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EDITORIAL STAFF

The European Journal of Parapsychology publishes original experimental and theoretical papers on parapsychology. In addition, articles presenting reviews of literature, case studies and papers on related topics are accepted in so far the subject can be considered relevant to the understanding of parapsychological phenomena or to the methodology of empirical research in parapsychology. Translations of papers originally published in a language other than English are also welcome.

Manuscripts should be written in English and should be typed, double-spaced throughout, with a wide margin, on one side of the page only. Number the pages consecutively beginning with the title page. Within the typescript indicate by a brief note where a table or figure should appear. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include a title page stating title, full names of authors, positions and other affiliations, and mailing addresses. Each table should be typed double-spaced on a separate sheet and given a descriptive title. Number tables consecutively with Arabic numbers in order of their mention in the text. Tables should be designed to fit into a space of 12 x 19 cm. or less. Figures should be drawn on white paper in black lines. Figures are normally reduced in reproduction and allowance must be made for corresponding reduction in the thickness of lines and the space between them. The numbered legend of a figure should be typewritten on a separate sheet of paper, and should not appear on the page with the figure. References in the text should be indicated by dates in parentheses following the authors' names and should correspond to an alphabetical list at the end of the paper. References to books should include year and

(continued inside back cover)

TO THE READER

We have pleasure in drawing your attention to this demonstration issue of a periodical which will be identified as the European Journal of Parapsychology. As has already been stated in the inside back cover, the journal will appear twice a year.

The main emphasis will be on experimental articles but theoretical articles will also be welcomed. The latter category should be concerned with issues such as definitions in parapsychology or design of experiments, or with the development of useful methods for the evaluation of parapsychological results or observations. It should be observed that theoretical must not be viewed as antithetical to experimental since theories are just as important for the scientific understanding as experimental data are for the theories. It should also be understood that experiments are no more and no less than observations made under rare and special circumstances which we choose ourselves, or at best "only experience carefully planned in advance" as R.A.Fisher put it. Under favourable conditions the variables which we try to identify and control in our experiments will enrich our experience and our knowledge, and if the design is a good one, it will maximize the information yield per unit of research cost.

When it comes to the ideal of science to which we adhere, we have to admit that we are rather strongly influenced by Karl Popper's school of thought, and his great emphasis on the fact that it should be possible to put a theory to a severe test of falsification in order to be a useful tool in science. When it comes to theories we also share his view that the more complicated a theory is the less it says, for the harder it becomes to falsify it, and the easier it becomes for those who defend it to "immunize" the theory against a test of falsification.

Another hallmark of our journal will be the avoidance of selective reporting, that is, the tendency to bury negative results and only to publish studies that "turned out". For one thing we believe that there is a chance to learn something important from negative findings; for another the policy of exclusively publishing

"supporting" findings will exert a strong temptation to "doctor" ones data. The quality of the design and methodology and the rationale of the study will be judged as more important than the level of significance of the outcome of the study. In the May issue of our journal an article will appear (by Martin Johnson) in which some basic questions related to the system of control and reporting in science will be discussed. The article has some bearing on what we would like to see as the basic research ideal and policy of publishing in our journal.

We hope to be able to extend the editorial staff in such a way that as many European countries as possible will have their representative on the editorial staff.

The main objective of the journal as we see it, is to stimulate and facilitate the furthering of European endeavours in experimental and allied fields of parapsychology. However, contributions from authors outside Europe are also warmly welcomed. We would like to think that the quality of the articles will maintain such a standard that they can favourable be compared with those appearing in the leading American contemporaries. In this way we may be able to establish a sense of friendly competition with the American journals and our colleagues there, to the benefit to our field in general.

Every research worker in our field is naturally invited to send a contribution for possible publication, if he feels he shares our research ideals, and can cope with them.

The State University of Utrecht is the juridical owner of this journal.

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Martin Johnson

Sybo A. Schouten

CODING OF TARGETS IN A CLAIRVOYANCE TEST

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INTRODUCTION

Experiments by McFarland and George (1937), Chauvin (1961), Sanders (1962), Freeman (1961) and Rao (1962), indicate that using contrasting targets in ESP tests gives different scoring on the two kinds of targets. On the basis of this work, Krippner (1965) tested the hypothesis that scoring would be different between clairvoyant response to target symbols in the form of words (non-coded targets), and target symbols in the form of photographs of the objects symbolized by the words (coded targets). The experiment confirmed his hypothesis: Subjects scored significantly positively on the non-coded targets while scoring at chance level on the coded targets.

As an explanation for the preference shown by the subjects for non-coded targets, Krippner suggests that perhaps non-coded targets are more easily perceived than coded targets, which would indicate a similarity between normal and paranormal perception. This however, seems unlikely since under both conditions (coded versus non-coded targets) subjects had duplicate postcards of the target objects in the code identical to that of the actual targets placed on the table in front of them for reference purposes.

That targets in the form of photographs of the objects were termed "coded" merely refers to the fact that under both conditions subjects had to circle the word of the chosen object on the answer sheet. Thus subjects knew in both conditions what type of target was being used, and it is quite possible that in both conditions subjects just chose one of the five postcards placed in front of them and then circled the appropriate name. If this was the case, it seems more probable that the differential response was caused

by a "psychological preference".

Now it would be interesting if it could be demonstrated that non-coded targets are more easily perceived than coded targets. This would suggest that in the forced choice type of ESP experiment - which is based on guessing from among a limited number of known objects - subjects match in each trial each object with the target and base their guess on a perceived concurrence between one of the objects and the target. This process would be quite different from the normally accepted hypothesis that it is the "idea" of the object which is transmitted. An indication which speaks in favour of this matching-hypothesis can be found in the findings of Fahler and Osis (1966) which show that subjects who scored at chance level were nonetheless able to check at a very significant level those guesses which afterwards turned out to be hits.

The purpose of the study reported here was to test whether scoring on targets would be different from scoring on the same targets in coded form without the subjects knowing that coded targets were used, in order to avoid psychologically based preferences. Since in the trials with non-coded targets, subjects could guess according to the matching-hypothesis but could not do so when targets were coded, it was hypothezed that scoring would be different for both conditions, favoring the non-coded condition.

PROCEDURE

An experimental session consisted of seven short series of 20 trials each. In each trial the subject had to choose between five different items. In each series a different kind of target material was used. The purpose of using short series and different groups of target symbols was to keep the subject as interested as possible during the experiment.

The stimulus materials used were:

1) Standard ESP symbols;
2) A varying number of dots, numbers ranging from one to five;
3) Handdrawn letters of the alphabet;
4) Colored cards;
5) Handdrawn clock faces, the hands indicating a certain time;

In the other series a mixture of symbols was used. Symbols were drawn in ink on small postcards, except for the colored stimulus cards, which were cut-up sections of regular paper.

In each series half of the stimulus cards contained handdrawn symbols (non-coded targets). On the other half of the stimulus cards, the words of these symbols were typed (coded targets). Thus each series contained 20 cards, four of each symbol, two being drawn and two typed. The use of equal numbers of each symbol in the series and in both conditions was necessary to eliminate the possible influence of preferences of the subjects for certain symbols. The cards were enclosed in small opaque envelopes, each envelope being given a letter, which indicated the series, and a number which indicated the specific target.

In all, 24 undergraduate students participated in the experiment. Each subject took part in one session, which lasted nearly one hour. Before starting the actual experiment, an informal talk was given on ESP testing in general.

The experimenter then gave the instructions about the experiment itself. The subject was told that the purpose of this experiment was to select high-scoring subjects for further clairvoyance experiments. The subject was not informed about the real purpose of the experiment.

Five postcards with drawn symbols, identical with the drawn symbols in the target deck of the appropriate series, were placed before the subject so that he could refer to them while taking the test. He was led to believe that all target symbols in the deck were of the same kind as the duplicate symbols. Then the envelopes of the deck were shuffled and placed before the subject. He was instructed to take the first envelope in his hands, then to look for one of the duplicate symbols which in his opinion would match the symbol in the envelope.

In one series the procedure was different. In this series the subject was presented with 20 decks of five envelopes each. He was told that four envelopes contained the same symbol, for instance red, one envelope however contained a different symbol, for instance the drawing of a house. The subject had to choose the envelope with the different symbol.

After making his choice, the subject called the name of the symbol and then showed the experimenter the code letter and number of the envelope. The experimenter recorded the answer. The experimenter did not know at the time of the experiment the relation between envelopes and symbols. At the end of the experiment the envelopes were opened and first recorded on a target sheet. After this the answer sheet were checked for hits and misses.

RESULTS

Greenwood and Stuart (1937) gave the formulae 1.02(pqn) for computing the variance in case the number of symbols in the deck are equal for each of the symbols. However, this formulae is not appropriate for the present data because the factor 1.02 is based on decks of 25 symbols; and in this experiment the decks were of a different size. For this reason it was decided to utilize a non-parametric test, the Chi-square analysis. The overall results for the experiment are presented in table 1.

TABLE 1

condition	trials	MCE	hits	misses	X^2	P
coded	1680	336	297	1383	5,66	.02
non-coded	1680	336	353	1327	1.08	-

In the coded condition, the subjects scored marginally significantly in a negative direction, but scored at chance level in the non-coded condition.

TABLE 2

	number of subjects scoring		
	MCE	=MCE	MCE
coded	4	5	15
non-coded	15	0	9

Table 2 presents the number of subjects scoring above, on and below chance level in both conditions. As the same subjects participated in both conditions, it was decided to utilize the

Wilcoxon matched-pairs signed-ranks test (Siegel 1956) to test the difference in scoring under the two conditions. The Wilcoxon test yielded a T=53, which for 22 subjects - two subjects got the same number of hits under both conditions - is significant at the .01 confidence level. Of this 22 subjects, 17 scored lower on the coded targets.

DISCUSSION

At first glance one would conclude that the stated hypothesis, which implied that subjects would score higher on non-coded targets, was confirmed by this experiment. However, since the condition involving coded targets yielded a marginally significant result while the condition involving non-coded targets yielded chance results, this conclusion cannot be maintained. Krippner's proposition, that coded targets are more difficult to perceive than non-coded targets, is not supported by this test.

Moreover, the results contradict the so-called matching hypothesis. The subjects showed a differential response to the two types of targets, but the experiment failed to give any indications about the reason for this effect.

SUMMARY

In a clairvoyance experiment with 24 subjects, coded and non-coded targets were used to test the hypothesis that non-coded targets are more easily perceived than coded targets. The subjects did not know the purpose of the experiment or that in half the trials they guessed for coded targets. The subjects scored significantly different in the expected direction in both conditions, but as they only scored marginally significantly in the coded condition, and not significantly in the non-coded condition, the hypothesis could not be confirmed.

ACKNOWLEDGMENT

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ESP AND SUBLIMINALITY

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INTRODUCTION

Myers (1895) has already proposed a relationship between preconscious processes and the paranormal. From the onset of case studies in parapsychology the reasons for the occurences of spontaneous psi phenomena were discussed in more or less elaborated psychodynamic terms.

As Beloff (1972, p. 16-19) has recently stressed, there are several similarities between what is described as subliminal perception and ESP. Beloff alludes among other things to Dixon's statement that when a stimulus falls between two critical levels of intensity, it tends to elicit a response which is not just weaker but qualitatively different from those elicited by supraliminal stimulation. For readers especially interested in the controversy over subliminality, see for instance Smith (1970) and Dixon (1971).

In an experimental context the influence of subliminally exposed stimuli on dreaming was probably first studied by Pötzl (1915). Fisher (1954, 1956, 1957, 1960), Fisher & Paul (1959) and Luborsky & Shevrin (1958a, 1958b, 1959) have reported findings supporting the hypothesis that subliminal perception, not consciously registered, later tends to emerge in the dreams of the subject. This seems to be especially true if the subliminal stimuli are related to "traumatic" experiences in the subject's life history. Windahl (1971) has recently reported striking and corroborative evidence of the effect of subliminally exposed "micro-traumatic" stimuli upon dreams.

As regards the concept of and the function of the "traumatic dream" in psychoanalytic thinking, see Schur (1966). During the last two decades Sweden has been one of the main centers for

experimental studies in subliminal perception. Kragh (1970) invented and developed the Defense Mechanism Test, the DMT, a projective test. When using the DMT threatening pictures are flashed by means of a tachistoscopic viewer. After each exposure the subject has to report his impressions verbally or by writing or by a combination of verbal and written reports. The stimulus intensity is increased by steps. This "fractionated" procedure will give us a set of different levels of interpretations from the subject's side starting at a rather subjective stimulus interpratation and ending as a rather veridical one. The responses are coded and interpreted according to a manual. The manual is based both on empirical findings and on theoretical assumptions within a psychodynamic frame of reference.

Over the years a number of ESP/DMT studies have been carried out by myself and associates (Johnson, 1967, 1970, 1971; Johnson & Kathamani, 1967; Johnson & Nordbeck, 1972). The principal finding is that there exists a relationship between ESP scoring direction and the degree and quality of perceptual "defensiveness"(described by Kragh as "precognitive defense organization" or PDO) as reported in a subject's protocol. Strong signs of certain perceptual-defensive structures tend to produce psi-missing whereas a low level of "defensiveness" or freedom from certain PDO's tend to yield positive scoring. It has also been shown that a person's ability to recall dreams - or it may be more precise to say, his willingness to report his dreams - can fairly well be predicted by DMT data. There are also tentative findings suggesting that a person's dream reports may be correlated with his scoring behavior on an ESP task. In a study not directly related to DMT work (Johnson, 1971), the present author obtained data suggesting that a person who ordinarily would be a psi-hitter can be manipulated to exhibit selective psi-missing by a relevant choice of life history target material. Some findings seem to indicate that if concepts related to "traumatic" episodes in the subject's anamnesis are used as target material, then the chances are that the subject will manifest "missing" on these targets (Johnson, 1971; Johnson & Nordbeck, 1972). These findings, tentatively interpreted within a psychodynamic frame of reference together with the notion of the experimental induction of a "micro-trauma" constitute the conceptual background of the present study and of the attempt to try to enhance a subject's psi vigilance by the use of a subliminal technique.

PROBLEM

In view of the discussed tentative findings the next logical step in our inquiry would be to try to find out if a subject's psi-vigilance is affected if the same picture motif as the one used for subliminal induction of a micro-trauma also is utilized as a target in a subsequent ESP task.

METHOD

Population

Eighteen subjects, three females and fifteen males took part in the "micro-trauma" experiment. They were all freshman in psychology at the University of Utrecht. The selection of the subjects was based mainly on their general attitudes towards the paranormal and their willingness to participate in a number of small psi experiments at the Psychological Laboratory, carried out by Sybo A. Schouten and myself.

By and large nine of the Ss could be classified as having an accepting or favourable attitude towards psi wheras nine were characterized as having a negative attitude.

Procedure of Inducing the "Micro-Trauma"

The day before a subject was going to take his ESP task he took part in a tachistoscopic experiment. The tachistoscope consists of a viewer and an electronic timer (This type of tachistoscope used for individual testing has been developed by professor Kragh and his group). Different pictures were shown. As stimulus for male subjects a nursing motif was utilized. For females a picture depicting intercourse between a wolf and a woman was used. Both pictures were supposed according to the psychoanalytic paradigm to function as preoedipal threat stimuli.

The picture was flashed six times at 10 milliseconds (beyond awareness). It was found that none of subjects could give a stimulus proximal or veridical description of what they had seen.

The ESP Task

Before the ESP-experiment, two sets of target items had to be prepared by a research assistant. One set was for male subjects and the other for females. Each set consisted of 100 identical envelopes, distributed into 20 batches, 5 envelopes in each. In each of the envelopes constituting a batch, a target picture was inserted after having been wrapped in aluminium foil.

The target picture was a photocopy of the slide used for "induction". In the remaining four envelopes of a batch, there were photocopies of the same size and material but depicting four of the classical ESP card symbols.

The final order of the envelopes within each batch was determined by the use of a random table. Serial numbers were written on the back of each of the randomly distributed envelopes within a batch. These numbers used for identification purposes, could not be seen during the procedure when the subject made his decisions in his ESP task. The subject was told that in one of the envelopes in each of the batches there was a picture similar to the one the subject had been exposed to subliminally the day before. The subject was encouraged to try to pick out the envelope he thought was the appropriate one. When he had done so, the experimenter made a notation of the number, put the envelope back into its batch; took the next batch and handed it to the subject, and so on until he had made his 20 choises. The results were evaluated at the end of the series. By means of the identification marks (digits) on the back of the envelopes, the experimenter did not have to open the envelopes and carry out the check until all 18 subjects had carried out their ESP task.

The Use of the DMT

This investigation offered another chance to cross-validate previous findings as regards the DMT. Furthermore the DMT was thought a potentially good source of information concerning how differences in personality, measured by the DMT, may respond to the subliminal micro-traumatic stimuli "induced" before the ESP task.

As a safeguard against the experimenter expectancy effect, it was decided that the experimenter should have no information about the DMT protocols of the subjects. In addition the scoring of the protocols and the predictions (based on the scorings and on previous findings of the DMT/ESP correlations) were carried out blindly and by DMT experts.

The Testing

Mr. H. Boerenkamp (H.B.) administered the DMT. In advance of the testing he received the necessary practical and theoretical training for administering the DMT. After the DMT testing had been carried out by H.B., he translated the statements given by the subjects in their protocols and in relation to sketches they had made, from Dutch into English. The translated protocols,

together with the sketches in the protocols were later send to Mr. S. Bàlint (S.B.), at the Department of Psychology, Lund University. S.B. has considerable experience in scoring DMT protocols.

Scoring the DMT-protocols

S.B. was responsible for the scoring of all the protocols.

Predictions Based on the DMT

S.B. sent the scored DMT protocols to Kragh. As previously has been mentioned. Kragh is the inventor of the DMT procedure as well as of the paradigm on wich the test is based. His task was to try to predict how the subjects should respond to the "micro-traumatically" associated ESP target. He was instructed to base his predictions in part on the perceptual defensive structures appearing in the subjects' protocols, in part on previous findings of the DMT/ESP relationship, and to some extent also on his own theoretical considerations. Finally he was asked to rank the 18 subjects from the one he thought should score highest to the one he thought should score lowest on the ESP task. He was also asked to dichotomize his ratings into a group of nine hitters and nine missers.

HYPOTHESES

1. Subjects having protocols characterized by no or moderate signs of the PDO classified as "isolation" should tend to manifest positive scoring in their ESP task, especially if there were also moderate signs of the perceptual defensive organization, defined as the sign of "projection".
2. Subjects having protocols characterized by strong signs of "isolation" should tend to manifest scoring below mean chance expectation, especially if combined with signs of "projection".
3. It was also tentatively expected that the "subliminal induction" should lead to an enhanced psi-vigilance among the subjects, an effect that according to Hypothesis 1 should enhance positive scoring among certain individuals and according to Hypothesis 2, other subjects should manifest a rather pronounced psi-missing syndrom.

TABLE 1

Outcome of predictions based on the DMT and on the ESP task

Ranking based on DMT	No of hits on ESP task	Dev. from M.C.E. (M.C.E. = 4)
1	6	+2
2	3	-1
3	8	+4
4	6	+2
5	7	+3
6	8	+4
7	1	-3
8	7	+3
9	3	-1
10	4	0
11	8	+4
12	1	-3
13	2	-2
14	3	-1
15	3	-1
16	3	-1
17	3	-1
18	4	0

Rankingnumbers 1 to 9: "Better half" according to predictions
10 to 11: "Poorer half" according to predictions

RESULTS

Kragh has to some extent been succesful in his prediction of the scoring behaviour of the subjects on their ESP task. Both with the Mann-Whittney U test and the Kolmogorov-Smirnov test for two independent samples a difference between the two groups of scores is obtained which is marginally significant (.05.LT.p.LT..06).

The individuals in table 1, constituting the "better half" obtained 13 hits more than M.C.E., which gives a CR = 2.42 with an associated p of less than .01 (one-tailed).

DISCUSSION

The outcome of this pilot study gives a suggestive and corroborative support of previous findings of a relationship between DMT measures ("preconscious defensive organization") and scoring behaviour on an ESP task. Hypothesis 1 and 2 can be said to be supported by the findings, while no conclusions can be drawn as regards Hypothesis 3.

We have not so far proved that the process of induction of supposed "micro-traumatic" stimuli really affects psi-vigilance of a subject. The findings are however judged as interesting enough to motivate further studies with an improved design that will make it possible to evaluate the supposed effect of an subliminal "induction". Such studies are now under way and will soon be reported.

SUMMARY

The notion of a relationship between ESP and subliminality is an old one. The controversy about the existance and character of subliminal perception seems to have diminished in certain respects and has made way for the psycho-dynamic interpretation. More sophisticated studies on the Pötzl-effect, for instance the effect of subliminally exposed "micro-traumatic" stimuli on subsequent dreaming, have recently been reported. Dr. Kragh's Defence Mechanism Test (DMT) has now been in use for more than a decade in both studies of perception-personality and as predictor of a subject's scoring behaviour in an ESP task.

In this study an attempt was made to influence the scoring behaviour by means of subliminally exposed "micro-traumatic" stimuli, wich in the subsequent ESP experiment was used as one category of targets, in a multiple choise situation (P=1/5; Q=4/5). 18 subjects took part in the experiment. 20 trials per subject were made.

The subjects were DMT-tested prior to the ESP task. It was hypothesized that the "subliminal induction" should lead to enhanced "psi-vigilance" among the subjects, that according to the character of their "perceptual defensive organization" in their DMT-protocols, should lead to either psi-hitting or psi-missing. The predictions were made by Professor Kragh. A marginally significant result was obtained. The outcome is judged as motivation for further studies, following an improved design.

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The Parapsychology Laboratory of the State University of Utrecht was established in 1974, in connection with the inception of the first regular chair and professorship in parapsychology in Europe (Director: Martin Johnson).

The laboratory has an independent status in relation to the Psychological Laboratory of the University (address: Varkenmarkt 2, Utrecht, the Netherlands). The Parapsychology Laboratory is located within the same building and has the privilege of sharing the library as well as having full access to equipment and apparatus within the Psychological Laboratory.

Between 1971 and 1974 the research center was identified as the Parapsychological Division of the Psychological Laboratory from which its Research Letter was issued.

The laboratory has four full-time positions one of which is intended for a visiting research worker.

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Contents of the forthcoming issue.

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The European Journal of Parapsychology publishes original experimental and theoretical papers on parapsychology. In addition, articles presenting reviews of literature, case studies and papers on related topics are accepted in so far the subject can be considered relevant to the understanding of parapsychological phenomena or to the methodology of empirical research in parapsychology. Translations of papers originally published in a language other than English are also welcome.

Manuscripts should be written in English and should be typed, double-spaced throughout, with a wide margin, on one side of the page only. Number the pages consecutively beginning with the title page. Within the typescript indicate by a brief note where a table or figure should appear. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include a title page stating title, names of authors, positions and other affiliations, and mailing addresses. Each table should be typed double-spaced on a separate sheet and given a descriptive title. Number tables consecutively with Arabic numbers in order of their mention in the text. Tables should be designed to fit into a space of 12 x 19 cm. or less. Figures should be drawn on white paper in

(continued inside back cover)

EDITORIAL

This copy represents our first "real" issue of the EUROPEAN JOURNAL OF PARAPSYCHOLOGY. Since our demonstration copy was issued our editorial staff has been extended to include a number of regional editors. We are proud to announce that we have succeeded in securing the cooperation of such distinguished parapsychologists and experienced authors as Dr. John Beloff and Dr. Hans Bender. We are also very grateful for the qualified help that Dr. Rolf Ejvegard in Sweden, Dr. Erlendur Haraldsson in Iceland, and Dr. John Mischo in W.Germany have promised to render us. We shall continue to work hard extending the editorial staff in such a way that as many European countries as possible will be represented on the editorial staff. It ought however be emphasized, that the regional editors should not in any way be held responsible for the acceptance of contributions for publication which are submitted from authors outside their respective region.

The name: EUROPEAN JOURNAL OF PARAPSYCHOLOGY does not in any way imply that the journal is intended as a mouthpiece for exclusively European contributors. We shall welcome contributions from all corners of the world. We would ask potential contributors to notice that they should send their manuscripts to their regional editor(s) if this is possible in their particular case. If not, they should send their manuscript directly to the address given for this journal.

As far as experimental articles are concerned, we would like to ask potential contributors to try and adhere to the publishing policy which we have outlined in the editorial of the demonstration copy, and which is also discussed at some length in the article: Models of Bias and Control of Bias, in this issue. In short we shall try to avoid selective reporting and yet at the same time we shall try to refrain from making our journal a graveyard for all those studies which did not "turn out". These objectives may be fulfilled by the editorial rule of basing our judgment entirely on our impressions of the quality of the design and methodology of the planned study. The acceptance or rejection of a manuscript should if possible take place prior to the carrying out and the evaluation of the results of the study.

The way in which we view the policy of publishing is rather different from the approach which Dr. J.B. Rhine, the nestor of

experimental parapsychology, has put forward in the Comments section of the Journal of Parapsychology (see The Journal of Parapsychology, June 1975, 135-141). One should be aware that differences of opinion as regards what constitutes a "good" method and a "sound" research programme, are by no means exceptional. No paradigm or opinion in science is beyond criticism. On the contrary, we tend to believe that the growth of scientific knowledge is to a considerable extent dependent on the existence of competing research programmes, or ideals of science. "If two teams, pursuing rival research programmes, compete, the one with the more creative talent is likely to succeed - unless God punishes them with an extreme lack of empirical success", as Imre Lakatos once put it.[1] This is probably true, even acknowledging the risk that human creative imagination is likely to find corroboration and new "facts" supporting a certain view, although very "odd" or "absurd" ideas may be involved.

1) "Criticism and the Growth of Knowledge". Edited by Imry Lakatos and Alan Musgrave. Cambridge University Press, 1970, p. 187.

Martin Johnson

Sybo A. Schouten

EXPLORATORY PK TESTS WITH A PROGRAMMABLE HIGH SPEED RANDOM NUMBER GENERATOR

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INTRODUCTION

The obvious lack of reproducible experiments is in our opinion one of the main reasons for the slow progress in parapsychology. One can maintain certainly that during some experiments with talented subjects results have been obtained which proof beyond any reasonable doubt the existence of psi. However, experiments in which the influence of different variables are studied, are not expected to yield consistent results. Indeed it is not unusual that upon replication some of the effects observed in the first experiment are found to be reversed in the replication experiment.

A method to get reproducible results may be found in large scale experiments, i.e. many subjects and/or many trials. Recent experiments of Levy et. al. (1973) and Schmidt (1973) are examples of this "large scale" direction.

It is remarkable that in both kinds of experiment the interaction is studied between a living entity and a machine. It may be that such a system is most promising for getting reproducible results, as one half of the agent-percipient pair is a machine, which behaviour is known to a larger extent than that of a human being or an animal. If we discuss the reproducibility of experiments we mean that a given experiment should be repeatable in any laboratory, without the need for the same subjects.

The proposal for a theory of psi-phenomena by Walker (1972) is our reason for the invisible versus visible trials in the present experiment, in connection with retro-PK. The theory, which is an extension of quantum mechanics, is in our opinion still incomplete, as it does not give a compelling argument for the proposed association of "hidden variables" connected to different quantum mechanical processes. In the case of PK, these processes are

proposed to take place in the brain of the subject and in the random number generator. However, many aspects of this theory are attractive. Concerning the present work it can be shown that for the relatively simple situation of a PK experiment, trials are expected to be influenced as they are observed, by either subject or checker/experimenter (Feather & Brier, 1968). We will refer to this conjecture as the "observational hypothesis on parapsychological phenomena" (OHPP). We do not go into details here, for the fact of using visible and invisible trials in our experiment can also be interpreted in a more classical way as a search for a differential effect on feedback conditions.

EXPERIMENTAL SETUP

Our apparatus (see figure 1) consists of a random number generator (RNG) which is coupled to a clock pulse generator (CPG), which governs the rate at which the binary random numbers are generated. The random numbers are fed into a minicomputer in the form of pulses on two channels, corresponding with the two alternatives possible. Other peripheral apparatus of the computer are: a magnetic tape drive, a display terminal with a storage CRT and a paper tape reader. The magnetic tape is used to store the frequencies of both alternatives in chosen time intervals. We use the display terminal to give commands to the computer and to display the excentricity of the events for each visible time interval.

This is done in the following way: For each time interval a point is displayed on the screen with the horizontal coordinate dependent on the frequencies of both alternatives. If one alternative occurs more than the other, the point lies in the left half of the screen and viceversa in the right half of the screen. The distance from the vertical middle line indicates the magnitude of the scoring. Subsequent points lie a fixed distance below each other. Immediately after the time interval is finished the point is projected, together with a line segment connecting it to the previous point. Thus the subject sees a broken line, showing the result of each time interval as it develops during a run. The target alternative is indicated by an arrow, displayed in the corresponding half of the screen before the run is started.

The paper tape reader is used to feed the parameters of a series of runs into the computer. Finally, there is a connection between the computer and the CPG by which the computer can switch the frequency of the CPG between two values (in this experiment about 100 and 1000 Hz).

It has to be mentioned that it is difficult to obtain a RNG which shows no deviations from ideal behaviour, that is, $P = .5$, and no dependency between subsequent trials.

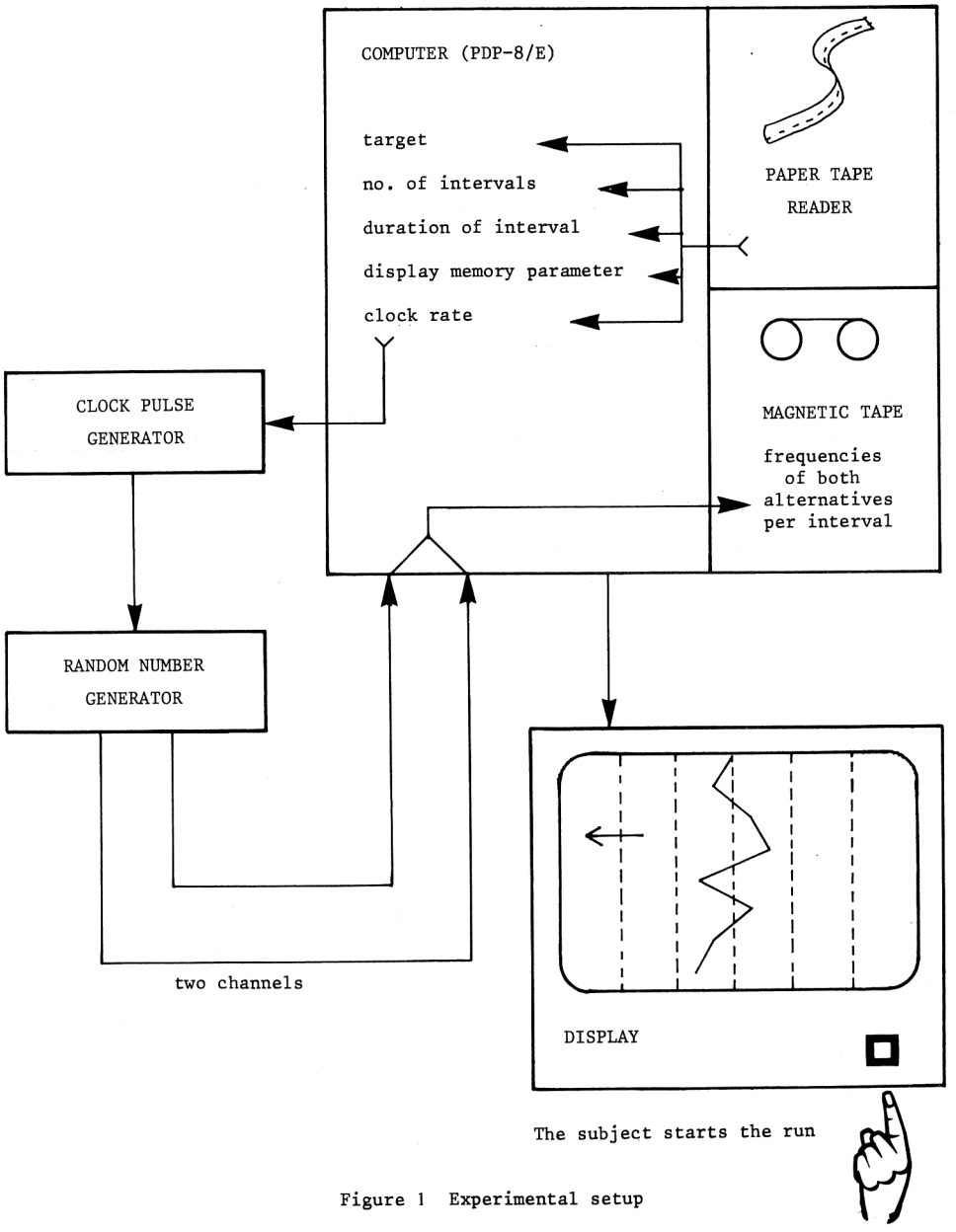

Figure 1 Experimental setup

SOFTWARE AND GENERAL EXPERIMENTAL DESIGN

The variables applied in this experiment are, apart from target direction:

1 Frequency or rate of the RNG
2 Length of the time intervals
3 Number of time intervals per run
4 Display memory decay time
5 Visible versus invisible trials

Ad 2 and 3: By combining the length of the time intervals and the number of them, we obtain a single variable called runlength.
Ad 4: The display memory decay time governs the way in which the subject gets the feedback. The two extreme conditions are a) At any moment the momentaneous performance is displayed, and b) that at any moment the total performance from the start of the run up to the actual moment is displayed (a cumulative way of displaying).

In this experiment some intermediate cases of display memory decay time are chosen. These are determined by the constant A, which we call the display memory parameter. The horizontal deflection on the display screen (X), after M intervals, is proportional to the exponentially decaying critical ratio:

$$X_M = \sum_{I=1}^{M} \{EXP(-A(M-I))(N_{1I} - N_{2I})\} \; / \surd \; \Sigma\{ EXP(-A(M-I))(N_{1I} + N_{2I}) \}$$

where N_{1I}, N_{2I} are the number of pulses on channel 1,2 during the Ith visible interval.
It can be seen that the value of A governs the decay time of the CR.
Ad 5: The invisible and visible trials are obtained by programming alternating invisible and visible intervals. Of both, the results are registered on magnetic tape, but the results of the invisible intervals are not added to the display memory and hence are not shown on the visual display.

Thus far the "real time" experiment has been described, that is, the type of experiment in which the subject is trying to influence events that are being produced by the RNG at the same time. To implement the idea of retro-PK, another program has been written to display the invisible trials registered during a previous series. For the subject this situation could not be discriminated from the real time experiment. The important difference was the direction of the target arrow, which is opposite to the direction in the real time experiment. This is done to be able to distinguish the influence of direct PK in the real time experiment, which should give a positive deviation, from the influence of retro-PK in the

play-back experiment, giving a deviation with the negative sign. It has to be mentioned that interpretations that include psi-missing complicate the picture.

The target side of a run is displayed beforehand by an arrow in the target direction, together with five vertical lines indicating the CR = -4, -2, 0, +2, +4 boundaries. A control run is indicated by the absence of an arrow.

Each run is started by the subject by pressing a key on the keyboard of the display terminal.

The number of runs and the values of the above mentioned parameters are fed on paper tape into the computer before each series. In this way various experiments can be done with our setup.

THE SPECIFIC EXPERIMENTAL DESIGN

The experiment consists of two parts, which we call part A and part B. In part A, invisible trials are just registered and later on analysed, while in part B the invisible trials are displayed later on as just the third part of the experiment (see above).

Part A consists of 18 runs and part B consists of 12 runs. For the values of the parameters of the runs see table 1.

TABLE 1

Values of parameters in the experiment

	Part A				Part B		
Run	Target	Runlength (S)	Frequency (Hz)	Display memory	Runlength (S)	Frequency (Hz)	Display memory
1	control	3	1000	short	4	100	long
2	left	3	1000	long	4	100	short
3	right	3	1000	short	4	100	long
4	control	3	100	long	4	1000	short
5	right	3	100	long	4	1000	short
6	left	3	100	short	4	1000	long
7	control	12	1000	short	9	100	short
8	left	12	1000	long	9	100	short
9	right	12	1000	short	9	100	long
10	control	12	100	long	9	1000	long
11	right	12	100	long	9	1000	short
12	left	12	100	short	9	1000	long

TABLE 1
(continued)

Part A				
Run	Target	Runlength (S)	Frequency (Hz)	Display memory
13	control	6	1000	short
14	left	6	1000	long
15	right	6	1000	short
16	control	6	100	long
17	right	6	100	long
18	left	6	100	short

Note: The actual frequencies in this experiment are 956 Hz and 93 Hz. The number of intervals per run is in part A: 60, of which 30 visible and in part B: 20, of which 10 intervals are visible. The values of the display memory parameter A are: Display memory long (more cumulative display) Part A: 0.1336; Part B: 0.2878. Display memory short (more momentaneous display) Part A: 0.2878; Part B: 0.6931.

Fourteen subjects, members of the study centre for experimental parapsychology, volunteered in this experiment. Subjects 1 to 8 were tested in the morning session, subjects 9 to 14 in the evening. During the evening session no slow RNG frequency was used due to a disconnection of the cable between the computer and the clock pulse generator.

RESULTS

Only the runs with a target direction will be analysed. The control runs can be incorporated as a separate target condition in an analysis of variance, which will be called for in further experimentation, but in the present exploratory phase a simple straightforward analysis will suffice. The overall result per subject, split for visibility, is given in table 2. In the tables, one sided probabilities are given. Of the 56 independent deviations in this table, 38 are in the target direction. The results are also scored in another way. The runs no. 2 and 3, 5 and 6, etc. are considered as units. A unit is a hit if over both runs together there are more trials in the target direction than in the nontarget

TABLE 2

Results per subject split for visibility and total results

	Part A			Part B		
Subject	Total no. of trials	Deviation Vis.	Deviation Invis.	Total no. of trials	Deviation Vis.	Deviation Invis.
1	43958	+148	-8	27225	+71	+178
2	43995	+226	+89	27245	+134	+141
3	44021	+22	-301	27270	-19	+123
4	44067	+70	-125	27281	-49	+38
5	44097	+30	+117	27274	+40	-66
6	44083	+236	-243	27290	+97	-91
7	44105	-69	+12	27290	+164	-190
8	44091	+6	+133	27304	-112	+30
9	80236	+122	-64	49669	+79	+166
10	80267	-91	+32	49661	+23	-176
11	80285	-276	+437	49688	+44	+86
12	80294	+132	+106	49679	-157	-22
13	80302	+50	-224	49725	+69	+62
14	80342	+293	+61	49739	+252	+429
Total	834143	+899	+22	516340	+636	+708

	No. of trials	Deviation	CR	P
Vis.	675241	+1535	1.86	.032
Invis.	675242	+730	0.89	.19
Total	1350483	+2265	1.95	.026

Note: We give deviations in tables 2 and 4 as the number of trials in the target direction (hits) minus the number of trials in the non-target direction (misses). The number thus obtained, is exactly twice the usual deviation, the number of hits minus the chance expectation of the number of hits.

TABLE 3

Runpair scoring

Feedback	No. of runpairs	Hits	Misses	Rate	CR	P
Visible	140	86	53	61.4%	2.80	.003
Invisible	140	79	61	56.4%	1.52	.071
Total	280	165	114	58.9%	3.05	.0013

TABLE 4

Scoring for different conditions

Variable	Value	Total no. of trials	Deviation Vis.	Invis.	Total
Frequency	low	50672	+174	-18	+156
	high	1299811	+1361	+748	+2109'
Runlength (sec)	3	119160	-245	+405'	+160
	4	158880	+532'	+274	+806'
	6	238320	+1065'	-127	+938'
	9	357482	+104	+434	+538
	12	476641	+79	-256	-177
Display memory	long	675241	+849	-108	+741
	short	675242	+686	+838	+1524'

Note: ' indicates P less .05

TABLE 5

Efficiency ($1000 \times CR^2$/duration) in experiments with visual feedback

	Frequency	Efficiency
Present study	100	4.29
	1000	4.20
Schmidt(1973)	30	42
	300	15

direction. We give the result of scoring per run-pair in table 3. In both table 2 and 3 the visible trials score higher than the invisible trials, but the difference is not significant.

Finally we give the results split for RNG-frequency, runlength, visibility and display memory parameter in table 4.

CHECKS ON THE RANDOMNESS OF THE RNG

Before and after the morning and evening sessions long runs without a subject were checked on randomness of the RNG. We tested our RNG for zeroth, first and second order effect, that is, bias, alternating and "one skipping alternating". The results of these checks were consistent with a previous series of tests in which we found on a total of 101211635 trials that there are slight deviations from ideal behaviour in our RNG: P = .50037 (bias is .074 percent) and excess alternating over persisting is .153 percent. One skipping alternating shows no detectable deviation from chance. The first effect is compensated for by balancing the number of trials for each target side. The second effect gives rise to a slightly smaller variance of the results, so that the critical ratios will be slightly conservative when applying the standard tests.

DISCUSSION

The first result to be discussed is the overall result. In table 2 and 3 we find one-sided probabilities of .026 and .0013 so that we can reject the null hypothesis. Remarkable is the (non-significant) enhancement of the significance when we use only the sign of the deviation in the target-direction per run-pair. Analogous effects have been observed in a number of other

experiments (Brier & Tyminski, 1970; Houtkooper, 1967).

The difference between the visible and the invisible trials is important for the experimental verification of the OHPP. However, we consider the present experiment rather as an exploratory investigation than as a crucial test, for the resultant direction of the "will" of the subject (psi-hitting or psi-missing) cannot yet be predicted with any reliability.

The results of the invisible trials of part A were only observed by the two experimenters. Their "will" was probably directed towards enhancement of the deviations in the target-direction. According to the OHPP, the effect in the invisible trials of part A is caused by the experimenters, however the effect is negligible (see table 2). On the other hand the invisible trials of part B were observed later by the subjects themselves, but their "will" was directed opposite to the direction during production of the same run. So the question can be put: "What influence on the invisible trials is greater, the (labeled) positive influence during the production of the run or the negative influence during the play-back?". We infer that the positive influence is predicted by the straightforward classical explanation (as we call it) while the negative influence is predicted straightforwardly by the OHPP.

As the outcome of the invisible trials of part B is slightly positive (CR = 1.39), the classical explanation is very slightly favored over the OHPP. Of course, explanations considering OHPP with psi-missing can be put forward, but this stretches the imagination. On the other hand, when considering a classical explanation, one has to take into account that the subjects were not aware of the invisible trials at the time of the experiment.

The data in table 4 give no significant effect of RNG-frequency, runlength, visibility or display memory parameter. We notice that visible trials are on itself significant (P = .032), while invisible are not (P = .19). As for runlength we observe that the visible trials at a runlength of 6 seconds are significant (CR = 3.09; P = .001). The nonsignificant scoring on the long runlengths of 9 and 12 seconds is consistent with the results found in our pilot studies. In these, we found that of runlengths of 3, 4.5, 13.5 and 42 seconds, only those of 4.5 seconds showed a significant deviation (P = .01).

We have calculated the efficiency of this experiment (see table 5). If we compare it to the efficiency figures obtained in Schmidt's experiment (1973) our efficiency seems rather low. In the first place Schmidt uses selected subjects in his experiment, about which he does not indicate the criteria on which selection takes place. That causes that his experiment cannot be exactly

replicated at another place in the world. For this reason we did no selection on our subjects.

Schmidt does not take into account the time invested in the selection of his subjects. Furthermore his setup was such that experimenter and apparatus should be available any time the subjects considered themselves capable of producing good runs. In our setup the whole experiment took 7 hours including introduction of the subjects to the experiment. As we took for each subject just half an hour, introduction and atmosphere were rather unpersonal and hurried. It is our feeling that a further study with subjects 2,3 and 14 may enhance the efficiency to a level comparable to that found by Schmidt. However, if this selection remains necessary the duration term in a honest efficiency calculation should take into account this fact, so that other laboratories that plan to replicate indeed get a good impression in how much time they can expect to obtain a significant result. Maybe it is sensible to avoid attaching too much meaning to efficiency in one sense or another before it has been shown to be a reliable measure. Meanwhile it is juggling with the few parameters available like trial rate, duration and obtained CR, while wishing to increase the last.

ABSTRACT

An experiment has been done on PK with an electronic random number generator coupled to a minicomputer. The task of the subject was to influence the RNG and to raise the frequency of the target alternative. Feedback was visible. Fourteen unselected subjects participated in the experiment.

The overall result is significant, having a one sided probability P = .026 when using the trial as a unit and P = .0013 when using only the sign of the deviation per run-pair. The variable conditions in the experiment are: RNG frequency (about 100 and 1000 Hz), runlength (3 to 12 seconds) and display characteristics. Short runs give considerable contributions to the overall effect, while long runs (9 and 12 seconds) contribute only slightly. This is consistent with pilot studies. Results are discussed in the light of a new theoretical approach, which amounts to an extension of quantum mechanics.

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AN ATTEMPTED CONFIRMATION OF THE RODENT ESP FINDINGS WITH POSITIVE REINFORCEMENT

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INTRODUCTION

Since the early experiments of Duval and Montredon (1968) considerable evidence has been accumulating to indicate that rodents are able to determine to a small extent but significantly above chance expectation, which option in a binary choice situation circumvents their receiving an aversive stimulus. Much of the work in this area has been carried out by W.J.Levy and his co-workers at the Institute for Parapsychology.

After the first replication of the Duval and Montredon work (Levy, Mayo, Andre & McRae, 1971) successive experiments examined the effect of a number of variables and thus established more sensitive criteria for evaluating the influence of psi (Levy & McRae, 1971; Levy, 1972; Levy, Davis & Mayo, 1973; Levy, Terry & Davis, 1973). A major feature of the later work was the introduction of automated techniques to minimize experimenter influence and control extraneous variables as much as possible.

One limitation of this work was that in all cases negative reinforcement was used, and it remained to be seen if animals could be induced to use psi to obtain rewards. Schouten (1972) used positive reinforcement with mice but was able to obtain only marginally significant results in the two conditions which he used. However, recently Parker (1974), working here at the University of Edinburgh, conducted an experiment with gerbils using positive reinforcement. In this experiment gerbils were reinforced with sunflower seeds for correct responses and a success rate of 52.8% ($P = .004$) was obtained for the total results. Random behavior trials yielded an even higher success rate of 55.0% ($P = .022$). This was interpreted as a confirmation of the utility of the RBT

criterion as well as an indication of the ability of animals to use psi in other than a negative reinforcement situation.

With results such as these obtained, the present experimenters were anxious to replicate this experiment (using essentially the same equipment) and extend its scope to cover such factors, suggested in the previous experiment, as individual differences between animals and possible experimenter effects. Consequently it was decided to initiate a new experiment which would consist of a pilot followed by a main study, the latter, hopefully, serving to confirm tendencies noticed in the former.

The automation of the experiments using negative reinforcement has gained the methodological advantage of reducing greatly the influence of the experimenter on the psi abilities of the animals. However, this has reduced the amount of information obtained on what influences there may be. Since nothing comparable to the sophisticated animal testing equipment of the Institute for Parapsychology was readily available to the present experimenters it was decided to take advantage of the necessity to handle each animal in placing it in the test box, removing it, and feeding it by alternating experimenters and examining the data for differential influences. Also, since the data from the Parker experiment suggested differences in the animals' psi abilities it was decided to look for these.

The advantages of using positive reinforcement as perhaps a more "natural" and psi-conductive state have been discussed by Schouten (1972) and Parker (1974). While it remains a matter of opinion as to whether the positive reinforcement paradigm (simulating food gathering at the animal's own pace) or the negative reinforcement paradigm (simulating escape from harm at the predator's pace) is a closer approximation to the natural environment in which psi may operate, the former paradigm, in the present experimental set-up, largely destroys the behavioral basis for the random behavior trials. In the negative reinforcement situation the RBT is clear-cut in that the animal changes sides for trial n after receiving no shock or after the five-second shock period has ended on trial n-1, i.e. the animal changes sides for no apparent reason. In positive reinforcement studies the RBT is defined as when after receiving a reward on trial n-1 the animal goes to the opposite key. The RBT concept was introduced by Duval and Montredon (1968) as a means of eliminating "static" behavior since it seemed likely that psi was not operating during these periods. In the positive reinforcement situations used so far the animal is forced to move in order to obtain a reward. In Schouten (1972), Parker (1974), and the present experiment the animal is required to move to another side of the test box to secure the reward if the key press

is correct. Thus, in this situation, the RBT is not an instance of overcoming static behavior but rather of overcoming the bias toward a key caused by the instance of receiving an operant conditioning reward on trial n-1.

It seems unlikely to the present experimenters that these two RBT conditions are equivalent. The Parker experiment did obtain significant above chance scoring for the total results in addition to the random behavior trials, however. The same was suggested by Schouten in his positive reinforcement experiment although few of the negative reinforcement experiments have been able to achieve total significance. It should be noted that in using the RBT criterion both of the previous experiments confirmed higher scoring rates for that condition. Consequently in the present experiment it was decided to examine RBT's according to the established criterion but the authors were unwilling to predict higher scoring for these trials.

The concept of the RBT itself has been questioned, whether in the negative or positive reinforcement paradigms. Schouten (1968) points out that this behavior is not truly "random" and describes the criterion as "rather arbitrary". One of the present experimenters (B.M.) has shown (Appendix A) that since RBT's are susceptible to optional stopping on the part of the animal the usual data analysis may overestimate the significance of the result. He points out that the number of RBT's must be specified in advance in order to avoid this overestimation. This general critique of the RBT criterion provided further reason for seeking only a total significance in the present experiment.

In most of the previous experiments a random number generator was used to determine the binary target sequence; however, for this experiment a satisfactory random number generator which could be coupled with the existing logic circuitry was not readily available. In the present instance random sequences from prepared random tables were coded onto paper tape and used to control the test situation.

While it can be argued that because of this the experiment is not strictly one of precognition but GESP since the tape sequences were preexisting (though, as will be seen, the animals could get no sensory cues from the equipment as to the coming targets) the use of prepared tapes does effectively eliminate the possibility of animal PK on the RNG as one source of the higher incidence of hits. This possibility can not be considered remote in the light of recent findings by Schmidt and Levy (Schmidt, 1970; Levy & Andre, 1970; Levy, 1971).

PILOT STUDY

APPARATUS

The apparatus used in this experiment is essentially that used in the Parker (1974) work. Some modifications were introduced between the pilot study and the main study and these will be noted below.

For the pilot study the test box was a standard operant conditioning cage measuring 38x38x38 cm. Three walls were opaque and the fourth was made of plexiglass. The hinged lid was made of darkened translucent plexiglass. On the wall opposite the transparent side there were two keys 2.5 cm in diameter, with centers 3.8 cm above the wire mesh floor and 15 cm apart center to center. The keys were made of translucent plastic illuminated from behind, red for left and green for right, and were operated by the animal's nose. Between the two keys was a small white light to illuminate the cage.

Mounted on the wall to the left of the keys was a standard grain feeder filled with sunflower seeds. Its operation cycle of eight seconds allowed the animal access to the seeds long enough to secure and consume one seed.

The logic circuitry controlling the test situation followed this contingency paradigm: "A press on the key initiates the trial. Information as to which key is pressed is stored and the controlling tape advanced by one position in the reader. At the end of a three-second waiting period the stored information is compared with the tape output. If it is found to be the same, i.e. a "hit", the feeder cycle is initiated. At the end of the feeder cycle the trial is completed and the animal is free to make another key press. If the stored information does not agree with the tape output, i.e. a "miss", the trial ceases and the animal is free to guess again. Any key presses made during either the three-second 'incorrect' trial or the eleven-second 'correct' trial are recorded on the event recorder but do not advance the tape nor alter the information in the circuitry."

The controlling tapes were prepared by punching in column one of the paper tape a sequence of holes or spaces based on whether a digit was odd or even in sequences taken from random number tables according to the procedure set out in Appendix B.

A twelve-channel event recorder running at a speed of 1.3 cm per minute recorded right responses, left responses, the operation of the feeder cycle, and whether the output of the tape for the trial had indicated the right or left key as the target.

SUBJECTS

Subjects were five Mongolian gerbils (Meriones unguiculatus), also called clawed jirds. Three were male and two were female. Two of the animals, gerbil one and gerbil five, were veterans of the Parker series. The rest were new to the test situation. All of the animals were mature.

HOUSING

All of the animals were housed individually in plastic cages with wire tops. Wood chip bedding was provided as well as a small sheet metal house in which the animals could make a nest. Water was available all the time and food was administered according to the schedule outlined in the following section of this paper. Lighting was on a 12-hour light and dark cycle with the light beginning shortly before the test sessions.

METHOD

Prior to testing, the three new animals had to be trained in the apparatus and the two veterans were given a refresher course. The procedure consisted of first training them to press either key for a reward, then, by manually controlling the distribution of rewards they were gradually "broken in" to the situation where not all guesses would produce rewards. When they were accustomed to about a 50% probability of success they were given further training on random tapes as well as an occasional shaping session to correct biases.

The animals' weights, and presumably their hunger, were carefully controlled in the following manner. After training their weights were gradually reduced until they were efficiently performing the number of trials per session required by the experiment. The animals' weights were held at that point by providing measured amounts (different for each animal) of a proprietary gerbil mixture at the end of each session to supplement the quantity of seeds received. Thus the animals were maintained at what appeared to be an optimal motivation level since observation of the animals' behavior had shown that when they were too hungry they quickly became frustrated by incorrect responses and resorted to continuous banging on one key whereas if they were not hungry enough they quickly became bored and ignored the reward altogether.

When the now trained animals reached the optimal weights and were stabilized there, the formal part of the pilot study began.

The number of trials for each animal per session was set at 80. A total of 2400 trials for the whole study was set giving six days of 400 trials each.

It was decided to alternate experimenters for the daily sessions and a toss of a coin determined which of the experimenters took the first session. On several afternoons prior to the start of the trial sessions the experimenters prepared the random tapes for their respective sessions. These tapes were checked against the original sequences and stored in a locked cabinet until required. The procedure for determining the sequences of random numbers is described in Appendix B.

Testing began at the same time each day for six consecutive days. The assigned experimenter arrived at the testing room at 9:30 am. The equipment was warmed up and the animals greeted. The animals were run in order of ascending number. The procedure for each animal was as follows: "Insert the random tape at the starting point and annotate the recording chart. Clean the test box and check the seed supply in the hopper. Remove the animal from its housing cage. Weigh the animal. Start the recording apparatus and place the animal in the test box." It was then free to initiate the session with its first response. At the conclusion of 80 trials the recording apparatus was switched off and the animal removed. When all the animals had been tested the supplementary food allotments were distributed and the experimenter left the room. The experimenter was present in the room during the entire session and was generally quiet and unobtrusive. He was unaware of the tape sequence as it was read by the machine.

The pilot study was completed on January 24, 1974. Each experimenter kept his own data and none were scored or even examined until the conclusion of the study.

SCORING

The records were scored in the following manner: Since the number of trials per animal per session was fixed the records were first checked against the tapes to see that they read correctly and the required number of trials were performed. The number of reward cycles was counted indicating the number of hits. Then the records of the animals' sequence of responses were examined to determine the RBT score according to the existing criterion. All the scoring was independently carried out on all the data by each experimenter. Any discrepancies were rechecked until agreement was reached.

(In the pilot study two of the records revealed that a machine malfunction had caused the number of trials to fall short. Since the computer program was prepared for fixed trial sessions it was

decided to increase the hits score by the MCE of the number of trials that the session was short. In one case it was 3 short and the other 1 short so the scores were increased by 1.5 and .5 respectively. This accounts for the curious result in the tables of having .5 of a hit).

RESULTS

It was decided before the start of the test sessions that several analyses would be made. These were for overall significance, significance of the results for each experimenter, and of the results for each animal. Also it was planned to perform a preliminary two-way analysis of variance to examine the variance between animals and the variance between days to suggest possible predictions for the main experiment regarding animal differences and experimenter effect. The bulk of the data was analysed on the university's ICL 4-75 computer with some additional analysis performed on the departmental Olivetti 101. The program was designed to calculate the scoring rates and C.R.'s for several other conditions such as each day's scoring but for the pilot these were considered post hoc analyses.

The results of the pilot study are summarized in Table 1. As can be seen the overall results for the study are very close to the chance level with a scoring rate of 50.46%. A curious, but totally nonsignificant effect was noticed with the scores obtained by each experimenter in that B.M. obtained an overall scoring rate slightly below chance (48.79%) and R.B. above chance (52.12%). Of course, when combined they yielded the near chance results of the total.

The two-way analysis of variance did not, however, confirm this suggestion of a difference between experimenters. The results gave a difference between gerbils variance ratio of F=3.184 (df=4/20, P.LT..05) and a difference between days of F=1.565 (df=5/20, n.s.). This suggested that the variance was concentrated in the differences between animals rather than between days, the latter being where the greater variance would have been expected had an experimenter effect been present.

In order to explore this effect further the same analysis was performed on the scores obtained by each experimenter. It was found that for experimenter B.M. both the animal variance and the day variance yielded no significant differences. For experimenter R.B. it was found that the variance between the animals, F=4.500 (df= 4/8, P.LT..05), yielded a significant effect while the day variance was not significant.

The only evidence of any significant above chance scoring came from gerbil nine who achieved a C.R. of 2.237 (P=.012, one-tailed).

TABLE 1

Breakdown of the results from the pilot study

	Total Data				RBT Data			
	Trials	Hits	Rate	C.R.	Trials	Hits	Rate	C.R.
Total Experiment	2400	1211	50.46	0.429	433	206	47.58	-0.961
Experimenter B.M.	1200	585.5	48.79	-0.808	209	96	45.93	-1.106
Experimenter R.B.	1200	625.5	52.12	1.443	224	110	49.11	-0.200
Gerbil One	480	238	49.58	-0.137	100	45	45.00	-0.900
Gerbil Five	480	228	47.50	-1.050	60	24	40.00	-1.420
Gerbil Seven	480	229.5	47.81	-0.913	71	30	42.25	-1.186
Gerbil Nine	480	265	55.21	2.237	81	42	51.58	0.222
Gerbil Ten	480	250.5	52.19	0.913	121	65	53.72	0.727

Random behavior trials were evaluated according to the criterion set forth in the introduction to this paper. They were found to constitute 17% of the total trials and are presented along with the other data in Table 1. As can be seen in this case the RBT's yielded a substantially lower scoring rate than did the total trials.

INTERIM

While the general lack of any significant above chance scoring was disappointing it seemed worthwhile to continue with a longer main study to see if any of the tendencies noticed in the pilot study might be confirmed. It was hoped that if any of the effects which were suggested by the pilot were found to be consistent and significant in a replication then these findings might go a long way to accounting for some of the difficulties in replicating psi experiments.

After the conclusion of the pilot study the experimenters began informally but systematically varying different parameters of the experiment such as the hunger of the animals, time of day for running, etc.. None of these seemed to make any noticeable difference in the performance of the animals.

During this period several modifications were made on the test box as well as in the animals' housing and these will be noted below. Shortly after the pilot study was concluded gerbil nine, the only animal which achieved significant above chance scoring, died.

MAIN STUDY

APPARATUS

Several modifications were introduced into the test box for the main study. The plexiglass wall was replaced by a metal wall and the feeder unit was remounted centrally opposite the keys so the animal would have to traverse the length of the cage for a reward and after consuming it would be roughly equidistant from both keys. The red light was changed to green so now both keys were illuminated with the same color light. This was done to help eliminate side preferences and no reduction in discriminatory ability was observed. Two electro-magnetic counters were placed in the circuitry to count hits and total trials. These were to act as a double check on the pen records of each session. The feeding cycle was reduced by two seconds since it was noticed that some

of the animals could regularly get two seeds out in the eight-second period. The remaining apparatus remained unchanged from the pilot study.

SUBJECTS

The animals were the same as for the pilot except that there were only four, number nine having died.

HOUSING

Housing remained essentially the same except that the animals were given a more enriched environment. Small animal toys, wooden blocks for gnawing, and pieces of burlap for nestmaking were introduced. Cage cleaning was increased to every third day and was done in the evening.

METHOD

This was also essentially the same as in the pilot study.The animals, of course, needed no training this time. For the main study it was decided to reduce the number of trials for each animal per session to 50 since observations during the pilot suggested that 80 trials might be too long, allowing some animals to lose interest before the end of the session. The total number of trials would be 3200 spread over 16 days with 200 trials each day. The main experiment was concluded on March 23, 1974.

RESULTS

The same statistical tests which were specified for the pilot study were to be applied to the data of the main study and the additional two-way analysis of variance on each experimenter's scores would be considered as part of the prespecified tests.

The main study results are summarized in Table 2. As can be seen the scoring rate for the main study (50.46%) is almost identical to the near chance scoring rate for the pilot (50.44%). The results for each experimenter maintained the same direction in their scoring rates but the magnitude of the difference between them was less than in the pilot. Experimenter B.M.'s results yielded a scoring rate of 49.81% and the experimenter R.B.'s a scoring rate of 51.06%. The scores of the individual animals displayed no consistent relationship to their previous scores.

The two-way analysis of variance for the total results yielded no significant differences for either animals or days, thereby

TABLE 2

Breakdown of the results from the main study

	Total Data				RBT Data			
	Trials	Hits	Rate	C.R.	Trials	Hits	Rate	C.R.
Total Experiment	3200	1614	50.44	0.477	701	361	51.50	0.755
Experimenter R.B.	1600	817	51.06	0.825	355	192	54.08	1.486
Experimenter B.M.	1600	797	49.81	-0.125	346	169	48.84	-0.376
Gerbil One	800	412	51.50	0.813	185	89	48.11	-0.441
Gerbil Five	800	405	50.62	0.318	174	90	51.72	0.379
Gerbil Seven	800	392	49.00	-0.530	157	90	57.32	1.756
Gerbil Ten	800	405	50.62	0.318	185	92	49.73	0.000

failing to confirm the suggestion of differences between animals found in the pilot. When applied to the scores of each experimenter no significant differences were found except in the case of experimenter R.B. where the variance ratio for the days was F=3.051 (df=7/21, P.LT..05). This was a reversal of the findings of the pilot.

Random behavior trials were analysed and the results are presented along with the other data in Table 2. They amounted to 22% of the total trials and in this case yielded a slightly higher scoring rate than did the total trials, also reversing the findings of the pilot.

DISCUSSION

The most obvious finding of this experimental series was the total lack of evidence of any above chance scoring by the animals. In light of the previous experiments reported these results are hard to explain. It is not possible either, to use the random behavior criterion to sort out the trials in which psi may not have been operating since the only result to receive strong confirmation was the overall scoring rate and it was found that the RBT's gave opposing results in the two studies. This last result suggests that the RBT criterion may not be applicable to the particular experimental set up used in the present studies and further work will be required to test its applicability to positive reinforcement paradigms in general.

There is little in the research published so far which might offer suggestions as to why this experiment failed to obtain any significant above chance scoring. There are some possible explanations which might apply to the present experiment but they are only suggestions and will remain unsupported until such time as corroborating evidence is found in similar cases. In the present experiment the animals were several years old and it is conceivable that this might have affected their scoring ability. It is possible that the presence of the experimenter may have inhibited psi generally. Perhaps during the training period the animals became too accustomed to the nature of the randomly distributed rewards. It is also possible that the careful control of the animals' weights may have worked against the experiment in inducing the animals to accept the fact that they would not starve no matter how they performed in the test situation thereby reducing the motivation to use psi. It must be remembered, though, that these are only suggested possibilities and that further information from experiments failing to obtain significant scoring is needed before any of these can be taken seriously.

It is most probably the case that there are variables in the operation or non-operation of psi in animals which are not at all evident in the research so far. This experiment was designed to look for certain factors which may have an influence on psi functioning, such as differences between animals as the work by Parker (1974) and Osis and Foster (1953) suggest, or the different influences which the experimenter conducting the experiment might have on the animals.

The results obtained in this experiment are interesting, but unfortunately not at all strong enough nor even consistent enough to allow a coherent interpretation.

It is worth noting that the direction of the scoring rate for each experimenter remained unchanged although there was a much smaller difference between these rates in the main study.

The finding in the pilot study that there may be significant differences in the psi abilities of the individual animals, especially with experimenter R.B., was not confirmed in the main study. In fact, with experimenter R.B.'s data for the main study a significant difference in days rather than animals was found at about the same level of confidence. 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 differences and day differences than did the other experimenter.

What, then, is the possible interpretation of this experiment? Are individual animal differences important factors in psi ability, or is the experimenter an important factor, or even the particular day? Unfortunately, from the results of this series it is impossible to determine. Some or all of these factors may be important, but it is equally probable that all these results are simply chance fluctuations of the data.

Because of the inherent interest of positive reinforcement in the animal experiments it may be helpful to future researchers to make a few comments on the practical disadvantages of the present experimental set-up which have been observed during the course of this series.

1) The RBT criterion is rendered suspect (as explained above) by the necessity for the animal to move away from the response key in order to obtain the reward.

2) A lengthy training period is required for the animals to learn to respond appropriately.

3) Animals tend to develop marked side biases which must be removed by extra training. There is little to cause the animal to move, since even if he does respond incorrectly, he only needs to wait a few seconds before having another try, whereas with the

electric shock he will tend to move in response to the current, thus automatically obviating long sequences of responses on one side.

4) The level of hunger varies from the beginning of the session when the animal is at the hungriest to the end. The response to shock, however, appears to be fairly constant. While little is known of the effects of motivation in these experiments it seems to the present experimenters that the available evidence suggests a Yerkes-Dodson curve, with a peak of ESP scoring at a level of motivation considerably below that found to be optimal in most psychological experiments. This is suggested by, inter alia, the failure of Schmidt to obtain overall significance when using a high shock level and the findings of Levy that the animal scores best when the previous trial did not result in shock and when there are few intertrial jumps (Levy, 1972).

5) Ethical objections. It has frequently been contended in the British literature that the use of mild electrical shock is objectionable, whereas having the animal work for its breakfast is not. Under the conditions of the present experiment, however, it was necessary to maintain the animals under conditions of food restriction for an extended period to produce the required behavior. The choice of the method on ethical grounds is therefore not as clear-cut as has often been represented.

Of these considerations points (1) and (2) seem most crucial. One method suggested for consideration is to employ two reward units integral with the two response units, thus maintaining the RBT criterion intact. The response unit could consist of a headsized hole giving access to the feedback apparatus. Across the hole is a light beam, the interruption of which by the animal's head constitutes the response. The animal is not therefore required to learn a novel response such as pressing a key but simply to modify its inherent exploratory behavior. This should be accomplished very quickly.

