

EUROPEAN JOURNAL OF PARAPSYCHOLOGY

VOLUME 4

NUMBER 1

NOVEMBER 1981

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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 as 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. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include names of authors, positions and other affiliations, and mailing addresses. Each table
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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 activity in 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 are considered more important than the level of significance of the outcome of the study. As a practical rule, we advise the potential contributor of an article to submit the design of his planned study before the work 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 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 dates, which are May 1st and November 1st.

PSI AND VOLITIONAL ACITIVITY:
AN ATTEMPTED REPLICATION

Giovanni Iannuzzo

The effect of opposing volitional activity on psi results of competing subjects has been experimentally examined by Rao, Kanthamani and Krishna (1979). The study was based on some speculations of Rao (1978) on the connection between volitional activity and psi experimental results. Rao et al (1979) affirmed that "when two subjects compete in an ESP test, their scores tend to vary significantly from each other if the target sequence is the same for both. This does not seem to be the case, however, if the target sequences are different".

Rao's experiment was carried out by the testing of three groups of two subjects (one of this always was Rao) that worked in the same and different target condition, and the obtained results supported their hypothesis. The subjects in the same-target condition obtained significant results (total chi-square value was 48.6, $df=25$, $P<.005$) and in the different-target condition they obtained non significant results (total chi-square value was 16.8, $df=24$). In the discussion, Rao et al affirmed that the hypothesis was confirmed by the experiment and suggested some implications. This study is a replication of their experiment.

Note: This paper fulfils the publication policy of this journal.

METHOD AND PROCEDURE

The method that was used is the same suggested and utilized in the Rao et al experiment. There were three groups of two subjects; the experimenter was always one of these. Three subjects were, therefore, selected on the ground of their good rapport with the experimenter and of their personal interest in parapsychology. The first subject was G.F., a lawyer who participated in the first session with G.I. (the author). The second subject was G.M. who competed with the author in the second series, and the third subject was G.B., a law student who has, likewise to G.M. and G.F., an interest in experimental parapsychology. G.B. was - with G.I. - the subject of the third session of trials.

G.F., G.M. and G.B. will be designated in this paper (see tables) as 'subject 2'. There is a very good rapport between the three subjects and the experimenter.

In the experimental situation chosen by Rao et al a computer generated sequences of randomized targets and then printed the target sheet and stored the targets for later computer analysis. In this replication, since I did not have a computer at my disposal the target sequences were generated in a different way. An independent collaborator provided the targets from an electronic random number generator; it is part of a scientific calculator (Casio Fx-310) and generates random numbers (0 till 999). There were checks for bias. In about 50,000 trials it did not show a significant deviation from theoretical randomness.

It was agreed that number 1 and 6 was circle, 2 and 7 cross, 3 and 8 waves, 4 and 9 square, 5 and 0 star. Rhine and Pratt (1957) pointed out a similar codification. The targets were generated with replacement as in the Rao study.

At the beginning of each series my collaborator generated the target sequence, and recorded it on a sheet. The number of sessions in a series was preset at 20, with 5 runs totalling 125 trials per session. For half of the sessions the target was the same for the two subjects. For the other half the target sequence was different. The same and different target conditions were arranged in a balanced, random order by my collaborator.

The subjects did not know the objectives of the experiment and they did not know the sequence of same-target sessions and different-target sessions. The experimenter did not know which runs were same-target and which were different-target. The three series were held sequentially.

At the start I explained to subject 2 that a collaborator had prepared the targets for the ESP test. I told the subjects, in a similar way to the experiment of Rao et al:

"The targets are 125 standard ESP symbols (circle, cross, waves, square and star) written on a record sheet in a random order. Our task is to guess the order of these symbols. We will record our guesses. If at the end of the experiment, we find that you got more hits than I, you will win and consequently I will pay you L. 5000 (about six dollars) that you can use as you want. If I win you will pay me the same sum of money".

My collaborator prepared separate sets of record sheets numbered consecutively for the two participating subjects, and handed me the two sets of record sheets in separate folders for use during the experiment. At the beginning of the experiment, the experimenter gave the first record sheet to the partner. The two subjects were in the same room; G.I. sat at his desk when he wrote his guesses; the other subject was sitting in an armchair a few metres away so as to preclude that the two subjects might sensorially influence each other's guesses.

The entire experiment was done in one session (20 runs). At the end of each series, G.I. took the call sheets and collected from the collaborator (who was in another room in the building) target sheets marked separately for the two participants. Then G.I. and the participating subject checked their calls with the appropriate target sheet and circled the hits.

After this the collaborator transcribed the results onto the same sheet on which were written the random target sequence and independently scored the hits. Then G.I. analyzed the results.

During the experiment the subjects were asked about their sensation of competition. G.F. told me that he felt a great sensation of competition. G.M. and G.B. told me that they felt no sensation of competition and that their interest and attention were engrossed by

the total results of the experiment.

The atmosphere of the testing was of low competition in the second and third series; the greatest competition was obtained in the first series. In all the three series, however, the volitional activity was fairly high and, on the whole, the experiment was realized in an atmosphere of low competition and high volitional activity. It seems that the subjects strongly desired to obtain high results, independently from the competition with me.

ANALYSIS OF DATA

The data was analyzed (since non consistency of results over 20 sessions was expected) by obtaining the critical ratios of the difference in the scores of the two participating in each session and to square them to obtain a chi-square. The chi-squares for all the sessions were added to obtain the probability values separately for each condition. This was the method of analysis of data in the different-target condition.

In the same-target condition the analysis of data was based on a different procedure: to eliminate the effects of call patterns (the two subjects guessed the same target) the corrections in computing the CR's were made, by a method (see Kelly & Burdick, 1977, and Rao's paper, 1979, pages 111-112) which utilized a different computation of variance for the runs in the same-target condition.

The variance for difference scores is $2k/5$, where k is the number of trials in which the subjects' calls differ. The derivation of this variance is shown in Rao (1979, appendix).

HYPOTHESIS

It was hypothesized that results of subjects tend to vary significantly from each other if the target sequence is the same for both, and not significantly from each other if the target sequence is different.

TABLE 1
ESP scores in the same- and different-target conditions

Series	Session	Same-target hits		Chi-square	Different-target hits		Chi-square
		Subj.1	Subj.2		Subj.1	Subj.2	
1	1	17	21	0.412	29	23	0.900
1	2	26	27	0.024	25	30	0.625
1	3	33	20	4.225	27	27	0.000
1	4	24	26	0.092	28	27	0.025
1	5	31	30	0.025	23	27	0.400
1	6	30	24	0.909	24	17	1.225
1	7	15	24	2.046	23	21	0.100
1	8	33	27	0.900	28	23	0.625
1	9	28	32	0.611	32	25	1.225
1	10	28	23	0.644	24	25	0.025
2	1	23	21	0.100	22	29	1.225
2	2	20	29	2.025	28	24	0.400
2	3	18	21	0.239	21	22	0.025
2	4	30	22	1.538	24	23	0.025
2	5	22	29	1.166	27	29	0.100
2	6	24	25	0.024	28	32	0.400
2	7	35	25	2.358	30	31	0.025
2	8	26	26	0.000	21	29	1.600
2	9	26	26	0.000	27	26	0.025
2	10	27	25	0.101	28	25	0.225
3	1	19	24	0.631	25	27	0.100
3	2	23	20	0.625	24	14	2.500
3	3	32	18	5.105	23	26	0.225
3	4	20	22	0.103	25	24	0.025
3	5	22	30	1.684	24	22	0.100
3	6	30	22	1.568	22	33	3.025
3	7	28	25	0.218	23	22	0.025
3	8	25	21	0.373	23	22	0.025
3	9	26	20	0.865	24	15	2.025
3	10	19	21	0.099	23	29	0.900

RESULTS

The results are shown in two tables: in the first are shown the hit for session and group of subjects, either in the same or in different target condition. In the second table there is a summary of results (sum of chi-square, degree of freedom, p-value).

TABLE 2
Summary of results

Series	Same-target condition			Different-target condition		
	Sum chi-squares	df	p	Sum chi-squares	df	p
1	9.88	10	n.s.	5.15	10	n.s.
2	7.55	10	n.s.	4.05	10	n.s.
3	11.27	10	n.s.	8.95	10	n.s.
Pooled	28.71	30	n.s.	18.15	30	n.s.

In the first series the combined chi-square for all the same-target sessions is 9.88 (df=10). This result is insignificant. For the different-target sessions chi-square is 5.15 (df=10) and the corresponding p-value is insignificant. In the second series chi-square for all the same-target sessions is 7.55 (df=10) and chi-square for different-target sessions is 4.05 (df=10). Both values are insignificant. In the third series the combined chi-square for the same-target condition is 11.27 (df=10). For the different target-condition the chi-square is 8.95 (df=10). These results are also insignificant.

The combined chi-square of all the three series is 28.71 for the same-target condition (insignificant) and 18.15 for the different-target condition (insignificant). The F ratio between the chi-squares of the difference between the scores of the two subjects for the comparison of the two conditions ($F=1.32$) doesn't suggest significance. There is a higher chi-square value, however, in the same-target sessions than in the different-target sessions; but this

difference (28.71 against 18.15) is insignificant.

DISCUSSION AND SUMMARY

The obtained results are statistically insignificant. The difference between the results obtained when the two subjects were tested in the 'same-target condition' and when they were tested in the 'different-target condition' is insignificant. The Rao et al hypothesis that the scores of two subjects who compete in an ESP test tend to vary significantly from each other when they attempt to guess the same target, but not when they attempt to guess different targets, is not confirmed in this experiment.

Since a situation of competition is considered by some authors as a 'psi-conducive condition' (see Steen, 1957; Ratte and Green, 1960; Kanthamani, 1966; Weiner, 1978) it is possible that the absence of this stimulating factor in the experimental situation may have been the cause of low scores.

REFERENCES

- Burdick, D.S. and Kelly, E.F. 'Statistical methods in parapsychological research', in Wolman (ed) 'Handbook of parapsychology', New York, Van Nostrand Reinhold, 1977.
- Kanthamani, B.K. 'ESP and social stimulus', J.o.P., 1966, 30, 31-38.
- Rao, K.R. 'Psi: Its place in nature', J.o.P., 1978, 42, 276-303.
- Rao, K.R., Kanthamani, H. and Krishna, S.R. 'Psi and volitional activity: An experimental study of psi in a competitive situation', J.o.P., 1979, 43, 101-113.
- Ratte, R.J. and Greene, F.M. 'An exploratory investigation of PK in a game situation', J.o.P., 1960, 24, 159-170.

Rhine, J.B. and Pratt, J.G. 'Parapsychology: Frontier science of the mind', Springfield, 1957.

Steen, D. 'Success with complex targets in a PK baseball game', J.o.P., 1957, 21, 133-146.

Weiner, D.H. 'High- versus low-risk condition in a computerized PK game', J.o.P., 42, 1978, 64-65. (abstract)

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ANALYSING SPONTANEOUS CASES: A REPLICATION
BASED ON THE SANNWALD COLLECTION

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In a previous paper (Schouten, 1979b) the results are presented of a quantitative analysis of a collection of spontaneous cases. That study was based on the cases as reported in 'Phantasms of the Living' (Gurney, Myers, and Podmore, 1886). The completion of this collection of spontaneous paranormal experiences was one of the first major undertakings of the newly established (1882) British Society for Psychical Research and illustrates the importance the founders of that illustrious society attached to these experiences. Despite the advancement of experimental parapsychology, that importance has not diminished over time. Indeed, the fact that throughout the centuries such experiences are reported and that they are still reported today provides the necessary background for making the results of experimental research intelligible. It is also noteworthy that present day developments in experimental research take a direction towards test situations which are much more similar to the circumstances of spontaneous experiences than the older forced-choice type of research.

Since spontaneous ESP experiences can be considered as the basis for experimental research in parapsychology, the aim of the above mentioned study was to find out whether these experiences are

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characterized by common trends and if so, do they suggest an ESP process or some alternative hypothesis and what could the implications of these findings be for a hypothetical ESP process. It turned out that several strongly significant trends and relationships between variables was observed. In this study it was shown that for spontaneous experiences, the familiar phenomena of a relatively high proportion of female percipients, of male target persons, and of cases between near relatives are not due, as has often been assumed, to various reporting effects. For example, cases between near relatives might be reported more frequently because they are more easily detected. On the contrary, these findings reflect some basic properties of these experiences. Other findings of interest were the nearly total absence of cases involving serious material damage and losses, the high proportion of cases related to something happening to some individual, the fact that the target person is more often correctly identified than the event, the relatively high number of events of a negative and threatening nature, the correlation between number of details and the correct identification of the event and the absence of such a relationship in the case of the identification of the target person. The reliability of these results is strengthened by the finding that in nearly all instances the cases for which testimonies were available from independent witnesses showed the same trends and relationships.

Except for mentioning some possible alternative hypotheses of a non-ESP character I did not discuss in that paper the implications of the study for the ESP concept, nor did I discuss all the hypotheses which could be derived from these data for laboratory testing. The main reason for these omissions is that I felt such discussions justified only after having verified that the data were generalizable beyond Brittany of the last century. The possibility that the findings observed are only valid for a certain culture was shortly mentioned in the previous paper in the section 'Alternative Hypotheses' under the heading: 'The Social Climate Hypothesis'. The essence of this hypothesis is that it might well be that in a given culture people expect 'paranormal' cases to take place, and to take place in a certain form, and that accordingly they are inclined to interpret certain occurrences in their life which fit these expectations as 'paranormal'. This might easily result in these cases showing certain culturally dependent patterns which could come as a surprise to an investigator not familiar with that culture and might set him on a false track. Considering the strong influence culture and society has on most aspects of human life it is reasonable to assume that culture

must exert some influence, if not on the experiences themselves, then very likely on the way these experiences are verbalized and reported (or not reported). So a discussion and interpretation of these data only makes sense when we know what findings can be attributed to the experiences themselves and which ones are culturally dependent.

That the 'social climate' hypothesis cannot explain many results of the previous study becomes already apparent when reading 'Phantasms of the Living'. After all, the authors of that book were undoubtedly the authorities in that culture on the topic of spontaneous paranormal experiences, and it is clear, judging from their publication, that they were unaware of several of the trends and relationships reported in the previous analysis, which was based on the cases collected by them. However, the best method to answer the question of cultural dependency is clearly a comparable study based on a collection of spontaneous paranormal experiences from some other culture. Two such replication studies are planned. At present one of these has been carried out, the results of which are reported in this paper.

It should be noted that I use the term 'different culture' here not so much in the anthropological sense but simply with the meaning: a different country, a different epoch. I do not consider it necessary to prove first that in nearly all cultures similar experiences and similar trends are found, it seems to me sufficient to show that even when strong cultural differences exist the same trends still appear. If this can be shown spontaneous cases could provide a more solid basis for research in parapsychology.

THE SANNWALD COLLECTION

The name 'Sannwald collection' denotes a collection of spontaneous ESP experiences which were sent to the 'Institut für Grenzgebiete der Psychologie und Psychohygiene' in Freiburg, Bundesrepublik Deutschland, between 1950 and 1959 and which formed the basis for Sannwald's dissertation 'Parapsychischen Erlebnissen und Persönlichkeitsmerkmalen (Relations between paranormal experiences and personality traits) (1961). Sannwald selected 1000 cases to make up his collection according to a number of criteria. However, it is not always entirely clear in what way these criteria were applied.

It is stated that cases were rejected in which the alleged

paranormal character could easily be explained by assuming other factors responsible such as predictable developments, errors in perception, or incorrect reporting. Another criterion, of relevance for the interpretation of the data to be presented, was that a case must contain sufficient details to render the 'coincidence' hypothesis unlikely. A third criterion of importance is that some cases were accepted even when the 'paranormal' character was only recognized after the event became known when such cases conformed to the general picture of a paranormal case ("wenn das Erlebnis nach Inhalt und Ablauf einem immer wiederkehrenden und vielfach bezeugten 'Geschehenstypus' entspricht" (Sannwald, 1961, p.69)). In other words, some cases were selected because they fitted the picture of the typical spontaneous paranormal experience, the 'social climate' hypothesis at work. Still another criterion concerns the time elapsed between the experience taking place and the moment of reporting. Cases which were considered too old were rejected.

Since the quantitative analysis of this collection is intended as a replication study it is of importance to note the main differences between the 'Phantasms' and the 'Sannwald' collection. The 'Phantasms' collection dates from Victorian England, the 'Sannwald' collection from Germany of the first half of this century. In addition, more than half of the 'Sannwald' cases are of the German Nazi period. Another important consequence of the period of time from which this collection dates is that in that period two extremely bloody and destructive wars were fought by Germany. Hence the 'Sannwald' collection contains a relatively large portion of war cases, mainly from the second world war.

A further difference between the two collections seems to me, although it is not easy to prove, that people of different social strata are involved. The 'Phantasms' collection was acquired mainly through the officers of the S.P.R. and through acquaintances of the investigators and other members of the Society, people mainly associated with intellectual circles. Thus it was stated: "A very large number of our first-hand witnesses are educated and intelligent persons, whose sobriety of judgment has never been called in question. For the most part, moreover, they have been in no way inclined to admit the reality of the phenomena, prior to themselves encountering it" (Gurney et al, p.120). And indeed, reading through the cases one gets the impression that most people involved belonged to the well-to-do and upper classes. It is also stated (ibid, p.160) when discussing the need for reliable reports that "we have frequently

thought it right to exclude cases, otherwise satisfactory, that depended on the reports of uneducated persons". On the other hand, the 'Sannwald' collection is nearly entirely made up of cases sent in by people of common stock, farmers, tradesmen, housewives, etc..

The notion that the experiences collected by the investigators could be considered as spontaneous telepathic experiences rather than as e.g., apparitions of the dead was quite novel for the period during which the 'Phantasms' cases were collected. It is stated: "The idea of apparitions after death has a wide and strong hold on the popular mind; the idea of apparitions at the time either of death, or of serious crises in life, has no established vogue" (ibid, p.121). And further on: "The class of apparitions and impressions which have corresponded with the death of the 'agent' has only be vaguely recognised (as a telepathic phenomenon); the class which have corresponded with a state of passing excitement or danger can hardly be said to have been recognised at all" (ibid, p.122). In other words, according to the authors of 'Phantasms of the Living', the notion of spontaneous telepathic experiences, especially in the case of non-death events, hardly existed in those days. The latter cannot be said of the period in which the 'Sannwald' collection was acquired. On many occasions one finds indications that the concepts of telepathy and spontaneous telepathic impressions were quite familiar to the person reporting a case.

A further difference between the collections concerns the selection of the cases. In the section above the selection criteria applied by Sannwald have been described. The British investigators applied criteria of a different nature, mainly because they put much more stress on the trustworthiness of the reports. For nearly all cases they personally questioned the contributors and witnesses of the case. Most importantly they insisted on only including first-hand accounts, and, even then, only if they were critically examined. That this examination was indeed critical in nature, and now and then gave offense, can be inferred from various statements in the book. For instance: "We have, - , excluded all narratives where on personal acquaintance with the witnesses, we felt that we should be uneasy in confronting them with a critical cross-examiner" (ibid, p.160). Also: "we have observed with pleasure that our somewhat persistent and probing method of inquiry has usually repelled the sentimental or crazy wonder-mongers who hang about the outskirts of such a subject as this" (ibid, p.LXVIII). Another indication for the thoroughness with which the investigators examined the accounts is their extensive

treatment (ibid, chapter IV) of potential sources of error. Judging from present-day critical literature on parapsychology, little has been added since.

