work has been severely criticized by Barber and Silver (1968; Barber, 1969), primarily for careless use of statistical

procedures, and serious doubts have been raised regarding the strength and pervasiveness of the so-called Rosenthal effect. It is thus not surprising that when Parker (1977) concluded his extensive study of the applicability of the Rosenthal model to parapsychological experimenter differences he was of the opinion that it was not sufficient to explain the sort of differences one typically finds in this work. The fact remains that the experimenter differences noted in many parapsychological experiments are primary, first order effects; not the second and third order interactions which characterize much of the later Rosenthal work and are justifiably criticized by Barber and Silver.

A very serious problem for those who would explain parapsychological experimenter effects by the Rosenthal model or by appeals to ill-defined personality differences is the fact that there are a few examples of strong experimenter differences where one or both of the experimenters had no contact whatsoever with the subjects.

The most notable of these experiments is a collaborative effort between G.W. Fisk and D.J. West, the former being known for his ability to obtain good scores in experiments and the latter having obtained only chance results for most of his work. In this experiment (West and Fisk, 1953) twenty subjects were tested by post using a new type of target. Half of the targets for each subject were prepared by Fisk and half by West, but as far as the subjects knew Fisk was the only experimenter since he alone corresponded with them. As the targets were returned they were

scored by the experimenter who had prepared them. The results for the experiment as a whole demonstrated highly significant hitting ($p = .0011$) but it was found that all of the above chance scoring was on the targets prepared by Fisk, whose data alone were highly significant ($p = .00015$), whereas the targets prepared and scored by West yielded only chance results.

Fisk and West (1958) conducted a similar type of experiment with a single subject performing a PK task. As before the subject believed Fisk to be the only experimenter and the experiment was by post. Just as before Fisk's data provided a significant positive deviation while West's was at chance with the difference between the two being just significant.

Osis and Carlson (1972) conducted three clairvoyance tests in which targets were displayed at a laboratory in New York City while subjects made their guesses in their homes. The two experimenters, who were rating their moods during the experiment, sat beside the targets which were on the floor. Subjects were instructed to focus their attention on Osis, whom they knew, but were not aware that there was a second experimenter. The planned correlation test between subject scores and experimenter moods revealed that the ESP scores were correlated only with the moods of the experimenter whose presence was not reported to the subject.

Experiments such as these make it clear that explanations of the parapsychological experimenter effect cannot be based on the Rosenthal model. They also seriously undermine the suggestions that the experimenter's communicable enthusiasm and pleasant

personality are the sole requirements for good scoring on the part of the subject.

The experimenter differences noted in some experiments, particularly the ones just discussed were rather discomfoting to parapsychologists. When differences of that sort could be attributed to agent differences, subject differences, observer differences, or any of the various differences which were safely on the 'other side' of the dividing line between observer and observed it was considered acceptable by parapsychologists. A number of experiments had demonstrated that scoring differences could be elicited by agent differences (e.g. Schmeidler, 1958, 1960, 1961; Ullman and Krippner, 1970). Feather and Brier (1968) have demonstrated that groups can score differently depending upon who will be checking their score sheets. Van de Castle (1970), in an extensive series of card guessing tests with Cuna Indians provided quite remarkable evidence on how subject's performance could vary according to the level of concentration by the agent. The interpretation which Van de Castle provides, that intense concentration by the agent is counterproductive, has been an important guide for subsequent advice given to agents. There is, however, a serious question as to whether this applies to the 'agent', as Van de Castle proposes, or the experimenter since they were one and the same person in this case (as in many parapsychology experiments).

THE EXPERIMENTER'S PSI

The crux of the problem, which parapsychologists with few exceptions have been loath to confront, is that there is virtually no difference between experimenter effects on the one hand and subject or agent effects on the other. The distinction in parapsychology between the experimenter and the various participants in the experiment to whom the 'cause' of various effects are attributed begs a very important question: If psi abilities are demonstrated in those people designated as subjects how can one be sure that the people designated as experimenters cannot also use psi, consciously or unconsciously and in all the ways that subjects do, to achieve desired ends in their experiments? Eisenbud (1963) was one of the first parapsychologists to recognize this blind spot in parapsychology and what he wrote over a decade ago can hardly be better put today:

" . . . It seems implicitly to be taken for granted that experimenters (or 'independent' judges or checkers or raters, for their part) will not, for whatever obscure reason, use any psi faculties they may have to muddy the field. In the conventional experimental report, in any case, the possible effect on valid inference of having these unregistered undercover agents around is glossed over in a triumph of 'There's no one here but us checkers' double-think. Everyone behaves, in short, as if there were some sort of gentleman's agreement committing subjects, experimenters, judges and other participating personnel to stick faithfully to their assigned roles in the experiment as scripted and to neither take any notice of nor infringe upon what any of the others are doing."

(Eisenbud, 1963; p.258)

The question is: Do the experiments reported in parapsychology, which obviously display extra-chance effects, reflect

the psi abilities of the subject in succeeding in trite and frequently boring tasks or the psi ability of the experimenter in getting complex situations (the experiment) to work out in a particular pattern? Sadly for the progress of parapsychology this question has all but been ignored by researchers, with the notable exception of Eisenbud and two others,¹² until 1976 when it was thrust upon the field by the virtually simultaneous (but independent) publication of two excellent review papers dealing with the problem of a psi-based experimenter effect.

Both papers, Kennedy and Taddonio (1976) and White (1976a,b)¹³ cover the same ground, including, of course the material which has been summarized here. They do, however, make different points which are worth reviewing.

The Kennedy and Taddonio paper begins by surveying the evidence which went to create the 'traditional view' that differences between experimenters can be ascribed to personality factors or failures to successfully motivate subjects to demonstrate psi. They feel that motivation may play a major part in the production of psi in the laboratory but place this aspect of the experimental situation in a new and very important perspective:

"A question central to the issue of experimenter effects is: Who has the greater motivation in the experimental setting, the subject or the experimenter?"

¹² White and Angstadt, 1965; Honorton, 1976.

¹³ Actually what is referred to as White's paper is a pair of papers. The first surveys the evidence for psi influences other than those of the subject and the second treats just the experimenter.

"Parapsychologists should face the fact that an experimenter is typically more motivated than his subjects to achieve successful results."

(p.6)

A likely candidate for the mechanism by which the experimenter can affect (or perhaps effect) the outcome of his experiment is PK. Whether the effects are ultimately attributed to the experimenter or the subjects it can hardly be denied that there are numerous, well done experiments demonstrating psychokinetic effects on mechanical, electronic, and biological systems. Rhine (1975a) raised the possibility of 'unauthorized PK' in a paper discussing the indeterminacy of psi. For him, as with others, this problem became more sharply focused during the early '70s when there was a large number of animal psi (anpsi for short) studies. These experiments claimed to show that animals had ESP. Since it had frequently been demonstrated that humans could affect the generation of random numbers (such as the type which might be used to control an animal experiment) by PK it seemed gratuitous to assume that placing an animal between the highly motivated human experimenter and the random number generator would make any difference. In short there was no possible way of determining whether the significant results of an anpsi experiment were due to the animals or to the experimenter.

Both papers (Kennedy and Taddonio, 1976, p.6; and White, 1976, p.157) treat the question of psi indeterminacy which Rhine raised and point out that it is more than just a side issue for parapsychology. Its significance was not lost on this

investigator and Mr. Millar since it fitted in quite nicely with informal observations on the Edinburgh anpsi work. Parker who had conducted the successful gerbil series had managed to get results in other parapsychological experiments. His active collaborator in the animal work, a postgraduate who declined to have his name associated with the paper because of the Levy scandal, was also known to have a real 'knack' for getting his (non-parapsychological) experiments to come out the way he wanted them to. Millar, on the other hand, in many years of parapsychological investigations, had never managed to find any evidence of psi in his work and his gerbil project continued that trend. This investigator had no record by which to judge his involvement in the gerbil experiment although his results are suggestive of his later successes in his own research.

With the indeterminacy of psi clearly established Kennedy and Taddonio go on to discuss the relevance of various models to the solution of the psi experimenter effect problem. For them Rosenthal's model, which may serve to explain many features of experimenter effect, fails in one crucial area. Rosenthal (1969) produces evidence, and indeed his model predicts, that some experimenters should get better at eliciting the desired performance, simply through practice. What is typically found in parapsychology is quite the opposite; replications by the same experimenter yield poorer results.

A better model for interpreting experimenter effects according to Kennedy and Taddonio is Stanford's (1974a,b) Psi

Mediated Instrumental Response model of psi (see Chapter II).

This model is a very well organized collection of many ideas which have been noted by parapsychologists over the decade. Stanford has applied them to cases of spontaneous psi phenomena although in many instances his model can be applied to experimental situations. The basic points which Stanford makes are, (1) Psi may be more common in everyday life than is commonly recognized, (2) the operation of psi in most situations is unconscious, (3) that psi is employed to serve goals or satisfy needs of the person employing it. The relationship between his model and the problem of experimenter effects is shown in an uncomfortable (for the experimenter) parallel which he draws between religious and quasi-religious rituals (which are at least reportedly efficacious) and the running of an experiment:

"An experimenter preparing his apparatus, getting his animals ready, and then leaving them with some feeling of assurance that the experiment will run and the animals will appropriately 'do their thing' cannot but remind us of certain aspects of magic, ritual, or perhaps petitionary prayer. Something is done with confidence that it will produce a desired result, and the participant, once he has done this, psychologically puts a distance between himself and the outcome. He is not trying to make things happen, but just trusts that they will. Again, such circumstances may provide an optimum opportunity for psychokinetic intervention."

(Stanford, 1974b, p.338)

Stanford's PMIR model, which is also heavily relied upon in White's paper is largely descriptive although certain predictive aspects are claimed for it. Its main use is to reinforce the suggestion that experimenters could use unconscious PK to get what they want. Unfortunately it does not provide any insights into

the way of separating the experimenter's influence on the outcome of a parapsychological experiment from the subject's influence. Consequently the suggestions for further research given by Kennedy and Taddonio are rather weak.

White's two-part paper provides a more thorough review of the literature suggesting experimenter effects and demonstrating their similarity to effects attributed to the non-experimenters in an experiment. White also invokes aspects of Stanford's PMIR model to support the basic argument that experimenters may have a paranormal influence on their experiments but she offers even less than Kennedy and Taddonio in the way of suggestions for future research. Indeed one of Kennedy and Taddonio's stronger suggestions, the fact that there seem to be characteristic 'styles' of affecting experimental outcomes which could eventually be pinned to certain experimenters, is discounted as a possibility by White:

"I question whether even the psychological earmarks and signs of psi Rhine mentions can be pinned down to any one participant in a test. There are no physical barriers to psi. Why should there be psychological ones?"

(White, 1976a, p.158)

Instead of seeking ways of separating the experimenter effects from the subject effects White argues that it may be necessary to abandon such attempts and take a holistic view of the experimental situation, a "*transpersonal, field approach*". Such approaches, while satisfying mystical yearnings, rarely yield the sort of testable hypotheses of which science is made. Thus White's paper, like Kennedy and Taddonio's, provides an excellent summary of

the considerable evidence showing that psi effects do not always come from where they are thought to come from but it falls short of supplying any of the vital answers to the questions which are raised.

A possible reason for this shortcoming in both papers lies in the fact that they both assume that the subject exerts an important measure of psi in the experiment. Indeed they largely reflect only a partial breakaway from the traditional view of experimenter effect. What might be needed is a more radical departure.

The view which is implied in these papers is mostly, though not exclusively a parapsychological analogue to the Rosenthal model. In Rosenthal's scheme the experimenter 'covertly communicates' the proper manner of responding to the subjects by subtle sensory means. In the Kennedy and Taddonio/White view the experimenter communicates by paranormal means the proper manner of responding to the subject who in turn uses his paranormal abilities to obtain the desired result.

To be fair, Kennedy and Taddonio devote several paragraphs to a discussion of the possibility that any experiment may be viewed as a single complex PK task, but in the end they express a view which suggests that they hope this is not the case. Likewise White refers to Schmidt's (1975) suggestions that the psi abilities of all the personnel in an experiment must be taken into account (to be discussed in the next chapter) but she uses this to

support her contention that any attempt to separate experimenter effects from subject effects may be futile. Thus they have failed to take up the more radical implications of the psi indeterminacy problem that Rhine had raised regarding the anpsi work. There it was argued that there is no way of separating the possible PK effect of the animals on the random number generator from the possible PK effects of the experimenter on the RNG to achieve a successful experiment. A strong case can be made for the view that the experimenter has the greater need to exert PK. The question which must now be confronted is: Given the problem of psi indeterminacy what is the difference between animals as subjects and unselected¹⁴ humans as subjects? If it can be argued that in certain psi experiments the animals are irrelevant to obtaining positive results then it can also be argued that in many situations human subjects may be irrelevant to the task of obtaining psi results.

One of the facts which has often been suspected by workers in this field but which was well confirmed by the two review papers is that successful psi experiments are more closely associated with certain individuals (usually the experimenter) than with particular hypotheses or experimental techniques. Parapsychology may lack repeatable experiments but it does have repeatable experimenters. This observation was made all the more important by Kennedy and Taddonio's revelation that many of the most successful parapsychology experimenters have also been very successful in PK tests.

¹⁴ More or less randomly selected as for any psychological experiment, not just those claiming or who have demonstrated psychic powers.

"If the hypothesis of experimenter PK is correct, one would expect successful PK experimenters to also be successful PK subjects. A preliminary look at the literature uncovered remarkably consistent support for this notion. For example, J.B. Rhine (1943; Averill and J.B. Rhine, 1945; J.B. Rhine et al., 1945), Nicol (Nicol and Carrington, 1946-1949), Humphrey (1947), McConnell (1955), and Forwald (1961, 1962, 1963; see also L.E. Rhine, 1970) were also successful PK subjects as well as successful PK experimenters. The same is true of W.E. Cox, long regarded as a high-ranking PK experimenter (Cox, 1962, 1965; L.E. Rhine, 1970). In addition, Helmut Schmidt (1973, 1974) finds he is often his own best subject."