One may wonder why an experiment such as this with a singular lack of evidence for psi and contradictory results in other areas even finds its way into print. It must be remembered that this experiment was undertaken as a carefully controlled replication and extension of previously reported findings.It was, unfortunately, a clear case of a failure to replicate. This may still be regarded as an extension of the previous work in that it demonstrates that before the ideal of simple replicability is achieved here it will be necessary to examine in detail the many factors which may have an influence on the psi ability of animals. The results obtained by the present experimenters were contradictory and difficult to reconcile with previous work. Yet they were obtained in a carefully

conducted experiment and must be taken into account.

John Beloff (1973) has suggested that parapsychologists are in danger of misleading themselves as to the true replicability of an experiment since failures to replicate simply do not find their way into the literature for one reason or another. The publication of this paper should serve as an answer to that warning and should set an example that the scientific approach to a failure to replicate is not to ignore or disbelieve it but to take the next step and design work to find out why.

APPENDIX A

A Critique of the Current Use of the RBT Criterion in the Rodent Precognition Work

In general in the published rodent work the results from the total data do not differ significantly from chance. The whole of the scoring is concentrated in the RBT data. Since the number of RB trials is not pre-specified, although the total number of trials is, the RBT data are subject to optional stopping (OS) by the animals. This on first sight appears to be a completely frivolous objection. It is not, of course, suggested that an animal may consciously employ an OS strategy to bolster his score but that the feedback situation might automatically, through another variable such as activity, affect the frequency of RB trials. Regardless, however, of the validity of the model to be presented the general point stands. In any experimental work, the number of trials of the type of interest must be pre-specified otherwise they are subject to the possibility of OS.

Before discussing the proposed model specifically, OS will be briefly discussed in more general terms. Consider the run as the basic unit of the animal experiments. It is clear that in each run of 25 total trials the animal may, in theory, make anywhere from 0 to 25 RB trials, this giving considerable latitude for OS. The basic paradigm of OS is 'Stop when the cumulative score is above MCE'. Although this paradigm is concerned with stopping when ahead, such behavior does not, in fact, alter the MCE (over a large number of runs). It is difficult to realize this at first sight. It does seem very reasonable that such a paradigm could only lead to above chance scoring. However, on evaluating the effect it is found that those runs where the subject has performed the maximum of 25 allowed guesses without having been at any point above MCE

and who has therefore been left at that point with, on average, a negative deviation, exactly balance on the basis of trials, the occasions where a positive score is obtained by OS. To make this clearer, consider Figure 1 which is a much simplified worked example. This employs a run of three trials instead of 25 and uses the paradigm 'Stop when above MCE, otherwise continue'. It should be noted that the following general comments are true not only for $P=\frac{1}{2}$ but for any P. Figure 1 is a tabulation of all possible outcomes. It can be seen that the MCE is $\frac{1}{2}$ here in the OS case, the same as in the usual situation. However, consider the probability of success i.e. scoring above MCE on the basis of runs:

FIGURE 1

Tabulation of the possible outcomes of a hypothetical three-trial run when the animal employs a "Quit when above MCE" OS strategy regarding RBT's

Sequence			RB Hits	RB Trials	
0	0	0	0	3	
0	0	1	1	3	
0	1	0	1	3	0 = Miss
0	1	1	2	3	1 = Hit
1'	0	0	1	1	' = OS of RBT's
1'	0	1	1	1	at this point.
1'	1	0	1	1	$P = \frac{1}{2}$
1'	1	1	1	1	
Totals			8	16	

For three trials OS P(success) = 5/8 (63%). This probability increases with the number of trials of OS, thus e.g.: For five trial OS P(success) = 11/16 (69%); For nine trial OS P(success) = 193/256 (75%). It will be seen that this is in accordance with the statement above that the effect of OS is not to alter the MCE. Instead, the probability of scoring a given amount above MCE is increased for any given run. This is achieved at the expense of having fewer trials in the run. It is as if the overall effect of

OS were to increase the variance (in a unilateral fashion) from the theoretical value. It now becomes clear why it is that, as is well known, in experiments subject to OS, the probability of a given outcome calculated in the usual manner is an underestimate of the true probability, perhaps a gross underestimate. Thus the result of the OS manoeuvre is to render a critical ratio of say 3 or 4, calculated in the usual fashion, actually not at all significant. Another way of looking at OS is to note that it allows the subject more than one attempt to get a good score.

An experiment, of course, consists not of just one run, but of many (perhaps 100). The total score however, is affected by OS in the same way as are the individual runs comprising it, although the magnitude of the effect is greater; so the remarks above apply mutatis mutandis to the whole experiment.

ONE MODEL FOR ANIMAL OS

The frequency of RB trials can be regarded as a measure of optimum activity. As activity becomes low the animals will tend to stop changing sides at all and thus tend to stop making RB trials. (Also, as activity increases too far the animals will tend to change sides during shock, thus violating the RBT criterion, again tending to stop them making RB trials.) Only the scoring of a miss, i.e. suffering a shock, will have any effect on the animals' behavior. A hit (no shock) will have no effect of its own. Shocks tend to increase the animal's activity. Then, if the animal starts off missing overall on total trials, i.e. being shocked, he will tend to become more active and make more RB trials, so that almost all his trials at this time are RB trials.

If he starts succeeding on these RB trials overall, i.e. suffering few shocks, his activity will fall and he will tend to stop making RB trials. Once he stops it may be assumed that he will tend to not restart making RB trials even on striking a negative scoring patch in total trials, since by this time he will be habituated to the situation.

It will be seen that the above is effectively the typical OS paradigm: 'Start off missing; continue guessing until the score is above MCE; then stop'. Many models of this general type can be constructed and depending on the exact specification of the parameters, the secondary findings of the animal work can also be accommodated. Similar arguments can be advanced for the positive feedback situation, involving food satiation.

Nonetheless it seems unlikely that the results obtained in the American work can reasonably be attributed to OS for two main reasons:

1) Although in theory OS behavior is quite feasible for the

animals it does not look as if they have actually 'taken advantage' of this since the proportion of RB trials is quite constant from experiment to experiment; so that in fact the number of RB trials is fairly closely pre-specified by pre-specifying the total number of trials. In one instance (Levy & McRae, 1971) the number of RB trials was almost completely pre-specified, the final results being evaluated for only eight more than this number of RB trials. The results were significant and of a similar rate to the other studies.

2) It seems unlikely, even on the most generous assumption of the amount of OS attributable to the animals, that the result would be to overestimate the overall significance by a factor of more than say 5. Even on this basis the Levy results still remain statistically significant.

The OS hypothesis is susceptible to test on data already in existence, by evaluating the data for the previous experiments out of say 35% of the pre-specified number of total trials, discarding for this purpose any experiment where less than this number of RB trials was performed.

CONCLUSION

While the OS hypothesis is improbable, sufficient reason has been shown, in the authors' opinion, that this possibility should be eliminated in future work by pre-specifying the total number of RB trials. This criticism, however, is a general one and applies to all cases where decisions are made by an organism under test in feedback conditions.

APPENDIX B

Determination of Random Sequences

The random number sequences from which were derived the binary random tapes that were used to control the target distribution came from two sources. In the case of the pilot study the Kendall and Babbington Smith tables (1939) were used. For the main study the tables of the Rand Corporation (1966) were used.

The entry point to the tables was determined by using the output of a five choice electronic ESP tester (Millar, 1973) and converting it to a decimal number. This represented the number of the digit that would begin the sequence. The starting digits were number 20,625 for the pilot and number 546,654 for the main study.

The tables were read across as a binary sequence of odd and even numbers. The tapes were prepared for the animals in the order in which they would be tested and the sequence used was continuous as

if the table represented the output of a RNG which was turned on only for the trial sessions.

One problem arose in that the pilot study tapes were prepared for sessions of 100 trials. Observations determined that 80 trials was a more practical cut-off point for the animals so for these tapes the last 20 targets were systematically ignored.For the main study the tapes consisted of 50 targets only.

ABSTRACT

An attempt was made to confirm recent findings regarding precognition or GESP in rodents using positive reinforcement. With a small number of animals in an experimental set-up requiring handling of the animals two experimenters looked for significant above chance scoring in the total results as well as indications of individual differences between animals and experimenter effect. Both a pilot and a main study were run but no consistent pattern of significant above chance scoring emerged from the data. Suggestions of possible animal differences and experimenter effect were noted but the data did not afford a sufficiently consistent basis for any firm conclusions. A critique of the random behavior criterion is offered and conflicting results obtained for RBT's in the two studies suggest difficulties in applying this criterion to experiments using positive reinforcement.

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MODELS OF CONTROL AND CONTROL OF BIAS

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MODEL 1: Lowest degree of intersubjective control.[1]

The experimenter carries out the investigation according to this model under such conditions that no relevant control securing intersubjectivity is possible. In its most extreme form, this is manifested by the omission of raw-data in the published article. The only type of scrutinization that is possible, from the standpoint of the reader or the critic, is to find out, if for instance, the statistics that have been worked out, are reasonably appropriate. In addition, one can try to find out whether the conclusions drawn by the author are really a logical consequence of the presented data. The reliability of the results can not be assessed by way of testing. The strength of the data depends on the reliability of the investigator, but unfortunately there does not exist a good method or indeed instrument, for such a measurement, when we are confronted with such a report. There is no available method, except perhaps independent replication, by which to judge the data as to whether or not there are motivational errors, re-stated objectives and hypotheses, selective reporting etc. An editorial "doctoring" of the data may also have taken place.

Commentary

Most exploratory studies in psychology as well as in parapsychology are carried out according to this model (see Figure 1).

In some studies within the behavioral sciences -including parapsychology, two supposedly independent experimenters take part. This procedure in experimental parapsychology is sometimes referred

1) "Intersubjectivity" is a concept of great methodological importance in science. It controls personal bias and provides an answer to the question: "Do you see what I see?"

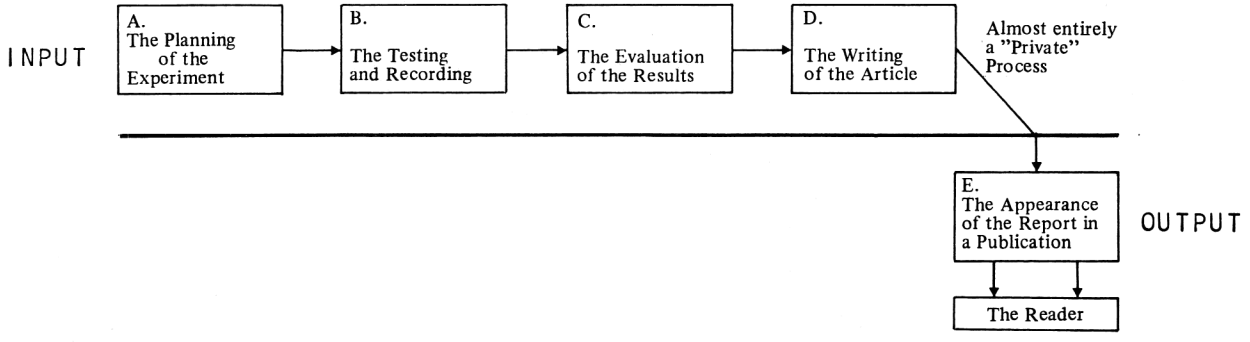

Fig. 1 Model 1. Lowest Degree of Intersubjective Control in a Psi-experiment.

to as the two-experimenter plan (Rhine & Pratt, 1962). This procedure is of course intended to enhance the "intersubjectivity" of the observations and also implicitly to make it a bit harder for the main-experimenter to cheat, or to "doctor" his data. It is however questionable, as to what extent the accuracy will be improved by having two experimenters instead of one. Under certain conditions, for instance if they have opposite expectancies, it seems reasonable to assume that their bias as observers should tend to cancel itself out. If however, the two experimenters share expectancies or are biased in the same direction in other respects, the improvement as regards the intersubjective control is rather questionable. One can also question the gain in intersubjective control with the use of two experimenters if:

a) the main-experimenter has been allowed to select his co-experimenter;

b) the main-experimenter is more senior than his co-experimenter.

Finally however, it should be stressed, that in practical life the use of two more or less independent observers is supposed to enhance the accuracy of the observations they make. Just as an illustration, think of an airline captain and his first officer, how they carry out the checking procedure by reading "against" each other.

MODEL 2: A recording and evaluation device is added to control unintended errors on the part of the experimenter.

This model of control is relevant to the case when the experimenter is aware of the potential risk of making errors of observation and recording. Two types of "personal" or human errors should be considered; one type (Type I) occuring without the awareness of the experimenter (unintentional errors); the second type (Type II) being pertinent to the case when the experimenter intentionally or deliberately manipulates the outcome of the experiment. Model 2 seems to be especially relevant for the Type I catagory. Its value for the control of Type II error seems to be very limited. In its simplest form, the only difference between Model 1 and Model 2 is, that in the case of Model 2, the experimenter utilizes equipment that automatically records the subject's calls or responses. By way of addition the recorder can be equipped with some kind of memory-unit that makes it possible to evaluate automatically the total number of hits that a subject has obtained during a test-session. A further sophistication of the automated procedure would be to use a programming-unit by means of which targets could be generated and recorded. In the latter case, one could consider using a computer for the generation of targets as well as for the

Fig. 2

Model 2. A Recording and Evaluation Device is Utilized in order to Control Unintended Motivational Errors.

recording of a subject's responses, and for the automated evaluation of hits.

Commentary

This model basically applies to the case when the deception hypothesis is not seriously considered (for instance under very exploratory conditions). The main advantage of the technical devices would be to reduce unintentional human errors in the procedure of recording and evaluating data.

In addition, it would relieve the experimenter from the burden of making certain observations and notations.

This model applies to the type of precautions which have sometimes been taken in more recent and advanced experiments in parapsychology. The model also depicts the types of safeguards against human errors that are frequently used in experimental psychology and other areas of experimental research. An inspection of the "critical" links or "synapses" of this model, will however, inform us, that this outline for the control of human errors does not help us very much in controlling fraudulent behaviour on the part of the experimenter (see Figure 2). This is especially true if the experimenter is also responsible for handling the equipment and for reading the obtained number of hits in his experiment.

MODEL 3: Optimized automated and intersubjective control of a psi-experiment.

As has previously been stressed there are a number of potential artifacts that may influence the outcome of a scientific study, from its outline up to the stage of reporting the results in a scientific journal. Further, it has also been shown, that deception on the part of the experimenter may not be such an infrequent phenomenon as is usually thought.

Even if I believe that the "fool-proof" experiment is somewhat of an Utopia, I nevertheless think that it is not only possible, but extremely important, to develop methods involving a wide range of precautions against artifacts related to "human errors" or human limitations, for want of better terminology.My position does not suggest that I believe there to be only one scientific method - the one! I am quite aware that all methods rest upon some assumptions as to the nature of "reality" and how to investigate it.

It is a truism to state that scientific methods are invented by man. But according to certain criteria, methods may vary in their sensitivity to artifacts. May I remind you, that deception in science is not a phenomenon restricted to parapsychologists but

most certainly applies also to psychologists as well as scientists in other areas of research. Based upon what has previously been said, I am quite convinced that it is a very important strategy of research to try and do everything we can to control and indeed tighten up the designs of our experiments in order to avoid the potential influences of "human artifacts" of all conceivable types.

I therefore suggest that we should try to analyze and identify the critical events and synapses of our experiments, from the very first outline to the published report.

I believe that in this context, the use of the system approach could be very efficient and appropriate. As a matter of fact, we have already used this method in our discussions and illustrations of what here have been described as Model 1 and Model 2. As regards Model 3, the objectives should be:

a) the model should offer the maximum possible automated control, out of reach of possible manipulations on the part of the experimenter;

b) the model should offer the maximum possible "public" control, or in other words, a maximum of intersubjective testability;

c) according to the philosophy of this model, the experimenter should define his problem, formulate his hypotheses, and outline his experiment, prior to commencing his study. He should write his manuscript, stating at least essential facts, before carrying out his investigation. This manuscript, in principle only lacking data in the tables, presentation of results, and interpretation of results, should be sent to one or more editors, and the experimenter should not initiate his study until at least one of the editors has promised to publish the study, regardless of the outcome of the experiment. In this way we could avoid selective reporting. Furthermore the experimenter will not be given the opportunity to change his hypotheses in such a way that they "fit" the outcome of the experiment;

d) a "neutral" agency should be responsible for generating the targets to which the subjects are supposed to respond. The same agency or institution should record the responses made by the subjects. Finally, the controlling agency should be responsible for the evaluation of raw-data, for instance the evaluation of the total number of hits.
Such a "neutral" agency could be a computer centre.
At the experimenter's laboratory there should be a computer terminal, through which the subjects are able to "communicate" with the computer centre.
Before the experiment commences, the experimenter should request and specify what kind of assistence he will need from

the head of the computer centre at a particular time (number of random "events", probability ratio, etc.).
Such a request should follow a certain routine. The head of the centre should in such a case, after evaluating the outcome of the experiment, fill in a form or empty spaces planned for Tables, sent to him by both the editor and the experimenter when the services were requested. The form, with the evaluated raw-data should be duplicated.
The head of the computer centre should keep one for himself and one copy should be sent to the experimenter, the other to the editor.

Commentary

One can always claim that the experimenter could have "bribed" the head of the computer centre to take part in a well-organized conspiracy, which might also include the co-operation of one or more of the editors. In my opinion the probability of such a hypothesis, from a common sense point of view, should be much smaller than the probability of an experimenter exhibiting deceptive behaviour under conditions when nobody can really control him. Assuming one can state that the hypothesis of a conspiracy is irrefutable, and since that is the case with all the discussed models, what is then the sense of making the experimental procedure more complex, time-consuming, and more expensive? My answer to this question is that one should notice carefully that not all empirically irrefutable hypotheses have the same probability. It should also be stressed, that in principle it should be possible to engage a great number of computer centres which can reserve time for a certain experiment. At the time of the experimenter's commencement, a random procedure, out of the reach or control of the experimenter, could determine which of these computer centres would take care of the requested services.

In this way, the possibility of obtaining positive results by way of a conspiracy becomes still more improbable.

Another type of criticism that I anticipate, as regards experiments carried out according to Model 3, concerns the idea that well-controlled experiments should always exert a devastating influence on the psi-ability of the subject or the experimenter-subject dyad, by imposing an "unfavourable" atmosphere. This kind of criticism seems to me to lack both sophistication and justification. If the experimenter himself accepts to work according to Model 3, and if his subject remains ignorant of the location where the targets are generated, what "psychological" impact could that have on the subject's scoring performance? It

should be noted that nothing in the laboratory setting will be allowed to reveal whether or not the targets are produced within the laboratory or externally. Especially in the predictive experiment (a test of precognition), the above-mentioned type of criticism seems to be quite irrelevant. "Model 3 conditions" actually relieve the experimenter from the rather dull duty of making registrations, while leaving him free to concentrate on creating a good "rapport" with his subjects, often thought, but scarcely proved, to have a vehicular effect on a subject's psi-ability. It is very probable that certain subjects would find this type of experiment to be of a very challenging nature.

I would like you to notice that I have never said that all studies in parapsychology should be carried out according to Model 3. Indeed, what I wanted to say is that I find it extremely important to identify the critical points or synapses in our experiments, to be able to control potential bias. I believe however, that some experiments in parapsychology, carried out by naive "verificationists" and thought of as "conclusive", should be subjected to the rigours of the discussed model. We have to confess that we still find ourselves in a "pre-paradigmatic" period of research (to use one of Kuhn's terms), in which we primarily involve ourselves in fact-gathering or at least data-gathering.

That means that there is also place for the use of more exploratory types of studies. It is a pity that there does not exist a scientifically sound formula by which we can decide how to order the priorities as regards application of approach methods. On the other hand, there do exist very general and well-validated research ideals to which we have every reason to adhere: we should always do everything we can to obtain the highest possible degree of intersubjective control in our studies. This also applies to case studies in parapsychology.

The positions taken in this part of the inaugural address are considered as some of the hallmarks of this journal. We will try to avoid selective reporting, that is, the tendency to bury negative results and only to publish studies that "turned out". For one thing we believe that there is a chance to learn something important even from negative findings; for another the policy of exclusively publishing "supporting" findings even if such a policy may have a rationale or at least easily can be rationalized, may probably exert a strong temptation to "doctor" ones data.

At the same time we do not want to make our journal a graveyard for all those studies which did not "turn out". This can be managed by adhering to the editorial policy that the acceptance or

rejection of a manuscript should be done on the basis of the quality of the design and methodology of the planned study and prior to the carrying out and the evaluation of the results of the study.

ABSTRACT

The author discusses how to increase the quality and reliability of the research and reporting process in experimental parapsychology. Three levels of bias and control of bias are discussed. The levels are referred to as Model 1, Model 2 and Model 3 respectively.

Model 1 is characterized by its very low level of intersubjective control. The reliability of the results depends to a very great extent upon the reliability of the investigator and the editor.

Model 2 is relevant to the case when the experimenter is aware of the potential risk of making both errors of observation and recording and tries to control this bias. However, this model of control does not make allowances for the case when data are intentionally manipulated.

Model 3 depicts a rather sophisticated system of control. One feature of this model is, that selective reporting will become harder since the editor has to make his decision as regards the acceptance or rejection of an experimental article prior to the results being obtained, and subsequently based upon the quality of the outline of the experiment. However, it should be stressed, that not even this model provides a fool-proof guarantee against deliberate fraud.

It is assumed that the models of bias and control of bias under discussion are relevant to most branches of the behavioral sciences.

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THE PARADIGM OF THE RHINEAN SCHOOL. PART 1

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Philosophers of science have so far neglected the field of parapsychology. They tend to view it, together with phrenology and psychoanalysis, as a convenient and pedagogical example of a pseudo-science without acceptable methodological foundations. In general philosophers of science are more familiar with the natural sciences than with the behavioural sciences, and parapsychology ranks much lower in the hierarchy of investigatory diciplines. There are exceptions however, - e.g. Micheal Scriven - but the sparse treatment of parapsychology within the philosophy of science shows profound ignorance and bias. (I do not include analytical philosophers as Broad, Flew, Mundle etc. in this generalization. They have of course made important studies of specific problems in parapsychology).

I shall give one example of a typical reaction from a meta-scientist. Mario Bunge's "Scientific Research" (1967) is an excellent and authoritative textbook. It has pedagogical merits and of course he does not want to confuse "good" science with "bad" science. Examples of improper methods and logic are naturally taken from recognized pseudo-sciences like parapsychology and psychoanalysis. The line between scientific and non-scientific research groups is kept intact. However when dealing with parapsychology he is guilty of simplifications and misreadings, which he does not exhibit in other parts of the book.

Bunge considers parapsychology as a pseudo-science for the following reasons (1967: 36-40). 1) Parapsychologists do not use open hypotheses. Concepts such as ESP force them to use certain interpretations. 2) The statements of parapsychologists are loose and without informative content. They are not connected to other fields of knowledge. If they were, they would cease to be parapsychological phenomena, i.e. non-normal or non-physical. 3) The claims of parapsychologists are non-naturalistic. The phenomena are a priori impossible, since they contradict accepted scientific knowledge. 4) The experiments suffer from methodological weaknesses as a) frauds, b) lack of repeatability (at least in front of

sceptics), c) optional stopping, d) misapplied statistics such as non-random samples. 5) Parapsychologists test their theses "en masse" and not one by one, and therefore they have no experimental control of independent variables. Also the tests are insensitive as the parapsychologist afterwards explains away any failures on the part of the subject as displacement-effects or psi-missing. 6) Parapsychology has not in 5000 years (!) produced one single law or definite prediction, nor even an empirical regularity or a certified fact. "In other words, psychical research has not attained the goal of science - and has never aimed at it" (p. 40).

Most of Bunge's criticism is based on an ignorance of the literature - his main sources being Price and Hansel - while some of his points would apply to research 30 - 40 years ago. However, some of the criticisms are an embarrassment to parapsychology, and these will be discussed below, together with the concept of science.

One of Bunge's arguments surprises me - the a priori - argument. It is a classical argument against parapsychology. It stems from Hume and has been used by Price and Hansel. Nowadays it is seldom used, but when specific critical arguments are presented the a priori argument is implicit: Parapsychological experiments have to be weak and untenable because the phenomena are impossible!

I do not believe in the a priori argument. The history of science shows that it is too dogmatic. It would reject all statements which question the orthodox system of science. Intellectual revolutions in science would therefore be impossible or belong to history. Nowadays science has reached a point where only minor corrections are necessary. This argument is based on an inductive concept of science, implying that scientific knowledge accumulates from basic and eternal facts, and that scientific laws are infallible. Empirical historical studies have proved, as shown by modern philosophers of science (Kuhn, Feyerabend and others), that this is a naive and outmoded concept of science.

The apparent contradiction is not one between psi and the laws of physics, but between psi and certain basic limiting principles (Broad, 1953), or paradigmatic pre-suppositions shared by most contemporary researchers, e.g., "no mental event exists without a physical basis", or "no effect can precede a cause". These principles seem to be metaphysical and not exclusively scientific, but they govern the selection of problems and hypotheses in a particular research tradition. It is from the standpoint of these metaphysical principles - world-picture assumptions - that theories and findings in science are interpreted and scrutinized. They can not be directly verified or refuted. They belong to the inner core of a paradigm and are rarely ever challenged. It is probably premature to state whether parapsychology threatens all, or some, of these priciples.

For the sake of argument let us accept the view that some laws of nature really are threatened. But laws of nature are never refuted. It happens that universal laws without exceptions and limitations in extension are stated, and after some time are proved to be false in areas in which they had earlier not been sufficiently tested. Such a law has to be restated as a law formula with a limited validity, beyond which it may be false. The special theory of relativity did not invalidate Newtonian physics, but instead it incorporated the classical physical laws as special cases within twentieth century science and with limited extension. They may not apply to micro- and macro events. In the same way, those laws which in the future may happen to conflict with established psi-phenomena will have to be improved with some restricted formulations. At any rate, they will not show up as "false laws".

The validity of parapsychological phenomena is not a question of relying on or not relying on e.g. the energy principle!

In my opinion there are very good reasons for a direct confrontation between parapsychology and the history and philosophy of science. Of course, if the evidence for psi is acceptable, the paradigm of behavioristic psychology is faced with a serious anomaly. This may require the help of philosophers of science in order to formulate the paradigmal confrontation and to work out a solution to the problem of relating parapsychological phenomena to the scientific framework.

But regardless of the claims of parapsychology being well-grounded or not, reasons still exist to consider parapsychology an important topic of research for metascientists. The philosophy of science may be conceived as a dicipline which serves the other sciences. When chosing topics of research, one of the first rules ought to be the application of metastudies within the field. In parapsychology the existing problem situation expresses great need for theoretical studies, which has been acknowledged by individual researchers (e.g. Thouless, 1968).

The purpose of the present paper is to formulate the world-picture and concept of science within a dominant tradition in modern parapsychology. I believe that a discussion of general and fundamental questions or the examination of a particular field from an outsider's viewpoint is an important contribution to a more mature self-reflection amongst researchers.

THE RHINEAN SCHOOL

There exists no consensus of opinion as to which investigators we should include as parapsychologists. A psychologist publishing a paper on ESP in a psychological journal may not regard it as parapsychological research. He can express himself as if he, being a psychologist, is only using a design similar to those used in parapsychology (e.g. Nathanson, 1965). Parapsychology enjoys too low a status for psychologists to want to associate with it. On the contrary there are popular writers who produce articles on occult matters without professional qualifications. However even among investigators who call themselves parapsychologists one can easily find disagreement on definitions and methodology within parapsychology. There are different schools with different traditions. A school is united in a paradigm. How concrete a parapsychological school is can be ascertained by its attitude to the following points: Use of spontaneous and qualitative cases, or exclusively laboratory and experimental evidence, selection of problems (which are the high risk-problems?), models of psi and types of explanations, attitude to negative results, etc.

I will in this paper restrict my discussion to the Rhinean tradition, the most dominant school of parapsychology during the last forty years. Its leader is Joseph B. Rhine and among its adherents we find most of the American researchers who publish in the Journal of Parapsychology and in the Journal of the American Society for Psychical Research. It is a coherent group united by a similar conception of science and world-picture and depending upon a similar educational background.

The grounds for considering the Rhinean approach as a separate school and J.B. Rhine its leader, are:

1) In the 1930's Rhine formulated in his Extra-Sensory-Perception (1934) an exemplar for the paradigm. He supplied the perspective and the instruments for the school and he initiated a research plan. He gave the intellectual framework for the experimental parapsychology. Similar attempts were made earlier in Germany and Great Britain, but they were not successful.

2) Rhine has been controlling the main journal of experimental parapsychology ever since he started it in 1937. In later years he has not been in the formal position of editor, but nevertheless he has always dominated the editing policy and the selection criteria.

3) Rhine has been the director of the parapsychological laboratory and from 1964 onwards director of FRNM. These institutions have trained most investigators in experimental parapsychology. Leading parapsychologists often stress the urgent need for a professionalization of the field. Research is carried out by investigators with a basic training as psychologist, physicist,

biologist, or engineer. But apart from this the personnel in parapsychology needs to be trained in the special methodology of the field. Up till just recently the only place were such training was available was Durham, under Rhine and his associates (e.g. the Visiting Research Fellowship Programme). This condition has guaranteed an extremely uniform growth in the field.
4) Rhine has personally inspired many younger parapsychologists. With his enthusiasm and demanding force he has been a father figure, in the same way as Freud was to psychoanalysts in the early part of the century. His attitude and personality has been an example to the ethos of parapsychology: Try to keep together in the family and identify yourself totally with the field and its needs.

There are many parapsychologists who do not belong to the school and some who belong only to the perifery. In Britain parapsychology has never been institutionalized in the same way, while on the continent a different school exists with strong historical bonds, and with a leaning towards spontaneous cases, physical phenomena, and dynamical personality models. In the following, parapsychology stands for the Rhinean concept of parapsychology, unless otherwise stated.

PERSPECTIVES ON SCIENCE

Philosophy of science is a dicipline which has other sciences as its axis. The examination of a science is always carried out in the light of a theory of research; a special perspective on science. However, there are many ways of looking at a science. One can look at science as:
1) results without research,
2) results and research without researchers,
3) results, research, and researchers.

The first perspective is a static one developed by the logical empiricists. It limits the study to the final product: completed theories, knowledge, and conceptual systems. It concentrates upon "the context of justification" and neglects "the context of discovery" as Hans Reichenbach (1951) has expressed it.

The second perspective is developed by Popper and Lakatos. They are interested in the growth of knowledge, how theories, problems and methods develop, but they disregard the researcher himself. "Knowledge in the objective sense", says Popper (1972), "is knowledge without a knower: it is knowledge without a knowing subject." Popper views the researcher as an incarnation of critical sense always prepared to give up his theories for new ones. But this perspective does not take into consideration that researchers are human beings and therefore not always rational.

The third perspective will be used in this study. To this belong,

among others, Kuhn and Feyerabend. They have exceeded the purely critical and rational aspects of research and have linked the philosophy of science with empirical diciplines such as history of science, psychology of research, and sociology of research. They study research 'in vivo'.

In my attempt to outline the paradigm of modern experimental parapsychology, I will use the approach of the "Gothenburg School" within the philosophy of science (Törnebohm, 1971; Törnebohm & Radnitzky, 1971). Research may be viewed as; growth and evaluation of knowledge, problems, and selection of instruments (intellectual techniques or hardware equipment). The task of the researcher is to draw increasingly detailed and more correct maps of an object of investigation; the territory. This transformation of knowledge is embedded in a human setting which directs the process. The directing factors are internal and external. The internal factors are here called the paradigm, a concept derived from Thomas Kuhn's "The structure of scientific revolutions" (1970). It will be used independently of Kuhn as a superior guiding factor in a research tradition. The paradigm determines the way investigators look at problems, select hypotheses and instruments. I will touch upon the following aspects of the paradigm:

1) Guiding interest of knowledge.
2) World-picture and image of man.
3) Concept of science and model of science.

Other aspects, e.g. ethics, esthetics, are of minor interest in the context of parapsychology.

The paradigm is bound to the tradition, but not necessarily articulated. Generally it exists as a pre-supposition of an intuitive or tacit character, which is supplied through education and different forms of co-operation and communication.

Of course, there are also important external guiding factors - interests of others outside the research group, neighbouring sciences, or other paradigms which affect the dicipline in question, social and economic conditions in science, politics of research, etc. - but I shall in this paper however concentrate on only the internal factors.

INTEREST OF KNOWLEDGE

What is the aim of science? To produce explanations for scientific problems. But what for? There are different types of interests of knowledge in science and these interests make up the social motivation for a science. They determine the economic support from the authorities and affect the planning of research. According to Habermas (1965) interests are collective and institutionalized

legitimacies for science. They are abstract, unlike the concrete motivation of the individual researcher. We can distinguish between at least four types of interests:
1) The hermeneutic: Its aim is to interpret meanings in communicative contexts. Interpretative diciplines are philology and traditions in history.
2) The technological: Its aim is to apply knowledge in order to improve conditions in society. Typical examples are physics and medical research.
3) The emancipatory: Its aim is to emancipate man from false assumptions by criticism and reflection, in order to create a free, mature, and autonomous man. The model science is psychoanalysis.
4) The innovatory: Its aim is to develop new perspectives and theories and to improve the scientific world picture.

The hermeneutic perspective does not seem to be of any relevance to parapsychology. The technological interest is the one most scientists stress, when they hope for economical support. Many claim that this interest distinguishes science from non-science. The primary characteristic of science is its predictive and controlling capacity. This interest is connected with some assumptions of reality: It is only succesful in dealing with objective regularities in nature. All the scientific data must be repeatable. Theories have their value as instruments for prediction.

Some parapsychologists have insisted on the possibility of putting PK and ESP into practice. They hope to develop a technology. In the early 1950's Rhine and others saw the possibility of using psi in psychological warfare or in the secret service (Anon, 1949; Rhine,1957). Also, subjects like Geller with his ability to affect the workings of a clock could make many skills unnecessary. But so far it is safe to say that the application of psi lies in the future. Some parapsychologists would even claim that the technological interest is non-existent, as the psi-phenomena are supposed to be non-physical and unpredictable (e.g. Pratt 1974). If one is unable to predict or formulate general laws, one cannot control and develop a technology. It is very hard to see how one will be able to attain scientific knowledge in that case. At least, parapsychology will not be a nomothetical science but an idiographical one. It will deal with particular and unique phenomena like historical events. (More of this distinction later on).

The most important interests of the Rhinean school are the emancipatory ones. In Rhine's opinion parapsychology tries to uncover hidden dimensions in man. It helps him to discover the true inner nature of man and to liberate him from the bonds of materialism. A new man will appear as the science of psi progresses.

This man will be free and self-conscious. He will have the capacity to reach beyond the limits of the self, to communicate directly with other human beings, and explore reality in an active way.

Rhine thinks that mechanism, behaviourism, and psychoanalysis have created, in the USA and of course still more in the communist countries, a split in the minds of men. The mechanistic perspective clashes with the civilized, idealistic, and christian moral code. In proving the existence of non-physical entities in man, parapsychology may be able to invalidate materialism and communism. Had parapsychology reached its current state before Marx and Lenin, it is possible that western civilization would not today have to struggle with the evil power of communism, as "the evidence of psi, showing that its operations defy physical description, experimentally disproves this materialistic theory of man on which communism has been founded." (Rhine 1957:246). Psi-phenomena may then provide an empirical foundation for the values of Western democracy.

When man discovers his own inner nature, he will also know how to live in harmony and happiness together in society.

> "What we most need to know, and I think you will agree with me, is enough of man's basic nature to provide us with the understanding we will have to achieve and maintain, individually, and in all our institutions; the dicipline and integration without which life and all other values become chaotic and meaningless" (Rhine 1959:15).

This true, authentic man is not bound by culture or society. He is universal. His nature is more positive than the behaviouristic and psychoanalytic image of man. It is closer to American democratic ideals.

You will not find this emancipatory interest so clearly stated as in Rhine's writing, but I believe most of the Rhineans would accept his view regarding the aim of parapsychology. It is also in agreement with the broad world view of "the third force" within american psychology (Allport 1955; Maslow 1962).

Finally, the innovatory interest is of course present in all scientific activities. But if it is too strongly expressed, as the only aim of science, then the concept of science is puristic ("science for its own sake"). Parapsychology is often viewed as unique. It will not provide us with bits of knowledge. It aims directly at the improvement of our world-picture. Parapsychology is the border science. I think this is a correct assumption: Parapsychologists are investigating the anomalous phenomena of the orthodox paradigm of science. Of course other diciplines also make anomalous discoveries, but it is not their salient characteristic. By admitting that the innovatory interest is a very important one

in parapsychology, it does not follow that individual parapsychologists within this institutionalized interest do not have personal motivations differing from a purely disinterested search for truth. The disinterestedness is part of the ethos of modern science, an imperative which parapsychologists attain to no greater extent than other scientists.

I think religious motivation and emotional personality characteristics tacitly determine the aspirations of most parapsychologists. But contrary to many critics (e.g. Rawcliffe, 1952) I do not mean that this necessarily invalidates parapsychology as a scientific dicipline. If parapsychologists would try more self-reflection and thus uncover unconscious motivations affecting their research, it is possible that they would gain a more objective and fruitful perspective of their problem-field.

THE WORLD PICTURE OF PARAPSYCHOLOGY

The most important part of the paradigm is the world-picture. By this I mean a conception of a greater part of the reality than the one studied by a separate group of investigators. The world-picture is composed of ideas of the universe and of man's place within it. It determines the categories and perspectives which the researcher uses when he tries to map the territory. A problem is formulated in such a way that when viewed from the perspective of the world-picture, it is supposed to cover the most essential aspects of the territory. Often these aspects will be totalized. In a mechanistic world-picture only mechanistic properties of the object appear as real, or possible to investigate in a scientific way. The researcher has to restrict himself to a certain aspect of the territory. Of course the world-picture has direct consequences for the concept of science. If only mechanistic properties are regarded as feasible in the study of parapsychological phenomena, the researcher will use methods commonly employed for the study of mechanistic events.

The world-picture is in a process of improvement. It originates from mythical and religious concepts. During the progress in research it has been refined and transformed into theories of a more special kind. There are no clear-cut distinctions between scientific and non-scientific assumptions about reality. Metaphysics is a necessary precedent and ingredient in all theoretical systems. However in the early phases of a science the impact of the world-picture is greater, as many pre-conceptions of the territory still have to be articulated.

Sometimes a more external aspect of the world-picture is important, an ideology or "Weltanschauung", i.e. global views on

life, on man's place in nature, and existential questions concerning the meaning of life. These conceptions are essential in many humanistic diciplines and therefore also in parapsychology. Often the scientific part of the world-picture becomes confused with the ideological outlook. An ideology or a world-picture cannot be refuted by scientific arguments; they have to be criticized from a metaphysical standpoint.

To focus on world-picture assumptions in parapsychology seems very natural, as parapsychologists themselves often formulate their positions in terms of world views, perspectives, and scientific revolutions. Of course, every part of research is not explicitly stated within a world-picture context. In the main American journals there are two types of paper; the empirical and technical reports, and reflective, philosophical articles on "Paranormal phenomena, nature and man", or "Quantum theory and parapsychology". We can speak of two types of parapsychologists. The first one is the ideal of the objective and impassionate researcher. He provides the dicipline with small pieces of research and tries to fit them into the developing problem-field. He rarely evaluates his attempts in terms of paradigmatic assumptions, even though he tries to induce explanatory concepts or theories (Roll, Stanford). The other type is the ideologist in the field. Sometimes he has been active as an experimentalist, but as he grows older and more experienced he turns to metaproblems (Rhine, Pratt, Murphy). Or he may be a professional philosopher attached to the field by interest (Broad, Chari). Or he may even believe that at present it is urgently needed to get better models in research, in order to improve the paradigm, and he pursues this parallel in his research (Beloff).

Many parapsychologists have found direct implications in parapsychology for our world-picture. Their findings have often been interpreted as arguments for a serious re-consideration of the assumptions about man and nature, which they think are present in orthodox contemporary science.

Rhine has been compared many times with Copernicus: His undertaking in the 1930's is thought to have had the same significance or even greater than the Copernican doctrine; a revolution which will restore the nobility of man where the Copernican revolution aimed at a new conception of the universe. But scientific revolutions are slow. Recently Pratt (1974) asked for an Einstein in parapsychology, a theoretical genius who would fulfill the new revolutionary promises.

When Kuhn's "The structure of scientific revolutions" appeared in 1962, some parapsychologists greeted his scheme of scientific revolutions as indirect support of the scientific status of

parapsychology and of their own revolutionary perspective (McConnell, 1968; Thouless, 1972). They interpreted Kuhn's concept of pre-paradigmatic phases of science - which Kuhn later abandoned - as implying that all sciences have once been in a pre-scientific state, and that therefore all pre-sciences like parapsychology will sooner or later end up within a Kuhnian paradigm.

Historical analogies prove nothing in contemporary science, but they may be illuminating. Let us take the case of Galileo and the Copernican revolution. Often this case is assumed to illustrate the consequences of a rigid a priori position for facts and theories which conflict with orthodox opinions. Galileo is regarded as the prototype for the detached and objective researcher, and his opponents as the same prejudiced sceptics just as the critics of parapsychology. Parapsychologists find it convenient to view the Galileo case as being a counter part to the debate on parapsychology. The two debates have some obvious similarities. However, from the internal evidence it could be argued that the Aristotelians were justified in rejecting the Copernican theory, as stated by Galileo.

The proposition that the earth moved around the sun was unwarranted, Aristotelians said, as it could not be brought into accordance with the then upheld tradition in physics, i.e. the Aristotelian, nor with the common man's conception of the world. Either the unexpected phenomena had to be fitted into the existing explanatory framework, or a new world-picture and physical theory had to be laid down, which would explain both old and new facts. If neither were possible, Galileo must be assumed to be wrong. (Galileo tried to illustrate the faults in the Aristotelian mechanics, but it was not until the Newtonian period that an explanation was supplied to incorporate all the new celestial observations.)

When Galileo's critics could not find any significance in a moving earth, they tried to criticize the weak points in his arguments. It was with the help of the new telescope that Galileo had made the startling observations of the satellites of Jupiter and the phases of Venus. No one could see these phenomena without the help of instruments, and the basic proposition about the movement of the earth was, according to Galileo, impossible to prove by direct verification. As is well known Galileo's instruments were brought into question. Some critics even refused to look into the telescope. Optics were notorious for their capacity to distort the true vision of objects. The telescope had an extremely small visual field and no legs. Besides, the lenses were dim. To be used efficiently the telescope demanded great

skill, patience, and sharp vision. Furthermore Galileo could not explain the function of the telescope to his critics. No wonder that he had difficulties in convincing them. They preferred to think that Galileo had been deceived by an optical delusion caused by the telescope.

These problems resemble the criticism of statistical design and procedure in parapsychology. A common belief at least among psychologists, is that the results of parapsychology could be explained as a methodological artefact. But the important point is, that most of the methodological criticism would not have arisen, had it not been for the fact that the clash between world-picture assumptions was so critical.

Among the different aspects of the paradigm in parapsychology it is the world-picture which differs most sharply from paradigms in other sciences. Of course, there are unique problems of methodology in parapsychology, but the main assumptions of what constitutes a science resemble those in e.g. experimental psychology. It is the world-picture and the image of man that have caused the primary scientific resistance; the way in which parapsychologists carry out research is merely a secondary source of opposition.

It is probable that the deepest roots of resistance lie in the cultural tradition in which the concept of the dualism of mind and body has developed, in the opposition between christian idealism and physicalistic science. The distinction between psi-phenomena and naturalistic phenomena is a sharp one in the Western culture, but this is not the case in the Indian tradition. Today a majority of Western parapsychologists stick to a dualistic ontology. They consider psi-phenomena as being the strongest available evidence for a dualistic world-picture and impossible to reconcile with present physical science. The world is made up of two different entities; a physical reality governed by physical laws, and a spiritual reality governed by non-physical laws. The image of man is Cartesian. Man consists of a physical body and a non-physical mind. They exist independently of each other, but they interact and exchange energy. Typical interactionist views of man have been developed by parapsychologists and philosophers as e.g. J.B. Rhine, Robert Thouless, John Beloff, and C.D. Broad.

The main assumption of Rhine's image of man is, I think, that psi-phenomena are extrasensory and non-physical. The non-physical aspect of psi is often expressed as a "fact", and it is generally acknowledged that no physical model of psi exists which is both testable and in accordance with the experimental findings in parapsychology. Furthermore, psi-phenomena refute the more crude versions of materialistic theories of mind, as e.g. the doctrine that all behaviour is explicable by events in the central nervous system, and in terms of present-day physical science. However, it

is not obvious that no neo-materialism based on tomorrow's science could be linked with parapsychology. Rhine's assumption of a clear-cut distinction between physical and non-physical phenomena is rather a strong one. I believe it is more reasonable to work out additional postulates in physical theories, than to completely give up the physical doctrines of modern science. At least it is premature to take such a decision. The law of gravitation in the late 17th century was impossible to assimilate with the contemporary mechanistic world view: No mechanism could explain how it worked. Of course some critics accused Newton of a metaphysical proposition. But the forced attempts to create a mechanism to illustrate gravitation caused the problem to survive until 20th century science and its refined physics found out, that the whole problem was ill-defined.

The Rhinean interactionism has left marks in both terminology and experimental designs. Parapsychologists with a physical model of psi have been active in the search for physical analogies, but the Rhinean perspective has given rise to experiments, in which the purpose is to show how psi operates independently of space, time, and material barriers. However, experiments where experimenters or subjects know about the actual physical conditions are ill-designed, as the non-physical assumption functions as a prerequisite. It affects the way in which the researcher views the problem and looks for solutions, and it possibly affects, in the case of parapsychology, also the outcome of the experiment. To sum up: A conservative position in the non-physical issue would be that psi cannot be accommodated in present physical science, and that there is small possibility that psi will never be integrated in future physical sciences. Such a development would however pose difficult problems for initiators of high-order theories in parapsychology.

In a very important way Rhine has changed the image of man in parapsychology. He started with the assumption that psi could be detected in most, possibly, all men. In this sense psi was taken as "normal". What followed was an interest in testing for psi in unselected groups of individuals. In the 1940's Schmeidler supplied the exemplar with her sheep-goat investigations. The earlier dominant assumption, determined by the historical and mythological contexts, was that psi was a rare quality. Researchers looked for specially gifted subjects or mediums. Even after Rhine's early success some British investigators persisted with this prescription.

Rhine also stressed that psi was not abnormal. In the early days of parapsychological investigation, when professional mediums were used as subjects, many investigators saw a connection between psi-

ability and a pathological or hysterical character. It is well known that psi has historically been regarded as a sign of magical powers. Many case reports have seen a relation between psi and individuals suffering from a state of stress, and there are studies of relations between psi and dissociated states of consciousness. Rhine however, says not only that psi is a general ability of all men in different degrees, but that it is related to "the higher thought processes". Psi-phenomena are not sensorimotor functions, but purely mind functions. The study of psi shows us a man of noble, free, and supreme character, contrasting with the image of man in psychoanalysis, where man, as Rhine views it, is depicted as a brute animal determined by low instincts (Rhine, 1953).

Rhine sometimes uses the expressions - the reach of the mind, or, mind over matter. He conceives of mind as superior to matter. This mind terminology has been noticed and criticized many times (e.g. Flew,1953). I have already mentioned Rhine's ideology or Weltanschauung; the close relevance of parapsychology to ethics and politics. Like the humanistic psychologists in the early 1950's Rhine found Western man in an existential crisis, religion fading away, but materialism and communism gaining in strength. In this materialistic movement behaviorism and psychoanalysis developed with a corresponding image of man: Man as totally determined by the external environment or by his biological needs. Parapsychology, as Rhine saw it, was the best weapon to conquer this negative and corruptive world view. Only the awareness of man's non-physical nature could give Western man a positive view of life, could make him transcend his limitations, and admit the establishment of his divine inner nature. Gordon Allport (1955: 99-101) said

> "I have written this because I feel that modern psychology is in a dilemma. Broadly speaking, it has trimmed down the image of man that gave birth to the democratic dream. - - - Soon, we venture to predict, psychology will offer an image of man more in accord with the democratic ideals by which psychologists as individuals do in fact live."

Replace psychology with parapsychology and we find similar notions in Rhine's "New World of the Mind", as for example (1953: 266,272)

> "It is, in fact, the main intellectual challenge of communism that it contests the rather loose Western concept of the personal freedom of the individual and confronts mankind with a philosophical determinism that, like an all-adequate religious creed, envelopes the whole of life. - - - But there is hope of a social awakening to a re-discovery of man - a man the sciences have never known."

The Rhinean ideology is an extremely idealistic and individualistic

one. It assumes that the production of ideas and the self-knowledge of men will be determinant of our history and will guarantee peace and harmony in our society (Rhine,1957; Rhine,1959). However, it has to be said that the ideological parts of the world-picture have not appeared to so great an extent in latter years. Even if Rhine himself may still see the connection between the non-physical assumption and the view of life, other Rhineans may admit the former but remain sceptical about the latter.

In the Rhinean paradigm there are also patterns of a behavioristic image of man. As these are connected to the concept of science in parapsychology, I will discuss them in the next part.

EFFECT OF REDUCING RESPONSE PREFERENCES ON ESP SCORES

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Usually in the standard type of ESP experiment in which subjects repeatedly choose from the same limited number of symbols non random response sequences are found. Non random response sequences are also found in different types of experimental psychological investigations, for instance in those in which subjects are requested directly to call symbols in a random sequence (see among others Tune 1964). It appears that generally subjects are unable to choose symbols at random, and this phenomenon is of consequence for ESP experiments.

The main idea, but one rarely discussed, which underlies the standard type of ESP experiment would seem to be, apart from the possibility to estimate exactly the probability of any number of hits, that the subject chooses each trial from a limited number of equivalent symbols. The symbols are all equivalent to him except for the fact that one of the symbols is the target. The subject is thus equally motivated to choose any of the symbols. Hence it becomes possible that even a weak ESP effect distorts the equality of the symbols in favor of the target symbol. If this reasoning is correct, it implies that any non paranormal variable influencing the decision in respect to symbol choice reduces the possibility that a paranormal influence determines the choice. Such a variable has to be considered as an inhibiting variable in an ESP experiment.

When in ESP experiments non random response sequences are found it follows that such inhibiting variables did influence the choices. Any systematical trend in a response sequence gives indication of the existence of a certain strategy in the way of choosing. As regards this sequential response bias, which means that each response is related to the immediately previous responses, is important since it implies that the majority of the choices were influenced by non paranormal variables. It appeared from previous research (Schouten, 1972) that the main phenomena

of non randomness in response sequences can be explained by assuming that subjects associate randomness with equal frequencies for all symbols and with the idea of being non systematical, which means not repeating the same sequences of choices. They realize these notions by employing a certain choice strategy. If this explanation is correct it follows that it is possible to teach subjects to choose more or less randomly by changing their concept about randomness and to teach them not to employ a specific choice strategy.

The aim of the present experiment is to investigate whether a relationship exists between the extent of non random choosing and the size of the ESP scores by comparing the ESP scores of the same subjects before and after a training aimed at reducing response preferences. If evidence for the existence of such a relationship is found and if it appears that choosing more randomly results in higher or more extreme ESP scores it might become possible to improve the efficiency of ESP experiments in general.

The majority of the subjects show as main phenomena of response preferences a tendency, in the case of the symbols being presented visually, to call centrally placed symbols more frequently than symbols placed at the extremities; a tendency to avoid repetition of the same symbol, and a tendency to balance the frequency of the symbols within a limited number of trials. The tendency to call centrally placed symbols more frequently than the others looks contradictory to the tendency to balance the frequency of the symbols. However, it is the choice strategy employed by the subjects which creates this contradiction. This choice strategy is responsible for the balancing effect and the existence of this effect shows that the subjects succeed to a fair degree in equalizing the frequencies of the symbols. However, they are not aware that they introduce on employing this strategy a tendency to accumulate all remaining differences between frequencies into differences between central and extreme positions. As this effect is related to the position of the symbols it can be eliminated, except for an excessively large number of symbols, by placing the symbols in relatively equal positions, e.g. by arranging them in a circle. Therefore in the present experiment the symbols were presented in a circle pattern.

The aim of the training has been the reduction of the tendency to avoid repetitions and the tendency to balance the frequencies. It is important to stress that the training was not directed to changing the response sequences themselves by teaching subjects random response sequences. As response preferences are caused by the fact that subjects employ a specific choice strategy, based on their interpretation of the concept of randomness, the aim of the training was in the first place to teach them not to be afraid

of choosing sequences which violated their interpretation, and secondly, to teach them not to employ any strategy.

METHOD

The experimenter, who acted as sender, and the subject were situated in different rooms. In front of both a display was placed on which six symbols (colors) were arranged in a circle. Communication concerning commencing and ending a trial were made with the help of light signals. The targets were selected by a random generator. The experimenter recorded the targets, the subject recorded his guesses both on a recordsheet and on tape by pushing a button on his display. One session consisted of 400 trials.

In the first session the subject was instructed only about the ESP part of the experiment, neither the aim of the experiment nor the concept of guessing behavior were mentioned. These topics were extensively discussed after the first session was completed. It was stressed that probably the weak results of ESP experiments with unselected subjects were due to non random guessing behavior, which in its turn was caused by a misinterpretation of the concept of randomness. The aim of the present experiment was to give the participants a better insight into the concept of randomness and to negate non random response behavior. The subject was shown his own response preferences based on the response series of the first session. In addition it was explained to him what criteria were to be applied (chi-square values, etc.) to accept a sequence of 400 responses as sufficiently random. It was pointed out repeatedly that the aim was not to learn a specific way of choosing but to negate wrong habits and to become more free in choosing. It was further explained that in view of the large number of guesses, 400 in all, it was impossible to construct a random sequence in a rational way, and that the only way to meet the criteria would be not to guess in any specific way and not to employ any strategy but to guess as indifferently as possible.

Instructions were given orally and adapted to the individual subject. Each session was preceded by a short session in which the subject made about 75 guesses. This sequence was immediately checked for the number of repetitions. The aim of this training session was to help the subject to surmount his negative attitude towards generating repetitions. All sessions can be considered as normal ESP experiments, the only difference being that the subjects were more aware of the mechanisms resulting in non random response sequences. No information about their ESP scores was given after completion of the experiment. The time lag between two sessions for an individual subject amounted on average to more than a week.

A number of eight sessions including the first non random session was fixed as the maximum number of sessions per subject. In the case that a subject did not meet the criteria in eight sessions, the most "random session" from the last seven sessions was included in the evaluation. The number of participating subjects amounted to 34.

Each response sequence of 400 calls was evaluated by applying the following methods.

a) The frequencies of the symbols were compared to the expected frequencies (MCE=66.7) by means of a chi-square test (zero order effect). This procedure is not entirely correct since mostly sequential dependencies will occur implying that the calls are not independent.

b) The number of repetitions was evaluated based on the assumption that in the case of n=399 and in the case of a probability of 1/6 for a repetition - which is true only if the frequencies of the symbols do not differ much - the number of repetitions is normally distributed. The difference between the observed and expected number of repetitions is expressed in a standard-normal distributed W - score.

c) Up till the 6-th order the matrices of all combinations of two successive symbols were evaluated by means of a chi-square test. This means that if the x-th order is considered, combinations are taken of each call and the x-th preceding call. For the chi-square test it holds, that dependencies between the calls are not allowed. Hence as long as response preferences appear, the application of the chi-square test is not correct. However, this objection is met when for all orders the sequence can be considered as a random sequence, which is the case when all chi-square values are not significant.

It was selected as criteria for a random response sequence that the results of all aforementioned evaluations had to be non significant (P less than .01). Then for the subjects as a group the results of the first session and the random or most random session are compared, applying the S - score distributions to evaluate the differences in the tendency to balance the frequencies (see Schouten 1972). The S - score is based upon the distribution of the frequencies of the symbols in a series of 25 calls. The S - score increases with an increase of the size of the differences between the frequencies.

RESULTS AND DISCUSSION

Six of the 34 subjects (18%) did not meet the required criteria for a random response series within eight sessions. For the 28 subjects

who succeeded the average number of sessions amounted to 4.5 including the first non random session. In table 1 data are presented with respect to mean duration of the sessions, the tendency to avoid repetitions and the first two order effects.

TABEL 1

Mean and standard deviation for duration of the series, W - score and chi-square values of the first two orders for the first series, and the (most) random series of the 34 subjects

		time in minutes	W-score	order 0	order 1
first series	M	29.2	-3.0	5.1	77.6
	sd	15.0	5.2	3.7	40.4
random series	M	19.8	0.3	5.7	31.0
	sd	5.2	1.5	3.6	12.8
critical value			2.58	15.1	44.3

The size and the sign of the mean value of the W - score for the first series shows the familiar tendency to avoid repetitions. The chi-square values for the zero order effect appear in both series to be non significant which can be ascribed to the fact that the symbols were given relatively equal positions by arranging them in a circle. The very significant mean chi-square value for the first order effect in the first series is to a certain extent a consequence of the tendency shown by the value of the W - score to avoid repetitions. Since the average duration of a session is considerably lower in the random series (on average 3 seconds per call) it can be argued that the production of these random series can not be due to the employment of a rational strategy by the subjects.

The S - score distribution, based on a sample of the 4th, 10th and 16th sequences of 25 guesses per subject, appears for the first series to be very significantly different from the expected distribution (Kolmogorov-Smirnov One-sample test Dm = 0.52 ; Dk = 0.16). The S - score distribution of the random series also appears to differ significantly but to a lesser extent (Dm = 0.24; Dk = 0.16). This result is probably due to the fact that 18% of

the subjects did not meet the criteria. Moreover the chi-square test employed for evaluating higher order effects is not very sensitive for detecting a tendency to balance the frequencies. The difference between the S - score distributions of the first and random series showed to be very significant (Kolmogorov-Smirnov Two-sample test; Chi-square = 29.3; df = 2). Although for the group of subjects the random series can not be considered entirely random due to the significantly different S - score distribution, it can be concluded that a considerable reduction as regards response preferences has taken place.

Table 2 presents data about the number of hits in both series. Neither the total score nor the distribution of the scores in

TABEL 2

ESP scores for first and random series

	n	p	hits	d	z	M_{hits}	sd
first series	13600	1/6	2266	0	0	66.65	7.67
random series	13600	1/6	2190	-76.7	-1.76	64.41	8.90
expected			2266.7			66.67	7.45

both series differ significantly from the expected values. In the first series one subject scored at the 5% level. In the random series three subjects scored at the 5% level, of which two exceeded the 1% level. Combining the scores of both series per subject only one subject scored at the 1% level (all levels two-sided).

It can be concluded that at least in this study and in the case of unselected subjects a reduction in response preferences does not increase the ESP scores. This finding implies that the principle underlying the standard method of experimenting in parapsychology, as described in the introduction, is probably not correct. From this it would follow that a transmission model for ESP - a target is transmitted from the sender to the receiver but will mosttimes result in an incorrect response due to 'noise' - is unlikely, since response preferences can be considered as an important source of noise.

Another explanation could be that reduction of response preferences fails to increase the ESP scores in the case of

unselected subjects but does have an effect on the scores of high scoring subjects. This explanation would imply that the generally accepted opinion, that all subjects are in principle capable of ESP, has to be modified.

ABSTRACT

Guessing a target symbol from a limited number of alternatives can be considered as the standard method of experimenting in ESP. The idea which underlies this method seems to be that in principle the subject is equally motivated to choose any of the symbols so that even a weak ESP effect can distort the balance in favor of the target symbol. From this it follows that other variables influencing the choices have to be considered as "noise", and act as inhibiting variables. The phenomenon of response preferences indicates the existence of such variables. It can be expected that reducing the extent of noise enhances ESP scoring. Thirty-four unselected subjects participated in an experiment in which they were trained to generate random choices. Although response preferences appeared to be strongly reduced no evidence was found that this effected the ESP scores.

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ACKNOWLEDGMENT

The nineteenth annual convention of the Parapsychological Association will be held in Utrecht, The Netherlands, from the 18th till the 21th of August 1976. Host to the convention will be the Parapsychology Laboratory of the State University of Utrecht.

A request for papers via the Parapsychological Association will probably have been sent to members and associates of the Association before this acknowledgment has been printed. In addition, a request for papers will be made in the relevant journals. Abstracts of papers and research briefs should be sent to Prof. Martin Johnson, Chairman P.A. Programme Committee (address: The Parapsychology Laboratory, Varkenmarkt 2, Utrecht 2501, The Netherlands). The deadline for the submission of papers is the 15th of May 1976. For the research briefs the deadline is the 15th of July 1976.

Further information about proposed papers and research briefs, as well as details regarding accomodation etc., will be announced at a later date in this and other parapsychological journals, and personally to members and associates of the Parapsychological Association.

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EUROPEAN JOURNAL OF PARAPSYCHOLOGY

The European Journal of Parapsychology publishes original experimental and theoretical papers on parapsychology. In addition, articles presenting reviews of literature, case studies and papers on related topics are accepted in so far the subject can be considered relevant to the understanding of parapsychological phenomena or to the methodology of empirical research in parapsychology. Translations of papers originally published in a language other than English are also welcome.

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(continued inside back cover)

EUROPEAN JOURNAL OF PARAPSYCHOLOGY

CONTENT

ON PUBLICATION POLICY REGARDING NON-SIGNIFICANT RESULTS

Some comments on Dr. J.B. Rhine's article in the comments section of the J.P., 39, No 2, 135-142

Martin Johnson

University of Utrecht

I have read Dr. Rhine's article with the greatest of interest. However, on certain issues in his article, I respectfully disagree. At the most recent Annual Conference of the Parapsychology Foundation, Inc., held in San Francisco in August 1975, Dr. Rhine and I had the opportunity of exchanging our views regarding publication policy.

Normally I would not have felt motivated to criticize Dr. Rhine on parapsychological matters, but since I strongly disagree with him on certain issues, I will be glad to clarify my position. Finally in order to avoid all potential misunderstandings, I would like to make it clear that I have admired Dr. Rhine for his unmatched pioneering work in parapsychology for too long to easily turn critic at this stage. Further, I am indebted to Dr. Rhine in many ways for the inspiring and substantial help he has rendered me over the years. I am not sanguine about my possibilities of changing his opinion, but I hope that our exchange of opinions will be helpful in illuminating problems which I believe are of the utmost importance for our field.

On the strict statistical issue, I agree with Dr. Rhine (see pp 135 - 136 in his article), namely that so far as the test of significance is concerned, one experiment should be considered as independent of another, and the results do not need to be pooled. However, if one accepts this rule, I believe that one should be consequent, e.g. one should refrain from the habit of combining p-values from various, hardly comparable investigations with the intention of demonstrating how well the psi hypothesis is validated by actual findings. This behaviour is not seldom manifested by parapsychologists in their apologetic zest.

Before leaving the statistical issue it should be stressed that all laboratory investigations have both an experimental and a

statistical side. The use of statistics may be more or less proper, but even the most proper use of statistics may lead to spurious correlations or conclusions if there are inadequacies regarding the research process itself. One of these sources of error in the research process is related to selective reporting; another to human limitations with regard to the ability to make reliable observations or evaluations. Dunette (1) says:

> "The most common variant is, of course, the tendency to bury negative results. I only recently became aware of the massive size of this great graveyard for dead studies when a collegue expressed gratification that only a third of his studies 'turned out' - as he put it. Recently, a second variant of this secret game was discovered, quite inadvertently, by Wolins, when he wrote to 37 authors to ask for the raw-data on which they had based recent journal articles. Wolins found that of the 32 who replied, 21 reported their data to be either misplaced, lost, or inadvertently destroyed. Finally, after some negotiation, Wolins was able to complete seven re-analyses on the data supplied from five authors. Of the seven, he found gross errors in three - errors so great as to clearly change the outcome of the results already reported."

It should also be stressed that Rosenthal and others have demonstrated that experimenters tend to arrive at results found to be in full agreement with their expectancies, or with the expectancies of those within the scientific establishment in charge of the rewards. Even if some of Rosenthal's results have been questioned the general tendency seems to be unaffected.

I guess we all can agree upon the fact that selective reporting in studies on the reliability and validity, of for instance a personality test, is a bad thing. But what could be the reason for selective reporting? Why does a research worker manipulate his data? Is it only because the research worker has a "weak" mind or does there exist some kind of "steering field" that exerts such an influence that improper behaviour on the part of the research worker occurs?

It seems rather reasonable to assume that the editors of professional journals or research leaders in general could exert a certain harmful influence in this connection. This may be especially true in areas of research where we find school-builders or paradigm-makers, or by and large in areas where a branch of science has to fight extra hard for its survival and recognition. There is no doubt at all in my mind about the "filtering" or "shaping" effect an editor may exert upon the output of his journal. It seems highly probable that he will be prone to champion his own "school of thought" or his cherished ideas. This implies that he prefers and "rewards" studies supporting his own

previously published findings and cherished hypotheses and tends to suppress the reporting of findings leaning in the opposite direction. Such an editorial policy may create a research climate and "system of values" which could account to a great extent for the phenomena described by Dunnette, Rosenthal, and others. I think that one should note that the very existence of a certain publication policy could influence an author's willingness to submit a paper for publication and his strategy in writing it.

To me the issue on how the publication policy may affect the content and output of the research procedure is a burning one. This is a highly relevant issue in most branches of science and I fail to see any good reasons why parapsychology should be exempted. On the contrary, I think it is especially relevant to parapsychology. As I see it, the major risk of selective reporting is not primarily a statistical one, but rather the research climate which the underlying policy create ("you are 'good' if you obtain supporting results; you are 'no-good' if you only arrive at chance results").

Such a policy, implicitly or explicitly expressed, is in my view bound to exert a distorting influence on its end product, the results. Furthermore I am afraid that the risk of this distorting effect is high in a field like parapsychology where we have almost no positions and where most people are badly lacking the security of employment. The situation is likely to become critical, if job security and "promotion" start to depend upon the outcome of one's experiments. I am not primarily thinking here of the probability of the occurrence of deliberate fraud, but rather of a wide range of more subtle ways of "adjusting" and "improving" one's data to meet the requirement of being statistically significant at a predetermined, but rather arbitrary level. This may involve for instance, re-stating one's hypotheses, small "rationalized" exclusion of data, post hoc analysis until something turns up.