Hence the main reason for rejecting cases for the 'Phantasms' collection was doubt as regards the correctness of the facts stated in the accounts. On the other hand, less is said about the type of cases which were to be included. It is stated that: "records (were invited) of apparitions at or after death, and other abnormal occurrences" (ibid, p.LXI). And on the following page (ibid, p.LXII): "When we began, - , to collect accounts of experiences which our informants regarded as unexplicable by ordinary laws". Furthermore on page XIX in connection with their definition of the concept of telepathy: "an arbitrary limit of 12 hours (between experience and event) has been adopted". So it seems that when collecting the cases for the 'Phantasms' collection, there has been a tendency to especially invite death cases, but in general all 'unexplicable' experiences were welcome, provided the investigators were convinced of the reliability of the narrative.

A last difference between both collections is related to the type of ESP experiences. As mentioned above, the 'Phantasms' collection is composed of only telepathy cases, defined as cases in which the time between experience and event was less than 12 hours. However, the 'Sannwald' collection is a mixture of telepathy and precognition cases (and a very few clairvoyance cases which were excluded from the analyses).

Despite these differences it should be kept in mind that both collections have in common that the majority of the cases are cases in which the ESP experience is related to some other person. Only the 'Sannwald' collection contains some clairvoyance cases and cases in which the precognitive event is related to the percipient her- or himself. Therefore it might well be that some conclusions which can be based on the findings of these studies are not applicable to cases in which no other person was involved. A separate quantitative analysis of a collection of such cases would be highly desirable.

THE ANALYSIS

Before the analysis was carried out it was decided:

1. That the replication study would involve all tables and other analyses reported in the 'Phantasms' study with the exception of the analyses of 'best-witnessed' cases since the required testimonies for singling out such cases were not available.
2. That the above stated differences between the collections as regards the inclusion of war cases and precognition cases will in principle be ignored when running the analyses. However, if differences with the results from the 'Phantasms' collection are observed it will be investigated whether these differences can be attributed to one of these variables (war versus non war cases and telepathy versus precognition cases).
3. If such is the case, that is to say, if non-war telepathy cases appear to confirm the 'Phantasms' data it will be decided that the finding is confirmed.
4. If other relationships emerge that are not reported in the previous publication they will be presented in a section 'Further Analyses'.
5. As in the 'Phantasms' study (but not reported in the previous publication) I will investigate all possible interactions between variables.

The analysis is based on 789 cases of the Sannwald collection. It was decided in advance to include about the same number of cases as was used in the 'Phantasms' study (562), but when it became apparent that time was available to score more cases I did so in view of the possible influence of the war and precognition cases on the results. As in the 'Phantasms of the Living' the majority of the cases were literally taken from the letters and typed up. These typed versions were used for the scoring which I carried out myself according to the criteria and categories applied when scoring the 'Phantasms' collection (see Schouten, 1979a). In fact I worked through the file of typed versions sequentially, first going through the whole file taking one case from each contributor. Then as long as time allowed I went over the file again scoring more cases. Some cases were not included for reasons such as no typed version being available, too few data

resulting in too many empty cells, being apparently provided by more or less professional mediums, in the case of second- or even third-hand cases, etc.. Precognition cases were scored in such a way to enable discriminating between precognitions related to the percipient or related to some other individual, or precognitions concerning general events. The latter have not been included in the main analyses because all 'Phantasms' cases are related to events concerning some person. For the same reason the few clairvoyance cases were excluded from most analyses.

As regards the precognition cases it should be noted that several of them concern experiences in which both the percipient and some other person or persons (for instance mother and child) were involved as target persons. Those cases were scored so as to enable separate treatment. However, if not otherwise stated, they have been treated as if the precognitive experience concerned the other person(s).

The significance level is set at $p=.01$. Strongly significant effects ($p<.001$) will be indicated by $p<<.01$. Unless otherwise stated all p -values given are two-tailed. When appraising the results one should bear in mind that the total number of analyses carried out is far over a hundred, many of them analyses checking for possible interaction effects. It is impossible to give full details of all these analyses, however, all analyses which yielded a significant effect are reported. In most instances in which the data of this study confirm the data of the previous study I will only mention the chi-square values and significance level. If not otherwise stated, the effect in these data is in agreement with the trend observed in the corresponding data of the 'Phantasms' study. In the chi-square analyses, cells with a frequency of five or less were excluded.

Occasionally differences in totals will be noticed for the numbers reported in the various tables. These are not counting errors but are due to the fact that in several cases one or more categories could not be scored for lack of data. All analyses were carried out by computer. In each analysis only those cases have been included in which all variables relevant for that analysis could be scored. To facilitate comparison with the data of the 'Phantasms' study for each analysis the page where the same analysis was reported or the number of the relevant table are given in brackets, preceded by the word 'Phantasms'.

RESULTS

The percipient

The distribution of male and female percipients for cases in which the experience was related to another individual is 136 male percipients (25.7%) against 393 female percipients (74.3%). Assuming that males and females were equally distributed in the population this difference is highly significant ($z=11.2$, $p<<.01$) confirming the same trend of a higher proportion of female percipients as observed in the 'Phantasms' study ('Phantasms', p.414). However, this distribution of male and female percipients turns out to differ significantly from the corresponding distribution found in the previous study ($\chi^2=38.3$, $df=1$, $p<<.01$). Female percipients dominate even stronger in this collection.

As stated above, when differences between both collections are observed I will first investigate whether such differences can be attributed to the inclusion of war and precognition cases. To this end I have split up the cases in four categories: telepathy -- war cases; telepathy -- non-war cases, the type of case which is comparable to the cases of the 'Phantasms' collection; precognition -- war cases; and precognition -- non-war cases. Comparing the distributions of male and female percipients for each category with the corresponding distribution observed in the 'Phantasms' study it appears that the higher proportion of female percipients observed in this collection can be mainly attributed to the war cases. The data are presented in table 1.

However, before drawing a conclusion I felt it necessary to study a possible interaction as regards sex of percipient between the variables war (war versus non-war cases) and type of ESP (telepathy versus precognition) for female percipients. This analysis yielded a non-significant result ($\chi^2=2.4$, $df=1$). As no interaction is observed I felt free to compare the distribution of male and female percipients for war versus non-war cases ($\chi^2=19.6$, $df=1$, $p<<.01$) and for telepathy versus precognition cases ($\chi^2<1$, $df=1$). From these analyses it becomes clear that the excess of female percipients compared to the 'Phantasms' data can indeed be attributed largely to the introduction of war cases. However, it should be noted that in all categories there are relatively more female percipients

TABLE 1
Male versus female percipients for different types of cases,
compared with the distribution of percipients as observed
in the 'Phantasms' study

category	male perc.		female perc.		chi-square	p
telepathy -- war	12	10.8%	99	89.2%	41.2	<<.01
telepathy -- non-war	69	33.8%	135	66.2%	5.7	ns
precogn. -- war	12	18.2%	54	81.2%	14.9	<<.01
precogn. -- non-war	43	29.1%	105	70.9%	9.9	<.01
'Phantasms'	247	43.8%	316	56.2%		

than in the 'Phantasms' study - the effect is even significant in the precognition -- non-war category - so it appears that there is another but weaker factor which also tends to increase the proportion of female percipients.

When we compare the total number of males and females involved in all cases, either as percipient or target person, with the corresponding distribution of the 'Phantasms' collection ('Phantasms', p.414) a difference is observed. In the 'Phantasms' study males dominated significantly, here we find a (non-significant) higher proportion of females: 662 females against 583 males. When we compare this distribution with the corresponding one of the foregoing study not surprisingly the difference turns out to be significant (chi-square=11.8, df=1, $p < .01$). Hence it can be concluded that in general in the 'Sannwald' collection more females were involved than in the 'Phantasms' collection. This might be a reporting effect or perhaps this phenomenon is related to some other difference between the two collections. In view of the latter possibility it seems appropriate to postpone a more detailed discussion of this finding until all further analyses have been presented.

In table 2 the data are presented for male and female percipients

over groups who differ in type of relationship.

TABLE 2
Male versus female percipients in different relationships

	male P	female P
spouses	12 13.2%	79 86.8%
siblings	12 27.9%	31 72.1%
parents and children	10 9.9%	91 90.1%
children and parents	28 40.0%	42 60.0%
other family	17 28.8%	42 71.2%
	chi-square=28.7, df=4, $p < .01$	

It shows from these data that the higher proportion of female percipients is not restricted to a special type of relationship, which confirms the corresponding finding in the 'Phantasms' study ('Phantasms', table 1). It is of interest to note that in the 'Phantasms' data one exception was observed: in those cases in which children had an experience related to one of their parents more male percipients were found. From table 2 it appears that here too this category yields the highest proportion of male percipients.

Studying potential reporting effects as described in the 'Phantasms' study on the pages 415-417 I first compared the distribution of male and female contributors who were not directly involved in the cases as either percipient or target person. A non-significant result was obtained ($z=1.6$), males contributing slightly more cases than females. In the previous study it appeared from the corresponding analysis that significantly more males than females contributed cases ('Phantasms', p.416). In the present study no such difference was observed, although it should be added that the proportions of male and female contributors who were not percipients or target persons in the two collections does not differ to a significant degree (chi-square=1.7, df=1). Anyhow, we can not assume that compared to males, females would

be more inclined to report a case which, since most cases are reported by the percipients, could otherwise explain the higher proportion of female percipients observed. The same holds true when we deduct from the number of male contributors those who because of their profession would have had a better chance of becoming acquainted with spontaneous cases ($z=1.3$). Although males contributed more cases than females when the contributor was not self involved in the case it turned out that, in agreement with the results of the previous study ('Phantasms', table 2), the distribution of male and female percipients in those cases is not different from the distribution in the cases sent in by the percipients or target persons ($\chi^2 < 1$, $df=1$). This implies that although the proportion of male and female contributors differ for both groups of contributors the sex difference for percipients is almost identical for both groups. (cases contributed by percipients and target persons: 30.1% male and 69.9% female percipients; cases contributed by others 31.5% male and 68.5% female percipients). In other words, no interaction exists between the variables category of contributor and sex of percipient.

Also as in the former study no tendency could be observed for male reporters to be more inclined to report a case when a female acts as percipient or vice versa ($\chi^2 < 1$, $df=1$) ('Phantasms', table 3). Hence all analyses in the present study as regards possible reporting effects yield the same conclusion as drawn in the former study: reporting effects can not be held responsible for the observed higher proportion of female percipients.

The target person

There are 366 male target persons (69.1%) and 164 female target persons (30.9%). Although compared with the 'Phantasms' collection ('Phantasms', p.417) a slightly higher proportion of male target persons is observed the difference between the two distributions is not significant ($\chi^2 = 2.2$, $df=1$). The difference between male and female target persons in itself is highly significant ($z=8.8$, $p < .01$) confirming the same trend found in the foregoing study. When comparing the distribution of male and female target persons over the different types of relationships (see table 3), the effect of a higher proportion of male target persons appears to be rather consistent except for the category 'children-parents', in which a child had an ESP experience related to one of the parents. In that category the

mother is found to be more often target person than the father.

TABLE 3
Male versus female target persons in different relationships

	male target person		female target person	
spouses	79	86.8%	12	13.2%
siblings	31	72.1%	12	27.9%
parents and children	82	82.0%	18	18.0%
children and parents	30	42.9%	40	57.1%
other family	32	58.2%	23	41.8%
acquaintances	94	65.7%	49	34.3%
	chi-square=47.9, df=5, $p < .01$			

Both results, the higher proportion of male target persons over the different relationships and the single exception to this finding, confirm the results of the previous study ('Phantasms', table 4).

This result cannot be attributed to a reporting effect. There appears no difference between the proportion of male and female target persons over the various groups of contributors (chi-square<1, df=1) ('Phantasms', table 5). Nor can the higher number of male target persons be explained by assuming that males would be more prone to meet a serious accident or get involved in negative events, the content of the majority of the cases (chi-square=5.6, df=3) ('Phantasms', table 6). All these findings agree with those observed in the former study.

The relationship between percipient and target person

The distribution of cases over the different types of relationships is presented in table 4. It goes without saying that the analyses reported in this section concern only cases in which the experience

was related to some other person.

TABLE 4
Number of cases for different groups of relationships

	n	'Sannwald'	'Phantasms'
close family	305	55.9%	51.6%
other family	60	11.0%	13.4%
acquaintances	153	28.0%	32.8%
strangers	28	5.1%	2.2%

It turns out that the percentages for both collections are rather similar. When the distributions are compared based on the observed frequencies the difference appears to be not significant ($\chi^2=10.7$, $df=3$). When this distribution of number of cases for different types of relationships is compared to the same distribution but only for death cases again a non significant deviation is observed ($\chi^2=5.52$, $df=3$) ('Phantasms', table 8). Hence the conclusion reached in the former study that the well-known phenomenon of a higher proportion of cases between near relatives can in principle not be attributed to a higher likelihood of detecting a case between members in this group is confirmed by this study. The 'in principle' is added because as will be seen later on, there appears a slight effect in the cases with non-serious events. As before no indication is found for the influence of a reporting effect assuming that the death of relatives might impress the people involved more than the death of a member of one of the other groups and would therefore be sooner reported ($\chi^2<1$, $df=2$) ('Phantasms', table 9). Also a non-significant effect is observed when analysing a possible relationship between percipients versus others as regards reporting a death case for the different types of relationships ($\chi^2=1.9$, $df=1$). However, this result deviates from what was found in the previous study ('Phantasms', table 10). There it appeared that percipients reported a death case relatively more often when other

family members or acquaintances were involved than when it concerned a near relative. It was assumed that this reluctance of reporting on a death of a near relative might have been caused by the feeling that it would not be proper to treat the death of a near relative as a case for a scientific investigation. If so we can conclude that such feelings do not appear to have had a noticeable influence in the 'Sannwald' cases. When we compare the distribution of death cases contributed by percipients for the two collections in terms of type of relationship, a significant difference appears ($\chi^2=13.5$, $df=2$, $p<.01$). Relatively more death cases contributed by the percipients in the 'Sannwald' collection were related to near relatives. When the same analysis is carried out for non-percipient contributors, no difference is observed ($\chi^2=2.7$, $df=2$). The data are presented in table 5.

TABLE 5
Comparison between the two collections of death cases
reported by the percipient or others for the different
types of relationships

	reported by percipient				reported by others			
	'Phantasms'		'Sannwald'		'Phantasms'		'Sannwald'	
close family	77	38.1%	119	56.1%	96	59.6%	47	61.8%
other family	45	22.3%	34	16.0%	20	12.4%	14	18.4%
acquaintances	80	39.6%	59	27.8%	45	28.0%	15	19.7%
	$\chi^2=13.5$, $p<.01$				$\chi^2=2.7$			

Since in this analysis a difference was observed between the studies I investigated whether the variables 'war' and 'type of ESP' could explain this phenomenon. There was no significant difference. Hence we can conclude that this finding reflects a real difference between the collections.

It appeared from the previous publication that studying the

distribution of cases over the various types of relationships within the group 'near relatives' yielded an interesting insight into the dependency of the sex of the percipient and target person on the relationship between them ('Phantasms', table 11). Therefore the same data are presented here in table 6.

TABLE 6
Number of cases between near relatives

main category	p	t	n
spouses	female	male	79
	male	female	12
parent/child	father	son	9
	father	daughter	1
	mother	son	73
	mother	daughter	18
child/parent	son	father	12
	son	mother	16
	daughter	father	19
	daughter	mother	23
siblings	brother	brother	9
	brother	sister	3
	sister	brother	22
	sister	sister	9

A Spearman rank correlation between this distribution and the corresponding distribution of the 'Phantasms' collection yields a significant value of $\rho=0.63$ ($T=2.83$, $df=12$, $p=.01$, one-tailed). Thus here again the data confirm the findings of the previous study. Therefore the hypothesis developed in that study based on these findings that the probability of becoming either percipient or target person is influenced by the nature of their relationship, can be upheld.

The nature of the experience

Table 7 presents the proportions of the different types of experiences. Comparing the actual frequencies observed with the corresponding values of the 'Phantasms' collection ('Phantasms', table 12), a very pronounced difference emerges ($\chi^2=280.6$, $df=5$).

TABLE 7
Distribution of types of experiences

	intuitions	23.6%
	dreams	59.0%
	visual waking imagery	7.5%
	auditory waking imagery	3.2%
	visual and auditory waking imagery	0.2%
	others	6.6%

Waking experiences are strongly reduced in frequency in favour of intuitions and dreams. Considering the magnitude of the difference we can conclude without further analyses that the nature of the experience is strongly influenced by cultural differences. At least, the difference is so enormous that it seems unlikely that it could be explained entirely by a reporting effect or investigator biases, although the latter certainly have played a role in the acquisition of the 'Phantasms' cases. This will be discussed more extensively later.

Since the different types of waking imagery experiences have such a low frequency in the following analyses they are all combined into one category 'waking experiences'.

The data confirm the finding ('Phantasms', p.427) that the type of experience is not related to sex of percipient ($\chi^2=9.69$, $df=3$). However, contrary to the finding in the previous study ('Phantasms', p.427) a just significant and weak relationship was

observed between type of experience (dreams excluded, waking experiences combined) and time of day ($\chi^2=18.7$, $df=6$, $p=.01$), mainly due to the fact that a much higher proportion of intuition cases took place by day than in the night or evening.

In the previous study it was found that the experience significantly more often took place in the evenings or nights than in the mornings or afternoons ('Phantasms', table 13, dreams excluded). In the present collection this trend is reversed to such an extent that the distributions of cases over the time of day for the two collections differ significantly ($\chi^2=40.4$, $df=3$, $p<.01$). This difference is clearly a consequence of the much lower proportion of waking experiences and the relatively higher frequency of intuition cases. Since for obvious reasons dreams are excluded these analyses between type of experience and time of day represent mainly the influence of intuition and waking experiences. The observed reversal in trend is not surprising since it was already noticed that intuitions tend to happen more often in daytime.

Just as in the 'Phantasms' study ('Phantasms', p.428) no relationship appears between season and type of experience ($\chi^2=9.7$, $df=9$). Comparing the collections on the distribution of cases in fall and winter versus spring and summer no significant difference is observed ($\chi^2=5.8$, $df=1$). Also as in the 'Phantasms' collection no relationship was observed between type of experience and the percipient being indoors or outdoors ($\chi^2=2.4$, $df=2$).