(Kennedy and Taddonio, 1976, p.8)

Charles Honorton, who for many parapsychologists is the best example of a researcher with a 'golden touch' for the parapsychological experiment has himself wondered whether the psi effects were from him or his subjects. Honorton and Barksdale (1972) revealed a situation similar to that in Edinburgh. In a muscle relaxation/tension and PK experiment with Honorton as experimenter significant scoring in the expected direction was obtained but with Barksdale as experimenter the results were at chance. Honorton and Barksdale suggest that this may represent a psi mediated experimenter effect. Even more dramatic evidence of Honorton's knack as an experimenter has been provided by one of his colleagues, John Stump. Stump examined the 'control' data used for Honorton, Ramsey, and Cabibbo (1975). In this experiment it was the procedure that a check on the random number generator was made before and after each subject. This was done by Honorton pressing the machine's response button while not 'trying' to influence it. Sample A was collected before and sample B after each subject was run in the experiment. These samples were analysed

for second-order randomness and each sample provided no evidence of non-randomness. However Stump found that the nonsignificant deviations in Sample A were consistently followed by nonsignificant deviations in the opposite direction for sample B making a nicely balanced randomness check for each run of the experiment. Stump's results were associated with a probability of 10^{-5} whereas the experimental hypothesis itself was confirmed only at the .001 level. As if this disconcerting finding were not enough Stump has found the same effect in an earlier experiment of Honorton's which used a similar procedure (Honorton, 1976).

Introductory statistical texts usually make the point that under the null hypothesis subjects are merely sources of random variation. For this investigator and his colleague, Mr. Millar, both from personal experience and the mass of previously unrecognized evidence, it seemed very possible that the history of experimental parapsychology may for the most part represent a history of gifted experimenters acting by paranormal means on complex sources of randomness. If the source of the psi effect were the experimenter and not the subject under test of a particular hypothesis it was no wonder that parapsychology had so far failed to achieve inter-experimenter repeatability. But how might the source of the psi effects be identified?

CHAPTER VI

OBSERVATIONAL HYPOTHESES

AND PSI

CHAPTER VI

OBSERVATIONAL HYPOTHESES AND PSI

The rather serious problems raised by the inability to separate the possible psi effects of the experimenter from those of the participants to whom the psi effects are traditionally ascribed were anticipated some years before the publication of the Kennedy and Taddonio and Rhea White papers by theoretical physicists attempting to provide some logically consistent model of the operation of psi phenomena in the world. Indeed, the problem of psi-based experimenter effect or psi indeterminacy is but a version of the more fundamental problem which the theorists are attempting to solve. That is: Granted that there are psi effects, what is their source and how are they brought about? During the course of this investigator's research into repeatable psi experiments two proposals have been developed which may prove to be the first step toward the much needed theoretical framework in which the search for the true source of psi effects may be conducted.

It is not surprising that these new ways¹ of looking at psi phenomena are the work of quantum physicists who are more at

¹ To be sure several 'theories' of psi had appeared prior to these but these were generally mere speculative excursions with no formal structure or testable hypotheses.

home in the counter-intuitive world presented by the prevailing world view of quantum mechanics. Despite occasional protestations to the contrary the general practice among parapsychologists was to assume models of psi phenomena which were guided by the laws of Newtonian Mechanics and in which the psi phenomena had rather clear-cut causes and effects, even if it was not possible to specify these completely. The fact that the 'causes' did not appear to be physically connected in any way to the 'effects' was grounds for some parapsychologists to claim that the evidence from their field overthrew the scientific world view. In fact the world view which the parapsychological evidence was thought to have overthrown had fallen some years earlier.

In the classical, Newtonian system it was found that the physical forces in nature had a limited range and that systems could be studied in the laboratory in isolation from the rest of the world, including the experimenter who was passively observing and measuring this isolated system. Thus it was thought that this 'isolated system' was in a single state which could be described by the solution to the classical equations of motion. Furthermore, if one could specify all the initial conditions for such an isolated system the succeeding states of the system could be calculated.

In quantum mechanics things are rather different. First of all a system is not in a single state but in an infinite number of states, each one of which represents a solution to the

Schrödinger equation given the same complete set of initial boundary conditions. The totality of these states is called the state vector. For quantum mechanics it is not a matter of the system rapidly shifting from one to another state but the system is said to be in all possible states simultaneously and the state vector provides the complete representation of that system. Obviously, when the system is observed or measured it is not found to be in a multiplicity of states, i.e. different velocities, different positions, but in a single state. It is precisely the act of measurement which is said to collapse the state vector into one of its component states. Which one of the many states the system is found to be in occurs only probabilistically, not deterministically. The state vector develops deterministically according to the Schrödinger equation. Upon observation, however, according to the Copenhagen Interpretation of quantum theory, the system will be found in a given state only with a given probability. It cannot be specified exactly. The necessity for two distinct equations, one deterministic the other stochastic, in the representation of physical reality with the only connection between the two being the act of observation forms the basis of the so-called 'measurement problem' in quantum mechanics.

In short, in quantum mechanics virtually all things are possible, it is just that some are more probable than others. As this applies to parapsychological phenomena Walker (1976) drives the point home:

"Assume the target, the psychic's goal, is to cause an object, say a pen, to move across a table without being touched or by any normal physical means. Such an event is allowed by quantum mechanics! The probability for this to occur by chance is exceedingly small, but easily calculable. Now, although it is possible for this to occur, given any particular situation, any trial, it is so unlikely for this to occur by chance alone that we may properly ignore the possibility in every usual physical sense. But suppose in some way one could select a desired state or bias the unlikely state so that it is more likely to occur. While there is no physical mechanism to bring this about, quantum mechanics does not forbid the existence of such a mechanism. Quantum theory does place constraints on how such a mechanism could be introduced into the theory and what role such a mechanism would have to take relative to the rest of the physical theory. Thus quantum mechanics itself gives detailed information as to the manner in which we can formulate a theory for the psi interaction with matter."

(p.40)

In the probabilistic world of quantum mechanics what is it that determines the particular event observed? Quantum theory has not yet provided any mechanism for this. All it says for the present is that the observer is implicated in the selection of the observed state. Needless to say this role of observer is a source of much discussion among physicists. Walker (1975) has pointed out that some theorists have shown that the Copenhagen interpretation of quantum mechanics leads to a picture of the physical world where consciousness plays a part. While the main objective of much of this discussion among physicists is to discredit the Copenhagen interpretation it has not managed to do so. Instead a large number of constraints have been shown to exist for any possible mechanism advanced as the means of state selection.

One possible way of securing state vector collapse has been suggested by Bohm (1952) by what are termed 'hidden variables'. For these to be allowed into quantum mechanics they must meet certain requirements, and Walker (1974) has detailed four of the more important: (1) Hidden variables must not be accessible to physical measurement (although associated measures are permitted), (2) Hidden variables must be non-local - i.e. they must not be functions of the spatial or temporal coordinates of the system being observed, (3) Hidden variables must be constrained so that only one observed state occurs and it is the same for all observers, (4) The measurement process involving the hidden variables must not be describable as part of the measured interaction. Thus the consciousness of the observer, as a hidden variable, must have properties quite distinct from physical objects subject to the Schrödinger equation.

By way of background it can be seen that not only does contemporary science as embodied in quantum mechanics permit the sort of 'impossible' phenomena of parapsychology but that it suggests that the manner of bringing about specific events (no matter how improbable) is intimately connected with the consciousness of the observers. The conditions which the observer, or whatever collapses the state vector, must satisfy to be allowed into quantum theory bear a striking resemblance to the conditions which are the most 'paranormal' of those observed about psi events, namely non-physicality, spatial and temporal independence.

SCHMIDT'S MATHEMATICAL MODEL OF PSI

The first of the two models of psi operation to be considered is that of Helmut Schmidt, the same researcher responsible for the type of random number generator which now bears his name. For the record, Schmidt's model existed for some time before its eventual publication in 1975 since it was circulating in typescript some years earlier.² That Schmidt had worked through the implications of quantum mechanics for parapsychology was evident as early as 1972 when at the Parapsychology Foundation Conference he read a paper entitled "A new role of the experimenter in science suggested by parapsychological research". Partly because of the relatively limited exposure of the Parapsychology Foundation Conference material this paper did not have the impact upon parapsychologists which it deserved since from a theoretical point of view it anticipated the empirical conclusions of Kennedy and Taddonio, and White by about four years.

In his 1972 paper Schmidt (1974) reviews the situation with regard to quantum mechanics and touches upon its relationship to paranormal phenomena. He then discusses his own extensive research with quantum process random number generators in which, as is well known, selected subjects achieved quite extraordinary odds against chance. If, as his and other experiments demonstrate,

² Schmidt had apparently formulated his theory while at the Institute for Parapsychology but Rhine, who still maintained his influence over the Journal of Parapsychology, declined to publish it. It was only after leaving the institute that Schmidt finally published the paper in the Journal of the American Society for Psychical Research.

certain persons can influence the statistical odds for the occurrence of a certain event, what might be said of the situation in which a physicist is looking for the traces of an elementary particle in cloud chamber pictures to support a particular hypothesis, or, for that matter, a parapsychologist in testing animals in a PK situation? If there is a disturbance of the statistical probabilities how does one know who or what was responsible?

Another question is raised by the Schmidt work and that is, "Assuming a person is influencing the RNG, how does he manage to time his influence to the millionth of a second and know where to apply it in the complex circuitry?" In fact, Schmidt has directly tested the complexity issue by having subjects attempt to influence both 'simple' and 'complex' RNG's. Subjects proved equally successful in either case. This generates one of the important new ways of looking at paranormal phenomena:

"This situation suggests that psi may, perhaps, not be properly understood in terms of some causal mechanism by which the mind makes the electron hit the Geiger counter just at a cleverly calculated time, but that it may be more appropriate to see PK as a goal-oriented principle in the sense that it aims successfully at a final outcome, no matter how intricate the intermediate steps are. The existence of a goal-oriented principle could be interpreted as a direct action of the future on the present. The electron arrives at the right time at the counter because the event is later displayed to the subject as a hit."

(Schmidt, 1974, p.272)

Schmidt concludes his paper by advocating a study of noncausal systems which may serve to select the particular world

history (or state) which is observed, and that in view of the current state of quantum mechanics more attention must be paid to the role that the experimenter may play in determining the statistical outcome of his experiment.

Schmidt's mathematical model of psi had its first public airing in 1974 at the same Parapsychology Foundation Conference at which Walker presented his theory which will be discussed later. It received much greater currency, however, following its publication in the Journal of the American Society for Psychological Research in 1975 under the title "Toward a mathematical theory of psi".

Strictly speaking, Schmidt's model is a purely mathematical construct in that it is not derived from physical principles, but it assumes the quantum mechanical view of the world. In the 'model world' which he develops he maintains the probabilistic view in which the world may have a large number of possible world histories, each with an associated probability, but he introduces the concept of 'psi sources' which have the ability to alter the numerical probabilities of future histories.

Schmidt's model world consists of three components: Random generators, computers, and psi sources. The first component is a device which, when triggered, sends randomly, i.e. based on a non-deterministic process, a signal to one of its outputs. The probability with which any single output will receive a signal is determined by the internal construction of the device. (In a

binary random generator such as that used at Edinburgh $p_1 = p_2 = \frac{1}{2}$.) The computer operates in a completely deterministic way to record data, evaluate it according to a program, and execute decisions based on that evaluation. So far, in a formal way the model world resembles the 'real world' and Schmidt notes that a human participant, for example the experimenter, is a combination of these first two components. Many of the experimenter's decisions may be made in a logical, deterministic manner but some may result from the known random processes in his brain. In this model world of computers and random generators the future is not uniquely determined by the present. Instead, it is probabilistic, and whenever a random generator is triggered in the course of history then this history splits into N (the number of outputs on the random generator) branches with weights of p_1, \dots, p_n .

The third component of Schmidt's model world is the psi source which, as he puts it, has "*the essential features of somewhat idealized PK subjects*" (p.304). The properties of the psi source are given in mathematical form with no attempt to reduce them to an underlying mechanism. Schmidt illustrates the working of a psi source:

"Consider a binary random number generator with two output channels \underline{P} and \underline{Q} with the associated probabilities \underline{p} and \underline{q} . If the generator is triggered in the absence of a psi effect, the \underline{P} -history (where \underline{P} receives the signal) and the \underline{Q} -history (where \underline{Q} receives the signal) occur with the probabilities \underline{p} and \underline{q} respectively.

"Next let the PK subject, whom we will call the 'psi source', be linked to the generator in such a manner . . . that with every \underline{P} -output signal from the random generator (i.e. for each \underline{P} -history) the

subject receives a rewarding stimulus whereas an output signal at Q has no effect on the subject. In the case of a human subject who is instructed to bring a high rate of P -outputs, this rewarding input signal could be simply a success indicator, and for an animal the input signal might consist in the administration of food, warmth, or some other reward.

"Many experiments with human subjects have shown that under such conditions the relative frequency of P -outputs from the generator may be systematically increased so that the presence of the subject (the psi source) changes the output probabilities of the generator from p, q into p', q' with $p' > p$."

(Schmidt, 1975; p.305)

Using the ratio p/q rather than the quantities themselves, since p/q can have any value between 0 and ∞ while p and q are limited to values between 0 and 1, Schmidt gives the basic psi axiom as

$$p'/q' = \theta p/q . \quad (1)$$

The factor θ , called the strength of the psi source, is determined by the PK ability of the subject and lies between 0 and ∞ . A successful PK subject who increases the frequency of P -outputs from the random generator has $\theta > 1$. When $\theta = 1$ there is no PK effect. For $0 \leq \theta < 1$ there is PK missing.