Nevertheless, I do not find it constructive to discuss the phenomenon of unreliable experimenters in terms of "weak" and "strong" minds respectively. I am very well aware that there are educational aspects to the problem, but by and large I believe that the most fruitful way of attacking the problem is to analyze the entire research process and the motivation behind it. To avoid misunderstanding I want to stress that I have never said that a scientist's reliability can not be affected by training in a favourable way, but one should notice that the scientist himself is only one part of a highly complex and dynamic process, the scientific research process.

I have previously made suggestions as to how to improve our understanding of the research process by pin-pointing its risky

links or synapses by the use of the "system approach" (2). The analysis I carried out has had practical implications for the publication policy which we have stated as an ideal for our new journal: the European Journal of Parapsychology. We are aware that the fool-proof experiment does not exist - although much can be done to improve the control of an experiment at different levels. In short, we shall try to avoid selective reporting and yet at the same time try to refrain from making our journal a graveyard for all those studies which did not "turn out". These objectives may be fulfilled by the editorial rule of basing our judgments entirely on our impressions of the quality of design and methodology of the planned study. The acceptance or rejection of a manuscript should take place prior to the carrying out and the evaluation of the results of the study. It also implies that the hypotheses as well as the number of subjects, number of trials, etc., should be carefully specified before the collection of data is made. Another advantage could also be that the research worker would be able to capitalize on criticism before the experiment is firmly outlined.

What I have described as "Model 3 Condition", (that is "optimized automated and intersubjective control of a psi-experiment") in the paper referred to, should relieve the experimenter from the rather dull duty of making registrations, while leaving him free to concentrate on creating a favorable "rapport" with his subjects - a psychological state which is often thought to have a beneficial influence on a subject's psi performance or the psi performance of the interacting experimenter-subject dyad. By the same token it is reasonable to assume that being relieved from the pressure of obtaining "positive" results should not be a disadvantage for producing psi phenomena.

I believe that I have rather good grounds to state that I am not the only person within our field who feels concerned about possible implications of different types of publication policies. Dr. John A. Palmer's idea of establishing a kind of "data bank" is an independently developed idea intended to tackle some of the problems which I have touched upon here. There are several good reasons for supporting Dr. Palmer's idea. Here I will mention just a few aspects.

1) If the proportion of successful experiments is small in relation to unsuccessful ones, this should be made public.

It may well have educational implications. I am under the impression that many an enthusiastic beginner who carries out an "unsuccessful" psi experiment may feel very frustrated and shy away from our field since they may think that they are "no-good" at all as experimenters. They may have got this idea from journals where only the "successful" experiments are published at full

length.

2) On the other hand if it should turn out that a high proportion of experiments were seccessful this too has a number of implications. How does one reconcile such a finding with the notion that there is an almost non-exixtent reproduceability in parapsychology? If the ratio of successful experiments is high, that would imply that we could commence with a more constructive and critical approach of theory testing, perhaps in accordance with the famous Popperian scheme:

$$P1 \rightarrow TT \rightarrow EE \rightarrow P2$$

where "P" stands for "problem"; "TT" stands for "tentative theory" and "EE" for (attempted) "error elimination".

3) If similar experiments (jointly planned and carried out with the same high degree of automated control) were carried out at different research centres and yielded strongly contrasting results (statistically significant between different centres) this would also be very important information. A systematic and penetrating analysis of possible causes for those differences, subsequently carefully tested, seems to me to be a fruitful way of enhancing our knowledge of critical factors affecting the outcome of a psi-experiment. The existence of a "data bank" could be a constructive step in making such comparisons possible.

Dr. Rhine may be right with his belief that because of enhanced knowledge and experience, the proportion of successful experiments is today higher, at his Institute and elsewhere, than it was during his early years at Duke University. This appreciation of the situation may be a correct one, but the notion is to the best of my knowledge unproven. A statistical assessment of this issue will be further complicated if a shift in publication policy has taken place since the early Duke days.

There are several other interesting statements and ideas that Dr. Rhine has put forward in his article which I would like to discuss and challenge, but for the time being I will restrict myself to the points I have already made. Readers whether they agree or disagree with the points I have made here are invited to discuss their views in this journal.

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PRELIMINARY REPORT ON THE AMSTERDAM EXPERIMENTS WITH MATTHEW MANNING

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J.M. Houtkooper

INTRODUCTION

The purpose of this series of experiments was to investigate the value of some testing methods which originated from the Durham-Institute (Schmidt, 1973; Levy, 1974) and which were further developed by the "Study Center for Experimental Parapsychology" in Amsterdam (SCEPP).

The idea has been that an outstanding subject can be considered as a strong source of psi signals. Just as one calibrates and tests equipment in let us say astronomy by using strong signals, we here wished to use M.M. in a similar way. We felt that, based on preliminary reports from the Toronto series (personal communication), the present subject could be considered very capable of producing a strong signal.

Three main experiments were conducted:

1) A PK experiment with direct feed-back of the behaviour of a fast random number generator to the subject.
(Some physiological signals: EEG, ECG and respiration were simultaneously registered)
2) A PK/clairvoyance experiment called "maze" in which the subject had to escape from a maze with hidden barriers which were generated by the computer.
3) A PK/clairvoyance experiment called "conway" in which the subject was asked to influence growing processes which were simulated by the computer.

No overall significant results were found. However during the "conway" experiment the computer program became stuck into a loop. Afterwards this was empirically established to have a probability P .05 for this series. It must be stressed that the subject did not show any significant results on other "classical" experiments either. Therefore no definite conclusions could obviously be drawn about the value of the above-mentioned three experimental

set-ups.

1 EXPERIMENT WITH A RANDOM NUMBER GENERATOR

This experiment tested the ability of the subject to influence a random process by psychokinesis. The random process was generated by a random number generator (RNG) that produced two different numbers (0 and 1) in a random sequence. The task of the subject was to influence the RNG in such a way that, when the target number was 1, the RNG produced more 1's and vice versa. The experiment was governed by a computer, to which a display was attached. On the screen a line was projected that indicated more 0's or more 1's according to its position: more to the left or more to the right. Thus the subject was immediately able to see whether or not he had been successful. Before each run an arrow was displayed to indicate the target number. For a detailed description of our experimental set-up see Bierman & Houtkooper, 1975.

The experiment consisted of three series of runs. The first series and the third series consisted of 18 runs with run-lengths of 4, 6, and 12 seconds. These series were identical in appearance for the subject, but in the third series the random numbers which were to be influenced were already recorded. Thus, the possibility of "retro-PK" was tested.

The results are given in Table 1.

TABLE 1

Results of 1st and 3rd series

Series	Total exp. trials	Deviation	CR	P
1	63805	+33.5	+0.3	N.S.
3	63798	+156	+1.2	N.S.

The second series was carried out simultaneously to the registration of three physiological variables, viz. EEG, ECG, and respiration. This second series consisted of 32 runs: 16 control runs and 16 experimental runs. Run-length was the same for all runs: 15 seconds, with a fixed pause before each run of the same duration.

This series served a dual purpose:

A To set up a methodology for tracing the possible influence of physiological variables on PK scoring.
B To obtain correlations between the subject's scoring and the above-mentioned physiological signals.
The results of the subject's PK scoring are given in Table 2.

TABLE 2

Results of 2nd series and pooled results

Series	Total exp. trials	Deviation	CR	P
2	348051	-162.5	-.65	N.S.
all	475654	-8	-.02	N.S.

As the over-all deviation in the second series is non-significant, different correlations with the physiological variables are not expected to yield very surprising results. We assume here that no psi-missing interspersed with psi-hitting took place.
However it is interesting to note the data about the pulse-interval and pulse-interval variability.

TABLE 3

Pulse-interval and pulse-interval variability (in milliseconds) - during the 2nd series

Period	Pulse-interval		Variability	
	Mean	s.d.(N=16)	Mean	s.d.(N=16)
Pre-control runs	761	45	63	28
Control-runs	759	34	58	22
Pre-exp. runs	759	40	82	32
exp. runs	738	52	50	19

The differences between the control and experimental conditions indicate a tendency on the part of the subject to show a different

pattern of pulse-interval parameters in the two conditions. One of the reasons for this could have been that the subject really did do his best to influence the RNG. However, as shown in Table 2, without any result.

The evaluation of the other physiological data is still in progress. As we pointed out we do not expect to find very significant correlations between this and the scoring. Though, if any correlation is found, a possible explanatory factor might be the subject's emotional response pattern to seeing his own success or failure. However, to take this factor into account or to eliminate its influence is a general problem in correlation studies combined with direct feed-back.

2 MAZE EXPERIMENT

Two maze-experiments were conducted on two subsequent days. Different experimenters, different computers, and a slightly different experimental set-up were used. The first experiment was also intended to be used as a predictor for the second one.

On a visual display terminal a maze (figure 1) was generated. The subject's start position was in the middle. He was able to tell the computer to move upwards, downwards, to the left or to the right by pressing corresponding keys. His purpose was to escape from the maze. This would be an easy task if it were not for the fact that some of the obvious ways out were invisibly blocked for the subject. These hidden barriers were also generated by the computer. For each game a completely new randomized maze was generated. The over-all probability that the subject hits a barrier is 1/3 per trial. Each move is considered to be a trial. The result of the two experiments is shown in Table 4.

Firstly the results are split for response-time. This is the time that elapses before the subject strikes a key to make a move. It can be seen that the best performance appears to take place in trials in which virtually no time was taken to think.

However, when tested by a chi-square test the present finding does not indicate significantly different scoring for different response-time categories. Levy's experimental results meanwhile have been invalidated because of experimenter unreliability (Rhine, 1975), so that comparison with the present results is not very useful.

Secondly the results are split for kind of trials. The reason for this is the following. The subject may react in an obvious way regarding his choices: e.g. he continues in the same direction unless he is blocked. These trials, including those in which there

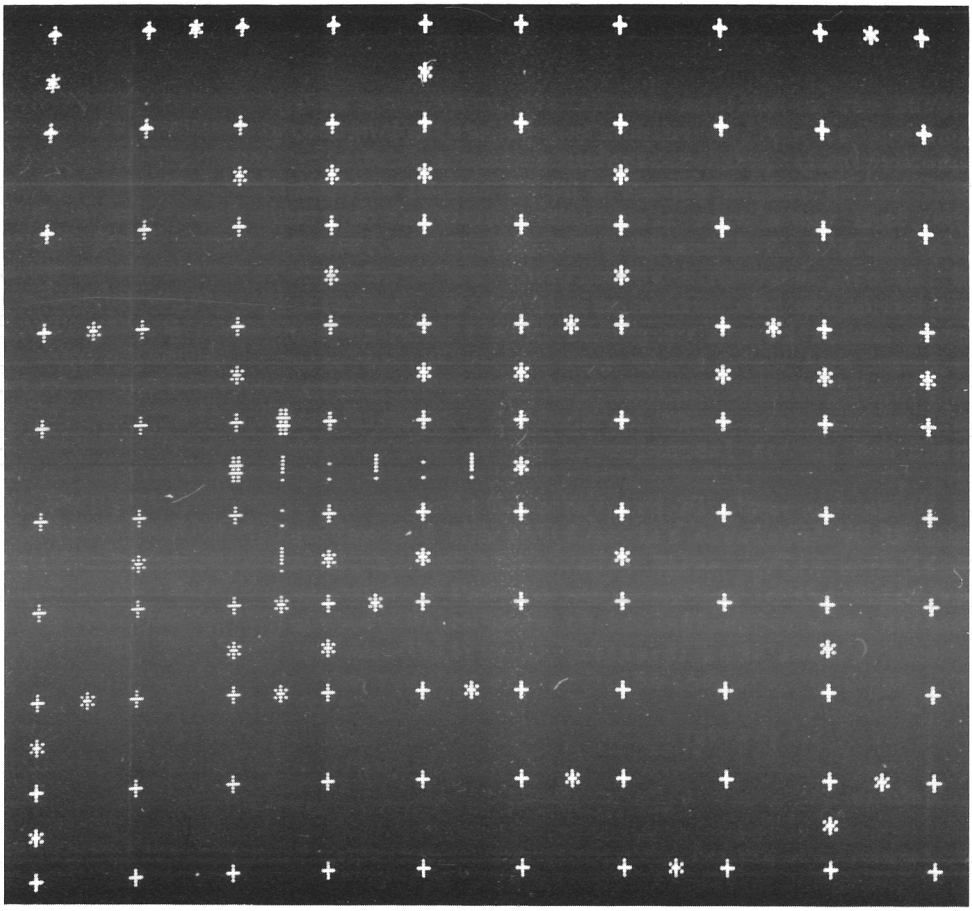

Figure 1
Example of a maze
Subject's route was: left, left, left (hidden barrier), up (hidden barrier), down. He must proceed here with: left, etc..

TABLE 4

"Maze": scoring for different response-times

		First day					
Response time:		0-1	1-5	5-10	10 sec	Total	CR
RBT	No. of trials	31	168	11	6	216	
	Hits	23	116	6	5	150	
	Deviation	+2.3	+4.0	-1.3	+1.0	+6.0	+0.87
All	No. of trials	146	572	26	7	751	
	Hits	103	369	17	6	495	
	Deviation	+5.7	-12.3	-0.3	+1.3	-5.7	-0.44
		Second day					
Response time:		0-1	1-5	5-10	10 sec	Total	CR
RBT	No. of trials	8	138	4	0	150	
	Hits	3	86	3	0	92	
	Deviation	-2.3	-6.0	+0.3	0	-8.0	-1.39
All	No. of trials	170	232	19	6	427	
	Hits	130	149	13	5	297	
	Deviation	+16.7	-5.7	+0.3	+1.0	+12.3	+1.27

is only one alternative (see figure 1), we call non random behaviour trials (NRBT). The remaining trials we call random behaviour trials (RBT). The reason for this procedure is that we expect the subject to sometimes react in a stereotyped way to the particular situation. In these trials we expect no paranormal effect. On the other hand, RBT's are choices that deviate from the obvious and in these the subject is expected to act more probably on his paranormal impression.

The results are not significant. However a further evaluation of the results of the first experiment showed that a calculation of "actual" probabilities of a hit in each different trial yielded higher scoring.

What we here call the "actual" probability of a hit is the probability based on the knowledge of the positions of the invisible barriers. E.g. if the subject makes a choice in a certain

position between, say, two ways, there exist the following possibilities: 1) Both ways are blocked by invisible barriers (actual p=0); 2) One way blocked, one way open (actual p=½); and 3) Both ways open: the trial will certainly be a hit, so the actual p=1.

These findings are now being investigated in the results of the second experiment.

3 CONWAY-EXPERIMENT

The computer generated a random pattern of stars on a visual display terminal (figure 2). Every 3 seconds this pattern was refreshed and changed following a fixed algorithm. If no further changes took place the pattern had reached stability and the run was automatically ended. It was also possible for a specific sequence of patterns to return after some (n) refreshments. In that case too the run was automatically ended. However the program did not anticipate loops with n 4.

There were four conditions:

1a) The subject was asked to keep the pattern changing ("alive" = L).
1b) The subject was asked to "kill" the pattern (= K).
2a) The goal ("alive" or "kill") was given before the subject had started the run (= B).
2b) The goal was given after the subject had started the run (= A).

Hence the combinations L-B, K-B, L-A, and K-A are possible. Condition 2 can be interpreted as follows: The program was written in such a way that pressing of the start key by the subject generated a completely random first pattern. The initial point in the random series which was used was calculated by taking actual computer time.

After this first completely random pattern the following patterns were generated using a fixed algorithn, which means that from that moment the number of cycles (refreshments) before the completion of the run was in principle determined. Therefore in condition 2a the result of the run was not basically determined at the moment that the order was given to the subject, while in condition 2b the result of the run was already determined at that moment, though only visible in the future.

The reason for the introduction of this condition was that from the theory of Walker (1972) one may infer that this condition will not yield different results. In this theory however only the act of observation of the result is important.

The results of the Conway-experiment are given in Table 5. The application of a Wilcoxon two sample test shows the differences in number of cycles between "live" and "die" conditions to be non-significant, though they are in the expected direction. In run 6

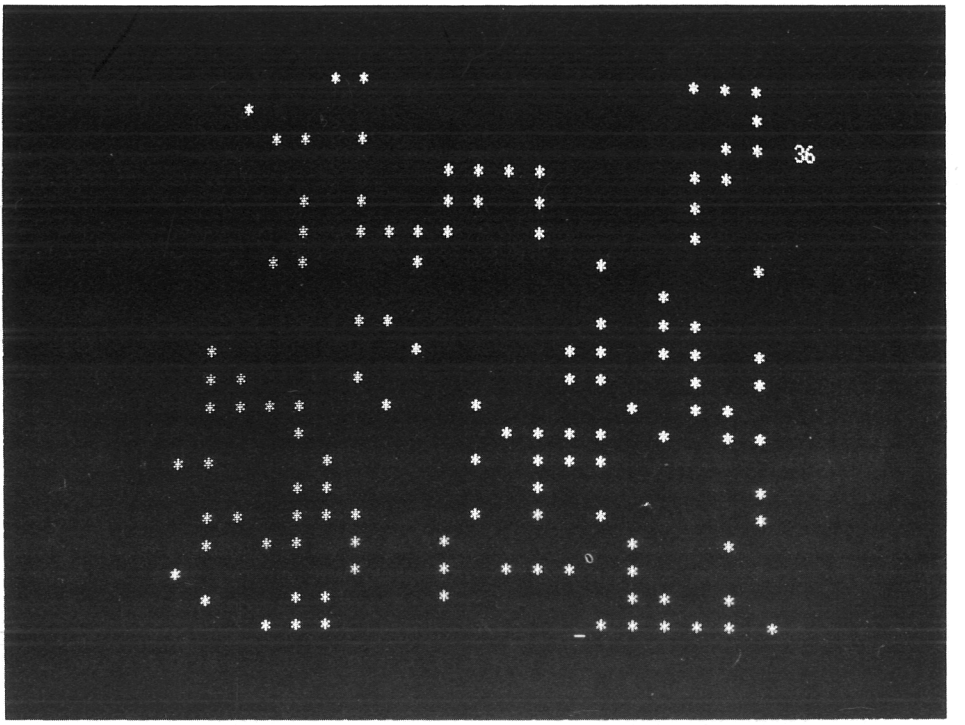

Figure 2
Example of a CONWAY star-pattern after 36 cycles

TABLE 5

Results of the Conway-experiment, Raw data

Game	Cond.	Cycles	Game	Cond.	Cycles
1	L-B	52	2	K-A	57
3	K-B	69	4	K-B	52
5	L-A	89	6	L-B	270'
7	L-B	55	8	K-A	114
9	L-A	143	10	L-B	26
11	K-B	79	12	L-A	239

' infinite loop, see text.

TABLE 6

Results per condition

	Live			Die		
	Cycles	Games	Mean	Cycles	Games	Mean
Before	403	4	101	200	3	67
After	471	3	157	171	2	86
Total''	877	7	125	371	5	74

'' For the difference: Wilcoxon two sample test: W=41, n.s.

the program got stuck in a loop with n=6. This was not foreseen and the run had to be aborted by the experimenter. In that run we used as experimental result the number of cycles before the loop occurred. Afterwards it was empirically found that the occurrence of such a loop has a probability of p = .004 per run. As 7 runs were done in the condition 1a (keep it "alive"), the empirical probability that such a loop will turn up in this condition was estimated at p = .028. The result of this post-hoc analysis should be of interest for further research.

DISCUSSION

The circumstances under which the experiments were carried out were deplorable. The preparation time was less than a month and the subject was very tired on arrival. Furthermore it was obvious that commercial interests played an important role on the subject's part. This caused the serious problem of keeping the press away from the location of the experiments. To validate the results of the experiments of the computer-type we prepared some more "classical" experiments.

These were:

1) An exploratory series of trials on "mental photography" based on the experiments with T.Serios as a subject (Eisenbud; 1970). After 20 exposures without result this series was stopped.

2) A "broken watch-test", see Cox 1974. In this test the watch was blocked by a tiny piece of thin paper. Some days before the experiment the watch was sealed. The subject was asked to make the watch run again. No success.

3) A series of Kirlian photographs was taken by Mr. v.d. Veer with the cooperation of two SCEPP-members (H. Spruit and R. Kattenberg, University of Utrecht). All the phenomena could be explained by normal means according to our observers. However, Mr. v.d. Veer suggested some effects to be present. In this case it is difficult to come to a conclusion because of the lack of an adequate evaluation technique.

Finally we wanted to carry out a metal-bending experiment. However, a week before the experiment we received the message that M.M. did not want to participate in this type of experiment. This contrasts in our opinion with the problem of the small amount of time available for the experiments. We had no complaint about that; i.e. if the subject needed, say, a week to acclimatize in the setting, we were not told beforehand.

It was our purpose to use M.M. for a test of our experimental apparatus and procedures. However, it was therefore necessary that the subject should really represent the strong "psi-signal" which he claimed to be.

As he did not perform very well in any experiment whatsoever, this condition was obviously not fulfilled.

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SOME REMARKS ON PRECOGNITION

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"Sort of like having built-in radar - you see things before you see them. The outlines of coming events."
Truman Capote,
In Cold Blood, p.73

INTRODUCTION

In this paper I intend, from a philosophical point of view, to discuss the concept of precognition.

I begin, by means of an ostensive definition, to explain what precognition is. The ostensive part consists of two case-studies taken from the female paragnost Ms. Eva Hellström's case-books.

I then give a short presentation of Ms. Hellström and her remarkable "dream diary".

After that, I touch upon the question whether precognition can be considered a form of knowledge and a form of perception.

Finally, I go, in more detail, into some of the repudiations put forward against the possibility of precognition.

A summary concludes the paper.

CASES

Two actual cases of precognition provide the grounds for an ostensive definition of the notion of precognition with which I commence my study. In a comment to the above mentioned cases, an explanation is given as to the meaning of the term - ostensive definition.

The precognition cases are of so-called sporadic type and I omit completely the type that is found in laboratory testing through statistical means. I believe, however, that everything I say about

sporadic precognition bears consequence to statistically found precognition.

Case 1

Background

Eva Hellström, a young wife, married to Bo Hellström, Doctor of Technology and Professor of Hydraulics at the Royal Institute of Technology, Stockholm, Sweden, remained at home together with her four children while her husband, due to his career, made long and numerous journeys to all continents of the world. Years later, when the children were grown, Eva accompanied her husband on several of his trips.

From the diary

On January 24, 1953, Ms. Eva Hellström wrote the following.

I sat mending at my sewing-machine. Suddenly, like a flash of lightning, I had a vision of myself walking down a street in Cairo. I feel convinced that Bo and I will be going there soon. Bo says absolutely no. He is to begin lecturing on the nineth of February and the lectures are scheduled to continue for six weeks. By that time, he says, it will be too hot (in Cairo). He has no intentions of going.

Affirmation

This entry into the diary of Eva Hellström is verified by Greta Norrlin and Britta Warbert.

Comments

The precognition took place in daytime while Eva was completely awake but seemingly in a rather relaxed mood. She was eager to note in her diary that this experience was unusual. Firstly, it did not occur in a semi-conscious state as did her previous precognitions and, secondly, it was not in color but in black and white. One of the outstanding features of her alleged precognitions otherwise had been that they, in contrast to her ordinary dreams, were in bright colors. A third and striking difference was that Eva had the vision as a participator and not, as otherwise usually was the case, as an observer. She did not see herself, her own body, walking a Cairo street, but she experienced being there walking along and looking around.

The vision was combined with a strong feeling of conviction that

it soon would happen. Her husband had said that no such journey would take place. None had been planned and there was at that time no reason for believing that one would take place.

On February 3, 1955, Eva accompanied her husband on a flight to Ethiopia. The reason for this journey being that Bo had received an invitation from the Ethiopian Government and although the request came unexpectedly it was not surprising. Prof. Hellström was a well-known scientist and consultant for firms and governments. When assistence in his field was needed, his name was likely for consideration.

At the end of February, on their return journey to Sweden, the Hellströms in fact landed in Cairo and spent a few days in the city. The journey to Africa took place at a time equally inconvenient as the time of her dream - at least from the university point of view concerning lectures and seminars.

Eva, at the time of her precognition, was convinced that her experience would be realized soon. Her husband could not persuade her to think otherwise. Often, it seems to be difficult for a paragnost to be more specific about the time elapse from the presumed precognition to the verification in reality.

For the sake of analysis, that the reader may wish to make, I want to add one more point.

Cairo was not an obligatory stop-station for flights between Europe and middle Africa. On their way to Ethiopia the Hellströms flew from Rome directly to Khartoum. In his autobiography, "Pa jakt efter vita kol" ("Chasing white coal"), Bo Hellström describes this flight in the following words:

> "The journey first went with SAS to Khartoum, the capital of Sudan, and during the flight we experienced a happy surprise: we could sleep in made beds. .... We went to bed in Rome at 10 PM and were awakened with breakfast in bed the next morning at 9 o'clock, one hour before arrival in Khartoum."

Case 2

Background

Eva and Bo Hellström lived in London from 1927 to 1934. Eva became quite accustomed to the English language and the British way of life. She enjoyed the cultural attractions of London and was a frequent visitor of the theater.

In the years following their stay in London, the Hellströms returned often to England on trips of business or pleasure.

From the diary

On the 12th of November, 1961, Eva wrote the following. (This case has previously - without comments or analysis - been reported in the Journal for Psychical Research, Vol.41, No.711, March 1962, pp. 252-254.)

I was having an afternoon nap. I had been asleep but was aroused. What caused me to awaken was the "sight" of a large ballroom where a crowd of teenagers was dancing. The atmosphere was very gay. The girls wore brightly colored dresses. I remember very well quite a lot of turquoise and cerise. Above the youngsters' heads I seemed to see something colored, either some kind of hats or headdresses or balloons or colored lanterns. I could not tell what it was. Last Thursday it was decided that today, November 12th, we should go to London. Perhaps this scene will happen in some form during the trip. I have just now, before leaving, told Bo about my experience.

Affirmation

Bo Hellström has with his signature verified the above entry in the diary.

From the diary (continued)

November 24, 1961: On the plane to London, Bo and I discussed which play we should see there. A stewardess gave us the Sunday Times and we looked through the advertisements. Bo preferred a musical comedy. I found one about which the Sunday Times said: "Best acted and danced musical in London". The name of the play was Bye Bye, Birdie and it was playing at Her Majesty's Theatre.

On Tuesday we were invited out for dinner at Scott's by a friend, Mr. V. Jansa, who had traveled together with us to London. After our meal we went to Her Majesty's to see the play. In the first act - about half way through, I pushed Bo's arm and whispered: "Look, here is the ballroom scene with the youngsters".

The whole play was about teenagers. In this particular scene a lot of young girls, in brightly colored dresses, and some boys were dancing and jumping about. The two lead-girls were dressed in cerise. Several others as well. The background on one occasion was turquoise. Some of the dresses were also turquoise. There was something above their heads which I did not quite see or remember.

After the first act Bo left - he did not feel too well - but Mr. Jansa and I stayed on. During the intermission I told him about my vision. When we returned to the hotel, I made him come with me into the bedroom, where Bo was in bed. I unlocked my

suitcase, took out my diary and read the account to them both. They agreed that the scene had been quite in accordance with my account.

Affirmation

Bo Hellström and Victor Jansa have with their signatures verified that the above statement is correct in every detail. The advertisement from the Sunday Times with the quoted sentence supports her statements.

From the diary (continued)

The day after, I went to the theater to see if there were any pictures outside which would show what it was that I had seen above their heads. There were, however, only pictures of the single actors, no scenes. There was a matinée on in half an hour, so I went in to see the first act again and find out what was troubling me. Two of the girls were wearing a sort of cap with big cerise colored woolen tufts at the top, which showed above the heads of the crowd. Two other girls had rods with tufts of long paper streamers in red and white at the top, which they were waving above their heads. It was such a lively and gay scene and it was extremely full of bright colors.

Comments

This dream or vision took place in daytime but in contrast to case 1 in a way that was customary for Eva: in connection with a rest. She had her precognition when she was slumbering away and it even caused her to awaken. The dream was in colors which play an important role in this precognition. She also sensed a strong feeling of conviction: she would live the dream.

She took her usual measures when she had, as she felt, a precognitive dream: she wrote it down in her diary and had the entry signed by someone else.

The alleged precognition took place four days after. She had the feeling it would be realized in the prospected journey to London.

For further analysis I want to insert three more points.

A This is the only dream among the over two hundred she has taken down that concerns a ballroom scene with dancing youth. As an answer to my direct question Eva said that to her recollection she had never before and never since had a similar dream or vision.

B Bo Hellström was not a specially keen theater visitor. He sometimes accompanied his wife to the theater. He almost never went to the movies. In an interview she said that she was the one who

on this occasion wished to make a theater visit and that she had in mind a "serious" play (as Shakespeare). Her husband wanted to come along only if it were something "light".
C Dr. Jansa's testimony is from a witness psychological point of view very weak. He learned of the dream only after the physical event had taken place. In the theater he only got Eva's assertion that she experienced the ballroom scene before. In the hotel, in a suggestive atmosphere created by Eva, she, not he, read from her diary. It is very likely, but cannot be proved, that she during or after the reading made comments so the written description still better fit the theater scene. He could not for certain know when the diary's words ended and the oral comments began.

Comments on both cases

I have now taken up two precognition cases from the diary of Ms. Eva Hellström. They will illustrate what the meaning of precognition is in an ostensive way. I will explain what I mean by that.

Ostensive definitions are crude ones and do not in a verbal manner clarify what the meaning of the definiendum is. One who has something explained ostensively has to form his own opinion or concept of the defined object.

Now, ostensive definitions are very common and they are the basic way of giving clarifications or definitions. We all have learned to speak our mother tongue through ostensively defining the objects or events around us. When we were children and walked through the park with our parents they stopped at a plant, pointed and said "flower". When this was repeated a certain amount of times we understood that an object of this shape was called a flower. If a four legged animal ran across the path in front of us our parents pointed and said "cat", "dog" or "squirrel" and we could eventually grasp the differences among these animals ourselves - we had to form our own conceptions.

Hence, in the same manner, I have now ostensively defined "precognition". The reader must himself form his concept of the item in question. This enables him to agree or disagree with what I am saying in the rest of the paper.

I have not given a verbal explanation of "precognition". Just as our parents did not verbally explain "cat" and "dog". They could not have done it in an other way. We did not command a language which enabled us to understand an explanation in words.

The ostensive definition can be false in the meaning that what one person defines as a precognition will not be accepted by another person. It may very well be so that some readers will reject my examples as being illustrations of precognitions. Analogically one parent in the park could have pointed to an animal and said: "Look,

a squirrel!" while the other parent could have responded: "No, it's a chip monk!". This is a disagreement about the notion. Such discrepancies are often encountered when one deals with ostensive definitions.

Finally, there are certain elements necessary to constituate a precognition. I wish to point them out.

Firstly, we have a person who in one way or another "images" something (a dream, a hallucination, a fantasy, etc.). This "imagination" is a mental act which is not a direct perception of events surrounding the person.

The "imagination" may or may not be accompanied by a feeling of conviction that the "imagination" will in the future materialize in one way or another. This feeling plays an important role in most of Eva Hellström's reported cases.

Secondly, we have a physical event of some kind that may be described as the realization of the "imagination". Many perplexing questions may arise as how to confirm that the physical event really corresponds to the imaginary event.

Thirdly, in order to call the imaginary event a precognition it must in time happen before the corresponding physical event takes place.

The time span between the dream and the verifying (physical) event is of interest. Obviously it is not possible to wait any amount of time for a dream to come true. Eva's precognition cases come true from within a couple of days to a couple of years. The two cases taken up here illustrate this.

Some of these problems will be analysed further on in this paper.

Eva Hellström and her diary

Eva Hellström was born in September, 1898. Her mother, a concert singer, died in 1908. Her father, a Doctor of Geology, Mineralogy, and Petrography, and also a member of the Swedish parliament representing the Social Democrats, tended to his home and his six children with help of three maids.

Eva married Bo Hellström when she was 19 years old and had four children. Outside the duties at home (raising four children with a husband often out traveling far from home could be very strainous) her greatest interests were music and parapsychology. She founded SSPR (The Swedish Society for Psychical Research) in 1947 and is at present its Honorary Secretary.

In 1949 she began to note in her diary dreams and visions that she thought had a precognitive significance. Her last documented case is from 1964 and all together there are 239 cases. She usually kept a diary on her journeys and it is mostly in connection with

travels that she has reported paranormal experiences.

It is very important that the entries of paranormal character in her diary have been verified by persons at hand (very often her husband) by signing their names to what she had written. In the two cases presented I have,under the heading "Affirmation", taken up the signatures and other material confirming these cases.

The confirmation of the alleged precognitive experiences she has done herself. It has been done in the way that she has had the feeling that this is something that is going to happen, it is going to happen within a certain period and at a certain circumstance. She has then at particular occasions suddenly seen or felt that this is it, "this is the dream I had, so it was a precognition as I thought".

To a great extent, it is possible for us to examine every precognition case by comparing the alleged precognitive vision with the corresponding facts Eva has reported. For the verification of the precognition she has collected signed statements, photographs, letters, paper clippings, etc..

There is no doubt that problems arise in identifying a dream as corresponding to the physical event later on. Some details correspond; some do not. How strong a demand should there be that all details fit? Take case 1 as an example. How should "soon" be interpreted and what importance should be placed on Eva's feeling that she "soon" would be in Cairo. This problem of accuracy is not at issue here.

Of course, what she has written in the diary are very short notes. She did not (and no one could demand it) describe in detail everything she saw in her dream, as for instance the dancing youngsters. She took up what she found most striking and important: that there were several youngsters of both sexes dancing and that the colors were strikingly vivid. This is also what she saw four days later. There is no doubt that she at once felt, when she saw the scene at the theater, that it was the same as in her dream. This feeling of recognition in the two discussed cases is unattainable for scientific examination.

More problems generally arise when an evaluation is done subjectively by the paragnost alone, than when it is done by an appointed researcher. This is something we must bear in sporadic cases. It is virtually only in laboratory testing of precognition that we can beforehand decide when an alleged precognition is supposed to be considered a "hit".

The 239 cases are not all examples suggestive of precognition. Many of them involve alleged telepathy and some of them presumed clairvoyance. How many belong to each category is difficult to say. One must first, to the fullest degree possible, establish which cases may be called genuine paranormal. After a thoroughful

investigation of the entire material more definate suggestions will be made of the statistics of the cases. I am in the process of analysing all the material.

Precognition as knowledge and perception

The questions whether precognition is a form of knowledge and whether it is a form of precognition are briefly touched upon.

Knowledge is an asset that may express itself and that may be used in many different ways. I do not intend to discuss knowledge or cognition here. For the purposes of this article it is enough to presume that we all have a fairly well established common sense understanding of this notion. Although there are philosophers of the sceptical school who deny that there is something we could call cognition, I here postulate as a psychological fact that we do remember things and that we therefore have knowledge. In remembering, recalling, recollecting, recognizing and similar acts we produce, in a mental way, something that stands in relationship to past events. This kind of cognition can be called postcognition or retrocognition.

Can we also recall future events? Certainly not. The words recall, remember, etc. cannot be applied to what will happen in the future, but from this does not follow that we are not able to have cognition of future events. We do not remember the future but we precognize it.

Cognition can be true or false. A statement of a fact as (1) In Amsterdam there is a museum dedicated to Rembrandt.,can be correct or incorrect. In the same way a recollection as (2) Yesterday I ate filled mignon., can be true or false depending on the fact what kind of dish I had yesterday. These sentences we call theoretical.

In exactly the same manner a sentence about the future can be theoretical. In this respect there is no difference between postcognition and precognition.

There are two procedures of deciding the truth-value of a theoretical sentence: the empirical one and the analytic.

To empirically establish the truth-value we confront the content of the sentence in question with reality. Sentences like (3) I will soon be walking the streets of Cairo., and (4) I will during my forthcoming journey to London experience a scene where young people, dressed colorfully, are performing a vivid dance., are of this category. (3) and (4) just as well (1) and (2) can empirically be proclaimed true or false.

An example of an analytic sentence is the following: (5) A bachelor is a married person.

You cannot by examining a bachelor find if he is married or not. It is quite enough to know the meaning of the word "bachelor" to

decide if (5) is true or false.

Precognitions are never of the analytic type.

In establishing the truth-value of an empirical sentence one has to go to the reality using the ordinary senses. In establishing the truth-value of an analytic sentence one does not have to leave the verbal situation and in this sense one could here talk about a non-sensory knowledge. In this case it is not a question of perception.

In epistemology it has been established as a general law that empirical investigation is sensory (requires the use of the senses) for observing facts and that analytical investigation is nonsensory (you need not use senses for fact observing). Now, precognition is clearly empirical but is it sensory?

Well, this question is indeed difficult to answer. The term ESP (extra sensory perception) implies that precognition is a perception; however, one not using the ordinary perceptors as the eyes, the ears, etc., but other perceptors. What these can be has so far not been satisfactorily clarified. It is highly doubtful that we have unknown perceptors; and, if we do not, then precognition cannot be a case of ESP, since then there is no such thing as ESP. This does not mean that there is no precognition, telepathy, etc.. It only means that ESP is a term that diverts us from proper thinking and hence a term that ought to be replaced with a more appropriate one.

Theoretical sentences

		analytical	empirical
sensory	ordinary	none	In Amsterdam there is a Rembrandt museum
	extra	none	I will be in Cairo in the near future
nonsensory		A bachelor is unmarried	none

Eva Hellström has had many experiences we call precognition , two of them are recounted in this paper. These experiences are factual. How they should be explained is the issue at stake. They do not have to be conceived as perceptions although she describes them as visions. Hallucinations can also be said to be visions but are

not perceptions in the ordinary sense of the word. Hallucinations are often visions of unreal things and not perceptions of outer stimuli.

Summerizing this section, we then state that precognition is a form of knowledge that we in one way or another obtain, maybe, but not very likely, through unknown perceptors.

I will not be able to tell what precognition really is, but I think I will be able to enlighten the query by examining three ways of repudiating precognition.

Three repudiations of precognition

The three repudiations of precognition I take up here have been called by Professor C.D. Broad: the epistemological, the causal, and the fatalistic.

The epistemological repudiation

To say that a person P at time t1 precognizes the event E is to say that E at t1 has the relation R to P. Formally we can write this in the following manner:

(6) $R (P_{t1} , E_{t1})$

A factual example of (6) would be: On November 12, 1961, Eva Hellström experienced a scene that she would see on November 25, the same year. Now, R can only hold when P_{t1} and E_{t1} both exist. E does not exist at t1 but at t2. This means that E_{t1} does not exist. (6) is therefore nonsense and precognition is an impossibility.

This criticism does not hold. Two different things have been confused. One must distinguish between E and the precognition of E. It is true that E_{t1} does not exist, and E_{t2} will occur later, but what exists is the precognition of E_{t2}.

Psychologically there is no contradiction in saying that a person experiences something that does not exist. Hallucinating is such a state of mind.

There is another way of showing the absurdity of the epistemological repudiation. Suppose that R in (6) means memory (memorizing) instead of precognition (precognizing). Then, if the repudiation of precognition were correct, one could not remember anything. I have already shown the logical mistake in the repudiation. Furthermore one can empirically prove the possibility

of remembering things.

The causal repudiation

Let us describe memory thus:

(7) $R (P_{t2} , E_{t1})$

This could be interpreted as follows. A person P remembers at a certain time the past event E. Let then R be a relation consisting of a series of events from t1 to t2. The series contains, among other events, P's perception of E at t1. We then here have a series of events, one event causing another (see formula 9 below), from the physical happening at t1 to the mental act at t2.

Analogically, perception can be formulated:

(8) $R (P_{t1} , E_{t2})$

This could again be a formulation of Eva Hellström's precognition. R is a series of events starting at t2 and ending at t1. Here we meet a difficulty. The precognition that occurs at t1 precedes in time the precognized event at t2.

Now, according to the law of causality, the cause in time precedes its effect. In the precognition it seems to be the other way around.

The repudiation then goes as follows. E causes P, but comes in time after P. That is an absurdity. It is impossible that E causes P, which means that there is nothing we can call precognition.

If the relation is not a precognition, what is it then? Several different answers are possible. I will in this section mention two of them and the whole of the next section is devoted to a third way of answering the question.

One can deny that there is a relation between P and E. This means that R is nonexistent. Instead, the occurences of P and E are entirely independent. It is just pure coincidence that they resemble or remind one of each other and it only looks like there was a connection.

One can also say that the relation is of a more logical kind. It is a reasoning at t1 that makes one believe that E will occur at t2. If this reasoning is meditated it is called a forecast, a prognose or the like. It is quite clear that these are not precognitions. When you watch the weather report on TV you are not listening to precognitions.

On the other hand, if the reasoning is unconscious it may be confused with or mistaken for a precognition. The unconscious

reasoning could be called intuition or something the like.

Common to both these interpretations of R is that they exclude precognition.

But if precognition is existent as a relation of a non-prognostic or nonintuitive kind, as described above, then we are obliged to consider what sort of relation R is. We have at least four means of explaining R.

(a) R is a causal relation and the effect may in time precede the cause. Take the case of sending a letter from Holland to Sweden, this means that there is nothing surprising in that, on very rare occasions, the letter arrives in Sweden before it is mailed in Holland. This kind of reasoning is not unfamiliar when it comes to micro-cosmos, especially elementary particle theory. You have here the assumption that there are no causal, only statistical laws. As far as I understand it is not adviseable to apply these statistical laws in macro-cosmos. The letter to Sweden will always be mailed in Holland before it arrives at its destination.

In this sense, precognition and causality do not combine.

(b) We can also say that R is of a teleological kind. Now, it is difficult to grasp what a teleological relation is. Aristotle (and to some extent also earlier Plato, and later, for instance, Thomas Aquinas) meant that everything in the universe is governed by teleological relations. An accorn strives to become an oak. Everything strives after fulfillment. The final magnet is Nous or God toward which the whole universe is directed.

I do not see how such an explanation can be applied in the case of precognition. What does it mean that Eva Hellström's vision of dancing youngsters strived after to be like the dancers at Her Majesty's Theatre? By using the teleological explanation we have really said nothing more than - she had a precognition.

To me the teleological explanation is not an explanation, just a way of rephrasing something inconceiveable.

(c) We can say that R is a normal causal relation. That means that it is P that is the cause and that E is the effect. This explanation is logical and conceiveable but very complicated. Let us examine the precognition example two. Eva Hellström has a vision of a ballroom scene. She is also convinced that it is a precognition: She will see this ballroom during her already decided upon journey to London. She now, in one way or another (we do not need to go into the question how), influences her husband to go to the theatre. She also has him to decide on Bye bye, Birdie. The paper they are consulting clearly states that this play contains dancing. It is likely that she will see her ballroom scene in this musical. This means that she by her behavior stears things so they will happen in a way that her vision will be fulfilled. This is all

probable, maybe even likely.

But, a further analysis leads us into trouble. Colors play an important role in the vision. They usually do so in her precognitions. Two colors dominate her vision: turquoise and cerise. These colors are also significant in the theatre scene. Two of the lead-girls and several others are dressed in cerise. Some other dresses are in turquoise and, on one occasion, which perhaps especially corresponds to the vision, the whole background is turquoise.

Could Eva Hellström in one way or another have influenced the dressing of the actors. If they had changed their dresses for this particular performance at which Eva was present, then the theory still holds. This was not the case. The dresses were designed and sewn long before Eva had her vision.

Can she have influenced the designers, directors and so on when the decision of the colors of the dresses were made? In our theory it is possible. But this means that she "knew" about this ballroom long before she had her vision.

In this theory Eva influenced people. What about material? Did she make the colors change? Very unlikely. But what about a precognition of the following kind.

P has a vision and conviction that a certain pendulum is going to fall from its place on the wall. A storm comes, rocks the house and the pendulum falls. The storm is the cause of the fall of the pendulum, but is then P the cause of the storm?

It is easy to see what an extremely difficult theory this is to uphold. Different paragnosts cause all sorts of happenings and activities in the world. But the world does not seem to be that chaotic.

To explain precognition in terms of PK is possible but it is a highly improbable theory.

The causality theory can be elaborated on in another way. I will take up that in the next section.

(d) Finally, we can in sporadic cases try to explain precognition in terms of other psi-factors as clairvoyance and telepathy.

We may say that the vision of the streets of Cairo and the vision of the ballroom in London were clairvoyance or that someone at these places telepathically sent these visions to Eva. But we cannot explain her feelings of being at these places as clairvoyance or telepathy.

Other psi-factors as clairvoyance and telepathy take care of details, they never explain the entire issue of precognition.

The fatalistic repudiation

The fatalistic repudiation is based on the assumption that man has

a free will. If man has a free will or not, is one of the big questions in Philosophy and one which has been debated for more than two thousand years. The assumption seems basically founded on psychological grounds. When Eva and her husband wished to go to the theater in London they had a choice among several plays. They had the feeling that they could choose to go to Her Majesty's, or if they had preferred, they could have gone to the Old Vic or to the Globe.

Eva Hellström (P) had at time t1 a vision of a dance that would take place at t3 when she was in London. The event in Sweden we call E1, and the event in London (seeing the dance) we call E3. A necessary condition for E3 at t3 to happen is that Bo Hellström at t2 prefers to go and see a musical and that he had his wife choose Her Majesty's. This happens on the plane from Sweden to England and we call it event E2.

The fatalistic argument now runs as follows. Since E1 is a precognition, i.e. E3 occurs, then E3 must have been predestinated at t1. But since E3 depends on the choice at t2, then also E2 must have been predestinated at t1. This means that Bo at t2 did not have a free choice, even though he thought so.

We can illustrate this in the following formula:

(9) $E1 \rightarrow E2 \rightarrow E3$

where $\rightarrow$ means causes.

This is a universal formula. Every event is caused by an earlier event.

From this we can draw the conclusion: Precognition exists if, and only if, fatalism is the case.

Now, according to our first assumption in this section, fatalism is not the case. Hence, there is no precognition.

We may of course instead accept fatalism as reality. Then there is no problem with precognition. Our case can be illustrated as shown in formula (10):

(10)
$$\begin{matrix} E1,1 \rightarrow E2,1 \rightarrow E1,3 \\ E1,2 \rightarrow E2,2 \rightarrow E3,2 \end{matrix}$$

The events represented by E ,1 are for example these:
E1,1 - a decision is made to play Bye bye, Birdie
E2,1 - decisions are made about scenes and costumes
E3,1 - Bye bye, Birdie is played at Her Majesty's
This chain of events could have been made much more elaborate, but that does not change our line of reasoning. The E ,2 events

can be interpreted like this:
E1,2 - decision to go to London
E2,2 - precognition of E3,1
E3,2 - confirmation of alleged precognition

This means that E3,1 and t3 does not stand in a causal connection with E2,2 at t2 and the precognition does not have to be explained.

There is also another and more complicated way of seeing this, and this is the more elaborated way of causality that I in the preceding section promised to take up.

There could be a third series of events, that we are not aware of, causing the precognition. Formula (11) illustrates this:

$$(11) \quad E1,3 \rightarrow E2,3 \begin{array}{l} \nearrow E3,1 \\ \rightarrow E3,3 \\ \searrow E2,2 \end{array}$$

E2,3 causes E2,2 and E3,3. In its turn E3,3 causes E3,1. Again the precognition at t2 has no direct causal connection with E3,1 at t3. In this case we have no difficulties in "explaining" precognition but it does not help us to understand precognition cases since ex definitione we are dealing with causality chains that are hidden for us. It is a matter of taste if we call this a fatalistic precognition or not. It is entirely a philosophical problem and I will not consider it here.

The conclusion of this section is that precognition from a fatalistic point of view is both fully understandable and explicable.

SUMMARY

In this paper I have discussed some aspects of precognition. I have not tried to give an explicative or stipulative definition of precognition, but I have as a starting point given the notion an ostensive definition and thereby used two cases from the diary of the Swedish paragnost Ms. Eva Hellström.

Then, I shortly cast some light on the question whether precognition can be considered a form of knowledge and come to the conclusion that it has to be taken as such if it will be of any use for parapsychologists. On the other hand, whether precognition is supposed to be a form of perception can be disputed.

Finally, I take up three ways of repudiating precognition: the epistemological, the causal, and the fatalistic.

I come to the conclusion that the epistemological repudiation does not hold. On the other hand, the causal one seems to bear effects as to the notion of precognition. However, if natural laws,

which are the standpoint of modern science, only are the utmost consequences of statistical laws, then precognition is both psychologically conceivable and logically compatible. But, and this has to be stressed, in that case precognition as an occurent factor in real life, is extremely unlikely. This means that most, but not every single one, of the cases that today are ostensively defined as precognition have to be explained otherwise.

If we, as in the traditional natural sciences, accept causation as the driving factor, then precognition can be the case in two different ways.

Pro primo, precognition may be mistaken for special cases of psychokinesis. It is the precognitive event that causes the event that verifies the precognition.

Pro secundo, precognition may be the case in a world where every event is predestinized. This means that the hypothesis of man's free will has to be refuted. On the other hand, however, this is entirely outside the purpose of this paper to show so, the fatalistic world, at least on its macro-level with working psychic minds, is very improbable.

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NINA S. KULAGINA: A STRONG CASE FOR PK INVOLVING DIRECTLY OBSERVABLE MOVEMENTS OF OBJECTS[1]

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BACKGROUND, PURPOSE, AND PREPARATIONS

A number of publications have already dealt with reports and evidence from movie films (Herbert, 1970a; 1970b; 1970c) which suggests that Nina S. Kulagina has been able to move static objects of some substance (up to 500 g. weight) on many occasions apparently with a high degree of control. Much of the original evidence was mainly based on unpublished reports by Russian scientists (Sergeev, 1970a; 1970b; 1970c; 1971). However in recent years a number of scientists from the West have also reported firsthand observations (Herbert, 1973; Pratt & Keil, 1973; Ullman, 1971; 1974) and although these took place under circumstances which cannot be regarded as laboratory conditions, it was possible to control a number of variables and to reduce the probability of alternative explanations. B. Herbert's report (1973) suggests that all counter hypotheses for some of the phenomena observed can be rejected with confidence. Although we agreed with B. Herbert's findings we attempted in a further visit to verify the paranormal nature of the phenomena in a perhaps simpler manner, rejecting in

1) After some discussion the authors agreed that there is some advantage in a new terminology distinguishing between PK which can be recognized on the basis of statistical analysis only and PK which is directly observable. The latter has recently been referred to as PK on static objects. This term seems to restrictive because PK may be directly observable even if an object is in motion before on account of PK this motion is changed or stopped.

particular again any suggestions that invisible threads might have been responsible for the movements but we also wanted to obtain a good movie film record of the phenomena.

Our visit to Kulagina did not come at the most convenient time for her and initially there was some general reluctance as well as some expression of frustration on her part suggesting that she was somewhat tired of these demonstrations which she had carried out for years and which had not led to an unequivocal acceptance of her abilities.

However gradually Kulagina became more and more positively motivated and she demonstrated movements with objects about 20 to 30 times over a period of time lasting about thirty minutes. The attempts to move objects were interrupted for periods of about 5 to 10 minutes on two occasions when Kulagina tried to create a sensation of heat by placing her hand on J.F.'s and J.K.'s arms.

We had brought along the following objects for the first time:

--One clear plastic cube (about 10 cm long on each side as sold in some stores for the display of photos) with a non-magnetic cylinder resting on the bottom surface and being fastened to one wall with a very weak thin expansion spiral steel spring. The spring was nevertheless strong enough to slide the cylinder back to a certain range of positions. Kulagina had moved this cylinder by itself previously for J.G.P. (Pratt & Keil, 1973). The cube has one open side which was usually at the top.

--One clear plastic cube (as above) with one open side and with a table tennis ball suspended (with a similar steel spring) from the centre of the top.

--One compass weighing approximately 35 grams.

The function of the steel spring would be fairly similar to that of a thin rubber band. (The steel spring was purchased at a novelty shop as one long piece from which a brightly coloured plastic bird was suspended)

Throughout the whole demonstration J.F. was able to watch Kulagina without any obstruction, or other tasks which might have diverted his attention. He remained within a distance of 70 to 100 cm from the objects throughout the demonstration. H.H.J.K. was to some extent occupied with taking movie films and particularly on account of some difficulties with photo lights was not always able to observe all phenomena. All demonstrations were carried out under good illumination conditions and most under more intensive photo lights bright enough to take 25 DIN movie film.

For a brief meal prior to the demonstrations, the table had been covered with a white (with some patterns) table-cloth which remained on the table throughout. There was no suggestion that the tablecloth could hold invisible threads and plates in connection

with the meal, were moved quite normally over the area of the table within which Kulagina later carried out her demonstrations.

Initially Kulagina made a number of short attempts to move the objects in the cubes without definitely being successful. It is very likely that at times she managed to set the table tennis ball in oscillatory motions by paranormal means. However, part of the movements were probably normal movements with the ball not being completely at rest to start with. It is also possible that her hands movements coincided with small body movements which in turn via chair, floor and table were transmitted to the table tennis ball keeping it in an oscillating motion. Nevertheless, it is quite likely that some of the oscillating motions were initiated, increased or sustained by paranormal means. But since it is difficult to separate the normal aspects of these motions we do not regard these pendulum motions as particularly important.

When Kulagina herself was not quite satisfied at an early stage with the response of the objects in the new plastic cubes we put the compass which we had brought along on the table and she was very soon (this refers to a time period of perhaps one minute or less) able to move not only the compass needle but the whole compass itself. This was the first clear sign of a movement of a static object. Kulagina also proceeded to move an inverted small glass placed inside an inverted large wine glass. When it became clear that she was at least able to move the kind of objects which she had moved before, we intentionally created a pause suggesting that she should try to produce with her hand a heat sensation on J.F.'s arm while H.H.J.K. could use this break to set up his movie camera.

It seemed inappropriate in terms of the general psychological conditions to start filming from the very beginning when Kulagina was still uncertain to what extent she might succeed. While such a complete film record would obviously have been more desirable we did not wish to take the risk that filming at too early a stage might inhibit further attempts.

H.H.J.K. had prepared a photo lamp attached to the camera which seemed strong enough for normal colour films and he had also checked out its two pin plug on Russian power points. However, the available power point was in use for other appliances and a three-way outlet with a free opening was too small. Two other photo lamps which we were able to secure and which fitted into the outlet solved the problem initially. However they had to be placed in a fixed position with somewhat unstable clamps. Unfortunately both clamps slipped off after a short time and both photo lamps broke. H.H.J.K. was able to use the camera photo lamp instead of one of the stationary ones but mainly on account of a

fixed reflector being in an unsuitable position for the camera lamp, the illumination was somewhat marginal. In spite of this approximately 60 ft. of super eight film gave a fairly clear record of some of the phenomena. Actually the events filmed turned out to be technically reasonably adequate. Nevertheless starting with the filming after some movements had already been achieved, and on account of the light problems only about one quarter to one third of the total phenomena were recorded on film.

MOVEMENTS RECORDED ON CINE FILM

As the film presents the clearest record of these events they will be described first.

1) Movement of inverted glass about 65 mm x 35 mm put over a green wooden object inside a larger inverted wineglass. The smaller glass moved from one edge of the larger glass to the other, a distance of approximately 20 mm.. The movement occurred towards Kulagina, with one short interruption approximately halfway. On the line of movement, that is, between the glasses and Kulagina was the compass which did not show any movement as one might expect if an invisible string had been used. The glasses were in a position about 25 to 30 cm. from the edge of the table. The green round wooden object situated inside the smaller glass was probably a pepper or salt shaker. No movement of this object relative to the smaller glass was noticed. The green object was resting against the end wall (with respect to the direction of movement) of the small glass; hence as soon as, and to the extent to which the small glass moved, the green object moved as well. If it was argued that the green object had a steel interior and that it could be moved with a strong magnet from underneath the table, then it was precisely in a wrong position to produce the movement of the glass which actually took place.

2) The whole compass housing situated almost right at the edge of the table, made several turning movements in a counter-clockwise direction, turning approximately 70 degrees and also sliding somewhat closer to Kulagina, perhaps 15 mm..

3) The table tennis ball was first moved down and then towards Kulagina (the open side of the cube being on Kulagina's side and the cube being situated close to the edge of the table). This movement was slow and can in no way be explained on account of oscillations which were discussed above. The beginning of this film sequence shows the ball already touching the bottom surface or almost doing so. This can be judged from the position of the ball and its reflection on the bottom surface. After a short period of resting on the plexiglass surface directly beneath the

point of attachment of the spring, the ball moved a very short distance (approximately 5 mm.) towards Kulagina and then made two or three vertical movements without quite returning to its normal suspension position. The ball then made contact again with the floor of the surface and continued to slide on this surface towards one side of Kulagina until it reached almost the edge of the cube.

It must be kept in mind that the ball is normally suspended about 20 mm. above the bottom surface. Consequently even when it appeared to rest on this surface a force was necessary to keep it there. The small vertical movements referred to above may correspond to positions between jerking movements when Kulagina moves say a glass on a horizontal surface. If this comparison is justified, it follows that during these stop positions a force may still be active (although not enough to continue the sliding movement) because the table tennis ball did not return to its normal position. In other words, the force was still expanding the spring to some extent. The spring was considerably expanded when the ball still in contact with the floor of the surface was finally moved almost to the edge of this surface. As soon as the force holding the ball was released the spring jerked the ball back with a quick movement. Although the angle from which this film was taken was not quite ideal it seems impossible to duplicate this movement using a string to pull the ball without creating a noticeable difference in the movement of the ball. Apart from the difficulty of attaching a string to an object moving about at the slightest touch and being continually in full view of J.F. and H.H.J.K. it seems very difficult and it may indeed be impossible with a string to produce a slow movement both straight down and towards the edge.

4) Movement of the small green object and of the small inverted glass discussed under (1). The green object was situated approximately 25 cm. from Kulagina and moved in a direction towards her. In the line of movement was the small glass, the nearest edge being approximately 4 cm. away from the green object; and further in the line of movement was the cube containing the cylinder. The nearest edge of the cube was approximately 15 mm. from the small glass.

The green object moved in a fairly straight line towards the small glass (i.e. towards Kulagina). After the green object touched the glass they both moved together 15 mm. further until the glass touched the cube. There was no noticeable movement of the glass or of the cube while the green object was moving by itself and there was no noticeable movement of the cube when both the green object and the glass moved together. The total movement

occurred in several stages consisting of 3 to 5 more or less clearly separated jerks. The way the movement of the green object together with the glass occurred, it is possible the glass was pushed along on account of the green object and not moving on its own. Following on from the discussion under (1) it could be argued that this movement could have been produced with a magnet from underneath the table if the green object contained an iron center. While this possibility is mentioned there was not the slightest indication that anything like that took place. A very substantial magnetic force would have been required to act in this way from underneath the table.

5) Movement of the small inverted glass alone inside an inverted wineglass. The situation was essentially the same as under (1) except that the green object was not involved at all. So magnetism can be ruled out with certainty.

FURTHER MOVEMENTS OBSERVED BY BOTH INVESTIGATORS

6) Movements of the table tennis ball different to those described under (3) but also of a kind that could not be explained on the basis of perhaps normal pendulum movements. When the ball was moving like a pendulum (approximately 15 mm. either way from the central position), on two separate occasions an additional fast movement occurred, knocking the ball right against the side wall with an audible noise. It is also difficult to see how this movement could have been faked with the use of an invisible string. However, this movement was fairly fast and it was difficult to observe whether the ball touched the sidewall in the middle or perhaps closer to the edge toward the open side.

Other movements were more of the kind as described under (1), (2), (4), and (5).

SOME CONSIDERATIONS FOR THE FUTURE

While no field study even under excellent conditions can be regarded as absolute proof, the observations with respect to some of the details discussed as well as with respect to the overall impressions of the total performance gave both J.F. and H.H.J.K. very strong confidence to regard the observed phenomena as genuine occurrences of PK.

Particularly since Herbert (1973) had come to a similar conclusion from the point of view of a physicist, it seems now desirable to conduct research with Kulagina in a somewhat different manner. Up to the time of Herbert's and this investigation a major consideration was always to insure that the movements are genuinely of paranormal origin. While future work should also try

to maintain controls to the extent it is possible to do so, it seems reasonable to assume that the movements produced by Kulagina are indeed based on PK. Consequently it seems desirable in the future to test the dimensions of PK as well as other theoretical aspects for which a suitable test can be devised. For this purpose it would be desirable if all scientists who are likely to visit Kulagina in the future could keep in touch with each other and with anybody else who is interested in the PK and related theoretical considerations.

To find an important question that could be answered under the limitations that exist during such a field study, is probably as important as the field study itself. Moreover as indicated above it seems highly desirable to present Kulagina with a motivating challenge rather than with the request to produce the same phenomena once more.

EFFECTS ASSOCIATED WITH KULAGINA'S "LAYING ON OF HANDS"

It seems in order to describe in more detail the heat sensations mentioned earlier. There is naturally more uncertainty whether these sensations and the associated observations properly belong to the area of parapsychology. Nevertheless, there is a strong suggestion from the way Kulagina uses her hands during a PK demonstration that they may play an important part in her ability to control to some extent the movements of the objects. Consequently a clearer understanding which may be reached about the heat sensations could provide useful information for the basis of her PK performance.

Herbert (1973) had already described that his own sensation was one of unbearable heat and pain which he only continued to endure for the sake of the scientific value such an experience could provide.

J.F.'s experience was similar although perhaps not quite as severe. He felt heat and pain after approximately 30 seconds. The sensation increased in intensity until after 2 to 3 minutes it became almost unbearable. The pain was real but not quite like pain caused by a hot object under normal circumstances.

A section of movie film showing J.F.'s arm after approximately 15 minutes clearly shows red burnt patches as one might expect them on account of sunburn particularly if the subject had little previous contact with the sun. These red marks were still visible several hours later. J.F. was amazed that they had disappeared on the following morning. There were no blisters or other negative aftereffects which J.F. had expected.

While Kulagina put her hand on J.F.'s arm a mercury thermometer

at body temperature was placed directly between Kulagina's hand and J.F.'s arm. The thermometer showed no change. According to Kulagina this seems to be in agreement with the theoretical expectations of Sergeev. We have no details about these but there is no doubt that heat sensations can be experienced on account of physiological changes other than an increase in temperature over an area of the body. H.H.J.K. experienced a similar heat sensation which increased to a level which seemed utterly real and unpleasant but which did not increase beyond this level. Hence no particular effort was required to endure this sensation for a period of several minutes.

Kulagina had a piece of lead approximately 2 mm. thick and 4 cm. wide with which she partly shielded her hand while touching H.H.J.K.'s arm. The subjective sensation was quite definitely that the lead blocked whatever produced the heat sensation. However, it is possible that the lead as a relative cool metallic object simply produced a cooling effect rather than a shielding effect. If the lead acted as a complete barrier then the shield should remain effective over a long period of time. For the length of time that these demonstrations were carried out (about two to three minutes) this seemed to have been the case. However, it is possible that the cooling effect of the metal could have remained effective over a similar period.

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THE PARADIGM OF THE RHINEAN SCHOOL
PART 2. THE CONCEPT OF SCIENCE

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A group of researchers share a similar view of their own activity as investigators, and also of the position of their science in the world of sciences. They have a common conception of how their discipline was born, developed, and what it will look like in the future. They also believe in certain rules for carrying out research.

Basic to the concept of science is the theory of knowledge, an understanding of the foundations of knowledge. However there are also normative conceptions of what science should be, what theories should look like, or which criteria one has to use in the search for truth. The normative part may be called the model of science. It is a value system. Investigators often look at a superior science and obtain their categories and perspective from this. Since the 17th century most investigators have used physics as a model, as it is supposed to treat the deepest level of reality. Therefore the history of psychology can roughly be depicted as a sequel to the physical sciences with a delay in time.

In parapsychology there have been a lot of theories and concepts modelled on physics. Obvious examples of physical analogies are aether-models, radiowaves and psi dimensions in space, but the S-R experimental model is also a physical model. Analogies have a heuristic value, as they put questions and prescribe new results. However they also narrow the perspective: the physical model - thinking in parapsychology has not lead to a better understanding of psi as a psychological process. At best it has given the field a scientific status.

A characteristic feature of a school-builder or paradigm-shaper is his occupation with problems of methodology and perspectives of science. This can also be said of Rhine. Both his books and his editorials in the Journal of Parapsychology are rich sources for one's understanding of his concept of science. From the 1930's

until today he has repeatedly discussed these problems and often modified his point of view.

As research progressed he has commented on the position of parapsychology. He regards the development of a science as a slow accumulation of facts and he can often announce an attainment of a milestone or a breakthrough in a certain area: 'Now we have enough of evidence to consider ESP proven, now it will suffice for the existence of PK'. When he began in the early 1930's at Duke he regarded himself as a pioneer. He was aware of what had been done earlier in the USA and England, and also to some extent in Germany and France, and some other European countries.But little of this research could withstand his criticism. He had to start from the beginning, fresh, and as he hoped, without any presuppositions as to the existence or nature of psi phenomena. In 1940 he was able to summarize the first step in the research programme: the existence of general ESP was settled (Rhine, 1940). The amount of 'facts' increased each year and Rhine attributed their meaning to the growth of scientific knowledge: Now we could acknowledge the dependence of psi on motivational factors, and now the existence of internal patterns in the ESP scoring, e.g. decline effects or U-curves.

Rhine has insisted more than anyone else, that parapsychology is an experimental science; that it uses truly objective scientific methods. He has been anxious to make it clear that parapsychology belongs to the scientific disciplines, and that it even adheres to higher standards of experimental control than most behavioral sciences. In a propagandistic manner he uses the expression "The Scientific Method" (TSM). He conceives of this as established and self-evident rules of inquiry. With the help of these, an investigator can easily decide upon what is true and what is not. It is the only way of arriving at truth. It is universal and 'generalized to fit any field of problems'. But it is an 'idealized framework', as in reality you can never achieve this completely. In "The Reach of the Mind" (Rhine, 1947) Rhine gives as examples of principles within TSM ten commandments which one must obey in order to be a faithful adherent of TSM: One must not start with unwarranted assumptions! One must not stick to one's own favorite hypothesis! One must not design experiments which provide no crucial tests for one's hypothesis! One must not draw premature and inconsistent conclusions! Etc..