Male and female percipients have a relatively equal number of experiences for each period of the time of day ($\chi^2=5.4$, $df=3$), confirming the result of the same analysis in the previous study ('Phantasms', p.429). The same applies to the finding that male percipients have an experience significantly more often when outdoors than female percipients do ($\chi^2=12.4$, $df=1$, $p<.01$) ('Phantasms', p.429). In the next analysis a difference between the collections is observed which again seems a consequence of the observed difference in frequency of waking experiences and intuitions. In the 'Phantasms' study no relationship was observed between social situation of the percipient (being alone or in company) and type of experience ('Phantasms', p.429), in the 'Sannwald' collection a strong relationship between these variables does exist ($\chi^2=32.0$, $df=2$, $p<.01$, dreams excluded) mainly due to the fact that intuitive experiences appear to happen about three times more often when

percipients are in company than when they are alone. In view of the 'social climate' hypothesis such findings are of relevance. Except for dreams which are by nature restricted to people being alone, significantly more ESP experiences happen when the percipient is with other people than when alone. In most countries this will undoubtedly conflict with the general opinion about these matters.

When comparing seriousness of event with type of experience in the previous study it was found that a strong relationship exists ('Phantasms', table 14) mainly caused by the strongly skewed distribution for waking experiences which were associated with the death of the target person in over 80% of the cases. The same trend is noticeable in the data of the present study (see table 8), but here the difference is not significant ($\chi^2=7.5$, $df=2$), due to the much lower frequencies of the waking experiences.

TABLE 8
Type of experience and seriousness of event

	death	non-serious
intuition	77	42
dream	204	145
waking experience	50	16

The same holds true for the analysis which compares the successful identification of target person and of event for the different types of experiences. From table 15 of the 'Phantasms' study one can easily notice that the strong relationship found there is again nearly entirely due to the skewed distribution of the waking experiences which are much more strongly related to the correct identification of the target person than to the identification of the event. Table 9 presents the comparable data for the 'Sannwald' collection. Although the same trend observed in the 'Phantasms' data is apparent, the difference here does not reach significance because of the low number

of waking experiences ($\chi^2=5.2$, $df=2$).

TABLE 9
Relationship between type of experience
and identification of target person and event

	target person identified	event identified
intuition	87	86
dream	242	259
waking experience	44	26

The event

A breakdown of the type of events to which the experiences were related together with the corresponding data for the 'Phantasms' collection are given in table 10. It is apparent that both distributions show the same overall pattern. Death cases dominate while serious and slight material damage hardly play a role. When comparing the actual frequencies for the different type of events in the two collections, material events excluded, a significant difference is observed ($\chi^2=66.2$, $df=4$, $p<.01$) which can be largely attributed to the differences in trivial and positive cases. When comparing both distributions excluding trivial and positive cases the difference drops to a non-significant level ($\chi^2=7.9$, $df=2$), whereas when comparing the two distributions excluding death cases the difference remains highly significant ($\chi^2=32.3$, $df=3$).

Since here again a significant difference is observed between the data of both collections a more detailed analysis was carried out. Splitting up the cases in which another person was involved along the dimensions 'war' and 'type of ESP' it is found that the distribution

TABLE 10
Distribution of events

	'Sannwald'	'Phantasms'
death	48.7%	66.7%
serious illness/accidents	13.7%	12.5%
slight injuries	9.2%	8.7%
serious material	2.5%	0.5%
slight material	0.5%	0.2%
trivial	14.1%	10.0%
positive	11.2%	1.4%

of events in all categories differ from the same distribution found for the 'Phantasms' study but to a different extent (chi-square values: telepathy-war: 75.3; telepathy -- non-war: 20.7; precognition -- war: 94.9; precognition -- non-war: 20.3; all analyses $df=4$). Comparing war versus non-war cases the distributions of events differ significantly (chi-square=36.9, $df=4$, $p<.01$) while no such difference is found when comparing telepathy versus precognition cases. The data of the war versus non-war analysis are reproduced in table 11 because they show that war cases are less related to trivial events but more often involve positive events than non-war cases.

Hence it can be concluded that the most important features found in the data of the 'Phantasms' collection are confirmed by the present data: the high frequency of negative events, especially death cases, and the near absence of events related to material damage. However, there are in general more trivial and positive cases in the 'Sannwald' collection, a difference of nearly 14%, the former usually associated with non-war cases, the latter more often taking place in connection with circumstances of war.

A study into the relationship between seriousness of event and time elapsed before reporting ('Phantasms', table 17) to investigate whether, as was found in the former study, trivial and positive events

TABLE 11
Distribution of events related to another person
for war and non-war cases

	war		non-war	
death	91	52.0%	198	56.7%
serious illness/accident	27	15.4%	51	14.6%
slight injuries	12	6.9%	32	9.2%
trivial	7	4.0%	47	13.5%
positive	38	21.7%	21	6.0%

are more easily forgotten than cases involving serious events yielded indeed such a trend, but due to the small number of cases reported within 1 year in this collection this trend does not reach significance. The data are presented in table 12. The finding that trivial cases are mainly sent in by the percipients ('Phantasms', table 18) is confirmed by the data of this study ($\chi^2=21.8$, $df=2$, $p<.01$).

The content of the experience

In 79.6% of the cases the percipient correctly identified the target person. When the frequencies of identified and not identified target persons for the collections are compared the difference is not significant ($\chi^2<1$, $df=1$) ('Phantasms', p.435). The event is correctly identified in 72.8% of the cases. This percentage differs significantly from the corresponding value (51.9%) for the previous study ($\chi^2=58.0$, $df=1$).

No difference is found between male and female percipients as regards the relative frequencies with which the target person was identified ($\chi^2<1$, $df=1$), nor as regards the identification of

TABLE 12
Relationship between seriousness of event and
time elapsed before reporting

	death	non-serious
reported within 1 year	4	9
older than 1 year	276	200

the event ($\chi^2=1.97$, $df=1$). As stated in the former study these results support the view that the difference between number of male and female percipients does not reflect a difference in ESP ability between these groups.

For the analyses of the 'conviction' or 'taking action' breakdown, most of the time data for only 'taking action' are presented because of the strong correlation between them. In all instances for 'taking action' and 'conviction' similar analyses yielded similar results.

The findings show that males do not differ from females as regards taking action ($\chi^2<1$, $df=1$) and that no relationship exists between indentifying or not indentifying the target person and taking action ($\chi^2<1$, $df=1$). These analyses confirm the similar findings of the previous study ('Phantasms', p.436), although it should be noted that in that study a weak trend was observed (significant at the .05 level) for males to take more action. Since 'focussing' has not been scored the next analysis presented in the previous publication ('Phantasms', table 19) could not be carried out on the present data.

A strong relationship is found between type of experience and taking action. Intuition cases trigger relatively more action than the other types of experiences ($\chi^2=109.5$, $df=2$, $p<<.01$). Not surprisingly a similar relationship exists between conviction and type of experience. Both analyses confirm the corresponding results of the

original study ('Phantasms', table 20, p.438).

Although the average number of correctly perceived details per case turns out to be somewhat higher than the number observed in the 'Phantasms' collection - in 52% of the cases instead of 32% at least one detail was present - the main conclusion drawn in the previous study concerning the relatively low number of details in spontaneous experiences is confirmed. As in the previous study no relationship was observed between number of details and time elapsed before reporting, time elapsed before reporting being dichotomized into '< 1 year' and '> 1 year'. However, little importance can be attached to this finding because the number of cases reported within 1 year is so low. This becomes apparent when the same analysis is carried out for all time intervals longer than 1 year being split up into intervals (<1 year, 1-2, 2-5, 5-10, 10-20, >20 years). Then a significant difference is observed (chi-square=30.0, df=10, $p < .01$). The data reflect a trend that cases with more details are relatively younger of date and that especially the older cases lack details. Hence we can conclude, contrary to what was found in the data of the 'Phantasms' collection, that there is a trend to forget details in the course of time ('Phantasms', p.440) although, as will be seen later, there is also a relationship between type of event and time elapsed before reporting. Trivial and positive cases which on the average contain more details are generally of more recent date. A similar relationship is found between length of report and time elapsed before reporting (chi-square=30.4, df=9, $p < .01$), reports of older cases being on the average shorter of length. As in the previous study the relationship between length of report and number of details turns out to be significant (chi-square=23.0, df=6, $p < .01$) yielding as expected a positive correlation between number of details and length of report ('Phantasms', table 22).

The relationship between number of details and type of experience confirms the results of the previous study ('Phantasms', table 23) that dreams contain the highest number of details (chi-square=102.8, df=4, $p < .01$). In the same vein the observed relationship between number of details and seriousness of event is confirmed by the present data (chi-square=100.9, df=2, $p < .01$). Here again it is found that details are much more often associated with non-serious events than with death. As in the 'Phantasms' study no difference was observed between number of correctly perceived details and sex of percipient (chi-square=2.5, df=2) ('Phantasms', p.442). Also the data concerning the relationships between identification of the target person, of the

event, and number of details yield the same results as observed in the 'Phantasms' study ('Phantasms', table 25). Because of the importance of these data for model building they are presented in table 13. Correct identification of the target person appears not to be correlated with number of details ($\chi^2=1.6$, $df=2$), whereas correct identification of the event and number of details correlate strongly ($\chi^2=96.5$, $df=2$, $p<<.01$).

TABLE 13
Relationship between identification of target person,
of event, and number of details

	number of details		
	0	1	>2
target person identified	234	90	73
not identified	53	27	22
$\chi^2=1.6$, $df=2$			
event identified	145	108	171
not identified	144	37	14
$\chi^2=96.5$, $df=2$, $p<<.01$			

All the remaining analyses subsequently reported in the previous study are confirmed by the data of the present investigation ('Phantasms', p.444,445). Conviction is significantly related to number of details ($\chi^2=13.0$, $df=2$, $p<.01$) in the same direction as observed before, viz. that relatively more conviction cases are observed when no details are present. This result is considered as supporting L.E. Rhine's (1978) conclusion that conviction has to be considered as a separate element, and is also of interest in view of the 'social climate' hypothesis. Distance is found to be slightly related to seriousness of event ($\chi^2=7.6$, $df=1$, $p=.01$), non-serious cases associated with close distances. Also distance and type of relationship are related, cases involving large distances

happening relatively more often between near relatives ($\chi^2=16.2$, $df=2$, $p<.01$). Distance has no influence on number of details ($\chi^2=6.2$, $df=4$) and also no relationship is observed between distance and type of experience ($\chi^2=4.4$, $df=4$).

DISCUSSION OF THE FAILURES TO REPLICATE

Considering all the analyses reported so far of the 52 analyses 13 appeared to yield a result different from the corresponding result obtained in the 'Phantasms' study. However, close inspection of these differences reveals that most of these failures to replicate are related to each other. They can be roughly ordered in three categories and will be treated accordingly.

1. Difference in type of experience

Five analyses which yielded a different result between the two collections are related to and a consequence of the observed differences in the frequencies of types of experiences. The most prominent change is the dramatic decrease in the proportion of waking experiences, from 53.9% in the 'Phantasms' collection to 10.8% in this one. Hence it is to be expected that certain trends observed in the previous study related to the high proportion of waking imagery experiences will disappear or be replaced by other trends more typical for other types of experiences. In this context it is important not to forget that most analyses relating type of experience with other variables have been carried out with dreams excluded, since dreams are by nature associated with night, being indoors, being alone, etc.. Consequently because of the relatively small number of 'other experiences' most of these analyses mainly reflect differences between waking experiences and intuitions. In the 'Phantasms' collection more than 5.6 times as many waking experiences than intuitions were included, whereas in the 'Sannwald' collection this ratio is reversed in favour of intuition cases (appearing 2.2 times more often than waking experiences).

Although I believe that this difference reflects a genuine cultural effect, it is certainly in part influenced by the fact that the investigators of the 'Phantasms' collection explicitly mentioned the word 'apparitions' when they solicited cases, thereby introducing a bias in favour of waking experiences and death cases. This is not

surprising, and certainly not to be considered as a weakness of their study, because the main theme of 'Phantasms of the Living' is to prove that apparition cases, until then mainly considered as apparitions of the dead, can be explained by telepathy, which the investigators considered as an experimentally proven phenomenon.

The non-confirmations which I consider to be consequences of the observed differences in intuition and waking experiences between the collections are as follows:

1.1 There is a relationship between time of day (night, morning, afternoon, evening) and type of experience when no such relationship was observed in the previous study. It turns out that waking experiences are nearly equally distributed over the time of day - hence the non significant finding of the previous study - but that intuition cases are strongly associated with the mornings and afternoons. In fact they appear more than twice as often by day than in the night. However, these analyses are to a certain extent misleading because dreams are excluded and the sleeping hours not accounted for. If we assume an average of 8 hours for sleep during which per definition intuition cases are not possible, then we find for the 10 hours of the morning and afternoon 70 cases, and for the remaining 6 hours of evening and night (14 hours total minus 8 hours sleep) 40 cases. Thus in fact the average number of intuition cases per hour is nearly equal over the whole day. The same reasoning applied to waking experiences yields of course a higher proportion of such experiences taking place in the evenings and nights (which confirms the corresponding finding in the previous study). However, no firm conclusion can be drawn in this respect since waking experiences also take place during the sleeping hours - the percipient wakes up and then has the experience while being awake - and thus the approach followed above is not entirely correct in this situation.

1.2 In the previous study in a significantly higher proportion of instances the percipient's impression took place during evening and night. In the present data this trend is reversed, again due to the higher proportion of intuition cases. However, the restrictions mentioned above apply to this situation also.

1.3 Without further detailed discussion it can be stated that the differences in type of experiences is also responsible for the finding that contrary to the data of the 'Phantasms' study, a relationship was observed between type of experience and social situation (being alone

or in company). Again this relationship is not due to the waking experiences which are about evenly distributed over both conditions but to the fact that intuition cases happen more often when the percipient is together with other people than when being alone.

1.4 In the 'Phantasms' collection a significant relationship was found between seriousness of event and type of experience (including dreams). Waking experiences appeared to be much more strongly associated with death cases than with non-serious events. The same holds true for the data of the 'Sannwald' collection. However, because of the relatively low number of waking experiences as well as the fact that the difference between death and non-serious cases is not as strong (50 versus 16) as observed in the previous study (233 versus 37), the trend does not reach significance.

1.5 The last analysis belonging to this group concerns the finding that the identification of the target person most often took place via a waking experience, while events were more often identified via dreams. Such a trend is also apparent in the 'Sannwald' data but again does not reach significance due to the low frequencies of the waking experiences. In order to present the reader with an example of the size of such trends I present the data for this analysis in table 14.

TABLE 14
Relationship between identification of the target person
and of event with type of experience

	'Sannwald'		'Phantasms'	
	target person identified	event	target person identified	event
intuition	87	86	33	26
dream	242	259	157	156
waking experience	44	26	245	97
	chi-square=5.21		chi-square=32.3, $p < .01$	

2. Differences in types of events

The differences between the frequencies of the types of events for both collections are not very impressive. In both distributions the same general pattern can be observed. But, since the differences are mainly concentrated in a lower percentage of death cases and a higher percentage of trivial and positive cases in the 'Sannwald' collection, some effects on other analyses can be expected. The higher proportion of non-serious cases must be to some extent responsible for the higher number of cases in which at least one detail was perceived. However, in this connection it should also be remembered that Sannwald's selection criteria for including cases in his collection must to some extent have favoured cases with details.

Another reason for the observed difference in types of events could be the already mentioned 'bias' in the way the cases for the 'Phantasms' collection were acquired. As observed previously, these investigators were mainly interested in apparition cases and invited in the first place "records of apparitions at or after death" (Gurney et al, p.LXI).

Failures to replicate which in my view can be attributed to the above mentioned differences in frequencies of types of events are as follows:

2.1 The identification of the event shows to be significantly higher in the 'Sannwald' collection. We know from both studies that the identification of the event is strongly related with number of details. Hence the higher proportion of trivial and positive cases which are associated with a higher number of details can explain at least part of this finding.

2.2 Contrary to the data of the previous study it was observed that a relationship exists between time elapsed before reporting and number of details. Such a relationship is to be expected in view of the unavoidable influence of time on memory. That this relationship reaches significance here is partly because of the higher number of details in this study.

2.3 The same applies to the observed relationship between time elapsed

before reporting and length of the report. Older cases show to be significantly shorter than more recent ones, again probably influenced by the higher number of details since in both studies a positive relationship was observed between number of details and length of report.

3. Miscellaneous

The remaining analyses with different results for the two collections are included in this group.

3.1 The total number of male and female participants in the 'Sannwald' collection shows a non-significant difference in favour of the females, whereas in the 'Phantasms' collection significant more males were involved. The difference between the distributions is significant.

When in the 'Phantasms' collection contributors other than percipient or target person are considered there appeared a slight tendency that males are more inclined to report a case than females ('Phantasms', p.416). In the present data no such difference is observed. In both collections other potential reporting effects related to the distribution of male and female percipients turned out to have had no influence. Nevertheless, although not significant for the non war -- telepathy cases, in all categories (war -- telepathy, non war -- telepathy, etc.) compared to the 'Phantasms' collection a higher proportion of female percipients is observed. Apparently the inclusion of war cases is not the only variable increasing the number of female percipients. The fact that in total more females are involved in this collection seems to me also a factor of influence, albeit less strong than the war - non-war variable.

In both studies it was found that sex of percipient is only related to the relationship between percipient and target person, and to no other variable (except of having the experience indoors or outdoors, which is irrelevant for this discussion. See also figure 1 of the following section on interactions). This implies that there is no reason to suppose that both sexes would have different ESP abilities. This makes it even more unlikely to assume that the difference in female percipients between both collections would be due to a difference in ESP ability between British and German females. Hence it

seems to me that contrary to what was expressed before, the excess of female percipients in the 'Sannwald' collection after all must be due to some sort of reporting effect, but one which can only be detected when different collections are compared.

One possible explanation (suggested by Jerry Solfvin) could lie in the way these cases were collected. The fact that males and females given otherwise equal conditions do not differ in their willingness to report a case as was observed in this collection should be distinguished from the problem of a possible difference between both groups as regards how they were invited to send in their cases. This can be illustrated as follows. Suppose males and females are equally willing to contribute a case, but for some reason only females are invited to send in cases. Then, because the majority of the cases are contributed by the percipients, it is easy to imagine that despite their equal willingness to report cases the number of female percipients will rise substantially. The 'Phantasms' collection was mainly acquired via the officers of the S.P.R., people moving in the intellectual circles. Since in those days few women completed an academic study it can be assumed that the initiative for soliciting cases originated from a male dominated part of the population. This agrees with the finding that in the group of 'other contributors' significantly more males were observed. I do not know how the contributors of the cases for the 'Sannwald' collection became aware of the existence of the Freiburg 'Institut' which prompted them to send in their reports but undoubtedly the public media (radio, magazines, etc.) will have played an important role. Thus it is very likely that the way these cases were acquired for these collections differed sufficiently enough as regards the number of males and females approached to create some effect on the proportion of female percipients. In this connection it is also of relevance to note that in the 'Sannwald' collection 73% of the cases were contributed by percipients, about 10% more than the corresponding percentage found in the 'Phantasms' collection.

3.2 In this study no difference was observed as regards the proportion of death cases involving near relatives, whereas in the 'Phantasms' study it appeared that percipients were somewhat reluctant to report on the death of a near relative. This finding implies that one of the few reporting effects observed in the previous study does not show up in the present study.