"Qualitatively speaking, the presence of psi sources changes the conventional probabilities for the different possible world histories in the sense that histories which lead to the stimulation of psi sources (by an input signal) become more probable. We see already an indication of the goal-oriented, non-causal aspects of psi, because in the example cited above the psi source affects the random generator before there is any physical interaction between the generator and the psi source."

(Schmidt, 1975; p.306)

The psi axiom has several implications which may be studied experimentally, the most interesting of these being the addition theorem. If two psi sources were to be stimulated by each P-output the strength of the first psi source θ_a would combine with that of the second psi source θ_b to produce

$$\underline{p}/\underline{q}' = \theta_a \cdot \theta_b \cdot \underline{p}/\underline{q} . \quad (2)$$

Any number of psi sources can be combined and if, in each case $\theta > 1$ (unlikely on existing evidence) then the hit probability \underline{p}' could be made to approach 100%. The additivity of psi sources, which in the general population may be expected to have a mean θ of 1, has important implications for the so-called divergence problem which raises the possibility that all subsequent observers may contribute to the psi effect.

Another implication of the model is that the hit probability \underline{p}' depends only on the \underline{p} , \underline{q} values and the θ of the psi source and thus is independent of the complexity of the random generator. This, as Schmidt indicates, is in accordance with the available evidence.

(The psi axiom can be used, as Schmidt demonstrates, to describe the operation of other forms of psi such as precognition and clairvoyance, although as these aspects are not germane to the basic point here they will not be treated.)

A final, most important aspect of the model is the important role which feedback or knowledge of results plays in the model. Schmidt emphasises:

"In the framework of our model a psi source can have an effect only if it is coupled to the outside world in such a way that it may receive a stimulus. Thus, in the example . . . it is the stimulus to the source which distinguishes the two classes of histories and increases the probability of the P-class. Therefore, in the model, the outcome of a random process can be affected by the source only if this outcome has, at some later time, a physical effect on the source."

(Schmidt, 1975; p.314)

The fact that several successful psi experiments have been reported in which subjects received no feedback serves to highlight the relevance of this aspect of the Schmidt model to the problem of psi-based experimenter effect. In all cases the experimenter receives feedback whether or not the subjects do.

WALKER'S QUANTUM MECHANICAL THEORY OF PSI

Walker, whose theory received its first detailed account in the same Parapsychology Foundation Volume as Schmidt's model (Walker, 1975) but has appeared elsewhere since (Walker, 1976), has proposed a theory distinctly different from Schmidt's although in agreement on certain important postulates.

Walker's theory is based solidly on quantum mechanics and information theory and in order to understand it it is first necessary to introduce certain concepts from Walker's more general quantum theory of consciousness which he has presented elsewhere

(Walker, 1970, 1971, 1974). Basically, Walker holds that consciousness can be identified with quantum mechanical hidden variables, c_i variables as he terms them. The theory rests on there being certain quantum mechanical processes taking place at a neuro-physiological level, specifically, a process involving quantum mechanical tunneling between the macromolecules (Gray's dense projections) lying on either side of the synaptic cleft. These c_i variables serve to connect the two physical processes which are not themselves directly coupled and to collapse the state vectors specifying both what might be termed 'outside reality' and the neurophysiological state into which the brain goes.

As a consequence of his theory Walker derives three data processing rates which should occur in the brain:

- (1) General data processing capability of the brain as a whole at the subconscious level (deterministic synaptic firing)

$$S = 2.4 \times 10^{12} \text{ bits/sec.} \quad (3)$$

- (2) The rate at which data are impressed onto consciousness by the physical processes of the brain (i.e. normal sensation reaching consciousness via quantum mechanically coupled synaptic firing)

$$C = 7.5 \times 10^8 \text{ bits/sec.} \quad (4)$$

- (3) The rate at which the state vector collapse impresses data onto the functioning of the brain (or vice versa)

$$W = 6 \times 10^4 \text{ bits/sec.} \quad (5)$$

The W process is termed by Walker the 'will' since he feels that as it represents consciousness effecting changes on physical systems

it can be identified with the philosophical concept of will. It is the W process which is involved in the state selection of psi events.

An unobserved system is in a large number of potential states simultaneously. Upon observation it is found to be in a single state thus making a transition from many states with differing probabilities to a single state with a probability of 1 while all other states have had their probabilities reduced to zero. For any system for which a change in probabilities has occurred it is possible, using information theory, to specify the amount of information which has been input to effect that change. This information comes via the W channel. The reason psi phenomena are sporadic and difficult to reproduce stems from the fact that in normal consciousness the W process is four orders of magnitude smaller than the C process which is ongoing consciousness. The psi channel has a very poor signal-to-noise ratio as $\underline{W} \ll \underline{C}$ and the chance of large scale psi control of physical events is very small.

Thus for Walker consciousness has the ability, as a hidden variable, to bring about state vector collapse when it interacts (the observation) with a physical system. In some cases consciousness can, via the information on the W channel, secure an alteration of the probabilities of various states such that an ordinarily improbable event can be brought about on state vector collapse.

The mathematical statement of Walker's psi theory

(1975, 1976) is:

$$W\psi\Delta t = -\log_2 P \quad (6)$$

where $W\psi$ is the magnitude of a psi event measured in bits/sec. and $W\psi \leq W$, P is the chance probability for the occurrence of any state that would be recognized as a target achievement and Δt is typically the time taken by the experiment. In order to make his theory comparable with Schmidt's which is a probability biasing model not having a time factor Walker has expressed his theory as

$$\Omega = \sum_i p'_i \log_2 p'_i/p_i \quad (7)$$

where p_i is the a priori probability and p'_i is the psi augmented probability for the i th target state and Ω is the psi information in bits/sec. Walker's Ω is an entirely different measure from Schmidt's θ but a θ_{qm} can be derived if the psi factor is made a function of the probability.

Walker's theory also has an addition theorem which is expressed

$$\Omega = \Omega_a + \Omega_b \quad (8)$$

Walker (1977) has demonstrated that his addition theorem and Schmidt's will yield different values, although on an experimental basis large numbers of trials would be necessary to make this difference apparent.

As Walker's theory applies to psi events such as telepathy, clairvoyance and precognition it is when the comparison between

target and call is being made that the $W\psi$ channel brings about the desired correlation between the call and target states. Thus all three processes are the same and involve state selection of the quantum processes in the participants' brains. In PK the situation is similar but the system affected is not the quantum mechanical process that makes the call but a physical process based on quantum randomness.

Setting aside for the present the details concerning data rates and other aspects of his quantum theory of consciousness it can be seen that the basic ideas behind Walker's model for psi and Schmidt's are very similar though the mathematical formulations yield different values. As these are admittedly first steps, perhaps first approximations to a reality which may later be specified more exactly, this situation is to be expected. It is very important to note that Walker has provided for his model the same constraint which Schmidt required in his, namely that feedback of the event is essential for any psi event to occur. As Walker states:

"Moreover, it is required that the individual acting as an observer be able to consciously recognize the state to be selected. No paranormal phenomenon is possible without both an adequate paranormal and a subsequent normal information link to the event."

(Walker, 1974; p.565)

This aspect of both theories has caused Schouten (1977) to call them 'observational hypotheses', and it is one of the most important predictions stemming from them. If it is in the conscious act of apprehending a meaningful correlation between call and target or

prediction and event that the psi event takes place then it requires a major shift in the traditional view of the parapsychological experimenter. What must be the interpretation of the many cases of reportedly successful psi experiments in which the subjects never saw their results or in which the results were sufficiently complex as to be meaningless to the subjects? Schmidt's and Walker's theories would exclude the subject as a possible source of the effect in such cases. In a typical experiment this leaves only the experimenter who does receive large amounts of feedback and is responsible for the ultimate measurements on the system to determine whether there was above chance scoring over all or if one group differed from another.

AN EXPERIMENT TO EXAMINE PSI-BASED EXPERIMENTER
AND SUBJECT EXPECTANCY EFFECTS

The literature surveys by Kennedy and Taddonio, and White discussed in the previous chapter indicated that virtually nothing had been done in the way of directly testing for psi-based experimenter effects. What was known about the phenomena had been gleaned from experiments meant to examine other matters. On the basis of what was understood from the observational hypotheses it seemed feasible to make a first experimental foray into this area.

Schmidt's model predicts that the more frequently a psi source is triggered the greater will be its effect on the world. Support for this prediction came from Schmidt's (1976) series of

experiments in which repeated presentations (unknown to the subject) of recorded PK trials yielded much higher scoring than singly presented trials. Using a computerised experimental set-up it would be possible to make use of the converse of this prediction, namely that minimal triggering of the psi source should reduce its ability to affect the world, to attempt a separation of the collective effect of the subjects and the possible effect of the experimenter. This could be accomplished by having the subject effect consist of N separate triggerings, one for each subject, to which the experimenter would be blind, while the experimenter would be limited to a single triggering - the moment when the computer prints the results of a pre-planned statistical test. The experiment would also be able to test the necessity for observation of the scores on the part of the subjects.

Method

This study was essentially a straightforward manipulation of the subjects' expectancies in a disguised ESP test. This was accomplished by means of a plausible cover story in a manner similar to the way 'sub-experimenters' in Rosenthal-type experiments are given certain expectations, as indeed are subjects themselves in certain cases. The unique feature of this experiment was that the manipulation of expectancy took place after the experimental task had taken place (but before the results were observed by the subject), so the effect, if any, would of necessity be paranormal. In a sense the subject could be viewed as a mini-experimenter with his own little experiment in which he is kept blind to the relevant information until the end, but with the added twist that he truly has no expectations until after his experiment is over. Thus it

would be hard to explain how his expectations could be influencing the outcome by what are presently considered normal psychological means.

Specifically the subject was told that he was participating in an investigation to determine possible sources of artefacts in psychological experiments, this being an understandable obsession of fastidious parapsychologists. The subject was told that the experiment would consist of two runs of what is known as a 'random guessing test' but nothing was said which might have led him to suspect that there would be any difference between the two runs. When the two runs of the experiment were completed the computer randomly assigned the subject to one of four conditions and printed an appropriately worded message 'explaining' the experiment in such a way as to give a reasonable expectation that the score on either the first or second run should be higher. A facsimile of such a note is provided in Figure 5. Without viewing it himself the experimenter gave the computer note to the subject to take away and consult at his leisure. The experimenter never saw the scores or knew which condition any subject was in, the relevant information having been stored by the computer.

A second aspect of the experiment was the control of the feedback to the subject. In order to do this the computer conducted for itself a 'pseudo-trial' to match each real trial of the subject. (This was simply allowing the random number generator to 'make a guess' at a target and will be explained in more detail below.)

THANK YOU FOR PARTICIPATING IN OUR EXPERIMENT.

THE INVESTIGATION IN WHICH YOU JUST TOOK PART IS BEING CONDUCTED TO EXAMINE POSSIBLE SOURCES OF ARTEFACTS IN PSYCHOLOGICAL EXPERIMENTS. WE ARE HOPING TO MANIPULATE ONE POSSIBLE SOURCE OF ARTEFACTS INTO PRODUCING EITHER A POSITIVE OR A NEGATIVE BIAS IN WHAT IS BASICALLY A RANDOM GUESSING SITUATION.

IN THE ABSENCE OF ARTEFACTS RANDOM GUESSING SHOULD PRODUCE SCORES AVERAGING AROUND 8, WHICH IS WHAT WOULD BE EXPECTED BY CHANCE IN THE 32-TRIAL RUNS. HOWEVER, IF WE ARE SUCCESSFUL WITH OUR EXPERIMENTAL MANIPULATIONS YOU SHOULD FIND YOUR SCORES ARE HIGHER IN THE POSITIVE ARTEFACT CONDITION AND LOWER IN THE NEGATIVE ONE.

YOUR SCORE FOR EACH RUN IS LISTED BELOW AND EACH IS LABELED ACCORDING TO THE CONDITION. AGAIN, OUR THANKS FOR YOUR TIME AND TROUBLE.

(NEGATIVE)	(POSITIVE)
FIRST RUN	SECOND RUN
5	9

FIGURE 5: Specimen of the note produced by the computer and delivered to the subject at the conclusion of the experiment.

For half of the subjects the scores reported on the computer note represented the true scores obtained by their guessing efforts. The other half of the subjects received on their notes the results of the computer's guessing on the pseudo-trials. This manipulation was intended as a way of seeing if subjects directed their psi efforts at the designated task which they performed or merely at the reported scores which represented the feedback event.

For the experimenter the feedback was reduced to a minimal level in that he saw only the results of t-tests on the comparisons of interest. The experimenter did not see any raw scores whatsoever. It was arguable that the single instance of receiving the final statistical results might be sufficient to trigger the experimenter as a psi source and, if so, this could cause a non-negligible psi-intervention by him. It was felt that until otherwise demonstrated it should be assumed that, according to the Schmidt model, this effect would be considerably less than that of the subjects. For the record, prior to the experiment the experimenter noted his feelings with regard to the possible outcome. He had mixed feelings; positive results in the study would hold the promise of the development of an exciting new methodology for psi research but by the same token it would seriously undermine the interpretation of several years worth of work already completed.

This stage of the study was considered exploratory and it was, as much as anything, an exercise to test the feasibility of the design before embarking upon larger scale studies. It also was, by the experimenter's own admission, an attempt to satisfy his

curiosity regarding the seemingly unbelievable idea that merely the observation of the results of a past event would be sufficient to psi influence them.

One hypothesis was being tested explicitly and it was similar to that used in previous parapsychological studies (e.g. Parker, 1975b; Taddonio, 1976), namely that subjects' scores in an ESP test can be manipulated by overt suggestion of different expectancies. Unlike all previous investigations of this sort, in this case the expectancy manipulation came after the ESP test. A second hypothesis was examined informally in the first instance and that was whether the feedback was necessary for the psi effect or was it possible for subjects to score on a task for which they received no feedback.

FIRST EXPECTANCY EXPERIMENT

Subjects

Forty unpaid volunteers, mostly undergraduate and postgraduate members of the psychology department served as subjects in this experiment. All were naive with regard to the aims of the experiment.