Of course no serious investigator would object to these general formulations. But there are some very ambiguous terms within the framework of Rhine's rules, e.g., 'unwarranted assumptions', 'crucial tests', the meaning of which has to be analyzed. I believe that the concept of TSM is a common one among most

scientists trained in the positivist tradition. But as Feyerabend (1970) has shown, it is a false concept. It is based on the assumption that irrespective of the nature of reality, or the peculiar characteristics of the investigated phenomena, one can always apply some variant of TSM. One always needs some critical acumen, logical ability, and carefulness, and we can state this in the form of rules, but the way in which one uses them depends on the nature of the problems one faces. There is always an interaction between instruments and properties in the reality one wishes to detect. Thus all methods are based on some assumptions as to the nature of reality. There are no fixed rules or principles which create a demarcation between science and nonscience.

There are inductivistic components in Rhine's concept of science. He believes in "facts", i.e. results reached by way of TSM. These facts are infallible and are true foundations for a structure of scientific principles. One set of facts only admits one conclusion, a conclusion in favour of a certain theory which is said to explain the phenomena. "Empirical data" obtained in an "objective" manner are often regarded as free from interpretation and infallible, as the foundational units of science. Other parapsychologists share this conviction: "facts remain what they are, whether or not they are recognized as such", "facts are facts whether or not they fit into belief systems" (Rao, 1966).

In an interesting paper on "The importance of parapsychology to William McDougall", Rhine explains how the evidence of psi has refuted "the logic of mechanistic biology". Psi experiments afford a crucial test for deciding between a mechanistic and a vitalistic biology. However Rhine does not discuss the problem of many biologists, whom he would consider mechanistic, emphasizing other types of experiments which they regard as evidence against a vitalistic conception (Rhine, 1971).

There are no immediately crucial experiments. The history of science contains an abundance of cases where experiments, with the help of hindsight, have been viewed as crucial in defeating an erroneous scientific theory, e.g. Galileo's kinematical experiments. But only history can determine this, not contemporary scientists.

Eighteenth century chemistry held that a certain substance (or principle), phlogiston, was expelled from materials when they were burned or calcinated. In 1775 the French scientist Lavoisier carried out an experiment in which he proposed to show that phlogiston did not exist; that combustion could be explained by the recently discovered oxygen. However this was what Lavoisier saw in his experiment. Other chemists held on to the phlogiston theory with the help of ad hoc explanations. Priestly in fact, was even able to devise a counter-experiment as convincing as

Lavoisier's in support of the phlogiston theory. Two crucial tests had been carefully carried out and both had been successful, yet they pointed to contrary explanations. The old phlogiston theory was abandoned after some time, but not because of lack of evidence. Rather, it explained too much without being specific enough to allow possible refutation and further, a new concept of science had gained ground in chemistry with Lavoisier, a concept in which quantitative evidence was more important than qualitative. (Toulmin, 1957).

In the Rhinean tradition crucial experiments have been important. But there is no consensus of opinion amongst parapsychologists as to which experiment constitutes the most crucial evidence for ESP, and critics have not been convinced by even the best controlled experiments. It seems that experimental evidence is not enough for the acceptance of unrelated and theoretically unexplained phenomena.

Every field of investigation has a specific territory. In addition, and as special feature in the field, parapsychology faces difficulties greater than most other disciplines as there exists no general agreement amongst the researchers as to the existence of psi, the territory of parapsychological research. The fundamental question is not whether theories or interpretations are valid in parapsychology, it is concerned with the actual existence of the phenomena. It is a unique situation, like denying the existence of matter for the physicist or the existence of macrobes for the bacteriologist!

The research plan for Rhine in the 1930's limited his study to establishing the reality of the territory. The main problem was existential: "Are there any psi phenomena?". He did not ask whether all individuals could manifest ESP but whether it was possible to locate it in just one case. The existential question can only be tested in a confirmatory way; it is irrefutable. It would seem that only one strict confirmation will be sufficient to test it, but as we know there are no crucial tests.

The existential question - does ESP exist? - is similar to the ancient query "does the Isle of Atlantis exist?". Suppose we should go looking for Atlantis. We detect a piece of land at sea but it is impossible for us to go ashore and explore it. How do we know it is Atlantis? From its position we can eliminate all other known islands. In this situation we have defined Atlantis negatively (just as with ESP): Atlantis is an island not identical with a, b, c, etc.. We still do not know if it is Atlantis or just a hitherto unknown piece of land. Rhine in (1940, p. 15) thought it possible merely to investigate the existential question without asking anything about its nature. He tried to avoid

unsupported assumptions and presented neutral definitions of his terminology. But how much do you in fact know when you have discovered something which you do not know anything about? The popularizer will immediately accept the mythological connotations of Atlantis and regard it as the discovery of the real Atlantis. When we succeed in investigating the island in more detail, we are unable to do this without presumptions. As Mario Bunge (1967: I, p.178) states, "a problem may be well-formed but its background may be defective or just vaguely indicated".

> No question is ever posed without presupposing something. Since there is no question without a background, and since the background may be constituted by falsities or just controvertible ideas, the naive acceptance of a question without examining its background is no better than the naive acceptance of an answer without examining its ground.

Therefore, when on our unknown island searching for the truth of its nature, we ask questions and pose problems, we do this knowingly or tacitly in the context of the old Atlantis mythology.

The occupation of the Rhinean school and of other researchers too, e.g. Soal, Tyrrell, with the problem of the existence of psi phenomena may be the main reason for the common stress on confirmation in testing parapsychological theories. Rhine has always insisted that it is only positive, i.e. significant, findings that count. The editorial policy of the Journal of Parapsychology has disregarded insignificant results, if the study did not contain some innovations of method or other "illuminating observations". "There are obviously too many ways of going wrong in the search for delicate capacities such as ESP and PK for us to draw any conclusions from a failure to obtain significant results", or "all he can say is that under such conditions he obtained no evidence of psi, and there is no reason to publish that" (Rhine, 1950). Just recently Rhine has defended his standpoint in a detailed discussion (Rhine, 1975).

There is a common belief in the Rhinean school that a psi investigator needs two qualifications. He must have the proper methodological training in The Scientific Method. He also needs to be a subtle personality in order to motivate subjects enough to produce psi. Therefore some researchers may never succeed in obtaining significant results. Critics have sometimes interpreted this thus - if one has to believe in psi to investigate it, this believer will be inclined to accept looser experimental conditions.

It is obvious that very good reasons must be given for not publishing insignificant results and this is certainly not the case before the hypotheses of lucky-unlucky investigators have been tested. The Rhinean theory is well worth a serious try. Recent

studies on experimenter effects give it credibility (Rosenthal 1966).

There are several possible reasons for negative psi results. They may arise from bad research work, i.e. psi was present but the investigator could not detect it, or psi was not present because the conditions were not favourable for it. To blame the investigator for not having elicited psi is to expect him to be too much the magician. But if the conditions - and experimenter variables may be included - were very similar to previous ones, the investigator may have refuted earlier results. Insignificant results must not be neglected in a discipline where the significant results are relatively few and unrelated. To consider the possibility of falsification is important, the paradigm is not to weaken and degenerate. It may be wise to be careful with the "facts" one has, but it is also necessary to tolerate refutations.

The common attitude to negative results reminds me of the psychologist who wanted to test the hypothesis: Psychotherapy can cure schizophrenia, an impossibility many psychiatrists would say. He reported success in a paper, where the effects of psychotherapeutic treatment had been quantitatively evaluated. The psychiatrists were quite surprised, but not as regards the results. Obviously their diagnoses were wrong, the subjects could not have been schizophrenics!

Early on Rhine engaged himself with the problem of repeatability. This is a stumbling-block for many researchers and much has been said and written about it both within and outside the field (Murphy, 1971; Rhine, 1954). The critics have demanded a truly repeatable experiment before accepting parapsychology as a science: A specification of necessary and sufficient conditions for the occurrence of psi phenomena, so that any competent investigator can reproduce earlier experimental results with approximately the same outcome. Often, repeatability is discussed together with the general demand for intersubjectivity. But to define objectivity as intersubjectivity is a statistical conception of truth which can be proven false in cases where all investigators repeating or observing phenomena share the same wrong assumptions as to the nature of the phenomena. In that case it is not objectivity but collective subjectivity. In the 18th century many investigators reproduced experiments purporting to show the existence of phlogiston and they were able to specify many necessary conditions for its appearance.

Of course, successful reproduction of psi experiments is desirable, but not for the sake of scientific recognition. Rather, it helps us to predict phenomena and to control them in order to

determine the process of psi itself. It is not the starting point of scientific investigation, but the conclusion. To achieve complete repeatability would imply to have knowledge of the sufficient conditions, and there are a lot of established phenomena in respectable sciences where we are not familiar with all the sufficient conditions, e.g. for pregnancy. The more one understands of the total context of the event the more repeatability one will have.

There is no repeatable experiment in modern parapsychology. In some areas there is some hope of a breakthrough, e.g. in animal psi or in the relation between motivational factors and psi. Some parapsychologists have claimed that it is futile to search for repeatability as psi is spontaneous. Of course the psi process can have randomness as one of its properties, but the fact that we so far have no repeatable experiment does not prove this. Perhaps the phenomena can only be repeated with a certain probability.

Psi phenomena are often described as spontaneous, but this is not identical to irrepeatable. It may suggest that we should give up looking for mechanical explanations and turn to a more teleological mode of explanation. Therefore we have to view the phenomena as purposive, as an expression for a person's life needs. Nevertheless, the phenomena can be controlled, but not in a technological way from the outside.

How do parapsychologists view their field in relation to other sciences? What is the place of parapsychology in the system of sciences? It is evident that the Rhinean school considers parapsychology as a very unique science. It is a border science but also a frontier science. It is the only science which has as its main territory, the Mind. Further as it is a science of anomalous phenomena it is also a revolutionary science. It is the anomalous character of psi phenomena that has caused so many controversies in parapsychology. Parapsychologists have, on the whole, preferred to stress the controversial character of the phenomena rather than trying to harmonize their findings with existing knowledge and values.

Rhine has defined parapsychology as "the study of those phenomena attributable to personal agency which in some degree transcend physical explanation". Therefore, "it is obviously a branch of psychology". He has considered it "only a matter of time until parapsychology is fully integrated with general psychology", an integration in which however, parapsychology will cause a re-orientation of psychology towards the study of mind, not just behavior (Rhine, 1949).

Rhine did not succeed in establishing parapsychology as an academic discipline, nor did he succeed in integrating it within

psychology. He left the Department of Psychology at Duke, and after the foundation of FRNM parapsychology had no formal connections with Duke University. His view of the systematic place of parapsychology also changed following this. In 1967 he summarized the relations between psychology and parapsychology through the century. Most psychologists were still as hostile to psi research as in the late 19th century. Rhine no longer believed in an unification, but stressed the independence of the two fields. Parapsychology has its own territory, its own problems and methods, and it ought to have its own recruitment and training. Parapsychology has become a specialty. In its stress for autonomy it has underrated the importance of close conceptual bonds to the psychological sciences (Rhine, 1968).

The Rhinean paradigm was born in the 1930's at a time when the behaviorist school had attained a dominant position. No doubt Rhine borrowed much of the behavioristic concept of science. He used the S-R-scheme as an experimental model. Parapsychologists started to look for a connection between variations in stimulus inputs (targets) and response outputs (guesses). In his early investigations Rhine used an objective definition of ESP, approximating to operationalization. He demanded strictly objective methods of observation and statistical evaluation. During the 1940's he worked with large groups of subjects and looked for intergroup differences, using averages for each group. These are also recognizable patterns in the neo-behaviorist paradigm of the 1930's.

Of course it would not be fair to label Rhine as a behaviorist. He did not look at behaviorism as an exemplar. On the contrary, William McDougall, his "teacher" and benefactor, was much more the ideal researcher for him. McDougall defined himself as a vitalist in the battle of behaviorism in the 1920's. But we also have to note that he in fact defined psychology as "the science of behavior" and did not oppose the positivist concept of science held by the behaviorists. It was the behavioristic world-picture which McDougall could not accept. When Tolman argued for strict observability in psychology, he also considered concepts like mind and soul to be unscientific: "An organism's private mind, if he have any, can never be got at". So if Rhine was a methodological behaviorist (together with Tolman and Lashley) his concept of science would clash with basic assumptions within his world-picture. He wanted to make parapsychology the Science of the Mind and he tried to verify the concept of mind by objective means. Finally, behaviorists have an empiristic concept of knowledge. But is it logically possible for a parapsychologist to be an empiricist, to think that all knowledge is derived from our senses, and still claim positive psi results? Our concept of science

depends on the concept of knowledge and this in its turn on the knowledge of man. It is possible that the concept of ESP involves some bad connotations; the psi process does not have to be perceptual. Yet if the evidence for psi is valid the informational capacity of man seems to differ considerably from what we are normally inclined to believe. Modern philosophers of science have used some arguments from and been influenced by the image of man developed within gestalt-psychology and psychoanalysis. However, the consequences of parapsychological research for our concept of scientific methodology have not yet been investigated.

Nevertheless, a few parapsychologists have recently questioned the experimental model, where individuals are regarded as closed passive systems manipulated by external factors. "The experimenter can by no means be considered as an uninvolved spectator" (Schmidt, 1974). Parapsychologists should be more aware that the methods they use limit the results or rather, lead to limited aspects of reality. Intentional and motivational aspects of the psi process have, for example, not been studied until very recently. Moreover, a phenomenological perspective may prove a valuable compliment to parapsychological research.

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AUTONOMIC PSYCHOPHYSIOLOGICAL REACTIONS TO SENSORY AND EMOTIVE STIMULI IN A PSI EXPERIMENT

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Although psychophysiological studies in ESP are still occasionally reported the interest in this subject seems to have diminished somewhat. At present Tart's statement, written in 1963: "If one may speak of 'fashions' in parapsychological research, the study of physiological correlates of extrasensory perception is probably the most current" is no longer valid.

This is particularly true for studies in which changes in psychophysiological variables of the organism, like skin resistance, EEG, etc., are utilized as an indicator of the operation of psi. Yet only eight years ago Green (1967) discussing the application of electro-physiological techniques in ESP research, expressed the view that "hitherto such apparatus has only been used as an indicator of ESP - a procedure which, though valid in itself as a way of registering unverbalisable ESP responses, does not advance our knowledge of the conditions under which ESP takes place".

Objecting to this seemingly one-sided interest, she stresses the importance of studies of the psychological state of the subject by means of psychophysiological techniques at the time of attempting ESP.

Green's statement creates the impression that at least in the beginning most psychophysiological studies utilized the psychophysiological response as indicator for ESP. However, even for the period under discussion this generalization does not seem entirely correct. As early as 1963 in a survey of literature by Tart (1963), it shows that of a total of 8 studies mentioned, only three belongs to the category in which the psychophysiological response itself functioned as ESP indicator. Nowedays this trend has become even stronger, the majority of research in this area being concentrated on the study of physiological states associated with enhanced psi performance.

There is general agreement among parapsychologists that psi is

basically an unconscious process. Hence many studies in parapsychology have tried to enhance psi performance by manipulating the state of the subject, in order to increase the influence of the contents of the unconscious on the behavior of the subject. Amongst others, hypnosis, relaxation techniques, altered states of consciousness, etc., are applied to this end. However, regardless of the type of state we want to induce in our subject, the moment will always arrive when experimenting when we require the subject to give a response; that is, conscious behavioral performance on the part of the subject, and ultimately the evaluation of the results will be based on these responses.

Apart from the possible distortions between the mental activity as for instance experiencing an image, and the overt behavioral act of describing this image, because of the necessity of observable behavior the subject has to change into a different, and relatively more active state. Thus when taking part in an experiment and forced to give responses, the subject is also forced to change his psychological state each time. Moreover, this will constantly remind him of the fact that he is a subject and that he is taking part in an experiment. One can argue that this aspect, i.e. the mere necessity of the subject to give behavioral responses, is a basic weakness common to all these studies.

However this objection can easily be met in psychophysiological studies. That responses, and even 'unconscious' responses, can be recorded without requiring observable behavior on the part of the subject is an unique property of the psychophysiological method.

The two applications of psychophysiological techniques, studying physiological states or applying the psychophysiological response as ESP response, are not mutually exclusive. In fact, they can easily be combined in the same experiment provided one selects the type of stimulation which draws a predictable psychophysiological response from the percipient. The latter, i.e. the nature of the stimuli to be applied, poses a problem which in my view is the main obstacle for the application of these methods at present.

In view of the above, it is surprising that relatively little interest has been shown to applying psychophysiological variables as response indicators of psi stimulation. Psychophysiological recordings permit the subject to relax and to concentrate on whatever he, or the experimenter likes, without having to perform any behavioral actions. Besides this, many practical disadvantages associated with other ways of responding, such as response preferences, etc., are automatically eliminated. Another advantage of this method is, that in some cases such unique responses are obtained that the experimenter can show that these single responses, according to some accepted criteria, have been due to some ESP process. This enables much more detailed process-analysis than when

the analysis has to be based on, for instance, those runs in which the subject scored above chance expectation.

Of course, the psychophysiological method has also certain disadvantages. The main objection against this method is the relatively unspecified nature of the response. Another, although minor one, is the somewhat threatening aspect of applying electrodes. A third objection is, that the range of possible targets which can be applied in these experiments is rather limited. As stated above, it is at present not even known which is the optimal type of target to apply in such an experiment. The investigation presented here deals mainly with this problem.

When reviewing the literature, limiting ourselves to those psychophysiological studies in ESP in which a psychophysiological variable has been utilized as an indicator of ESP, little is found as regards the effect of different types of targets on the psychophysiological reactions of the percipient.

Hettinger (1952) was one of the first to apply the psychophysiological response in an ESP experiment. He reported that percipients showed a higher amount of GSR activity when the agents were sensorially stimulated. However, he presents so few details about these experiments, that a proper evaluation of these findings is hardly possible.

Figar (1959) recorded the plethysmographic reactions of two subjects simultaneously. Unfortunately both subjects were located in the same room, only separated by a curtain. The experimenter showed one of the subjects a card with a two-digit number, and requested the subject to multiply this number with another number. During this mental activity the subject indicated the period involved by pressing a button. Then vascular reactions in both recordings were compared to see of they coincided in time.

He found that both during the rest period and during the trials a high number of parallel reactions appeared. Although he did not "claim the results mentioned as proof of the existence of a so-called telepathic transference" Figar felt that "the proportion of positive cases - - was relatively high".

West (1959) in a re-analysis of Figar's data divided the recordings into 10 second intervals and calculated the expected number of chance coincidences between the recordings of agent and percipient, based on the assumption that the spontaneous deflections in both recordings were randomly distributed in time. He concluded that the observed coincidences in the rest periods were five times higher than expectation while those in the periods of mental arithmetic concentration were $2\frac{1}{2}$ times higher than expectation. Hence it is shown that the amount of parallel reactions was relatively higher during the rest periods than during trial periods.

Nash & Nash (1962) criticized both Figar's and West's treatment

of these data. They explained the differences in coincident reactions between rest and trial periods as being due to a measurement artefact. Applying the same criteria for measuring a reaction in both conditions they found a non-significant difference in number of coincident reactions in both conditions and concluded "there is no indication of a greater frequency of coincident vasoconstrictions in the resting periods than in the calculation periods".

Whatever the reason might be for the observed high amount of parallel reactions, most probably being the result of sensory stimulation shared by both subjects, it can be concluded that the mental effort of calculating did not noticeably increase the number of parallel reactions.

Figar's study was replicated by Nash & Nash (1962) who excluded every possible effect from coinciding sensory stimulation by placing the subjects 7 miles apart. The commencement of a session was synchronized by telephone. However the ten pair of subjects did not produce any significantly deviating number of coinciding reactions, and hence this study did not confirm Figar's finding.

Plethysmographic reactions as psi responses were also utilized in a series of studies by Dean (Dean; 1962, 1966, 1967, 1968, 1969; Dean & Nash, 1967; Dean & Otani, 1971). Dean was the first to apply systematically different targets in such a study. He presented the agent with names of persons, who were either relevant to the agent or to the percipient, or were unknown or neutral to both of them. He measured the size of the vasoconstrictions under these conditions, but only of the percipient.

Dean found that names known only to the percipient or names known to both the agent and the percipient yielded significant reactions from the percipient, but names known only to the agent did not. His main conclusions were that "the largest responses occurred when the name-card presented to the agent contained a name known to the percipient only; these responses were significantly larger than those occuring on blank cards" (Dean,1970). Dean also observed that names presented to the agent should preferably have an emotional meaning for the percipient.

Hence in these experiments the successful targets consisted of the agent looking at names, which most of the time held no significance for him but only for the percipient. Since the results are based on a comparison of the reactions of the percipient under this condition with his reactions under the condition when the agent looked at names known only to himself, or looked at blank cards, it is difficult to assess exactly what in this case should be considered as the relevant aspects of the target. At least it can not be the emotional stimulation of the agent, since the names which yielded the percipient's reactions

were entirely unknown to the agent and had no significance at all to him.

Furthermore it is of interest to note that Dean sometimes observed stronger reactions in the percipient's recording when the agent was presented with blank cards rather than when presented with cards containing names. For instance, in Dean (1968) ten of the total 32 pairs of subjects participating showed such a "reversed" reaction pattern.

Esser et. al. (1967) used Dean's method - with the addition of a sound-proofed and shielded room for both agent and percipient - and also reported significant results.

Tart (1963) attempted "to find changes in skin resistance, finger pulse volume, and the EEG, which could be considered responses to randomly occuring stimuli which could affect the subjects only if they employed some form of psi cognition". The percipients in this study were only required to respond to the presence or absence of stimuli. They were told that subliminal stimuli would be presented at random moments, and that their task was to guess when such a stimulus had been presented. In the shock-condition the agent received electrical shocks at random moments, in the non-shock condition the current was fed into a resistance.

In the evaluation of each trial in the shock or non-shock condition a control block of equal length was determined in the inter-trial period. The comparison of the number of reactions in trial periods versus control periods showed that the number of finger pulse responses in both shock and non-shock conditions was significantly higher than in the control periods, and the same pattern was manifested in the GSR responses although falling short of statistical significance. It should be observed that the differences between number of responses between trial and control periods was larger in the non-shock condition than in the shock condition. As regards the EEG analysis, fewer alpha frequencies were observed in both conditions than in the control blocks.

Thus in this experiment it can be seen that the actual stimulation of the agent by administering an electrical shock did in itself not result in a significantly greater number of reactions from the percipient, but that, as far as the plethysmographic reaction is concerned, both experimental conditions did influence the percipient. One wonders whether it might not be more plausible to assume that the experimenter who controlled the distribution of the current to either the percipient or the resistance was acting as the real agent.

Another important aspect would seem to me to be, that in this experiment a possible psi influence was manifested in the plethysmographic reactions rather than in the GSR.

Sanjar (1969) studied coincident autonomic activity in closely

related pairs of subjects. The subjects were not given any information regarding the purpose of the experiment. Simultaneous recordings from agent and percipient were taken. Stimuli presented to the agent were a mixture of interviews, humorous postcards, color projections of surgical procedures, and sound stimuli. Although he observed coincident reactions, the author concluded: "it would appear that other than the long-term trends in level of autonomic activation, there was no co-variation that was not explainable on the basis of chance or of a common response to some external stimuli".

Beloff et.al. (1970) used in principle photo-slides of the subject himself as target stimuli. Subjects were schoolboy volunteers. When the subject was actually presented with the stimulus, in order to make sure that the stimuli were sufficiently emotive to the subject, significant GSR reactions were noted. However, when the agent was presented with the same stimuli, there was absolutely no sign of any carry-over in the extrasensory condition. According to Beloff (1974) Mackintosh, who likewise used the GSR as his index and studied student volunteers who were friends, also failed to obtain positive results.

Haraldsson (1970) applied Dean's technique and presented the agent with either a blank card or a card containing a name with some emotional value to the percipient. Ninety-four pairs of subjects were studied, classified as either 'close' (related cq married) or 'casual' (friends cq acquaintances). A hit was scored when the percipient's plethysmographic recording showed greater activity in response to the name than to the blank, a somewhat surprising scoring method in view of Dean's and Tart's findings. Although no overall significant scoring showed, it was observed that the scoring correlated significantly in a positive direction ($r = .95$!) with the degree of emotional content the name held for the percipient.

Another effect noted was, that those subjects who started the experiment acting as agents scored significantly higher when acting as percipients than those subjects who started the experiment acting as percipient. This finding suggests that familiarity with the target material might be helpful to the percipient.

This review is limited to those studies in which either GSR or plethysmographic reactions to psi stimulation are studied. Hence recent studies with for instance, evoked potentials (Lloyd; 1973) are left out of consideration.

From the studies discussed a rather confusing picture emerges. The results suggest that when presenting different targets, it is important to compare the percipient's reactions during the

stimulation of the agent with his reactions in a control block, when the agent was not stimulated at all, rather than with his reactions during a period when the agent was presented with supposedly neutral stimuli.

As far as more specific conclusions are concerned, it is obvious that the results of the studies discussed so far are difficult to compare. Different types of targets, on the whole either 'names' or sensory stimuli, have been applied, while on the other hand both GSR and plethysmographic reactions have been used as a psychophysiological index for an ESP response. Yet a systematic comparison of the effect of different types of target stimuli and different psychophysiological response mechanisms is lacking.

In view of the above it was decided to carry out an experiment in order to study the effect of presenting both sensory and emotive stimuli to an agent, while recording the GSR and plethysmographic reactions of the percipient. In order to be able to compare the results of this study with the studies mentioned above it was decided to utilize names, meaningful to either agent, percipient, or to neither, as emotive stimuli (N Condition). Strong auditory signals were applied as sensory stimuli (NR Condition). In view of Haraldsson's finding that percipients familiar with the target material scored significantly higher than percipients unfamiliar with the target material, a condition was introduced in which the percipient was also occasionally stimulated with the same auditory signals as were presented to the agent (R Condition).

No hypotheses were formulated. It can only be said that the experimenter expected a differential scoring with both types of target stimuli.

METHOD AND PROCEDURE

Thirty-three pairs of subjects took part in this study. All subjects were first and second year university students. One subject of each pair was randomly assigned the task of agent, the other one acted as percipient. The percipient was seated in a sound-proof, electrically-shielded room, designed for use in sensory-deprivation studies. Adjacent was the monitoring room, while the agent was seated in a third room.

Finger pulse volume was measured by means of a photo-electric device placed on the top of the middle finger of the percipient's left hand. The electrodes for measuring the skin resistance variation were placed on the left hand at the base of the second and fourth finger. The output of the skin resistance device and the plethysmographic reactions were recorded on a Elema-Schonander polygraph. It should be noted that at the time of carrying out this

experiment no apparatus was available to measure the basic skin-resistance level. Only short-term variations could be recorded.

Prior to the experiment, agent and percipient each provided five names of persons who were emotionally meaningful to them. Besides this, five neutral names were chosen from the telephone directory.

Both subjects were told that the purpose of the experiment was to investigate ESP transmittance between agent and percipient. The general design of the experiment was explained to them but no mention was made of the specific conditions under investigation. The agent was told that he would receive certain loud auditory signals by earphone and that he would have to concentrate on certain names which one of the experimenters would present to him. In both cases the purpose was that the percipient should become aware that he had received one of the stimuli.

The percipient was told that he would during a certain period occasionally be presented with a loud tone by earphone and that he would be informed at the beginning and at the end of the period. In between and in the remaining part of the experiment the agent would be presented with a tone or would concentrate on one of the names. The purpose of the experiment was that the percipient should react physiologically during these periods. It was stressed that ESP is basically an unconscious process and that the percipient himself would not become aware of his own physiological reactions. The percipient was advised not to try to guess at which moment the agent would be stimulated, but to relax as much as possible. His body would do the work for him.

In order to diminish habituation both agent and percipient were instructed to press a button as soon as possible after having received the auditory stimulus.

The experiment was divided into two parts. In one part five sound stimuli were presented to agent and percipient and another five sound stimuli only to the agent (R Condition). The stimuli were presented in random order and at random moments. After or preceding this period unknown to the percipient the agent was presented with five sound stimuli (NR Condition), also at random moments. In the other part of the experiment the fifteen names were presented in random order to the agent with 30 second intervals between each presentation (N Condition). The percipient was not informed about the moment of onset of this condition, nor about the length of the time intervals between the presentation of each name. He was led to believe that all stimuli would be presented at random moments.

The order of the conditions was randomized over the sessions. In each session a random generator was applied to determine at which moments, varying from 20 till 60 seconds after the preceding stimulus, and in which order, the stimuli would be presented.

The time of presentation of an auditory stimulus was three seconds,

that of presenting each of the names to the agent 10 seconds. The moment of starting the presentation of a stimulus was recorded on the polygraph. At least two experimenters took part in each session.

RESULTS

The strength of the skin-resistance reaction depends on the level of the basic skin-resistance at the moment the reaction takes place. An equal stimulus will yield different deflections on the polygraph in the case of different basic skin-resistance levels. Therefore the GSR reactions in a ten second interval after the onset of the presentation of a stimulus (the ESP interval) were compared to the occurrence of GSR reactions in a ten second interval immediately preceding the ESP interval (the Control interval). It was assumed that except for possible ESP influences the basic skin-resistance level could be considered to remain constant during a 20 second period.

As a criterion for a GSR or plethysmographic reaction it was agreed that there should be an upward and a downward slope for a deflection in the interval. The strength of the reaction was measured by taking the vertical distance in mm. between the lowest point of the upward slope and the top of the deflection. The deflection should be at least one millemeter in order to be counted as a reaction. For each pair of ESP and Control blocks it was determined in which interval the strongest reaction took place.

For each condition the number of strongest reactions for both intervals was compared by a Sign Test. A more sensitive test, like for instance the Wilcoxon matched-pairs, signed-ranks test could not be applied, since the difference in millimeters between the strength of the reactions in each pair of intervals did not represent the same strength of difference in physiological reactions. In the case of GSR reactions, the values of the basic skin-resistance level at the beginning of the interval were not known. In the case of plethysmographic reactions, although the differences in millimeters between the reactions in each ESP and Control interval represented a same amount of difference in reaction strength as long as the same subject was concerned, such was not the case when dealing with differences in reactions between subjects. The reason for this is the difference in amplification of the apparatus between subjects.

Table 1 presents the number of stronger reactions in the ESP and Control intervals for the pairs of intervals in the various conditions for both type of measurements.

Since the majority of zero differences between an ESP and Control interval were caused by a lack of reactions in both intervals, it

TABLE 1

Number of stronger reactions in ESP and Control intervals

Condition	R			NR		
	ESP	Con	CR	ESP	Con	CR
GSR	12	41	3.85&&	20	30	1.27
Plethysm.	62	58	.27	55	65	.82

Condition	Agent			Names relevant to Percipient			None of both		
	ESP	Con	CR	ESP	Con	CR	ESP	Con	CR
GSR	22	28	.71	32	25	.79	29	32	.26
Plethysm.	75	60	1.20	74	50	2.07&	73	52	1.79

&&: $P < .001$ (two-tailed) ; &: $P < .05$ (two-tailed)

can be concluded from the size of the total number of differences between ESP and Control intervals, that random deflections showed more often in the plethysmographic recordings than in the GSR recordings.

The results show a difference as regards the effect of the type of stimulus on the psychophysiological response.

The sensory stimulation of the agent by presenting the agent with strong auditory signals resulted in significantly lower GSR reactions from the percipient in the ESP intervals of the R Condition. The same trend is noticeable in the results of the NR Condition, although in this condition it does not reach a statistically significant level. However, the difference between the R and NR Conditions is not significant ($\chi^2 = 2.85$; df = 1). The pooled results of the R and NR Condition are therefore not surprisingly also strongly significant (CR = 3.74; $P < .001$).

On the other hand, none of the conditions in which the agent concentrated on a name yielded significant GSR reactions, and the pooled results for these conditions are also not significant (CR = .88).

It should be noted that the effect on the GSR in the R Condition, and to a lesser extent in the NR Condition, resulted in weaker reactions in the ESP interval than in the Control interval. To

check for the possibility that this was caused by an excess of stronger reactions in the Control interval, a comparison was made between the original Control interval (C1) and a second Control interval of equal length (C2), preceding C1. The results are presented in table 2. Although a weak trend can be observed - the number of stronger reactions is higher in the interval preceding the ESP interval than in the C2 interval - the differences do not reach a significant level.

TABLE 2

Number of stronger GSR reactions in C1 and C2 intervals

Condition	NR			R			R + NR		
Interval	C2	C1	CR	C2	C1	CR	C2	C1	CR
	22	27	.57	23	31	.95	45	58	1.18

The differences in GSR reactions between ESP and C1 intervals did not reach a significant level in either of the conditions in which the agent was presented with names. However, as regards the plethysmographic recordings a reversed pattern can be observed. Although none of the conditions yielded a significant difference in reactions, a common trend can be found in those conditions in which the agent was presented the names. In these conditions stronger reactions are found in the ESP intervals. The pooled results are significant (CR = 2.90; $P < .005$; two-tailed).

DISCUSSION

The results of the present study seem to confirm the conclusion, formulated at the end of the introduction, that it is better to base the evaluation on a comparison of scoring between ESP and Control blocks, than on a comparison of scoring in different ESP blocks in which different types of targets, for instance 'emotive' versus 'neutral', are used.

Furthermore the results suggest a relationship between the type of target and the type of psychophysiological response. Emotive stimuli tend to affect the plethysmographic reactions, while sensory stimuli tend to affect the GSR. Reviewing the literature again, a similar trend can be found. Assuming that the target had

mainly an emotive value to the percipient, this could explain why Beloff et. al. and Mackintosh did not find significant ESP reactions when applying the GSR. That the subjects showed a significant GSR reaction when they were presented sensorially with these stimuli does not necessarily invalidate this hypothesis. It could also explain the results of Tart's experiment, assuming that in this experiment the experimenter himself, distributing electrical shocks to the agent, played a more important role in the experimenter-agent-percipient triad than the agent.

However, an unexpected finding is that in the ESP blocks significantly lower GSR reactions appeared than in the control blocks. This can be seen both from the significant difference between ESP blocks and 'first' Control blocks, and from the non-significant difference between 'first' and 'second' Control blocks. This finding contradicts the effects observed by Hettinger (1952), where GSR reactions from the percipient coincided with the stimulation of the agent.

As regards the interpretation of this finding much depends on the assumed relationship between basic skin-resistance level and the size of the response amplitude. If the Law of Initial Values, which states that with increased basic level the size of the response amplitude would decrease relatively, also holds for the skin-resistance reactions, then this finding would indicate that the basic skin-resistance level increased due to the ESP stimulation. This would suggest, that the organism becomes less sensitive to normally sensory stimulation, when it shifts the attention to a source of ESP information. But this also implies, that the reactions in the ESP blocks should be considered as random reactions or 'noise' like those in the Control blocks, and not as being the direct results of a form of ESP stimulation of the percipient. Thus, it must be concluded that the organism did indeed react to the stimulation of the agent, but did not react as if it was receiving such a stimulus itself.

On the other hand, the difference in results between the R and NR Conditions which shows a stronger effect with regard to GSR reactions in the R Condition, does not seem to be in accordance with this hypothesis. However, a possible explanation might be that the organism, knowing that it would receive strong auditory stimuli in the R Condition, built up an expectancy set and 'defended' itself against these unpleasant sensory stimuli by raising the basic resistance level even more.

If this is true, the strong effect in the R Condition is not so much related to a 'tuning-in' effect by presenting sensorially similar stimuli such as the target stimuli to the percipient, as, more probably, to a general tendency of the organism in this condition to shield itself from sensory

stimulation.

However, it should be noted that the above-mentioned hypotheses are more or less speculatory, and that other explanations could be provided just as well. But at least these considerations can be fruitful when designing further investigations.

Finally the fact that the responses in the ESP blocks appeared to be less strong than those in the Control blocks, indicates that the precautions taken against sensory leakages have been sufficient. Otherwise one should have expected stronger reactions in the ESP blocks.

As regards the plethysmographic reactions, it can be concluded that although the effects observed were not strong enough to yield significantly stronger reactions in the separate conditions, the overall tendency is in accordance with the findings reported in the literature. Presenting the agent with names resulted in stronger vasoconstrictions from the percipient. Since it was found that different types of targets had different effects, Haraldsson's finding - higher scoring when the percipient was familiar with the target stimuli (names) - could not be confirmed nor contradicted, since in the present study the percipient was equally familiar with the target material in all conditions. It remains unclear what exactly the effect on both agent and percipient is of presenting different types of names as target stimuli.

ABSTRACT

The effect of different types of targets, emotive and sensory stimuli, presented to the agent, were studied in an experiment with thirty-three pairs of subjects. Both GSR and plethysmographic reactions from the percipient were recorded. It showed, that the sensory stimulation of the agent mainly affected the percipient's GSR reactions, while emotive stimuli presented to the agent resulted in stronger plethysmographic reactions from the percipient.

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THE USE OF SHORT-DURATION GANZFELD STIMULATION TO FACILITATE PSI-MEDIATED IMAGERY

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INTRODUCTION

One of the most successful procedures for eliciting psi-mediated imagery in unselected subjects has been the use of ganzfeld stimulation. In this setting, subjects are cut off from normal sources of sensory stimuli by viewing a uniform visual field created by staring into a bright light through halved ping-pong balls placed over the eyes. Continual auditory stimulation is provided by having the subjects listen to white-noise through headphones for the duration of the experiment. Hypnagogic-type imagery is generally reported by subjects when subjected to this setting. The subject describes his mentation to the experimenter over an intercom and at some time during the session an agent in another room attempts to influence this mentation by looking at a target picture or viewmaster reel.

The length of the ganzfeld stimulation in ESP tests has varied between experiments and experimenters. In his first study, Honorton used periods of 35 minutes (Honorton and Harper, 1974). Stanford and Neylon used periods of 25 minutes (Stanford and Neylon, 1974) and at the time of this pilot study, there had been little consideration of the optimal duration of the ganzfeld for optimum psi retrieval. Recent work by one of the authors (S.R.) with a gifted subject in Los Angeles (Rogo, 1975) revealed that long sessions (20 - 30 minutes) caused the subject to displace her ESP and describe alternate targets in the target pool or to describe more than one target within the pool (which consisted of four magazine pictures). It was hypothesized that this subject's psi was most vigilant at the beginning of the ganzfeld session and that the length of isolation was causing these unexpected psi responses and displacements due to her continuing psi-scanning

process. In a critical series of 10 trials, this hypothesis was partially confirmed. The subject gave better and more focused ESP responses when the periods were reduced to between 7 - 12 minutes. The present experiment was designed to determine if short-duration ganzfeld sessions would facilitate psi-mediated imagery in unselected subjects as well, and was conducted at the Division of Parapsychology and Psychophysics, Maimonides Medical Center, in February, 1975 during a visit there by S.R.. M.S. and J.T. acted as co-experimenters.

PROCEDURE

Twenty subjects were run, all of whom were either volunteers or staff working within the division, or casual visitors to it. Each subject contributed a single session. The subject was escorted into a sound attentuated booth by the first experimenter (usually M.S. or J.T.) where halved ping-pong balls were placed over his eyes and a red light adjusted to create an undifferentiated visual field. During this procedure a second experimenter acting as agent (usually S.R. but sometimes a person chosen by S.R.) went into a separate room, shuffled a deck of numbered cards a prescribed number of times and cut it to determine which of 31 packets of 4 thematically different viewmaster reels would be used for the session. The uppermost reel in the packet was designated as the target. However, even after this selection the second experimenter did not look at the reel until the session actually began. A speaker was placed in the agent's room so that he could hear subject's mentation report.

Subjects were instructed to verbalize aloud any images, thoughts or feelings that they might have during the session. The first experimenter read the instructions to the subject, sealed the booth, and entered the adjacent control room to adjust the white-noise level to create a comfortable undifferentiated auditory field for the subject. He then signaled over the speaker to the agent and the subject that the session was to begin.

The first experimenter recorded the subject's mentation. As soon as the subject reported his first visual image (excluding any initial description of feelings, thoughts, etc. if they preceded actual mental imagery) the agent would insert the target reel into the viewmaster and "send" all 7 thematically related tableaux which comprised the reel. The sending period lasted for 5 minutes and at the end of that time the agent would wait for 2 minutes during which he would shuffle the target back into the pack.

At the end of the 2 minute period, the agent would leave his room and silently place the packet in the control room with the first experimenter, then leave. This signaled the first experimenter

that the session was over. The first experimenter then took the packet into the booth and asked the subject to rate all four viewmaster reels in order of correspondence with his mentation report. First and second choices were designated "hits" and third and fourth choices "misses". Further, the subjects rated each target on a 30 point scale of the degree of correspondence of each reel with his mentation. The sessions lasted from 7 to 17 minutes, the average lasting from 8 to 10 minutes.

RESULTS

Although there were some hits that showed strong qualitative resemblances to the targets, the overall results were not statistically significant, though the deviation from chance was in the predicted positive direction. The number of direct hits (first choices) was above mean chance expectation (N=7) but the C.R. was a non-significant 1.03. Pooling first and second choices on a 1/2 probability was not significant due to the small number of trials. Analysis of the ratings on the 30 point scale of correspondence was also non-significant (t=1, n.s.).

DISCUSSION

The experiment described above was designed for work with a gifted subject. This pilot would indicate that short-duration ganzfeld stimulation may not be suitable for unselected subjects. The length of the ganzfeld seems to be an important factor when experimenting with inexperienced subjects.

Honorton (1976) reported an analysis of duration of ganzfeld stimulation in relation to psi success for all psi ganzfeld studies reported as of the time of the 1975 Parapsychological Association Convention (N=15, including the present study). He found that the mean duration of ganzfeld for the successful studies (i.e. those giving overall significant psi results) was 37 minutes, while the mean duration of ganzfeld for the unsuccessful studies (i.e., with overall nonsignificant psi results) was 22 minutes. The mean difference in ganzfeld duration between the successful and unsuccessful studies was 15 minutes (t=3.41, 13df, p=.0047, two-tailed). Honorton suggested, on the basis of some nonparapsychological sensory deprivation research, that duration of isolation in sensory deprivation studies is analogous to dosage effects in drug studies. Further research is clearly needed to ascertain the optimal duration of isolation for successful ESP tests.

We did achieve some rather striking qualitative hits with certain subjects. A post-hoc examination of the data revealed that subjects

who had been successful subjects during earlier ganzfeld research at the Maimonides Division continued to score direct hits coupled with strong qualitative responses. This however is a partially subjective evaluation by one of the authors (S.R.).

The following examples represent some of the qualitative hits achieved during the experiment:

Target	Response First choice
"Probing the past" (Scenes depict ancient artifacts and structures including an old Roman villa with stone fountains)	"... started thinking about E's hit of Las Vegas - - - what a marvelous hit in E's film. I remember the stardust... I remember it from when I was there too. Some imagery, not so much waterfall, but fountains, water quirting up, coming down. Ceasar's Palace has fountains like that. Roman fountains in white stone or marble." (Note: The subject in this experiment is referring to a striking hit on the reel "Tour of Las Vegas" which occurred during a filming of the ganzfeld at Maimonides by a Canadian broadcasting company.)

Target	Response First choice
"Gulliver's travels" (Scenes depict models including a clipper ship, small human figures and buildings, etc..)	"... Clipper ship, came from Matthew Noonan. Large door of building opened, Colonial front, red brick... Disney like characters, like Beatles in Yellow Submarine. Images of far away, mesas, natural settings. Seeing very high rock walls, people dressed as arabs. Cairo arabs." (Note: Matthew Noonan was a merchant marine.)

These types of results are not qualitative very different from those reported by Honorton and Harper using lengthier sessions. So at least this study indicates that short-duration ganzfeld stimulation does not bar ESP performance. Both the above responses were given by previously successful ganzfeld subjects who had participated in other Maimonides projects, so it seems that short-duration ganzfeld stimulation may be a viable testing procedure for gifted subjects in general rather than unselected subjects. However, a replication of this procedure using a larger number of unselected subjects might prove fruitful.

SUMMARY

Twenty subjects contributed a single ganzfeld session in an experiment designed to see if the duration of the ganzfeld was a critical variable for facilitating psi-mediated imagery. Earlier work by one of the authors (S.R.) indicated that a gifted subject performed better during short ganzfeld sessions (7-12 minutes) than during longer sessions (20-30 minutes). In this experiment each subject contributed a session which lasted between 7-17 minutes. Subjects then had to choose which of four viewmaster reels had been chosen and sent by the agent as the target. While the overall results were non-significant, the deviation from chance was in the positive direction and several qualitative strong hits were given. These came from previously successful ganzfeld subjects. The outcome of this pilot study coupled with the later analysis by Honorton, is that the length of the ganzfeld stimulation is a pertinent variable in sensory-isolation psi research and that while short-duration sessions may not be suitable for unselected subjects, the procedure may be successful when working with gifted (selected) subjects.

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CONTENT

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EUROPEAN JOURNAL OF PARAPSYCHOLOGY

The European Journal of Parapsychology publishes original experimental and theoretical papers on parapsychology. In addition, articles presenting reviews of literature, case studies and papers on related topics are accepted in so far the subject can be considered relevant to the understanding of parapsychological phenomena or to the methodology of empirical research in parapsychology. Translations of papers originally published in a language other than English are also welcome.

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(continued inside back cover)

CONTENTS

SOME REFLECTIONS AFTER THE P.A. CONVENTION

Martin Johnson
University of Utrecht

The 19th Annual Convention of the Parapsychological Association was held in Utrecht, the Netherlands, from the 19th to 21st of August. 145 delegates registered for the Convention. The mass-media from a number of countries gave the Convention a rather broad coverage. The participants at the Convention came from 19 countries and from all continents.

The formal opening took place in the old and venerable aula of the "Rijksuniversiteit Utrecht" and the participants were welcomed by Dr. A. Verhoeff, Rector Magnificus of the University. The Presidential Address, entitled: "Problems, Challenges and Promises" was delivered in the afternoon of the first Convention day. The Invited Dinner Address, one of the highlights of the Convention, was delivered by Sir John Eccles. The address was entitled: "The Human Person in Its Two-way Relationship to the Brain", an address in which he gave an account of his strong dualist-interactionist position on the brain-mind problem.

25 papers and 43 research briefs were reported during the conference days. In addition, special symposia and workshops were included in the agenda and were devoted to the following topics:

1) Coordination of research facilities;
2) Psi and physics
3) Expanding research activities;
4) Geller-type phenomena;
5) New concepts in RSPK research.

Moreover, opportunities were given ex agenda to present recent research findings, educational films, etc..

By and large, there seemed to exist a concensus that the Convention was a rather successful one from an organizational point of view and that a substantial number of interesting research findings were reported. One issue of divided opinions was evidently the fact that a parallel program was run; sometimes there were even three simultaneous activities. Although parallel sessions are a common thing at most conventions within the better established branches of

science, it does not however represent the ideal solution for parapsychologists.

The research brief sessions are definitely a perennial and growing problem since the number of submitted briefs is increasing each year. In many cases the acceptance of a brief functions as an excuse for rejecting a paper. The Council of the P.A. will at their next meeting try to find a solution to the problem. It may turn out to be advantageous to eliminate the research brief section as such, and to eliminate the distinction between briefs and papers. There are also indications as to the need for much more time to develop special, more or less spontaneously organized, sessions to discuss particular matters in a much more penetrating way than has been possible so far at P.A. Conventions.

IS THERE A NEED FOR A EUROPEAN REGIONAL BRANCH OF THE P.A.?

The idea of the establishment of a European branch of the P.A. is not an entirely new one. However, a number of European participants demonstrated during and after the Convention their interest in such an enterprise. How such a body of professional and semi-professional parapsychologists should formally be attached to the P.A. remains to be investigated, but I believe that such formalities can be solved in a constructive way. Readers who are interested in the establishment of such a body are kindly asked both to indicate their interest, and to discuss and forward their viewpoints in this journal.

THE NEED FOR COORDINATING OUR RESEARCH PLANNING AND RESEARCH EFFORTS

The picture of parapsychological research as I see it, is characterized excessively by small pieces of research - small pieces not infrequently without any organic relationship to precisely formulated ideas. I certainly firmly believe in coordinated research programs involving long-term planning. As a first modest step to make integrated research programs possible, I suggested in my Presidential Address the establishment of a project catalogue in which abstracts are presented focusing on topics, objectives, choice of methods, necessary or available equipment, soft-ware etc., for planned investigations or investigations in progress. At the previously mentioned workshop on "Coordination of research facilities" several ideas were put forward, and hopefully something constructive will follow from these discussions.

SHOULD PARAPSYCHOLOGISTS CO-OPERATE MORE CLOSELY WITH MAGICIANS?

The question may sound like a far echo from last century but with Uri Geller and a number of mini-Gellers around the question may be a relevant one.

The night before the formal opening of the Convention a reception was offered by the Board of the University. For the final part of this reception entertainment was provided by a skilled magician. A well-known Swedish journalist Mr. Ulf Moerling, who in addition is a very skilled magician (with some interest in parapsychology), had been invited to give a performance in which a few clearly stated demonstrations of faked PSI phenomena were given.

1 A demonstration of his "precognitive" ability

Mr. Moerling told the audience that he had a medallion in his right back pocket of his trousers. The medallion was placed in a purse and could be opened by a zip. The day before, according to Mr. Moerling, he had engraved three digits on the reverse side of the medallion. He then asked someone in the audience (a parapsychologist) to call a digit between 0 and 9 and make sure that he would later be able to recall his digit. After he had called the digit, the magician wrote it down on a card. Then Mr. Moerling asked the person to select another parapsychologist in the audience to be the person who should call the second digit. Then after that person had called his digit (and Mr. Moerling had written it down on his card) he was asked by the magician to select a third person from the audience to call a digit. When the three digits had been called and written down on the card. Mr. Moerling asked another parapsychologist in the audience to come and take the purse out of his pocket, open the purse, take out the medallion, and read aloud the three digits engraved on the back of the medallion. Of course Mr. Moerling had a hit!

2 The demonstration of faked metal-bending

Mr. Moerling also gave a demonstration of metal-bending. He placed a nail in the hands of no less a person than Dr. John Beloff. And the nail bent beautifully!

3 Demonstration of faked GESP

Finally Mr. Moerling gave a quite astonishing performance of faked GESP. Previously the same day he had called me up and asked me to make a drawing of whatever object I could think of. He asked me to put the drawing in two sealed envelopes (similar to what Uri Geller used to request) and bring the set of envelopes to the seance, but

not to give him the envelopes until he had officially announced what I had drawn. I thought I took reasonably good precautions: I used a special type of envelope with an unusual letter-head, deposited in the closet in my office. Then I brought the envelopes, a pencil, and a sheet of paper into the Men's Room at our Department, locked the door carefully, and made a drawing of a car with rather particular details. No pad was used - my lap functioned as the support for the drawing. I then folded the drawing twice and slipped it into one of the envelopes which I sealed. After this I folded this envelope once and slipped it into the other envelope which I also sealed. Prior to that, I had made small but rather specific signs of identification inside each of the envelopes to be able to check whether the magician opened and changed the envelopes. Finally, I put the set of envelopes inside my pants to make it as hard as possible for Mr. Moerling to get hold of the envelopes. A few minutes prior to the seance I went into a W.C. and transfered the set of envelopes from my pants into the left, inside pocket of my jacket. At the end of Mr. Moerling's seance he told the audience about the experiment with "phoney-telepathy" which he was going to carry out with "Professor Johnson as the subject". Mr. Moerling started to make a drawing on a slate. After a while he stopped and turned to the audience and declared: "I think Professor Johnson has made a drawing of a car", whereupon I took the set of envelopes out of my pocket, handed it over to Dr. John Beloff (sitting in the first row) and exclaimed: "This is fantastic". Dr. Beloff opened the two envelopes and unfolded the drawing, and confirmed that I had made a drawing of a car.

There were several striking similarities between my drawing and the one Mr. Moerling had made, which strongly suggests at least to me that Mr. Moerling had in some peculiar way managed to get hold of the set of envelopes and may have used an illumination device to obtain the necessary information to make a very similar drawing. When and how he got hold of the set of envelopes and when and how he returned it is open to speculation. If I had been careful enough to wrap the drawing in aluminum foil the explanation of the use of some illumination device as a source of information could have been more or less ruled out.

After the seance, something which I consider as rather embarrassing happened: quite a number of parapsychologists started to discuss the possibility that Mr. Moerling was a medium without being aware of it? More or less fantastic hypotheses were put forward as to how to explain his success (the hypotheses ranging from a dishonest and provocative cooperation between Mr. Moerling and myself to subliminal stimulation given by Mr. Moerling over the telephone!). I am positively shocked that so many parapsychologists (between 10 and 20

according to Mr. Moerling's and my independent estimations) in spite of his assurance that his performance was based on trickery, seriously put forward the PSI hypothesis as the explanation for what they had observed. I have no reason not to believe Mr. Moerling when he assures me that he succeeds almost every time with the "drawing-experiment". Mr. Moerling has a sincere respect for what parapsychologists can perform in a laboratory setting, but he thinks they should be much more careful than they usually are when it comes to making statements based on what they have observed under more or less "naturalistic" conditions, especially when the subject has a background as some kind of stage performer. He thinks that many parapsychologists have made fools of themselves in connection with studies of Uri Geller and mini-Gellers. Be that as it may. At least to me his demonstration was rather educational.

Mr. Moerling's astonishing performance does not in any way disprove that things performed by Uri Geller or mini-Gellers could not be the effect of a genuine PSI ability, but it highlights the fact that even experienced parapsychologists are mediocre observers outside a laboratory setting and that at least some of them seem to favour interpretations of observed, odd phenomena in terms of PSI. I do think that the P.A. should try to establish contacts with for instance such a renowned organization as the British Magic Circle. Their expertize could very well save us from some mistakes and embarrassment when it comes to field-studies of metal-benders,etc..

CLAIRVOYANCE AND PSYCHOKINESIS IN TRANSCENDENTAL MEDITATORS AND MATCHED CONTROL SUBJECTS: A PRELIMINARY STUDY

William Braud

Janice Hartgrove

INTRODUCTION

On the basis of a review of the recent parapsychological literature, Honorton (1974) has suggested that an important factor common to a number of psi-conducive states is the withdrawal of attention from external sensory and somatic stimuli and a concomitant shift toward internal processes such as thoughts and images. Such a maneuver may facilitate psi performance by attenuating the psi-irrelevant sensory, perceptual, and somatic "noise" which may ordinarily interfere with normally weak psi "signals". We extend and elaborate Honorton's model by suggesting that psi-interfering noise may arise from a number of different sources: a) exteroceptive stimulation (sensory, perceptual noise), b) somatic, muscular activity (bodily noise), c) excessive autonomic activity (emotional noise, excess arousal), d) excessive mental activity, especially analytical, linear, logical, "left-hemispheric" activity (cognitive noise), and e) noise produced by excessive striving to retrieve psi information. Any of these noise sources may direct attention toward themselves and away from weaker psi "inputs".

Since Transcendental Meditation (TM) is accompanied by a hypometabolic condition (Wallace, 1970) in which the noise sources described above are significantly reduced, TM may be a psi-conducive technique. One might expect that long-term practitioners of TM would be able to enter a state in which they have improved access to psi information which might be ignored otherwise. This suggestion is supported by anecdotal accounts of enhanced psi in meditators (Hartgrove, 1975), by almost universal references to the development of psychic abilities ("siddhis") at certain stages of formal meditative practice (see, for example, Patanjali's Yoga Aphorisms - Prabhavananda and Isherwood, 1953), and by some laboratory research on psi in practitioners of forms of meditation other than TM (Dukhan

and Rao, 1972; Osis and Bokert, 1971; Roll and Solfvin, 1975). In a recent review of studies involving psi tasks during or following meditation, Honorton (1976) reported that out of sixteen experiments, nine were independently significant at the .05 level or lower, whereas o.8 significant series would be expected on the basis of chance error. The combined results for all sixteen experiments were highly significant ($p=6x10^{-12}$).

The purpose of the present experiment was to study clairvoyance and psychokinesis in long-term practitioners of TM and in a matched control group of nonmeditators. For the psychokinesis task, the subjects attempted to influence a Schmidt random number generator (without feedback) while meditating or resting. For the clairvoyance task, the subjects attempted to gain impressions of a 35 mm slide concealed in an opaque envelope; this clairvoyance task occurred while the subject was terminating his/her meditation or rest.

METHOD

Subjects

Twenty subjects participated in the study. Ten subjects were meditators associated with the San Antonio Center of the International Meditation Society. The only criterion for the selection of these subjects was that they must have meditated regularly for a minimum of eighteen months. The four male and six female meditators ranged in age from twenty-five to forty-eight years. The length of time these subjects had been meditating varied from eighteen to sixty months, with a mean of thirty-two months. Ten nonmeditating (control) subjects were solicited from among persons in attendance at introductory lectures on TM. These five males and five females, ranging in age from eighteen to sixty years, were not yet meditators, but were assumed to have personality and interest characteristics similar to those of the meditators.

Procedure

The subjects reported to the Psychology Laboratory of the Mind Science Foundation in San Antonio for an experimental session lasting twenty-five minutes. During the first twenty minutes, the subject meditated (if a meditator) or rested (if a control) with eyes closed while keeping in the back of his/her mind an intention to influence a binary random number generator (RNG). This RNG was designed by Helmut Schmidt and has been used successfully by him in a number of psychokinesis experiments (Schmidt, 1970, 1973). The

RNG produces sequences of binary random numbers of specified length at speeds up to 1000 binary numbers per second. For each test run, the cumulative numbers of produced +1 s and -1 s are continuously displayed on readout tubes. Electronic noise in semiconductors provides the basic random element in the system. Randomness tests indicate that the generator produces equal numbers of +1 s and -1 s in the absence of attempts to influence it.

For the psychokinesis (PK) task, the generator produced +1 s and -1 s at a rate of 50 per second for twenty 20-second run periods, separated by 40-second rest periods. Thus, 1000 numbers were generated for each run, yielding a total of 20,000 PK trials. Half of the subjects attempted to influence the RNG so that more +1 s were generated. Half attempted to increase the frequency of -1 s. Subjects were instructed that they could influence the RNG psychokinetically by intending for the desired outcome to occur and by confidently imagining a successful outcome. An experimenter (J.H) remained in the subject's room and recorded the RNG scores at the end of each of the twenty runs. W.B. was also present in the room to supervise each session.

The subject gradually terminated his/her meditation or rest during the next five-minute period, which constituted an impression period for the clairvoyance portion of the experiment. The subject had been instructed to keep his/her mind still and to allow impressions of the target to enter consciousness throughout the impression period, the onset and offset of which were signalled to the subject by means of a bell. The target was a 35 mm slide, randomly selected from a pool of 1024 slides by an assistant who had no contact with the subject. The target was sealed inside of an opaque envelope (a number 000 "Jiffy Bag", stapled shut) which the subject held in his/her lap throughout the session. At the end of the impression period, the subject stopped meditating or relaxing and wrote and drew on paper his/her impressions of the target slide. The impressions were then coded as to the presence or absence of content in ten categories, according to the system developed by Honorton (1975). The accuracy of the subject's impressions was assessed statistically by comparing the binary content code of the impressions with that of the pictorial target itself.

After the subject had coded his/her impressions, he/she completed a number of questionnaires assessing attitudes toward psi and altered states of consciousness, mood, state of consciousness, physical state, and imagery. The questionnaires were an extended form of one used by Braud and Braud (1974) in a study of relaxation and psi, and the Betts QMI Vividness of Mental Imagery Scale (Sheehan, 1967). The items of the Braud and Braud questionnaire

are given, in abbreviated form, in table 1. When all questionnaires had been completed, the envelope was opened to reveal the target picture and the impression-target correspondence was quantified. The subject was then informed of the results of the experiment and the session was concluded.

RESULTS

The major findings of the experiment are shown in table 1. On the binary coding scores for the clairvoyance task (for which mean chance expectation is 5), the meditators scored significantly higher than the nonmeditators (CR_d=1.98, p=.024). A one-tailed test was used since a directional prediction was made on the basis of the studies reviewed above. The meditators obtained six hits (scores > 5), three misses (scores < 5), and one chance score (score $= 5$); the nonmeditators obtained two hits, seven misses and one chance score. Neither the performance of the meditators nor that of the nonmeditators differed significantly from chance.

The meditators and nonmeditators did not differ significantly on the psychokinesis task. Combining the two groups yielded significant PK missing overall (CR=-2.11, p=.034, two-tailed). When assessed independently, neither the performance of the meditators nor that of the nonmeditators differed significantly from chance.

To determine whether amount of prior experience with meditation was related to ESP performance, a Spearman rank-order correlation coefficient was calculated for number of months of regular practice of meditation versus clairvoyance score. A correlation of + .51 was obtained which, however, was not quite significant for the small sample size of ten subjects.

Table 1 also indicates the questionnaire items on which the two groups differed. There were significant tendencies for the meditators to be vegetarians, to be in a better general mood than nonmeditators, to consider the ESP task less important and to like the PK task less than did the nonmeditators. There was a tendency, approaching significance, for meditators to feel more detached from their physical bodies than did the nonmeditators. The two groups did not differ significantly on any of the other questionnaire items. Means are provided for all items, however, as normative data for these particular samples.

Results for a selected subject

In figure 1, we present clairvoyance test scores for a selected

TABLE 1

Means and summary statistics for psi scores and questionnaire items for meditators and nonmeditators

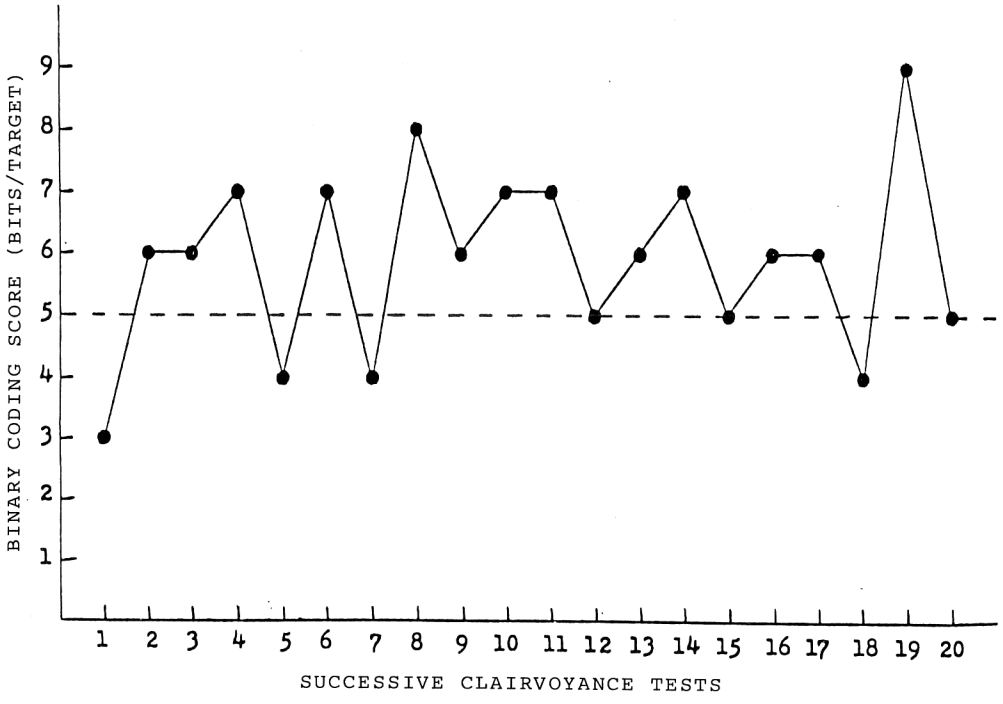
	Measure	Med	Non	Diff.	P
1	Clairvoyance	5.5	4.1	CR_d=1.98	-.024[&]
2	Psychokinesis	9967	9966	CR_d=0.17	n.s.
3	Vegetarian	60%	0%	Fisher's Exact Test	.0054
4	General mood today	14.5	42.5	U=4	<.002[&&]
5	Degree of detachment	23.5	43.4	U=24	<.10[&&]
6	Importance of ESP task	38.6	20.1	U=21	<.05[&&]
7	Liking for PK task	35.5	15.0	U=19	=.02[&&]
8	Dream recall	3.4	3.4		
9	Betts imagery score	92.1	81.2		
10	General belief in ESP	14.9	16.1		
11	ESP in this experiment	25.9	24.4		
12	Personal success in this experiment	33.8	52.1		
13	Attitude toward experimenter	16.6	12.1		
14	Mood to participate	16.6	26.6		
15	Physical relaxation	25.8	20.6		
16	Mental relaxation	33.2	38.0		
17	Believe state to be psi-conducive	27.7	25.0		
18	Physical activation	26.6	25.9		
19	Mental activation	31.6	38.2		
20	Mental distraction	46.5	37.4		
21	Focused attention	41.1	26.9		
22	Attention directed internally	39.0	21.8		
23	Spontaneous dreamlike mental activity	39.0	34.1		
24	Awareness of physical body	30.0	30.8		
25	Feeling of unity, relatedness	43.1	34.4		
26	Stillness of mind	30.1	39.0		
27	Confidence in accuracy of psi	44.1	43.0		
28	Usualness of state entered	40.4	49.4		
29	Welcome and facilitate unusual state	20.2	11.6		
30	Enjoyment of unusual state	25.6	24.6		
31	Intrigued by unusual experiences	46.6	43.2		
32	Liking for clairvoyance task	29.9	25.9		

[&] one-tailed test [&&] two-tailed test

(continued next page)

FIGURE 1

Binary coding scores (bits/target) for twenty clairvoyance tests for a selected meditating subject, J. H.



subject. This subject is the experimenter, J.H., a 27-year-old female doctoral candidate at the University of Houston. J.H. had been meditating regularly for four and a half years at the time of the experiment. She might be described as a "still-somewhat-skeptical sheep" who recently has been having an increasing number of moderately impressive spontaneous psi experiences. Since J.H., as experimenter, was unaware of the contents of the target slides inside of the envelopes, she decided to try to gain clairvoyant impressions of these targets herself, during activity-free periods while she was testing the subjects. Therefore, during the five-minute impression periods, J.H. meditated (using the TM technique) and noted and remembered her spontaneous imagery during those periods. She recorded and coded her own impressions while the subject she was testing was doing the same. Twenty scores were obtained for J.H., collected during the tests of each of her twenty subjects.