3.3 Two other analyses, one different from the findings of the

previous study and one confirmatory, should be treated with caution since the numbers involved are so low that a non-significant difference is almost unavoidable. These analyses concern the variable 'time elapsed before reporting' in which cases less than one year old are compared with the older ones. In the previous study ('Phantasms', table 17) a significant relationship was observed between seriousness of event and time elapsed before reporting. The same analysis in this study proved to yield a non-significant result which, in my opinion, for the reason mentioned earlier must be considered an artefact. The same applies to an analysis which did confirm the corresponding result of the 'Phantasms' study, namely that no relationship exists between number of details and time elapsed before reporting. In this case the artefactual nature becomes quite apparent from the same analysis carried out with time elapsed before reporting split up in more categories than just <1 and >1 year old. Then a strongly significant relationship appears.

CONCLUSIONS

When leaving out the two analyses discussed above in 3.3 because of their apparent artefactual nature it can be concluded that of the 50 analyses reported in the 'Phantasms' study the results of 12 were not confirmed by the present study. Except for one reporting effect observed in the previous study and absent in the present data all these failures to replicate can be largely attributed to the following differences between the collections:

1. There is a striking difference between frequencies of types of experiences. In the 'Phantasms' collection the waking imagery dominates, while in the 'Sannwald' collection the intuitions and dreams do.
2. There are more positive and trivial cases reported in the present collection, partly a consequence of the inclusion of war cases which tend to increase the number of positive cases, but also reflecting a general trend because the number of trivial cases is also higher despite the fact that this number is negatively influenced by circumstances of war.
3. Compared to the previous study in which male participants dominated more female participants were observed.

Two other conclusions of importance following from this study are:

4. War cases increase the number of female percipients and involve less trivial cases but more cases with positive events.

5. In no instance was a difference observed between telepathy and precognition cases when the latter were also related to some other person.

All the other findings of the previous study were confirmed by the present data. An overview of the main relationships observed in the present study will be presented in the next section. As stated before an interpretation and discussion of all these findings will not be presented until a third replication study has been carried out.

INTERACTIONS

The interactions between the variables was studied by making a drawing in which all significantly related variables were connected by solid lines. Next I checked for all possible relationships between variables - except for some which apparently do not make sense such as sex of target person versus length of report - and when a significant relationship was found the proper line was added on the drawing. After that the position of the variables on the drawing was changed relatively towards each other in such a way that a more or less recognizable pattern emerged. That is to say, I tried to form as much as possible closed triangles or polygons. The result representing an overview of all relationships observed in the data is presented in figure 1. The picture shows clearly that several independent clusters of relationships can be distinguished.

A: As has been more extensively described in the previous study, and confirmed by the data of the present study, the sex of the percipient and the target person seems dependent on the relationship between both. In addition it was found that female percipients have more often an experience when indoors than male percipients, undoubtedly due to different styles of life. It is of importance to observe that no other relationship with sex of percipient was significant, supporting the view that the excess of female percipients does not imply that women would have a better capacity for ESP.

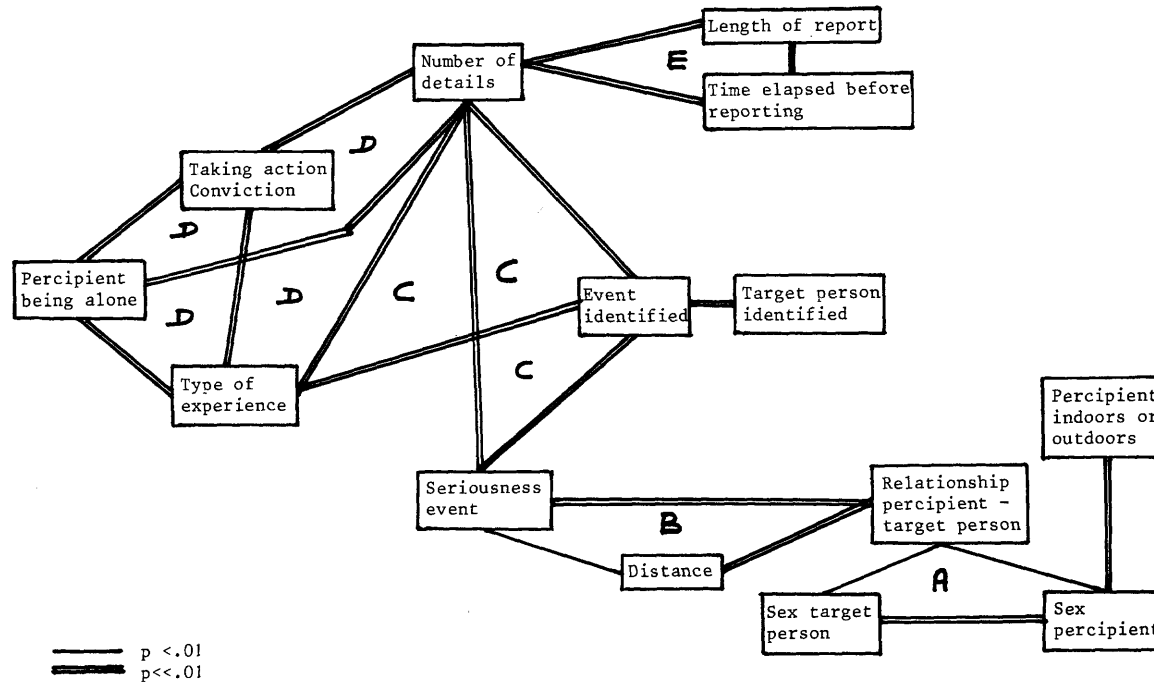


FIGURE 1
Relationships between variables

B: Compared with death cases, non-serious cases happen relatively more often between friends and acquaintances. Also cases involving non-serious events happen more frequently at shorter distances. This set of relationships might be explained by assuming that non-serious cases have a lower likelihood of detection when long distances are involved. However, it might also reflect a genuine dependency of ESP on distance, since non-serious cases require on the average more details to be perceived correctly than death cases.

C: There appears a strong relationship between seriousness of event and number of details. A higher number of details are on the average associated with non-serious events. The likelihood for correct identification of the event, but not of the target person, increases with the number of details. Non-serious events happen to be relatively more often correctly identified than serious events. Since dreams contain on the average the highest number of details, it is not surprising that they also yield a higher proportion of correctly identified events.

D: Dreams contain in general more details than intuitions or waking experiences. However, the more details the less the experience results in action or conviction. Thus dreams are least accompanied by taking action or conviction. Action is more often taken when the percipient receives the experience when with others and intuitions, which generally lack details, are correlated with the percipient not being alone.

E: The number of details is correlated positively with length of report. Older cases contain on the average less details, so it is not surprising that older reports tend to be shorter.

FURTHER ANALYSES

When studying the distribution of male and female percipients it appears that in war cases the proportion of female percipients is significantly higher than in non-war cases. In the previous publication I have argued that the roles of percipient and target person are mainly dependent on the relationship between them, and that especially events threatening to that relationship will be the subject of the ESP experience. Hence one can assume since many war cases involve the fate of soldiers that in those cases also more male target

persons should be found.

The distribution of the sex of target person was nearly the same for both collections. Therefore there appeared no reason to study the effect of the inclusion of war cases on this variable. Furthermore, that finding does not lead us to expect different distributions of sex of target person for the war and non war cases. Nevertheless, such a difference appears to exist to a significant degree ($\chi^2=34.0$, $df=1$, $p<<.01$) and gives support to the hypothesis stated above. No interaction is found between sex of target person and type of ESP. The data are presented in table 15.

TABLE 15
Sex of target person for war and non war cases

	male	female
war	152	25
non war	214	139
		$\chi^2=34.0$, $df=1$, $p<<.01$
total	366	164
'Phantasms'	352	193
		$\chi^2=2.2$, $df=1$

Another prediction following from the above stated hypothesis is that in the case no relationship between percipient and a target person is involved, e.g. in those cases where the percipient had a precognitive experience related to her- or himself, no difference is to be expected between number of male and female percipients. This also turns out to be the case. Although there is still a trend towards a higher number of female percipients, again most likely reflecting the more active role females play in this collection, the difference is not significant anymore (male's 76 (44.4%), female's 95 (55.6%), $z=1.45$). Also the difference between this distribution and the distribution of male (136) and female (393) percipients for cases in

which a different target person is involved is highly significant ($\chi^2=20.6$, $df=1$, $p<<.01$). In my opinion these results strongly support the hypothesis that the differences in male and female percipients are largely related to the relationship between percipient and target person, and not to differences in ESP ability.

Cases in which the experience was related to the percipient

It seems of interest to compare cases where the experience was related to the percipient, mainly precognition cases, with the cases in which the experience concerned some other person.

As has been reported above, in these cases no difference is observed between the number of male and female percipients. The distribution of type of experience does not differ from the corresponding distribution for the cases involving a target person. ($\chi^2=6.6$, $df=3$). However, a significant difference is observed when comparing the distribution of events for both types of cases. From the data presented in table 16 it is apparent that when the experience is related to the percipient serious events (death and serious accidents) are much less frequent whereas trivial cases are much more frequent.

There appears no relationship between the percipients or others contributing cases (target persons excluded) as regards reporting cases of both types (cases related to a target person versus cases related to the percipient) ($\chi^2<1$, $df=1$). As can be expected there appears a significant difference in number of details between these two types of cases ($\chi^2=28.6$, $df=2$), the cases related to the percipient being more detailed. This finding is undoubtedly a consequence of the already observed difference in number of death and trivial cases. No further findings of interest were observed.

Thus it can be concluded that when the experience concerns the percipient few differences are observed when compared to cases in which the experience is related to someone else. The main difference concerns the type of events. Percipients tend to have less frequently premonitions of death and serious accidents about themselves and more often experiences related to trivial events. It is noteworthy that compared with death and serious accidents here again serious material events play a minor role. However, considering the fact that serious events happen so much less often in the life of a person than trivial

TABLE 16
Distribution of events for cases related to some other person
and cases related to the percipient

	cases related to a target person		cases related to the percipient	
death	289	54.6%	59	31.7%
serious illness or accident	78	14.7%	20	10.8%
slight injuries	44	8.3%	22	11.8%
slight material	0	0.0%	4	2.2%
trivial	54	10.2%	47	25.3%
positive	58	11.0%	22	11.8%
chi-square=56.5, df=5, $p < .01$ (slight material excluded)				

events it can be maintained that even in these cases, in which the proportion of non-serious events (51.1%) is slightly higher than the proportion of serious events (48.9%), serious (negative) events have a higher likelihood of becoming the subject of a spontaneous experience than unimportant events.

ABSTRACT

In a previous study a quantitative analysis of a collection of spontaneous cases yielded various findings of interest, for example that the familiar phenomena of a higher proportion of female percipients, of male target persons, and of cases between near relatives, are not due to possible reporting effects. In addition strongly significant relationships were observed between different variables.

The aim of the present study was to investigate to what extent these findings are dependent on cultural differences. Therefore a similar quantitative analysis was carried out on a collection of spontaneous cases from a different country and epoch. It turned out that of the 52 analyses reported in the previous study the results of 13 analyses could not be confirmed. However, the majority of these failures to replicate are related to each other and are mainly a consequence of a few important differences between the two collections. These culturally dependent differences are as follows:

- a) The distributions of type of experience differ. In one collection waking imagery experiences dominate, while in the other dreams and intuitions are more common.
- b) The distributions of type of event differ. Although in both collections the same overall pattern was observed - a high proportion of negative events and few cases in which serious material losses are involved - there appeared to be a difference in proportions of trivial and positive cases reported.
- c) A difference in proportion of total number of females involved in both studies, of influence on the proportion of female percipients, was observed. This phenomenon is attributed to differences in the way the cases for these collections were solicited.

Contrary to the original study the collection used for the replication study involved many war cases and precognition cases. It showed that war cases increase the proportion of female percipients and the proportion of cases involving positive events, while decreasing the proportion of cases involving trivial events. In no instance was a difference observed between telepathy and precognition cases. In view of the amount of data and to assure that the results are of general validity, a discussion of the results from both studies will not be presented until a third replication study is carried out.

REFERENCES

Gurney, E., Myers, F.W.H. and Podmore, F. 'Phantasms of the living', London 1886. Reprinted Scholar's Facsimiles. Gainesville, Florida, 1970.

Rhine, L.E. 'The psi process in spontaneous cases', J.o.P., 1978, 42.

Schouten, S.A. 'Analysing spontaneous cases', Research Letter Parapsychology Laboratory, University of Utrecht, 1979, No. 9. (a)

Schouten, S.A. 'Analysis of spontaneous cases as reported in 'Phantasms of the Living', E.J.P., 1979, 2, 4, 408-454. (b)

Sannwald, G. 'Parapsychischen Erlebnissen und Persönlichkeitsmerkmalen', Dissertation University of Freiburg, 1961.

Sannwald, G. 'Statistische Untersuchungen an Spontanphänomenen', Zeitschrift für Parapsychologie, 1959, 3.

ACKNOWLEDGMENT

I am indebted to professor Hans Bender for making the 'Sannwald' collection available to me for this investigation, to Dr. Eberhard Bauer and Dr. Walther von Lucadou for their support in getting the work done, and to Jerry Solfin and John Palmer for their comments and discussions.

ERRORS AND CONFUSIONS IN ESP

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There have been many theories which attempted to understand ESP in terms of the more familiar psychological processes of memory and perception. Among them are several relating ESP to memory, the most well-known of which is Roll's (1966) 'memory theory of ESP'. I have suggested five approaches to testing the relationship between ESP and memory or perception (Blackmore, 1980a). These are 1) The study of errors and confusions made in ESP; 2) Studies of correlations between ESP and memory (Blackmore, 1980b); 3) Investigation of the effects of varying target material on ESP performance (Blackmore, 1981); 4) The use of the recall situation as a psi-conducive state; and 5) Studies of associative habits. Here I shall consider just the first of these, that is the study of errors made in ESP.

Examination of errors is a powerful method of investigating any cognitive process. An obvious example is the investigation of errors in memory, which can give clues to the underlying organisation of storage (Norman, 1969) or to the coding strategies being used. For example in a verbal learning task confusions of the words big and large would indicate semantic coding, of rough and bough, visual coding, and of court and caught, auditory coding (Baddeley, 1976). Visual illusions provide another example. Attempts to explain, say, the Muller-Lyer illusion have led to insights into the processes involved in depth perception (Gregory, 1970) and study of the errors made in vigilance tasks casts light on the underlying attentional processes (Neisser, 1967).

Numerous other examples could be given to illustrate the value of studying errors. In the case of ESP experiments errors may be very common and this could be exploited by making use of them. Since a great deal is known about the kind of errors made in tasks using perception and memory, it should be possible to compare them with the errors made in ESP.

At the most general level we should predict that if ESP resembles other cognitive processes, then the errors made should be, at least to some extent, systematic. Perhaps subjects would tend to make particular errors in particular circumstances, or when they confuse two items there might be some discernible relationship between them. This is seen in other cognitive processes. If we forget a word we can usually produce another related word or, as in the case of the tip-of-the-tongue feeling (Brown and McNeill, 1966), we can say something about it. We may have access to some of its attributes such as its first letter or number of syllables. Similarly, if we make a mistake in identifying a word, a wild flower, or even a friend, it will usually be because we have confused it with ones similar along recognisable dimensions. If ESP is like other cognitive processes we should be able to find similar patterns among the errors made.

More specific predictions may be derived from certain theories of ESP. Firstly, if ESP is seen as a process analogous to perception, with information input directly from a target, then we should expect the properties of that target to be important and the errors made to correspond to confusions based on the perceptual characteristics of that target. For example, subjects in ESP tests might confuse targets which looked similar. If this occurred then it would be possible to investigate which particular characteristics of the target were most important.

Different kinds of errors would be predicted by various models which compare ESP with memory. Several describe ESP and memory as aspects of the same process. These include the 'Psychic ether hypothesis' put forward by Price (1939), Carington's (1945) 'Association theory of telepathy' and Marshall's (1966) physical theory of ESP and memory. Since all these theories include the idea that ESP and memory involve the same process they would presumably predict that errors in ESP should resemble those in memory. However, errors made in memory vary with the situation and the task, and none of these theories is so well articulated that it would allow us to specify which type of memory task to compare with ESP. Nonetheless, they might predict that

associative rather than perceptual errors would dominate.

Even less unambiguous predictions follow from the only memory theory that has been extensively tested, that is Roll's 'memory theory of ESP'. This is based on the idea that 'The ESP response consists of the percipient's own memory traces and that the effect of the external (ESP) stimulus is to activate memory traces rather than supply new ideas or images' (Roll, 1966, p.505). Although superficially this theory appears to relate ESP and memory, I have argued that it is in fact barely distinguishable from a simple perceptual model of ESP. That is, one which likens ESP to a 'sixth sense' with input occurring as in other sensory modalities (Blackmore, 1980a). Such models face serious theoretical difficulties. It is hard to understand how the information is transmitted, how received (unless there is some undiscovered ESP sense-organ), and how transformed so as to make sense to the perceptual system. And, I believe Roll's 'memory theory of ESP' also faces these problems since it requires a stimulus (he calls it the 'ESP stimulus') to select which memory traces are to be activated. For this reason, at least, it is not easy to predict from this theory what sort of errors would be made in ESP.

Predictions are therefore not simple. None of the theories predicts precisely the types of errors we should expect, but they do predict general differences. A knowledge of the kinds of errors made in ESP would make it easier to assess the relationship between ESP and more familiar processes, and so to assess the theories. We may consider this in two stages. The first step is to establish that there may be consistency in the errors made in ESP, the second to find out what types of confusions are involved.

Several early studies reported consistency among the confusions made in ESP. Warcollier (1939) suggested that the elements of an ESP target may become scrambled and recombined, and listed various rules by which this may occur. However, his experimental methods cannot be compared with those used today and his findings may reflect his own associations rather than any intrinsic to the ESP process.

The tendency to make consistent confusions was later termed 'consistent missing' by Cadoret and Pratt (1950). They first suggested statistical methods for detecting such an effect and defined it as the 'tendency of the subject in an ESP test to mistake a given symbol for a certain other symbol to a greater-than-chance degree when making his calls' (p.244). Cadoret and Pratt were the first to suggest that a

psychological, or cognitive process rather than a motivational one could underlie such an effect. This was particularly important with respect to psi-missing. Timm (1969) suggested that consistent missing could be responsible for overall scores lower than chance, rather than some kind of negative motivation.

More recently Kennedy (1979) reviewed the literature on consistent missing. He analysed available data and sought to determine the extent of consistent missing, its relationship to scoring rates and the factors which lead to it. From data for 11 subjects he found about half showed the effect. However the results relating to scoring rate were not so clear. If consistent missing is a cause of psi-missing then one would expect to find a more consistent missing effect when overall scores are lowest, but this was not found by Kennedy. There appeared to be no reliable relationship between the two.