Apparatus

A Linc-8 computer controlled the experiment as described below. The responses were made on four buttons mounted in a slight arc on a metal instrument box measuring approximately 15 x 10 x 2.5 cm. The arc was to suggest a correspondence with a

possible auditory cue for the purposes of maintaining the cover story. A white noise generator and a 1 kHz tone generator were also employed to support the cover story. Modulo-4 random targets were generated by a software routine which sampled a noise-driven binary random number generator (see appendix A) twice and returned a number 1, 2, 3, or 4 to the main program. Preliminary tests indicated satisfactory first order randomness and test runs totalling 100,000 trials were interspersed throughout the experiment.

Procedure

The experiment was explained to the subject as a study of artefacts and how they may intrude upon psychological and parapsychological experiments. The subject was told that he would be asked to do two runs of a 'random guessing test' in which a computer randomly chooses a target corresponding to one of the four buttons on his response box and indicates a guess should be made. It is then his job to guess correctly which button has been selected. He was also informed that he would be listening to 'white noise' during the procedure and he was asked to put on the earphones to see what it was like. The experimenter asked the subject's assistance in 'balancing' a stereophonically presented tone and then gave the subject what appeared to be an auditory threshold test. The subject was left to speculate that the 'artefacts' would be in the white noise but nothing was said about this nor was anything said to indicate that there might be a difference between the two runs. In fact, the white noise and tone routine served no other function than to support the cover story. Regarding the guessing test the instructions were "to press whichever button seems correct at the moment and not to spend too much time puzzling over your choice".

When the subject understood his task and was ready to begin the experimenter left the room and initiated the first run by signalling the computer from his office. When the first run was over the experimenter was signalled by the computer and he allowed about one minute to pass before initiating the second run. At the end of the second run the experimenter returned to the subject and explained that his results were being prepared by the computer and would be ready in a moment. It was emphasized that for reasons of experimental control the experimenter must not see the scores so they would be given to the subject, along with an explanation of the experiment, to be taken away with him. The subject was asked to refrain from discussing his results with others for some time to allow the experiment to be completed and that it would be preferable for him simply to discard the computer report after he had read it.

The experimenter then proceeded to the computer room and with eyes thoroughly shut he entered and removed the printed note, folded it securely and then returned to the subject. (The room was arranged so that this could be done easily and without risk of injury. The procedure was well practiced.) This sequence of operations was designed to keep the experimenter blind to the conditions for the subjects and to prevent him from receiving any feedback of raw scores. With regard to the possible paranormal effects, whether or not the experimenter saw the scores was of no consequence. This procedure was related to the feedback hypothesis and as such it was in the interests of the experimenter that it be carried out correctly.

The actual conduct of the experiment was under program control by the Linc-8 computer located in another wing of the department. On command from the experimenter it read in the stored data and checked to see that the total number of subjects had not yet been reached. On command it began the first run consisting of 32 repetitions of the following cycle:

"Pause for one second; light the 'guess' light and generate a modulo-4 target. Await the subject's response and upon receipt of the subject's response extinguish the 'guess' light and store the response. Compare the response with the target and record the hit if indicated. Conduct a pseudo-trial by generating a fresh target then generating a second random number and checking if they agreed. Return to the pause."

Naturally all the computer operations occur at a fast rate so for the subject things appear to happen instantaneously, apart from the pause.

At the completion of the first run there was a one-minute pause, indicated to the subject by the lighting of a 'pause' light. On command from the experimenter a second run, identical to the first, was carried out and when that was completed the 'pause' light was again lit. On command from the experimenter the program began the printout of the message for the subject (see Figure 5). At a specified point in the message routine the computer randomly assigned the subject to one of four possible conditions (with the proviso that at the end there would be an equal number in each group). For conditions 1 and 3 the message stated that the first and second runs should be high and low respectively. For conditions 2 and 4 it was just the opposite. Then for conditions 1 and 2 it

provided the true scores obtained in the guessing test. For conditions 3 and 4 it provided the pseudo-scores obtained in the concurrently run pseudo-trials. Figure 6 illustrates the arrangement of conditions. On completion of the printout the condition number, true scores, and pseudo-scores were recorded on magnetic tape.

	Expects high score on First run	Expects high score on Second run
Subject receives Genuine scores	Condition One	Condition Two
Subject receives pseudo-scores	Condition Three	Condition Four

FIGURE 6: Diagram of the conditions used in the expectancy experiments

Data Analysis

At the completion of the experiment a custom written program was called to analyse the stored data. This program read the stored data, analyzed it, and printed on the teletype only the test identification, t-value and degrees of freedom. The raw data and intermediate results remained unseen. Naturally the program had been thoroughly tested for accuracy with supplied data.

A consideration in the planning of the tests was to strike a balance between the necessary program development time and the amount of information which could be obtained from this first experiment. It was decided that several simple t-tests would be used and

that even though this was not the best procedure for proper statistical inference they would be sufficient to test the main hypothesis and guide the design of a confirmatory experiment if warranted. The following tests were planned:

1. A t-test on the high vs. low scores for all the scores actually reported to the subjects (as if the computer printouts were collated and analyzed in the manner of a traditional experiment).
2. A t-test on the difference between the high and low³ scores for the reported vs. the unreported scores.
3. A set of three t-tests on the scores of the subjects' actual guessing tests:
 - 3a. t-test on the high vs. low scores for the subjects who actually saw their results (genuine feedback group).
 - 3b. Same thing for subjects who did not see their scores (pseudo-feedback group).
 - 3c. A t-test comparing high-low differences for the group which saw their results with the group which did not see them.

All t-tests save 3c were for correlated data and a one-tailed evaluation was planned for test 1 and test 3a.

Test 1 tests the hypothesis that subjects can influence the reported results in the desired direction irrespective of whether the scores were from the test which they took or one of which they

³ The high-low difference, used in most of the tests, consisted of the score labelled 'positive' minus the score labelled 'negative' and was used as a measure of the effectiveness of the expectancy manipulation. Under the null hypothesis the mean should be 0 but a positive value would indicate that the expectancy manipulation had an effect.

were unaware. Test 2 examines whether it is important to see the scores in order to have a psi effect. The three tests of number 3 examine the results of the actual guessing test to see (a) if there was a significant expectancy effect for the actual guessing test in this subgroup; (b) if such an effect continued for subjects who never got to see the results of that test; (c) if there was a difference between the two groups.

Results

Chi-squared test of the results of the 100,000 test trials indicated no significant departure from chance expectation, $\chi^2 = 5.67$, $df = 3$, n.s.

The result of test 1, t-test on the high vs. low score for all subjects' reported scores was significant, t = 1.918, (df = 39, p = .03, one tailed). Test 2, a t-test on the difference between the high and low scores for the reported vs. the unreported scores was also significant, t = 2.705, (df = 39, p = .01, two tailed). However, the test 3 set yielded no significant differences; test 3a, t-test for the high vs. low condition for those subjects who saw the results of the guessing test, t = 0.840, (df = 19); test 3b, the same test for those subjects who did not see the results, t = 0.062, (df = 19); test 3c, independent groups t-test for the high-low differences between the groups, t = 0.579, (df = 38).

In considering the results of the first two tests it appeared that whatever gave the significant differences it was not

primarily due to the actual guessing test. Initially, perhaps naively, it was not thought necessary to use the group 3 set of tests on the pseudo-scores, because, after all, nothing should happen in data for a test about which the subjects knew nothing. However the substantially significant differences in the apparent effectiveness of the expectancy between the reported and the non-reported scores (test 2) could hardly have been due to the results of the genuine guessing test since the group 3 tests demonstrated virtually no effect at all in these data. This suggested a negative effect in the pseudo data. It was thus decided to call the group 3 set of tests to analyze the pseudo-trial data before all the data were destroyed. There it was found that on the pseudo-trials, the group which had these scores reported (3a) obtained $t = 1.777$, ($df = 19$; $p < .05$, one tailed). For the subjects who did not see their pseudo-scores (3b) a very significant effect in the opposite direction was obtained, $t = -3.519$, ($df = 19$; $p < .01$, two tailed). The difference between the two groups (3c) was significant, $t = 3.491$, ($df = 38$; $p < .005$, two tailed).

Discussion

For clarity the various t -tests and their relationships are set out in Table 14. The main hypothesis, that subjects can influence in a specified direction the scores which they receive even when the direction is not established until after the scores are recorded, was confirmed. The hypothesis that it is necessary to see the scores to have an effect in the desired direction was supported by the results of test 2 but the post hoc analysis

TABLE 14: T-test results showing relationships between one another.

When the	was	Reported	Unreported
GENUINE TEST	↓	High vs. Low $\underline{t} = 0.840$ $\underline{df} = 19, \text{n.s.}$ (Test 3a)	High vs. Low $\underline{t} = -0.062$ $\underline{df} = 19, \text{n.s.}$ (Test 3b)
		(H-L) vs. (H-L) $\underline{t} = 0.579$ $\underline{df} = 38, \text{n.s.}$ (Test 3c)	
PSEUDO-TEST		High vs. Low $\underline{t} = 1.777$ $\underline{df} = 19, \underline{p} < .05$ (Test 3a <u>post hoc</u>)	High vs. Low $\underline{t} = -3.519$ $\underline{df} = 19, \underline{p} < .01$ (Test 3b <u>post hoc</u>)
		(H-L) vs. (H-L) $\underline{t} = 3.491$ $\underline{df} = 38, \underline{p} < .005$ (Test 3c <u>post hoc</u>)	
		High vs. Low $\underline{t} = 1.918$ $\underline{df} = 39, \underline{p} = .03$ (Test 1)	
		(H-L) vs. (H-L) $\underline{t} = 2.705$ $\underline{df} = 39, \underline{p} = .01$ (Test 2)	

revealed that instead of there simply being no effect in the unreported condition there was a strong effect in the direction opposite to that which was specified for the reported scores.

The apparent opposite direction scoring in the pseudo-score condition was not at all anticipated. It did not seem likely, though, that the relatively large number of significant differences could be ascribable to chance. The entire experimental system had been 'exercised' by the experimenter no less than four times in the course of testing it and never once did a score approaching significance turn up.

The proliferation of t-tests that resulted from the actual experiment made for a less than tidy package but they did provide some interesting results. Besides the confirmation of the main hypothesis two facts emerged from the data: (1) reported scores were affected in the desired direction and unreported scores were affected in the opposite direction, (2) there appears to have been a greater effect in the concurrently run pseudo-trials than on the trials of the actual guessing test.

One possible interpretation of the subsidiary findings was that the Schmidt model is not entirely accurate in its interpretation of the amount-of-triggering aspect of the experimental situation, and that the experimenter could have had a substantial influence on the data even though he saw only the final results. Theoretically at least, the subjects could not have affected the scores which they never had reported to them and the models would

suggest experimenter involvement. The nature of the experimenter's effect could have been to try to maximize the test 2 result (the observed vs. unobserved comparison) and to do this in the face of the meagre positive scores in the observed data he 'produced' a negative effect in the unseen data, particularly in the pseudo-scores which were not originally planned for analysis.

To be fair, the section of Schmidt's model concerning multiple versus single feedback is not considered by him as firmly established. In fact an attempt to replicate his repeated presentations experiment (Schmidt, 1976), which demonstrated higher scoring on targets presented four times, was conducted in the Edinburgh laboratory just after the expectancy experiment and no psi effect at all was found (Millar and Broughton, 1977). Also, it should be noted that this expectancy experiment was not a case of repeated presentations to a single subject but a large number of subjects in comparison to a single experimenter who did not also see the subject's raw data.

Walker's model has no postulates regarding the number of presentations or the amount of feedback as such but regards the psi effect as a function of the time spent on the experiment, Δt . Walker's model remains rather unclear, perhaps deliberately so, regarding the problem of designating the beginning and end of the relevant time period. Does it begin with setting up the equipment, testing the programs, etc. or is it limited to the time spent 'running subjects'? Does it include the time spent analyzing data? No clear answer exists in the theory and to that extent it could not shed any light on the results obtained in the expectancy experiment.

An interesting theoretical development occurred not long after the conclusion of the experiment. A paper by Walker (1976) in a new journal arrived at the Edinburgh unit in June 1976. It was mainly a restatement of his psi theory with some suggestions for experiments to test aspects of his theory. In considering one of the suggested experiments he treats the problem of the experimenter seeing the accumulating results as against seeing only the final result. He calculates the amount of information (in bits) necessary to achieve a given final outcome probability for a run on a trial-by-trial basis and then applies that amount of information to just a final result probability and finds that a given amount of psi information may be more efficient if directed to the final achieved probability rather than the individual probability on each trial. He then makes the statement which rather startled this investigator who was still pondering the implications of the recently concluded experiment:

"The remarkable result is that, if conducted so as to maintain subject interest for high performance . . . a higher score can be achieved by holding the results until the end of the run than by looking at each trial immediately."

(Walker, 1976, p.50)

Walker goes on to say that whether, in a psi experiment, the subject has any effect at all depends critically on the experimental procedure. If all the subjects' subsequent interactions with the experiment are independent of the results in the experiment then the subject has no effect (because he has had no feedback). That was, of course, the situation with regard to all but the subject's own scores in the preceding experiment.

If, as the events and reports detailed in the previous chapter suggested, the investigator himself could be a potent source of psi influence then the expectancy experiment, quite unwittingly, had been loaded in favour of the one person who had a sustained high interest in success - the experimenter.⁴

INTERIM

Before the Walker (1976) paper had arrived a report on the expectancy experiment had been submitted and accepted for presentation at the 1976 Parapsychological Association Convention to be held in August. The author had included a provision that before the paper was presented a replication study would be carried out since the results seemed too important to stand on a single experiment.