The clairvoyance scores for J.H.'s twenty trials are independently significant, with a mean score = 5.9, CR=+2.54, p= .0055 (one-tailed). There were thirteen hits, four misses, and three chance scores. No evidence of a decline effect was apparent in J.H.'s scores.

DISCUSSION

The superiority of the TM practitioners over the control subjects, on the clairvoyance task, confirms similar findings (reviewed by Honorton, 1976) of the superiority of meditators (of other varieties) over nonmeditators on tests of receptive psi. However, the fact that neither group differed significantly from mean chance expectation is puzzling. The findings may be interpreted in terms of Palmer's (1975) recently proposed "Model 3". In this model, it is assumed that two sets of factors influence performance: one set operating uniformly upon all subjects and another set operating upon the subjects differentially. In the present case, it might be assumed that a "psi-missing factor" influenced all subjects, but

Continuation text table 1: Items were worded and scaled so that for all items in this table except items 1, 2, 3, 18, 19, 20, and 24, a low score indicates a high value of the measure listed. For the seven items just mentioned, that relationship is reversed. All items except 1, 2, 3, 8, and 9 were measured in millimeters from zero on a 100 millimeter scale

that a "psi-hitting factor" operated more strongly upon the meditators than upon the nonmeditators. It is likely that the psi-hitting factor is the hypometabolic state, low in psi-irrelevant "noise", which should be induced much more strongly by meditation than by mere resting. The psi-missing factor is more difficult to identify. Perhaps it is related to the subjects' lack of belief in personal success in the experiment and their lack of confidence in the accuracy of their psi, as indicated by their scores on questionnaire items 12 and 27. Or perhaps there is some as yet unidentified negative factor in our setting or procedures which discourages hitting.

Our failure to observe a difference between our two groups on the PK measure is inconsistent with previous findings of a PK superiority of meditators (Matas and Pantas, 1971; Schmidt and Pantas, 1972; Honorton and May, 1976). Two factors which may account for the deviation of our findings from those of previous investigators are: a) our testing subjects for PK during their meditation, rather than following it, and b) the absence of feedback for correct performance in the present design. Feedback was omitted since we felt it would disrupt the meditative state.

One might think that the relaxed, receptive state characterizing meditation might be incompatible with PK - an "active" form of psi. Such a notion, however, is not consistent with the PK-meditation findings reviewed by Honorton (1976).

Another factor which could have resulted in psi missing on the PK task may be the presence of a "preferential" or "differential" effect (Rao, 1966, p.123) which drew subjects "toward" the clairvoyance task and "away" from the PK task.

Finally, there exists the possibility that the locus of the PK missing effect was really in the experimenter (J.H.) who monitored and recorded the random numbers generated, rather than in the subjects themselves.

ABSTRACT

Long term practitioners of transcendental meditation (mean length of practice of 32 months) and a matched control group of nonmeditators were tested on clairvoyance and psychokinesis tasks. The clairvoyance task involved gaining impressions of a target slide concealed inside of an opaque envelope. The psychokinesis task involved influencing a binary number generator, without feedback. The psychokinesis trials occurred during, and the clairvoyance trial immediately following, a twenty-minute period of either meditation or rest. The meditators scored significantly higher than did the nonmeditators on the clairvoyance task; however,

neither group differed from mean chance expectation. The two groups did not differ in terms of psychokinesis scores. Combining the two groups yielded evidence for overall significant PK missing. Results are also reported for a selected subject, J.H. (a meditator), who completed twenty of the free-response clairvoyance tests with a significant overall scoring rate (CR=2.54, p=.0055) and no indication of a decline.

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This experiment was conceived and designed by W.B.. The subjects were recruited and tested by J.H., with W.B. personally supervising the entirety of each session. Data reduction was done by J.H. and W.B.. Statistical analyses were done by R.W. and W.B.. The actual paper was written by W.B.

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TIME-SYMMETRIC THERMODYNAMICS AND CAUSALITY VIOLATION

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INTRODUCTION

There is an impressive amount of evidence for the occurrence of certain phenomena, often classified under categories such as ESP, PK, or precognition, and generally referred to as psi. A characterisation of these phenomena is that the occurrence of certain specified events is found to be more often or less often than would be expected from an analysis of known and understood prior influences on the events. For example, certain quantum decays may be correlated with other events in the past or future in a way inexplicable by existing scientific theories. The idea of psi is usually taken to hypothesize the influence of humans or other living organisms on such events. The phenomena also may be referred to as instances of causality violation. This descriptive phrase emphasizes a breakdown in the way the past states of the universe are normally considered to influence the future states of the universe.

There have been a considerable number of theories offered to explain instances of causality violation, but none of these has yet achieved the status of a general working hypothesis (Chari, 1974). Indeed, this lack of a theoretical underpinning is probably one reason why many scientists have so readily ignored available experimental evidence. In this paper we present a formalism for time-symmetric thermodynamics which we feel may serve as a basis for explanation of occurrences of causality violation.

In physics, the universally observed increase in entropy with time may be considered to arise from boundary conditions in the past. The past is known to be a highly ordered or unusual state (thermodynamically), whereas normally there are considered to be no restrictions on the development of the universe which are based in the future. This asymmetrical situation is a favoured, (but not

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the only) explanation of the second law of thermodynamics, and the associated unidirectionality of time (Terletskii, 1971). This explanation is related to intuitive ideas about past and future. From the point of view of a single observer, the past seems irrevocably fixed, while the future seems subject to partial control as a result of actions performed in the past or present.

There are reasons to re-examine these ideas, perhaps the best of which is the evidence for causality violation from experiments on psi. A completely different reason has been presented by Cocke (1967). It has been argued that in closed oscillating cosmological world models, the direction of time, as indicated by the observed change in entropy with time, must reverse itself. The problems associated with this reversal led Cocke to posit an hypothesis of complete time symmetry in oscillating cosmologies, and to develop a formalism for thermodynamics in which there are boundary conditions at two times, one in the past and one in the future.

In such a cosmology, if both past and future boundary conditions require the universe to be in a highly ordered or unusual state, the most probable general type of path between the two times is characterised at the beginning by a general increase in entropy, and later by a general decrease. However, during the entropy increase phase (for example) there are also anomalous occurrences of spontaneously decreasing entropy (so-called pre-effect), due to the influence of the future boundary conditions. Schmidt (1966) has also developed a model of an oscillating cosmos, but without an emphasis on anomalous instances of spontaneously decreasing entropy.

It would seem fruitful to relate this source of spontaneous decreases in entropy, or thermodynamic pre-effect, with observed instances of causality violation. Therefore we have been led to the development of a formalism for time-symmetric thermodynamics, a generalisation of Cocke's ideas, for the purpose of explaining instances of causality violation in psi. In this formalism there are two types of entropy. One type (future directed entropy, or f-entropy) may be identified with conventional entropy: it is associated with boundary conditions in the past, and increases towards the future. The second type (past directed entropy, or p-entropy) is associated with boundary conditions in the future, and increases towards the past. For the case in which p-entropy is maximal and constant, the time-symmetric thermodynamics reduces to normal thermodynamics with normal causality. Although our formalism contains boundary conditions on the future possible states of the universe, these do not have to be as restrictive or complete as the boundary conditions on the past (as is the case in Cocke's work). The first selection following is a description of

our formalism for time-symmetric thermodynamics.

Although we have tried to make the description of this formalism as simple as possible, it still probably requires some background in statistical mechanics for full understanding. The essence of the section is that it is possible to develop a formalism which allows future events to influence the past as well as for past events to influence the future. In this formalism, p-negentropy is the means through which the future can influence the past.

To supplement this formalism, we tentatively assume that p-entropy is not maximal. (This means that p-negentropy is not quite zero.) We also assume that living organisms have developed the capacity to utilize p-negentropy. In the second section following we describe these assumptions, and suggest some reasons for their adoption. With the formalism and the two assumptions, we are then able to explain a number of features of causality violation experiments, and to make a few further predictions. These features and predictions are the concern of the final section.

TIME-SYMMETRIC THERMODYNAMICS

Our aim here is to illustrate a formalism for a thermodynamics that incorporates boundary conditions on both the past and future. This formalism contains conventional thermodynamics, which incorporates boundary conditions only on the past, as a special case. There are other ways to extend thermodynamics to include future boundary conditions in a time-symmetric way, but we hope that the method presented here is a minimal extension of conventional thermodynamics.

In our presentation, we will first describe the relation between ensembles and probability; then construct an ensemble whose members change with time according to specified transition probabilities; and finally introduce an ensemble constrained by boundary conditions on past and future. For this latter ensemble we define two types of entropy: future directed entropy, which may be identified with conventional entropy; and past directed entropy, changes in which may serve to explain occurrences of causality violation. After describing our formalism, we will point out some of the ways it agrees with and differs from conventional thermodynamics.

We begin by describing some aspects of ensembles. Let E denote an ensemble, or collection of systems. Each system or member of the ensemble may be considered to correspond to one unique microstate history, over a finite time period, of an (approximately) isolated section of space in which thermodynamic processes occur. For example, E might contain sets of space-time histories for a group of molecules in a box.

It is useful to imagine that there may be more than one system in the ensemble corresponding to any given unique microstate history. In other words, the ensemble may contain a number of copies of a specified microstate history. It is convenient to define certain operations on ensembles. If E and F are ensembles, the ensemble consisting of all systems in E and F may be formed, and denoted E+F. If c is a positive real number, the ensemble consisting of c copies of each system in E may be formed, and denoted cE. We assume that each ensemble is sufficiently large that it may be divided into parts without changing the ratios between the numbers of different systems in the ensemble. Therefore c does not have to be an integer.

We wish to relate probabilities to ensembles. At any given time t we may characterise each system in the ensemble E as being in a state k, k=1,2,.... For example, the systems might be classified into states according to the number of electrons in them, their total energy, etc.. Let $E_{k,t}$ be the ensemble of systems in E that are in a state k at time t (i.e. $E_{k,t}$ is obtained from E by discarding all systems not in state k at time t). Let n(E) be the number of systems in E. The probability that a system in E is in state k at time t is given by $n(E_{k,t})/n(E)$, according to the frequency definition of probability. In general both $n(E_{k,t})$ and n(E) will be infinite, but the ratios will still be meaningful in the sense of limits.

Consider now how systems in the ensemble change from one state to another with time. Let $P_{ij}(t_1,t_2)$ be the probability that a system in state i at time $t=t_1$ is in state j at time t_2. Thus far we have not specified how the ensemble E is constructed, so for the time being we can consider the P_{ij} to be an arbitrary set of probabilities, i.e. a set of real numbers satisfying, for all i and j, $P_{ij} \geq 0$ and $\sum_k P_{ik}(t_1,t_2)=1$.

We call an ensemble microreversible over the time interval (t_1,t_2) if there exists a set of numbers V_i, all non-zero, such that

$$V_i P_{ij}(t_1,t_2) = V_j P_{ji}(t_2,t_1). \qquad (1)$$

Cocke (1967) finds this condition sufficient for time symmetry in processes describable by a Markov chain formalism. He also demonstrates that the quantum measurement process, as well as classical and quantum mechanics, are completely time symmetric. Therefore all natural systems appear to be reversible in the sense

of (1). (In some types of ensembles, the number V_i in (1) may be identified with the statistical weight or degeneracy of state i.)

We now consider a particular type of ensemble, an equilibrium ensemble. This is an ensemble for which $n(E_{k,t_1}) = n(E_{k,t_2})$ for all k, t_1 and t_2: in an equilibrium ensemble the number of systems in any state k is independent of time. Let $E_{i,t_1;j,t_2}$ be the sub-ensemble of systems in E that are both in E_{i,t_1} and E_{j,t_2}. Given the probabilities P_{ij}, we construct a particular equilibrium ensemble, G, such that

$$P_{ij}(t_1,t_2) = n(G_{i,t_1;j,t_2}) \; / \; n(G_{i,t_1}). \qquad (2)$$

In other words, out of possible equilibrium ensembles, the ensemble G is chosen so that the probability $P_{ij}(t_1,t_2)$ is identical with the transition probability calculated by counting the number of appropriate systems in G at times t_1 and t_2. Noting that by definition $G_{i,t_1;j,t_2}$ is equivalent to $G_{j,t_2;i,t_1}$, and substituting for P_{ij} and P_{ji} from (2) into (1), it follows directly that for the ensemble G,

$$n(G_{i,t}) \propto V_i \qquad (3)$$

In words, V_i is proportional to the probability at any time that a system in G is in state i.

For sections of spacetime sufficiently large that the gravitational energy is comparable with the rest mass, there is no thermodynamic equilibrium, since entropy may increase without limit (Tolman 1934, p. 420). In this case this formalism does not apply, but on any sufficiently small section of spacetime it remains valid.

We now proceed to construct a non-equilibrium ensemble K from different sub-ensembles of the equilibrium ensemble G. We expect that the ensemble K is able to represent any thermodynamic process. That is to say that K will contain all possible microhistories compatible with the observed macroscopic features of the thermodynamic process. K is defined only for times t satisfying $t_1 \leq t \leq t_2$. Using the operations of merging and making copies of

ensembles, described earlier, K is constructed from a set of real non-negative numbers $u_{ij}(t_1,t_2)$:

$$K = \sum_i \sum_j u_{ij}(t_1,t_2)\ G_{i,t_1;j,t_2} \tag{4}$$

In words, K is an ensemble composed of a weighted sum of sub-ensembles of G, in which the value $u_{ij}(t_1,t_2)$ weights the sub-ensemble of systems in G that are in state i at time t_1 and in state j at time t_2.

We shall concentrate mainly on the important special case in which the initial and final conditions are independent, so that $u_{ij}(t_1,t_2)$ may be factorised:

$$u_{ij}(t_1,t_2) = w_{fi}(t_1)\ w_{pj}(t_2). \tag{5}$$

$w_{fi}(t_1)$ is called the future directed weighting of state i at time t_1; these weights represent the past influencing the future. $w_{pj}(t_2)$ is called the past directed weighting of state j at time t_2; these weights represent the future influencing the past.

We wish the weightings at different times to be related so that K is independent of the times t_1 and t_2. A sufficient condition for this to be true is that the weightings satisfy

$$V_i\ w_{fi}(t_2) = \sum_j V_j\ w_{fj}(t_1)\ P_{ji}(t_1,t_2), \tag{6}$$

$$V_i\ w_{pi}(t_1) = \sum_j V_j\ w_{pj}(t_2)\ P_{ji}(t_2,t_1). \tag{7}$$

We call (6) and (7) the simple time development equations. In the appendix we show how they may be derived using the implicit definition of the weightings by (4) and (5), as well as the relations (2) and (3).

The entropy associated with the past directed weightings, called p-entropy, may be defined statistically as

$$S_p(t) = -k \sum_i V_i\ w_{pi}(t)\ \ln w_{pi}(t), \tag{8}$$

where k is Boltzmann's constant and the w_{pi} are normalised so that $\Sigma_i V_i\, w_{pi}(t)=1$. When the V_i may be normalised to be integers (as is always the case in quantum systems), then one may define a new subscript i' and a new set of weightings $w'_{pi'}$ such that $w'_{pi'} = w_{pi}$ for all i' satisfying

$$\sum_{k=1}^{i} V_k \geq i' \geq \sum_{k=1}^{i-1} V_k .$$

In other words, the index i' has a different value for each individual system, whereas i labels a particular state comprising V_i systems. In terms of $w'_{pi'}$, the p-entropy appears in the more familiar form

$$S_p(t) = -k \sum_{i'} w'_{pi'}(t) \ln w'_{pi'}(t). \qquad (9)$$

The entropy associated with the future directed weightings, called "f-entropy", may be defined analogously to (8) and (9). It is possible to show that f-entropy increases towards the future and that p-entropy increases towards the past, by using the simple time development equations (6) and (7), the microreversibility condition (1), and the standard assumptions about coarse-graining.

It is well known that negentropy, the difference between the maximum possible entropy and the actual entropy, can be considered to be a measure of the information available about a system (Brillouin, 1962). If we assume that p-negentropy is zero, then p-entropy is maximal and so the w_{pi} are equal. In this case the past directed weightings may be ignored and our formalism reduces to normal thermodynamics. Comparing our notation to that of Tolman (1938, p.460), our i, i', $w'_{pi'}$, w_{pi}, and V_i may be identified with his , n, P_n, P_ν/G_ν, and G_ν respectively, where P_n is the coarse-grained probability for the states n in a group of G_ν neighbouring microstates. Taking w_{fi} to correspond to

$$\sum_{j=1}^{V_i} \rho_{jj} \;/\; V_i \quad ,$$

where ρ is the quantum mechanical density matrix, S_f becomes equivalent to the Gibbs definition of entropy for non-equilibrium

systems (see also Terletskii, 1971, p.163).

It may be useful to describe in a bit more detail the relation of our formalism to conventional thermodynamics. We again refer to the treatment by Tolman (1938). First, the normal way of setting up an ensemble is by including a range of microsystems such that the ensemble average agrees with available knowledge of thermodynamic variables (such as temperature) that have been measured. Thus, a representative ensemble might be given by

$$L = \sum_i w_i(t_1)\, G_{i,t_1} .$$

The microsystems are chosen assuming equal a priori probabilities and random a priori phases (Tolman, 1938, p.524). These assumptions mean that the microsystems in state i at time t_1 are not likely to be unusual in terms of what is known about the macrosystem (i.e. the ensemble average). These assumptions are reflected in our choice of sub-ensembles of G, rather than of some non-equilibrium ensemble, to construct L. Our procedure differs from this conventional one only in that the ensemble K is set up on the basis of knowledge of thermodynamic variables at two times, instead of one time as for L.

In conventional thermodynamics, time development equations similar to (6) and (7) are the consequence of physical laws, such as are represented by Schrödinger's equation, applied to the systems in a representative ensemble (Tolman, 1938, p.395-452). In our formalism, the simple time development equations are a consequence of requiring K to be independent of its defining times. However, the two procedures are in essential agreement, since our transistion probabilities are arbitrary and may be chosen so as to represent features of an observed thermodynamic process. A possible advantage of our approach is that it does not require full knowledge of the physical laws causing transitions between states; instead, transition probabilities may be inferred from observations.

Our formalism reduces to Cocke's two-time thermodynamics if the initial and final boundary conditions are similarly unusual thermodynamically. An example of this would be the case in which $w_{fi}(t_1) = w_{pi}(t_2) = \delta_{ij}$: the thermodynamic process is in state j at both the initial time t_1 and the final time t_2.

THE EXISTENCE AND USE OF P-NEGENTROPY: TWO ASSUMPTIONS

We have shown how our formalism for time-symmetric thermodynamics reduces to normal thermodynamics if there is no past directed negentropy. However, to be of interest in explaining experimental

results involving causality violation, we must assume the existence of a sufficient quantity of p-negentropy at the surface of the earth. Furthermore, we must assume that biological organisms are capable of utilising this negentropy. We here discuss briefly these two assumptions and some plausible arguments for their tentative adoption.

It is possible to speculate on the existence of localised sources of past directed entropy and negentropy (or in other words, of boundary conditions in the future). Possibilities are black holes (in which closed timelike paths may occur when a sufficient concentration of angular momentum causes a sufficient rollover of light cones, generating p-negentropy by backscattering of f-negentropy) and singularities (in which p-negentropy may arise analogously to the way f-negentropy may be considered to arise from a past initial singularity). However, it is not necessary for there to be localised sources of p-negentropy. It is possible to assume that p-negentropy has its source in the distant future, in the same way that normal negentropy is assumed to arise from boundary conditions in the distant past. Finding a source of p-negentropy is not difficult in principle, and is mainly of interest in determining the precise details of how and in what manner p-negentropy might be incident on the earth and thereby available for utilisation by organisms.

From the overwhelming dominance of normal causality, it is evident that the amount of utilisable p-negentropy at the surface of the earth must be very small. Consider the amount of p-negentropy needed to ensure that an event which would have otherwise had a probability of $\frac{1}{2}$ (assuming no change in f-entropy - e.g. due to equal future directed weightings as well), has instead a probability of 1. Using (8),

$$\Delta S_p = S_p(0,1) - S_p(\tfrac{1}{2},\tfrac{1}{2}) = k \log_e 2.$$

The negative of this is the amount of negentropy required: $-\Delta S_p = k \log_e 2$. This may be contrasted with $-\Delta S_f \sim 3 \times 10^{27}$ k which is roughly the amount of negentropy utilised by a human in one day's bodily metabolism (assuming a heat production of 3000 Kcal and an external temperature of 27 degrees Celcius). Thus the amount of p-negentropy required to produce significant effects would be negligible by everyday standards.

Now assume that p-negentropy exists naturally at the earth's surface. A life form that could collect and use p-negentropy in the same way it uses f-negentropy might be expected to have an evolutionary advantage, at least in certain cases. This would be true even if the quantity of available p-negentropy were extremely

small, because in an otherwise random process even a small amount of p-negentropy could produce a marked change in the most probable result. Since in utilising p-negentropy an organism would be using the future organisation of the universe to influence the past, the organism would appear to act as if it knew the future. The evolutionary advantage of such an ability should be obvious.

At this stage it may be considered rather premature to ask about details of how organisms could be able to collect and utilise p-negentropy. Our assumption that they can is based only on the analogy to the known ability of organisms to collect and use f-negentropy. The question might be worth considering if other aspects of our model were found to be useful. However, to help provide a "feel" for what the use of p-negentropy might entail, it may be useful to provide a hypothetical example.

Before doing this, we wish to comment that there are a number of ways of thinking about p-negentropy and related phenomena, such as in terms of future boundary conditions. None of these ways of thinking is immediately easy to adopt; we ourselves required a considerable period of time before feeling natural in thinking of the formalism. This is because the idea of the future affecting the past is not part of our normal way of thinking about the world. Indeed, many of our tools for understanding the world, such as language, have built into them the assumptions of normal causality.

We find that the easiest way to think about p-negentropy is by analogy with f-negentropy: effects are the same in every respect (except the quantitative amount) with a reversal in the time direction. For example, since one stores f-negentropy in the past to use in the present or future, one would store p-negentropy in the future to use in the present or past.

Consider the analogy to f-negentropy from a slightly different perspective. One tries to affect the future by ordering the past in certain ways to make certain future events more probable. To order the past one uses f-negentropy and thus alters the future directed weightings w_{fi} at time t_1. Therefore certain systems in the ensemble K become more probable. By analogy, the utilisation of p-negentropy in the future changes the past directed weightings w_{pj} at time t_2. This also makes certain systems in the ensemble K more probable.

With this background, we now present a hypothetical example of how organisms might use p-negentropy. Consider a sugar molecule. An existing sugar molecule represents f-negentropy: it is a much more ordered form than its constituents CO_2 and H_2O. That is, the sugar molecule represents a narrow range of initial conditions each with a high future directed weighting, whereas the constituents CO_2 and H_2O of a potential sugar molecule represent

a much larger range of initial conditions, each having a relatively lower future directed weighting. The sugar molecule was created in the past by utilisation of f-negentropy from the surrounding environment. By decomposing the molecule, an organism changes the future directed weightings w_{fi}, and thus influences the likely future state of the universe. Now imagine an organism which has control in the future over whether a sugar molecule will be synthesized out of less ordered constituents. This would be the result of utilisation of p-negentropy from the surrounding environment. Depending on whether this hypothetical synthesis were to take place, the most likely present state of the universe would be altered. For example, an organism might synthesize a sugar molecule in the future after a certain nuclear decay took place, and not synthesize the molecule in the future if the particular decay did not occur. By controlling the future synthesis, the occurrence of the nuclear decay thus could be made more likely. In other words, certain members of the ensemble K, which include the nuclear decay, would be given a preference by changing the past directed weightings. We again emphasize that the sugar molecule example is only for the purpose of illustrating how the use of p-negentropy might operate. It is not likely to be by this particular means, if only because of the large amount of negentropy held by a sugar molecule.

FEATURES OF CAUSALITY VIOLATION EXPERIMENTS

We now consider some predictions based on our formalism for time-symmetric thermodynamics and our assumptions about the use of p-negentropy by organisms. In each case we compare the predictions with selected available evidence on causality violation, in particular the admirable work of Schmidt, which is well adapted for our purposes.

If the p-negentropy available to influence a given event is less than $k \log_e 2$, then for the results to be noticeable the process must be random or near random with respect to future directed weightings. This requirement accords well with the fact that the most statistically significant results on causality violation have involved near-random processes, such as sequences of cards or of radioactive decays. On the other hand, our suggestions, at least at first sight, cannot explain events apparently requiring massive amounts of p-negentropy, such as spoon bending, table tapping, or levitation.

The requirement that events affected by application of p-negentropy be random or near random leads to a specific prediction. In a series of truly random events - such as nuclear decays - p-negentropy

applied after the series could influence every single event. But in a series of pseudo-random events - such as computer generated random numbers - the influence of p-negentropy normally could not be so great. In a series of pseudo-random events, the sequence is fixed. The degree of randomness associated with the series is dependent on the number of possible (fixed) sequences that might have been generated. For a sufficiently long series, therefore, the degree of randomness associated with truly random events will be greater than for pseudo-random events. We predict that in such a direct comparison, higher significance levels will be achieved using truly random events than using pseudo-random events. Furthermore, by using a suitably limited generator of pseudo-random events, the expected difference in significance levels can be mathematically specified.

There is a somewhat subtle qualification to this prediction. It assumes a fixed sequence of choices by the subject. That is, successes above a chance level result from the use of p-negentropy to select out sub-ensembles of K which include sequences of random events which, for a given subject selected input sequence, result in a more than average number of hits. There is also the possibility that the subject might use p-negentropy to select out his or her own choices. But the amount of p-negentropy required to do this would almost certainly be much larger than that required to affect the sequence of truly random events. The difference between the two methods of applying p-negentropy becomes apparent in the case of a subject who continually makes the same choice (as is the case for one of Schmidt's (1969) subjects). Assuming the p-negentropy explanation, this technique can work well when a true random source is operative, since each event can be influenced after the trial. For a pseudo-random sequence of events, this method obviously would not be nearly so successful.

Next, consider <u>when</u> p-negentropy must be applied to change the probability of an event. Take the analogy to f-negentropy. If one uses f-negentropy at a time t_1 to change the weightings $w_{fi}(t_1)$, this will influence the weightings $w_{fi}(t)$ at all future times, $t > t_1$. Thus by changing the present, one influences the future. By analogy, if one were to use p-negentropy at a time t_2 to change the weightings $w_{pj}(t_2)$, this would influence the weightings $w_{pj}(t)$ at all previous times, $t < t_2$. In other words, by changing the present, one might influence the past. This means that it does not matter whether the event to be influenced by application of p-negentropy occurs in the present or in the past. In each case p-negentropy is applied in the future of the event. This prediction agrees with the observed independence of psi under time

displacement (Schmidt 1975). In this vein, with p-negentropy it is no more difficult to explain precognition than ESP or PK. In each case the subject can be thought of as altering the probabilities of an event by applying p-negentropy in the future, or equivalently by changing the boundary conditions in the future.

In using p-negentropy in the future, and changing the most probable systems in K, it should not matter what happened to cause the event being influenced. One is changing the weightings of the possible histories of the universe on the basis of particular outcomes. In particular, it should not matter whether the system giving rise to the event is simple or complex, or whether the subject knows the operational details of the process whose result is being influenced. This prediction agrees with available experimental evidence (Kanthamani, 1974; Schmidt 1974; Schmidt & Pantas 1972) which shows that causality violation apparently occurs independently of the complexity of the source of the events (e.g. of a random number generator), and of knowledge by the subject as to the complexity of the source.

In using p-negentropy in the future, and changing the most probable systems in K, it should also make no essential difference where the event being influenced occurred. Again, one is changing the probabilities of the possible histories of the universe on the basis of particular outcomes. Aside from possible differences in psychological conditions and feedback, causality violation should occur independently of the distance of the agent from the event. This again is in agreement with the available evidence (Osis et.al., 1971). Indeed, the p-negentropy mechanism for causality violation avoids what has been considered a major problem in explaining psi phenomena (Beloff, 1970) - how the subject discriminates the target from an infinite number of other objects in the environment. With p-negentropy, the subject does not communicate with objects directly, but introduces correlations between different events by changing future boundary conditions, and thereby selecting out certain space-time histories of the universe as being more probable than they otherwise would be.

Suppose that an organism were to lose its ability to collect p-negentropy at some time in the future. Then after that time in the future it no longer would be able to exert further influence on events in the present. Our hypotheses suggest, therefore, that if an organism were prevented from collecting p-negentropy at all times after experimental tests for causality violation, then no significant results would be obtained, compared to the situation where the organism was able to collect p-negentropy. Therefore an experimental test of our ideas would be to compare test results on subjects that are and are not isolated from p-negentropy after the

tests.

The obvious difficulty with this test is that we do not know how p-negentropy is utilised by organisms. One way around this problem would be to use plants (assuming that psi effects were not due to the experimenter) and to (say) incinerate selected groups immediately after the tests. This would certainly close all channels to p-negentropy. The idea here is that the longer the organism is able to collect p-negentropy after the tests, the greater the possible significance in the results. If an organism died immediately after testing, the chance of significant results having been recently achieved would be minimised; if the organism only died years later, p-negentropy could have been applied during the interval - the chance that death at this later stage would affect the results would usually be small.

This difficulty - that we do not know how p-negentropy is utilised by organisms - could be a source of further information. It might be possible to isolate subjects after completion of tests in different ways, and thereby to locate the channel through which p-negentropy is utilised. For example, if the skin were such a channel, one might find that clogged pores or deodorised skin reduced the effectiveness in using p-negentropy. Another important question involved in such tests concerns the length of time required by an organism to gather a quantity of p-negentropy. This might be roughly guessed by analogy to the time taken to gather f-negentropy, or inferred from psi experiments (e.g. Schmidt, 1973).

Often a certain experimental situation will lead to results showing significant causality violation, whereas a similarly prepared situation may unexplainably give results little different from chance. This could be explained in terms of our model if it were found that future histories of the experimental setups were significantly different. Thus it might be that in the experiment in which null results were obtained, the agent was in the future isolated from collecting and using p-negentropy. In other words, whereas the two experiments may have been identical with respect to past boundary conditions, they may have differed in terms of their future boundary conditions. We predict then that the outcome of tests for causality violation will be sensitive to changes in conditions, especially those depending on the results themselves, after the completion of formal testing.

Our formalism for time-symmetric thermodynamics gives mathematical predictions about how much a given amount of p-negentropy will change the probabilities for the occurrence of different events. For example, one may obtain a formula for the combined effect of different psi sources. The mathematics in this case is identical with that of Schmidt (1976). (Indeed, there are many similarities between this work of Schmidt's and our ideas.

P-negentropy may be considered to correspond to Schmidt's inductively postulated psi source.) Further assumptions are required before comparisons between theory and experiment can be made. For example, if one assumes that p-negentropy from a single source is supplied at roughly a constant rate to a series of otherwise random events, and is applied uniformly to the individual events, the increase in the scoring rate should vary inversely as the square root of the rate of generation of the random events (for small increases). This prediction is not incompatible with the results of Schmidt (1973). More generally, if a given amount of p-negentropy is applied with complete effectiveness to a series of events, the statistical significance of the outcome should not depend on the number of events being influenced.

There are a large number of other features of experiments on psi, such as chronological declines, psi-missing, temporary inhibition by change of task, position effects, the "sheep-goat" distinction, differential effects, etc.. Our hypotheses do not permit predictions concerning most of these features, especially about psychological aspects of psi. In some cases plausible subsidiary hypotheses enable an explanation to be made. For example, scoring especially well at the beginning of a test session could be explained by greater available time in the test situation for collecting and applying p-negentropy after the testing begins; later testing might be the time-reversed version of "warming up" or experience. The importance of these features for our purposes, though, is that none of them seems immediately incompatible with our hypotheses.

This is not to say that every bit of evidence unambiguously supports our model. At least some features of psi experiments are puzzling in terms of our hypotheses. For example, we would predict that PK experiments with dice would give results depending only on the outcomes, and therefore independent of the density of the dice, whereas this is not always observed in practice (Cox, 1971). Anomalies are expected with any model, and if ours is found to be useful, further elaborations certainly will be required.

Because of the inevitable presence of anomalies (Kuhn, 1970), it is not inconvenient for a model to have an all-encompassing explanation for most recalcitrant evidence. For our model, this involves reference to long term effects. Although p-negentropy used to change the probability of a current event usually will be collected and applied in the period immediately after the occurrence of the event, there is always the possibility that it may be applied long after the event. P-negentropy applied in the relatively distant future changes the past directed weightings at that future time. This change, propagated into the past through a long causal chain of events in the future, then can affect an event in the presence. For

example, the outcome of one experiment may affect the design of a later experiment. P-negentropy exerted in the later experiment will favour some designs more than others, and this could affect the outcome of the earlier experiment. (For example, an experiment where the dice are rigged so that one can not achieve a non-random score would probably be given a low past directed weighting.) If our model is to be useful, though, use of such a method of explanation must be kept to a minimum.

Our model predicts that feedback is essential. For application of p-negentropy to change the probability of an event, it is absolutely essential that the agent, at some time in the future, gain some knowledge of the outcome. This knowledge does not need to be complete; for example, it might be only that an interesting result was or was not achieved. Because of ambiguities concerning the most effective amount, timing, and type of feedback, we make no detailed predictions on this issue. Suffice is to note that the need for feedback for the effective application of p-negentropy accords well with the generally observed importance of feedback in experiments on causality violation.

We have discussed a number of areas where our hypotheses lead to testable predictions. In several cases they are in agreement with known features of causality violations in experiments on psi: that it is normally manifested in otherwise random sequences of events; that it is manifested independently of the event-generating apparatus; and that space or time displacement of the events has little effect on the significance of the results. Further plausible assumptions lead to other predictions in agreement with known results, such as that the statistical significance of the outcome should not depend on the number of events being influenced in a given time. Other features of psi - such as psychological aspects and the significance of feedback - are quite compatible with our hypotheses, although we do not make detailed predictions concerning them. Finally, we make some predictions that have not yet received an experimental test: that the significance of psi tests using truly random events will be greater than those using pseudo-random events; and that experimental conditions after the completion of formal tests, such as the isolation or disturbance of agents, will influence results.

SUMMARY

A formalism for time-symmetric thermodynamics is presented, and used as a basis for explaining the observed occurrences of causality violation in experiments on psi. In this formalism there

are two types of entropy: f-entropy (identifiable with normal entropy), arising from boundary conditions in the past, and p-entropy, arising from boundary conditions in the future. We suggest that p-negentropy may exist in small amounts and that it can be used by living organisms. With the thermodynamical formalism and with these latter two assumptions, a number of features of documented instances of causality violation are explained: that it is noticeable mainly in otherwise random sequences of events; that it is manifested independently of the complexity of the event-generating apparatus; and that it is manifested independently of time displacement of the events. Further, yet untested predictions also are made: that the results of psi tests using truly random events will be more significant than those using pseudo-random events; and that results will be sensitive to experimental conditions after the completion of formal testing.

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APPENDIX

We wish to show that the simple time development equations (6) and (7) are sufficient to make the ensemble K independent of the defining times t_1 and t_2. In specific terms this means that $n(K_{i,t})$ is to be unchanged when K is defined in terms of $w_{fi}(t_3)$ and $w_{pj}(t_4)$, where $t_1 \leq t_3 \leq t_4 \leq t_2$.

For convenience we define an ensemble

$$K^f = \sum_i w_{fi}(t_1)\, G_{i,t_1} \tag{A-1}$$

The ensemble K is then

$$K = \sum_j w_{pj}(t_2)\, K^f_{j,t_2} \tag{A-2}$$

We will show that the simple time development equation for the w_{fi} follows from requiring that $n(K^f_{k,t})$ be independent of t_1.

Define an alternative ensemble to K^f,

$$K^{fa} = \sum_i w_{fi}(t)\, G_{i,t} \tag{A-3}$$

From (A-1) and (A-3) we have

$$n(K^f_{k,t}) = \sum_i w_{fi}(t_1)\, n(G_{i,t_1;k,t}) \tag{A-4}$$

$$n(K^{fa}_{k,t}) = w_{fk}(t)\, n(G_{k,t}) \tag{A-5}$$

Setting $n(K^f_{k,t}) = n(K^{fa}_{k,t})$, and using (2) and (3) gives the simple time development equation (6) for w_{fi}.

If this demonstration is to hold for the full ensemble K, then using (A-2) it must be true that:

$$\frac{n(G_{i,t_1;k,t;j,t_2})}{n(G_{k,t;j,t_2})} = \frac{n(G_{i,t_1;k,t})}{n(G_{k,t})} \qquad \text{(A-6)}$$

The truth of (A-6) can be seen by interchanging $n(G_{k,t;j,t_2})$ and $n(G_{i,t_1;k,t})$ and noting that in a system described by a Markov chain formalism (i.e. the transitions between states in the equilibrium ensemble G), the future development of the system depends only on the present state.

The derivation of the simple time development equation (7) for w_{pj} is entirely analogous to the above.

GROUP SELECTION OF TARGET PAINTING

Carroll B. Nash

William G. and Lendell W. Braud (1973) reported that volunteer subjects showed significant performance on a free-response ESP task after they had undergone a procedure combining progressive muscular relaxation, suggestions of mental and physical relaxation, and suggestions that the induced state of relaxation was optimal for ESP. After a fifteen minute period of instruction and relaxation, during which the agent concentrated for five minutes on a randomly selected target picture, the subject was returned to his normal state of consciousness and asked to express his impressions. In individual tests of subjects the impressions were recorded on tape, while in group tests of subjects the impressions were recorded by the subjects on sheets of paper. In the group tests each subject attempted to identify the target picture out of a set of six pictures shown to the group as a whole.

In the present experiment in each of nine runs, subjects in the experimenter's parapsychology course attempted to identify which of four color transparancies of paintings the experimenter looked at while the subjects were in a state of relaxation. The painting that served as the target in each of the nine runs was randomly selected from the set of four paintings for that run. A different set of four paintings was used in each run.

At the beginning of each run, the subjects were progressively relaxed both physically and mentally through oral instructions given by the experimenter. After a five minute period of instruction and relaxation, the experimenter looked at the color transparancy of the target painting in a viewer for twenty seconds. He told the subjects he was doing this, instructed them to attempt to visualize the painting, and suggested that they would be able to select it from the four paintings that he would show them after they had been returned to their normal state of consciousness. The subjects were then awakened and shown the four paintings by projecting them consecutively on a screen, each for ten seconds. The subjects were then told to write on a slip of paper, along with their name and the date, the number of the

painting that they thought the experimenter had looked at. In order to provide a measure of the psychological preference for each of the four paintings in a set, a second session was performed similar to the first with the same nine sets of paintings, but with different subjects.

The score (number of direct hits/number of subjects) on the painting in the first session when it was the target was compared with the score on the same painting in the second session when it was not the target. The results are presented in table 1.

TABLE 1

Run	Hits/subjects	
	1st session	2nd session
1	4/22 = .18	20/67 = .30
2	9/24 = .38	28/69 = .41
3	5/26 = .20	14/58 = .24
4	7/24 = .29	15/50 = .30
5	2/26 = .08	8/28 = .29
6	4/25 = .16	9/39 = .23
7	2/27 = .07	6/30 = .20
8	5/26 = .19	8/24 = .33
9	4/20 = .20	7/23 = .30
total	42/217 = .19	115/388 = .30

It can be seen from the table that in all nine runs fewer hits per subject occurred in the first session when the painting was the target than in the second session when a different one of the four paintings was the target. The probability of this occurrence in all nine runs is $(\frac{1}{2})^8 = .0039$.

The present experiment differs from that of the Brauds in that the subjects were relaxed for five minutes instead of fifteen, the agent concentrated on the target picture for twenty seconds instead of five minutes, the target picture was one of four pictures instead of one of six, the subjects selected the target picture directly rather than through the use of their recorded impressions, and success was determined by a different method as described above.

The significant psi-missing in the present experiment is attributed to the experimenter as it is not likely that it would

have been manifested by so many subjects solely by their own action. The experiment indicates that significant results can be obtained in an ESP test of painting identification by methods simpler than those which have previously been reported.

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A LARGE-SAMPLE CLASSROOM ESP CARD-GUESSING EXPERIMENT

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Because of the unreliability of ESP scores contributed by unselected subjects, large samples are often needed to detect significant effects. While such effects are usually quite weak, they nevertheless can contribute important clues to our understanding of the psi process. Indeed, much of what we know about the nature of psi comes from research on unselected subjects.

We were presented with an opportunity to collect ESP data from an unusually large sample of unselected subjects in conjunction with an attempted replication of an experiment designed primarily to demonstrate that subjects with above average ESP ability can learn to stabilize and enhance this ability through feedback training (Tart, 1976). The experiment to be reported here was part of a screening process to select suitable subjects for such training. Its success in this regard will be reported in another paper. We will focus here on how ESP scores in this screening experiment related to certain psychological variables introduced for the purpose of gaining further understanding of the psychology of psi.

The one variable systematically manipulated in this experiment was whether a "clairvoyance" or "GESP" procedure was used. Very briefly, on one run of each session a standard BT procedure (Rhine & Pratt, 1957) was followed, while on the other run one or more experimenters looked at the face of the target card on each trial while attempting to "send" it to the subjects.

Because of its possible relation to the reality of telepathy, GESP versus clairvoyance has been one of the most important conceptual and methodological distinctions in ESP research. Probably the most ambitious research project in which such a manipulation was included was the famous experiment by Coover (1917), who used playing cards as targets. Coover found significant overall evidence of ESP when he pooled his GESP and clairvoyance trials, but he found no significant difference between them. Although this

variable has been manipulated frequently in subsequent card-guessing experiments (e.g., Adcock & Quartermain, 1959; Bevan, 1947; Casper, 1951), we are aware of no group experiments that have demonstrated a clear-cut significant difference between these two types of test. Our own reason for introducing this manipulation again was simply to see if it would yield significant results using a larger sample of subjects than had been used in previous research.

A related motive prompted us to include a question about subjects' belief in ESP. A large number of experimental results converge on the conclusion that subjects who believe in ESP ("sheep") score more positively on ESP card tests the first time they are tested than do non-believers ("goats") (e.g., Schmeidler & McConnell, 1958), but such effects are rarely significant in individual experiments with small sample sizes (Palmer, 1971). We hoped that with our unusually large sample a clear-cut confirmation of the "sheep-goat hypothesis" could be obtained.

Finally, we asked our subjects to rate their moods at the time of testing. Although the relationship between ESP and mood has been explored in some experiments (e.g., Rogers, 1966) enough consistent results have not accumulated to justify a prediction.

METHOD

Subjects

The great majority of our subjects were students in selected undergraduate classes at the University of California, Davis. Twenty-three classes were tested, ranging in size from eight to 260. The total number of subjects tested in this way was 2360, of whom the majority were students in psychology or biology classes. Although it is inevitable that a small proportion of subjects were tested twice, we think that this percentage is much too small to bias our conclusions.

A few subjects (N=65) were recruited in response to media advertisements soliciting volunteers to participate in screening sessions for the ESP training experiment. Eight such sessions were held, but because the advertisements were not particularly effective, only a handful of persons came to each session. The procedure for these sessions was the same as for the formal classroom sessions.

Experimenters

The experimenters were students in an upper-level undergraduate course in experimental psychology taught by the second author. The

sixteen students in the class were divided into five teams of three to four students each. The students decided the composition of the teams among themselves, based partly on shared periods of "free time". Team members were instructed to seek out professors whom they knew and ask for 10 to 15 minutes in one of their classes to conduct the ESP testing. Cooperation was generally good but not universal. The actual procedure for conducting the experiment was worked out in the experimental psychology class, and each team rehearsed the procedure in front of their classmates before actual testing commenced.

The same students also conducted the sessions held in response to the advertisements, but team compositions generally were not maintained for these sessions.

Procedure

At the beginning of each session the team members introduced themselves and distributed the record sheets. Subjects first were asked to fill in their names, ages, etc., and to answer the mood and belief questions. Then one of the experimenters explained the procedure for the ESP test.

The target material consisted of a modified deck of 48 playing cards, each containing 12 aces, 12 twos, 12 threes, and 12 fours, with uniform backs. This master deck was then separated into two decks of 24 cards each. Two of the teams (3 and 5) divided the master deck in such a way that each component deck contained an equal number of each target, while the other teams simply divided it in half after shuffling. (The exact method for splitting the deck had not been specified in advance.) The subjects' task was to guess on each trial the number of the target card; thus the probability of a hit was one in four.

The cards were thoroughly shuffled before testing began. Before each run, the component deck to be used for that run was placed face down on the table in front of the class. One of the experimenters (E1) called out the word "next" or gave some comparable auditory signal every five seconds. At the time the first signal was given, another experimenter (E2) picked up the top card, held it for a couple of seconds, and then placed it face down on another pile. This process was repeated every five seconds until the run was completed.

On clairvoyance runs, E2 did not look at the card when he picked it up. On GESP runs, both E2 and any other experimenters present except E1 concentrated on the face of the target card during each trial. E1 was kept blind as to the identity of the target cards, because such knowledge conceivable could be transmitted to the

subjects through auditory cues (e.g., voice inflections) associated with the time signals. Although it is technically possible that visual cues could have been provided by the senders, this possibility is extremely remote. Such a hypothesis requires us to assume that experimenters trained to be sensitive to the problem of sensory cues were unconsciously making lip movements or other subtle gestures that naive subjects were capable not only of detecting but also of decoding without benefit of feedback, or that the experimenters were so inept or dishonest that they gave overt cues and that none in the class, detecting such cues, called them on it. Furthermore, E2 operated behind a cardboard box or similar contrivance that shielded the cards, and to a large extent E2 himself, from the subjects' view.

When the experiment began, it was agreed that the GESP run be given first. About midway through the experiment, however, the teams were advised to begin giving the clairvoyance run first as a control for confounding between type and order of runs. Subjects in the later classes simply were instructed to fill out the clairvoyance column on the record sheet first.

In many of the classes, particularly the larger ones, the testing sessions were rushed and somewhat chaotic. Some students came to class late, talking, shuffling books, and generally creating distractions. In several classes "wise guys" tried to ridicule and disrupt the experiment. In spite of these difficulties, the testing was successfully completed in all the classes.

Recording and analysis of data

Immediately following each session, the experimenters recorded the target sequences from the decks of cards onto scoring templates. They later used these templates to hand score all the record sheets to identify persons who might qualify for the later training study.

When the experiment was over, all the record sheets (which had been stored in separate packets for each session) were given to professional punchers at the U.C. Davis Computer Centre. These keypunchers, who had no interest in the experiment, transferred subjects' ESP responses, mood and belief ratings, class code, and demographic data onto IBM cards. The keypunchers were instructed to leave columns blank if no response or an ambiguous response was given on a particular trial or question, and to ignore record sheets that obviously were not completed in good faith. All cards were verified after being punched initially. The target sequences were punched and verified by the first author directly from the scoring templates.

A computer program was written to punch a new deck of cards

containing number of hits on each run for each subject, along with other information (class code, mood, belief, etc.) transferred directly from the cards punched by the keypunchers. The program was written such that cards were not punched for any subject who did not make 48 scorable ESP responses. The effect of this decision was to substantially reduce the sample size, from 2425 to 1835. Although some of these data may have been salvageable (e.g., data from subjects who successfully completed one of the two runs), this conservative procedure left us with quite a healthy sample size and maximized the chances of eliminating subjects who were confused about the procedure or who did not take the test seriously.

Simple statistical analysis (t tests, correlations, one-way ANOVAs) were performed on this new deck of cards using programs in the SPSS package (Nie & Hull, 1970). The more complex analyses will be described in the next session. All p values are two-tailed unless stated otherwise.

RESULTS

The mean ESP score for the 1835 qualifying subjects was 12.06, which did not differ significantly from the expected mean of 12.00 ($t=0.89$). Likewise, the variance of 8.94 was almost identical to the expected variance of 9.00 based on the normal approximation to the binomial, i.e., NPQ ($CR=0.22$). Scores ranged from 3 to 23.

However, when the total ESP scores were broken down into GESP and clairvoyance subscores, a more interesting pattern emerges. The relevant means are listed in table 1. "Order" is a between-subjects

TABLE 1

Mean ESP scores on GESP and clairvoyance runs

	GESP	Clairvoyance	Total
Order 1 (N=850)	6.11	6.06	12.17
Order 2 (N=985)	6.19	5.79	11.98
Total	6.15	5.91	

variable that refers to which type of run was administered first. "Order 1" means that the GESP run was given first, while "Order 2" means that the clairvoyance run was given first. The columns represent the GESP and clairvoyance means, irrespective of order.

A two-factor analysis of variance (ANOVA) with repeated measures on one factor (Winer, 1962) was performed on these means. The unequal sizes of the two groups on the between-subjects factor in this and subsequent multifactor ANOVAs were corrected by the unweighted means solution (Winer, 1962, p.374). This procedure gives equal weight to each group, i.e., it treats the groups as if they were of equal size. These multifactor ANOVAs were each computed twice on an electronic calculator, using means, sum scores, etc., provided by the SPSS output.

The results of the first analysis are summarized in table 2. The

TABLE 2

Summary of analysis of variance
(Type of run by order)

Source	SS	DF	MS	F
A (Order)	9.13	1	9.13	2.04
Subjects w/grps.	8186.74	1833	4.47	
B (Type of run)	45.63	1	45.63	9.89&&
A x B (Position of run)	28.29	1	28.29	6.13&
B x Subjects w/grps.	8455.16	1833	4.61	

&: $p < .05$; &&: $p < .01$

significant main effect on type of test indicates that subjects scored significantly more positively on the GESP run than on the clairvoyance run. The significant interaction reflects the fact they also scored significantly more positively on the second run of the session than on the first run; in other words, there was a significant "incline effect". Given the absence of a significant main effect for order, these two significant effects may be considered additive.

In terms of the divergence of various run-score means from the expected value of 6.00, the following effects were noted. The overall mean for the GESP runs was significantly above chance

TABLE 3

Mean ESP scores on GESP and clairvoyance runs for each team

	GESP	Clairvoyance	Total	$t_{diff.}$	$F^{a}_{1,1779}$
Team 1 (N = 628)	6.27	5.67	11.94	4.89&	11.02&
Team 2 (N = 238)	6.22	5.88	12.10	1.70	3.48
Team 3 (N = 181)	6.20	6.29	12.49	0.40	0.21
Team 4 (N = 533)	6.04	6.14	12.18	0.72	0.27
Team 5 (N = 204)	5.98	5.79	11.77	0.84	0.99

&: $p < .001$
a: pairwise comparisons based on ANOVA (table 4)

TABLE 4

Summary of analysis of variance (Type of run by team)

Source	SS	DF	MS	F
A (Team)	40.61	4	10.15	2.27
Subjects w/grps.	7966.42	1779	4.48	
B (Type of run)	24.87	1	24.87	5.42&
A x B (Interaction)	48.35	4	12.09	2.63&
B x Subjects w/grps.	8167.42	1779	4.59	

&: $p < .05$

($\bar{x}$=6.15, t=2.94, $p < .01$) while the mean for the clairvoyance runs was not significant ($\bar{x}$=5.91, t=1.80). The overall mean for the second run of each session was significantly above chance ($\bar{x}$=6.13, t=2.54, $p < .02$) while the mean for the first run was not significant ($\bar{x}$=5.94, t=1.32). As for individual cell means of table 1, the lower left cell (GESP when given second) was significantly above chance ($\bar{x}$=6.19, t=2.62, $p < .01$), while the lower right cell (clairvoyance when given first) was significantly below chance ($\bar{x}$=5.79, t=3.20, $p < .01$).

Post-hoc analyses revealed that the effect of type of test on ESP scores differed among the five experimenter teams. (Data from subjects tested in the special sessions were excluded from these analyses). The relevant means are reproduced in table 3 and the ANOVA in table 4. The significant interaction effect in table 4 confirms that the superiority of GESP over clairvoyance scoring was not consistent across teams. Inspection of table 3 reveals that Team 1 is largely responsible for this superiority. Pairwise comparisons for each team separately reveal that the GESP-clairvoyance difference is highly significant for Team 1 (F=11.02, $p < .001$), and this difference remains significant ($p < .05$) even when corrected for multiplicity of comparisons by the highly conservative Sheffe test.

The GESP mean for Team 1 subjects was significantly above chance ($\bar{x}$=6.27, t=3.15, $p < .01$) while the clairvoyance mean was significantly below chance ($\bar{x}$=5.67, t 3.85, $p < .001$). A straightforward t test assessing the difference between these two means was highly significant (t=4.89, df=627, $p=10^{-6}$).

On the contrary, the significant incline effect was generally consistent across the five teams (see tables 5 and 6).&

The sheep-goat hypothesis was not supported. The mean ESP scores for each response alternative are presented in figure 1. The results of a one-way ANOVA comparing these means to one another was non-significant (F=0.74). The only encouraging sign was the fact that the extreme "goats" scored significantly below chance on the

& The main effect for position of run is not significant in this analysis primarily because the "unweighted means" procedure gives added weight to the results of teams who tested the fewest subjects. It so happens that subjects tested by these teams did not show the incline effect or did not show it as strongly as the teams who tested most of the subjects, hence the overall significance of the effect was reduced. On the other hand, team differences in this regard were not strong enough to yield a significant interaction, so it is proper to conclude that the incline effect did not differ significantly as a function of team.

TABLE 5

Mean ESP scores on first and second runs for each team

	1st	2nd	$t_{diff.}$
Team 1 (N = 628)	5.86	6.08	1.75
Team 2 (N = 238)	5.90	6.19	1.45
Team 3 (N = 181)	6.20	6.29	0.40
Team 4 (N = 533)	5.94	6.24	2.39&
Team 5 (N = 204)	5.97	5.79	0.84

&: $p < .05$

TABLE 6

Summary of analysis of variance (Position of run by team)

Source	SS	DF	MS	F
A (Team)	40.61	4	10.15	2.27
Subjects w/grps.	7966.42	1779	4.48	
B (Position of run)	14.37	1	14.37	3.10
A x B (Interaction)	22.38	4	5.59	1.21
B x Subjects w/grps.	8248.12	1779	4.64	

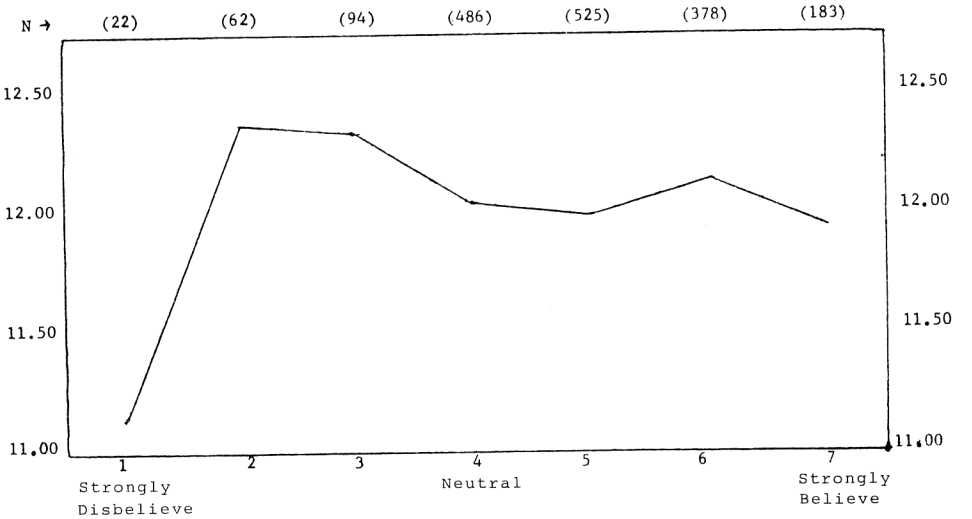

FIGURE 1

Mean ESP scores as a function of belief ratings

basis of a one-tailed test ($\bar{x}$=11.14, t=1.73, $p < .05$). The Pearson correlation between ESP scores and belief ratings was -.00.

Likewise, there was no significant relationship between ESP scores and mood ratings. The correlation here was -.02. Finally, there was no evidence of a sex difference. The mean ESP score for 825 males was 12.14 compared to 11.99 for 952 females. The difference did not reach significance (t=1.03).

For none of the above individual difference variables was there a significant interaction with type or position of run.

DISCUSSION

We are aware of no nonparapsychological explanation that can reasonably put forward to account for the results of this experiment. experiment. The safe-guards against sensory cues have already been described. Even if one were to propose sensory cues as the explanation for the significant positive GESP scoring of subjects tested by Team 1, this could not account for the even more significant negative scoring of these subjects under clairvoyance conditions.

Although we are confident that recording errors were minimal in this experiment, we are not prepared to conclude they were nonexistent. Even professional keypunchers will occasionally make errors on a task of this magnitude. Although the experimenters were instructed to double check the recording of target sequences, it is possible a few mistakes may have been made here as well. As a check on the target sequences, the first author examined each one to see if each symbol appeared an equal number of times. Out of 31 such sequences, he found only one where one symbol appeared too often at the expense of another symbol.

What is important, however, is not to eliminate all errors but to eliminate systematic errors that might bias the results. Recording of the target sequences was unbiased, because neither the experimenters nor the first author had knowledge of the distributions of subjects' responses at the time. Unfortunately, due to an oversight on the part of the first author in giving instructions to the experimenters, tally marks were made next to the hits on the record sheets of about two thirds of the subjects. Thus, the keypunchers did have partial information about the target sequences in many of the classes, and this information could conceivably have led to systematic recording errors.

Such recording errors are unlikely, first, because the keypunchers had little or no reason to make motivated errors and, second, because the significant effects involved differences between run scores rather than total scores. Nevertheless, an empirical check

was made by comparing the results of classes where target information was recorded on the record sheets with results from classes where it was not. The GESP - clairvoyance effect was actually stronger in those classes where the target information was not recorded (Most of these classes were tested by Team 1). Although the incline effect appeared predominantly in those classes where the target information was recorded, this effect can hardly be attributed to recording errors, because information about order of runs was not present on the record sheets. If the keypunchers, for example, had a tendency to make more errors on the first run they punched, this would sometimes be the first and sometimes the second run of the session. In conclusion, we cannot see how recording errors can account for the results of this experiment.

Various randomness checks on the pooled target run sequences of each team resulted in only one significant departure from randomness, an outcome quite consistent with chance probabilities. The one significant effect was a difference in the frequency of occurrence of the four targets in the GESP and clairvoyance sequences of Team 4 ($\chi^2=9.27$, df=3, $p < .05$). Further analysis revealed that this effect was attributable almost entirely to the target sequence used in one class consisting of only 17 subjects. The results from this class tended to dilute both significant effects represented in table 2.

The only other potential criticism we can think of that merits discussion is possible bias introduced by a "stacking effect" (Greville, 1944). This bias results from the fact that all subjects in each class responded to the same target order. Although it would have been a practical impossibility to generate a separate target order for each subject in this experiment, there were 31 target orders (48 trials each) overall, and the most frequently called target order applied to only 210 subjects, 11% of the total sample of 1,835. Thus the stacking effect is quite diluted. We are ignorant of how a proper correction for the stacking effect could be applied to the analyses described in the last section. However, extrapolating from a comment by J.G. Pratt (personal communication) that critical ratios are generally reduced about 10% by simple stacking effect corrections, we note that all our significant test statistics remain significant after such a reduction. In other words, the significant effects obtained in this experiment are robust enough to withstand any reasonable correction for a stacking effect, especially since the stacking effect is only partial. (One measurable symptom of bias produced by a stacking effect would be an artifactual increase in the variability of class means on the ESP test combined with a reduction of within-class variability of the ESP scores. These effects would both contribute to a significant

one-way ANOVA comparing the mean ESP scores of the 23 classes. The fact that such an analysis produces a very small F ratio ($F_{22,1784}$= 0.64) is further evidence that a stacking effect is not a serious source of bias in this experiment.)

Having concluded that the psi effects in this experiment are genuine by generally accepted methodological and statistical standards, we now address the question of how such effects are to be interpreted. The more theoretically interesting of these effects is probably the superior rate of scoring on the GESP runs. The fact that this effect varied as a function of team indicates that some kind of experimenter effect is involved. Although subjects were not randomly assigned to the teams, the subject population was so homogeneous that it is unlikely subject differences are responsible for the interaction. Of greater relevance, in our judgment, is the fact that members of Team 1 (the team most responsible for the main effect) strongly believed that the GESP procedure was more likely to be effective than the clairvoyance procedure. This attitude easily could have been communicated to their subjects by subtle verbal and/or nonverbal cues, thereby affecting their motivation and task orientation on the two types of test. Such differences in turn could have influenced ESP scoring. Thus one need not assume "active agent telepathy" or any parapsychological contribution on the part of the experimenters, although this remains a possibility that deserves consideration. The fact that GESP superiority seems to depend on the experimenters may also help explain why it has not been found in earlier experiments.

Given the legendary prominence of decline effects in ESP testing (Pratt, 1949), the incline effect that emerged in this experiment was initially a bit of a shock to us. On the other hand, two runs is a quite brief ESP test, and one would really not expect a decline effect to take hold this quickly. It is our suspicion that the incline effect came about because of the harried nature of the experimental situation. Subjects had very little opportunity to get settled before the first run was suddenly foisted upon them, and this may have been responsible for the chance scoring on this run. By the second run we may assume they had become adapted to the testing routine and were better able to exercise their psi capabilities.

The failure of the "sheep-goat" hypothesis to be supported is particular noteworthy. Although it has generally been the position of the first author that the results of individual experiments as such should not be given much weight in evaluating this hypothesis (Palmer, 1971), the present experiment is an exception for two reasons. First, the sample size was sufficiently large that a real

effect of the magnitude expected on the basis of previous research should be statistically significant. Second, there was independent evidence of genuine ESP in the data.

The one secondary finding that supported the sheep-goat hypothesis was the significant psi missing on the part of the extreme goats. If Schmeidler's original sheep-goat classification scheme had been used, this finding would have been somewhat impressive, because she classified all but extreme goats as sheep (Schmeidler & McConnell, 1958). However, the question asked in our experiment was not whether ESP was possible in the testing situation (the question asked by Schmeidler), but whether ESP existed at all. According to Palmer (1971), the question we asked represented criterion 2 rather than Schmeidler's criterion 1, and criterion 2 was found in previous research to separate high and low scorers most effectively when undecideds were classified as goats (Palmer, 1971).

Clearly, the sheep-goat hypothesis based on criterion 2 was not supported in this experiment. In an earlier experiment reported by the first author (but conducted by Schmeidler) where the various sheep-goat criteria were compared, criterion 2 was not only unsuccessful in providing significant support for the sheep-goat hypothesis, but it produced a reversal of the predicted effect. Classification by criterion 1, howver, yielded a significant confirmation of the sheep-goat hypotheses (Palmer, 1973).

In his original monograph, Palmer (1971) concluded that on the basis of evidence available at the time both criteria 1 and 2 had been shown to discriminate above and below chance scorers to a significant degree on first testing. The results of the present experiment and the one cited in the previous paragraph have convinced him that criterion 2 should be excluded as a successful predictor. This does not mean that experiments using criterion 2 will not on occasion produce positive results; after all, criterion 2 is rather highly confounded with criterion 1. Nor should the dethronement of criterion 2 necessarily be considered a setback, insofar as it helps define more precisely the nature of the believe variable that does correlate with ESP scores. Clearly, criterion 1 is more sensitive than criterion 2 to the nature of the experimental situation, which research has shown to be increasingly important in determining the outcome of ESP experiments. To the degree that believe in ESP reflects some underlying personality trait, it is also possible that due to cultural changes certain types of people who tended to disbelieve in ESP at the time of the early sheep-goat experiments now are inclined to believe in it. Such a factor would tend to affect criterion 2 classification more than criterion 1 classification, because the latter is more

situation specific.

It is the first author's position that this experiment should be construed neither as confirming nor disconfirming the sheep-goat hypothesis on criterion 1. All of the extreme goats in this experiment would be classified logically as goats by criterion 1 as well as by criterion 2, but so would some other subjects who believed that ESP was possible but not with this experimental procedure. Several subjects spontaneously wrote on their record sheets that while they believed strongly in ESP, they did not believe it could be demonstrated in this experiment, either because they did not believe it could be shown in card tests or that it couldn't be shown in the somewhat chaotic circumstances alluded to previously. There may have been a fair number of criterion 1 goats among the criterion 2 sheep who scored poorly in the experiment. Had these subjects been classified according to criterion 1, it is conceivable that a significant sheep-goat effect would have been demonstrated.

It is our general conclusion that the results of this research lend additional support to the proposition that experimenters, experimenter attitudes, and the nature of the experimental situation are important variables in ESP experiments. Careful attention to these factors in the design and execution of such experiments should significantly increase the likelihood of obtaining strong and reliable ESP effects.

ABSTRACT

As part of a larger experiment designed to locate talented subjects for extended ESP training, over 2,000 college students were administered 48 ESP card-guessing trials in a classroom setting. Half of the trials were given in the GESP mode and half in the clairvoyance mode, the order being reversed about halfway through the experiment. The experimenters were students in an experimental psychology class, divided into five teams.

Subjects scored significantly higher on the GESP run than on the clairvoyance run, although this effect was almost entirely attributable to subjects tested by one of the five teams. Subjects also scored significantly higher on the second run than on the first run, regardless of type. The means for both the GESP run and the second run were significantly above chance. The hypothesis that there would be a significant positive relationship between ESP scores and level of belief in ESP was not supported.

Various nonparapsychological interpretations of the findings were considered and rejected. It was concluded that the results lend further support to the notion that experimenter variables play an

important role in ESP experiments.

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PARAPSYCHOLOGICAL DEVELOPMENTS IN THE NETHERLANDS

G. Zorab

Taken in a general sense, the developments that finally led to scientific interest in parapsychological studies in the Netherlands followed more or less the same lines as those to be found in various countries of the western world. In the United States, England, France, Germany and some other countries we can remark the same sequence of events that led via mesmerism and modern spiritualism to the experimental investigation of the phenomena specially belonging to the field of psychical research and parapsychology. In many aspects the phenomena that gave rise to the interest in mesmerism and spiritualism are the same as those on which the whole field of psychical research and parapsychology are based, e.g. phenomena of a paranormal nature.