That consistent missing occurs at all may be evidence that ESP resembles other cognitive processes, but to investigate this further it is important to know what kinds of confusion occur. The usual analysis, a chi-square test on the complete confusion matrix, or all except for the direct hits, cannot reveal this. It can only detect an overall effect. Kennedy states "the confusions apparently can stem from factors such as the visual similarity of the targets or from the more abstract relationships of associations between targets" (p.126). In order to compare ESP with other psychological processes we need to know about these factors. Several studies are relevant here.

In studies by Rao and his associates (Rao, Morrison and Davis, 1977; Rao, Morrison, Davis and Freeman, 1977; Rao, 1978), subjects learned paired associates consisting of a trigram and one of ten words. The same words acted as ESP targets to be paired with new trigrams. Each subject gave association ranking scores for each pair of words and it was therefore possible to tell, when an error was made, how closely associated were the chosen word and the target word. Rao, Morrison and Davis found that psi-hitters chose more closely associated words than missers, on missing trials, but this was not confirmed in the subsequent studies. If confirmed this indicates the kind of lawful relationship in errors which would be expected if ESP is like other cognitive processes. It was also suggested that a relationship between ESP and memory was indicated, but this conclusion was not supported since the expected effect was not found in memory trials in the subsequent experiment.

Particularly relevant to a comparison of ESP with perception are studies by Kelly and his colleagues. Consistent missing was studied in two exceptional subjects (Kelly, Child and Kanthamani, 1974). In tests with playing cards, with the subject B.D. similarities were found between ESP errors and visual errors for both number and suit. With another subject (S.H.) no such effects were found. In a second study (Kelly, Kanthamani, Child and Young, 1975) B.D. guessed the identity of long series of playing cards, shuffled and taken from a drawer out of his sight. In an analogous visual task he was presented, tachistoscopically, with slides of playing cards and asked to identify them. Confusion matrices for the two tasks were then compared. A strong confusion structure for the visual task was obtained. For the ESP task the structure was very weak and was only extracted by the use of multi-dimensional scaling techniques, but it was found to resemble the visual structure, especially in high scoring runs. This indicated, for this subject at least, a similarity in processing between ESP and vision. But as Kelly et al pointed out, this could be due to intrinsic similarities between ESP and vision, or to the way this particular subject relies on visual imagery.

Another method for investigating errors in ESP is to design specially related targets and measure how often each is confused with another. Clock cards (Fisk and Mitchell, 1953) were designed so as to have more or less closely associated targets. Subjects could make a direct hit on the number or could make a near miss by choosing one close to it on the clock face. But though this method used related targets they were not related in the ways of interest here. Nash and Nash (1961) also designed specially related targets. They used eight animal pictures which were in groups of four mammals and four birds, and in pairs of very similar animals. However, they found no consistent confusions.

These few studies provide some evidence on ESP errors, but it is very limited and the results have rarely been consistent. If we are to compare ESP with the psychological processes of memory and perception, more specific information is needed. As a start, a simple question which could be answered by experiment is 'Are the errors made in ESP based on perceptual or associative cues?' In other words are perceptual or associative errors predominant in ESP?

Of course there may be no simple answer to this question. Both types of error may occur in different circumstances or in subjects who habitually make different types of error in memory or perception. Even

if an answer were obtained, we should not be able to decide unequivocally between the theories. Nevertheless, with even a partial answer we should come closer to understanding the relationship between ESP and psychological processes. Accordingly the experiments reported here were addressed to this question.

There are various methods which can be used to investigate errors and confusions. One has already been described, that used by Kelly et al (1975). The comparison of confusion structures in this way is potentially a very powerful method but was not used here for three reasons. Firstly it depends on highly complex statistical techniques which, apart from the work involved, may introduce doubt as to whether the stringent assumptions on which they depend have been met (indeed Kelly et al comment on this problem in their analysis). Secondly this study used only one subject. If models of ESP are to be tested more general principles need to be explored, testing many subjects. However, it should be noted that any such approach depends on assuming that the errors or confusions made result from general characteristics of the ESP process rather than individual idiosyncracies. Finally Kelly et al used playing cards as targets, but these, or ESP cards, will produce confusion structures which are unlikely to be meaningful or to shed much light on underlying processes. Indeed, the ESP cards were specially designed not to be easily confused. Instead specially designed targets are needed. Nash and Nash (1961) and Rao (1978) used specially designed targets but used relationships between targets which are not relevant to the question posed here. Bearing these points in mind it was thought preferable to test many subjects, to manipulate the relationship between targets and to use somewhat simpler statistical techniques.

The method chosen was to incorporate into ESP tasks the opportunity for subjects to make specific types of error or confusion. For example, associated targets and targets which looked similar were devised and the numbers of confusions between each type were recorded. It was hoped that if preliminary experiments showed a preponderance of one type of error it would be possible to examine further the variables influencing those errors.

Three pilot studies were carried out. They were performed quickly using large numbers of subjects with only one target order. They were therefore subject to a stacking effect but it was hoped that they would provide hypotheses to be tested in further experiments. Because these studies suffered from various flaws they are only described in

outline here.

PILOT STUDY 1

Eighty-nine students in a parapsychology class took part in a GESP task. Nine pictures were designed in groups of three so as to incorporate two possible types of error. In each group one key word was related to one other by association and one by visual similarity. For example the key picture of a leaf was related to that of a tree by association and to that of a fish by visual similarity. It was therefore possible to see how many direct hits were made and how often subjects chose each type of related picture.

An assistant (KK) prepared the target sheet from random number tables, sealed it in two opaque envelopes and took no further part in the experiment.

An agent and an assistant were chosen from among the students, left the lecture theatre and locked the doors. The assistant then opened the sealed envelope and gave the pictures to the agent to look at one at a time at four minute intervals. Timing was coordinated by the experimenter who controlled a light switch.

Meanwhile the experimenter explained the task to the remaining 89 students. They were told that the agent was going to look at a series of 9 pictures and they were asked to try to imagine what was being looked at and draw a picture on a prepared sheet. There was one run of nine trials. After all the drawings were completed the subjects were shown the 9 target pictures, each lettered A to I and asked to decide which was target for each of their own drawings and to mark them accordingly with the appropriate letter. The task was thus equivalent to a forced choice task although the subjects did not know this at the time of their drawing. It was hoped that this method would be more conducive to the operation of ESP than a simple forced choice procedure while allowing the same simplicity of analysis.

RESULTS

The complete results are best shown in a 9x9 confusion matrix (see

appendix 1). The most important results concern the comparison between the numbers of direct hits (type 1), the two special types of error (types 2, associative, and 3, perceptual error) and misses. Spurious results could be obtained if one did not take account of the differential popularity of the various targets. This is allowed for by calculating the expected number of responses for each cell of the matrix by dividing the total number of times any picture was chosen by 9 (the number of targets). Expected means for hits and errors can then be calculated and compared with the obtained means. Note that these are means for each target. The results for all subjects have been pooled. These results are shown in table 1. It should be noted that the degrees of freedom vary because there are 9 possible ways of making a direct hit, but only 6 each of the two kinds of error.

TABLE 1
Results for experiment 1 pooled for 89 subjects

Hits type	Total	Mean	Expected mean	t	df	p
1	80	8.89	9.9	0.99	8	0.35
2	77	12.80	9.6	3.48	5	0.02
3	77	12.83	10.0	1.30	5	0.25
Comparison of 2 and 3				4.91	2	0.04

There are no more direct hits or type 3 hits than would be expected by chance but there are significantly more type 2 (associative) errors than expected ($t=3.48$; $df=5$; $p<.04$). In addition for the key pictures only, a direct comparison can be made and this shows that there were significantly more type 2 (associative) than type 3 (perceptual) errors. This may appear to support the hypothesis that errors made in ESP more closely resemble those made in memory than in perception, but inadequacies in the experimental design make such a conclusion unwarranted.

Firstly all subjects were tested simultaneously using the same target order. It is possible that this order itself produced the particular distribution of hits and misses. A stacking effect like this can lead to an overestimate of the significance from conventional statistics. This can be dealt with by various methods such as the laborious procedure of calculating the true variance (Greville, 1944) or using a majority vote technique or index of preference (Thouless and Brier, 1970). In this case the latter would reduce the data so far that no conclusions would be possible. It is in fact far preferable to eliminate this problem altogether and to do this multiple target orders should be used preferably a different one for each subject. This was not possible here for a GESP task but the second pilot study used several target orders rather than one and it was hoped that this would be an improvement.

Secondly the target pictures were not ideal and could be improved, especially since the relationship between them was unknown. It would be preferable either to measure associations for the stimuli or to use those of known association value. This is easier for words than for pictures. The main experiment used words as targets.

Thirdly in this experiment three key targets and six others were all presented as possible targets to the subjects. This method means that special allowances have to be made for preferences of each type to each target which not only complicates the analysis but may introduce a possible source of error.

Other designs are possible. For example only the key pictures might be presented to the subjects while all the pictures are used as targets. This would avoid the complications noted above but would change the experiment from a study of errors (since a direct hit would in any case be impossible on many trials) to one on confusions, or the effectiveness of different types of targets. The main experiment described here used this method, as did a later one using child subjects (Blackmore, 1980d).

Fourthly, although the subjects were told that the selection of targets was random, they might nonetheless feel constrained to use one of each, especially since there were nine targets and nine trials. This problem of dependence of responses would be much less if more trials were used, as in the later experiments.

Finally in this experiment the subjects marked each others' answer

sheets. Obviously this introduces the possibility of cheating, though this was reduced by various simple measures. It could of course be eliminated altogether by marking the sheets in the subjects' absence. However, for this experiment it was thought more important to give immediate feedback. Some of these problems were eliminated from the later experiments.

PILOT STUDY 2

The same target pictures were used as in the previous experiment but there were two main differences in procedure. Firstly there were four runs with a different target order for each, instead of just one run. The targets were again chosen from random number tables by an assistant (KK) who prepared the pictures, sealed them in opaque envelopes and then took no further part in the experiment. Secondly a quicker forced choice procedure was used instead of the time consuming procedure used previously. Subjects were 84 students in a parapsychology class.

An agent and an assistant were chosen from among the class and left the lecture theatre, the assistant being responsible for locking all the doors and presenting the targets to the agent at the right time.

Meanwhile the subjects were given an answer sheet, the 9 target pictures were shown throughout on the overhead projector and subjects were asked to record which they thought was target for each trial. There were four runs of 9 trials each. Trials were at 45 seconds intervals with a few minutes break between runs. Timing was synchronised with a light operated by the experimenter as before.

RESULTS

The complete results can be seen, as before, in a 9x9 confusion matrix (see appendix 2). This shows the results pooled for the 4 runs and for all subjects. The important comparisons are shown in table 2. All means and expected means were calculated as for the previous experiment.

It can be seen that the significant results found in experiment 1

TABLE 2
Results of experiment 2 pooled for 84 subjects

Hits type	Total	Mean	Expected mean	t	df	p
1	360	40.0	37.3	1.25	8	0.25
2	219	36.5	37.9	0.88	5	0.42
3	230	38.3	37.4	0.39	5	0.71
Comparison of 2 and 3				0.12	2	0.92

are not replicated here. There are no significant differences either between the obtained and expected means or between the numbers of the different types of errors. Neither hypothesis is supported.

DISCUSSION

The results of this experiment do not support those of experiment 1. One possible conclusion is that the latter were due to uncontrolled target order and were spurious. However, the differing results could be due to other differences between the two experiments. Most important among them being the difference in procedure. Experiment 1 used an ostensibly free response procedure while 2 used a forced choice method. Since this difference might account for the difference in the results, the two procedures were tested and compared in a further experiment.

PILOT STUDY 3

Sixty five students in a parapsychology class took 2 ESP tasks. They were divided into two groups. Group 1 (N=41) took the free response

test (as in experiment 1) first followed by the forced choice task (as in experiment 2). Group 2 (N=24) took the same tests in reverse order. The groups were of different sizes because they depended on volunteers.

For the free response method at least it is desirable to have pictures which correspond closely with subjects drawings. Therefore 53 drawings from experiment 1 (801 drawings in all) were consulted and 5 pictures designed to be most representative of these. These were used as targets in both tests.

Target orders were chosen from random number tables and the pictures were sealed in opaque envelopes by an assistant (KK) who took no further part in the experiment.

The procedures used were the same as those for the previous experiments. Each test consisted of one run of 10 trials. Between the two tests there was a short break.

RESULTS

The MCE for each run is 2.0. With results for all subjects in both groups pooled the obtained means were as follows:

Test a	'free response'	M=2.09
Test b	'forced choice'	M=2.05

Neither of these means is significantly different from MCE and there is no significant difference between them ($t=0.23$; $df=64$; $p=0.82$). There is no indication that either method is more effective.

DISCUSSION

Neither of the methods used here produced any evidence of ESP occurring and there was no obvious difference between them in effectiveness. The most parsimonious account of all the results so far is that the significant effects in experiment 1 were spurious and a result of a stacking effect, and no ESP occurred in any of the three experiments. Of course other interpretations are possible. Differences

in the procedure, experimenter's expectation or subjects may have produced positive results on the first experiment but not on the others, but such a conclusion cannot be justified on the basis of these results.

One further experiment was carried out which incorporated various improvements suggested by the previous experiments. Words rather than pictures were used as targets, the method of confusions with only key words used as responses was adopted, and all subjects had individual target orders.

Like the pilot studies this experiment investigated the relative importance of meaning (or association) and visual appearance of ESP targets. The aim was to determine whether ESP is more effective when the target is identical to the response, or when it is related by association or by visual similarity. This experiment differs from the previous ones in several respects, to be discussed below.

MAIN STUDY

METHOD

Subjects were 59 students in a parapsychology class.

Targets were words. There was a pool of 12 response words given to the subjects, but a larger pool of 36 target words. Twelve of these were identical to the response words (type 1 targets), 12 were related by association (type 2) and 12 looked similar (type 3). It should be noted that this differs from the pilot experiments in which the response and target pools were identical. This method was preferred because it eliminates the necessity to allow for preferences. However, it also means that the experiment is strictly one comparing target effectiveness, or confusions rather than errors.

Target lists consisted of 72 words, each of the 36 target words appearing twice. Individual lists for each subject were prepared by computer and sealed in envelopes by an assistant (TT) who took no further part in the experiment. Examples of the response and target words can be seen in table 3.

TABLE 3
Examples of target and response words in the main experiment

Response words	Target words		
	Direct hit type 1	Associative hit type 2	Perceptual hit type 3
black	black	white	slack
cat	cat	dog	cap
love	love	hate	live

PROCEDURE

Subjects were given a response sheet with the 12 response words, 72 spaces for their responses and a sealed envelope containing their own target list. They were asked to use their clairvoyant abilities to 'see' what was in their own personal envelope and to write their answers on the prepared sheet in their own time. When all had completed the list (about 15 minutes) they were asked to swap sheets with a neighbour before checking. No subject was allowed to open any envelope until this time and the experimenter checked that this rule was obeyed.

The rationale behind the experiment and the method of scoring were explained to the subjects and the complete list of words shown to them. They then marked each others' sheets and the results were roughly calculated. All sheets were double checked at a later date.

RESULTS

Obtained and expected means are shown in table 4. Overall scores pooled for all subjects and for all three types of hit, did not differ

TABLE 4
Hits of three types in the main experiment

Hit type	Total	Mean	Expected mean	t	df	p
1	93	1.58	2.0	3.14	58	0.003
2	116	1.97	2.0	0.09	58	0.93
3	138	2.34	2.0	1.83	58	0.07
Total	347	5.88	6.0	0.29	58	0.77

significantly from MCE. However, there were differences between the numbers of hits scored on the different types of targets. Direct hits were significantly below MCE possibly showing psi-missing, while type 2 hits were close to chance and there were most type 3 hits. A one-way ANOVA shows a significant effect of word type on the number of hits ($F=4.95$; $p<0.01$). Additional comparisons were made between the different hit types. Only the comparison between type 1 and type 3 hits shows that there were significantly more type 3 hits than direct hits (see table 5).

DISCUSSION

On a simple hypothesis, assuming ESP to operate like known psychological processes, one might expect to find most direct hits with fewer of the other types of hit. The order of effectiveness of the types would be either 1-3-2 or 1-2-3, and might indicate that either perceptual cues or meaning were more important. However, the results obtained (3-2-1, with direct hits below MCE) highlight the impossibility of making such assumptions when dealing with ESP. In particular the possibility of psi-missing has been ignored. If this is taken into consideration a number of other arguments become possible

- 1) Since there are most type 3 hits it could be simply concluded that perceptual cues are more important for ESP.

TABLE 5
Comparisons between types of hits for the main experiment

Hit types compared	t	df	p
1 and 2	1.91	58	0.06
1 and 3	2.95	58	0.005
2 and 3	1.40	58	0.17

2) Since there is evidence for psi-missing in the experiment it could be assumed that any ESP occurring acted negatively and so since there were fewer type 2 hits, meaning or association is more important than visual appearance.

3) Since only direct hits differed significantly from MCE it could be concluded that psi only operated when the target was identical to the response and not when it was merely associated or visually similar.

These arguments lead to quite different conclusions. To decide which argument to adopt one needs a model for the operation of psi and psi-missing. Many have been suggested such as by Scott (1961) and by Palmer (1975). However, none is universally accepted and I had not decided, prior to the experiment which model I intended to use. It therefore seems that no definite conclusions can be drawn from the results obtained. The results highlight the fact that possibly untenable assumptions were made in designing the experiment. However, without making some such assumptions the experiments could not have been designed at all. Without more knowledge of the operation and limits of psi, experiments such as this may be incapable of providing meaningful data and so perhaps should not be attempted. Less pessimistically, it could be argued that with more data from similar experiments a regular pattern might emerge and this might make meaningful conclusions possible.

DISCUSSION AND SUMMARY OF RESULTS

The experiments reported here aimed to answer the question: 'Are the errors made in ESP based on perceptual or associative cues?' The number of direct hits (type 1) were compared with the number of errors or confusions made with targets related by association (type 2) or by perceptual similarity (type 3). Is it now possible to answer the question on the basis of the results obtained?

Direct comparison is not strictly fair since the experiments varied in procedure and design. Nonetheless an indication can be given by looking at the order for the hit types in each case. This is shown in table 6, which should show whether any systematic pattern emerges.

TABLE 6
Results of three experiments on errors or confusions in ESP

Experiment	N	Test type	Overall ESP?	Above or below MCE?	Order of hit types	Sign. effect of order?
1	89	GESp	No	below	2-3-1	yes: 2>3
2	84	GESp	no	above	1-3-2	no
3	59	Clair	no	below	3-2-1	yes: 3>1

Clearly it does not. Indeed the order is different in each case and there is no obvious pattern.

Experiment 1 may be excluded because of the faulty method used. This leaves two adequate experiments providing very different results. The order of hit types is different in each case and although one of these differences is significant it is only one among six similar analyses. In neither was there any evidence of ESP occurring and the most parsimonious account of the results seems to be that they were entirely due to chance. If this is the interpretation accepted we can only conclude that it was impossible to investigate the types of error

made in ESP in the absence of any ESP.