CONFIRMATION STUDY ON PSI BASED EXPERIMENTER AND SUBJECT EXPECTANCY EFFECTS

Method

The confirmatory study was run in precisely the same manner as the previous study. Forty fresh subjects, this time

⁴ As it happens, in a very recent paper Walker (1977b) admits that there was an error in the calculations concerning the difference between end-of-run and trial-by-trial feedback. He now maintains that both should have an equal effect rather than the slightly enhanced effect claimed for the former. This would not appreciably alter the interpretations of the expectancy experiment results. Indeed, the major impact of the earlier claims may have been the psychological impact on the experimenter.

paid volunteers recruited through a notice in the student union were used. Controlling computer programs, equipment, and the method of dealing with the subjects were identical to that used in the first experiment.

The data analysis differed in the confirmation study. To improve statistical inference it was planned to analyze the second study by analysis of variance and the submission to the Parapsychological Association indicated that this would be done. Upon examination it became apparent that the experiment did not in fact consist of a true factorial design and could not be analyzed in the planned multivariate analysis of variance. Instead two single factor analyses were done and these were directly equivalent to certain t-tests of the previous experiment. The analyses were for any difference in the effectiveness of the expectancy manipulation (high-low difference) between the reported and the unreported scores (equivalent to test 2) and between the genuine test scores and the pseudo-scores. Also the original analysis was used to provide only the test 1 result (effectiveness of the expectancy manipulation on the reported scores). These changes in the method of analysis of course had no effect on the conduct of the experiment as far as the subjects were concerned.

It can be said that for all practical purposes the confirmation study was identical to the first study, with one important exception; the experimenter did not have the same feeling toward the experiment. Specifically, he felt far more suspicious that he may be responsible for the results.

Results

The results of the confirmatory study were nil. In marked contrast with the earlier experiment none of the prescribed tests even approached significance. As examples, the general test of the psi-based expectancy effect (test 1), which in the first experiment yielded $t = 1.918$ ($df = 39$, $p = .03$ one tailed), gave a result in the confirmation study of only $t = 0.875$ ($df = 39$, n.s.) and the test for the difference between the seen and the unseen scores (test 2), which earlier yielded $t = 2.705$ ($df = 39$, $p = .01$ two tailed), provided in the confirmation study an $F = 0.032$ ($df 1,39$; n.s.).⁵ Table 15 presents the results of the confirmatory study in a format for easy comparison with the results of the first experiment in Table 14.

Discussion

It seems difficult to ascribe the total failure of the second experiment to differences in the methodology since this was the same in both studies. Every effort was made to interact with the subjects in the same manner as in the first experiment, and this was not difficult considering the relatively short time which the experimenter spent with the subject. While there was a small difference between subject pools in that the second were paid while the first were not it seems very unlikely that this could have been a major factor in the failure of the second

⁵ Recall $F = t^2$ for purposes of comparison.

TABLE 15: Results of the pre-planned analyses of the confirmatory study set out for comparison with Table 14 (p.195).

When the	was	Reported	Unreported
GENUINE TEST		(H-L) vs. (H-L) $F = 0.581$ $df = 1,38$ n.s. (Test 3c equivalent)	
	(H-L gen) vs. (H-L pseud)	$F = 0.705$ $df = 1,39$ n.s. (no equivalent)	
PSEUDO-TEST		(H-L) vs. (H-L) $F = 3.017$ $df = 1,38$ n.s. (Test 3c <u>post hoc</u> equiv.)	

High vs. Low
 $t = 0.875$
 $df = 39$, n.s.
 (Test 1)

(H-L) vs. (H-L)
 $F = 0.032$
 $df = 1,39$ n.s.
 (Test 2 equivalent)

experiment since both groups of subjects were recruited in the university environment and to all appearances both groups seemed to possess equal interest in the work at hand.

While it could not be proven in any rigorous fashion the events seemed to force only one conclusion and that was stated by this investigator in the paper which he presented to the Parapsychological Association at the August 1976 Convention in Utrecht:

"Rather than attribute this failure to replicate to gross differences in the psi ability of the two groups of subjects or in the experimenter's ability to motivate them to use their retroactive PK, possibilities which to this investigator seem unlikely, it would be more parsimonious to attribute the outcome to a psi-failure on the part of a single person, namely the experimenter himself, for whom the conditions between experiments may have changed considerably. It should be noted that the pilot study was undertaken by the experimenter alone as a matter of curiosity regarding the Schmidt model and its possible use in studying psi-based experimenter effect. By the time the confirmatory study had started the full implications of the problem were becoming clear and the experiment was now the vanguard of an entire programme of research at the Edinburgh Unit. Taking into account the added pressure of having already submitted a paper to the P. A. Convention it can be said that for at least one possible psi source conditions had changed between experiments."

(Broughton, 1976b)

The predictions of the observational hypotheses favour the interpretation that the primary psi-source was the experimenter even though this was directly contrary to the intentions of the original experiment. This appears especially true in the case of the first experiment where there were highly significant differences in data entirely unknown to the subjects. In such a situation

only the experimenter, and anyone who may see the results subsequently, could have an effect according to the theories.

There is no doubt in the mind of at least one of the theorists as to who was responsible for the extra-chance scoring observed in the experiment. At the South Eastern Regional Parapsychological Association Convention in January 1977 Walker (1977a) presented a paper in which he produced equations derived from his formulae to demonstrate that the effects noted in the foregoing expectancy experiments were far more likely to be due to the experimenter than the subjects, including, of course, the failure to replicate. Unfortunately the equations fit rather too well due to the almost gratuitous assumptions regarding the relative amounts of time spent on the two experiments, a weak point in his model discussed earlier. Likewise he makes certain assumptions regarding the differing attitudes of the experimenter for each of the experiments which are very unlike the investigator's own recollections of his feelings. Nonetheless, for the various reasons outlined above this investigator is inclined to agree with his general conclusions.

If it was the case that the experimenter was responsible for the extra-chance scores of the first experiment why was he unable to continue it for the second? Indeed it would seem more to his benefit if he could replicate his own results. Unfortunately this remained a mystery but it did begin to appear that the investigator would have to take stock of his feelings toward the running of the experiment and his feelings when he is about to

learn the results of his efforts. There are, of course, traditional superstitions in science about waiting until one is in a good mood before examining the data, or not looking at the data as it accumulates but waiting until it is all in. This investigator's experiences made him less likely to scoff at such ideas.

The possible importance of the experimenter's attitude at the moment the results are observed appeared to be related to the failure of a 'covert' test of this investigator's psi conducted by his colleague, Brian Millar (Millar, 1977). Millar's experiment involved certain randomness checks which were being run on the random number generator used in many of the experiments reported herein. Though the data acquisition programs had been prepared by R.B. the analysis programs had been written by B.M. Consequently, R.B. was unaware of the fact that B.M. had arranged that each day one of the several figures reported as part of the test would be inflated so as to make the machine appear biased. The rationale behind this was that since R.B. had made heavy use of the RNG in several experiments a latterly discovered bias would be a severe problem and to avoid this R.B. would use his PK to counteract the unknown inflation of the scores by causing the real RNG output to deviate in the opposite direction. It was a clever experiment, especially since R.B., who realized such a test should be done, nonetheless only weeks earlier had boasted that no one could get away with it because he would be looking out for it. The experiment did not produce any evidence that R.B. had used PK to counteract the manufactured biases.

This experiment may have foundered on the question of what would have best served the experimenter's needs. As it happened, while this experiment was going on R.B. was occupied with other matters and although he conducted his share of the tests as required by the test protocol he was oblivious to the alarming number of 'biased' figures which were accumulating. Secondly, B.M. was unable to contain his glee at the success of his coup and almost as soon as R.B. had completed the last of his tests B.M. handed him a preliminary write-up of the experiment. As this investigator reported to B.M. afterwards:

"As I began to read the paper I suddenly realized that I had been 'done' and my first reaction was to shuffle through the pages looking for the results. When I found them and noted that the experiment had failed to show any psi on my part I felt a great relief come over me. After all, it would be very embarrassing had you publicly demonstrated that I could affect data like that."

Obviously correcting the artificial bias, which would have had to come to light anyway, was not as important as avoiding a situation which might jeopardize the interpretation of previous work.

In conclusion, the expectancy experiment is a classic example of the experiment which raises more questions than it answers. There can be little doubt that in the first experiment there were extra-chance effects. That experiment demonstrated either that subjects could affect part of the experimental outcome by the observing of the results or that the experimenter, blind to all but the final statistics, could have a substantial psi effect on the data, or both. That the subjects had no effect in

the second experiment points to the experimenter as the 'culprit'.
Although these experiments failed to identify the source or
sources of psi phenomena conclusively it is safe to say that the
roles of the subject and the agent could no longer be looked at
in the same way as before.

CHAPTER VII

HEMISPHERE DIFFERENCES

IN ESP REVISITED

CHAPTER VII

HEMISPHERE DIFFERENCES IN ESP REVISITED

The results of the expectancy experiment discussed in the previous chapter raised many questions, not the least of them being, "What might the apparent psi abilities of this experimenter have had to do with the elicitation of the results obtained in the hemisphere specialization series of experiments?" Clearly those experiments had demonstrated statistically significant effects, but whose effects were they?

In retrospect there were many features of both the shapes experiments and the reaction time experiments which would have pointed to the experimenter as the main psi source on the basis of the observational hypotheses. In the shapes series, while the subject was blindfolded making guesses, the experimenter was recording the calls and the targets so he was acutely aware of the hits or the lack of them. Moreover, the experimenter knew which conditions he would like to see highest when he totalled the columns. Subjects knew nothing of this until after the experiment when the experimenter told them the results and explained what the experiment was about.

In the reaction time experiments the subjects never even saw their scores nor knew 'how they did'. When the session was completed the experimenter explained the nature of the experiment

but told them that there were really no scores to see but a rather meaningless list of millisecond response times which had to be analyzed at a later date. In this case the experimenter was the only person to see the scores and that happened when another program was called to print the data from all subjects.

The reaction time experiments are typical of a number of experiments in parapsychology where either the raw data are not interpretable to the subject or the hypotheses are too involved so that the subject is not told the details of what effects are expected. According to the observational hypotheses, if there was no intermediate feedback to the subjects, as in the reaction time experiment and others like them, the subject cannot possibly have a psi effect. A psi source must have knowledge that it has had an effect.

While considering the implications which the new models had for the hemisphere work several incidents which earlier were thought only curious now seemed important. One example concerns the observation of the reaction time print-outs for the second, successful experiment. At the time the experimenter did not really expect much in the way of significant effects in the first reaction time experiment without the reading, and that is what he got. For the second experiment with the reading he did expect a difference between the advance and control conditions. Recall that there was a significant interaction effect and one of the conditions, the left hemisphere, yielded an independently significant difference between the advance and control conditions. Now

the investigator really would have preferred the independently significant difference to have occurred in the right hemisphere, but, no matter, it could be accommodated. It now appears that there may have been a reason for this that had very little to do with brain specialization.

As it happened, the important scores were printed at the bottom of each long sheet as it came off the teletype in a roll. The scores which formed the basis of the later analysis, the means, were arranged in a 2 x 2 matrix as illustrated in Figure 7.

GRAND MEANS	CON	NO	AAS	NO
LEFT HAND	250	39	252	41
RIGHT HAND	257	39	266	41

FIGURE 7: Lower part of the data sheet for a subject in the reaction time experiment showing the matrix of mean scores which were used in later analyses.

Notice that the bottom score was 'Right Hand'. As the print-outs tediously rattled off the teletype the investigator periodically checked that all was going well and in doing so he quickly glanced over the data, pulling the roll of paper back up from the floor through his fingers. Thus the very bottom score was the most prominent and there were a noticeably large number of differences in the same direction on that line. "Looks good for the right", thought the investigator, but alas it had slipped his mind that

what he was looking at was the right hand, not the right hemisphere. Of course when the data were analyzed there were differences in both the 'Left Hand' and the 'Right Hand' conditions but only the 'Right Hand', meaning the left hemisphere, was significant. It ever remains a point of speculation that had the score matrix been arranged so 'Left Hand' was on the bottom the investigator might have had his data exactly the way he wanted it.

Another incident concerned the data of the third and most successful of the shapes experiments. The investigator was scheduled to give a lecture to the Society for Psychical Research reporting on his progress. This was to happen in the midst of running subjects for the Shapes III experiment so the first half of the subjects, up to the lecture date, were run in the customary manner but the data were quickly filed without much thought on the part of the investigator. Since Shapes I was rather inconclusive and Shapes II appeared at the time to be a loss, the investigator at the last moment decided to look at the first half of the data in the hope of being able to provide an encouraging forecast. Sure enough, the first ten of the planned 20 subjects were already highly significant in the expected direction. At that rate it appeared the experiment would be wildly successful. The S. P. R. received an encouraging report, but the scores did not continue that way. The experimenter was now very conscious of the scoring rate and every time he sat with a subject and ticked off the hits in the 'high score' condition he found himself very anxious about whether or not the subject would in fact do well and would the

experiment succeed after all. The scores in the second half did nothing to help the experiment, and in the end the significance was slightly lower than at the halfway point. Did the increased anxiety of the experimenter cause him to 'choke up' his psi ability? One can only speculate at this stage.

The predictions of the observational hypotheses, the results of the expectancy experiment, and the speculations of the investigator concerning those 'curious incidents' made it imperative to re-confirm the hemisphere differences findings before any further work could proceed on that front. If the effects are true results of the subjects' behaviour under the experimental manipulations then the experiment should easily replicate, especially at the hands of the original investigator. If instead the effects were psi-based and due to the experimenter then they might or might not replicate. The experience of the investigator with the second of the expectancy experiments suggested that a psi-based experimenter effect would fail to replicate. If the experiments did replicate it would then be up to independent investigators to replicate the work as well, the situation which existed prior to this investigator's involvement in the observational hypotheses and experimenter effect.

It was thus decided that two replications would be necessary, one of the Shapes III experiment and one of the Reaction time II experiment. Both experiments would be conducted as closely to the originals as possible. Furnishings and equipment were arranged as they were earlier, with the exception that the rooms had

been painted and were much improved thereby, and all the original scripts of subject instructions were employed. The replications would naturally include sex of subject as a factor and half of the subjects would be of each sex but this could only affect the analysis and in no way would it alter the general characteristics of the experiment.