Also in Holland we may observe the same developments that took place in the above named countries. During the first half of the 19th century public and scientific interest was specially evoked by the ESP phenomena that manifested themselves by the alleged application of what was then named the animal magnetic fluid. The magnetic fluid was believed to induce the state of somnambulism, and it was in this condition that various kinds of ESP became manifest. In 1852 - 1854 American modern spiritualism invaded the European continent, and its teachings and spiritistic techniques (e.g. table-tilting) spread like wild-fire throughout the length and breadth of the old Continent. The somnambules so to say turned overnight into spiritualistic mediums that acted under the influence of the spirits of the dead taking possession of them. The phenomena of ESP that in the foregoing period of mesmerism were considered to be produced by the somnambule's own spirit or soul, urged to activity by the charge of the animal magnetic fluid, were in the spiritualistic period believed to be undeniable proof of the presence and activity of spirits of deceased persons. In short, paranormal phenomena that in the first half of last century were regarded as sound evidence of the existence and the exceptional nature of the animal magnetic fluide, became in the second half uncontestable proof of the survival of the human personality (soul), and the possibility of communication between this world and the next.

Such was the situation in England, the States and various other countries, including Holland, during the last two decades before the turn of the century. There were in the 1870's a few men in the Netherlands - as the most outstanding one A.J. Riko should be named - who in a sense might be termed psychical researchers, since they attempted to investigate psi by applying standards that were not wholly devoid of an insight in experimental scientific methods. Still, when it was believed that genuine "paranormal" phenomena had been ascertained, the only explanatory hypothesis advanced in such cases invariably remained the spiritualistic one.

As one of the first pioneers of psychical research in the Low Countries we would like to mention Frederik van Eeden (1860 - 1932), M.D., poet, author, and social reformer who, like the famous F.W.H. Myers, was a man very much "tourmenté des choses divines". In Holland van Eeden was the first to apply hypnotic suggestive therapy in his medical practice. It was during such treatment that he first came across a case of ESP. It seems that in 1892 van Eeden was already acquainted with the work of the English S.P.R., for he sent a report on his experience to F.W.H. Myers who published it in one of his articles on 'The Subliminal Self' (1895, p.362-363). In 1895 van Eeden is mentioned as a corresponding member of the S.P.R.. The case in question that van Eeden qualifies as one of telepathic influence is the following.

> "On January 11th, 1892", thus van Eeden reported, "I treated miss M. for hysteris aphonia. I sent her to sleep in my own study. As she showed signs of simulation, and seemed to need an energetic treatment, I threatened to throw a glass of cold water in her face, if she did not speak immediately in a clear voice. I was obliged to fulfil the threat with the expected result. She awoke and spoke in a clear loud voice. This was the first time in five years' practice that I took this measure, and I told nobody of it.
>
> On January 13th, I treated miss F. in the same chair in my study. I had treated the same lady several times before in the same place, and she always went into a quiet sleep. But this time she told me after the treatment that her sleep had been very much disturbed. For she had heard me saying in a threatening voice: "I shall awake you by throwing a glass of cold water in your face". As she was very afraid of cold water, she was in constant fear and uneasiness....".

Later on van Eeden added: "According to miss F.'s version, I was (in her hallucination) speaking to somebody else, and on this point she is positive. As for me, I am absolutely sure I did not say anything of the kind, during her sleep, nor did I mention the

fact to anyone."

Myers in an addenda to the above, writes: "Miss F. gave me orally (at Utrecht, Sept. 19, 1895) an interesting account of her somewhat unusual experience under hypnotism. There is no trance, and no break of memory, but a unique rush of 'illusions hypnagogiques', among which the impression above related was specially distinct."

At the turn of the century van Eeden conducted a series of sittings with Mrs. Thompson as the medium. The latter was greatly appreciated by Myers who at the Paris Conference on Psychology in 1900 declared to be wholly convinced of personal survival and communication with the dead. There seems to have been a consensus of opinion that Myers came to his conviction through his experiences with Mrs. Thompson's mediumship. Van Eeden, after his sittings with the above named medium, declared that during a few moments he had heard a few words spoken in Dutch, and also that correct answers were received on questions spoken in the Dutch language. The alleged spirit manifesting itself through Mrs. Thompson purported to be van Eeden's son Paul who committed suicide. There were several who regarded such manifestations as sound evidence of authentic xenoglossy and therefore of spirit communication. For how could the English lady, Mrs. Thompson, know anything about the Dutch language, so it was argued. By a chance coincidence, however, I discovered that Mrs. Thompson had once lived at Amsterdam during six months, and thus, of course, could have picked up a smattering of Dutch.

Toward the end of last century the investigations of the S.P.R. (founded in 1882), the publications of that Society, etc., pointing to good evidence for the existence of that remarkable phenomenon, telepathy, regarded as an inmaterial, spiritual condition, independently in being next to the so well-known states of mass and energy, started to draw the attention of some scholars, students and writers in the Netherlands. There was, on the other hand, an enormous resistance prevalent in scientific and philosophical circles to accept even the possibility of telepathy or other such spiritualistic "nonsense". The above point of view nearly dealt a fatal blow to the career of the later Dr. Leopold, a classical scholar of repute. He devoted his doctor's thesis to Cicero's treatise "On Divination", and advanced the telepathic hypothesis to explain some of the cases Cicero quoted and which we in modern times regard as belonging to the so-called crisis-ESP type. What hardly ever happens, Leopold's thesis was refused on the ground that telepathy was non-existent and could therefore not be advanced as a hypothesis in a scholarly paper. Leopold was forced to choose another subject and study a couple of

years longer!

The above was told me by Dr. K.H.E. de Jong, a friend of Dr. Leopold, and who was also studying classical languages at the University of Leiden. At the time (about 1899) de Jong was preparing a thesis on "The Mysteries of Isis", and he too was advancing a telepathic hypothesis in order to explain some mystic experiences. However, when he was informed about the fate that Leopold's thesis had suffered, he quickly covered up the possible telepathic influence in the case of the mystic in such equivocal phrasing that his professors did not notice it, and de Jong's doctor's thesis was accepted with honours. In later years Dr. de Jong developed into a prominent parapsychologist (of the psychical research type) who in his many books and papers made use of our advancing knowledge concerning psi to approach philosophical and other problems. In 1940 he was a 'privaat docent' (private lecturer) in parapsychology at the University of Leiden.

The student of biology, the later Dr. P.A. Dietz, became so impressed by the findings of the English S.P.R. that in 1900 he placed an announcement in the principal daily papers and spiritualistic periodicals calling on the Dutch educated classes to come together and establish a Society with the special purpose to investigate the supernormal phenomena as psi-phenomena were then termed. There was but little response but Dietz' interest in the paranormal never left him anymore. Dietz changed over from biology to medicine and psychiatry, and in 1932 he was nominated the first 'privaat docent' in Holland to lecture on parapsychology to the students of the Leiden University.

Between 1900 and the beginning of World War I there were in Holland one or two prominent men who in opposition to the general trend of negation with regard to the existence of phenomena we now term 'paranormal' attempted to draw the attention of the Dutch learned circles to the reality of the so remarkable and significant psi phenomena.

Both of them were converted to the authenticity of the paranormal and its importance for our view of life by attending successful sittings with Eusapia Paladino at Rome, and witnessing her PK and materialization phenomena.

The first was the author and playwright Marcellus Emants (1848-1923). He published a report on his experiences with Eusapia in the high standard literary periodical 'De Gids'. The sitting took place on April 16th, 1903. Curiously enough the sitting took place in the house of a friend, a Mr. R., who may have been the later Prof. G. van Rijnberk, the other person we were going to mention who too had sittings with this medium at Rome.

It was in connection with his Roman experiences that Emants in

the press called upon the educated classes to establish a foundation in order to investigate psi on a scientific basis, taking as an example the English S.P.R.. On the whole it may be said that the academic circles in the Netherlands paid little or no attention to Emants arguments in favour of a systematic investigation of paranormal phenomena which were generally regarded as spiritualistic nonsense or fraudulent practices. Holland had to wait another 15 years before finally the first Dutch S.P.R. (Studievereniging voor 'Psychical Research') was established.

The other defender of the possibility of psi, although for many years he did not dare to do so under his own name but presented the case for the defence using a pseudonym, was the professor of physiology, at the Amsterdam University, Dr. G. van Rijnberk. The latter was professor at Amsterdam during nearly 40 years, while being at the same time Editor of the principal Dutch Medical Journal during many years.

Dr. van Rijnberk too was at Rome during Emant's sejourn there. He must have had more than one sitting with Eusapia and described his experiences with that physical medium in a Dutch periodical, under the cover of a pseudonym. At a seance, while the table was moving about, van Rijnberk looked under the table and thereby elicited Eusapia's remark: "Ah, you are looking for 'il doppio'! (i.e. the 'dubbel','astral body', etc.). It was one of the phenomena that Eusapia produced, consisting of the appearance out of the medium's body of a longish,often black protuberance (pseudopoda) that gripped e.g. one of the legs of the seance-table moving it about or levitating it.

Prof. van Rijnberk had a great interest for the occult and the esoteric, and on these subjects he published several books. He was a great francophil, and consistently refused to use, for instance, the term 'parapsychology', etc., but kept to the French terminology, favoured by the great French physiologist Charles Richet. That van Rijnberk had devoted much time to studying the paranormal is shown by his only book on the subject, originally written in French.

THE FIRST LABORATORY FOR THE SCIENTIFIC RESEARCH OF THE PARANORMAL

The first laboratory for the scientific research of the paranormal (the psychic) was established at Amsterdam in 1907.

In August, 1906, the medical student, Floris Jansen, with no spiritualistic background, suggested in the then most prominent Dutch Spiritualistic periodical, 'Het Toekomstig Leven', to investigate the so-called spiritualistic phenomena in a purely scientific matter. For such an approach it was necessary to set up a laboratory where experiments could be conducted under strictly

scientific conditions. Few people, however, were interested enough to contribute the asked for equivalent of $ 1.- per annum in order to cover the expenses for the establishment of such a laboratory and the publication of a quarterly Journal. Finally, however, in March, 1907, when Jansen had already obtained his medical degree, his plans materialized and a laboratory was founded and the publication started of a quarterly journal that was the very first parapsychological periodical in the Netherlands.

One of the principal targets Jansen was aiming at was to gain an insight in the possible relationship existing between the phenomena of living organisms and the energy of the 'lumineferous ether'. In those days most people still believed in the existence of an all pervading ether as the carrier of energetic processes! Jansen started with a series of experiments to test the value of Paul Joire's sthenometer that was believed to indicate the existence of an human emanation. Jansen, however, came to the conclusion that the sthenometer's needle was not set in motion by the hypothetical magnetic fluid, 'Od', or some other mysterious human radiations, but by known physical causes.

Von Reichenbach's hypothetical Odic emanations were also subject of a series of experiments. Jansen wanted to repeat the London S.P.R.'s 1883 experiments with magnets, and also those conducted by the Roachas, Luys, and others. Von Reichenbach believed that he had found evidence that some persons, 'sensitives', 'mediums', etc. could perceive certain light phenomena surrounding the poles of magnets, crystals, etc.. Jansen's experiments took place in the large room (6x4 m.) of his laboratory. The room was completely darkened. On a table stood a wooden standard with on top an electromagnet. Switching the electric current on and off was randomized by a revolving contact dial. Opposite to the table an armchair was placed seating the subject who then faced the magnet.

The results obtained seemed to support the idea that some persons could perceive the switching on of the electric current by 'seeing' light phenomena in the direction of the spot where the magnet was placed. 13 Persons out of the 83 subjects tested were aware of light phenomena every time (or most of the times) the magnet was charged. Whether from our modern viewpoint these 13 subjects should be regarded as ESP subjects becoming aware of the charging of the magnet by subjectivally perceiving light flashes and the like, remains an open question. For one thing, all possible auditive cues were not eliminated, since the switching on of the electric current was accompanied by a loud tic that the experimenter tried to drown by various devices.

Jansen also conducted telepathic experiments (mental suggestion) that gave some very significant results in the case of a number of

subjects.

After a year's work, in 1908, there came an end to Jansen's activities in the field of 'occultism', as parapsychology was then termed in some of the European countries. The journal and the work in the laboratory were discontinued.

THE FOUNDING AND WORK OF THE DUTCH S.P.R.

Directly after World War I had ended certain developments came to pass in the Netherlands that led to the conducting of a highly significant series of quantitative ESP experiments, and also to the founding of the Dutch S.P.R. (Studievereeniging voor Psychical Research) on April 1, 1920.

In his presidential address, Prof. G. Heymans, of the Groninger University, pointed out that the new Society in every manner would follow the footsteps of the English S.P.R. that with its scientific approach in investigating the so-called psychical or occult phenomena had already done much of the necessary spade-work to convince many prominent men that phenomena such as e.g. telepathy could be experimentally tested. He further remarked that since the term 'psychical research' had now become in many countries a well-known expression, and that the educated classes at the present time understood what was its meaning, the Council proposed to use this name as an indication of the Society's field of research. When Heymans announced the founding of the Dutch S.P.R., the terms 'parapsychology' and 'paranormal' were not yet in use in our field, though already coined some twenty years before. In 1924 the Germans would be the first to change the name of their most prominent journal dealing with occultism, i.e. Psychische Studien (Psychical Research) into Zeitschrift für Parapsychologie (Journal of Parapsychology).

The constitution of the Dutch S.P.R. under the presidentship of prof. G. Heymans, a renowned psychologist and philosopher, excited great interest in the Dutch University and other Dutch learned circles. The list of members showed various names of professors of all the five Universities in the Netherlands, while more than half of those that contributed to the Society's finances were in possession of an academic degree. Just as happened at the time the English S.P.R. was founded, prominent spiritualists were encouraged to take part in the Society's researches; a number were nominated to sit in the S.P.R.'s Council and various Committees dealing with the investigation of sensitives and the holding of Censuses.

QUANTITATIVE ESP TESTS WITH THE STUDENT A. van DAM

In the spring of 1919 a young german, Mr. Rubini, who had fought in the first World War visited Holland to give demonstrations of what was then claimed to be a telepathic effect, thought-reading and the like. But in reality the effect was a 'normal' one, known as 'Cumberlandism', 'willing-game' or 'muscle-reading'. I myself was present at one of these exhibitions in the town of Nijmegen, in March 1919.

It seems that the student of physics, A. van Dam, attended a demonstration at the university town of Groningen. When he returned he told his friends that he too was able to perform the same feats he had seen that evening. Soon after this van Dam was also seen to be capable to get good telepathic results without the slightest contact with an agent, i.e. the 'thought-transference' or 'thought-reading' manifested itself without the help of 'muscle-reading'. It suggested that van Dam was a gifted ESP subject.

Prof. Heymans, lecturer on psychology at the Groningen University, heard about van Dam's telepathic performances. This induced him to start a series of ESP tests concerning the transference by mental suggestion of taste-impressions, thought of colors, forms, etc., hereby assisted by Dr. H.J.T.W. Brugmans, psychologist, and A.A. Weinberg, M.D. and psychiatrist. The results of these experiments, though of a positive nature, could not be considered highly impressive. When sometime later it was discovered that van Dam was a person of a pronounced motoric disposition, the three above mentioned gentlemen decided to test van Dam's possible ESP gifts by not only making use of a watertight experimental set-up that would cut out all possible sensory cues, but that also would be one to greatly favour the subject's motoric expressivity.

Here follows a brief summary of the experimental set-up that was combined with a set of apparatus registering the subject's pulse-beats, his breathing rapidity, etc. during the experimental sessions. This was, I believe, the first time that such apparatus was used in parapsychological tests. In an appartment of the Psychological Laboratory at Groningen the subject was seated in a kind of dark cabinet that shut out the subject's view in all directions except directly in front of him where a narrow slit in the thick covering curtains was let open to allow van Dam's hand to pass through and rest on a kind of chess-board that was placed there, which had 48 squares drawn upon it. In the ceiling above the dark cabinet and in the floor of the appartment situated directly above on the next store a square aperture was cut out and covered by thick sheets of glass. The agents of the ESP tests, standing near the aperture could look down directly on to the chess-board, and follow all movements

of the subject's hand hovering above the board's squares. The subject himself, however, was not in the position to see or know above which square his hand was held. The subject also could not see or hear anything that the agents above him were doing or saying.

The target to be pointed out on the board (the blindfolded subject had to tap twice with his finger on the spot he believed the target to be situated) was randomly chosen by the drawing of two cards, one from a set with the letters A to H printed on them, and the other from a deck of six cards showing the numbers 1 to 6. Thus targets could be e.g. A.6., C.3., F.2.,etc..

The agent acted in the following manner. Once the target square had been determined, the agent continually kept an eye on that square, and started to concentrate on mentally directing the subject's hand towards that square. The idea regarding telepathy was that it should be seen as purely a thought-transference that could influence the subject from moment to moment, more or less in the same manner as one can pilot an aircraft to a certain landing-field.

The results obtained were highly significant. In 32 of the 80 calls, that is to say, in 40% of the tests the exact target was indicated by the subject. The odds of obtaining such a result by chance alone can be calculated to form a round figure with as many noughts as to fill one whole line of this page. In view of the above the writers of the report state that in their opinion it is practically certain that the results were brought about by what one could term telepathy (ESP).

The above quoted results were obtained in the course of the so-called distance experiments, i.e. while the agents found themselves in the story above the subject, and all possible sensory cues could be regarded as excluded. The series of tests, however, in which the agent stood in the same room as the subject and only a few feet away from him, gave less spectacular results, although auditive cues, e.g. the agent's breathing, could not be eliminated. These nearby tests resulted in 23 hits during a series of 77 experiments, i.e. 30% of the total number of calls in that series. The authors of the report point out that the lower results of the nearby tests enhances the value of the distance tests, showing that the significant results of the experiments could not have been due to sensory cues guiding the subject's hand to the desired squares.

Prof. Heymans had already been interested in telepathy some time before he was able to test its reality by experiment. His interest in telepathy was due to the fact that he believed its existence would greatly support the truth of his philosophic system, termed 'Psychical Monism'. At the same time his conception regarding

telepathy was a very restricted one, namely that thought-transference was only possible when thinking or remembering actively during the waking state. The experimental set-up of the van Dam experiments was based on the above mentioned conception and led to mental concentrating to guide the subject's hand from one part of the board to another. Many Dutch scholars at the time shared Heymans' opinion with regard to telepathy's restricted sphere of action. When Vout Peters gave his demonstration in object-reading or psychometry, regarded as methods to communicate with the dead, there was a case that Peters named a certain person in connection with a treated object that was not recognized by the man who had presented the object. It was only some time later that the latter remembered that the name mentioned by Peters was quite correct. The reporters of this special demonstration concluded that the case could not have been one of telepathy, since the presentator of the object did not think of the name nor recollected it, so that it was not in his mind. But the case did point to psychometry, i.e. the subject 'read' the name from the 'engrams' imprinted on the object!

When many years later I met the then Professor Brugmans who was one of the experimenters during the van Dam tests, I asked whether he was sure that it was only by exercising mental suggestion that van Dam's hand was steered to the target square. In Brugman's opinion there could be no other explanation. Just as was the case with Heymans, Brugmans too believed that telepathy, thought-transference, was the only paranormal phenomenon to be taken seriously. He did not believe in clairvoyance, etc..

I think that Brugmans' opinion could be put to the test, if we should be in a position to re-analyse the listing of the hits and misses of the van Dam experiments. It seems to me that if Brugmans' idea of a successful gradual steering of the subject's hand to the target square by mental suggestion is well-founded, one should find a rather significant higher number of near misses than a random distribution would allow. I wonder if such an analysis would still be possible.

Some years already after the founding of the S.P.R. bickerings started between the believers of spiritualism and the more scientifically oriented investigators. The former complaining that the latter insisted on ignoring the spiritualistic hypothesis when attempting to explain a number of paranormal phenomena. The situation greatly worsened after Professor Heymans' decease, and the presidential chair occupied by Professor L. Polak, Heymans' disciple and successor at the Groningen University. Polak greatly annoyed a large number of the Society's members by bluntly declaring that telepathy was just nonsense, and that therefore it

was absurd to go on investigating a non-existing thing.

PARAPSYCHOLOGICAL DEVELOPMENTS DURING THE PERIOD 1928 - 1941

About 1928, eight years after the founding of the S.P.R., the Society got into serious difficulties owing to a continuous withdrawing of memberships, so that the previously rather strong financial basis of the S.P.R. was threatened with disaster. General discontent reigned and parapsychological research had come to a complete standstill.

It was in this time that Dr. P.A. Dietz who had been interested in parapsychology during the last 25 years, and Mr. W.H.C. Tenhaeff who had graduated in psychology and would write his doctor's thesis on a parapsychological subject five years later, decided to edit together a parapsychological periodical: Tijdschrift voor Parapsychologie (Journal of Parapsychology), the first issue of which was published in November 1928. Publisher and owner of this periodical was Mr. E. Wegelin. It should be noted that in those days the Tijdschrift voor Parapsychologie had no connection whatsoever with the S.P.R.. This magazine only came into possession of the S.P.R. after the liberation of the Netherlands in the autumn of 1945. One of the reasons why the Tijdschrift voor Parapsychologie was founded was apparently that the S.P.R.'s Proceedings (Mededeelingen) was practically knocked-out by shortness of funds, so that little or no parapsychological information came to hand. An urgent need therefore was felt for a more extensive and profound enlightment of the general public and research workers in the various scientific departments interested in the field of parapsychology.

In 1929 a General Meeting of the S.P.R. was convened in order to discuss the point: liquidation or continuation of the Society? At the instigation of Mr. J.J. Poortman, then still a student of philosophy, a new Council was elected to continue the Society's work in conformity with the desires and views of most members and investigators who were favorably disposed towards the possibilities of psi. As the President was nominated Professor M.C. van Mourik Broekman (theologican) Mr. W.H.C. Tenhaeff, secretary, and further the gentlemen Dietz, Poortman, Fremery, and some others. One of the most prominent Council members was the above often quoted Professor Brugmans. Five years later Brugmans withdrew from the Council, since he no longer could agree with the unscientific approach of the subject of one of the most prominent members of the Council (as I heard him say myself during the Society's General Meeting in 1934. I had just then started on the parapsychological war-path, and had that year joined the Dutch S.P.R.)

One of the first measures taken by the new Council was to stop

the publication of the Society's former periodical, and to supply all members with a subscription of the Tijdschrift. The latter periodical from the very start made a good impression, and there is little doubt that it was responsible for the substantial increase of the Society's membership and the raising of the intellectual quality of those who joined the Society. In a general sense the Editors took the German Zeitschrift für Parapsychologie as their example, and just as was usually the case in the latter magazine, the Tijdschrift too devoted a great deal of space to theoretical and historical reviews, combined with many articles refuting the arguments of die-hard disbelievers of the authenticity of paranormal phenomena. The fact that both Editors from the very start were completely convinced of the reality of psi often led to a lowering of their critical standards in differentiating between what was really authentic and what only seemed so. Nevertheless, the Tijdschrift was of great importance in furthering parapsychological interest in Dutch academic circles, and further in various ways paved the way to the obtaining of a more or less official recognition at the Dutch universities that the field of parapsychological research was a valid one.

The result of the growing interest in parapsychology to be noted in academic circles led in 1932 to the nomination of Dr. Dietz as a private lecturer on the subject at the University of Leiden, followed in 1933 by such a nomination of Dr. W.H.C. Tenhaeff at the University of Utrecht. It may be said that these two nominations clearly showed that parapsychology had raised itself to the height of a respectable University status, and was now taken seriously, at least in the Netherlands. Incidentally, Dr. Dietz may be regarded as the very first academical lecturer on the subject of parapsychology in the world. A 'privaat docent' (private lecturer) is not a professor, but a scholar who without receiving any money regularly lectures to the university students on a certain subject.

The gulf separating the parapsychologically-minded approach and the spiritualistic one had been steadily widening in the 1930's. The newly installed S.P.R. Council was loath to take steps to bridge-over that gulf in view of the highly uncritical attitude of the Dutch spiritualists at the time. The narrowing of the mentioned gulf was also very much hampered by the many exposures of so-called direct-voice mediums round about 1933. There was even a public law-suit against one of these mediums on the charge of swindling. The judge, however, refused to punish the accused for being a fraudulent medium, since it was his opinion that "adults should not be so very foolish to believe that the accused was able to have their friends and relatives appear in spirit form". But he did sent him to prison for a year for having a homosexual relationship with his minor co-medium!

In the early 1930's investigations in the Netherlands in general only centred on possible ESP phenomena, either of a spontaneous nature or manifesting by means of 'object reading', in Holland known as 'psychometry' or 'psychoscopy'. Most of the sensitives were spiritualists (a renowned one was Mrs. Akkringa who discovered her gift after attending a sitting with Mr. Vout Peters),and thus believed that the spirits of the deceased were attracted to the medium when the latter was handling an object that had belonged to that particular spirit or had something to do with its living relations. For that reason it hardly ever happened in those days that the psychic received precognitive impressions, since the interest of the consultants centred on receiving evidence of the presence of their deceased friends and relations.

About 1933 the physicist and mathematician, Dr. J.M.J. Kooy, who many years later became an internationally known expert of rocket techniques and space travel, became acquainted with J.W. Dunne's book: An experiment with time. And as Kooy himself had had a few precognitive dreams that had greatly puzzled him, he started to try out Dunne's hypothesis, and so took the trouble in writing down all his dreams and see in what way such dreams would correspond with future events, either happening in his own private life, or which would come to his knowledge by reading the daily papers, seeing films, etc.. Owing to Kooy's own investigations in this special field and Dunne's descriptions of such dreams generally possessing a symbolic and fragmentary character, the precognitive items being intermixed with the dreamer's memories and past experiences, parapsychologists in the Netherlands coined the term Dunne-effect to denote precognitive dreams the contents of which are only partially congruent with the final reality of the precognized future event. This in contrast to dreams and visions that are fully congrous with future reality. The latter dreams, precognitive impressions, etc. were named precognitive, profetic impressions.

During three years, often enough night after night, Kooy kept up his dream studies, meticulously noting down the contents of his dreams directly he awoke after having one. Dream contents and what was believed to be their future realization Kooy recorded in special note-books. Only during the last three months of his dream studies Kooy sent copies of his dream notes to Dr. Tenhaeff directly the next morning in order to have a reliable witness to testify to the fact that if precognitive data came to light, it could be proved that such data were already made known before the precognized events were realized.

The most interesting and impressive Dunne-effects occurring in Kooy's dreams, however, remained unwitnessed, i.e. their

authenticity depend only on Kooy's declarations, though, if required, the evidence for all the unwitnessed cases, is very much enhanced by the recording in Kooy's note-books which are still in existence and could be consulted.

The probable reality of what the term precognition denotes was for Kooy a revelation. Kooy had made a special study of the theory of Relativity and Kooy therefore came to the insight that maybe precognition could well be fitted into the framework of a four-dimensional space-time reality. This idea Kooy developed in several articles in various journals and periodicals, under which the Journal of Parapsychology should be named. Kooy's conception in this matter is his suggestion that Reality may better be expressed in terms of a four-dimensional space-time continuum. In the latter's framework precognition might conceivably be as natural as obtaining sensory impressions in a three-dimensional world. Those who are acquainted with Dunne's book will recall that he needed a five-dimensional world to make sense of precognition.

I quote here one of Kooy's dreams that, however, cannot be termed a typical Dunne-effect but should be regarded as fully congruous with a future event, and therefore of a 'profetic' nature.

Kooy writes: "In my dream I saw that one of my aunts was lying on the sofa in our house at Bosch en Duin (a village near Utrecht). Her head was lying on a white cushion, her hair was spread out as a halo around her face, while the rest of her body was covered with a blanket." "Now the lady in question who was my mother's sister and a practizing medical doctor had always been very strong and healthy, so that the dream vision seemed to be most improbable. However, two days after I had my dream this aunt visited my parents' house and had once again sprained her knee (this had happened once before). She therefore was forced to stay on in our house. When I arrived home that afternoon, I came across her in the very circumstances I had seen her in my dream, that is to say, lying on the sofa, carefully covered up by a blanket, her head on a white cushion and her bobbed hair in the form of a halo around her face."

This whole period right up to the German invasion may be regarded as one of collecting and reviewing Dutch and foreign cases of ESP and other presumed psi phenomena. The Nazi government had silenced German parapsychology, and we in Holland had only the French leaders in our field to guide our steps. Towards the end of the 1930's there was a kind of invasion coming from across the German border of what were termed 'earth rays' which could only be traced by means of the dowsing rod, and therefore were advertised as being of a paranormal nature. As these earth rays were claimed to cause cancer, rheumatism and other awful illnesses, the public got into mortal fear and paid prices to self-made dowsers to inspect their homes to point out the spots where these dangerous rays surfaced and

threatened the inhabitants. The rays were generally to be found under the beds, but never in the W.C.! After the war the earth rays became truly epidemic and even penetrated into royal palaces.

It was in 1936 that I came in contact with the later well-known Dutch psychic Gerard Croiset. At the time he showed no indications of possessing great ESP gifts. He had joined a spiritualistic Society, and was more interested in stage-hypnosis than in extra sensory perception. It was only after the end of the war that his ESP gifts started to come to the fore, and were investigated in various ways by members of the S.P.R.. Between 1936 and 1940 an enormous correspondence developed between Croiset and myself, and practically every day one of his letters arrived with discussions on auras, dowsing, animal magnetism, trance, spirit communications, and the like. Then already he was trying his hand at 'unorthodox healing' by making passes over the patient. There can be little doubt that his successes in this field were assisted by his budding ESP powers, that often were able to penetrate into intimate situations and urges of the patient, thereby greatly increasing his suggestibility towards Croiset, the healer. Croiset's healing practices formed the backbone of his income, and directly after the war became the cause that parapsychologists became interested in him. It was about 1947 that Croiset's ESP gifts had apparently fully developed, and that he was able to apply at will his paranormal perceptive powers to the various time directions, i.e. past, present, and future. His precognitive so-called empty chair tests that imitated the same kind of experiments that E. Osty started with the French psychic P. Forthuny, commenced about this time. Croiset possesses excellent show talents (his brother is a renowned Dutch actor), and very soon after Holland was liberated toured the country with a kind of Croiset show, demonstrating the moving of paralysed limbs (polio) by making 'passes' over such limbs. Further, there were demonstrations of ESP, etc.. As far as we know Croiset produced never any PK effect.

About 1938 Rhine's quantitative experiments with ESP cards became known in Holland. Although they evoked a good deal of enthusiasm among the principal Dutch parapsychologists, none of them ever took the trouble to repeat them, even on a small scale.

The year 1938 too was a time in which serious bickerings between the two editors of the Tijdschrift nearly brought about the S.P.R.'s downfall. This was finally avoided by certain measures that for the time being assured the further functioning of the Society's Council and that of the Tijdschrift. Not for long, however, for in 1941 the Nazi invaders suppressed both the S.P.R. and the Tijdschrift under the pretext that parapsychological,

spiritualistic and more such Societies were in truth Freemason Lodges in disguise!

DEVELOPMENTS AFTER THE LAST WAR (1945 - 1975)

When towards the end of 1945 things became more settled in plundered and ruined Holland, the S.P.R. was reconstituted and measures taken to again publish the Tijdschrift. In the newly formed Council the ethnologist, Professor Fischer, was nominated as the President, while Zorab was installed as the Honorary Secretary. The greater part of the Society's work then was to come abreast with all the new developments in parapsychology that had occurred during the war in the countries of the free world, such as Rhine's PK experiments, Soal and Carington's ESP tests with their displacement effects, etc.. The Dutch were then thirsting to know what had happened in the free world during the five years of war, and this applied especially to parapsychology that always had something of the miraculous in its phenomena. The lectures on this subject were crowded, people flocked to Croiset's 'shows', newly published books on parapsychology sold like hot cakes, etc.. The interest in investigating psi phenomena was equally intensified. Dr. Tjaden started a series of 100,000 dice throws in order to gather evidence for the existence of PK. Unhappily his tremendous exertions gave no significant results. A census was organized with regard to possible precognitive impressions relating to world war II and the events connected with it. Dozens of well-evidenced cases were reported making it highly probable that in dreams, visions, during consultations of psychics, etc. events and situations were indicated and described pointing to a coming war and things to happen in the greater framework of the world-wide catastrophe that was to come over the world. There were indications connected with certain hostilities, persecution of the Jews, etc.,etc.. Most of these predictions were concerned with individual experiences (that came to pass because a war was waging), but there were also such precognitive impressions, though these were much rarer, regarding totaly unpredictable future happenings of a more general nature, involving large numbers of the population. For instance, the catastrophical bombardment of part of The Hague (March 18, 1943) that burned down a thousand houses and killed hundreds of people, and secondly the food-droppings toward the end of April, 1945. In the former case the psychic described which part of the town would be destroyed, even indicating which houses in which streets would be saved. As to the latter the psychic announced: "I see that food for the starving Dutch will be dropped from the sky, just as happened with the manna for the ancient Jews when wandering

in the desert!" Curiously enough, as I learnt many years later, the code-word for the food-dropping action was 'Operation Manna'.

THE FIRST DUTCH PROFESSORSHIP IN PARAPSYCHOLOGY

In the Netherlands three kinds of professorates are known. The first two kinds are appointed by the Government that also pays the professors' salary, their pension, etc.. The third kind, however, is one in which e.g. a Society appoints a University Lecturer with the approval of the Government, and also finances the expenses of the professorate. The latter kind of professorship is termed in Dutch 'byzonder hoogleraarschap', i.e. a special professorate. As far as I am aware of such 'special' professorates are only found in the Netherlands.

In 1947 the S.P.R. Council believed that the time was then favourable to attempt to obtain such a 'special' professorate in parapsychology, and appoint Dr. Tenhaeff as the first Lecturer on the subject. The Minister of Education at the time, the theologian, Professor van Leeuwen, was rather kindly disposed towards parapsychology, so that it was believed that from that side no objections would be encountered. There was a long and weary procedure to be followed before permission could be obtained to found such a professorate, but when finally success seemed to be in sight Professor van Leeuwen suddenly died, and his successor having different views on the subject refused to proceed with the application. And so the matter was dropped for the time being.

In 1952 we were informed that the situation had so far changed that a new application would probably be favourably received. And so at last (1953) the permission was given to the Society to establish a professorate in parapsychology at the University of Utrecht. As professor to lecture on the subject Dr. Tenhaeff was nominated by the S.P.R.. It is generally believed that Tenhaeff was the very first professor of parapsychology in the world. Taken in a literal sense this idea is not correct.

During the 1930's there existed in The Hague a kind of parapsychological society led by the Germans loving Dr. E. Greven who invited several important German philosophers (among whom was e.g. Hans Driesch) to lecture to his Society. Towards the end of 1940 when the Dutch were already subjugated by the Nazi invaders, Dr. Greven had himself nominated by the Nazi Governor, Seys Inquart, as professor of parapsychology at the University of Leiden. However, soon after this professorship had been installed, the Nazis closed down the Leiden University owing to the out-cry of the professors and students against the dismissal of the famous (jewish) Professor of Law.

After Dr. Tenhaeff had become professor emeritus, it was decided,

after long discussions, to nominate an ordinary (ordinarius) professor of parapsychology at the Utrecht University. The choice fell on Martin Johnson who in October, 1973, was nominated to be the professor of parapsychology at the Utrecht University, thereby stressing the importance of parapsychology as one of the accepted departments of modern science.

THE FIRST INTERNATIONAL CONFERENCE FOR PARAPSYCHOLOGISTS HELD AT UTRECHT (JULY, 1953)

In 1952 the Parapsychology Foundation was founded at New York with the purpose to further parapsychological progress as much as possible by stimulating parapsychological investigation of a purely scientific manner, etc.. President of the newly formed P.F. was Mrs. Eileen Garrett, internationally well-known medium and sensitive. Enabled to do so by its strong financial position the P.F. decided to concentrate its first activities in calling together an International Conference in which prominent parapsychologists of various disciplines could meet and discuss specific psi problems. In the beginning Paris was thought to be the most suitable site for the Conference but soon it became clear that Paris in those days lacked sufficient good hotelaccommodation to house the large group of delegates that would be present at the future Congress. It was then decided to have the Conference at Utrecht, since the University there agreed to allow the Conference to be held in the University's buildings, while at the same time suitable hotel-accommodation could be found in the neighbourhood of the Conference rooms. Another point that weighted the scales in favour of Utrecht was the fact that Tenhaeff's nomination as "special" professor of parapsychology at the Utrecht University had just come off, and thus made it probable that the Conference's subject would be kindly received by the representatives of official science.

The Conference, presided by Dr. Gardner Murphy, was an outstanding success. The greater number of those present were psychologists, medical doctors, psychiatrists and philosophers. The physicists being in the minority. There were a number of old-timers, such as Dingwall, Soal, Salter, Price, Thouless, etc., but there were also quite a bunch of youngsters that in later years would become prominent in our field (Ullman, Mundle, Flew, Roll, etc.). There were very interesting discussions, and valuable papers read. We recall, for instance, the rather heated discussion between Dr. Soal and Mr. Spencer Brown on the value of significant critical ratios. Spencer Brown defending his thesis that a CR of say 3 need not necessary mean that ESP was functioning in a certain experimental series; chance alone often enough could produce such CR's, as has been proved by experiment in a number of cases. He argued that

therefore quantitative experiments producing so-called significant results should not be regarded as evidence pointing to the existence of ESP. Soal, on the other hand, retorted with confronting Brown with the CR of 13 in the case of the Shackleton displacement experiments, and the CR of 27 that the combined Mrs. Stewart ESP tests produced. Brown had some difficult in answering this repartee but went on maintaining his idea that it could have been a lucky chance-coincidence that brought about the highly significant results in question.

It was at this Conference that Dr. Meier, from Zurich, acted as the sponsor of the newly developed Jung conception of synchronicity, or a-causal coincidences. His explanation and defence of the synchronicity principle was met with a great amount of doubt and disbelief, especially by the Freudian minded psychiatrists and psychologists. A long discussion ensued on the concept that Jung and Pauli had developed together, and it soon became clear that the Jungian and Freudian psychoanalysts present in the Conference-hall did not understand each other's terminology (this especially applying to the word archetype!).

The Conference was the first of a five years plan of a series of parapsychological meetings on an international basis, all of them sponsored by the Parapsychology Foundation, the President of which should be regarded as the driving power of all the organizing work behind the extensive meetings held in Europe. Each meeting discussing a special subject and its bearing on parapsychology.

There is little doubt that the Utrecht Conference gave a world-wide impuls to parapsychological research, not in the least by rousing the interest of many representatives of various generally accepted disciplines, and thus inducing them to enter our field and try their hand at investigating the reality of psi.

THE VAN BUSSCHBACH ESP EXPERIMENTS WITH CHILDREN

Why children were chosen to be the subjects of a large-scale quantitative ESP (telepathic) experiment may be considered to be based on the following consideration.

During the 1920's Freudian psychoanalysts had developed the concept that Haeckel's 'fundamental biogenetic law', that is to say that the individual organism in its development is to a great extent an epitome of the form-modifications undergone by the successive ancestors of the species in the course of evolution, should also be applied to the mental development of the human individual, and the concomitant development of that individual's brain. It was therefore believed that a young child, mentally and with regard to its brain-function, stood a good deal nearer to e.g. the anthropoids than the adult. Primitive, semi-cultural

peoples and the like also should be regarded as more backward (some anthropologists at the time believed the latter to be incapable of what we term 'logical thinking'!) in mental capacities than the 'civilized' populations.

And since psi gifts were considered by many psychologists and psychoanalysts interested in parapsychology as an atavistic quality, it was reasonable from this point of view to regard children and also primitive man as better psi subjects than the adults of the 'civilized' peoples whose brain development inhibited the manifestation of psi phenomena.

It was with the above described priciple in mind that the Inspector of schools, Mr. J.G. van Busschbach, took it upon himself to conduct a series of ESP experiments on a quantitative basis and thereby making use of children visiting primary schools. The experimental set-up was well thought-out, and adapted to the comprehension and mental range of the children acting as subjects. The schoolchildren of the fifth and sixth grades (i.e. boys and girls between the age of 10 to 13 years) of various schools at Amsterdam were invited to collaborate in what was announced as a 'guessing' game. The sessions to be held in the same class-rooms in which they attended school. The ESP targets consisted of three series of five separate target cards. These were all objects familiar to children of the age quoted above. One series consisted of five different colours; the second of five simple arithmetic signs, e.g. +, -, 0, etc.; the third to five well-known words, such as: blackboard, window, etc.. The tests were of the GESP type, the agent being the schoolmaster or schoolmistress of the class that was being tested. Mr. van Busschbach never acted as an agent but was present at every class being tested as a whole and kept an eye on the proceedings and saw to it that the rules of the experiment were strictly carried out. The subjects had to note down their guesses on the documents that were headed with their name, date, etc.. The high-scoring subjects received a small present.

All possible measurements were taken to cut out sensory cues influencing the experimental results. The agent was seated behind a large screen absolutely invisible to the subjects in the class-room. The target cards were looked at one by one and chosen according to a randomized list of figures, which van Busschbach had taken from a telephone directory (of the town of Amsterdam). Some objections were raised with regard to the use of a telephone directory for compiling a list of random figures, since figures chosen in this manner cannot be considered of a strictly random nature. Still, this slight beauty flaw was not considered of such importance as to make the experimental results unacceptable as evidence for the reality of ESP. The final results of this first series of tests with schoolchildren in for them familiar

surroundings and with agents with whom they were well acquainted, were quite positive. The experiments were conducted in 21 classes, with in all 673 schoolchildren who noted down 20190 guesses. The number of correct guesses were so much higher than that to be expected by chance alone that the significant CR of 3 could be calculated. This first series of experiments were conducted during the second half of 1951, and published in 1952.

I then had the happy idea to translate van Busschbach's paper on his ESP research with primary school children into English, and send the translation to Dr. Rhine, in Durham. It was from that time that Rhine became interested in the van Busschbach research that ultimately would lead to an invitation bringing van Busschbach to the States to superintend there a similar ESP investigation with American schoolchildren as subjects.

In the beginning of 1954 van Busschbach repeated his ESP-research but this time took as subjects the teenage pupils of various secondary schools. The age of these pupils ranged from 12 to 20 years. The task of agents was taken over by the schoolteachers of these secondary educational institutes. The experimental set-up was for all practical purposes the same as that in use during the first primary school ESP tests. In these tests 669 pupils belonging to 31 classes were involved. Although the experiments were conducted on the very same basis, the results were not significant, since the number of correct hits remained on the level of what chance-coincidences would have produced. These negative results came somewhat as a surprise to van Busschbach as he believed to have discovered an experimental set-up that was bound to produce significant results. Why the elder pupils of secondary schools produced chance-results van Busschbach was at a loss to say. Later on it was suggested that the cause might well have been the relationship existing between the pupils of primary schools and their schoolteacher which is, at least in Holland, a quite different one than that between secondary schoolteachers and their pupils. The primary school relationship resembles more that between parent or beloved relation and the young child, while at the secondary schools there is more of a spirit of rebellion to be found between the teacher and his pupil; the pupils are entering puberty, a condition that stimulates agression towards the superiors, instead of furthering a condition of affection for the latter, which is often combined with feelings of wanting to please such superiors (schoolteachers and agents) by producing high ESP scores. Between teaching in the primary and in the secondary schools there exists also this difference (in Holland) that in the primary classes the pupils are instructed by the same schoolteacher during one whole year; the secondary school pupil, on the other hand, receives instruction from a teacher during only about 45 minutes, after which

another teacher stands before the class to teach another subject than the one before him. It may well be that the primary school situation is better suited to develop strong bonds of affection between pupil and teacher than that to be met with in the secondary schools.

However this may be, the same curious discrepancies between the ESP manifestations of primary and secondary schoolchildren came to light when van Busschbach organized ESP tests with schoolchildren of various schools at Utrecht, a university town in the centre of the Netherlands. These experiments conducted in 1954 comprised pupils of the higher grades of the primary schools as well as older pupils of secondary schools. Again, during these tests the primary schoolchildren scored significantly (CR=2.70), while the pupils of the secondary educative institutions produced only chance results. The same was the case with university students whose guessing did not lead to any significant result. All the tests mentioned were supervised by van Busschbach in person.

In 1956 van Busschbach accepted Dr. Rhine's invitation to come over to the States for a six month stage and test the ESP capacities of American schoolchildren at Duke's. The tests there were conducted with practically an identical experimental set-up as was developed at Amsterdam and Utrecht. To the great surprise of everyone concerned the American and Dutch results appeared to be the same. The pupils of the primary schools came to the significant result which was wholly identical to the one calculated for the Utrecht ESP tests, namely a CR of 2.70! The secondary schools' pupils, on the other hand, remained completely within chance probabilities.

Is it surprising that, in view of the above quoted identical significant ESP results produced in two countries so far apart as the States and Holland, van Busschbach by now became completely convinced that he had found the so long for repeatable psi experiment? That from now on one only needed to test classes of schoolchildren any day, any time to get results that would even convince the most headstrong disbelievers in the paranormal. Van Busschbach himself gave me as his personal opinion that the repeatable psi experiment was now within our reach, and that one had only to follow his way of testing to be sure of constant positive results. That was his remark in 1959. How disappointed he would be on this point a couple of years hence!

The experimental results that apparently pointed to the possibility that younger children gave better ESP results than older ones (fifth and sixth grade classes of the primary schools (pupils to the age of 12) scored on a significant level, while pupils of 13 years and older produced only chance results), gave van Busschbach the idea to concentrate his ESP tests on children

belonging to the first and second grade, i.e. pupils of the age of 6 and 7 years. The experimental set-up that was applied was changed in so far that the noting down of guesses by the young subjects was adapted to their still low skill in writing. The experiments were conducted at various primary schools at Amsterdam and Dordrecht, a town about 15 miles south of Rotterdam, in 1958. I would like to stress the point that these tests were not conducted under the direct supervision of van Busschbach, as were the other experimental series quoted above.

The final results were most remarkable as regards their extremely high significance (CR=5.16). This extremely high CR was wholly dependent on the scoring of the Dordrecht girls. Without the latter's assistance the scoring of the Amsterdam and Dordrecht pupils would barely have been above the chance level. The CR obtained at the Dordrecht primary schools is so unique, and so in contrast to what happened at Duke's in the States when van Busschbach repeated the same experiments about a year later, that I will confess to be somewhat uneasy about the reliability of the Dordrecht test results. There may have been a flaw in the recording of the girls' guesses, or some other mistake, though, of course, if mistakes were made they will no longer be traceable after all these years.

As mentioned above, van Busschbach repeated his ESP tests with first and second grade schoolchildren at Durham in 1959. These tests however were completely devoid of any significant results. Only chance scores were recorded. There was no significant scoring differences between boys and girls, neither could any difference be observed between the first grade and second grade classes. This result was a tremendous blow to the so energetic and competent van Busschbach. All his sanguine hopes of having discovered a reliable method for a repeatable ESP experiment were wrecked. In fact, van Busschbach never recovered from this blow. He was so disappointed that he gave up the ESP testing of schoolchildren and withdrew from all parapsychological activities. However, his great merits, I am happy to say, were honoured by his nomination of being the very first person to win the McDougall price that was founded by the Parapsychology Laboratory, Durham.

The postwar history of the Dutch S.P.R. is not such a happy and prosperous one. Towards the middle of the 1950's it became clear that certain grouos in the Society were rather careless with regard to a strict application of scientific standards in parapsychological research. These groups looked upon parapsychology more as being a religion, the truth of which should be established by every mean at their disposition, either by hook or by crook. There was also a great deal of personal ambition and glorification in this matter. This was one of the reason why Zorab gave up his Secretaryship in

1957. In 1960 this unscientific approach and handling of parapsychological research so much disgusted practically all prominent S.P.R. members that they as a group withdrew from the Society, and founded a new and independent one, issuing their own Journal.

With the financial support of the Parapsychology Foundation two extensive censuses were held in order to gather information with regard to the occurring of paranormal phenomena among the Dutch population. The censuses revealed that the majority of the Dutch people understood the significance of the term paranormal, and also could distinguish between normal and paranormal phenomena. The answers received showed that on the whole people had a good idea of the meaning of such terms as telepathy, clairvoyance, precognition and the like. Finally, it should be mentioned that the second census comprising 10,000 randomly chosen adults above the age of 21 seemed to indicate that 3% of the Dutch population once or more times in their lives experienced an allegedly paranormal phenomenon.

The first census took place in 1957/'58, while the second was held in the early 1960's.

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BOOK REVIEW

PSYCHIC REALM - WHAT CAN YOU BELIEVE
by Naomi A. Hintze and J. Gaither Pratt, Ph.D.
New York; Random House, 1976, Pp 269. $ 8.95

I have seldom read a book which has fascinated me as much as " PSYCHIC REALM - WHAT CAN YOU BELIEVE ". The very idea of the composition of the book and the way it has been brought about is most original and creative. Here we find the fruitful cooperation between two distinguished and skilled people in their respective fields; Mrs. Hintze, the noted and very intelligent mystic writer, and Dr. J. Gaither Pratt, who ranks as perhaps the world's most experienced experimental parapsychologist, and who also has extensive acquaintance with spontaneous PSI - phenomena. In many cases a cooperative effort of this type between a fiction-writer and a scientist would probably have failed, but here we have an exception. The very outline of the book is a modern one and has great educational merits. The basic concept can be said to be based on programmed instruction. Problems are illustrated, questions are asked and answered step by step, and the authors have succeeded in doing this in a much more thrilling and educational way than most books on programmed instruction. This book is a "must" both for laymen and professionals, or one could say, for all who have a genuine interest in PSI - phenomena. All should read it with a feeling of entertainment and reward. It is to be hoped that this successful cooperation between a knowledgeable, sophisticated, fiction-writer, and a very distinguished parapsychologist will continue and result in another joint work.

Martin Johnson

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EUROPEAN JOURNAL OF PARAPSYCHOLOGY

The European Journal of Parapsychology publishes original experimental and theoretical papers on parapsychology. In addition, articles presenting reviews of literature, case studies and papers on related topics are accepted in so far the subject can be considered relevant to the understanding of parapsychological phenomena or to the methodology of empirical research in parapsychology. Translations of papers originally published in a language other than English are also welcome.

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(continued inside back cover)

EUROPEAN JOURNAL OF PARAPSYCHOLOGY

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PUBLICATION POLICY

Twice a year the Parapsychology Laboratory of the University of Utrecht publishes the European Journal of Parapsychology. The object of the European Journal of Parapsychology is to stimulate and enhance the interest within this field, especially in our corner of the world, by communicating research results and issues related to professional parapsychology. Although there will be an emphasis on experimental work, theoretical articles are also welcome. Contributions from all over the world will appear in the journal.

A hallmark of the European Journal of Parapsychology is the attempt to avoid selective reporting, that is, the tendency to bury 'negative' results and only to publish studies that 'turn out'. To avoid turning the journal into a graveyard for all 'unsuccessful' studies, we require that the acceptance or rejection of a manuscript should take place prior to the phase when the experimental data are collected. The quality of the design and methodology and the rationale of the study should be judged as per se more important than the level of significance of the outcome of the study. As a practical rule, we advise a contributor of an article to submit a design of his planned study before the study is actually carried out. The rationale of the study should be stated, as well as all the hypotheses related to it. Furthermore one should try to specify the number of subjects, the number of trials, etc., plus the type of statistical methods one plans to use for one's evaluation.

Priority will be given to the publication of studies which fulfil the above-stated publication policy.

The final manuscript with presentation of results must reach us two months in advance of the official publication date, that is May 1st and November 1st.

A STUDY OF REPEATED RETROACTIVE PSYCHOKINESIS IN RELATION TO DIRECT AND RANDOM PK EFFECTS

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We set out to explore a particular kind of PK experiment, one that promises to bring about enhanced effects as compared with the conventional sort of experiment. This kind of experiment with repeated retroactive PK is defined as consisting of trials which are observed repeatedly, by the same subject or by different subjects, each time with the intention of exerting a PK influence. These trials, generated by a random number generator, are recorded before the series of repeated observations start, hence we are considering the influence on trials generated in the past, from the viewpoint of the subject exerting the influence. This is called retroactive PK or retro-PK. As we are especially interested in the combined effect of repeated observations, we call this repeated retroactive PK (RRPK). The interest in this kind of experiment stems from the various predictions for expected effects that can be made using recent theoretical approaches (Walker, 1974; Schmidt, 1975). In what follows we shall give a brief outline on the state of the art of the theory, as seen from the viewpoint of the experimentalist who wants to derive testable hypotheses.

For a long time theories in parapsychology were not very successful in the predictive sense. Theories that are of the form: 'Telepathy exists because of interaction between persons through construct X; the existence of X is proved by the existence of telepathy' are a case in point. However, Rao (1966) mentions quite a lot of them. Perhaps it is useful to read them for their suggestive value, that is, the surplus of meaning put in X that is not essential for the logic of the theory. Doing this we encounter interesting things such as 'psychical energies', 'resonance of complex systems', 'psi-fields','collective unconscious', 'subliminal self', 'exo-somatic processes', 'guess work', 'synchronicity', 'specious present'. All these things are, when stripped of their surplus meaning that sometimes conveys a glimpse of enlightenment, just X as mentioned

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above, conveying nothing. However, we have to note that these attempts have different values when considering their generality (explaining more than one phenomenon) or their potentiality to be fitted into an existing (normal) theoretical framework. The approaches of Walker and Schmidt described below deserve more attention in my opinion, because there is at least uncertainty as to whether they belong to the same class as the earlier attempts.

OBSERVATIONAL THEORIES

The theory of Walker (he calls it a theory himself, Walker, 1974) ties in with existing notions about quantum mechanics. In this branch of physics, which is experimentally very successful, there exist some controversial philosophical points mainly having to do with the probabilistic versus deterministic nature of the universe. The most prevalent view held by most experimentalists, is that of a probabilistic universe. Several attempts have been made to modify this theory by adding variables to the model that describe the exact outcome of events, otherwise described by probabilities of possible outcomes (and thus attributed to chance). These 'hidden' variables have remained elusive as far as the role they might play in experiments is concerned. Walker has taken advantage of this 'opening' in physics to hypothesize a theory which describes an interaction between the 'will' of an observer and the outcome of an observed event, which is in the quantum-mechanical sense, probabilistic. Hence, this is a theory that potentially ties in with physics. Other theories that possess this feature are e.g. electromagnetic theories. However, when compared with the results from experiments in Faraday cages, or dependency on distance, or dependency on time (e.g. precognition or retro PK), these theories fail. From the characteristics derived for the hidden variables in quantum mechanics Walker derives for parapsychological phenomena independence of distance in space or time. The important constraint on these phenomena is that they occur only in relation between observer and observed quantum mechanical random process. This is most easily understood for PK, but the same formalism can be applied to ESP. In that case the experimenter checking the results observes the success or failure of the subject, constituting a random process in relation to the target sequence. An important feature of Walker's theory are the QM processes in the brain and especially those that would be connected with the 'will'. (As we are presently concerned with PK).

One prediction Walker makes is that the QM processes in the brain limit the number of events per time interval (trial rate) that can be influenced. This limit is predicted to be 10,000 trials per

per second, which is still a feasible number for an empirical test. However, he does not exclude the possibility (especially in 'altered states of consciousness') that it is much higher, of the order of 100,000,000 trials per second. The main points of Walker's theory are exposed in the above summary, particularly insofar as they are essential for the present topic which is the prediction of the results of certain PK experiments.

A second theoretical approach, which is best seen as complementary to Walker's conjecture, is Schmidt's mathematical model of psi (Schmidt, 1975). Schmidt simply assumes the mechanism that Walker tries to construct from the unsolved puzzles of quantum mechanics plus a rather speculative neurophysiological conjecture and its psychological interpretation. The premises of Schmidt's model are: 1) Psi acts when an observation is made by an intent observer. 2) Psi action is independent of space and time. 3) The magnitude of the psi action is describable as a simple parameter, the strength of the psi source, that acts as a multiplier on the ratio of the probabilities of a hit and a miss. This list will be discussed in the next session. The last feature makes it possible to do calculations for simple psi experiments. Schmidt shows that precognition and clairvoyance are equivalent to PK, that is, a psi source of one kind can act as the other when placed in a suitable experimental arrangement. This transformation occurs without loss of information (or psi strength) so that we can consider, when accepting Schmidt's framework, the problem of the relationship of the different parapsychological phenomena as solved once and for all. Schmidt also makes predictions on the effect that repeated observations of the same pre-recorded random events have. This effect, to be called repeated retroactive PK (RRPK), is the main subject of the derivations and the experimental work described in the next sections. We have to note about the observational theories however, that their falsifiableness is doubtful (Houtkooper, 1977), due to the same effect as RRPK when occurring in an uncontrolled way.

DERIVATION OF DIRECT AND RANDOM PK EFFECTS IN A REPEATED RETROACTIVE PK (RRPK) EXPERIMENT, AS COMPARED WITH AN EQUIVALENT MOMENTARY PK (MPK) EXPERIMENT.

The introduction of a few terms is useful at this point.
Direct PK: The PK effect summed over a certain segment of data as deviations in the target direction. (The total effect can be negative: psi-missing).
Random PK: The PK effect as an enhancement of the variance between

parts of a certain segment of data. The parts have to be larger than one trial, otherwise the effect can not be observed. On the other hand if we take our segment of data as one part, the random PK effect is equivalent to the direct PK effect. If we divide the data into two simple segments, we merely consider a simple decline/incline effect. The division into parts that we shall consider here is the smallest observable unit, the visible interval. Another sensible choice would be the run, something we shall include in the explorations. The equivalence of psi-missing would here be the diminishing of the variance by a PK influence that cancels the random fluctuations. We shall not consider this possibility here (See conclusions).

Momentaneous PK: The PK effect on trials that are generated while the subject is applying some effort to them and observing the result.

Retroactive PK: The PK effect on trials that have been generated and recorded some time before the subject tries to influence them and observes a play-back of the recorded trials.

Repeated retroactive PK: The PK effect of repeated play-back of the same trials. The subject is trying to influence these trials each time.

We shall here consider an RRPK experiment consisting of m segments of n pre-recorded trials which are observed r times. We shall compare this situation with an experiment consisting of r groups of m segments of n momentarily generated trials. This is an ordinary or momentary PK experiment (MPK) with the same number of observed trials as the RRPK experiment.

Furthermore we shall consider trials from a binary random number generator, so that a priori probabilities of a hit and a miss are $p=q=\frac{1}{2}$.

In a momentary PK experiment we have to give a formulation of the strength of the PK influence. For this we use Schmidt's strength Θ of the psi source. We write Θ as $1+\varepsilon$, so that ε is small, where the a priori probabilities are related to the actual probabilities in the experiment p' and q' by:

$$p'/q' = \Theta.p/q = (1+\varepsilon)\ p/q \qquad (1)$$

A. Direct PK in momentary PK experiment

Applying this to one segment of the momentary PK experiment we get a deviation from chance expectation of : $\Delta = n(p'-p)$ with a standard deviation $s = \sqrt{npq}$. For $p=q=\frac{1}{2}$ this gives a z-score of the deviation of:

$$z = \frac{\Delta}{s} = \frac{\varepsilon\sqrt{n}}{2+\varepsilon} \qquad (2)$$

For small ε, this gives $z = \frac{1}{2}\varepsilon\sqrt{n}$. We take ε here, for direct PK, as constant during the experiment. More rigorously, the resulting z of a segment, where the psi source has a strength $(1+\varepsilon)$, is an approximate normally distributed stochastic variable with mean $\frac{1}{2}\varepsilon\sqrt{n}$ and s.d. 1. Or:

$$\underline{z} = N(\tfrac{1}{2}\varepsilon\sqrt{n},\ 1) \qquad (3)$$

where $N(\mu,\sigma^2)$ denotes the normal distribution with mean μ and variance σ^2. If we calculate z for r groups of m segments, we get the direct PK effect in the momentary PK experiment:

$$\underline{z}_{D,M} = N(\tfrac{1}{2}\varepsilon\sqrt{rmn},\ 1) \qquad (4)$$

B. Direct PK in the RRPK experiment

If we first consider r=1, a simple retroactive PK experiment, we can reproduce the calculation for the MPK experiment. It is even possible to state that, under the same conditions, Θ should have the same value. At least the present observational theories state no reason for Θ to be different, while Walker's theory explicitly states that the hidden variables are time independent. For repeated observations we look at Schmidt's formulation: $p'/q' = \Theta p/q$ where Θ is a factor slightly larger that 1. This formula is applicable to the first observation. For the second observation we take the a priori probability to be p'/q' instead of p/q, and we get the probability after two observations:

$$p''/q'' = \Theta.p'/q' = \Theta^2.p/q \qquad (5)$$

where we take Θ to be constant.

Schmidt (1975) who proposed this kind of experiment implies this argument. However, the 'addition theorem' from which it is derived is just put forward; and has been more explicitly stated in a recent article (Schmidt, 1977). This assumption would be very reasonable if the result would be enhanced bit by bit with each observation. One has to realize however that the first observation is already on the fully influenced trials, as is the last. Furthermore it has to be emphasized that in observational theory the time order of the subsequent observations has no relevance. Moreover the subject observes not the influence he has, but only the influenced result,

whatever it is. For the present derivation we shall assume additivity of the effects of subsequent observations as a working hypothesis. An experimental test of this will help to clear the possibly somewhat confusing picture.

Thus, we obtain with r repeated observations:

$$p^{\&}/q^{\&} = \theta^{r}p/q \tag{6}$$

If we write θ as $1+\varepsilon$ where ε is small, we can write:

$$p^{\&}/q^{\&} = (1+r\varepsilon)p/q \tag{7}$$

This result is used to calculate the direct PK effect in the RRPK experiment:

$$\underline{z}_{D,R} = N(\tfrac{1}{2}r\varepsilon\sqrt{mn},\ 1) \tag{8}$$

We see that the expected z score is in this case $\sqrt{r}$ larger than in the MPK experiment. We note that, so far, the division of the session in m segments is not essential.

C. Random PK effects in the MPK experiment

The random PK effect occurs when the psi source has a variable strength. We are considering here a pure variable source with a net strength zero, but with a certain variation about zero, expressed as a standard deviation σ. Furthermore we have to specify on what time scale the variations occur. The characteristic time interval has to be both larger than the intertrial interval and smaller than the total duration of the experiment. The characteristic time interval will correspond with what we here call a segment of data.

For such a segment we have derived the direct PK effect as (3) where ε was taken to be constant. We now take ε to be a stochastic variable:

$$\underline{\varepsilon} = N(0,\ \sigma^2) \tag{9}$$

Substituting $\underline{\varepsilon}$ for ε in (3):

$$\underline{z} = N(0,\ 1+\frac{\sigma^2 n}{4}) \tag{10}$$

This result can be interpreted as the variance of z being composed of the random (sampling) variance plus a contribution from the variance in strength of the psi source.

N.B. We assume there is no correlation between the psi influence

and the sampling variance.

This excess variance can be tested for in the following way. For one segment we can square the z score:

$$\underline{z}^2 = \chi^2(1) \cdot (1 + \frac{\sigma^2 n}{4}) \tag{11}$$

Summing the squared z scores over r groupes of m segments results in:

$$\underline{S}_{R,M} = \chi^2(rm) \cdot (1 + \frac{\sigma^2 n}{4}) \tag{12}$$

(Under the null hypothesis $\underline{S}$ is χ^2 distributed with rm degrees of freedom). This expression for the random PK effect can be simplified by the approximation:

$$\chi^2(rm) \simeq N(rm, 2rm) \qquad (rm > 30) \tag{13}$$

We obtain a resultant z score by taking the difference between the expectations of $\underline{S}_{R,M}$ and $\chi^2(rm)$:

$$\Delta = E(\underline{S}_{R,M}) - rm = rm(1 + \frac{\sigma^2 n}{4}) - rm = \frac{rm\sigma^2 n}{4} \tag{14}$$

and dividing by the standard deviation ($\sqrt{2rm}$):

$$z = \sigma^2 n \sqrt{\frac{rm}{32}}$$

so that we write:

$$\underline{z}_{R,M} = N(\sigma^2 n \sqrt{\frac{rm}{32}}, 1) \tag{15}$$

D. Random effects in the RRPK experiment

As in the previous section, we assume a psi source with a random strength given by (9). If we want to calculate the influence on a single segment of data we have to assume additivity of r repeated observations. Here we assume that the different values of during the different repeated observations are independent of each other. The total effect can be given a resultant psi source with strength:

$$\underline{\varepsilon}_r = N(0, r\sigma^2) \tag{16}$$

When we substitute $\underline{\varepsilon}_r$ for ε in the expression (3) for the psi effect in a single segment we obtain:

$$\underline{z} = N(0,\ 1+ \frac{\sigma^2 rn}{4}) \tag{17}$$

For m segments we can take the sum of the squared z scores:

$$\underline{S}_{R,R} = \chi^2(m)\ .\ (1+ \frac{\sigma^2 rn}{4}) \tag{18}$$

The test statistic $\underline{S}_{R,R}$ for the random PK effect in the RRPK experiment is χ^2 distributed within degrees of freedom.

When we approximate $\chi^2(m)$ by N(m, 2m) we obtain the resulting z score by taking the difference between the expectations of $\underline{S}_{R,R}$ and $\chi^2(m)$:

$$\Delta = E(\underline{S}_{R,R}) - m = m(1+ \frac{\sigma^2 rn}{4}) - m = \frac{rm\sigma^2 n}{4} \tag{19}$$

and dividing by the standard deviation ($\sqrt{2m}$):

$$z_{R,R} = \sigma^2 rn\sqrt{\frac{m}{32}}$$

or

$$\underline{z}_{R,R} = N(\sigma^2 rn\ \sqrt{\frac{m}{32}},\ 1) \tag{20}$$

Comparing this result with that of the previous section, we see that the expected effect is $\sqrt{r}$ times larger in the RRPK experiment than in the MPK experiment.

EXPERIMENTAL DESIGN OF THE PILOT EXPERIMENT

The experimental setup consists of an electronic random number generator (RNG) with visual feedback given to the subject by a computer (see Bierman & Houtkooper, 1975).

The pilot experiment described here consists of 32 sessions. Each session consists of the taking of POMS (Profile of Mood States, McNair et al, 1971), followed by the performance on a PK series of 12 runs. The subject is the author himself.

The POMS (preliminary version) consist of 65 items (Dutch words) describing feelings. The subject responds by pressing a key that indicates to what degree the item corresponds with his own feelings at that time. The items can be put in the following 6 mood scales:

a) Tension T: 7 items
b) Agression A: 14 items
c) Fatigue F: 6 items

d) Depression D: 12 items
e) Vigour V: 8 items
f) Confusion C: 10 items

The 6 scales can be combined in a Total Mood Disturbance score (TMD) by summing the 6 scores, weighting Vigour negatively.