ABSTRACT

If ESP is related to more familiar cognitive processes, the errors or confusions made between targets should show consistencies. For example, they may resemble those made in perception or memory. If so this could throw light on theories relating ESP and memory. Previous studies of errors in ESP are considered and four experiments are reported. Two used special picture targets between which confusions of either association or visual similarity could be made. The first produced more confusions between associated pictures but the method proved to be flawed. A second study using a different design showed no significant differences. In a third a comparison of the two designs was made but no difference was found. None of the three experiments provided significant extra-chance scoring. A final experiment used words as targets, related to each other by either visual similarity or by meaning. Again no significant overall scoring was obtained. Although there were significantly more visual confusions than direct hits this unexpected finding must be considered in the context of the non-significant ones. The conclusion reached was that chance best accounted for the results. From these results it was not possible to determine whether visual or associative confusions would predominate.

REFERENCES

- Baddeley, A.D. 'The psychology of memory', Harper Row, London, 1976.
- Blackmore, S.J. 'ESP as a cognitive process', unpublished Ph.D. thesis, University of Surrey, 1980. (a)
- Blackmore, S.J. 'Correlations between ESP and memory', EJP, 1980, 3, 2, 127-147. (b)

Blackmore, S.J. 'The effects of variations in target material on ESP', Research Letter Parapsychology Laboratory, University of Utrecht, No. 11, 1981. (c)

Blackmore, S.J. 'A study of memory and ESP in young children', J.SPR, 1980, 50, 501-520. (d)

Brown, R. and McNeill, D. 'The 'tip of the tongue' phenomenon', Journal of verbal learning and verbal behaviour, 1966, 5, 325-337.

Cadoret, R. and Pratt, J.G. 'The consistent missing effect in ESP', JoP, 1950, 14, 244-256.

Carington, W. 'Telepathy: An outline of its facts, theory and implications', Methuen, London, 1945.

Fisk, G.W. and Mitchell, A.M.J. 'ESP experiments with clock cards: A new technique with differential scoring', J.SPR, 1953, 37, 1-13.

Gregory, R.L. 'The intelligent eye', Weidenfeld and Nicolson, London, 1970.

Greville, T.N.E. 'On multiple matching with one variable deck', Annals of mathematical statistics, 1944, 15, 432-434.

Kelly, E.F., Child, I.L. and Kanthamani, H. 'Explorations in consistent missing', JoP, 1974, 38, 230-231 (abstract).

Kelly, E.F., Kanthamani, H., Child, I.L. and Young F.W. 'On the relation between visual and ESP confusion structures in an exceptional subject', J.ASPR, 1975, 69, 1-31.

Kennedy, J.E. 'Consistent missing: A type of information-processing: error in ESP', J.o.P., 1979, 43, 113-128.

Marshall, N. 'ESP and memory: A physical theory', British journal for the philosophy of science, 1960, 10, 265-286.

Nash, C.S. and Nash, C.B. 'An ESP experiment with targets that differ in degree of similarity', J.ASPR, 1961, 55, 73-76.

Neisser, U. 'Cognitive psychology', Appleton-Century-Croft, New York, 1967.

Norman, D.A. 'Memory and attention', Wiley, New York, 1969.

Palmer, J. 'Three models of psi test performance', J.ASPR, 1975, 69, 333-339.

Price, H.H. 'Haunting and the 'Psychic Ether' hypothesis', Proceedings SPR, 1939, 45, 307-343.

Rao, K.R. 'Further studies of memory and ESP', JoP, 1978, 42, 167-178.

Rao, K.R., Morrison, M. and Davis, J.W. 'Paired associates recall and ESP: A study of memory and psi-missing', JoP, 1977, 41, 165-189.

Rao, K.R., Morrison, M., Davis, J.W. and Freeman, J.A. 'The role of association in memory-recall and ESP', JoP, 1977, 41, 190-197.

Roll, W.G. 'ESP and memory', International journal of neuropsychiatry, 1966, 2, 505-521.

Scott, C. 'Models for psi', Proceedings SPR, 1961, 53, 195-225.

Thouless, R.H. and Brier, R.M. 'The stacking effect and methods of correcting for it', JoP, 1970, 34, 124-128.

Timm, U. 'Mixing-up of symbols in ESP card experiments (so-called consistent missing) as a possible cause for psi missing', JoP, 1969, 33, 109-124.

Warcollier, R. 'Experiments in telepathy', George Allen and Unwin, London, 1939.

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APPENDIX 1

Results of experiment 1. Frequency of responses to each target,
pooled for 89 subjects on 9 trials

Targets	Responses	A	B	C	D	E	F	G	H	I
A		10	13	15	10	5	8	5	14	9
B		9	8	11	9	15	7	11	12	7
C		12	6	5	10	8	12	7	16	13
D		5	7	8	8	16	15	8	9	13
E		11	5	12	5	12	6	14	16	8
F		9	5	10	10	5	7	12	14	17
G		9	22	12	6	11	6	6	11	6
H		2	10	9	12	17	8	13	8	10
I		17	8	5	6	10	10	13	4	6
Totals		84	84	87	76	99	79	89	104	99

APPENDIX 2

Results of experiment 2. Frequency of responses to each target,
pooled for 84 subjects for 4 runs

Targets	Responses	A	B	C	D	E	F	G	H	I
A		42	33	41	33	35	32	43	37	40
B		42	52	35	44	36	33	43	26	25
C		30	35	49	39	36	40	32	37	38
D		51	32	34	36	37	36	39	37	34
E		48	35	28	35	32	35	40	39	44
F		33	38	46	37	44	42	35	30	36
G		32	43	47	44	35	32	37	30	36
H		42	40	29	37	33	41	34	40	40
I		27	45	46	44	42	38	34	30	30
Totals		347	352	355	349	330	329	337	306	318

THE HEALER: PROBLEMS, METHODS AND RESULTS

Piero Cassoli M.D.

Terminology and definition

When we are talking or writing about 'healers' it is very important, today more than ever, to reach an agreement on the meaning we shall give to this word. This is the first of the problems I will try to face here shortly.

For most people, scientists or not, this word means all of those who, outside of orthodox Medicine, work for the healing of those who suffer. For this reason, this term still indicates those 'wizards' (called 'mediconi') of our countries that use all kinds of magical or clearly religious practices. Sometimes they employ monotonous songs and sometimes pharmacopoeias of different kinds. These pharmacopoeias go from remedies used by physicians for the simplest of medical treatments, to unclean concoctions in which urines, blood, (menstrual or not), spider's webs, worms, insects, etc., are used as essential components.

But unfortunately this term mistakenly encompasses, due to the insufficient agreement on this word 'healer', all of those who, with quite a different dignity, competence and theoretical basis, carry on a curative activity that makes them 'heretics' of Medicine. For example, there is Samuel Hanemann's Homeopathy, and the very old therapeutic practice of 'Acupuncture'.

Among the 'healers' are also included the 'Chiropractics' (those who manipulate, handle the bones) whose activity is not mysterious or magic. In fact it refers to the correction of some dislocation of the bones, especially vertebral dislocation that can produce a compression on some nerves or vessels. The list of 'heretics of Medicine' could grow longer if we think that psychoanalysis is today still considered by some a mystification and the unconscious a useless conception. Hypnosis also is still considered something heretical since only recently has it entered into the University establishment and not without difficulties. In view of this complete confusion in terminology it is important that we agree on the meaning of the term 'healer'. Personally I wish to study a certain type of healer, that who in my opinion as a parapsychologist, is linked with parapsychology itself. The type of healer I have studied can be defined as a person who seems to have influence on the state of disease in human beings, and perhaps in animals and plants, by the unique imposition of his hands on the patient's body, at a brief distance or with direct contact, without using other means.

At once an interesting and elegant question arises in Italy and in other parts of the world. Judicial and common law add to this definition a fundamental side-note: "without being a Doctor of Medicine". The Supreme Court in Italy recognizes those who hold a degree in Medicine and a license to practice (Esame di Stato) and gives them sanction to practice Medicine with freedom to use every means of cure. Therefore an M.D. can treat patients by the laying-on-of-hands as long as he operates with competence, prudence and conscience.

There are several other problems the researcher must face. The second problem is the most important and I would say prejudicial, and, if it is not solved, makes vain all the others.

Problem II: Does the healer cure or, at least, relieve pain, even if only temporarily? In other words, can the healer interfere in a positive sense on the course of an illness and more particularly on those factors that determine its course, e.g. micro-organisms?

Problem III: In the case of a positive answer, how does the patient heal? Through what ways and means does he operate? (considering that they must be 'his own' means)

Problem IV: Is it legal? Here in Italy there is this problem, both for

people interested in research and for patients who intend to apply to a healer.

Problem V: This is an epistemological problem. Is the research on the competence of healers a part of Medicine or of Parapsychology?

I'll not discuss here the fourth and the fifth problem. I am going to dwell especially upon the most important one from the human and scientific point of view. This is the second problem: can the healer interfere in a positive sense on the course of illness and particularly on factors determining it? This raises another important problem that is connected to this one: If the answer to this second question is positive, does the healer act in connection with some illnesses only? And in particular does he act in connection with psychosomatic diseases only?

METHODS

Using what kind of methodology can we work out the fundamental second problem? Till now a reliable and documented answer hasn't been given. We must keep in mind that this answer comes before laboratory research. If there is no clinical documentation for healing, all laboratory research is, from the clinical and human point of view, speculative only, even if it can be considered very important from the scientific (heuristic) point of view. While cases have been reported by the hundreds, almost every report has only an anecdotal value. On the other hand to deal with this fundamental problem: 'does the healer heal?', we must consider some objections that have been raised in response to those who say that it does.

First objection: The reports are not trustworthy, they don't correspond to the real course of the events, they are emphatic, or they have been altered, consciously or not.

Second objection: The reports are trustworthy, but the diagnosis was wrong: in fact it was a question of quite another illness commonly curable or spontaneously clearing up.

Third objection: The report and the diagnosis are correct, but it is a question of a temporary improvement, independent of the healer's action and typical of that illness (for instance, the case of multiple

sclerosis).

Fourth objection: The illness was actually one of those considered incurable (typical case: carcinoma) but it was a question of an exceptional natural recovery.

Fifth objection: The healer operates always on 'nervous illnesses' (we would say 'psychogenic' or 'psychosomatic' illnesses).

It is plain that, until the experimenters respond to those objections, we will not be able to say to the patients: "yes, the healer heals". First of all we must agree on this: when a Medical Doctor prescribes a treatment to a patient, we don't claim this treatment always leads to a recovery. It is often sufficiently satisfying, for example, that the treatment interferes favourably on some symptoms, such as the pain, appetite, conditions of nutrition and sleep. Last but not least there is the psyche, the Kinesthesia of the patient, giving him again confidence and serenity. This brings us back to the first problem I mentioned since we must not be conditioned by the term 'healer'. It would be a good thing to change it for the term 'pranotherapist'. It doesn't mean anything in a scientific sense, but it gives the idea of a person who treats and not necessarily heals or cures. However we must clearly keep in mind that a sufficient motivation for scientific and clinical research is the fact that a pranotherapist brings a real relief to those who suffer without using medicines or other physical or chemical means.

Now we again take the five objections listed above into consideration to see what the rare and exceptional researchers have done to solve them and, above all, what we can propose from the point of view of methodology to the researchers to come. In 1954 in Saint-Paul-de-Vence, Prof. Robert H. Thouless, a psychologist at Cambridge and past-president of the British S.P.R., proposed a research method that, by itself, would help to solve all five of the objections raised at the methodological level. In substance Thouless proposes N pairs of patients be selected, each composed of two patients with similar diagnosis, similar course of illness, and similar prognosis. The diagnosis must be sure and completely documented. The healer will operate on one member of each couple, while the other serves as control.

In this report Thouless settles very quickly the great problem of clinical methodology (diagnosis, course and prognosis). He isn't an

M.D. and obviously is interested in statistical questions only, but his proposals or suggestions remain for now the optimum for answering the principal problem. Obviously they are irrelevant for solving the 'how' question, since in this kind of research there isn't any possibility of using a double blind method and therefore we cannot leave out the problem of suggestion. Thouless discussed his suggestion in the Conference organized by the Parapsychology Foundation on 'Unorthodox Healings' (1954). But unfortunately the Conference had only one important result, a research carried out in 1955 at Freiburg by prof. Hans Bender with medical consultation of prof. Sarre of the local medical Clinic. The healer was dr. Kurt Trampler, a german sociologist. The study showed that out of 538 people submitted to the healer's action 39% improved, 22% had a temporary improvement, 29% showed non modification and 10% worsened. These figures were for a subjective assessment. For the objective data on 247 cases that were followed up, 9% had an improvement, 75% non modifications and 14% a worsening. When the researcher tried to see whether the improvements happen more in organic or functional diseases, he found no difference. Did prof. Bender answer or try to answer what I have called the fundamental problem? I don't know.

Here we are in 1981. Twenty-six years have elapsed since the conference: a quarter of a century. In these years reports at an anecdotal level have continued to be published in newspapers and magazines, but documented reports of controlled studies are very rare. Ramakrishna Rao wrote in 1975 that all the reports of healings taken together and separately, do not succeed in providing satisfying evidence. Jeffrey Mishlove in 1977 wrote: "Part of the evidence which refers to such healings must be considered with caution because research has been conducted in an informal manner without adequate references about experimental controls". What must we do if there aren't now, and may not be in the future, university clinics favourable to carry out the kind of research suggested by Thouless?

I will make an easy precognition: if we consider the attitude of most of my medical colleagues at the university or at hospitals, before we find a willingness there to carry out such research, there will have to arrive at the chair the young people of today, those that live the deep crisis of the social medicine and are just now getting their degrees. This is approximately the space of one generation, still 25 years. But in the meantime pranotherapists, law allowing or not, go on operating, and, against medical expectations, they continue to seem to influence illness (or patients) favourably. Therefore shall

we leave things go on as now, namely that the desperation of those who suffer 'feeds' the work of honest healers, but of a growing stream of profiteers as well? Personally I say 'no' resolutely to this attitude. We find it also in scientists and critics who are fearful of their reputations or of parapsychology. We must persevere in this research even with our very limited means.

First of all we mustn't be frightened about objection five that the healer works and operates only on psychogenic or psychosomatic illnesses. In a preceding report of mine I gave an enumeration of controlled cases (June, 1975 issue of the Italian review 'E.S.P.'). This objection was perhaps the most important for me. Therefore I chose to communicate results with organic diseases like epilepsy, mongolism, oligophrenic cerebropathies demyelinating diseases, progressive muscular dystrophy and so on. Many years have passed by and my experience in this field has been growing more and more. Everyday I am more convinced that psychological factors always play an important role in disease. There are some stimulating works I have quoted in my book concerning psychogenesis (or psychogenetic components) of malignant neoplasmas. There is also some striking data reported in the British Medical Journal (1975) that seems to indicate that some patients are able to 'postpone' the date of their death. A study of patients beyond 75 years old, for whom every birthday may represent a tangible symbol of victory over death, showed the smallest incidence of death, statistically significant, in the month before their birthday.

Therefore I renounced little by little this standard of preferential choice of organic cases and I tried to find clinical cases that could satisfy, methodologically, the first four objections given above. I tried to get a diagnosis as certain as possible (valid clinical and instrumental documentation) and followed these patients for a more or less long time depending upon the prognosis of the disease. I tentatively proposed a classification of my cases into nine classes. Classification is based principally on the confidence in the diagnosis. I will continue increasing this case record of treatment by healers. It is the first, to my knowledge, in the world and I am hoping, but without confidence, that others will join me in this research. It is hoped that we will be able to find a scientific journal disposed to publishing these cases, so that we can increase this case record and encourage others to take this research into consideration.

There remains further discussion of the third problem: "If the healers heal or improve illness, by what means do they act? I began to answer this question in my book 'The healer' with a complete review of experimental work in laboratories. Each kind of research has its problems, its methodology and its results. It would be perhaps damaging to report here too briefly the laboratory research. I will say only that we can see the researcher's purpose, expressed or not, of demonstrating something coming out of the healer's body that is not attributable to suggestion. This common objective, from Mesmer onwards, to the results of so called 'pranotherapy', maintained and maintains the secular diatribe between psychologists and 'fluidists'. The laboratory research is more like explorative or pilot study than of experimental research. Some of these studies remain well documented and await the interest of all who have the means to do so. There is also a moral and institutional duty to do so: and this is the responsibility of the university. In my book, and I apologize for once more quoting myself, I reported on the different kinds of laboratory research and, moreover, I gave some orientations and proposals for new methodology. I'll mention here briefly the different fields: Mummification - Action on growth of plants and/or seeds (in both senses of encouraging or blocking it) - Effect of healer on wounds on animals - Effect on microorganisms (favouring or preventing their growth) - Effect on biologically active material (enzymes and hemoglobin) - Effect on photographic plates - Effect on Kirlian photography.

DISCUSSION

I have published my clinical results and the experimental ones are referred to in my book: if we don't find them convincing or definitive, we can at least say that they are promising and stimulating.

It seems to me that this is not the right place to communicate extensively the detailed clinical descriptions of some demonstrative cases. What I want to communicate is my deep belief that 'pranotherapy' is indicated as at least a support in every kind of disease and for its effect on pain, energy level and appetite; in other words, its general effects. As to its effect on pain, the clinical results seem to confirm a precise and often strong activity. Recently, a discovery was made (1975) of substances, the endorphines,

which act against pain and are much more potent than morphine and produced by the organisms themselves. This has led some researchers to the supposition that both acupuncture and pranotherapy could stimulate the production of these substances. This interesting hypothesis doesn't convince me, because the healer's action lasts too long. However, if this explanation should prove to be true, we might hypothesize that the healer reactivates the natural mechanisms for releasing such substances, and returning the organism to its normal production of them. This interpretation fits with other reactivations the healer is able to produce and is more plausible to me.

Considering the healer's action from the antalgic point of view, we have brilliant results on chronic incurable headaches in many women, on arthritic and arthrosic processes, on neuralgias refractory to treatment (trigeminal, sciatica, occipital, intercostal; the last one especially post-herpetic), on pains from metastasis of malignant neoplasmas, and on colics of every kind (biliary, menstrual, renal). It is a pity that these indications and suggestions are very often ignored, because we are conditioned to take a chemical remedy against pain for months or even for years (the dependence on Optalidon is well known) rather than to call a healer.

Quite recently I have observed pain relief in coronaropathies, but the scientific - or even clinical - demonstration of this last result is possible only with a large sample. This possibility can only be realized in hospitals or in university institutes. Further, it is possible to make a listing, based on my clinical experiences, of the diseases which seem to have received the most benefit from pranotherapy. First there are the Epilepsies. I believe that every case of epilepsy should be submitted to an expert 'pranotherapist', controlled by an M.D.. In cases of epilepsy, as in other diseases, there are some pranotherapists, who, as their first act, ask or order their patients to stop taking 'those nasty things' - nasty things are those blessed remedies used in epilepsy that have made possible a completely normal life for patients. But those remedies seldom lead to a complete recovery and this is the reason that I suggest every case of epilepsy should be submitted today to pranotherapy, even better it should be submitted to more than one pranotherapist and pranotherapy in case of initial failure.