REPLICATION OF THE REACTION TIME II EXPERIMENT

As the original equipment for the reaction time experiment was already in place in the experimental rooms and the Shapes III experiment would require a major rearrangement it was decided to do the former first. The details of the experiment are given in Chapter IV and will not be repeated here. Briefly the experiment was looking for a hemisphere differential effect in a subject's reaction times to a tone with the differential being produced by the psi influence of an agent. The subject was required to press a button with either the right or left hand in turns in response to a bilaterally presented tone while performing a reading task. Unknown to the subject, on randomly selected trials (about half the total) a partner would receive a tone, to which he responded, one-quarter of a second before the subject; on the remaining trials the partner's tone came after the subject had responded. There were four conditions: Left hand with advance tone, Left hand with control tone, Right hand with advance tone, and Right hand with control tone.

The results of Reaction Time II demonstrated a very significant hand by condition interaction effect ($p = .006$) indicating that the effect of the advance and control conditions were exactly reversed for the right and left hands. The advance condition resulted in shorter mean response times on the left hand and longer ones on the right. The difference between the advance and control conditions on the right hand were significant ($p = .022$).

Subsequently sex differences were noted in the data and a reanalysis indicated that the interaction effect was almost wholly confined to the data of the male subjects. This strongly corroborated other suggestions that the left hemisphere task (reading) was less effective for females, even as it affected ESP.

Method

The method was precisely the same as outlined in Chapter IV. No changes were introduced except to balance subjects by sex. There were 12 male and 12 female volunteer subjects recruited via notices placed in the university environment. Every effort was made to have the experimenter interact with the subjects in precisely the same manner as in the previous experiments. Two veterans of the previous experiment were brought in to assess the similarity of the experimenter's manner and they reported no differences. The experiment was conducted in November and December 1976.

Results

In short, the results of the replication did not resemble the previous results. No indication of any hand by condition interaction was found, nor of a sex by hand by condition interaction, both of which were significant in the original. In the replication an effect of hands was found, $F = 6.19$ ($df\ 1,22$; $p < .02$) but this was a normal effect and was expected although, curiously enough, was never found in the original experiment. The means by condition are set out in Table 16a and the analysis of variance results in Table 16b. These tables should be compared with Tables 9a and 9b on page 129.

TABLE 16a: Condition means (in msec.) according to sex of subject for the replication of Experiment II of the reaction time series.

	Left Hand Control	Left Hand Advance	Right Hand Control	Right Hand Advance
Males	381.4	392.1	399.3	398.6
Females	400.2	397.7	418.2	415.6

TABLE 16b: Summary table for three-way analysis of variance with repeated measures, subjects nested in sex for the replication of Experiment II of the reaction time series.

Source	SS	df	MS	Error	F
Sex	5445.1	1	5445.1	s(S)	0.458
Hands ¹	5445.1	1	5445.1	sH(S)	6.190 (p < .02)
Condition	36.3	1	36.3	sC(S)	0.368
subjects	261373.4	22	11880.6		
S x H	195.5	1	195.5	sH(S)	0.222
S x C	333.7	1	333.7	sC(S)	3.385
H x C	201.3	1	201.3	sHC(S)	1.467
s x H (S)	19353.1	22	879.7		
s x C (S)	2169.0	22	98.6		
S x H x C	189.8	1	189.8	sHC(S)	1.384
s x H x C (S)	3017.6	22	137.2		

Discussion

The results largely speak for themselves. Nothing like the effects of the previous experiment were found in the replication. The only condition which provided a significant effect was the hands condition but as mentioned above this was a completely natural effect which had in fact been expected for all three experiments of this type but appeared only in this one. The main effect which was to be replicated was the hand by condition interaction but this yielded a nonsignificant $F = 1.467$. The three-way interaction of sex by hand by condition which was also significant in the post hoc analysis of the second experiment was also nonsignificant at $F = 1.384$. An effect approaching significance ($p = .08$) was

¹ The similar sums of squares for the first two conditions is correct. It may be noticed from Table 16a that the means of male and female groups are exactly the same as those of the left and right hand respectively. No explanation of this coincidence can be offered at this time.

noted in the sex by condition interaction with $\underline{F} = 3.385$ but there was no hint of this effect in the previous results where the same condition yielded $\underline{F} = 0.432$.

Unfortunately it is not possible to advance any suggestions regarding differences in the methodology of the two experiments which could explain the total failure to replicate. The results of the replication could only be interpreted as casting severe doubt on the origins the effects in the previous experiment as being brain hemisphere asymmetries.

REPLICATION OF THE SHAPES III EXPERIMENT

Because of the fact that the reaction time experiment was so structured that subjects never saw their results there was a chance that it might have been particularly susceptible to an experimenter psi influence. The situation was different with the shapes series because those subjects did get to see their scores at the completion of the experiment, albeit after the experimenter had seen them, so there was still a chance that these scores were less psi-influenced by the experimenter and more likely to replicate. It was a possibility that the reaction time results were psi influenced by the experimenter to conform to an established trend.

The details of the Shapes III experiment are provided in Chapter III. That experiment was intended as a confirmation of the effects noted in the first half of Shapes I. The task

for the subjects was an ESP guessing test but the guesses were made by fingertips from among five three-dimensional wooden shapes roughly analogous to the five ESP card symbols. Subjects made their guesses with one or the other hand in runs of 25. Also, they would be sitting relaxed with eyes covered or reading aloud, the latter task being used to occupy only the left hemisphere. Subjects came with partners who saw the target shape and attempted to influence their partner's choice on each trial.

The results of Shapes III represented a striking confirmation of the Shapes I findings. When subjects were using their left hands and reading at the same time they scored significantly above chance suggesting that the right hemisphere was playing a lead in ESP with the left hemisphere being kept busy with the reading. Analysis of variance revealed a significant effect of the reading-relaxed condition with the reading condition yielding higher scores. A very small interaction was obtained indicating a high degree of independence for the effects.

Subsequently sex differences were noted in the data and a reanalysis revealed that the males were almost exclusively responsible for the significant effects. A post hoc analysis of variance including sex as a variable indicated that in addition to a significant effect of condition there was a significant sex by condition interaction.

Method

Precisely the same method was used in the replication as in the Shapes III experiment, even to the extent of having the

same reading material. Details of the method may be found in Chapter III. As in the previous replication every effort was made to treat the new subjects exactly as the previous ones were treated. Also as in the previous replication the subjects were balanced by sex, 10 males and 10 females. The experiment was conducted in April 1977.

Results

For the confirmation experiment there were 80 runs, 20 in each condition. First order randomness for targets generated during the experiment was satisfactory, $\chi^2 = 4.77$, (4 df, n.s.).

The overall results are presented in Table 17 which should be compared with Table 5a on page 96. There was no evidence to suggest that any of the effects of the Shapes III experiment were replicated. The left hand while reading condition, which previously achieved a significant above chance score with $t = 2.939$ ($p = .008$, 19 df), yielded only $t = 0.322$ (n.s., 19 df) in the replication.

TABLE 17: Totals and summary statistics for each condition of the replication for Experiment III of the Shapes series.

	Left Hand Reading	Left Hand Relaxed	Right Hand Reading	Right Hand Relaxed
MCE	100	100	100	100
Score	103	97	94	96
Dev.	3	-3	-6	-4
Mean	5.15	4.85	4.7	4.8
<u>t</u> -test	0.322	-0.307	-0.754	-0.363

The three-way analysis of variance with repeated measures, subjects nested in sex failed to demonstrate any significant differences in the conditions which were of interest. The formerly significant reading/relaxed condition yielded an exceptionally small value, $F = 0.043$ (df 1,18) . In the replication a totally new and difficult to interpret three-way interaction of sex by hand by condition (reading/relaxed) appeared, $F = 6.914$ (df 1,18, $p < .02$) . It appears that this effect was mainly due to the males having higher scores for the reading condition and the females having higher scores for the relaxed condition with males having generally higher scores than females. The means and analysis of variance summary are presented in Tables 18a and 18b and these results should be compared with Tables 6a and 6b on pp.104-5.

TABLE 18a: Condition means according to sex of subject for the replication of Experiment III of the Shapes series.

	Left Hand Reading	Left Hand Relaxed	Right Hand Reading	Right Hand Relaxed
Males	5.1	5.3	5.8	4.2
Females	5.2	4.4	3.6	5.4

TABLE 18b: Summary table for three-way analysis of variance with repeated measures, for replication of Exp.III

Source	SS	df	MS	Error	F
<u>Sex</u>	4.05	1	4.05	s(S)	0.754
<u>Hands</u>	1.25	1	1.25	sH(S)	0.327
<u>Condition</u>	0.20	1	0.02	sC(S)	0.043
<u>subjects</u>	96.70	18	5.37		
S x H	0.05	1	0.05	sH(S)	0.013
S x C	7.20	1	7.20	sC(S)	1.532
H x C	0.80	1	0.80	sHC(S)	0.229
s x H (S)	68.70	18	3.82		
s x C (S)	84.60	18	4.70		
S x H x C	24.20	1	24.20	sHC(S)	6.914 (p < .02)
s x H x C (S)	63.00	18	3.50		

Discussion

Again the results indicated a total failure to replicate any of the effects which were found in the earlier experiment. As can be seen from the means presented in Table 17 and those in Table 18a not even a trend reminiscent of the earlier results could be discerned. Instead an entirely new three way interaction effect appeared. It is possible that this effect could be pursued; for example the right hand reading score of 3.6 for the females constitutes a significant negative deviation, $t = -3.10$ ($p = .01$, 9 df), but in the total absence of any suggestions of this finding in earlier experiment any interpretation of the findings would be ad hoc in the extreme.

If the effects noted in the original studies had been due to the actions of the subjects working under the experimental conditions it could be expected that there would be some hint of those effects in the data of the replications. Clearly there is

none despite extreme attempts to duplicate precisely the experiments. In this, as well as the previous replication, it is unlikely that one could follow more closely Honorton's advice for achieving successful replications in parapsychology, i.e. that replications may need to be carried out in the same laboratory with the same arrangements as the earlier work (Honorton, 1976). Yet these were unable to replicate the earlier findings.

Obviously it could be argued that the subjects were able to 'see through' the experimenter's 'acting' and detect his real uncertainty and dampened enthusiasm for the hypothesis and thus not themselves feel motivated to produce psi in the required conditions. But for almost half a century parapsychology has tolerated ad hoc arguments such as this and it remains no closer to a solution of the repeatability problem than it was at the start. The particular care taken to insure the closeness of the replication in these instances severely strains the ability of such ad hoc explanations to provide a really credible reason for the failure. It is necessary to face the fact that the coherent and seemingly robust effects of the earlier work were probably not due to the subjects at all.

CHAPTER VIII

TO ASK BETTER QUESTIONS

CHAPTER VIII

T O A S K B E T T E R Q U E S T I O N S

It has often been said of science that its purpose is not to provide answers but to enable one to ask better questions of nature. It may be admitted without shame that the research reported herein has not provided any firm answers to the vexing problems facing parapsychology but it may also be hoped that this research will enable parapsychologists better to formulate the questions they ask of the phenomena.

In many respects the research programme on brain hemisphere differences and ESP undertaken by this investigator is a miniature of parapsychological research in general. A promising hypothesis was developed by an enthusiastic investigator and the results of his early experiments demonstrated a trend which supported the hypothesis. Then, quite suddenly, the results stopped coming, even in precise replications of the formerly successful experiments. Time and again this has happened in parapsychology and there always remained the question of what to do with the original findings.

Hitherto the problem could be disposed of in one of two ways. The sceptic could say that the earlier results were mere statistical 'flukes' or methodological artefacts which did not

appear in the more carefully controlled later work. The believer could argue that there is really no problem. No doubt the failures were due to a diminished enthusiasm on the part of the investigator and the original findings should be incorporated into the growing body of knowledge about psi effects since, as Rhine has stated, "*There is no way to explain away a successful experiment by an ever-so-careful chance replication*" (Rhine, 1975a, p.141).

Neither position is satisfactory in this case, or in most others concerning parapsychology. It is not scientific for the sceptic to assume that the original results were artefactual; that allegation must be proven. In the case of the hemisphere differences and ESP findings which were sustained in a number of separate experiments and which dove-tailed with related findings in other areas of hemisphere research, the possibility that this all could be a methodological artefact seems remote. (A methodological artefact should have replicated in any case since this would have been identical in the replication.) In the absence of specific proof of an artefactual basis the evidence obtained in the earlier experiments demonstrating statistically significant departures from chance must be allowed to stand.

At the same time, however, the believers position is scarcely more helpful since it is exactly this attitude which has bequeathed parapsychology its legacy of impossible-to-verify 'facts' about the operation of psi. Rhine's statement about chance

replications is a dangerous half-truth which arises from a particularly 'separatist' attitude toward psi phenomena. This position can best be illustrated by reference to remarks by Rao (1976), the recently appointed director of Rhine's Institute for Parapsychology. He advances a view of parapsychological research in which psi comes and goes like a fairy, occasionally gracing an experiment with her presence but likely as not, capriciously staying away. The researcher carries on manipulating the variables in his experiments in the hope that psi will be there. If she is he will be lucky and may have the opportunity to see how she relates to the variables in his experiment; if she is not then it will be a waste of time as the required presence will be lacking.

In fact very few active parapsychological researchers subscribe to this view. The interpretation of the results of an experiment carried out on a random sample of subjects meant to be representative of the general population is, "Subjects, therefore people in general (or some subgroup thereof), working under such and such conditions demonstrate more psi than subjects not under those conditions." or "Subjects, therefore people in general, of this type display more psi than subjects of that type." The interpretation is not, except perhaps to the Rhine-Rao school, "When psi is present in your experiment the subjects in your experiment working under such and such a condition will demonstrate more psi than subjects not in that condition." nor "When psi is present in your experiment your subjects of this type will show more psi than your subjects of that type."