Each PK series consists of 12 runs. Each run has 10 visible intervals. The duration of the runs and target directions were as follows.

TABLE 1

Conditions of the pilot experiment

Run	Target	Duration of the run (s)	Run	Target	Duration of the run (s)
1	left	9	7	right	18
2	right	9	8	left	18
3	right	15	9	left	6
4	left	15	10	right	6
5	left	3	11	right	12
6	right	3	12	left	12

The frequency of the RNG throughout the experiment was about 35 Hz. There were two types of PK series. Before the start of the series, a trial from the RNG was taken to decide whether the series contained:

1) Momentaneously generated trials only (MGTs),
2) Momentaneously generated trials mixed with pre-recorded trials (MMTs + PRTs).

In the first case the feedback to the subject shows the result of the trials generated at that time by the RNG. The horizontal deflection is proportional to the cumulative z score of the difference between the numbers of trials on both channels of the RNG.

In the second case the feedback consists of the same number of momentaneously generated trials as above, but now the result of a pre-recorded series is added. This addition is done per interval. As the pre-recorded series was similar to the actual series, the feedback contains twice the number of trials of the first case. As the result is displayed as a z score, it is not possible to distinguish both cases visually.

It should be noted that the same pre-recorded series was used in all sessions with mixed feedback. The number of sessions with mixed feedback is not determined beforehand, but according to the procedure described above should follow a binomial distribution with $n=32$ and $p=\frac{1}{2}$. The 95% confidence interval should therefore be $10 < r < 22$ where r is the number of sessions with mixed feedback.

HYPOTHESES OF THE PILOT EXPERIMENT

The hypotheses tested in this experiment are first divided according to type of effect:

a) Direct PK effect
b) Random PK effect

Secondly we can divide them according to type of trial:

1) Momentaneously Generated Trials
2) Momentaneously Generated, Mixed Trials
3) Pre-Recorded Trials (Mixed with 2)

This defines the six PK hypotheses.

The POMS is used quite exploratively. The most general hypothesis is a correlation between PK effect and Total Mood Disturbance (TMD) score. This we do for all sessions combined, for the direct PK score per session and the random PK score. Here we ignore the difference between the sessions with momentaneous PK only and the sessions with mixed feedback (see next session). Thus we get two hypotheses about the relation between mood state and PK scoring behaviour.

To be more precise, we take for the direct PK effect the overall deviation from chance on the total number of trials N. We calculate the z score, approximating the binomial distribution. We test two-sided. We take for the random PK effect a sum of squared z scores over all intervals (120 per session). This gives a value that is chi-square distributed under the null hypothesis. We test for an extra-chance increase in this value. For the large number of degrees of freedom of the chi-square we can use Wilson and Hilferty's approximation which gives us the deviation from chance (of the variance) as a z score.

EXPECTED EFFECTS IN THE PILOT EXPERIMENT

Theoretical considerations lead to predicted values in this experiment. When we ignore for the moment certain absolute predictions Walker (1976) has made, we can still predict the ratios of some z scores used to test the hypotheses.

The PRTs and MMTs in the sessions with mixed feedback are assumed to be equivalent. This means we assume each trial to be subject to an influence (per session) of the same magnitude, irrespective of

the kind of trial. Thus the ratio between the z scores of the PRTs and the MMTs is predicted to be $\sqrt{r}$, where r is the number of sessions with mixed feedback:

$$z_{PRT} = \sqrt{r} \cdot z_{MMT} \qquad (21)$$

This relationship holds equally well for the direct PK effect derived from (4) and (8) as for the random PK effect (from (15) and (20)), where the z scores are calculated by Wilson and Hilferty's approximation.

The relation between the effects on the MGTs and the MMTs is less easy to predict. The rationale we choose here is the following: The number of trials to be influenced is in the case of mixed feedback twice the number of momentaneously generated trials, as the mixed feedback consists of equal numbers of MMTs and PRTs. We assume this to be equivalent to the situation where we compare series with different RNG frequencies. Where the RNG has a frequency of 35 Hz, we assign to the mixed feedback case a (pseudo) frequency of 70 Hz. A difficult point is the fact that the same PRTs are used r times in different sessions. The additivity theorem however, states that the PRTs undergo an influence that is r times as large as the singly influenced MMTs. Now the influence of RNG frequency is a comparatively well known (!) field. Walker (1974) shows a $\sqrt{f}$ dependency in his theoretical derivation; this is consistent with observations by Schmidt (1973). Assuming this dependency it follows that z scores of otherwise equal tests with different frequencies should be the same. Applying this to our mixed MMTs and PRTs, the influence of a session on MGTs constitutes half the trials in the mixed case and therefore the effect should be $1/\sqrt{2}$ as large. Therefore,

$$z_{MMT} = z_{MGT}/\sqrt{2} \qquad (22)$$

Again, this holds for the direct PK effect as well as for the random PK effect.

RESULTS OF THE PILOT EXPERIMENT

The results of testing the eight hypotheses in this experiment are:

H_1: Direct PK in the MGTs: dev=+144 (N=65632): z=+1.12 (n.s.)

H_2: Direct PK in the MMTs: dev=+71.5 (N=74427): z=+0.52 (n.s.)

H_3: Direct PK in the PRTs: dev=+76 (N=4380): z=2.28 ($p<.05$)

H_4: Random PK in the MGTs: X=1816.75 (df=1800): z=0.29 (n.s.)

H_5: Random PK in the MMTs: X=2022.41 (df=2040): z=-0.27 (n.s.)

H_6: Random PK in the PRTs: X=142.27 (df=120): z=+1.40 (n.s.)

H_7: Correlation between direct PK and TMD: r=0.008 (n.s.)

H_8: Correlation between random PK and TMD: r=-0.010 (n.s.)

In the previous section we derived that the highest effects would be expected in the PRTs, that is, for H_3 (as compared with H_1 and H_2) and H_6 (as compared with H_4 and H_5). The results are in accordance with this expectation. The direct PK effect is significantly present in the PRTs. From the magnitude of the z score (H_3) we derive (from (21)) for H_2: $\sqrt{17}$ times smaller or z=0.56; and for H_1: $\sqrt{15/2}$ times smaller or z=0.84 (from (21) and (22)). These nonsignificant derived values are reproduced to a remarkable degree of accuracy in the experiment.

EXPLORATIONS ON THE PILOT EXPERIMENT

The effect for different runlengths is shown in table 2. In this

TABLE 2

The results of the pilot experiment split for runlegth and type of trial

Runlength (s)	PRTs Deviation	z	MMTs Deviation	z	MGTs Deviation	z	Combined z
9	+10	+0.76	+55	+1.06	+19.5	+0.39	+1.06
15	+7	+0.40	-18.5	-0.27	+1.5	+0.03	+0.32
3	-2	-0.21	-4.5	-0.13	+23.5	+0.82	+0.06
18	+42	+2.35	+2.5	+0.03	+40.5	+0.58	+2.35
6	+20	+1.91	+48.5	+1.14	-3	-0.06	+1.98
12	-1	-0.04	-11.5	-0.18	+61	+1.08	+0.29
Total	+76	+2.28	+71.5	+0.52	+144	+1.12	+2.58

table the need arises to combine rows of data in a sensible way. Of

course, the number of trials and the deviations can be simply added, but in this way the PRTs, which theoretically should have a better performance than the rest, drown in the mass of momentaneously generated trials. As we have calculated the ratio of the predicted z scores as 1 :√1/17 :√2/15. the optimal combined test-statistic is:

$$z_c = (z_{PRT} + z_{MMT} \cdot \sqrt{1/17} + z_{MGT} \cdot \sqrt{2/15})/\sqrt{1+1/17+2/15} \quad (23)$$

which is again standard normal distributed when the same and stochastic independence holds for the small z's, which conditions are fulfilled under the null hypothesis. The resulting values of z_c are shown in the last column of table 2.

An alternative choice, instead of the interval, for the unit of observation in the calculation of the random PK effect, is the run. The results for both choices are shown in table 3; together with the combined z.

TABLE 3

The random PK effect in the pilot experiment

Type of trial	PRT		MMT		MGT		Combined
Unit	X	z	X	z	X	z	z
Interval	142.27	+1.40	2022.41	-0.27	1816.75	+0.29	+1.32
Run	19.31	+1.40	197.48	-0.29	216.11	+1.82	+1.83

The last analysis to be mentioned here involves a closer look at the correlation between the TMD and the direct PK scores per run. In table 2 different effects for different runlengths are apparent, so a different relationship between TMD and different runs may not be too far-fetched. The direct PK effect in runs nos. 1, 4, 6 and 8 shows correlations with the TMD with suggestive p-values (<.10, two-tailed). Of these, run 1 and run 8 are in the 'natural' direction, that is positive PK scores are concomitant with low Total Mood Disturbance Scores. In run 4 and run 6, on the contrary, positive PK is associated with high TMD. The 12 correlation coefficients (for the 12 runs) have a combined probability which is smaller than .05. This finding may well be spurious, but the likelihood of finding relationships between PK scoring and TMD can be said to be

at least undiminished by this experiment. The same analysis as for the TMD was repeated for the six subscales of the POMS. Of these, A, D and C promise most, while V shows the least indication of being related to PK, as judged by the combined probability of the correlation coefficients.

THE CONFIRMATION EXPERIMENT

This experiment was designed to confirm the findings of the pilot experiment that PRTs show larger effects in terms of z scores than the momentaneously generated trials. Modifications were made such that:

1) An effect of mood would appear in the PRTs, so that the detection efficiency of such an effect would be enhanced.
2) The POMS would be changed, so that the validity would be better compared with the preliminary version.
3) The assumptions that underlie the theoretical derivation of the ratio of the z scores of pre-recorded trials and momentaneously generated trials could be put to a test.

The confirmation experiment consists of 18 sessions with a single subject, Mr. H.A.. Each session consists of the taking of the POMS followed by a PK series of 12 runs.

The POMS here is the second Dutch version, consisting of 35 items, attributable to 7 scales:

1) Tension (T): 9 items
2) Agression (A): 12 items
3) Fatigue (F): 7 items
4) Depression (D): 15 items
5) Vigour (V): 8 items
6) Confusion (C): 7 items

The remaining 7 items belong to a Friendliness scale which is thought to measure a personality trait rather than a dimension of the mood state of the subject. The six mood scales are combined in the Total Mood Disturbance scores as defined previously. The parameters of the PK series are changed to longer durations, since the largest effect in the pilot experiment was observed for a runlength of 18 seconds.

In this experiment the PK series are of the mixed type, that is, they consist of momentaneously generated trials mixed with pre-recorded trials (MMTs with PRTs).

There are two series of PRTs of which one is chosen for each session. This choice is made according to the change in the TMD score: PRT series no.1 is chosen when the TMD is lower than in the previous session, otherwise PRT series no.2 is chosen.

The hypotheses about this experiment involve:

1) The direct PK effect in the two PRT series combined, to be tested

TABLE 4

Conditions of the confirmation experiment

Run	Target	Duration Run (s)	Run	Target	Duration Run (s)
1	right	9	7	left	27
2	left	9	8	right	27
3	left	18	9	right	6
4	right	18	10	left	6
5	right	4	11	left	12
6	left	4	12	right	12

two-tailed.
2) The random PK effect in the two PRT series combined; the enhancement of the variance to be tested one-tailed.

Departures from the theoretically predicted ratios between z scores in the PRTs and in the MMTs are tested using (21). The hypotheses for testing this prediction are:
3) On the direct PK effects in the PRTs and the MMTs.
4) On the random PK effects in the PRTs and the MMTs.

RESULTS OF THE CONFIRMATION EXPERIMENT

In this experiment both PRT series have undergone 9 observations. The results for the first two hypotheses in this experiment are:
H_1: The direct PK effect in the PRTs: Deviation = -28.5 (n=10529); z = -0.55; p = 0.582, two-tailed.
H_2: The random PK effect in the PRTs: X = 179.42 (df = 240); z = -3.01; p = 0.999, one-tailed.

Both hypotheses have to be rejected in favour of the null hypothesis.

The testing of the third and fourth hypotheses entails the definition of a test-statistic, which becomes zero when equation (21) is fulfilled. This test-statistic is chosen such that it is standard-normal distributed under the null hypothesis:

$$z_d = (z_{PRT} - z_{MMT} \cdot \sqrt{r}) / \sqrt{1+r} \qquad (24)$$

where r, the number of repeated observations, is 9 in this experiment.

The results are:

H_3: The departure from the prediction on the direct PK effects in PRTs and MMTs: $z_d = -0.66$; $p = .510$, two-tailed.
H_4: The departure from the prediction on the random PK effects in PRTs and MMTs: $z_d = -2.34$; $p = .020$, two-tailed.

The null hypothesis is here the theoretical prediction (21), which has to be rejected in the case of H_4, when we test at the .05 level of significance.

EXPLORATIONS ON THE CONFIRMATION EXPERIMENT

A summary of the results is given in table 5.

TABLE 5

The results of the confirmation experiment split for runlength, type of trial and relative mood state of the subject

Mood State	Lower TMD				Higher TMD			
Runlength	PRTs (1)		MMTs (1)		PRTs (2)		MMTs (2)	
(s)	Dev.	z	Dev.	z	Dev.	z	Dev.	z
9	-6	-0.44	+34.5	+0.91	-24	-1.88	+38.5	+1.01
18	-2.5	-0.11	+18	+0.33	+11	+0.59	+77	+1.44
4	+16	+1.87	+5.5	+0.20	-3	-0.30	-7	-0.26
27	-1	-0.02	+48.5	+0.74	-14.5	-0.65	-42	-0.64
6	+7.5	+0.69	-3.5	-0.10	-9	-0.83	-23	-0.73
12	+6	+0.38	-41	-0.94	-9	-0.59	-25	-0.56
Total	+20	+0.54	+62	+0.56	-48.5	-1.32	+18.5	+0.16

The effect of mood state can be expressed as a z score of the difference between the direct PK effects in the PRT1 and PRT2 series. This result in $z_d = 1.32$, the difference being in the natural direction (lower TMD associated with positive PK). However, when this difference is evaluated as a two sample t-test on the z scores of the intervals, we obtain $t = 2.11$ ($df = 238$) which would be significant at the .05 level (two-tailed). The combined z scores of PRTs and MMTs can be calculated by $z_c = (z_{PRT} + z_{MMT} \cdot \sqrt{1/9}) / \sqrt{1+1/9}$, analogous to (23).

TABLE 6

The combined z for the direct PK effect, split relative mood state of the subject

Mood State	PRTs Dev.	PRTs z	MMTs Dev.	MMTs z	Combined z
Lower TMD	+20	+0.54	+62	+0.56	+0.69
Higher TMD	-48.5	-1.32	+18.5	+0.16	-1.20
All	-28.5	-0.55	+80.5	+0.52	-0.35

TABLE 7

The combined z for the random PK effect with the interval as unit of observation

Mood State	PRTs X	PRTs z	MMTs X	MMTs z	Combined z
Lower TMD	86.53	-2.36	1152.40	+1.54	-1.75
Higher TMD	92.89	-1.86	1104.29	+0.53	-1.59
All	179.42	-3.01	2256.68	+1.46	-2.39

TABLE 8

The combined z for the random PK effect with the run as unit of observation

Mood State	PRTs X	PRTs z	MMTs X	MMTs z	Combined z
Lower TMD	7.58	-0.91	110.58	+0.22	-0.79
Higher TMD	7.94	-0.81	128.79	+1.38	-0.33
All	15.52	-1.31	239.37	+1.12	-0.89

Some of the combined z values are given in tables 6,7, and 8.

The random PK effects (tables 7 and 8) can be compared with the corresponding data on the pilot experiment in table 3. The direct PK effect (table 6) can be compared with table 2. The curiously negative random PK effect in the PRTs of the confirmation experiment (hypothesis H_2) is not concentrated in one of the relative mood state conditions.

CONCLUSIONS

The data obtained from both experiments discussed are not always consistent with each other. In the pilot experiment in which the author was his own subject, the following results were obtained. We found an indication that retro PK occurs as a direct PK effect (hypothesis H_3, $p < .05$). This in itself can as yet only be accounted for by an observational theory. The data are furthermore consistent with the predictions by the theory on relative strength of RRPK and momentaneous PK. The combined evidence for PK in the pilot is calculated as a combined z value of 2.58 with a two-tailed $p < .01$. The hypothesized random PK effect, which was theoretically analysed too, was not present to a significant degree. Neither was a clear relationship established between mood state of the subject and PK (on the momentaneously generated trials), although an indication of a more complicated relationship was found by analysing the correlation between the mood scores and the PK scores per run.

The confirmation experiment was also performed by a single subject, an acquaintance of the author, and not entirely naive as to the purpose of the experiment. The number of hypotheses was kept to a minimum in the light of the outcome of the pilot. As the strongest effect was expected in the PRTs the direct and random PK effects were tested in these. The direct effect was nonsignificantly negative. The test on the random effect produced an unexpected outcome: Instead of an increase in variance, a decrease was observed! The magnitude of this decrease ($z = -3.01$, $p = .003$, two-tailed) calls for a discussion as to the possible cause of this effect, should it not be an improbable chance occurrence.

The random PK effect (hypothesis H_2) is tested one-tailed in the direction of an increase in variance, because a decrease in variance is not meaningful within the model on which the derivation of equation (20) is based. However, this model hinges on the assumption implicit in equation (10) that the fluctuating PK is independent of the random variations in the results of the intervals due to sampling a finite number of trials per interval. This assumption of independence may be too simple. Instead one might hypothesize

a model with a PK influence correlated with the sampling variation, thus accounting for a decrease in the variance in the case of a negative correlation. It does not seem appropriate however, still to call the correlated PK influence a random PK influence.

This surprising finding has its implications for hypothesis H_4 in the confirmation experiment. The result is that the comparison of the random effects in the PRTs and in the MMTs is not in accordance with the theoretical prediction. The decrease in variance in the PRTs is contrasted by an increase in the MMTs (see table 7). The conclusion to this finding must be that it is doubtful whether the assumption of equivalence of PRTs and MMTs can be upheld. A revision of the model however, entails such difficulties that it is first necessary to await more experimental data.

A conclusion about the use of the POMS is that results are encouraging, both from the pilot experiment and from the confirmation experiment. In the latter a suggestive difference was obtained between the two sets of pre-recorded trials, later observed under conditions of different mood states. If this finding is confirmed, it will form independent evidence that retro PK truly is exerted after the trials are generated.

ABSTRACT

Theoretical developments by Schmidt and Walker lead to the prediction of an enhanced efficiency in tests of repeated retroactive PK compared with momentaneous PK. Relative effects on different trials in an electronic RNG setup are predicted.

The possibilities of a psi source that has variable strength are explored, both as a random PK effect brought about by rapid fluctuations of the source and as a dependency on the mood state of the subject.

In the pilot experiment the direct PK effect is significantly ($p<.05$) present in the pre-recorded trials. No significant random effects are detected, while only an indication of a possible relation with mood state is obtained.

In the confirmation experiment, the random effect obtained on the pre-recorded trials is the opposite of the expectation, i.e. a decrease in variance instead of an increase ($z=-3.01$, $p=.999$). Exploratively, a relationship between mood state and scoring on pre-recorded trials is indicated ($t=2.11$, $p<.05$).

While the theoretical model is not yet proven, it is encouraging that effects are, in this case at least, more pronounced in the pre-recorded trials.

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TESTING SOME IMPLICATIONS OF A PK OBSERVATIONAL THEORY

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In recent years a new theoretical approach towards explaining psi phenomena has attracted much attention and interest (Walker, 1974; Schmidt, 1975, Donald & Martin, 1976). Walker and Donald & Martin base their approach on quantum mechanical notions, while Schmidt offers a mathematical model of 'a world with psi' which is also strongly influenced by quantum theory. Because these three models have so much in common both as regards their basic assumptions as well as the hypotheses which follows from them, I will treat them in the following discussion as being different 'versions' of the same model. Since in these models the observer of the phenomena plays an essential role, this theoretical approach may be characterized by the term 'observational theory'. This approach is mainly based on new insights in physics and contains some aspects which distinguish it favourably from former theoretical approaches in parapsychology.

Classical physics can be characterized as being 'causal' and 'deterministic'. It is supposed that there ideally exists a complete system of laws with which the physical reality can be described, and given the knowledge of this system all future situations can be derived and predicted from the present. Once given a complete description of the initial state of a system and of the variables exerting an influence, the subsequent state of the system is determined and can be predicted accurately. The processes involved are causal and deterministic, that is, there is only one specific effect which can be the result of a specific causal process. In this world-view randomness is unavoidably always a 'quasi' randomness, in the sense that the randomness of a system can only be created artificially by constructing such a complex system that it becomes practically, but not theoretically, impossible to acquire a complete description of the initial system - for instance the system which produces electronic noise in a random number generator (RNG) on which ultimately the outcome of the RNG is based.

However, experimental findings in quantum physics have challenged

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this view of a one-level deterministic physical reality. Schmidt (1976) presents an interesting example of a hypothetical experiment with a RNG which illustrates nicely why physicists are forced to reconsider this view of physical reality. In quantum mechanics a given initial state can develop into any number of subsequent states. Hence there exist different potential 'physical realities' concurrently, and only the observer who observes the subsequent state can determine into which state the system has developed. Therefore, as Schmidt states (ibid, p.272): "'Reality' is only what the experimenter observes". This view implies the loss of causal 'laws', which have to be replaced by statistical predictions specifying the probabilities for each of the potential subsequent states, and the existence of a 'true' random process, a random process in which the outcome remains unpredictable even given a complete knowledge of the initial state of the system. In fact, the existence of such 'true' random processes might be considered as the reason why psi phenomena are possible at all.

The theoretical approach in parapsychology based on the above described world-view has many advantages over previous theories. Psi phenomena lose their exclusiveness and become to a certain extent compatible with modern physics, at least to a greater extent than when they are fitted into a deterministic world-view. In a deterministic physical reality precognition seems a 'contradictio in terminus' and, in the case of ESP, we must assume some causal process between the object and the human agent. As far as I know there exists hardly any experimental evidence suggestive of the existence of such a process. Since it looks as if there is no known physical process which can be considered as the medium between the object and the agent, we are forced to introduce a medium like the 'psi-field', which in analogy to the famous ether possesses only one property, i.e. it is the carrier of the psi process. Such a concept remains rather artificial and, without further assumptions, impossible to falsify.

A further advantage of this theoretical approach is, as Schmidt (1975) has demonstrated, that it allows a mathematical description of psi phenomena in terms of probabilities. The vital role of mathematics in most branches of science is undeniable. However, in view of the application of mathematical descriptions in social sciences it remains to be seen how valuable the contribution of this possibility will show to be for the understanding of paranormal phenomena.

At present the greatest strength of a theoretical approach based on quantum mechanical concepts seems to me to lie in its practical consequences for experimental research. This theory generates both some new and original ideas about psi phenomena as well as a

relatively large amount of testable hypotheses. Psi phenomena can be considered possible because randomness can be considered as a basic feature of nature. Moreover, the observer of the phenomena or the experimental results play an essential role in the whole process. The outcome of a test might depend more on the test conditions at the moment of observation of the results, than on the conditions at the time the targets were generated. And to mention a few testable hypotheses: The subject's scoring rate should not depend on the complexity of the 'true' RNG which is used to generate the targets (Schmidt, 1974). Feedback is essential; at some time in the future the agent must gain some knowledge of the outcome. The scoring rate must differ depending on whether the targets in a PK experiment are generated via a 'true' random process or via a 'quasi' random process (for instance when using an algorithm) (Donald & Martin, 1976).

Although this theoretical approach has many promising features, it leaves many questions unanswered and it also raises some new problems. One weakness it shares with previous theories is that it does not at present give any indication about how an organism can apply psi ability and which mechanisms are involved. One problem it raises is the so-called 'Divergence Problem'. As Schmidt (1975) states: "The space-time independent formulation of the psi axiom leads to a typical difficulty: the outcome of a PK test depends not only on the overt PK subject, but also on all the observers who look, no matter how much later, at the results, provided these observers can exert some PK effect." Although the divergence problem is a consequence of Schmidt's mathematical model, it is according to Houtkooper (1977) inherent to all observational theories. Houtkooper (ibid) illustrates mathematically that the divergence problem leads to absurdities, but even without applying mathematics it is clear that accepting the probability of different physical realities depending on future observers would in principle result in a daily rewriting of history.

I believe that the divergence problem is really an unnecessary burden to observational theories, and is rooted in a wrong reasoning, i.e. that the rejection of the world-view of a one-level deterministic physical reality would imply that also the existence of deterministic processes has to be rejected. It is quite possible to accept the concept of different physical realities as a fundamental property of nature but, especially at a macroscopic level, once nature has 'decided' upon a specific physical reality, this reality remains determined independent of time or future observers. For instance, suppose I choose a number randomly, let's say 57. The choice of this number might be considered as basically a quantum-mechanical process (although psychological variables will

exert an influence too), but from the moment I write down this number and observe it, a deterministic process commences. Excluding perceptual errors all future observers will read the number 57, and not any other number. When an initial state develops into a subsequent state, it must develop into one observed specific state, and that state must be the same for all observers.

Another objection might be raised about the relevance of the divergence problem. In Schmidt's example (1976, p.269) he shows, that given two different experimental arrangements, the observed physical reality depends on the type of arrangement applied, rather than on a specific observer. In one arrangement an electron behaves as if it is either in one place or in another, while in the other arrangement the electron behaves as if it travels as a wave through both places simultaneously. This means that with each arrangement a certain physical reality is associated. In itself these experiments are repeatable, or to a certain extent deterministic. Given the same arrangement all observers will observe the same physical reality. And since in each individual psi experiment, one session with a subject, we are dealing with one specific arrangement, it is similarly to be expected that all later observers will observe the same outcome.

Because of the above mentioned considerations the theory is, in my opinion, applicable as far as systems are considered which include the first observer(s), but once the observations have been made the outcome is determined and no further observers can influence this observed outcome.

One objection against this argumentation might be that granted the possibility of retro-PK, the influence of the future observers is exerted before the first observation is made. That would imply that we have to consider each experiment as an unlimited 'open' system. However, accepting a space-time independent formulation does not necessarily have to lead to the rejection of possible isolated systems. One could argue that once the observations are made the system develops into a definite state and is 'closed' from then on. That does not contradict the time independency of psi. The latter implies that, suppose I generate the targets in a PK experiment today, it should not make any difference as regards the significance level of the outcome whether I observe the results tomorrow or after a couple of years. But once I have made the observation the experiment can be considered as complete.

If one argues that any system has in principle to remain 'open' infinitely, in the sense that all future observers are able to act as 'hidden variables' influencing the development of the initial state into the observed resultant state, then any experiment must be considered as possibly being influenced by an unknown but in

principle infinite number of future observers with all unknown goals. Moreover, these goals will partly be influenced by the observed results which are themselves influenced by the goals. In my opinion this would reduce parapsychology to an equation with an infinite number of unknowns. In other words, we cannot interpret any experimental result anymore, and I suspect that the same would apply to the experimental results in physics which delivered the basis for this model.

To sum up, this theoretical approach based on quantum mechanical notions seems to me to be an interesting an fruitful one, but somewhat too far-fetched when it also states the assumption that all systems remain infinitely 'open' so that all future observers are able to influence the outcome of past experiments, or that all different future observers are able to experience a different physical reality when observing the outcome of an experiment. These assumptions seem to me unnecessary and I wonder whether they really are unavoidable conclusions when interpreting present quantum theory.

HYPOTHESES

The aim of the present study is to test some hypotheses which follow from the theoretical approach already discussed, but within the framework of the above-stated considerations, i.e. that once the observations have been made influence by future observers on the outcome is excluded.

In an observational theory the act of observing is considered an essential element. Hence, it is not surprising that when such a model is applied to psi phenomena, the act of observing or feedback is given an equally prominent role. Both Schmidt and Donald & Martin stress that without feedback psi is impossible. Schmidt (1975, p.314): "The outcome of a random process can be affected by the (psi) source only if this outcome has, at some later time, a physical effect on the source. Thus a PK subject could not score significantly if he never got any feedback on the outcome of his trials". And Donald & Martin (1976, p.32): "it is absolutely essential that the agent, at some time in the future, gain some knowledge of the outcome. This knowledge does not need to be complete; for example, it might be only that an interesting result was or was not achieved".

If the last statement is true then it is impossible to falsify this hypothesis directly, since observing the outcome of a condition in which subjects did not receive feedback creates a new experiment in which the observer becomes the subject. On the other hand, in

such a case at least a differential scoring should be observed between the scoring of the different subjects in the feedback condition compared to the scoring in the non-feedback condition, which actually is the scoring of the observer. If each subject exerts a certain psi influence and feedback is not important, then we should expect some correlation between the scores of the subjects in feedback and non-feedback conditions. If feedback is essential, such a correlation could only be obtained when the first observer of the results of the non-feedback condition is determined to prove that feedback is not important, by 'matching' his scoring (non-feedback condition to the subjects) with the scoring of the subjects in the feedback condition. To eliminate this explanation in the case of a significant correlation we should make use of an observer who has no idea about the role of feedback in PK experiments and who is not goal-oriented towards obtaining a particular result. Hence, if we observe in this design a significant correlation between the scoring in feedback and non-feedback conditions, we can conclude that the hypothesis - without feedback no psi - is falsified, provided that we accept the assumption that an observer who is not informed about the experiment and cannot be considered goal-oriented should score differently from goal-oriented subjects. To test this hypothesis a non-feedback condition will be included in the experiment.

Donald & Martin predict that "in a direct comparison, higher significance levels will be achieved using truly random events than using pseudo-random events" (1976, p.28). This hypothesis will also be tested by including a condition in which the targets are not generated by a RNG, but rather by applying a quasi-random process based on an algorithm.

Another hypothesis is provided by Schmidt who states: "A quite specific and easily testable feature of this model is, furthermore, that the repeated play-back of a pre-recorded target sequence to the subject should lead to an increase of the PK scoring rate." (1976, p.269). In other words, repeated feedback will enhance scoring. In the present experiment this condition is introduced by again presenting in the non-feedback condition the feedback of previous trials of the 'repeated' feedback condition.

The last hypothesis to be investigated is not directly derived from the observational model but concerns the influence of a psychological variable. Subjects are presented quasi-feedback indicating positive scoring which is not related to the true scoring in the trial. Since no 'true' feedback is presented, the model predicts - assuming a neutral observer - no extra-chance scoring in this condition.

PRESENTATION OF THE EXPERIMENT

The experiment is automated and run by a PDP-11 minicomputer, to which a RNG is coupled. The subject is seated in front of a computer-display in a room one floor above the floor on which the computer and the RNG are located. After receiving instructions and the subject's name having been typed in the experiment starts. In the instructions presented the concept of PK is explained and some information given about the observational theory, however, without mentioning any of the hypotheses which are investigated in the present experiment. According to the instructions the purpose of the experiment is to select high-scoring subjects. The subject's task is to influence the generation of random numbers 1 or 2.

Feedback is displayed on the screen after each trial in the following manner. The screen is divided into two halves by the symbol 'M' in the middle of the screen. The deviation of the trial (the excess of '2' over '1') is presented as a standard score (CR value) either to the right - when the deviation is positive - or to the left - when the deviation is negative - by an 'X'. The size of the observed deviation of the trial is indicated by the distance 'M' - 'X', and its sign by the direction of 'X' left or right from 'M'. Hence the subject's task is to influence the system in such a way, that all 'X' es show up on the right half of the screen, as far from 'M' as possible. After a subject has completed the experiment the data - total scores per condition - are stored in the computer and not inspected for one month after the completion of the experiment.

CONDITIONS

In condition 1 the random numbers generated are based on a quasi-random procedure provided by the computer. Given a starting point the sequence of the subsequent numbers is 'determined'. In all other conditions the random number samples are taken from the RNG. Condition 2 is to be considered as the standard PK condition. Condition 3 is the non-feedback condition. After each trial in this condition feedback is presented from the preceding trial of condition 4. Condition 4 is the repeated feedback condition. Feedback of these trials is presented after trials of condition 4 itself and after trials of condition 3. To avoid suspicion on the part of the subjects, a small random number is added to (when the deviation is positive) or subtracted from (when the deviation is negative) the deviation of condition 4 to present a slightly different value after a trial of condition 3. Because of this repeated feedback, a condition 4 trial always precedes a condition 3 trial. In

condition 5 quasi-feedback is presented. The quasi-feedback is computed independently from the observed deviation in these trials and based on a quasi-random process. This procedure generated an 'X' varying from 7 points left of the 'M' to 32 points right of the 'M', each point having an equal probability of being chosen. Hence in approximately 82% of the trials in this condition a positive feedback is displayed.

The trials of the five conditions were run in four different sequences, a trial of condition 4 always preceding a trial of condition 3. These different sequences are: 1,2,4,3,5; 2,4,1,5,3; 4,3,5,1,2; and 5,2,1,4,3. In this order the four sequences are run 15 times, in total 60 trials per condition.

Each trial consists of 100 randomly generated numbers 1 or 2, at a rate of 100 numbers per second (RNG) or in condition 1 at a rate of approximately 100 per second. After each trial the difference between the numbers of generated values '2' and the expected number of values '2' (50) was calculated and expressed as a CR value. This CR value fixed the position of the 'X' on the screen for the feedback.

EXPERIMENTERS, SUBJECTS AND OBSERVER

The experiment was designed and supervised by the author. The experimenting was carried out by Mr. v.d. Brink. He was not informed about the purpose of the experiment nor about the nature of the conditions under investigation. It was hoped that in this way the possible influence of experimenter bias could be diminished. As soon as the experiment started the experimenter left the room to ensure that only the subject would observe the feedback.

In total 60 subjects, mainly students in psychology, took part in the experiment.

One month after the experiment was completed the list of data was printed in such a way that no one could observe the data. This list was then given to a secretary who was unaware of the nature of the experiment. She was asked to draw up a new list containing the names and the CR values associated with the scoring in the various conditions for all subjects, to ensure that she really had observed all the data. This list is reproduced in table 1.

THE RNG

The RNG ia based on the principle of electronic noise. Prior to the experiment the RNG was tested. The total deviation over 101,698,376 samples showed to be -8653, a 'bias' of less than 0.017 percent. No first-order dependency was observed.

RESULTS AND DISCUSSION

Due to computer failure the data of only 55 subjects is available. Furthermore the data from condition 1 could not be included in the analysis. It showed that the possible range of the starting point of the sequence of numbers generated by the algorithm in the quasi-random condition was too restricted. Consequently, the series of generated numbers for most subjects in this condition overlapped, which resulted in a high correlation between the scores of the different subjects. Hence the data of this condition was dropped from the analysis.

The scoring of the subjects in conditions 2-5 expressed as standard normal scores is presented in table 1.

TABLE 1

Scoring of the subjects in the conditions as standard normal scores

Subject	Conditions 2	3	4	5	Subject	Conditions 2	3	4	5
1	0.75	1.21	0.27	1.19	2	-1.03	0.80	-0.21	0.36
3	0.26	-1.47	-1.56	-1.19	4	-1.42	0.18	0.67	0.08
5	0.41	-1.34	-1.42	1.52	6	0.62	-1.76	-1.45	0.67
7	-0.34	1.51	-1.34	0.62	8	-1.06	-0.03	-1.39	0.44
9	0.62	1.10	-0.83	-0.48	10	0.15	-0.83	-0.21	2.12
11	-1.23	1.11	-2.71	0.93	12	1.55	1.52	0.41	-0.26
13	0.21	-0.34	0.21	-1.76	14	-0.98	-1.16	-1.45	-0.70
15	0.59	0.05	0.28	0.54	16	1.34	-1.68	-0.03	1.19
17	-1.86	-0.28	0.67	-1.11	18	0.67	-0.44	1.39	-0.65
19	-0.41	-0.03	0.36	1.42	20	-1.08	-0.08	-0.80	1.68
21	-0.08	0.85	1.45	1.58	22	2.48	-0.10	-1.14	-0.05
23	-0.22	1.24	0.10	-0.05	24	-1.42	0.01	1.76	-0.65
25	1.19	0.98	0.61	-0.22	26	-1.08	0.67	0.39	0.54
27	0.44	-0.67	0.41	-0.03	28	0.08	0.03	0.13	0.05
29	-1.42	-0.57	-3.85	0.36	30	-0.57	-0.57	0.98	1.29
31	0.98	-0.23	0.00	-0.72	32	0.90	1.38	0.98	-0.03
33	0.72	2.00	-1.58	-0.90	34	0.18	0.28	0.04	-1.34
35	0.83	0.44	0.80	-0.13	36	0.03	-0.23	-1.19	-0.15
37	-1.01	0.83	1.24	0.44	38	-0.10	-2.65	0.93	-0.88
39	0.72	1.58	0.27	-1.63	40	-1.14	-0.15	-0.54	0.67
41	1.78	-0.49	-1.39	-0.75	42	-0.75	-2.69	1.32	0.03
43	-1.21	-1.60	0.23	-0.28	44	-0.21	-0.41	1.85	-1.45
45	1.06	-0.57	-0.39	-0.62	46	-0.39	0.54	-2.38	-0.85
47	-0.72	0.15	-0.77	0.15	48	-0.18	-0.39	0.26	-0.52

TABLE 1
(continued)

Subject	Conditions 2	3	4	5	Subject	Conditions 2	3	4	5
49	-0.34	0.98	-1.78	0.08	50	-1.19	1.08	-0.65	-0.54
51	-1.08	1.47	0.93	-0.26	52	-0.03	-2.01	-0.05	-1.05
53	0.08	-0.44	-1.08	-0.18	54	-1.96	-1.55	-1.58	1.76
55	0.39	-0.65	-0.13	0.31					

The t-test for standard normal distributed scores yielded non-significant values for the four conditions (t_2=-0.76; t_3=-0.42; t_4=-1.47; t_5=0.09). The t-test for related samples showed that conditions 3, 4 and 5 did not differ significantly from condition 2, the standard PK condition ($t_{2,3}$=0.19; $t_{2,4}$=-0.68; $t_{2,5}$=0.56). The correlation between conditions 2 and 3, and between conditions 2 and 4 were also non-significant ($r_{2,3}$=0.07; $r_{2,4}$=0.06). The total score per subject over the four conditions showed to be significant at the .01 level for one subject, and to be significant at the .05 level for three subjects (all tests two-tailed).

Unfortunately none of the hypotheses under investigation can be confirmed or rejected as the subjects proved to be unable to influence the RNG. One possible disadvantage of the experimental design, which might have contributed to this result, is the nature of the feedback presented to the subject. After each trial, that means approximately every second, feedback was presented by writing a new line with a 'M' and a 'X' on the display screen. When a new line was written all previous written lines moved up one position. This resulted in a moving pattern with a strong perceptual influence which greatly distracted the subjects. This might have had an inhibitory influence on the subject's psi ability.

ABSTRACT

In a PK experiment with a high-speed random number generator some hypotheses following from the theoretical approach based on quantum mechanical notions, the 'observational theory', were investigated. No results were obtained since the subjects proved to be unable to influence the random number generator.

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EXPLORATORY TEST ON THE PROSCOPIC THEORY KNOWN AS THE 'ADVANCED WAVE HYPOTHESIS'

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Physical theories have typically, sufficient symmetry (between past and future) to suggest that phenomena akin to precognition should occur in a manner qualitatively, although not necessary quantitatively, similar to the occurrence of precognition.

For instance the equations describing the behaviour of electromagnetic waves do have a general solution which allows for waves travelling backwards in time. Feinberg (1974) suggests that the poorly understood process of short term memory is time symmetric in a manner equivalent to electromagnetic waves. Precognition might then be considered as basically remembering future things.

This assumption yields at least two hypotheses which might be falsified by a quantitative experiment:

1) A subject is not able to predict events he will never become aware off.

2) The longer the time between prediction and observation of the event the less accurate the prediction will be.

The present exploratory series tests these hypotheses by introducing two conditions in a plain number precognition experiment. In the first condition the numbers which are to be predicted are fed back visually to the subject. In the second condition, however, these numbers are not revealed to the subject.

METHOD

The events to be predicted are a series of random numbers. The subject gives his prediction by pushing the corresponding numerical key on the keyboard of a computer terminal. A software random number generator is immediately activated and a random number is produced in the range (1,5) within 10 milliseconds.

Each run consists of 50 trials. The first and all other oddly numbered trials are followed by feedback of the generated number. However, the second and all other even trials are not followed by

feedback. In fact no one ever knows what the generated number was.

After the run has ended the experimenter lists the datafile on the screen and only then can the subject and experimenter observe the total score on the 'blind' (even) trials.

SUBJECTS

Six subjects participated in the present experiment. Three of them were paranormal healers and two were visitors to the laboratory. They all did a single run. Another 37 runs were done by the experimenter himself acting as a subject. These runs were done at the end of a daily routine consisting of a series of PK experiments which will be reported elsewhere.

It should be mentioned that the experimenter did not believe in the suggested 'advanced waves'.

RESULTS

The series were stopped due to external circumstances. The reason was that the above mentioned PK experiment resulted in a too large amount of data. It was decided to rewrite the computer programs which controlled these experiments in such a way that the data could be stored more efficiently. Therefore all the old data including that of the precognition experiment was analysed and subsequently removed.

In total 42 runs of 50 (2x25) trials were carried out in the period between May '76 and December '76. The total number of hits on the visible trials was 204. The expected number of hits was 42x25/5=210 hits per condition. The deviation of -6 was not significant (CR=-.46). The total number of hits on the 'blind' trials was 235 against the same expectation value of 210 (deviation is +25, CR=1.93). Although these latter trials considered by themselves are marginally significant ($p<.05$, one-tailed), the difference between both conditions is not (CR=1.69).

DISCUSSION

The present data may not be considered conclusive as regards the suggested theory of 'advanced waves'. However, it certainly does not support this theory.

In another theory (Walker, 1974) the observation of an event is considered as the ultimate decisive point in a parapsychological experiment. In this case there is no essential difference between the 'visible' series and the 'blind' part of the experiment: the

visible part consists of 42x25 observations on the outcome of a random process, each outcome having a probability of 20%, while the blind part consists of 42 observations on the outcome of a random process (namely the observation of the sum score); each outcome having different probabilities. In this model there is no need to discriminate between PK, clairvoyance, telepathy and precognition.

It should also be rather obvious that the experimenter could often exert tremendous influence on the results, he being the 'main observer' of them.

Walker shows that this description of paranormal phenomena is not in conflict with quantum theory. On the contrary, it might be interpreted as an elegant way out of the long controversy about the measurement problem in quantum mechanics.

Different quantitative results between the 'visible' and 'blind' part of the experiment are accounted for in Walker's theory, as both processes under observation (visible trials and blind sum score) have different statistical properties (Walker, to be published). This phenomenon can be compared with the fact that runscoring is more significant than trialscoring in different PK experiments (Bierman & Houtkooper, 1975; Schmidt, 1973).

ABSTRACT

A theory has been tested which 'explains' precognition as an 'advanced solution' to the (yet unknown) equations describing short term memory.

In this experiment 2100 numbers were predicted. In 50% of the trials feedback was given to the subject. The total result on these trials was not significant. After the other 50% of the trials no feedback was given. The checking was done by a computer and only the total score was shown to the subject after the run of 50 trials had ended. The total result on these trials was marginally significant (CR=1.93; $p<.05$, one-tailed).

These results contradict the suggested 'advanced wave' hypothesis. They may however be understood in terms of Walker's 'observational' theory of paranormal phenomena.

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A FURTHER ATTEMPT TO VALIDATE THE DMT AS A PREDICTOR OF SCORING DIRECTION

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INTRODUCTION

The Defense Mechanism Test (DMT) developed by Dr. Ulf Kragh is a projective test in which a tachistoscopic technique is utilized. The DMT was first introduced into parapsychology by the senior author during his stay at the Duke Parapsychology Laboratory in 1963 (see Carpenter, 1965). In the first studies carried out, M.J. was successful in predicting the scoring behaviour of subjects in an ESP test based on their DMT data. Further studies by Johnson and Nordbeck (Johnson & Kanthamani, 1967), Johnson and Kanthamani (ibid), Johnson (1967, 1970, 1971, 1975), and Johnson and Nordbeck (1972) have corroborated the view that subjects displaying a high degree of 'perceptual defensiveness' (or 'precognitive defense organization', PDO, to use Kragh's term) in their DMT protocols tend to score at mean chance expectancy level (M.C.E.) or below ('psi missing'),whereas subjects who do not manifest a high level of 'PDO' (especially of the 'isolation' type) when presented subliminally with threatening pictorial material, tend to score at M.C.E. level or above.

These views have to some extent also been corroborated by a couple of American studies (see Braud, 1975, and Miller & York, 1975), although differences in the techniques utilized make a direct comparison of the results somewhat hazardous. As regards a brief summary of ESP studies in which the DMT has been used as predictor, see Johnson (1975).

In the present study subliminal stimulation was utilized as an independent variable. The outline of the study was also to some extent based on some suggestive findings regarding the possibility of enhancing a subject's 'psi-vigilance' by subliminal 'induction', which in a subsequent ESP experiment was used as one category of

target (Johnson, 1975).

HYPOTHESES

The selection of a cognitive task related to the ESP task

We hoped to enhance the subjects' motivation towards the ESP task by getting them intellectually and emotionally involved in a problem-solving task the solution to which was later on going to re-appear as ESP target. By interrupting the subjects before they arrived at the solution and by arranging an intermission before the ESP task was presented, we hoped to trigger cognitive and preconscious processes which could be conducive to eliciting the psi performance needed for success in identifying the target envelopes which contained a visual representation of the solution to the cognitive problem.

Hypothesis A1: The above mentioned procedure should enhance the subjects' psi-vigilance.

Hypothesis A2: Subjects who obtain the solution to the problem subliminally should score differently from those who receive a subliminal stimulation not depicting the solution to the problem. No directional prediction was made.

The DMT/ESP relationship

Hypothesis B1: Subjects having protocols characterized by no or moderate signs of PDO classified as isolation should tend to manifest positive scoring (above mean chance expectancy, M.C.E.), especially if there are also moderate signs of the PDO classified as projection.

Hypothesis B2: Subjects having protocols characterized by strong signs of isolation should on the whole score according to chance level except when signs of isolation classified as 'discontinuities' are present, or where these signs are in combination with (moderate) signs of denial and/or projection, when subjects should tend to manifest psi missing (below M.C.E. scoring).

For further information on classification of PDO in the DMT, see Kragh (1970, pp 108-113).

PROCEDURE

1 The cognitive task

The subjects had to carry out a problem-solving task known as 'the cheap-necklace problem' (see Wickelgren, 1974). Each subject was given three minutes to solve the problem. It was presumed that this time was adequate for the subject to become absorbed by the problem yet short enough to prevent him from arriving at the solution. (In fact, 14 subjects succeeded in solving the problem before the three minutes had elapsed.) Between the end of the period for the problem-solving task and the next procedure (the subliminal induction), there was an intermission of five minutes duration.

2 The subliminal induction

Fifty percent of the subjects, determined on a random basis, were given a subliminal stimulus depicting the solution to the cognitive problem, whereas the remaining fifty percent of the subjects were given a subliminal stimulus depicting the necklace, but not depicting the solution to the problem. The stimuli in question were flashed tachistoscopically three times with an exposure time of 1/100 second. At the time of the experiment both experimenters were unaware as to the kind of subliminal stimulus a subject had received. One experimenter (C.L.) was responsible for part 1 and 2 of the experiment.

3 The ESP task

After another intermission of five minutes the subjects were sent to the other experimenter (M.J.) to take part in an ESP test. Each subject had to carry out fifty trials in an ESP multiple-choice task. One trial consisted of the presentation of a transparent plastic folder in which five identical looking sealed opaque envelopes were placed in a fixed, pre-determined geometrical pattern (a star-like pattern). On the back of each envelope there was a number (written by another experimenter who had prepared the cards) to make later identification possible. The subject was instructed to use his intuition to choose the envelope which he thought contained the target, that is, a picture depicting the solution to the cognitive task. In the non-target envelopes there was a picture of the necklace corresponding to the one not providing the solution to the problem, which fifty percent of the subjects had received as stimulus during the 'subliminal induction' procedure. The subject had to indicate his choice by pointing with

his finger at the envelope which he thought contained the target. This multiple-choice experiment had a 1/5 probability for success. The scores were not evaluated until all the subjects had been tested. The evaluation of scores was carried out jointly by M.J. and C.L.. The subjects were promised to be informed of the correct solution to the problem ten days after the ESP-testing was carried out.

4 The DMT

A few days after a subject had taken part in the previously mentioned cognitive and ESP task he was asked to come back to be DMT-tested. The DMT-testing was carried out in small group sessions with three to five subjects being present on each occasion. Expressed concisely, the DMT technique consists of flashing certain threatening pictures tachistoscopically. After each exposure, the subject has to report his impressions of what he thought he saw either by verbal reporting (with individual testing), or by writing, or by way of a combination of verbal reporting, writing, and making drawings. The stimulus intensity is increased by steps leading to 'fractionated' levels of interpretation usually terminating in rather veridical reports of what the subject thought he saw. The subject's report after each exposure is later on coded and interpreted according to the manual (Kragh, 1969). For a more detailed description of the DMT technique, see Kragh, 1970.

The test conditions complied with those recommended by Dr. Kragh. The subject wrote down and made drawings of what he thought he saw using prepared test sheets. Because of lack of time only one of the DMT series (Hy 3) was administered. The testing was carried out by C.L., the taped instructions were in Dutch.

The coding and ranking of the protocols was later performed by M.J.. As a precaution against deception on his part, all names were removed from the protocols and identification numbers used instead. Finally the identification of the subjects' names was made jointly by M.J. and C.L. in order to be able to relate M.J.'s rankings based on the DMT data to scoring on the ESP test.

SUBJECTS

54 first-year students in psychology at the State University of Utrecht took part in the experiment on a voluntary basis. It was possible to trace 49 of the 54 subjects in connection with the subsequent DMT-testing. Except for the DMT the subjects were tested individually.

RESULTS

As can be seen from Table 1, the over-all outcome of the experiment was disappointing. Hypothesis A was not supported by actual findings. The different treatment of the two groups of subjects did not seem to influence the scoring behaviour on the ESP task.

TABLE 1

ESP-scoring in relation to 'subliminal induction'

Subliminal induction	N	Total ESP score	Deviation
The solution to the problem	20	207	+7
Not the solution to the problem	20	208	+8
Subjects who solved the problem	14	136	-4

Sum total: 551 MCE: 540 Diff.: +11

Hypothesis B was not supported to a statistically significant clear-cut extent (see Table 2). Neither the Spearman rank correlation (r_s=0.26, corrected for ties $p < .05$ one-tailed) nor the χ^2-analysis (Table 3) yielded statistically significant results. One might say, however, that a tendency was manifested regarding a correlation between the performance on the ESP task and predictions based on the DMT data. The tendency is in the expected direction, although the correlation is not strong enough to be statistically significant.

DISCUSSION

This time the DMT failed to function as a useful instrument in predicting the scoring behaviour of the subjects taking part in the ESP task. However, the outcome weakly supports the trend that has shown up in previous investigations. One should bear in mind that the constant appearance of a rather strong correlation would imply the existence of a parapsychological experiment with a rather high degree of repeatability, and further that this would be rather odd

TABLE 2

Outcome of predictions based on DMT

Ranking based on DMT	No. hits on the ESP task	Deviation from MCE (MCE=10)	Ranking based on DMT	No. hits on the ESP task	Deviation from MCE (MCE=10)
"Better half" according to prediction			"Poorer half" according to prediction		
1	15	+ 5	26	11	+ 1
2	22	+12	27 (d)	5	- 5
3	13	+ 3	28	9	- 1
4	10	0	29	9	- 1
5	11	+ 1	30	8	- 2
6	9	- 1	31	6	- 4
7	3	- 7	32	10	0
8	12	+ 2	33	10	0
9	11	+ 1	34	12	+ 2
10	8	- 2	35	6	- 4
11	9	- 1	36	9	- 1
12	13	+ 3	37	13	+ 3
13	9	- 1	38	11	+ 1
14	12	+ 2	39 (d)	8	- 2
15	14	+ 4	40	14	+ 4
16	12	+ 2	41	8	- 2
17	8	- 2	42	9	- 1
18	10	0	43	8	- 2
19	18	+ 8	44	13	+ 3
20	16	+ 6	45 (d)	9	- 1
21	11	+ 1	46 (d)	6	- 4
22	6	- 4	47	13	+ 3
23	11	+ 1	48 (d)	5	- 5
24	10	0	49	11	+ 1
Sum total:	273	Dev.: + 33	Sum total:	233	Dev.: - 17
25	10	excluded			

(d): sign of denial in DMT protocol

TABLE 3

χ^2 - test performed on the DMT and ESP scores

		ESP scores above MCE	ESP scores below MCE
DMT ranks	upper half	14	7
	lower half	8	14

$.025 < p < .05$ (one-tailed)

when one considers the rather low predictive validity of psychological tests in general and those of the projective type in particular.

A closer examination of table 2 may be informative. The score of the subject with rank 2 proves to be strongly significant. (In subsequent experiments which will soon be published this subject is shown to have manifested a rather stabilized level of extra-chance scoring.) More disappointing, however, is that the subject with rank 7 - supposedly a high scorer - turned out to score significantly below the MCE. A post hoc inspection of the DMT protocol for this subject does not give any clues as to an interpretation. Rank 19 illustrates another anomaly. The post hoc inspection of the DMT protocol for this subject does not offer an explanation of why this subject should score significantly above the MCE.

A trend that may be informative and of value as regards the formulation of conjectures to be tested in subsequent studies, is the fact that the subjects with the ranks 27, 39, 45, 46, and 48 are the only subjects who manifested the DMT sign of denial (the threat is explicitly denied in the report: "There is nothing threatening in the picture."). It implies that all five subjects with the perceptual defensive structure (PDO) of denial can be found in the 'poorer half', according to predictions based on DMT data. It also implies that they scored 'negatively' (see table 2). This observation is consistent with previous findings which tentatively pointed in the direction of a relationship between the PDO of denial and the psi-missing syndrome. A tentative explanation of such a correlation has been touched upon by the senior author (Johnson, 1971, pp 27-31). According to data on which the DMT has been standardized, at least ten per cent of the subjects belonging to the standardization population manifested 'denial' in their

protocols (Bálint, 1976).

A fruitful research strategy which one could follow might be to try to trace as many subjects as possible who have manifested 'denial' in their DMT protocols in studies carried out other than for parapsychological purposes. These subjects with 'denial' in their protocols could then be subjected to ESP studies and compared on ESP performance with a control group of subjects who did not manifest the PDO of 'denial' in their DMT protocols.

SUMMARY

The present study in which 54 subjects took part had two major objectives. One was to carry out a further cross-validation of previous findings regarding a relationship between certain signs (preconscious defensive organization, PDO) in the Defense Mechanism Test (DMT) and a subject's scoring behaviour in an ESP test. The other was to try to enhance the psi-vigilance by cognitive and subliminal manipulations of the subjects.

Subjects were given a problem-solving task, supposedly rather absorbing. The task was interrupted before most subjects had solved the problem, thus creating a residual motivation strongly focused towards solving the problem. In the ESP test (of the multiple-choice type) the target constituted a visual representation of the solution to the cognitive task. As a further possible means of influencing cognitive as well as preconscious processes that might be instrumental for the discrimination of the target on a psi basis, a subliminal stimulus was given to the subjects prior to the ESP task. Fifty per cent of the subjects were given a stimulation in which the solution to the problem was presented, whereas the remaining fifty per cent received a stimulation relevant to the problem but not offering the solution. Precautions were taken to avoid an experimenter expectancy effect.

The outcome of the predictions made on the basis of the DMT data as regards ESP scoring behaviour was not clear-cut. However, the results are suggestive and in the expected direction. The subjects, considered as a group, scored on the ESP test according to chance expectation, implying that the endeavours made to enhance their psi-vigilance were not successful. The two types of subliminally presented stimuli did not lead to different results. Finally a suggestion is made as to how one may try to test the idea of a correlation between the precognitive defensive structure of 'denial' in the DMT, and the psi-missing syndrome.

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EXPERIMENTING WITH POLTERGEISTS ?

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While the laboratory parapsychologists is trying to negotiate the rocky road to a repeatable experiment, the poltergeist continues to perform for a small but growing group of researchers. Cases of poltergeist are referred to as examples of recurrent spontaneous PK, or RSPK, that is to say they erupt unexpectedly but once set in motion, there are repeated movements of household objects, percussive sounds or other physical occurrences. In other words, the investigator may be able to observe instances of 'macro' PK and perhaps discover the conditions on which they depend.

The poltergeist has a reputation for being too elusive for controlled observation. I think this is an unfair prejudice. The poltergeist may be a better subject for experimental research than rats or gerbils or even most humans. In this paper I shall explore the extent to which the poltergeist can be domesticated for experimental research. On a later occasion I shall describe some of the features of the poltergeist which may point to the RSPK process.

What are the possibilities of coming up against an active poltergeist? We do not know the incidence of poltergeist eruptions in the population. All we know are the cases which come to our attention, either directly from the family or through newspapers and other public media. Some years ago I subscribed to a newspaper clipping service hoping to learn of new cases this way, but none appeared I had not already heard about. I would guess that parapsychologists in the United States learn of about five promising cases a year of which two or three are actually investigated, and that one is judged to include genuine examples of RSPK. Anyhow, from 1950 to 1974 there were 19 RSPK cases which reached the professional parapsychological literature in the US, an average of nearly one a year. This may be increasing: in the last ten years of that period there were twelve cases, and in the last year, 1974, there were four.

The number of cases which the investigator judges to include genuine RSPK effects versus the spurious or indefinite cases is then

&: The author had a stay at our laboratory during the fall of 1974. At that time he initiated the reported article.

somewhat on the order of one out of three. It should not be forgotten, however, that a personal investigation is usually only made after interviews by phone or letter with witnesses to the incidents. For each case which survives this screening there may be three which are dismissed. With further attention from parapsychologists these figures would probably at least double and with some investment to improve the channels of communication with the public (through social service departments, the police, clergy and the media) the cases which reach us would undoubtedly also increase substantially. Is increased attention to the poltergeist justified? The answer to this question presumably depends on the ease or difficulty of conducting a serious study and on the likelihood that we may learn something about psi while doing so. To get an idea of the testability of the poltergeist and also of the clues it may provide to the psi process, I have made a survey of published cases.

TEMPORARY AND GEOGRAPHIC DISTRIBUTION

To be reasonably certain that we are dealing with genuine psi phenomena, I have restricted the survey, firstly, to published reports by persons held to be serious and truthful by their contemporaries. Secondly, the authors, or persons they have interviewed, must have witnessed at least one incident which convinced them they were dealing with psi and which remains convincing in the telling. There were 116 such cases. Of these, 97 appeared in professional parapsychological books or journals. All 97 occurred after 1849 though the published accounts often came later (I have used the beginning date to designate the year for a case). The poltergeist cases receiving scientific attention thus fall in the period 1850 to the present. I have divided this into three sections: 1850-1899 where there were 25 cases, 1900-1949 where there were 38, and 1950-1974 where there were 34. Prior to 1850 there were 19 cases which fulfilled my criteria. The first of these was the Devil of Mascon in 1612. This comes to us thanks to Robert Boyle who brought the case to public attention by recommending its publication in English: the father of chemistry also assisted in the birth of poltergeist research.

These four divisions give a picture of the poltergeist over four centuries and an opportunity to learn which of its features change over time and therefore may be determined by extraneous factors and which are stable and may therefore express the underlying process.

The geographical distribution of the cases however is limited: one hundred and six, (91%) occurred in Europe and the United States (see table 1). This clustering in the West is probably the result

of the easy means of mass communication coupled with the presence of parapsychologists. Is the poltergeist as pictured in the present survey then a product of Western beliefs and attitudes? This seems doubtful since several of the cases come to us from isolated farms or villages or from families who seemed to have no idea or expectancy about such phenomena before they erupted in their own homes. (for simplicity's sake I speak about the persons primarily affected as the family and the place as a home though in some cases we deal with places of business or areas in the open). Moreover the

TABLE 1

Geographic distribution of cases

Country	Number	Percentage
United States	31	27
British Isles (England, Scotland, Ireland)	26	22
Germany, Austria, Switzerland	21	18
Italy	9	8
France	7	6
Holland, Belgium	5	4
Scandinavia (Finland, Iceland, Sweden)	4	3
Russia	2	2
Czechoslovakia	1	1
Europe and United States	106	91
Indonesia	5	4
India	4	3
Mauritius	1	1
Rest of world	10	9

focal person (or poltergeist agent) who seemed to play a central role in the incidents was often in the early teens and presumably less affected by cultural norms than his or her elders. It is difficult to suppose that the characteristics of the poltergeist are all formed by prior beliefs.

OBSERVERS AND OBSERVER EFFECTS

If we ask how seriously the 116 cases can be taken as evidence for parapsychological phenomena, the credibility of the witnesses becomes important. In 105 (90%) of the 116 cases outside witnesses were present during one or more apparent RSPK incidents while in 11 cases the reports were due to the members of the affected families (see table 2). Seventy-three (63%) of the observers had some professional training and of these 25 (22%) of the total belonged to parapsychological organizations. This gives some assurance about the reports.

We can not generalize from this sample and conclude that the poltergeist in general stands up well to scrutiny by outsiders: more than anything else it is the fact that the phenomena were well witnessed that opened the pages of a journal to a case. It is, however, encouraging to notice a steady increase of professional observers over time, from 37% of the witnesses in the first period, to 64% in the second, 66% in the third, and 73.5% in the fourth. This in part is a reflection of the growing involvement of parapsychologists from 0%, 8%, 24% to 41% over the four periods.

Another question related to the research possibilities of the poltergeist concerns the effect the outside witnesses have on the phenomena. It sometimes seems that visitors put a pall on the activity especially when they first arrive. Other times the observers appear actually to enhance the events. In the Olive Hill case (Roll, 1976) the RSPK movements had begun in the home of the grandparent's of Roger, a 12 year-old boy who seemed to be the center for the events. Altogether there were 170 such incidents lasting about a month in November and December, 1968. There had been no disturbances in the boy's own home except possibly for three occasions when his parents told us they heard knocks on the outside doors and no one was found. But when John Stump and I visited their home, there were 20 movements of furniture and household objects in a ten hour period. For instance, I was once standing in the open doorway between the children's bedroom and the living room with my back to the door frame and facing the bedroom dresser, when I saw a bottle with toilet cream leave the dresser and move about four feet into the room and fall to the

TABLE 2

Professions of main outside witnesses

Witnesses	1612-1849		1850-99		1900-49		1950-74		Totals	
	N	%	N	%	N	%	N	%	N	%
Members of the parapsychological research organizations			2	8	9	24	14	41	25	22
Police officers, Court testimony, Government officials	2	10.5	9	36	9	24	5	15	25	22
Psychologists, Physicians, Scientists, Lawyers, Clergy	4	21	4	16	4	10.5	5	15	17	15
Teachers, "Official committee", Navy officers, Fireman	1	5	1	4	3	8	1	3	6	5
Total professional observers	7	37	16	64	25	66	25	73.5	73	63
Other outside witnesses	9	47	8	32	9	24	6	18	32	28
Total outside witnesses	16	84	24	96	34	89	31	91	105	90.5
No outside witnesses mentioned	3	16	1	4	4	10.5	3	9	11	9
Totals	19		25		38		34		116	

floor. Though I was facing the dresser, I was also keeping track of Roger out of the corner of my eye. He was on my right at the other end of the living room. His sister was on my left and slightly behind me. There was no one else in the bedroom. This bottle had moved earlier at which time I examined it and the dresser for strings and other mechanisms which might have been used to produce the event fraudulently. I again examined the bottle and dresser after the second event. Since I found nothing suspicious and since I was the person closest to the dresser I thought this was probably a genuine RSPK effect. Stump and I observed several other events under equally good conditions.

As a result of this eruption in a home that had escaped the violent events in the grandparents' house, Roger's mother concluded that Stump and I had brought the poltergeist with us from the other place and asked us to leave, hoping it would follow. This would have simplified things for both parties but the poltergeist was not tempted by a visit to Durham (we were also unsuccessful in persuading Roger's parents to allow him to come). Whatever can be said about the opinion of Roger's mother, it did seem that we somehow stimulated the effects.

The observer influence is well known from experimental psi research: some people seem to suppress psi in their subjects and other enhance it. Again we must remind ourselves that we are not dealing with a random sample of poltergeists. In the hope that the present collection may nevertheless give an indication about the observer effect, I analysed the 105 cases where outside observers were present from this point of view. This analysis is complicated by several factors. Sometimes a new visitor to the home seemed to suppress the incidents at first, but then they would resume or even become more violent. In other cases one observer seemed to suppress the events while another enhanced them. In general large crowds of curiosity seekers put a damper on things. Disregarding these and emphasing the overall trends, I obtained the figures in table 3. In six (6%) cases the poltergeist seemed to retreat with observers on the scene, while in 23 (22%) cases the incidents seemed to increase. In most of the reports 67 (64%) the events continued much as before. Altogether in 90 (86%) cases the presence of observers did not seem to impede the activities.

There is another observer effect which needs to be taken into consideration for planning RSPK strategy. In some cases, observation seems to have a special inhibitory effect. Whether or not the witnesses suppress the events, a direct gaze on objects sometimes seems to inhibit or retard movement. John Bristow, the main witness of disturbances in a carpenter's workshop in Swanland, Yorkshire in

1849 told F.W.H. Myers (1891-92, p.384-394) that the pieces of wood "sometime seemed to come from the floor; sometimes from near the roof. No one ever saw a missile actually starting on its course. It seemed as though one caught sight of them about 6 inches from their initial point.... Sometimes one of us would look fixedly for many minutes at a bit of wood on the floor. It never moved while we looked at it. But once let our attention be relaxed and that very bit of wood would come flying at us from some distant point. Mr. Crowther .... used sometimes to sit in the shed for two or three hours at a time, watching to see a piece of wood start on its course. He never saw one start; though, like the rest of us, he saw many which seemed to have just started. We could never make up our minds whether the pieces began their flight invisibly, or only when our attention was diverted." (Myers, 1891-92, p.390-391).