I would further suggest the use of pranotherapy in every case of 'sepsis' with chronic course, typically found in urinary infections. Similarly, I recommend to persevere wherever there is a suspicion of

latent microbiosis (sinusitis, adnexitis, chronical otitis and synusitis, chronical appendicitis). Verrucas or warts often find a definitive relief, as they do also by hypnosis, while medical treatments are disappointing and painful. I would point out that some handicapped children, even mongoloids, have received considerable help from pranotherapy. On the contrary it seems to not be worth while to attempt pranotherapy in some neurological diseases like spastic cerebropaties, and in some degenerative illnesses of the nervous system like lateral amiothropic sclerosis. I mention these because I have found many cases of these hopeless patients who were treated by pranotherapists. There were never any positive results, even temporary. I would always try pranotherapy if it is early enough, in the so called and frequent muscular progressive dystrophias. Here I venture to say I have got some results in malignant neoplasmas. Against the scepticism of many people, I think this illness must be treated from the beginning by selected pranotherapists as a support for orthodox treatments, especially after a destructive operation like mastectomy. Cases of this kind, where patients have been relieved of pain, are steadily increasing. One must have personally experienced what it means to have a member of their family hit by this terrible disease, destroyed by pain and morphine, to understand why just the symtomatic action of relief from pain is considered providential and irreplaceable.

I wish to offer a few final words on the interpretation of the phenomenon. We can offer three models for consideration by scientists:

1) A physical model that involves or supposes something flowing out of the body and physically demonstrable. This seems to me beyond our reach at this stage of the research.

2) A psychological model that finds its fundaments in research on intervention via the phenomenon of suggestion and in research on 'placebo effects': this seems to me reasonable and tenable in many cases. With regard to the phenomenon called 'placebo effect', recent findings about drugs, such as heroine, and about endorphines have lead to the unsuspected result that the 'Nalaxone' antagonist of heroine, blocks the 'placebo effect'. With regard to the hypothesis of the intervention of suggestion, I ask myself how, when an idea is somehow suggested, verbally or otherwise, does it 'travel' in the meanders of the mind and how could it result in recoveries such as I have seen. In any case, the hypothesis stands firmly.

3) The parapsychological model. We suppose here a psychokinetic effect. This is valid in the light of experimental laboratory research. It is within the boundaries of these two models - psycho and parapsychological - that young researchers must go. Personally, I think that 'healing' is a question of something which is strictly connected with the vital processes, with the phenomenon of 'life' and even if one day someone is able to demonstrate that something flows out of the body, something psychokinetic, it will always be a reflection or projection on the physical level of something that in its nature and in its essence, is non physical.

MURDEROUS ESP - A CASE OF STORY FABRICATION

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The main bulk of information directed at the public as regards suggestive paranormal cases most probably comes through the mass media. Under favourable conditions, when the information has been carefully researched, it could be of considerable value not only to the seriously interested layman but to the professional parapsychologist as well. Regretfully enough, stories implying paranormal events are usually reported for the sake of sensationalism. Not rarely may some stories be an act of intentional fabrication.

In this paper we report a case of suggestive apparition of the dead that was published in a Swedish magazine in 1978. The interesting results of our follow-up and testing of the reliability of this story as to its paranormal connotations are presented accordingly. The author of the article gave specific details and assured in other ways, for instance, by name-giving of reliable witnesses, that it was a true story. If this really was the case it would suffice as a comparatively well documented case of the apparition of the dead or dying person. As we discovered it was not only impossible but also irrelevant whether the case was considered an apparition of the dead or regarded as an out-of-body experience.

THE UNEXPLAINABLE - THE STORY IN THE SWEDISH MAGAZINE

Virginia visited her parents a few hours after she was found murdered some 600 kilometers from home.

When the doorbell rang at the Vicarage in Coleshill, a suburb of Birmingham in England, a few minutes past ten o'clock one May evening in 1977, the vicar and his wife Doris Helen Capper were not especially surprised because they were accustomed to people with problems coming at all hours of the day wishing to speak to the vicar.

Mrs Capper, 44 years old, had just prepared the evening tea and after carrying it on a tray into the living room where her husband sat reading, was placing it on a small table. She frowned showing a slight irritation at being disturbed at such a late hour. She went to the door, opened it, then turned with a very astonished look on her face.

There on the steps stood her 26 year old daughter Virginia Ann. The last person in the world she expected. Her daughter lived in Scotland, 600 kilometers away, at a home for the handicapped, and she had in no way informed her parents of her plans to visit them.

"Oh, come in Virginia", Mrs Capper said excitedly. "We weren't expecting you. Why didn't you let us know you were coming? Is there anything the matter, dear?"

Virginia, a brown-eyed brunette, was badly crippled by polio when she was five years old. The illness had also left her with some mental disturbances. She stared at her mother who noticed that her daughter was very pale and wearing a dirty, wrinkled dress that was even torn in several places.

The vicar entered the hall then, as he had become curious as to whom his wife was talking. Astonished but glad he hugged his daughter, then putting his arm around her, he helped her to a chair. "What's happened, Virginia?", he asked. The girl gazed at her father. "I've been attacked, daddy. A man hit me and hurt me." The shocked Mrs Capper quickly poured a cup of tea for her daughter but Virginia only dismissed her attentions. "I just wanted to come home and sleep in my own bed", she said. "But how did you get home? Did you take the train?" wondered Mrs Capper, but the girl only stared at her mother and repeated what she already had said: "I want to sleep in my own

bed". "I'll go immediately and put your room in order", said her mother, "Stay with your father and tell him everything that happened." The vicar held his daughter's cold and trembling hands while trying to calm her.

Four years ago Virginia had moved to a home for the handicapped in the vicinity of Aberdeen, Scotland. There she tried to live a fulfilling life. Her parents visited her every three months and each holiday she spent a week or more together with them. Her parents knew she was happy being together with 60 other men and women of the same age and situation as hers. Some of the people at the Institution were cripples, others were mute, but they all found a happiness and kindness there and they trained hard in order to manage as well as possible on their own. The last time the vicar and his wife had met their daughter was in the first week of April and she had seemed perfectly content together with her friends.

"You say you were attacked", said the vicar, "Tell me exactly what happened, Virginia, who attacked you?" "A man", answered the girl in a very distraught way, "While I was out with the others on our usual evening walk, I had thoughts of going to bed about nine in the evening, but then I decided to walk along the railway as I often do." The girl became quiet but her father encouraged her to continue. "There are high bushes along the track and a man was hiding behind them", she went on, "I didn't notice him until he jumped on me and beat me to the ground. I tried to struggle free, but in vain. You know I can't run and I couldn't defend myself very well. He tore off my jacket and my dress, pulled off my underclothes and raped me." "Have you contacted the police?" asked the father, "Have they caught the maniak who did this?" She shook her head. "No, not yet. He's in his twenties, tall, and has a short hair cut. He almost looks like a gorilla. I've seen him before in the village and noticed him looking at me." She continued to describe the man for her father and then said once more: "I just wanted to come home and sleep in my own bed." "But how did you come home?" her father wondered. Just then his wife came downstairs. "Your bed's ready now, Virginia. Wouldn't you like something to eat first? You can tell us everything tomorrow after you had a good rest. You must be exhausted." Without answering her father's question, the girl simply said: "I don't want anything, thanks, Mom. Can I go to bed now?"

Mrs Capper helped her daughter into bed and then she joined her husband downstairs. She was still as confused as he but she was sure

the morning would bring an explanation as to how her daughter had made her way home.

Slightly more than an hour later, just as the vicar and his wife were preparing for bed the doorbell rang once again. When they opened it they were surprised to see Mr Smythe, the curate, looking very serious indeed. "I just received a call from Aberdeen and I hurried over at once to tell you. I have bad news I'm afraid." As he stepped into the house he continued: "The Aberdeen police called and asked me to inform you. Virginia is dead. She's been murdered. Her body was found alongside the railroad tracks about 8 p.m. this evening. I'm so sorry to be the one to tell you this but the police thought it better than a strange policeman coming."

Filled with doubt, Mrs Capper faced the curate: "This is impossible. Virginia is at this moment asleep in her own bed upstairs. She came home and told us she'd been attacked and I myself have tucked her in bed. There must be a mistake." "There's no question of a mistake, sorry to say," said the curate. "The police are now searching for the man who did it. They told me that a group of people from the institute took their usual evening walk but Virginia walked further on alone and never returned. This man must have been familiar with her extra walks, that's the police theory, and he was lying in wait for her. He raped her and murdered her. There's no room for doubt. Her body was found by a woman doctor about half an hour after the crime was committed. The police were notified immediately. She was raped before she was killed, her underclothing was ripped off and she was half naked when found."

Mrs Capper stood up and went instantly upstairs to her daughter's room. She returned quickly looking very pale and in a state of shock. As her husband and the curate sprang to give her support, they heard her whisper: "She's not there. The bed's not been touched, even though I watched with my own eyes as she crept under the covers."

The vicar went upstairs and looked into his daughter's room. It was true, there was no sign of the girl. "I know how my daughter was murdered. I can even give a detailed description of the murderer." He turned to Mr Smythe: "Please drive me to the police station." Turning to his wife he said: "You'd better come along. I dare not leave you alone now."

When they arrived at the police station they were promptly admitted into a detective's office. "What I am going to tell you will seem

absurd, but I can do nothing else, then I leave the rest to you", said the vicar. He gave an account of what had happened, exactly what Virginia had said and her description of her attacker. The policeman rang Aberdeen at once to give them this information.

Early the next morning the vicar had a call from the detective who said the police in Aberdeen, because of the description they had received, had immediately recognised a young man who was already well known to them. His name was Frank McLellan Greig, 21 years old. He was arrested on suspicion of murder.

The Aberdeen police were of course curious, as to how the vicar could so accurately describe the young man's appearance. The vicar could only confirm what he had already confided to the local police. Greig's clothes were sent to the criminal laboratory for tests. Blood of the same blood type as Virginia's and her hair were found on them. When he was apprehended Greig was taken by complete surprise. He was so sure of not being suspected that he hadn't even bothered to wash himself properly after his attack. On August 23, 1977 in Aberdeen Greig went to court accused of murder. He pleaded guilty and was convicted.

The strange visit Virginia had paid to her parents at a time when she was already dead and the detailed description she had given of the criminal was made known to all present in court. "This was a cruel and horrible murder, worse than any we've had here in court. The punishment will be 20 years in prison but with regards to the brutality involved, this man ought never to be set free again", proclaimed the judge. Virginia Capper's dead body was brought to her home and she was buried by the church where her father was vicar.

"I don't understand how it was possible for our daughter to appear before us", the vicar said to two newspaper reporters. "I can only give an account of what happened. I am convinced that our daughter, in her moments of anguish, desperately wanted to be with us in the security of our home. I believe that this wish was so strong that she managed to appear as a final farewell to us, her parents. I am a man of the church, a God-fearing man, but I still cannot explain what happened. For most people, this seems quite unlikely. But to all doubters, I'd like to add, how could my wife and I describe so accurately the murderer's appearance if Virginia had not told it to us?"

DOCUMENTATION

One of the things you learn from a case such as this is that obtaining the necessary material is a very difficult and often unrewarding task. Even if a layman has the interest and the time to pursue an investigation he may not be able to accomplish his task simply because he, being a layman, will not receive any answers, or very scanty ones, from involved authorities, publishers, authors, and so on. It may also be difficult for the layman to be granted interviews with persons who could give him important information. It is, however, probably somewhat easier for an established professor of a state university to obtain the information he is seeking, but, as can be shown here, not even a professor always has his efforts rewarded.

Mr. Anders Delang who drew our attention to this case had written to the Coleshill Chronicle, the newspaper where the article in question or some of the information about the murder supposedly had originally been published. He never had a reply. We at this laboratory also wrote to the Chronicle but with the same result. We even sent a registered letter. Having investigated further we found no newspaper listed under that name.

Mr. Delang wrote to the Europa-Press Agency, too, (the letter was not preserved) and inquired about the article. He was advised by reply (also not retained) that he should consult the author whose name (Michael Sheldon) and address were given. Mr. Delang did so but to no avail. He did not make copies of these letters. He then wished to send a reminder to the author but had lost the address. Mr. Delang once again wrote to the Swedish agency and explained the matter. This time the agency refused to supply the address for two peculiar motivations: It was not ethical (sic) to reveal the address, and the agency could not, so long afterwards, find the requested information.

One of us (R.E.) paid a personal visit to Europa-Press to see what we could find out. Europa-Press is an agency with an office in Stockholm, Sweden, and a staff of eight. It deals with articles, stories, features, political comments, etc. from the English-speaking world and sells to daily papers as well as periodicals of different kinds in all Scandinavia. Michael Sheldon, who Delang was led to believe was the author, is a former journalist of Belgian extraction who has set up his own agency, dealing with articles, etc., in

Ireland. Europa-Press considered him to be a very reliable source and he supplied them with the article in question. Europa-Press was not particularly keen on identifying the author but they revealed that he really was a South African and that his name had been stated in the English version of the article. We were told that they did not have a copy of the original article.

From this laboratory we wrote many letters and received many replies. We do not list them here since they are all taken up in the reference list. Besides the Coleshill Chronicle, the Birmingham police never answered.

REPORTS IN NEWSPAPERS

An important part of the documentation is the press coverage of the murder-case. Has any newspaper indicated the occurrence of paranormal events in connection with this criminal story? How were the Aberdeen police so quickly able to find the murderer? These questions seem to be fully answered through the newspaper reports.

In the following account we do not separate the different sources all of which are consistent in fact presentation. Even in details the newspaper reports coincide and some of the articles are thorough descriptions of what happened. Some articles are rather extensive and give background material. The story in itself is so emotional and provoking that additions of fake paranormal events were hardly needed in order to draw the attention of the public. In the References the newspaper articles are listed.

Frank Greig lived with his mother at 96 Inchrae Drive, Garthdee, Aberdeen. Virginia was a resident at the Newton Dee home for handicapped adults at Bieldside. The Procurator gives another address. We have not found why this discrepancy is at hand since we think it of no great significance.

Frank had 23 previous convictions. Several of those were sexual offences. Other convictions were for house-breaking, arson, and theft. At the same time as he was charged for murder, he was also charged for two indecent exposures on May 16, in Pitfodels, involving two separate women, the assault on a 15-year-old in an Aberdeen park on May 14 (one paper writes May 4) by kicking him in the face, and the theft of a

purse from a home in Aberdeen on May 15. He was freed on all the charges but the murder.

Greig was in and out of young offenders' institutions all his youth. Two psychiatric reports obtained by the Crown and presented at the trial showed that Greig was sane and fit to plead. This is extremely surprising as two earlier reports on Greig in 1972 and in 1975 stated that his antisocial tendencies from an early age were "typical of a psychopathic personality" and that Greig was impulsive and showed "little or no remorse for the consequences of his actions and predicament". As we shall see he had an apt to suppress uncomfortable thoughts and feelings. The 1975 report concluded with: "One can be only pessimistic about his future".

When Greig was in Craiginches Prison he pleaded with the prison doctor for psychiatric treatment. This was granted and he spent ten months in Cornhill Hospital. Patients were supposed to take part in group therapy, which involved discussing their problems together. He was too shy to speak about his problems in front of the other patients, especially the female ones. Of course, when he was urged by his inner drives and committed his sexual offences he showed no shame, especially not in front of females.

This kind of treatment was maybe not the correct type for a person who sometimes could not recall what he had done. His mother said that he never talked about the bad things at home. He told her that he simply could not remember doing them. To him it seemed as if someone else was the culprit. About the murder he confided in his mother: "I don't think I could do such a terrible thing - but my mind is just a blank about that night".

Frank was the only child of trawlerman George Greig and his wife Margaret. The mother said that Frank was a normal, well-behaved boy until he became a teenager. The father did not understand why it went wrong for Frank but believed that his long spells away at sea had something to do with it. He thought Frank got spoiled when he was not at home. Said the father: "When I came home from sea, he was obviously used to getting anything he wanted. If he had been given a good hiding early on, it might have been different." The father though, paid his son's fines.

At first Greig pleaded innocent in the court trial but suddenly changed his plea to guilty. Why this, is not clear from the newspaper

reports. After the change of plea Greig told about the incident. It then seems that he after all could remember what he had done. On the other hand, he hardly gave any details that were not mentioned earlier by the police, witnesses and the procurator. He told the court that he went for a walk that evening along the railway line. He saw a girl walking towards him and just as they passed each other "something came over me" and he picked up a boulder and hit the young woman on the side of the head.

At home the slim, immaculately-dressed Greig was considered a loving and gentle son, husband and father. His mother said he was helpful, tidy and polite. She took her son with her when she went to the Kingsway bingo hall and there he met a young employee by the name of Fiona. She was sixteen and she and Frank got married only three weeks after their acquaintance. Everyone was against the marriage. "They were both so young", was Mrs. Greig's reason. Frank told Fiona that he had a record and had had medical treatment. Fiona's comment was: "All of that meant nothing to me. I was young and I was in love."

On the day after the wedding Greig appeared in court for an offence he had committed some time before. He was put on probation for a year. A few weeks later he broke his probation and was sent away for a year. The couple's child, a girl, was born while he was in the young offenders' institution.

Fiona got a house at 14 C Logic Avenue but Greig never came to live there. When the baby was born Fiona stopped visiting Frank. She gave up the house and started divorce proceedings. "That does not mean I am not still fond of Frank. I just realize there is no future with him", she explained in an interview and continued: "Despite all our troubles, Frank was always gentle towards me. I never once saw him use violence. He loved his daughter and I just cannot believe that he could have committed this terrible crime."

Greig could not keep a job for long. People always found out about his record. So he mostly stayed at home to play with his daughter and take her for walks. Mrs. Margaret Greig added: "Whatever he got up to outside the house, he never misbehaved here or ill-treated the baby" - "Frank doted on the child."

The body of Virginia Capper was found on the forenoon of the 18th of May about seven kilometers from Aberdeen in secluded undergrowth by the side of the old Deeside railway line, which had been out of use

since the early 1960s. The spot was barely 25 meters from the first tee of Deeside golf course. It was a 73-year-old retired medical doctor Williamina Sole, 4 Golfview Road, Bielside, who found the body about 15 meters outside the gate of her house.

Immediately after the discovery, senior police officers, Chief Det. Sup. William Archibald and Chief Superintendent William Adams, went to the scene and set up headquarters in a caravan placed in the car park of Deeside Golf Club. Dr. William Hendry and his assistant Dr. Gordon Cockburn made a detailed examination of the body before removal some four and a half hours after it was found. Later a post-mortem was carried out. Virginia had been struck seven blows on the head. She had also been struck on the mouth causing internal bleedings of the lips. She suffocated from inhaling blood. She had been a virgin before the attack. It could not be established if the intercourse took place before or just after her death.

About fifty policemen made a house-to-house investigation in the area. Three tracker dogs were used in the wide search of the area. A bloodstained boulder was found in a van not far from where the body lay.

The police appealed over the radio to the public and wished to communicate with everybody who could help in any way at all with even the smallest piece of information. The police force was especially interested in coming in contact with persons who had seen a man with bloodstained or marked clothing between 8.30 and 11 p.m. on Tuesday, May 17, on the disused Deeside railway line between Bielside and Cults or in the surrounding area. A number of people called to pass on information to the police.