The purpose of parapsychology is to uncover lawful relationships between psi phenomena and man, and for the most part this assumption underlies most contemporary research. Parapsychology, however, will get nowhere if it remains a collection of experimental incidents with no lawful relationship to anything because one half of that relationship is defined as having the power to come and go as it pleases without reference to the conditions under study. Those who wish to see parapsychology progress as a science view failures to replicate as important not because they wish to deny that earlier experimental results were paranormal but because a failure to replicate questions whether the variables and lawful relationships claimed to be relevant in obtaining the psi effects are in fact so.

With this approach applied to the failed replications reported in the previous chapter it may be concluded that the manipulation of hand response and hemisphere distraction are not, as previously thought, sufficient for the elicitation of psi in normal subjects. From where, then, came those effects in all the previous experiments suggesting that hand response and hemisphere distraction were sufficient to produce psi in the responses of the subjects?

Fortunately the parallel investigations reported herein indicate that it is no longer necessary to choose between the two unsatisfactory positions regarding failed replications. This new development points to the experimenter as a major source, if not the only source, of paranormal effects in the data of experiments

with ordinary subjects. The effects in the experiment may be quite genuine, assuming of course that they are not fraudulent, but not so much due to the particular hypothesis under investigation as to the experimenter's paranormal ability to cause random numbers to cohere into non-random patterns which serve to give the appearance of supporting the hypothesis.

This rather radical view of paranormal phenomena has its roots in the now well-documented but previously unrecognized fact that paranormal effects in experimental data are far more closely linked with individual experimenters than with particular hypotheses. The traditional view that this is due to personality factors and the manner in which the experimenter deals with the subjects has been found to be inadequate. On the other hand some of parapsychology's most successful experimenters have shown themselves to be highly effective sources of PK effects when they serve as subjects. In view of parapsychology's continuing inability to uncover a hypotheses which produces stable results the time is now ripe to make the obvious connections between the foregoing observations and advance the suggestion that certain gifted experimenters are able to use their subjects more or less as random number generators upon which they may exert a potent psychokinetic effect.

Two current theories of the operation of psi based on quantum mechanics have added strong support to the idea that many of parapsychology's effects may be due to the psi ability of the

experimenter in that they predict that a psi effect can only take place in situations where the source of the effect has knowledge of the results of his psi intervention. Subjects frequently are told (but not always) how well they have done (almost invariably after the experimenter already knows) so there is scope for an effect on the part of the subjects, if they have any reason for exerting it, not to mention the ability. But much of contemporary parapsychological research involves differential effects between groups or conditions about which the subject knows little or nothing. When it comes to totalling up the scores and comparing the conditions it is the experimenter who is getting the requisite feedback of any psi effects and there is little question of his desire to see 'effects' in the data.

It should be noted that the case for psi based experimenter effects as a major constituent of the findings of parapsychology does not rest solely on the predictions of the observational hypotheses. Indeed the problem is no less obvious than the psi indeterminacy problem which Rhine outlined with respect to the animal work (see page 158). He has pointed out that there was no way of conclusively attributing psi effects to the animals in the studies dealing with them since there was no way of ruling out the possible psi effect of the experimenter on the random generator which was intended for the animals. The case with regard to human subjects is exactly the same! On balance the evidence now seems to rest on the side of the experimenter.

The expectancy experiment reported in Chapter VI demonstrated that significant expectancy effects can be generated in a parapsychological experiment at the time of examining the data. Those results strongly supported the experimenter as psi source interpretation.

The evidence which has been gathered in the course of this research does not, unfortunately, permit the drawing of a definite conclusion concerning either hemisphere differences or the necessity of a psi-based experimenter effect interpretation of parapsychology. It does, however, point to a tentative conclusion which shall stand until the situation is shown to be otherwise.

The conclusions which this investigator wishes to draw from the research are as follows: (1) There was and still is a good case for the investigation of any possible relationships between hemisphere specialization and the workings of psi, (2) The experiments conducted to this end by the investigator did not provide evidence supporting an interpretation along the lines of ESP laterality effects but did demonstrate strong psi effects apparently produced by the experimenter himself on the scores of the subjects (as random numbers under the null hypothesis) and these psi effects, when present, conformed to the experimenter's desire to demonstrate laterality effects.

IMPLICATIONS FOR THE FUTURE OF PARAPSYCHOLOGY

As noted above it has not been possible to prove in a rigorous fashion the conclusions drawn from this research and for that reason many parapsychologists may choose to ignore them, particularly as they imply a radical reinterpretation of the findings of experimental parapsychology. Nonetheless these conclusions must be put forward and the alternative picture must be examined so that research in the future may decide which of the interpretations presents a better explanation of the findings of experimental parapsychology.

The first and most obvious implication of this research is that much of what has been reported in the parapsychological literature may be similar examples of psi-based experimenter effect. The effects reported in the vast majority of cases may have little or no relationship to the hypothesis under investigation except as that hypothesis has guided the experimenter in the application of his PK ability. It is easy to see that those experimenters who have this 'gift' could well be a self-selected group of individuals since those without the 'gift' would have given up trying long ago (with a few exceptions of course).

On this basis failures to replicate effects are to be expected. Failed replications need no longer be taken to represent the inexplicable failure of a designated group of subjects or a particular experimental manipulation but rather the more understandable failure of an individual source of psi influence - the

experimenter. Unselected subject groups may serve as convenient sources of random numbers upon which certain experimenters can exert a paranormal influence. Hypotheses about the characteristics of ESP may simply serve to channel an experimenter's expectation and psi influence in a certain direction. Thus for the problem of repeatability the subjects and the manipulations of the variables may well be irrelevant. The reason that experimental repeatability in this field has not yet been achieved may be because parapsychologists have been looking for the source of the psi effects in the wrong place.

The second implication for parapsychology is that extra-sensory perception as such may be an artefact of the test situation. The dominant psi effect may be psychokinesis in the sense of it being the ability to 'cause' quantum process sources of randomness to become non-random and meaningful. The fact that a subject appears to perceive extra-sensorially the order of the ESP cards may merely be a by-product of an experimenter's PK ability to make his subjects produce numbers greater than the chance expectancy. If the output of a random number generator was being matched against targets it would not be claimed that the machine knew what the targets were, yet it could well be the case that unselected subjects function much as random number generators for the experimenter. Even the cases of amazingly close correspondences for free response material in the dream or relaxation research as well as in spontaneous cases may be included within this framework if one allows for the possibility that the same goal-oriented ability to cause otherwise random numbers to 'make

sense' may affect the quantum noise in the brain and cause it to cohere into patterns to produce responses as suggested by Eccles and Stanford (see p.55 ff). Viewed from another perspective, however, it could indicate that the assumption made by L. Rhine (1953) and others, that the rich spontaneous material was the same phenomenon as correctly guessing cards in the laboratory, is incorrect and they are in fact quite different types of psi phenomena. It will be up to future work to resolve such questions.

Obviously this alternative view of parapsychology is not meant to suggest that all the effects lie with the experimenter alone. The situation regarding the few gifted subjects who have appeared from time to time is an example of the effect probably having its source with the subject. Gifted subjects are the exception, though, rather than the rule, rather like gifted experimenters. The cases of astronomically high scoring occasionally reported may be due to the joint action of a gifted experimenter and a gifted subject. Similarly the virtual abundance of high scoring subjects in the early days at Duke University may have been due to the coming together of a few gifted experimenters with only modestly gifted subjects. The fact is that gifted subjects, those individuals who regularly can bring about significant departures from chance, are extremely rare so it has always been an anomaly that certain experimenters could demonstrate, seemingly with great ease, high scoring with groups of randomly selected individuals. Presumably, if the psi originates with the members of the groups, some or all of these persons, if given the large

numbers of trials that high scoring subjects undergo, should also show themselves to be 'gifted' individuals. This does not, however, appear to be the case and one is left wondering why certain experimenters can find groups which score so well.

The point which is made by the research reported herein and must be recognized by parapsychologists is that they may no longer assume that the paranormal effects in their data arise from their subjects operating under the conditions set for them. Parapsychological research must be turned to look for the source of the psi effects in experimental data. Only when the source of the effects can be identified can controls be introduced and progress toward repeatability be made.

Acceptance of this alternative view of parapsychology will put severe strains on parapsychologists' ability to devise appropriate methodologies for the investigation of psi sources. The observational models of psi suggest leads which may be followed, particularly in their emphasis on the role of the knowledge of results in the psi process. The models are not without problems, among them being the difficulty of distinguishing between the various observers and their effects on an experiment (the divergence problem) and the lack of falsifiability which this implies if all subsequent observers can equally affect the results. Steps can be taken in this direction, by way of examining such matters as first observation, psychological variables of the observers, single versus multiple simultaneous first observers, etc. but the experimental road is sure to be a difficult one.

If in the end parapsychology can demonstrate that certain individuals do manage to cause complex sources of random variation, as found in experimental situations, to cohere into meaningful patterns then both it and its sister science, psychology, and even the whole of science, will never be the same.

A P P E N D I C E S

APPENDIX A

DESCRIPTION OF THE COMPUTER-LINKED RANDOM NUMBER GENERATOR USED IN CERTAIN EXPERIMENTS IN THE EDINBURGH LABORATORY

The 'Schmidt type' random number generator has become one of the basic tools of the parapsychologist. Many research organizations have devices based on his design. The core of the system used by Schmidt is a fast oscillator driving a ring counter (1,2,3,4,1,2,3,4...). The oscillator is connected for a random time interval and the state in which the counter is left defines the target. In Schmidt's first machines the source of the random time interval was the emission of alpha particles in radioactive decay. This, relatively speaking, allowed only a slow rate of trials (unless a prohibitively strong radioactive source was used) so his later machines, the 'fast' version, used electronic noise produced by an electronic component known, appropriately enough, as a noise diode.

The Edinburgh device utilized a complementary principle. A 'Random Switching Unit' designed by D. Wight in collaboration with B. Millar produced 'squared' white noise, i.e. the random spikes of the noise diode's output were converted into randomly timed square pulses. This signal was fed into a Linc-8 (Digital Equipment Co.) computer. The computer's software, designed by R. Broughton, sampled the signal a fixed number of times in a set

time period. (Initially it was 63 times in about 0.8 msec. although later it was determined that 15 times in 0.2 msec. was entirely adequate.) The number of times which the computer sampled and found a pulse were counted and that result was used to determine the outcome of a single binary trial depending on whether an odd or even number of pulses had been counted.

Although it was standard practice to run randomness tests automatically each time the device was used in an experiment, an initial acceptance trial of 25,000,000 individual trials was run. This yielded a total deviation from chance of only -42, a result exceptionally close to chance.¹

If other than binary numbers were required it was a simple matter to use the computer program to combine single binary trials to yield numbers of modulo 4, 8 or higher by powers of 2. For numbers not a power of 2 the unrequired digits were systematically ignored (as would be done with the unwanted digits of a random number table). Tests indicate that this is a very satisfactory way of obtaining high quality random numbers of any modulus and has been used in experiments, though not those reported here. In all cases where the randomness of the machine was used to test the hypothesis the experiment contained automatic checks of randomness at critical points in the experiment.

¹ This series was conducted by running 1,000,000 on 25 consecutive working days by R. Broughton and B. Millar on alternate days. It is interesting to note that R. Broughton was responsible for running the 25th test on which the deviation moved from approximately -550 to the final result of -42.

APPENDIX B

EVENTS FOLLOWING THE SUBMISSION OF THE BROUGHTON-MILLAR GERBIL ESP PAPER INCLUDING THE LEVY AFFAIR

Upon completion of the gerbil study (p.124 ff) the investigators felt strongly that their efforts should be made public for two reasons: (1) hopefully, criticisms of the report might bring to light the differences which caused the experiment to fail, and (2) the emerging picture regarding the ease of replicability in the animal studies needed to be corrected. In mid-May 1974 the paper was submitted to the Journal of Parapsychology, which up to that time had been responsible for publishing all of the animal research.

In a letter dated June 7, 1974 the editor of J. P. replied asking for one further analysis to be done and for clarification of certain points. The authors supplied the requested material. After a few more weeks a letter came back indicating that the editor, with Rhine's backing, had decided not to accept the paper for full publication although agreeing to publish an abstract in a separate section. The editor explained,

"Since the results were nonsignificant, we do not want to devote the necessary space that full publication would require even though we consider this a well designed and executed experiment - and well presented too."

The authors were not satisfied with the offer to publish an abstract so they accepted an invitation to publish the paper in the newly established European Journal of Parapsychology (Broughton and Millar, 1975).

That, however, was not the end of the matter. For many years the Journal of Parapsychology had been evasive concerning charges of editorial censorship of negative findings, such as those made by Beloff (1973, p.291), and even attempted to deny that it had such a policy in a review of Beloff's book by W.J. Levy (1974). Many colleagues, therefore, were interested in the progress of the Broughton-Millar paper and were disturbed by the confirmation of the J. P.'s censorship policies.

At about the same time parapsychology was in the throes of the first ever major scandal concerning an active experimenter. The experimenter was none other than W.J. Levy, then director of Rhine's Institute for Parapsychology and the principal investigator in most of the published animal studies. For the details of this case Rhine (1974b) may be consulted. In brief, the events were that Levy had been observed acting suspiciously by coworkers during an experiment in mid-May 1974. By 12 June there was hard evidence that he had been faking results and when confronted with this by Rhine he acknowledged his guilt and resigned immediately.

The news came as a personal loss to the many who knew W.J. Levy but it was even a greater loss to parapsychology since what had been hailed as one of the most promising developments in

the history of parapsychology had at a stroke been rendered almost worthless. A report on the case was read to the Parapsychological Convention several months later, coincidentally the same convention where the report of the failed replication of the gerbil work by Broughton and Millar was read. The gerbil paper, which under normal circumstances would have passed almost unnoticed, was now of great interest as the issue of editorial censorship became enmeshed with that of experimenter fraud.