Other cases belonging in this general category include most of the stone throwing poltergeists and household objects moving from empty rooms, or moving only when it is too dark to see. There are 80 cases with movements of solid objects where the effect of outside observers was indicated. If we distinguish between cases where the origin of movements were not seen and where it appeared that the outside witnesses inhibited the movements if they were watching the objects, it appears that more than one half, namely 42, suggested an inhibitory effect while in 39 cases the poltergeist seemed impervious to direct observation (see table 4). The percentages of the total of 94 are 45% versus 41% with 14% (13 cases) where the influence of direct observation, if any, could not be determined. These proportions are closely reflected in each of the four periods.

In discussing why observers may miss the beginning stage but see the object in flight or as it lands, W.E. Cox has drawn an analogy to the common observation that we frequently see birds in flight, but less often birds taking off: similarly we are likely to pay attention to a flying porcelain plate but not to one sitting on its shelf. I have tried to take account of this possibility in the analysis.

A person who plans to investigate RSPK should be prepared for the possibility that direct observation may retard movements and also for the more remote possibility that his presence may inhibit the events. It is fairly easy to provide for controlled observation of an area without having the objects located in this area in direct view. In the Miami case (Roll and Pratt, 1971) we often had the target areas under complete surveillance when objects moved - though we never saw them take off.

The situation is more difficult if the visitor suppresses the events by his mere presence. In that case he might obtain the help of a colleague who does not have this effect or rely on other

TABLE 3

Effects of outside witnesses on number or severity of occurrences

Period	Total number	Uncertain N	Uncertain %	Enhances N	Enhances %	No effect N	No effect %	Total Enhance & No effect N	Total Enhance & No effect %	Inhibits N	Inhibits %
1612-1849	16	1	6	7	44	8	50	15	94	0	0
1850-1899	24	2	8	3	12.5	17	71	20	83	2	8
1900-1949	34	3	9	5	15	26	76	31	91	0	0
1950-1974	31	3	10	8	26	16	52	24	77	4	13
Totals	105	9	9	23	22	67	64	90	86	6	6

TABLE 4

Effect of visual observation by outside witnesses on beginning of movements of objects

Period	Total number	Uncertain N	Uncertain %	No effect N	No effect %	Inhibits N	Inhibits %
1612-1849	13	2	15	5	38	6	46
1850-1899	23	2	9	9	39	12	52
1900-1949	32	7	22	13	41	12	37.5
1950-1974	26	2	8	12	46	12	46
Totals	94	13	14	39	41	42	45

witnesses, whose presence does not affect the phenomena. Video, film or other electronic surveillance might also be tried.

DURATION OF CASES

The first time a parapsychologist often hears about a case is when it's over. Important information can be obtained about past cases especially if there are well-witnessed events and if the family allows interviews. In this paper, however, I am interested in the potential of the poltergeist as an experimental subject - that is with the possibility of controlled research. In other words what is the possibility that the events are still occurring when the investigator gets on the scene?

The duration of the disturbances was reported or could be inferred in 98 cases (see table 5). The time varied from one day to six years. The average was 5.1 months, but the median only two. The large averages in the first and last periods are due to two unusually long-living poltergeists.

The poltergeist explorer will have a good chance of finding an active case if he gets to it within a few weeks of the first events. Again, we must remember the limitations of the present sample: it is more likely that long lasting cases will reach public and professional attention than brief eruptions. If all cases were know the figures for the average and median durations would probably shrink. Nevertheless, it is clear that the poltergeist explorer will have a good chance of coming up agains an active case if he gets to it within a few weeks - and that he may be in luck even if the case has run for several months.

MAIN PHENOMENA

Poltergeist investigations typically concern ordinary physical events occurring under extraordinary circumstances. The incidents may have unusual aspects, for instance, moving objects may show unusual trajectories, nevertheless we are dealing with obvious physical occurrences.

Of the 116 cases, 105 (90.5%) involved recurrent movements of objects (see table 6). The Olive Hill case already mentioned is an example. There are 63 cases (54%) which had knocks, raps or other percussive sounds. (When I speak about sounds in the following, I refer to percussive sounds.) In 55 (47%) of the cases there were both movements and sounds. (There are also several cases where objects were said to land with unusually loud noises.) In 10 cases (9%) there were lights, but only one (1%), the Clayton case (Roll,

TABLE 5

Cases where duration was reported

Years	Total number	Average (in months)	Median (in months)
1612-1849	18	9.6	3.0
1850-1899	18	4.15	2.0
1900-1949	30	2.5	1.3
1950-1974	32	5.5	1.95
Totals	98	5.1	2.0

Note: duration varied from one day to six years.

TABLE 6

Classification of main occurrences

Period	Movements		Sounds		Movements & Sounds		Lights		Water	
	N	%	N	%	N	%	N	%	N	%
1612-1849	16	84	11	58	8	42	3	16	0	0
1850-1899	24	96	15	60	14	56	2	8	0	0
1900-1949	36	95	20	53	18	47	1	3	0	0
1950-1974	29	85	17	50	15	44	4	12	2	6
Totals	105	90.5	63	54	55	47	10	9	2	2

1976, p.59-69) consisted entirely of light flashes. Two cases (2%) involved only the appearance of water (Cox, 1961, p.68-69; Bender, 1974, p.138-141). If we look at the distribution of incidents over the four time periods, we see that the proportions of movements and sounds remained about the same. It is sometimes thought that poltergeist missiles are warm to the touch. But this was only reported for six (6%) of the 105 cases with moving objects. Seven (6%) of the 116 cases had accounts of cold areas or feelings and in one, a room felt hot.

Again we should be alert to sampling bias: it may be easier to determine that something unusual is afoot if objects move or knocks are heard than if water appears mysteriously or if there are occasional flashes of light.

While we need to be aware of the possibility that there may be concealed RSPK effects, the poltergeist in general seems to be an exhibitionist. Unlike the proverbial good child the poltergeist likes to be seen and heard and this is a great help for its students.

There are other effects. In 27 of the cases (23%) apparitions or hallucinations were seen, representing human figures, animals, demons, hands, fingers, or amorphous shapes. In 13 (11%) intelligible voices were heard, including whispering and singing. Seven of these also had apparitions. In eight others (7%) there were also special sound phenomena variously referred to as sounds of wind, whistling, groans, laughter, screams, and so on. In five cases (4%) one or more persons were wounded or slapped by some unknown agency or stigmata appeared on their bodies and in five (4%) people were pulled or lifted. In four cases (3%) feces appeared, usually smeared on people's faces. The victims of the woundings and other unpleasantries were usually the poltergeis agent. There were four reports (3%) with unexplained fires, three (3%) with writing, and two (2%) where the manes or tails of livestock were braided. Several other cases showed pecularities of their own.

Aside from the movements and sounds, most of the effects were not common enough to be regarded as typical for RSPK. If we arbitrarily set 10% as the cut-off place, the apparitions and voices remain. I shall take a closer look at them in the second paper. As a rule the study of apparitions and voices do not play an important role in determining whether a case has evidence for RSPK - but they may contribute to an understanding of the RSPK process.

THE FOCUSSING EFFECT

In RSPK the term focussing refers to repeated incidents with the same or similar objects or taking place in the same area. In the

first place there is 'focussing' on the house or area where the events are concentrated. Secondly, there may be focussing on a special location in that area. For instance a room or a shelf in that room may have a concentration of incidents. This is called area focussing. Sometimes it seems that a special object, such as a kitchen cabinet, or a special class of objects, such as stones, are singled out. This is object focussing.

I have tried to determine whether focussing might arise as an artifact or whether one kind of focussing may be reduced to another (Roll, 1968, 1975). In the three cases for which we had sufficient data for analyses, there was evidence for focussing on types of objects, individual objects, and areas which could not be easily dismissed in other terms.

It is rarely possible to isolate the focussing effect from the other effects in the present collection of RSPK cases. But in 107 (92%) of the total of 116 cases there is prima facie focussing of one or more kinds (see table 7). In 89 (77%) there is focussing on objects,either individual or types of objects (in 12 of these there is also area focussing) and in 18 (15.5%) on areas. The evidence for focussing runs about equally strongly through the four collections.

RSPK is not only spontaneous and recurrent but the recurrence often involves the same objects and places. This feature is a powerful aid in research. When the investigator has discovered that certain objects or areas in the locality are more prone than others to become targets for RSPK, he can concentrate on them, rather than attempt to survey all the contents of the home. Focussing offers a means for experimental studies of RSPK.

EXPERIMENTING WITH POLTERGEISTS

In several of our cases 'tests' of one kind or another were done with the poltergeist, as a rule using the focussing priciple (see table 7). Such tests were reported in 48 cases of which 46 were successful, that is 40% of the total of 116. Usually the tests were of a rudimentary or exploratory type as in the following example: In December, 1921 a 15-year-old girl, Hannie was fired from her job at an inn in Lieserbrucke, Austria because of heavy damage to crockery, glasses and other objects in her vicinity. A British Naval Commander, Kogelnik, who was interested in parapsychology hired Hannie so he could investigate the phenomena at his home in London. On one occasion, a small iron box came off its shelf in the kitchen as he was giving some orders to the girl. Kogelnik replaced the box and then asked the poltergeist to repeat the performance so that he could see how it was done. "I waited for five minutes - for

TABLE 7

Focussing effect

Period	Total number	No focuss. N	%	Object focuss. N	%	Area focuss. N	%	Object & area focuss. N	%	Total cases with focussing N	%
1612-1849	19	2	10.5	11	58	4	21	2	10.5	17	89
1850-1899	25	4	16	18	72			3	12	21	84
1900-1949	38			26	68	6	16	6	16	38	100
1950-1974	34	3	9	22	65	8	23.5	1	3	31	91
Totals	116	9	8	77	66	18	15.5	12	10	107	92

TABLE 8

Cases with successful RSPK 'experiments'

Period	Total number	Number	Percentage
1612-1849	19	10	53
1850-1899	25	8	32
1900-1949	38	17	45
1950-1974	34	11	32
Totals	116	46	40

Note: in two cases unsuccessful RSPK tests were reported.
Note: in six cases during the last period, standard ESP or PK tests, usually with cards or dice, were used. Four of these also involved RSPK 'experiments'. Three of the four ESP tests gave significant results and so did three of the six PK tests.

ten minutes - life has taught me patience; and suddenly a smart 'bang' and the fragments of a porcelain cup were on the kitchen floor! This cup was kept on the same shelf as the iron box. Who could have thrown it? Not Hannie, for she was seated at the window under my eye, at no more than four yards distance. Or myself? I must ask this preposterous question, for there was no one else in the room. Between 5 and 6 p.m. on 6 May it would hardly be dark, and there were no shutters to the large window. So I must suppose an Invisible Third" (Thurston, 1954, p.34).

The fact that the poltergeist responds so readily to challenges of this type is consistent with the finding that visitors more often enhance the phenomena than suppress them.

It is curious that the lowest proportion of RSPK tests appeared in the most recent period (see table 8). On the other hand, standard ESP or PK trials, usually with cards or dice were sometimes administered to the RSPK agent. In six studies such tests were reported. Four also involved RSPK tests of the types discussed before. Three of the four ESP tests gave significant results and so did three of the six PK tests.

In Bender's experience "In all cases where laboratory experimentation with the subjects were possible, we found highly significant ESP scores but could not elicit experimental PK" (Bender, 1974, p.130). With regard to PK research in poltergeist studies, the most fruitful approach is to use the objects and places on which the activity already centers.

Sometimes the early RSPK tests were sufficiently well controlled to be a guide for contemporary investigations. In 1876, Sir William Barrett (1918, p.38-43) learnt that raps and other noises were disturbing a lawyer, his wife and ten-year-old daughter. This study, one hundred years ago, is an instructive example of what can be done in the investigation of sound effects, even without tape recorders and instrumentation. As in most other successful RSPK tests, Barrett based his study on the focussing principle. He reports:

> Mr. and Mrs. C. gladly acceded to my request for a personal investigation, and I came the next day after breakfast. It was 10 o'clock and a bright summer morning - - Mr. and Mrs. C. with Florrie and myself, no one else present, sat at a large dining table with no cloth on, and the French windows opening on to the lawn, let in a flood of sunlight, so that the sitter's hands and feet could be perfectly well seen. A scraping sound was soon heard, then raps, sometimes on the table, sometimes on the backs of our chairs. Florrie's hands and feet were closely watched, they were absolutely motionless when the sounds, which rapidly grew in loudness, were heard . . . occasionally changing to a

loud rhythmic scraping, as if the bow of a 'cello were drawn on a piece of wood. Again and again I placed my ear on the very spot whence this rough fiddling appeared to proceed and felt distinctly the rhythmic vibration going on in the table, but no tangible cause was visible either above or below the table.

Doubts have been suggested as to the possibility of localising sounds; with some kinds of sounds this is difficult, but direct experiments which I made for this purpose showed that when blindfolded most people can pretty accurately locate the position of sounds such as I heard on this occasion . .. On one occasion I asked for the raps to come on a small table near me, which Florrie was not touching, they did so; I then placed one of my hands on the upper and the other on the under surface of the table, and in this position I felt the slight jarring made by the raps on the part of the table enclosed between my hands. It made no difference whether Florrie and I were alone in the room, as was often the case, or other observers were called in. This latter was done occasionally when the raps were going on, to test my hallucination theory, but everyone heard the sounds.

The alphabet was slowly repeated and questions were answered by the unseen intelligence giving a rap when the right letter was arrived at. In this way we were told the communicator was a lad named 'Walter Hussey', ... I took down some of the answers obtained by means of the alphabet, they were just such as the child herself would have given, merry and meaningless . . .. To test a favourite anatomical theory that the raps were due to a trick which the medium might have acquired of slipping the toe or knee joints partially in and out with a click, I asked Florrie to put her hands flat against the wall and to see whether, when I did the same, she could stretch out her feet away from the wall as far as I could, pretending it was a new game between us. When we were both in this strained position, and any muscular movements of the limbs impossible, I asked 'Walter' if he was amused at our game; instantly a brisk pattering of raps came in the room, the child's hands and feet being absolutely motionless, while no one but Florrie and myself were present in the room (Barrett, 1919, p.38-42).

In my own experience the best example of an experimental investigation of RSPK occurred in 1968 when Pratt and I (Roll & Pratt, 1971) made a study of RSPK occurrences in a novelty warehouse in Miami Florida. Before we came, police officers and others had noticed that glasses, beermugs, ashtrays as well as cartons of merchandise were more likely to move if placed on certain shelves in the stockroom than on others. This room contained three

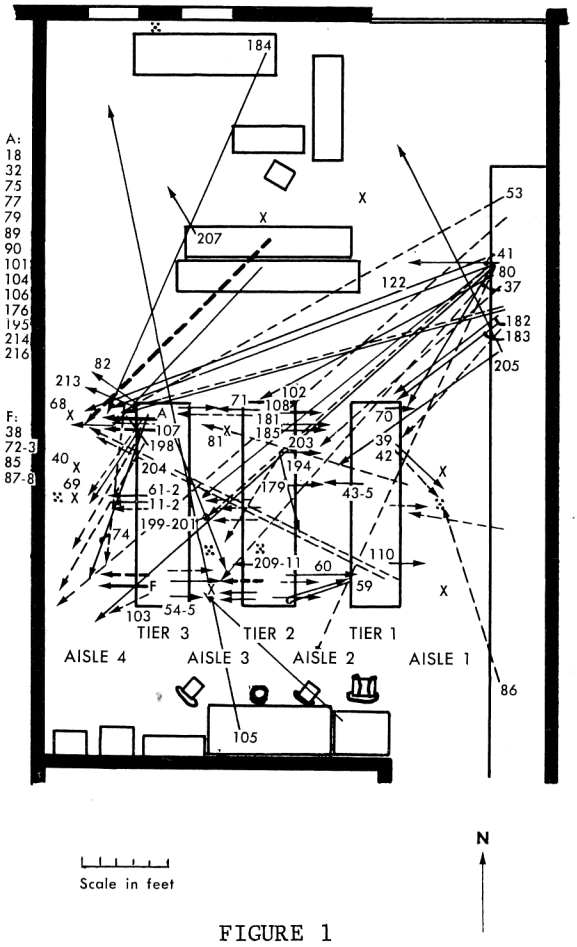

FIGURE 1

Floor plan of the stock room at the Florida novelty wholesale business where most of the occurrences took place. The numbers refer to the incidents and the arrows give the approximate direction and distance of motion. The solid arrows refer to events which could not have been caused normally according to witnesses who were present and the broken arrows to unwitnessed events. The crosses indicate places where objects landed whose origin was not known. The heavy lines show movements of three or more objects, as indicated by the letters and numbers. The office area and front entrance are south of the stockroom. The back entrance is at the north end.

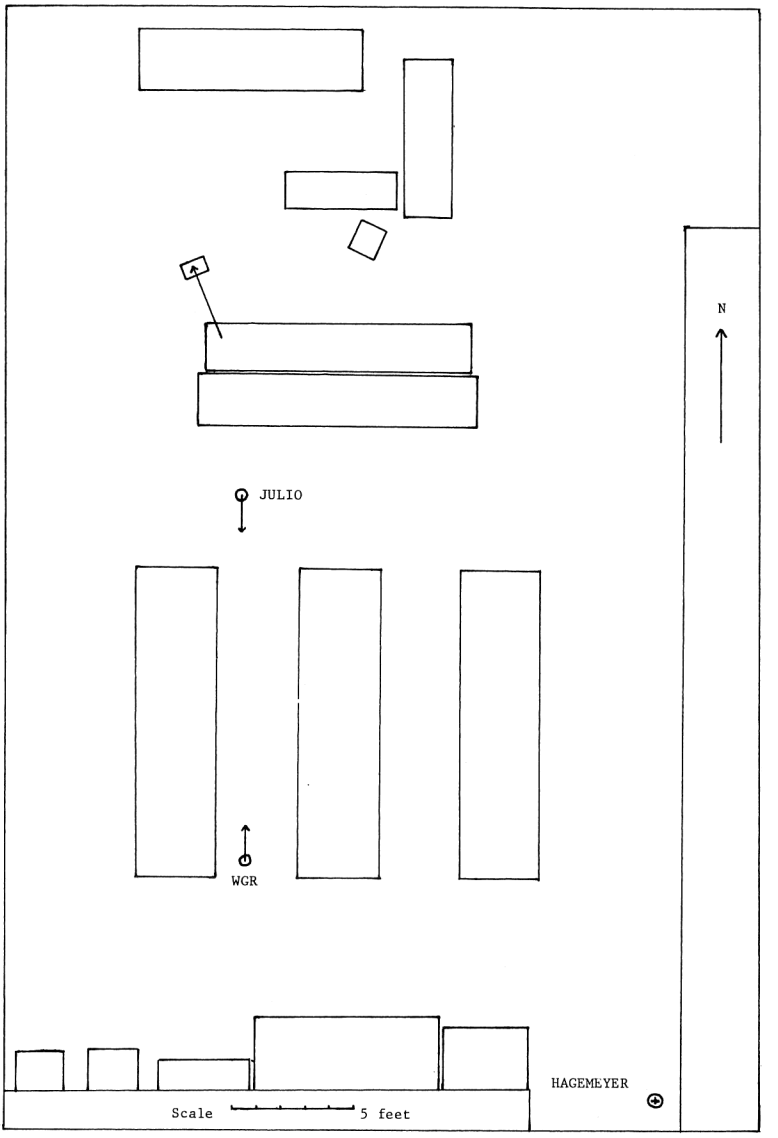

FIGURE 2

Event 207: The movement of a box of beermugs from the shipping desk to the floor. The circles with arrows indicate the positions of Julio and W.G.R.. It was not known which direction Mr. Hagemeyer was facing at the time of the incident.

freestanding wooden tiers of shelves and one along the wall, as well as desks and shipping tables. Some of the shelves and tables were particularly prone to be the scene of a disturbance, in other words, there was area focussing (see figure 1).

We chose the most active places in the room as target areas and placed on them some of the kinds of objects which had moved such as beermugs and glasses. This made it possible to make a careful examination of the shelves for stability, faulty construction, and magical devices before the incidents took place. The objects we chose as targets and placed in these areas were similarly examined. After the areas had been prepared we kept them under surveillance so that we could be certain that no one other than ourselves had entered the area prior to an event.

The focal person for the occurrences was Julio, a 19-year-old shipping clerk, and he was nearly always in the room when something happened.

There was particular strong focussing on beer mugs, amber colored glasses with wooden handles fastened by metal bands. On Tuesday, January 21, the poltergeist had exhausted the stock of beer mugs. When a new shipment came in, we put out a carton with ten glasses, chosing one of the shipping desks where there had been area focussing as a target area. At 2:40 p.m., the box fell to the floor two to three feet northwest of the shipping desk, the glasses spilling to the floor and three breaking. Julio was north of Aisle 3, five feet from the box's original position, and walking south away from it. I was in the south end of Aisle 3, 11-12 feet from Julio and looking directly at him. The only other person in the room, Mr. Hagemeyer, another shipping clerk, was in the southeast corner of the room behind me (Roll & Pratt, 1971, p.447-448).

This is one of ten target objects which moved from an experimental area when we a) had previously examined the object, b) had previously examined the target area, c) had the area under surveillance prior to the event, d) had the area under surveillance during the event, and e) examined the target object and area after the event. It is also one of seven incidents when Pratt or I had Julio in direct view (see table 9).

In addition to enabling the investigator to control the situation, the experimental approach to RSPK enables him to obtain more detailed information than otherwise is possible. Since we not only knew the place the objects landed but also their starting position, as well as Julio's location in the room, we could plot the exact distance and direction the object moved on our chart in relation to Julio's location (see figure 2). As I shall show in the second report, such data may provide important information about the RSPK

TABLE 9

Ten events involving target objects when investigators had target area under observation

Event number	Object	Area placed in	Area moved to	Object placed and/or examined by	Target area supervised by	Julio's distance from object in feet	Other employees or owners present	Comments
176	Alligator ashtray	T3	A4	W.G.R.	W.G.R.	4	C.Hagemeyer I.Roldan B.Rambisz	W.G.R. had Julio in direct view
181	Spoondrip tray	T2	A2	J.G.P. W.G.R.	J.G.P.	8	A.Laubheim and possibly others	J.G.P. had Julio in direct view (arms and lower torso)
185	Zombie glass	T2(b)	A2	J.G.P. W.G.R.	J.G.P.	5	C.Hagemeyer	J.G.P. and W.G.R. had Julio in direct view
194	Zombie glass	T2(b)	A2	J.G.P. W.G.R.	J.G.P.		C.Hagemeyer	
195	Alligator ashtray	T3(a)	A4	J.G.P. W.G.R.	J.G.P.		C.Hagemeyer	
198	Tab bottle	T3(a)	A4	W.G.R.	W.G.R.	7	C.Hagemeyer	
203	Zombie glass	T2(b)	A2	W.G.R.	W.G.R.	4	C.Hagemeyer I.Roldan	W.G.R. had Julio in direct view
207	Box beer mugs	Shipping desk	Floor	W.G.R.	W.G.R.	5	C.Hagemeyer	W.G.R. had Julio in direct view
214	Iron beer bottle	T3(a)	A4	W.G.R.	W.G.R.	8	C.Hagemeyer A.Laubheim P.Wolfe,R.May	W.G.R. had Julio in direct view
216	Beer mug	T3(a)	A4	W.G.R.	W.G.R.	4		idem

process.

FRAUD

Sometimes controlled research leads to uncovery of fraud. In our investigation in 1961 of the Newark disturbances (Roll, 1969) there was strong circumstantial evidence of fraud for the later incidents - but no clear proof. The phenomena continued unabated even when Arnold, the 12-year-old focus for the events was with his grandmother at the Duke Parapsychology Laboratory - but always out of our sight. We therefore placed Arnold and his grandmother in a room which had a one-way screen. We put out some objects, which might tempt the poltergeist, on a table while Pratt settled down in the adjacent room next to the one-way window. Soon he saw the boy hide the objects beneath his shirt and then later throw them.

In 19 (16%) of the 116 cases one or more incidents were discovered where the focal person, or in one case a relative, produced one or more events by trickery (see table 10). In four of these the person experienced dissociated states and it seemed likely that the events were done in such a state, but in others the trickery was probably done consciously.

Cox (1961) draws a distinction between "imitative" and "total" fraud. In imitative fraud the person copies the genuine RSPK phenomena by trickery while cases of total fraud are entirely made up of deception. Cases of total fraud have presumably been sifted from the present collection, but some instructive examples are presented by Owen (1964, p.27-87).

If we look at fraud incidents over the four periods, there is a significant increase in the detection of fraud in the last period ($p < .05$, two-tailed). I believe this is due to increased scrutiny of RSPK cases in recent years and the increasing attention paid to the focal person. In early times, if events took place which clearly were not caused by anyone living, they were usually presumed to be due to devils or spirits, and the observers might ignore as irrelevant instances where the maid or child, who was usually present, caused some of them normally.

The increase in the reports of fraud may not be entirely due to better observation and reporting. It sometimes seems that the visitor or investigator who wants 'something to happen' may help to bring this about. Young children may be more susceptible to this kind of influence than older people and may generally be less inhibited in adding to the commotion. The average age for the focal persons who were discovered in trickery is 12.9 and the median age is 12. As we shall see in the next paper these are lower than the

TABLE 10

Cases were fraud was detected

Years	Total number	Number	Percentage
1612-1849	19	2	10.5
1850-1899	25	3	12
1900-1949	38	3	8
1950-1974	34	11	32&
Totals	116	19	16

Note: The average age of focal persons detected of fraud is 12.9 and the median is 12.
&: The increase in the detection of fraud in this period is significant at p .05.

average (16 years) and median (14 years) for the total sample.

If there are instances of fraud in a case where other events have been witnessed under good conditions, this should not cause the researcher to dismiss a case. As we shall later see, RSPK occurrences seem to be due to psychological or physiological tensions and not to any desire to provide scientific proof of psychical phenomena. When the events are simulated, they should be studied together with the apparently genuine events for any light they may shed on the psychological features of the case.

Fraud incidents emphasize that RSPK occurrences are person oriented. They also emphasize that the most plausible normal explanation for ostensible RSPK events is deception by the person around whom they occur. The main effort of poltergeist researchers in determining whether a case includes genuine RSPK effect consists in setting up controlled conditions which will exclude normal interference by the focal person or anyone else in the group among whom the disturbances occur.

INSTRUMENTING THE POLTERGEIST

In a recent poltergeist investigation in the Boston region, Gerald Solfvin attached a telemetry EEG unit to the apparent agent, a boy about ten years old. In this arrangement, the scalp electrodes feed

into a transmitter about the size of a packet of cigarettes which is worn on the belt or in a pocket. This unit, designed by Dr. Fritz Klein of the Duke Department of Anesthesiology, transmits to a standard FM radio receiver somewhere in the house which is plugged into a stereo tape recorder. One side takes two EEG channels (one from each hemisphere) and the other picks up sound activity in the room from a conventional microphone. This makes it possible to record a person's brain waves while he or she is freely moving about. It was out purpose to register the boy's brain waves during RSPK incidents but Solfvin discovered another application: by turning on the volume of the radio, he could hear changes in the EEG frequency, in particular he could hear when the boy was moving, a shrill whine produced by muscle artefacts. On one occasion there was a series of apparent poltergeist knocks at the same time as the tell-tale whine was heard. Solfvin thought that the boy was producing the knocks normally and placed himself in a position where he could witness the next incident unobserved, and saw the boy stamp his foot on the floor.

Film and video equipment can also serve two purposes: to tell whether fraud is involved and, if it is not, to help fill in the picture of the poltergeist. In particular film or video may reveal pecularities in trajectories of moving objects. If fraud is likely or if one is dealing with an observer-shy poltergeist, it is important to make the cameras as inconspicious as possible. Compact and silent instruments are expensive and the cost increases because they must be capable of operating unattended for several hours at a stretch. So far this type of recording has not figured prominently in poltergeist studies. In one case only were we able to survey a large part of a poltergeist home by rented cameras (Mathews & Solfvin, 1976), and then only after the incidents had subsided.

My first attempt at an instrumental study was in 1965 in Detroit, Michigan. An informant, John Buta, in that city had visited a poltergeist home were apparitions were among the phenomena reported. Mr. Buta had himself reported a misty shape near a wall and said that when he placed his hands over it, the hairs stood up. Since this might result from an electrostatic field, I borrowed a voltmeter from the Duke Electrical Engineering Department for my visit. On my second day in the home, the mother and teenage daughter said they saw the gost and I placed the two leads on the place. The voltmeter did not register anything. I had my doubts that this crude instrument, which could only register above 600 volts, would respond to the apparition, even if it somehow existed. My faith in the substance of this vision was further diminished when I interviewed the two people and learnt that the ghost(s) they saw was quite dissimilar. The remark by the mother that "whiskey makes it worse",

rounded out the picture of the spirit.

William Joines (1974) had better success in a RSPK case we were investigating in the Bronx, New York City, in 1971. On the theory that the events were associated with electromagnetic radiation, he explored the regions near two objects which had just moved with a common FM radio receiver. He detected "a spherical region of space which apparently emitted radiation at a frequency of 146 MHz . . .. The region was about two feet in diameter, which is consistent with the 146 MHz frequency, and detectable transmissions persisted for about one minute" (Joines, 1974, p.149. I shall discuss the wave theory for RSPK in the second paper).

To see the possibilities of instrumental RSPK, we must also go to Germany. Hans Bender and his associates have used electronic devices for a long time: At Neusatz in 1951 (Bender, 1969, p.88-89) the poltergeist seemed to retreat in face of the electronic gear. But in Nicklheim, 1968-9 Bender (1969, p.95-99) had at least partial success: An object, which had been involved in a previous incident, fell again after it had been placed in a box protected by an electric curtain. His most interesting study was done in December, 1968. That month Bender (1969, p.93-95) brought in two physicists to determine the nature of the energy which caused the electrical equipment in a lawyer's office in Rosenheim to malfunction in strange ways. Lamps 'exploded', fuses blew though the current was stable, and the phone company registered calls no one had made. The occurrences were connected with a 19-year-old secretary and included conventional poltergeist movements of objects and furniture. The physicists, K. Karger and G. Zicha reported that "on December 8, 1967, the line recorder of the power station was fitted with a voltage magnifier and set up in the passageway of the lawyer's office to record the main voltage. Between 4:30 p.m. and 5:48 p.m., the recorder registered about 15 strong deflections at irregular intervals. At about the same time we heard loud bangs similar to those produced by discharging spark gaps, but not for every deflection. The noises were tape-recorded". The physicists ruled out deception as well as known physical forces and attributed the incidents to "mechanical influence without apparent cause" (Karger & Zicha, 1968, p.33-34). The investigators also succeeded in filming swinging ceiling lamps and wall pictures.

American investigators have had visions of mobile electronic laboratories (Tart, 1965; Artley & Joines, 1969; Joines, Owen, Roll, and Bender, 1970), but so far little has materialized. Tape recorders and conventional cameras are common. RSPK sounds have been recorded (Pratt & Palmer, 1976) and a swinging lamp filmed (Rosenberg, 1974). More sophisticated equipment has been beyond our resources.

CONCLUDING REMARKS

In this survey I have looked for features in poltergeist occurrences which may help in the design of research. In a later paper I shall explore the patterns for pointers to the nature of RSPK.

The cases span four centuries and 17 countries but are mostly confined to the West. They have a basic similarity consisting of spontaneous and repetitive physical occurrences, usually movements of objects or percussive sounds. Over and above this similarity there is variability which may be due to the psychological, social, and other circumstances of the focal person.

In the present examination of RSPK cases it is necessary to distinguish between two questions: Firstly, what are the characteristics of the poltergeist, as shown by our sample, which have a bearing on RSPK research strategy? Secondly, are these characteristics typical of the poltergeist in general? This paper has in the main been directed to an answer of the first question. We cannot extrapolate an answer to the second question without qualifications.

The 116 cases all appeared in print. That is, they convinced not only the primary witnesses but also editors of professional publications that RSPK phenomena had probably taken place. We cannot go on from there and predict that the features shown by these cases will occur as strongly in a random sample of RSPK: The decision to publish a case largely depends on the creditability of the witnesses to the events and of the writer who reports them. This may have slanted the sample towards features allowing for convincing observation.

Though the sample is restricted to published cases, there are differences over the four periods which allow us to make some conjectures regarding the second question.

There was an increase in the number of professional observers (37%, 64%, 66% and 73.5%) and particularly of parapsychologists (0%, 8%, 24% and 41%) over the four periods. If the presence of observers and more modern methods of observations are associated with patterns in the data which make for poor conditions of observation, the answer to our second question is likely to be that the features of the poltergeist found in this study may not hold true in general.

We discussed two observer effects: the effect of the presence of observers on the number and severity of the incidents and the effect of direct visual observation on the beginning of movements. For the first there was a slight increase in inhibitory tendency over the four periods (0%, 8%, 0%, and 13%) though the cases where there was no such tendency or an enhancement effect were always substantially

higher (94%, 83%, 91% and 77%). For the second observer effect, the inhibitory effect stayed about the same (46%, 52%, 37.5% and 46%). The focussing effect also stayed about the same (89%, 84%, 100% and 91%). There was a reduction in successful RSPK tests (53%, 32%, 45% and 32%) in the last period as compared with the first but this does not tell us much because the figures (except for two cases) cover all reported attempts at testing. There was one definite difference between the four periods: the increase in cases were fraud was discovered (10.5%, 12%, 8% and 32%). It is tempting to suggest that with increased control there would be an increase in the discovery of fraudulent incidents to the point that all or most of the ostensible RSPK occurrences would evaporate. The fact that young children were more often caught than older ones might be because the latter were too clever for the investigators. The only satisfactory way to answer this question is to study the individual incidents: the fraud hypothesis as a general explanation of the present cases is difficult to reconcile with the reports. This theory also seems to be inconsistent with the repetitive or focussing aspect of RSPK. "Do it again" we tell the magician, knowing that this is the best way of discovering the trick. For all the drama of poltergeist incidents, an element of what seems like simple minded perseveration runs through the data. The focussing on special objects or area does not make sense for someone trying to perpetrate fraud because the attention of the investigator is naturally directed to that object or area and the element of surprise so important for a successful magical performance is reduced correspondingly. When we further see that even in the period with the fewest number of RSPK tests, nearly one third included such tests - usually based on the focussing principle - we must conclude that the poltergeist offers excellent prospects for controlled research.

At this time in the history of parapsychology, it is not enough that the phenomena we study provide convincing evidence for psi. They must also offer good prospects for increasing our understanding of the nature of the beast. In a later paper, I shall explore the poltergeist from this point of view.

CORTICAL AROUSAL AND PSI: A PHARMACOLOGICAL STUDY

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Research into personality correlates of psi functioning has been in decline in recent years as a survey of leading journals will show. One would find it hard to disagree with K.R. Rao when he says that one reason for this may well be the fact that "attempts to explore the relationship between certain personality factors and ESP have rarely proceeded in a systematic and comprehensive fashion" (Rao, 1974, p.62). Much of the literature on psi and personality consists of isolated reports of a correlation between psi test scores and personality factor X from questionnaire Y with no attempt to place this finding in any sort of context, no attempt to integrate the finding with what may be known about the role of this personality factor in areas of perception, and no attempt to explore the boundary conditions of the relation. The experiment reported in this paper was designed to test a psi/personality relation with a strong theoretical/functional basis for experiment.

Eysenck's cortical-arousal theory of extraversion runs briefly as follows (for a fuller statement, see Eysenck, 1953). Extraverts are differentiated from introverts by having a tonically lower level of cortical arousal produced by their having higher thresholds in the elements of the ascending reticular activating system responsible for relaying certain afferent inputs to the cerebral cortex. Eysenck argues (Eysenck, 1967) from spontaneous evidence that low arousal-states are psi-optimal and that, therefore, extraverts should be superior to introverts on psi tests. Eysenck reviewed the relevant experimental evidence in his 1967 paper and argued that this was generally the case. In fact, from the experimental evidence Eysenck's contention is generally very strongly supported. We need to introduce here the distinction between evidence inconsistent with a qualitative theory and evidence incompatible with it: whilst there is quite a lot of evidence inconsistent with Eysenck's theory (i.e. papers which report no relationship between psi test performance and extraversion) there

is very little contradictory evidence (i.e. papers which report a superiority of introverts in psi tests. One of the very few exceptions is Szczygielski & Schmeidler, 1975).

We will accept, then, that extraverts are generally accepted as being superior to introverts on psi tests. Now, Eysenck's theory is by no means the only way of explaining this finding. Rao argues that the difference may simply lie in a "differential reaction to the testing situation" (Rao, 1974). Another reason which will occur to the parapsychologist is that extraverts are simply more fun to test than introverts, in most cases. However, Eysenck's theory, with its strong physiological basis, makes certain predictions which - if verified - could hardly be explained in terms of 'differential reactions' or social reinforcement effects. The predictions his theory makes about drug effects are one such example.

Eysenck's theory predicts that drugs which increase cortical arousal level - like the amphetamines - should decrease psi scoring, whereas drugs which depress cortical arousal - like the barbiturates - should improve psi scoring. This is, of course, exactly the reverse of what these drugs do to 'orthodox' perceptual processes, and suggests some antithesis between psi and other sensory modalities.

Some work has, of course, been done with such drugs, although very little has been done in recent years (see McCreery, 1967). Rhine (1934) reported some work done with the outstanding Duke subjects of the thirties, using caffeine, amytal, and alcohol, but these studies - being essentially exploratory in nature - did not use double-blind procedures and therefore expectancies could have affected results (which were in contradiction to Eysenck's theory). Cadoret (1953) used amphetamine and found a decline in card-test psi scoring (but an incline in free-response psi) after administration of the drug, effects which were very closely paralelled by the effects of sodium amytal. The commonality of drug effects suggests some milieu effect affecting psi scoring. His study did use double-blind procedures.

Huby & Wilson (1961) carried out a large-scale study using a variety of stimulant and depressant drugs in order to investigate their effects on forced-choice psi scoring. In their first experiment, they found (like Cadoret) that amphetamine produced a decline in scoring although this did not even approach significance. They also found an intriguing placebo effect (lactose was the placebo) which they could not replicate. Huby & Wilson came to the conclusion that their results, overall, did not constitute a replication of Cadoret's findings. However, whilst their sample size was large, the number of trials per S was arguably too small (n=50 with p=.20).

It must be admitted that previous work with stimulant and depressant drugs has not yielded coherent evidence. This experiment aimed at correcting two factors in the Cadoret and Huby/Wilson studies which might have affected results. Huby and Wilson used too few trials - this experiment involved the collection of 18,000 calls from a single subject. Cadoret's data was marked by general tendencies to decline, which looks as though his subjects were, simply, becoming bored with proceedings, so this study used an S with a high degree of motivation - C.L.S..

GENERAL PLAN OF EXPERIMENT

Six experimental days comprised the duration of the experiment. On each of the six days, during a five-hour period (12.00 to 17.00 hrs), C.L.S. sat alone in his office (which is very quiet) and recorded 50 calls every 5 minutes, giving a total of 3000 guesses per day. The targets were the digits 1 through 5. The six experimental days were split into two blocks of three spaced seven weeks apart (in each case, a Monday, a Wednesday, and a Friday. In each block of three, a 'control' day, a 'placebo' day and a 'drug' day was included.

At 17.00 hours on each of the experimental days, C.L.S. would take his guess sheets and get them signed, dated, and witnessed by P.F. from the Department of Biochemistry or P.B. from the Department of Chemistry. The next day the targets would be given to C.L.S. and the data checked and scored.

TARGETS

The targets were computer-generated random numbers prepared by A.G. and A.D. at the Department of Psychology, University of Nottingham. They did not deviate significantly from an ideal random series in either frequency or sequential-dependency aspects. A.G. despatched them to J.M. at the Department of Psychology at this University, and J.M. randomly selected a segment of these targets to be passed on to C.L.S. the day after each batch of guesses had been recorded; these targets were signed and dated by J.M.. The psi test may thus be regarded as one of clairvoyance on the part of J.M. or of precognition or PK on the part of C.L.S.: it will be assumed that C.L.S. was the psi focus in this experiment since as results show scoring varied as a function of his psychological state.

DRUGS

The four capsules used in the experiment were not distinguishable by visual or gustatory senses. They were prepared by a research assistant of T.R. at the Department of Psychology at Cambridge. Two capsules contained an inactive placebo (lactose); a third contained metamphetamine, 10 mgms.; and a fourth contained dizepam (Valium), 10 mgms.. C.L.S.'s weight at the time of the first block of three experimental days was 86 kgms and at the time of the second block 84 kgms..

On the placebo and drug days T.R. would be given one of these capsules by his research assistant and would then administer it to C.L.S. at 14.30 hrs.; that is, half-way through the five-hour guessing period. He stayed until satisfied that C.L.S. had actually swallowed the capsule and then departed.

The order of conditions was (neither T.R. nor C.L.S. knew this until all data had been collected and the first three data-checks completed):

I. Control-Diazepam-Placebo
II. Control-Amphetamine-Placebo

It was considered best to do the control run first in each block of three experimental days.

PREDICTIONS

1 The post-drug scores will be significantly higher than the pre-drug scores in the case of diazepam administration;
2 The post-drug scores will be significantly lower than the pre-drug scores in the case of amphetamine administration;
3 There will be no significant differences in first-half and second-half scoring rates in the control and placebo conditions.

On advice from T.R. the data from the first 100 trials following drug administration were discarded since approximately 10 minutes is required for drug absorption. Therefore this data was discarded rather than halting guessing for ten minutes which might have disrupted Ss guessing rhythm.The data from the 100 guesses recorded immediately before drug administration was also discarded for the purpose of applying a matched t-test (Zenhausern, 1974) to the data. To make data from different sessions exactly comparable, this procedure was adopted for data from all six experimental days.

CHECKING OF RESULTS

The results were checked by C.L.S. with the help of P.F. three times

before the information about test order was given. The scores were then checked once more. With this procedure recording errors of a systematic nature should have been minimised.

RESULTS

The scoring rates for the various conditions are set out below.

TABLE 1

Summary of results

Condition	12.00 hrs.	14.40-17.00 hrs.
Control I	271	266
Diazepam	268	297
Placebo	263	270
Control II	290	272
Amphetamine	295	256
Placebo	263	279

Note: MCE=280 with σ=14.97

The fate of the predictions was:
1 Not confirmed. The effect is in the predicted direction but is not statistically significant (t_{27}=1.36, 0.1>p>0.05, one-tailed).
2 Confirmed (t_{27}=2.34, p<.014, one-tailed).
3 Confirmed; no inclines or declines in control or placebo sessions approach significance.

One point which might be raised is that the assumption is made that first-half (12.00-14.20) scores are genuinely indicative of the Ss psi ability at the time in question. On the other hand, it could be argued that those scores are only chance fluctuations and that if, say, the pre-drug scores for the amphetamine and dizepam conditions had - by chance - happened to be the other way round, then the results of the experiment would have been completely null. Several points may be noted with respect to this argument. Firstly, scrutiny of table 1 will show that C.L.S. was generally disposed to score more highly in the second experimental week than in the

first experimental week. Secondly, the post-drug scores for the amphetamine and diazepam conditions differ significantly ($t_{27}=1.95$, $p<.04$, one-tailed); this analysis was not planned because it seemed essential to compare psi performance after drug ingestion with a contemporaneous control, but it is a valid analysis if one argues that the first-half scores are only random fluctuations around a chance mean. Lastly, if the pre-drug score for the diazepam condition had been the 263 hits recorded in the placebo session, the drug would have produced an incline significant at the .05 level of significance. The question debated above is essentially one of the 'What if?' variety and not of any great interest or importance.

FURTHER COMMENTS AND INTROSPECTIONS

It was immediately obvious to me when I had been given amphetamine; I became very restless and had to pace up and down the room between psi tests, and was kept awake until 7.00 a.m. the next morning. On the other hand, I could not discriminate diazepam from placebo.

CONCLUSIONS

The results of this experiment provide some support for Eysenck's hypotheses about cortical-arousal and psi processes. His prediction about the effects of amphetamine on psi scoring is clearly confirmed. The prediction about barbiturate effects was not confirmed but given the low dosage (the dosage of 10 mgms. was decided upon because in the past C.L.S. has been found to be mildly allergic to diazepam) and the fact that the effect is clearly in the predicted direction the result obtained was not in contradiction to Eysenck's theory.

Finally, there has been much mention in the parapsychological literature of late (Kennedy & Tadonio, 1976; Millar & Broughton, 1976; Millar & Mackenzie, 1976; Parker, 1976; White, 1976) of 'experimenter psi effects' being in large part responsible for the differences between results obtained by 'successful' psi experimenters and 'unsuccessful' psi experimenters. I have been generally successful in my research (Sargent, 1976, in press, in preparation) and whilst I have reservations about the experimenter psi hypothesis (because it seems to be irrefutable) it may be of interest in this context that this study represents demonstration of lawful psi effects by a relatively successful psi experimenter.

ABSTRACT

The experiment reported tests predictions made by Eysenck's theory of cortical arousal and psi regarding the effects of stimulant and depressant drugs on psi test performance. Evidence for this theory is presented, alternative explanations of the results are briefly considered, and the results are considered in terms of the 'experimenter psi' hypothesis.

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FREE RESPONSE GESP PERFORMANCE FOLLOWING GANZFELD STIMULATION VS. INDUCED RELAXATION, WITH VERBALIZED VS. NONVERBALIZED MENTATION: A FAILURE TO REPLICATE

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In recent years, increasing consideration has been given to the reduction of mental and physical activation as a possible method of facilitating psi performance in a laboratory setting. General procedures used to induce mental and physical relaxation have been examined, yielding evidence suggesting that these may be more psi-conducive than states characterized by higher levels of activation. Two such activation-reducing procedures are: progressive muscle relaxation with mind stilling suggestions (used by Braud & Braud, 1974) and the ganzfeld technique for perceptual isolation (described by Honorton & Harper, 1974).

Ganzfeld stimulation and induced relaxation are similar procedures in that both techniques are associated with reduced exteroceptive (sensory/perceptual) stimulation and reduced somatic stimulation (decreased body awareness). Both are associated with a withdrawal of attention from external sensory and somatic stimuli and a concomitant shift toward internal processes (images, feelings, thoughts). Both appear to be conducive to psi retrieval, perhaps because the attentional shift attenuates psi-irrelevant sensory, perceptual, and somatic 'noise' which may ordinarily interfere with weak psi 'signals'. Honorton (1977) has summarized the overall success rates of psi studies using ganzfeld or relaxation-inducing techniques. In his review of 16 experimental psi studies using ganzfeld stimulation, Honorton cites eight as obtaining significant overall levels of accuracy; the combined probability of all 16 studies was 2.1×10^{-9}. In reviewing experimental studies of psi during induced relaxation, Honorton cites 10 out of 13 studies which provided significant evidence for psi; the combined probability of all 13 studies was 1.2×10^{-9}.

While it appears that ganzfeld stimulation and induced relaxation both may be psi conducive, at the same time, both techniques contain procedural differences that could possibly yield differences in psi performance. In the experiment reported here, we explored the possibility of one activation-reducing technique being more conducive than the other.

In previous psi experiments involving ganzfeld stimulation, we had asked our subjects to give us continuous verbal reports describing their mental imagery. A number of our subjects who reported their imagery complained that talking out loud would disrupt or prevent imagery. Therefore, we entertained the hypothesis that allowing subjects to merely observe ongoing images without having to talk about them might eliminate the disruption of imagery and, hence, facilitate retrieval of any psi information mediated by imagery. Additionally, we hypothesized that eliminating the need to verbally describe imagery, to translate more 'right hemispheric' material into words and thereby activate more 'left hemispheric' processes, would also eliminate another source of psi-interfering noise (Braud, 1975; Braud, 1977), again facilitating psi. Thus, we felt it useful to compare the psi performance of persons giving continuous verbal reports of imagery with that of persons for whom this verbal translation and expression was not required. We report this experiment as an extension of previous research to determine an optional procedure for retrieving psi in a laboratory setting.

METHOD

Subjects

Twenty-four persons each participated in all four of the conditions in the experiment, i.e., a with-in subjects design was used. The subjects were unpaid volunteers who expressed an interest in the experiment during visits to the laboratory after learning about the Foundation through local newspaper advertisements and articles, notices posted throughout the city, lectures given by the Foundation staff at local colleges and universities, and comments from other persons who had participated in earlier experiments. Their motivation was evidenced by their willingness to commit themselves to at least four visits to the lab.. The subjects ranged in age from 19 to 65 years, with a mean age of 34 years. Eleven of the subjects were male and thirteen were female.

Procedure

Before becoming involved in the experiment, each subject was given a tour of the Foundation's facilities and the various ongoing experiments were described. If someone expressed an interest in the present study, the time requirements were explained and a testing schedule was arranged. When the subjects arrived at the laboratory for the first session, they signed an Informed Consent Form and completed a Subject Information Questionnaire.

The experiment was conducted in two rooms of the suite of rooms that comprises Mind Science Foundation. One room housed the percipient's reclining chair, a lamp containing two 15-watt flourescent bulbs covered by a blue filter and positioned 12 inches directly above the subject's eyes for ganzfeld stimulation, headphones for auditory ganzfeld and relaxation-inducing tape, and an intercom. Another room, 70 feet and two doors away, housed the agent's reclining chair, a slide projector, the 1,024 slides of the binary target pool, a tape recorder, a 'pink noise' generator, a six-channel audio mixer, and an intercom.

Subjects were tested individually. At the beginning of the experiment, they were randomly assigned different sequences of the four experimental conditions. Twenty-four combinations of the four conditions were obtained to counter-balance for any possible sequence or order effects.

The subjects were already familiar with the test environment when instructions for the experimental condition were read aloud to them by the experimenter-agent (R.W.) at the beginning of each session. The instructions to the subjects have been reported and may be found in the Appendix. After presentation of the instructions, the subjects then participated in one of the four experimental conditions described below.

Ganzfeld (verbalization and nonverbalization)

The percipient was seated in a reclining chair in a room remote from the experimenter-agent's room. The agent prepared the subject for ganzfeld stimulation by taping halved ping pong balls over his eyes and adjusting the halves so that homogeneous illumination was visible when the subject viewed the lamp. The subject was instructed to blink normally, but to keep his eyes otherwise open. He was then asked to relax and look into the uniform blue field. Loud 'pink noise' was provided through headphones. The subject adjusted the volume of the homogeneous noise so that it was quite loud, but not so intense as to be unpleasant.

The agent then left the subject, closed the door behind him and returned to the agent's room to randomly select a target slide for a 5-minute GESP impression period. The visual-acoustic ganzfeld was maintained for thirty-five minutes with the GESP task reserved for the last five minutes. The GESP period was signaled by the pre-recorded sound of bells filtering through the 'pink noise'. The termination of the ganzfeld session was also signaled by the sound of bells.

For the ganzfeld verbalization condition, the subject was instructed to speak about anything he thought of, imagined, felt or sensed. Subjects were also instructed not to cling or hold on to their thoughts, but to observe and verbally report them as they happened. They were told to put everything into words. For the ganzfeld non-verbalization condition, the subject was instructed to observe internal mental processes and to passively observe all thoughts, images, and feelings without clinging to them. Emphasis was put on observing thoughts without putting them into words, with the added suggestion that mentation would be remembered well.

Relaxation (verbalization and nonverbalization)

The percipient was seated in a reclining chair in the room remote from the experimenter-agent's room. The agent, having previously read instructions to the subject, fitted the set of headphones comfortably on him, turned out the lights, left the room and closed the door. The agent then went to his room to start the tape recorder and randomly select a target slide. The tape recorder played thirty minutes of instructions the subject was to follow in order to relax his entire musculature using an abbreviated version of Jacobson's progressive relaxation procedure very similar to that used by Braud and Braud (1973). This procedure involved alternately tensing and then relaxing each part of the body in turn until a profound state of muscular relaxation was produced. The subject then attempted to mentally relax, at first employing imagery of pleasant and relaxing scenes, then blanking his mind and becoming as passive as possible (see Appendix). At the end of the relaxation and mind-blanking procedure, the tape signalled the percipient that it was time to begin the 5-minute GESP part of the session. The subject already knew that the agent would 'send' impressions of a target slide and was to pay close attention to any mental impressions with the suggestion that the impression would be remembered. For the relaxation nonverbalization condition, subjects were instructed to passively observe all thoughts, images and feelings without putting them into words. In the relaxation verbalization condition, the

subjects were required to describe their mentation verbally, to put everything in words and to keep talking. The tape recording signaled the end of the 5-minute GESP impression period in both conditions.

Having gone to the agent's room after preparing the subject for testing, the experimenter randomly selected the target slide from the binary target pool. The agent used card cuts and random number tables to choose one slide out of 1,024. Without yet looking to see what the target slide was, the agent inserted it into a slide projector and then waited throughout the 30-minute period of relaxation and ganzfeld stimulation. When the tape recording, monitored by the agent, signaled the subject that it was time to begin the GESP part of the session, the agent turned on the slide projector and viewed the target picture, attempting to send psi impressions of the target to the subject. The agent's method of sending was to observe the target in a normal waking state of consciousness while alternately focussing attention mentally on the picture as a whole, and then as individual parts, all the while with the intent of transmitting psi information. At the end of the 5-minute impression period, the agent stopped trying to send psi impressions, turned off the slide projector lamp, allowed the projector's fan to 'cool off' the target slide, and randomly selected three other slides without viewing their contents. Recording the target slide number, the agent then put all four slides in ascending numerical order in an envelope, walked down the hall to the subject's room and quietly slipped the envelope under the door without sensorily encountering the subject.

At the termination of the psi impression period, the subject self-terminated the session's experimental condition, recorded his impression's on paper and completed a binary coding questionnaire used with the binary target system developed by Honorton (1975). The subject had been instructed to refer to 'The binary coding of pictorial material: Coding manual' (Harper, 1975) if any questions arose pertaining to the coding of the impressions on the questionnaire. Finally, the subject retrieved the envelope containing the four slides and rank-ordened them in terms of their likelihood of being the correct target. He assigned a rating of 1 to the slide which corresponded most to his impressions, and a rating of 4 to the one that corresponded least. The remaining two ranks were given to the intermediate correspondences. No ties or omissions were allowed. His ranking completed, the subject went to the agent's room where he was given feedback about his performance and any other information about the experiment he wished to know.

RESULTS

Three psi measures were used for statistical analysis: (a) 'hits' (target rankings of 1 or 2), (b) 'direct hits' (target rankings of 1), and (c) the scoring of the number of correct binary target coding correspondences as described by Honorton (1975). By the definition of hits and misses there were 46 hits and 50 misses for all four conditions combined, yielding a CR of -.41. Using hits and misses for each condition separately yielded a CR of -.41 for ganzfeld verbalization, a CR of -.82 for ganzfeld nonverbalization, a CR of .41 for relaxation verbalization, and a CR of 0 for relaxation nonverbalization. Thus no significant psi was obtained in the experiment as a whole or in any one condition.

Comparisons between combined relaxation versus combined ganzfeld conditions as well as comparisons between combined verbalization versus combined nonverbalization conditions (using Wilcoxon's matched-pairs signed-ranks tests on hits and misses) yielded no significant differences. These analyses were repeated using direct hits and binary coding scores, yielding similar non-significant results. Table 1 contains data for all three measures for all four conditions.

To check for a possible decline effect caused by experimenter fatigue, the subjects were divided into groups based upon whether they began to participate in the experiment during its first or last half. Comparing the first session scores of individuals whom R.W. tested first with those individuals R.W. tested last, by means of Mann-Whitney U tests, revealed no significant difference in performance.

DISCUSSION

Since no significant psi hitting occurred in the experiment, we were unable to replicate and extend the generality of earlier findings that activation-reducing techniques facilitate receptive psi performance. Our results suggest that ganzfeld stimulation and induced relaxation are not sufficient conditions for the reliable demonstration of receptive psi. Perhaps some unknown crucial ingredient in the recipe for a psi-conducive procedure was absent in this particular study. Our analysis of this experiment suggests that increased attention be paid to psychological factors and experimental design considerations in an effort to isolate other possible variables effecting laboratory studies.

It is possible that psi was present in the experiment but that

TABLE 1

Summary statistics for the four conditions, for the three psi measures

Condition	Relaxation verbalization			Relaxation nonverbalization		
Measure	Target ranking hits	Target ranking direct hits	Binary coding scores	Target ranking hits	Target ranking direct hits	Binary coding scores
Overall mean	2.38	2.38	4.42	2.38	2.38	5.04
Frequency	13	8		12	8	
Condition	Ganzfeld verbalization			Ganzfeld nonverbalization		
Measure	Target ranking hits	Target ranking direct hits	Binary coding scores	Target ranking hits	Target ranking direct hits	Binary coding scores
Overall mean	2.63	2.63	5.00	2.67	2.67	5.00
Frequency	11	6		10	4	

the scores were brought down to the chance levels of performance via an experimenter effect. To understand better how and in what direction this experimenter effect may have operated, it is necessary to record here the experimenter's expectations. The principal experimenter, (R.W.), had participated as agent for an earlier psi-ganzfeld study (Braud, Wood & Braud, 1975) which yielded high psi scores. His mood and expectations at that time were characterized by a high degree of enthusiasm and involvement, not only with the experiment itself (which was a new and somewhat exciting experience for him), but with the subjects as well, since many of them were classmates and personal friends. At the beginning of the present experiment, R.W. was confident that psi would automatically manifest itself in the experimental conditions as it had in the earlier study. This thought, as well as other factors such as daily dull routine, some room-scheduling conflicts, long hours and poor subject turn out helped to decrease the experimenter's enthusiasm. Many of the subjects, most of whom were strangers to the experimenter at the beginning of the experiment, traveled many miles to participate in each of the four sessions. The inconveniences to the subjects, many of whom were slow in completing the sessions, might have been partly responsible for their lowered enthusiasm and may have caused some guilt feelings for the experimenter. These factors may have combined to help suppress the scores.

The experimenter expected above-chance performance in all four conditions and that the nonverbalization relaxation condition would yield the most significant result. This expectation might have unconsciously motivated the experimenter to psychically suppress scores in the other conditions. Overall, mental conflict concerning his expectations may have helped foster an attitude of non-involvement on the part of the experimenter toward the experiment which was made evident in the scores.

The verbalization versus nonverbalization portion of this study was suggested by the comments of some of the subjects of our previous experiments that talking about images as they occurred seemed to disrupt the images. It is likely that there exist large individual differences among subjects in terms of whether and to what extent imagery is disruptable by concurrent verbalization. In fact, concurrent verbalization may even potentiate imagery in certain persons. It is possible that approximately equal numbers of subjects preferred the verbalization and the nonverbalization conditions, with a resultant cancellation of a consistent, overall effect favoring either condition.

A final (and speculative) explanation for the absence of psi

hitting in the experiment as a whole depends on the validity of a hypothesis which the last author (W.B.) has recently begun to entertain. This hypothesis (which has been informally labeled the 'spreading thin' hypothesis) is still in its earliest stage of development and will not be completely elaborated until a number of experimental tests of its implications have been completed. Some aspects of the hypothesis, however, may be mentioned here briefly, since they bear on the negative findings reported above. The hypothesis suggests that for a given person, for a given time period, there exists only a definite 'quantity' of psi. If some of this quantity is 'expended' in a certain place, at a certain time, to create a certain effect, then it is no longer available to be expended elsewhere. This hypothesis has certain implication for the distribution of psi effects in experimental designs. A simple design, such as a 'demonstration' study with only one condition, would be expected to yield a large psi effect since such a design includes only one 'opportunity' for the manifestation of psi. With increasing complexity of the experimental design, it becomes increasingly likely that the available quantity will become distributed throughout the various conditions, with a consequent 'watering down' of psi effects in the separate conditions. In the present experiment (considering only one psi measure), there were ninety-six 'opportunities' for psi to be manifested - perhaps a sufficient number to allow the psi quantity to be 'spread so thin' that it was indeed no longer evident. We offer this hypothesis as a possible explanation of failures to observe strong psi effects in complex, multiple-condition experiments such as the one reported herein. The validity of the hypothesis will ultimately have to be assessed by means of careful inspections of psi distribution effects in existant studies, as well as the results of new experiments in which complexity is directly manipulated. The effect, if real, would be expected to occur not only within subjects (as in the type of design employed in the present study) but also within experimenters (in between-subjects design) as well as other persons involved in a psi experiment. Investigations of such phenomena as decline effects and differential effects may yield data which are especially relevant to the 'spreading thin' hypothesis.

ABSTRACT

The present report describes an attempt to replicate and extend previous research on the psi-facilitating effect of ganzfeld stimulation and induced relaxation. The relative effectiveness of four experimental techniques was directly compared through the use of a within subjects design. The four conditions were: (a) ganzfeld-

verbalization, in which the subject continuously verbally described ongoing mentation while receiving uniform visual and acoustic stimulation, (b) ganzfeld-nonverbalization, in which the subject merely observed and remembered ongoing mentation without attempting to put anything into words, (c) relaxation-verbalization, in which the subjects verbalized mentation after following progressive muscular relaxation exercises, and (d) relaxation-nonverbalization, in which the subject observed, remembered, but did not verbalize mentation which occurred after progressive muscular relaxation exercises. The ganzfeld and relaxation procedures each lasted 30 minutes, and were followed by a 5 minute GESP impression period during which the subject attempted to become aware of a 35 mm slide, randomly selected from the Maimonides binary target pool, and viewed by a remote agent (R.W.). Psi was assessed by means of 'hit', 'direct hit', and binary target scores. Twenty-four volunteer subjects were each tested under all four conditions; the twenty-four possible permutations of the four conditions were randomly assigned to the twenty-four subjects.

No evidence of psi emerged in this experiment, either overall or in any of the four conditions considered separately. The results suggest that ganzfeld stimulation and induced relaxation are not sufficient conditions for the reliable demonstration of receptive psi.

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This experiment was conceived and designed by Robert Wood and William Braud. The subjects were recruited and tested by Robert Wood. Data reduction and statistical analyses were done by Robert Wood and James Kirk. The final paper was written by Robert Wood and William Braud.

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APPENDIX

GENERAL INSTRUCTIONS TO SUBJECTS

(with instructions for each condition inserted)

This is an experiment to measure your telepathic ability in different situations. You have randomly been assigned a sequence of four different conditions during which I will send you psychic impressions of a 35 mm target slide. (Condition specific instructions include:)

For ganzfeld - Covering your eyes with these ping pong ball halves and your ears with these headphones is what we call ganzfeld stimulation. These ping pong ball halves and this blue light will produce a uniform visual field. Adjust the ping pong balls so that all you can see is a diffuse blue light: adjust them so that you can't see through any cracks around the edges. Please keep your eyes open throughout the experiment. You may blink normally, but keep your eyes open otherwise. Just relax and look into the blue light. This will help you to relax and block out any distracting sights. In a moment, when I put the headphones on your ears, you'll hear a loud hissing sound called 'white noise'. This noise blocks out any distracting sounds and also produces uniform stimulation.

For verbalization condition - As you relax and look into the blue light while listening to the white noise, we want you to 'think out loud'. Repeat all thoughts and feelings which pass through your mind. Do not try to cling to any of them. Just observe them as they go by. Say anything that pops into your mind. No matter how silly or strange, keep talking. Put everything into words.

After the headphones are in place and you've had a few minutes to relax, you will hear the sound of bells through the headphones. This is the signal for you to begin to 'think out loud'.

For non-verbalization condition - As you relax and look into the blue light, while listening to the 'white noise', we want you to observe your internal mental processes. Passively observe all thoughts, images, and feelings which pass through your mind. Do not try to cling to any of them. Just observe them as they go by. Do not try to put them into words. Simply observe them, and you'll remember them.

After the headphones are in place and you've had a few minutes to relax, you will hear the sound of bells through the headphones. This is the signal for you to begin to observe your imagery.

Conclusion for ganzfeld condition - At some time later in the session, you'll hear a second set of bells through the headphones. This is the signal for the part of the experiment involving ESP. Release all conscious hold of your body and allow it to relax completely. At this time, I will observe the target material in the room down the hall. Do not try to anticipate or create images of the target: just give yourself the suggestion - right now - that the image will appear in consciousness at the appropriate time. Tell me aloud, whenever you hear the bells.

Relaxation verbalization condition - Throughout the experiment you will be listening to a set of pre-recorded relaxation and mind-blanking instructions. Listen carefully to the tape recording and follow the instructions given in it. At some time later in the session, the tape will signal you that it is time to begin to 'think out loud'. Repeat all thoughts, images, and feelings that pass through your mind. Do not try to cling to any of them. Just observe them as they go by. Say anything that pops into your mind, no matter how silly or strange, keep talking. Put everything into words. Release all conscious hold of your body and allow it to relax completely. At that time I will observe the target material in the room down the hall. Do not try to anticipate or create images of the target: just give yourself the suggestion - right now - that the images will appear in consciousness at the appropriate time. Passively observe all thoughts, images, and feelings which pass through your mind, and describe them verbally. Do not try to cling to any of them. Just observe them as they go by. Tell me aloud, whenever you hear the bells.

Relaxation nonverbalization condition - Throughout the experiment, you will be listening to a set of pre-recorded relaxation and mind-blanking instructions. Listen carefully to the tape recording and follow the instructions given in it. At some time later in the session, the tape will signal you that it is time to begin that part of the experiment involving ESP. Release all conscious hold of your body and allow it to relax completely. At that time, I will observe the target material in a room down the hall. Do not try to anticipate or create images of the target: just give yourself the suggestion - right now - that the images will appear in consciousness at the appropriate time. Passively observe all thoughts which pass through your mind. Do not try to cling to any of them. Just observe them as they go by. Do not try to put them into words. Simply observe them, and you'll remember them. Tell me aloud

whenever you hear the sound of bells.

Instructions concluding all four conditions - When the impression period has ended, you will hear a voice through the headphones instructing you to return to a normal state of consciousness and to write and draw your impression of the target. Write and draw only the impressions you had after the second bell. Then code these impressions, on the binary coding sheet, using the coding manual as a guide. You will find an envelope on the floor, near the door. This envelope will contain four slides - one of these is the correct slide, the one I was looking at. The other three are control slides which no one has seen. Examine each slide carefully, noting details of shape as well as meaningful content. Then compare each slide carefully with your written and drawn impressions. Rank order the slides in terms of how closely they correspond with your impressions. Give a rank of '1' to the slide which you think was most likely the target, '2' to the next likely, '3' to the next, and '4' to the least likely to be the target. Rank all slides. No ties are allowed. Identify the slides by the numbers on them.

When you've finished, ring the bell on the table next to you and I'll come in and tell you about the rest of the experiment.

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