Immediately Levy had departed the Institute's efforts were begun to replicate all the major findings which were associated with his name. This work continued through the year and by the Spring it was clear to some of the investigators that this work was not going to succeed in replicating Levy's findings. One investigator from the institute, J. Terry, submitted a report of this failure to be read at the 1975 P. A. Convention. Apparently the submission had been over Rhine's objections and Rhine attempted to suppress the report by contacting certain P. A. officers. A rather unseemly row erupted over the matter but in the end the failed replications report was presented and the P. A. Council unanimously passed the following motion which is now appended to the annual 'Call for Papers' notice:

"That Council go on record as opposing any policy of discouraging the publication or public presentation of nonsignificant results or a policy of refusing to allow publication or dissemination of such results."

The immediate impetus for this action by the P. A. Council was the row which broke out over the Terry (1976) paper but the

council also was mindful of a recent article by Rhine (1975b) entitled "Publication policy regarding nonsignificant results" which was published in the J. P. only two months before the convention. Rhine's article was in direct response to the growing dissatisfaction among parapsychologists with his handling of the Broughton-Millar paper which, although unnamed, serves as the first of his examples on page 141 of the article. Rhine invited comments on his publication policy and in due course a reply from the Edinburgh parapsychologists restating the obvious disadvantages of such a policy was published (Beloff, Broughton, and Millar, 1976).

It was unfortunate that the question of editorial censorship of negative findings which was reawakened by the Broughton-Millar paper became entwined with the sad demise of one investigator but it brought a measure of satisfaction to the authors to have been partly responsible for bringing the issue to the fore and prompting action by the parapsychologists' professional organization.

APPENDIX C

LETTER TO THE EDITOR OF THE JOURNAL OF THE AMERICAN
SOCIETY FOR PSYCHICAL RESEARCH CONCERNING AN
'EXTENSION' OF THE SHAPES EXPERIMENT

To the Editor of the Journal:

It was encouraging to see from the recent paper by Maher and Schmeidler (July 1977 issue of this Journal) that some investigators are attempting to follow up my work concerning brain hemisphere differences in ESP function. I should, however, like to take this opportunity to comment on certain issues facing would be replicators.

The field of brain hemisphere specialization is a very complex area of study which belies the facile generalizations frequently used to describe the findings. Fortunately Maher and Schmeidler show themselves aware of this but I have noticed a tendency elsewhere for parapsychologists to be rather loose in the interpretation of the highly specialized findings regarding hemisphere asymmetries. Such labels as 'logical' for the left or 'intuitive' for the right are largely speculative leaps from more limited findings. I have even noticed second order speculative leaps disguised as facts creeping into the literature, e.g. since the right hemisphere is intuitive it must be more inwardly oriented.

Current research presents a different, and to my mind far more interesting picture. The emerging view is that the two halves of the brain work in close cooperation to generate unitary conscious experience with each half specialising in a particular form of information uptake. The right hemisphere handles an externally attracted environmental scan for sources of information while the left employs an internally directed and highly selective strategy by which particular items in the sensory field are matched against expectations. Parapsychologists intending to work in this area would do well to be thoroughly aware of the most up-to-date research as it is far more helpful for an understanding of hemisphere function than the frequently encountered lists of 'abilities' for each hemisphere, most of which are by-products of the restricted manner in which they are measured.

The Maher and Schmeidler paper raises a number of questions concerning procedures used to test hemisphere differences as well as some more general questions regarding the proper way to go about replicating and extending the findings of other researchers. As hemisphere function and ESP is likely to be an increasingly popular area of research in the future and replication remains a vital part of parapsychological research I should like to address these questions in this letter.

The first problem of which investigators in this area must be aware is that certain ESP tasks may have cognitive aspects which favor one hemisphere. Maher and Schmeidler are certainly aware of this but their solution seems to encounter the baby and

bathwater problem. The authors express an interest in the fact that my experiment made use of three-dimensional target shapes. This, of course, was the essential feature of the method which associated the target selection with one or the other hemisphere since, as Maher and Schmeidler correctly point out, the primary neural focus of fingertip sensation and motor control is unilaterally represented in the contralateral hemisphere. They were not happy with this arrangement because, as they claim, 'The right hemisphere is known to be specialized for tactile-spatial processing'. One would have wished for a reference or two to support this very general claim because while there is much evidence supporting the 'spatial' part of the hyphenation there is rather less, and none that I can find, Witelson¹ notwithstanding, which gives such unqualified support for the 'tactile' part. While a right hemisphere advantage has been demonstrated in tactual identification or memory for meaningless shapes, abstract wire forms, and certain tactile mazes, there has been no evidence of which I am aware that contradicts the early findings of Sperry and his colleagues showing no hemisphere advantage in the tactual identification of commonplace three-dimensional objects. Thus the use of such objects as a rough approximation to an ESP probe of a single hemisphere does not appear to be contraindicated by the evidence so far. The task used in my work was specifically designed to tap the neural substrate of possibly ESP guided recognition of tactile targets and was intended as a direct parallel to card guessing viewed as an ESP guided visual recognition task. When all possibility of tactually discriminating the targets is eliminated, as in the Maher and Schmeidler experiment, then the subjects' selections are being made

on some basis other than the tactual information specifying the differences between the targets although it is only this information which can be said to be unilaterally represented. Whereas without ESP the tactual discrimination between cone, cube, ball, etc., which it is hoped may be influenced by ESP, to some degree can be said to be occurring within one hemisphere it remains an open question as to where occurs the discriminations between identically feeling plastic cubes containing different objects (clovers, bits of plastic, or slips of paper). In other words the change introduced by Maher and Schmeidler obviates a very fundamental aspect of the experimental design used to separate functionally the responses of the hemispheres in my experiments.

Future investigators must be very careful then in deciding between these two methodologies, or in creating their own, because, as the foregoing shows, there may be very fundamental differences in otherwise apparently similar tasks. For those worried by the whole problem of the possible cognitive loading of many ESP tasks they may consider developing methodologies along the lines of response time measurements of ESP influence where hemisphere differences have been noted as well.²

A second point of caution for intending investigators of hemisphere differences in ESP concerns the choice of unilateral distracting tasks. Only part of the problem is finding a task appropriate to each hemisphere. One must also choose a task which can be monitored as a continuous process on the part of the subject to insure he is not 'turning it off' at the moment of making his

guesses. Maher and Schmeidler report that I had used a distracting task for the left hemisphere, 'reasoning that for ESP, the left hemisphere was best kept out of the way'. This is a rather simplistic misrepresentation of my position since I had clearly stated that the reason I had not used an obvious balancing condition was that I could find no suitable candidate for the right hemisphere.³ The pattern tracing task used by Maher and Schmeidler was among the first to be deemed unsatisfactory due to a serious confounding of hemisphere activation which, for some reason, has been ignored by those investigators.

Unlike a task which is monitored by verbal output, such as reading or the answers to syllogistic problems, the pattern recognition task requires an output consisting of finely tuned hand movements, a major component of which is arguably unilateral in origin. Thus when the pattern recognition is in progress (occupying the right hemisphere) and the left hand is selecting a target cube the right hand is tracing the pattern thereby involving the complimentary sensorimotor area of the left hemisphere in the supposedly right hemisphere (only) distracting task. One can see that it is not an easy matter to find tasks for the hemispheres which are sufficiently symmetrical in output to allow proper comparison.

Of course how these differences in methodology may have affected the results is a moot point. The analysis of variance which Maher and Schmeidler used to evaluate their factorially designed experiment demonstrated that no condition differed

significantly from any other condition. This being the case, extreme caution must be exercised in the interpretation of the results obtained from the many ways of splitting or combining the data.⁴ The problem in the case of Maher and Schmeidler's results is further compounded by the fact that the significant departures from chance which were noted were in most cases obtained using the C.R., a method generally agreed to be inappropriate in this situation.⁵ This oversight is particularly unfortunate coming as it does so hard on the heels of the exchange between Stanford and Tart (January and April 1977 issues of this Journal) in which precisely this problem was much under discussion. Thus until more appropriate analyses are available little can be said on how the Maher and Schmeidler findings relate to other work in this field.

I am sorry that it requires a letter to the editor to draw attention to the very important differences between my work and that of Maher and Schmeidler since ordinarily such matters can be determined by the readers themselves in comparing the reports. For some reason, however, the readers are not given this opportunity by Maher and Schmeidler. The main statements of the rationale behind and the procedures used in the work which they are claiming to extend have appeared in previous years in a well-known parapsychological journal⁶ yet there is no reference whatsoever to these papers. Instead all that is provided is a reference to the abstracted report published in Research in Parapsychology 1975.⁷ It is certainly unfair to readers and, more importantly, would be replicators, that they should be misled into supposing that this was the only report to be consulted in conjunction with the Maher and Schmeidler paper.

Another curious omission on the part of Maher and Schmeidler concerns the matter of sex differences which were noted in the data of my shapes guessing experiments and reported in the 1976 paper. Though claiming to extend this very research Maher and Schmeidler make no mention at all of these findings let alone any attempt to relate them to their own, seemingly unexpected findings on this topic. It is true that the sex differences reported in my work are not entirely compatible with their interpretation but it is plainly unscientific to pretend that they do not exist.

Sex differences in ESP/brain hemisphere research were first reported by me at the P.A. Convention in Utrecht in August, 1976. This was in an experiment using a different methodology but the sex differences were reported as being similar to that found in the shapes guessing experiment. Coincidentally the next paper read was a report by Maher and Schmeidler of what appears to be the same experiment that was reported in this Journal but without any mention of sex differences and indeed the junior author admitted to me that they had not thought to look for sex differences before that time. (The uncertainty as to whether the two reports refer to the same experiment arises from the fact that the Utrecht report clearly stated there were 5 male and 19 female subjects while the later report in this Journal which included the sex difference findings refers to 7 males and 17 females. Presumably there is some simple explanation for this discrepancy.) It should be emphasized that the point at issue here is not a claim

to priority, though Dr. Schmeidler has shown herself sensitive to this matter (see Correspondence, page 443, October 1974 issue of this Journal), but whether it is acceptable investigative procedure to carry on what claim to be extensions of another investigator's work while at the same time selectively ignoring large portions of his findings and providing readers no clue as to where this ignored material is to be found.

I am still of the opinion that there are very good reasons for parapsychologists to continue investigations into the relationship between ESP and brain hemisphere specialization. As one of the instigators of the present wave of interest in this topic among parapsychologists I feel some responsibility for insuring that this research maintains a suitably high level of quality. Such work is too important to allow unforeseen difficulties to weaken the interpretations after the work is completed.

My own research has taken an unexpected turn which makes the need for replications and extensions all the greater. Having experimentally stirred the murky waters of psi-based experimenter effects and found strong suggestions that I as experimenter can influence results in even highly computerized experiments⁸ I have undertaken two very careful replications of my earlier studies on hemisphere differences. Neither of these produced results even approximating those of the earlier experiments. (A full report on this recent work is in preparation.) Where this leaves hemisphere differences and ESP I am not sure but it is clear that independent research in other laboratories is necessary.

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NOTES

1. The term "tactile-spatial" is used by Witelson in a paper cited in another context by Maher and Schmeidler (Witelson, S.F., "Sex and the single hemisphere: Specialization of the right hemisphere for spatial processing." Science, 1976, 193, 425-427.) although the tactual task was limited to palpating two meaningless shapes.
2. Broughton, R.S., "Brain hemisphere differences in psi-influenced reaction time." In Morris, J.D., Roll, W.G., and Morris, R.L. (Eds.) Research in Parapsychology 1976. Metuchen, N.J. : Scarecrow Press, 1977, ??-???
3. Broughton, R.S., "Possible brain laterality effects on ESP performance." Journal of the Society for Psychical Research, 1976, 48, page 398.
4. See Barber, T.X. and Silver, M.J., "Pitfalls in data analysis and interpretation: A reply to Rosenthal." Psychological Bulletin, 1968, 70 (6, pt.2) 48-62; or Wicklund, N., "Parapsychological temptations: a discussion of decline effects, sex-effects, and post-hoc effects." In Morris, J.D., Roll, W.G., and Morris, R.L. (Eds.), Research in Parapsychology 1976. Metuchen, N.J. : Scarecrow Press, 1977, ??-??.
5. See Stanford, R.J. and Palmer, J. "Some statistical considerations concerning process oriented research in parapsychology." Journal of the American Society for Psychical Research, 1972, 66, 166-178.
6. Broughton, R.S., "Psi and the two halves of the brain." Journal of the Society for Psychical Research, 1975, 47, 133-147. Broughton, R.S., "Possible brain laterality effects on ESP performance." Journal of the Society for Psychical Research, 1976, 48, 384-399.
7. Broughton, R.S., "Brain hemisphere specialization and its possible effects on ESP performance." in Morris, J.D., Roll, W.G., and Morris, R.L. (Eds.) Research in Parapsychology 1975. Metuchen, N.J. : Scarecrow Press, 1976, 98-102.
8. Broughton, R.S., "An exploratory study on psi-based experimenter and subject expectancy effects." in Morris, J.D., Roll, W.G., and Morris, R.L. (Eds.), Research in Parapsychology 1976. Metuchen, N.J. : Scarecrow Press, 1977, ??-??.

(Editor's Note: The question marks refer to the page numbers in a volume which is not yet out but is expected shortly, we hope.)

R E F E R E N C E S

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Abbreviations of frequently cited journals

- | | | |
|--------------|---|---|
| E.J.P. | - | European Journal of Parapsychology |
| I.J.P. | - | International Journal of Parapsychology |
| J.P. | - | Journal of Parapsychology |
| J.S.P.R. | - | Journal of the Society for Psychical Research |
| Proc. S.P.R. | - | Proceedings of the Society for Psychical Research |
| J.A.S.P.R. | - | Journal of the American Society for Psychical
Research |

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