Mr. and Mrs. Capper were on holiday near Lerwick, Shetland Islands, at the time of the crime and they flew down to Aberdeen to help the police.

The police went to Greig's home around 9 a.m. on May 19. This means that less than 24 hours after the police started their search the right man was arrested for the murder. The two police officers said that Greig had blurted out a statement to them: "Before she reached me I picked up a big boulder and hit her on the head with it as she passed."

As can be seen by this account the newspapers had a thorough

coverage of the case. All details have not been taken up in this summary. A story fabricator using this material did not have to invent anything outside the paranormal field.

It is also clear that the authorities in Aberdeen devoted all available resources to solving the murder case as quickly as possible. Greig, who had been spotted by several persons at the place and moment of the murder, did not live far from there and was known to the police for violence and the previous offences he had committed. As an impulsive psychopath with a long criminal record he was automatically a suspect and already at his arrest, which was done without a warrant, he admitted to the assault (which he later denied to finally plead guilty) and the police officers found evidence of importance in his bedroom. The police had no help from Birmingham in finding the culprit and they needed none. Within 24 hours the case was virtually, but not formally, settled.

The two questions at the beginning of this chapter have herewith been satisfactory answered. Nothing paranormal is to be found in the English and Scottish newspapers. The picking up of the murderer was a mere routine job done with heavy resources and utmost efficiency.

TEXT ANALYSIS

Nowhere in the Swedish magazine it is stated that the story is fiction. The editors, upon questioning, did not admit the story to be fictitious. They merely made reference to the woman who had translated the article from English to Swedish. She, however, said that she had done her task and nothing more, meaning that she had neither added nor subtracted anything. She, then, referred to the author of the original story. Strangely enough no author is named in the Swedish version. On the other hand it is mentioned that the material was issued by Europa-Press (who has the Swedish copyright).

The story is written in such a way that the reader really believes he is faced with facts:

- 1 Photographs of the people concerned are supplied.
- 2 The involved persons' names, ages, occupations and city addresses are revealed.
- 3 Exact data of the crime and its consequences are included such as: the time of the murder; how and where the murder was committed;

how and when the murderer was arrested; how the parents were contacted; when and where the murderer was sentenced, and what sentence he was given.

4 Lots of details are added which lead the reader to believe that the author has interviewed the involved persons. The author, for instance, describes how Mr. and Mrs. Capper were just about to have tea when the doorbell rang; that Virginia was pale, trembling, had cold hands and was wearing a dirty, wrinkled and torn dress; that Virginia did not tell how she had managed to make her way home; that the curate came an hour after Virginia; that the Cappers were surprised to see their daughter and the curate; that the curate was looking very serious; that the curate had received a telephone call from the Aberdeen police; that the body of Virginia was found at 8 p.m.; that it was a female medical doctor who found the body; that the curate drove the Cappers to the police station; that Mr. Capper, due to the circumstances, found it unwise to leave Mrs. Capper alone at home; that the Birmingham police at once contacted their colleagues in Aberdeen; that the Aberdeen police called Mr. Capper the next morning to tell them that a suspect had been arrested; that the Aberdeen police were curious as to how Mr. Capper had been able to describe the suspect; that the suspect had not tried to clean away traces of his crime, and many more details of that sort.

There are a couple of details that seem a little far fetched: Who would tell the author that Mrs. Capper frowned when she heard the door bell? How could he know that she had placed the tray on a small table? These details are not of a kind that one reacts to when reading the story. It is first when the suspicion of story fabrication arises that these details seem puzzling.

The whole story has an air of telling facts and speaking the truth. Certain facts in the story can be checked by reading the Birmingham (Coleshill) and Aberdeen newspapers published at the time and by checking with the authorities, for instance the court in Aberdeen.

Doing so we found the following facts: There was, at the time of the event, a vicar by the name of Capper in Coleshill. He had a wife called Doris Helen and she was 44 years old. They had a daughter who was 26 living in a home for the handicapped, the Rudolf Steiner Establishment, in Aberdeen. She was, one May evening on the 17th or 18th, 1977, attacked near the disused Aberdeen to Ballater railway line which had been converted into a country walk for pedestrians. The assault was carried out by a man called Frank McLellan Greig who was

21 years old. He picked up a stone and repeatedly struck her on the head, dragged her to an adjacent path and raped her.

Soon afterwards he was arrested. He was sentenced on August 23, 1977, in an Aberdeen court. He did not get 20 years as the writer says but life imprisonment.

All the basic facts about the crime are true. So there is no doubt that this story is given as an entirely true story. If an author wants to tell a spooky story he hardly brings in a very sad episode and with it people who have grieved over a gruesome crime and the terrible loss of a loved one. The writer instead simply invents all the facts or if his imagination is limited he takes a murder case he has read about in the paper as a starting point or model and changes the names of the persons and the places, changes the occupations, the ages and the dates.

After contacts with Mr. Capper and the police in Birmingham and Aberdeen it can be established that most of the information given in the story is sheer fantasy.

In a handwritten letter, Mr. Capper informs us that he and Mrs. Capper were not home on the day of their daughter's murder. He writes: "I'm afraid I cannot be of much help. My wife and I were away on holiday when our daughter was murdered."

This means that they could not have received any visitors in their home that evening; that they did not go to the Birmingham police; that the Aberdeen police did not call the vicar the following day, etc..

Mr Capper continues: "We have not heard of any manifestation of her personality since her death and had never heard of the magazine article you mention." Mr. Capper denies what is the core of this story: The Cappers know nothing of Virginia's appearing in the vicarage the night of her death. The Cappers never told a reporter or interviewer so. It all seems to be an invention on the part of the writer. The Cappers never told him that they were having tea that evening and so forth.

Mr. Capper adds: "Certainly nothing happened here and there was nothing here for any local paper to report. So I do not know where the (Swedish) magazine obtained its information" . . . " I have never heard that she has appeared to anyone since her death."

The Procurator Fiscal of Aberdeen, M.T. MacNeill states the following: "Greig's identification as the criminal was in no way connected with any information from Birmingham or Coleshill. He was identified by persons who had seen him at the old railway line at points not far from the locus at appropriate times. He was one of a number of suspected persons interviewed by the police to whom he made a confession . . ."

The Procurator Fiscal sets the record straight. The convincing statements by the vicar of the story: "What I am going to tell you will seem absurd . . ." and the stress given to the Aberdeen police curiosity as to how Mr. Capper could describe the murderer have been proved to be complete falsehood.

WHO DID THE DOCTORING

Mr. Capper, in his written statement, assures us that his daughter had not manifested herself as an apparition. The Aberdeen authorities state that they have not been in contact with the Birmingham police or with the Cappers regarding the arrest of Mr. Greig. Assuming these two statements to be correct, one has to conclude that large parts of the story are fictitious. Therefore who added these parts?

From the results of our detective work and our reasoning we also conclude the history of creation to be as follows.

A real murder crime took place. It was reported by local press, predominantly by newspapers in Aberdeen and Birmingham. The press articles appeared on three different occasions, namely, when the police were out looking for the murderer, after the arrest of suspects, and after the trial and sentencing of the murderer to life imprisonment.

A South-African author read the accounts of the crime and got the idea to mix in some paranormal events. He sold his story to the agency in Ireland run by Michael Sheldon, who in his turn sold it to Europa-Press in Stockholm. This latter agency, in its turn, sold it to the Swedish magazine 'Min Varld'. Mrs. Marianne Wilof translated it, the editors of the magazine approved of the Swedish version, and published it.

It could also have been the case that the fabrication was carried out in some other way by someone else. Maybe Michael Sheldon or one of his associates read about the murder and decided to change it from a primarily criminal story to a spooky one. Perhaps the Swedish translator was inspired to do something more with it than just translation; she put in all the fiction, omitted the author's name and supplied her rewritten product with only the translator's name. It could, also, be so that the editors of 'Min Varld' got hold of a flat criminal story which they wanted to spice up to a more thrilling story. Therefore, they had someone on their staff to do the rewriting before publication.

Another possibility is that falsification took place step by step. The journalist who first reported the murder case made some additions. This 'padding' was continued by small details being added by each person (or most persons) who dealt with the material. Each doctoring was aimed at making the story more sensational and more trustworthy.

By adding something sensational and unbelievable to a story it becomes less trustworthy and thus risks its publication possibilities. Alterations of the sensational kind must be accompanied by additions and changes of such a nature that evidence for the truthfulness of the story is strengthened.

Exactly who changed what cannot be established by us. The motive of the doctoring is on the other hand quite clear: money. And, as we all know: Sensations can be turned into money.

Authors, agencies, translators and editors are all interested in having an article published. All their activities are to promote their own work so they can do more of it: write, sell, translate, publish. It directly gives them money and indirectly, at least as long as no one starts to investigate and criticize the cases, paves the way for new similar articles.

This last point is a very serious one for parapsychology. We have during the seventies had a veritable boom in the occult. Some magazines have been flooded by stories about unbelievable events. Newspapers have taken up stories of the same kind. Material about the occult has proved to be good merchandise. Can it be so that a few authors and agencies massproduce these articles and sell them all over the world? Can it even be so that most of these stories emanate from a few agencies as the one run by Michael Sheldon in Ireland?

CONCLUSIONS

We found it superfluous to speculate about the psychological probability of the story as presented in the article for the following two reasons. Firstly, because we think we have been able to demonstrate in a clear-cut way that we have encountered a case of story fabrication; secondly, because of the lack of a reliable, well-defined methodology for the assessment of the general credibility of the given information.

The Aberdeen-Coleshill case, because of its blend of truths and falsehoods, is a type of article that preferably can only be sold and published outside the country where the alleged story took place.

Parapsychologists all over the world should perhaps engage themselves more in checking fantastic stories of paranormal interest which are published under the label of, for instance, 'True stories from real life'. They must start accusing newspapers and magazines of fraud when they publish trash of the same kind as the striking example we have brought forth. Maybe the worst cases should be taken to court. Here we think that an important contribution can be made by the local parapsychological societies. They are in a good position to carry out the type of follow-up studies that we have carried out at a distance with all the adversities that this implied for us. An interested and knowledgeable non-professional person may be just as suited for such a task as the professionally working parapsychologist.

So far we have discussed the abuse of parapsychological concepts in the mass media but one undeniable and regrettable fact seems to be that too many parapsychologists (even some members of the P.A.) have too often manifested a surprisingly large amount of gullibility and poor judgement in their pronouncements on these matters. During the Geller-psychosis, for instance, we think that some professional parapsychologists overstated the case and contributed to sensationalizing their field of endeavour.

It seems quite clear to us that the occult boom has been nothing else but damaging for parapsychology as a science. For its own survival parapsychology should involve more activity in revealing fraudulent newspapers and magazines.

Murderous ESP - yes murderous for scientific ESP research if we are

unable to stop the flow of fabricated 'ESP' stories.

ACKNOWLEDGMENT

The authors wish to express their gratitude to all who furnished and checked information pertinent to this case. We are especially indebted to Mr. Anders Delang who drew our attention to the case and to Mr. Michael Goss who provided us with some of the newspaper clippings much needed for the investigation.

REFERENCES

Letters from the Parapsychology Laboratory, University of Utrecht, to:

The Coleshill Chronicle, Birmingham, England, October 3, 1979 and December 12, 1979.

The Police Department, Aberdeen, Scotland, October 4, 1979.

Aberdeen High Court, Scotland, October 31, 1979.

The Press and Journal, Aberdeen, Scotland, February 26, 1980.

Evening Express, Aberdeen, Scotland, February 26, 1980.

People's Journal, Dundee, Scotland, February 26, 1980.

Letters to the Parapsychology Laboratory from:

Mr. Anders Delang, Skarholmen, Sweden, August 31, 1979, October 5, 1979, and January 22, 1980.

Rev. J. Capper, Coleshill Vicarage, England, November 1, 1978.

Translator Marianne Wilof, Stockholm, Sweden, October 3, 1979.

Europa-Press, Stockholm, Sweden, September 26, 1979.

Mr. Michael Goss, Grays Thurcock, England, April 4, 1979, and January 17, 1980.

Grampian Police, Chief Constable Alexander Morrison, Police Headquarters, Aberdeen, Scotland, October 16, 1979.

Sheriff Clerk's Office, Sheriff Clerk Depute W.A. Stevenson, Aberdeen, Scotland, November 6, 1979.

Procurator Fiscal's Office, Procurator Fiscal M.T. MacNeill,
Inverlair House, Aberdeen, Scotland, November 8, 1979.
The Press and Journal, Evening Express, Aberdeen, Scotland, May
1980.
People's Journal, Dundee, Scotland, May 7, 1980.

Official material: The charges against Frank MacLennan Greig of
The Right Honourable Ronald King Murray, Her Majesty's Advocate.

Articles:

Police plea to the family of child-killer (by Michael Orr),
Birmingham Post, May 18, 1977.
Midland vicar's daughter murdered, Birmingham Post, May 19, 1977.
Police plea after 'brutal' killing (by Donald Smith), Press and
Journal, May 19, 1977.
Man on murder charge in Aberdeen, Press and Journal, May 20, 1977.
Man denies murdering city girl, Birmingham Evening Mail, August
12, 1977.
Aberdeen man charged with murder, Press and Journal, August 13,
1977.
Aberdeen man on trial today, Press and Journal, August 23, 1977.
Court hear of head blows (by Colin Smith), Press and Journal,
August 24, 1977.
Man who killed vicar's daughter jailed for life, Birmingham
Evening Mail, August 24, 1977.
Life term for callous city killer, Press and Journal, August 25,
1977.
Double life of a killer (by Colin Smith), Press and Journal,
August 25, 1977.
Life for killer of handicapped girl, Birmingham Evening Mail,
August 25, 1977.
Det oforklarliga (The unexplainable), Min Varld, Sweden, 18, 1978.

POLTERGEIST AGENTS:
A REVIEW OF RECENT RESEARCH TRENDS AND CONCEPTUALIZATIONS

Alfonso Martinez Taboas

Carlos S. Alvarado

Historical surveys of poltergeist phenomena (e.g., Carrington and Fodor, 1951; Roll, 1977) do not leave any possible doubt on the curious fact that in a substantial number of cases the phenomena is associated, spatially and temporally, with some specific person. If this is true, more problematic is the reason for that association. Demonologists such as Guazzo (1970) were inclined to the view that the manifestations were diabolic phenomena caused by a witch. Writers with a survivalistic inclination see intents of a discarnate agent to communicate with some person (e.g., Pierce, 1973; Stevenson, 1972). With the advent of dynamic psychiatry others see the expression of repressed tension and anger (e.g., Fodor, 1968; Roll, 1972).

Our purpose in this paper is to evaluate all published studies, from 1973 to 1979, whose authors made a judgement or clinical study of the poltergeist agent, as it relates to the etiology of the manifestations. The year 1973 is chosen as we were interested in seeing how other investigators tried to replicate Roll's (1972) conclusions, and to extend to more recent studies the critical evaluation of this topic published elsewhere by one of us (Martinez Taboas, 1977; 1980). Besides the analysis and discussions of these recent cases, we will also present a criticism of a recent theory of poltergeist phenomena: the central nervous system (CNS) or epileptic theory (Roll, 1978).

Psychological aspects

Before proceeding, we think it is important to review some recent criticisms made by Martinez Taboas (1977, 1980) concerning the methodology and assumptions involved in the so-called 'psychopathological' model of poltergeist phenomena. Martinez Taboas has discussed various points, among them:

- a) Numerous epidemiological studies of mental health report that more than half of the population on study has at least some mild dysfunctions or symptoms of behavior problems;
- b) Controlled clinical studies with adults and children has indicated that a clinician is predisposed to 'see' or encounter 'psychopathology' on normal people if they are informed offhand that they will see a patient;

By psychiatric standards the majority of poltergeist agents do not have any obvious psychopathologies or neurosis.

By looking closely at the classic evidence for the psychopathological model of poltergeist phenomena, Martinez Taboas found that this is what typically happened: A psychologist fully aware of the theory and its implications (in other words, not 'blind'), talks with the agent and his family. Usually he does not see any obvious neurosis, so he uses projective tests (highly dubious on reliability and validity) and his/her 'intuitions' and . . . finds psychopathology everywhere.

Martinez Taboas remarked in his papers that a basic reading of some books of pitfalls in human research (e.g., Barber, 1976; Jung, 1971; Rosenthal, 1976) will give us serious doubt about the conclusions of those clinicians. He concluded that in no single study have the psychopathological model been evaluated in a scientific manner. Since the most recent article by Martinez Taboas (1980) did not quote any study from 1973 to 1979, this too is an opportunity to see if the recent studies are more sound than its predecessors.

We could locate 9 studies in which its authors made some comments about the etiology of the manifestations, and one in which the author found a correlation but made no speculation on it (McHarg, 1973). All nine authors supported, some without qualifications, the

psychopathological model. A brief review of them will help us see if their conclusions are sound.

In the studies of Zorab (1973) and Pratt (1978) there is not a shred of evidence for sustaining a psychopathological model. The mere fact of this, and that its authors nevertheless stubbornly insist on psychopathology is additional evidence for Martinez Taboas (1980) argument that the poltergeist agent has a stigma before being investigated.

In the study of Eisler (1975) we are told that a girl of 8 years old had 'repressed tensions'. The evidence for this is reduced to a talk which the author had with a psychologist who knew the family and who said that the girl was trying to obtain attention through the phenomena.

Likewise, in Rogo's (1974) study we are presented with the usual terms: 'tensions', 'repression', etc.. In the final analysis we are dealing with a purely subjective interpretation of its author.

Something interesting occurred in Barrington's (1976) study. In 1965 Barrington reported this case concluding, in the typical vein, that an 8 years old girl was in 'tension', etc.. Some years later the mother of the girl commented to Barrington that by the time of the poltergeist disturbances she (the mother) was very depressed and sad. So, we are informed in the 1976 article that the agent was not the child but the mother. Needless to say is that no systematic effort was made to sustain this opinion.

Palmer's (1974) study offers, in our opinion, the most serious recent attempt to evaluate this model. He reported that a child of 12 years old was the agent, and that the dynamics involved were feelings of aggression. Palmer used the services of a psychologist who administered to the boy some psychological tests. The fact that many of the tests were of the projective type, and that the psychologist knew that unexplained movement of objects had been associated with the boy, are serious drawbacks to this study.

Hastings (1978) studied an agent of 21 years. A battery of psychological tests were given to him and to some of his co-workers. Reading the results one cannot find nothing extraordinary nor suggestive of psychopathology in this young man. Nevertheless, Hastings tries to convince us that the model is right, as the young

man "was newly married and making double payments on a car..."

In the Pratt and Palmer (1976) study there are two possible agents: a girl (10 years old) and a boy (9 years old). We are told that unconscious hostile impulses are at the root of the manifestations. The only evidence they give is that the girl had some 'potential epileptiform activity' some years before the poltergeist disturbances, and that in a word association test that Palmer himself administered, the girl made a "slight nervous giggle".

Solfvin and Roll (1976) report a case in which a 21 year old agent had grand mal epilepsy and serious health problems. In their opinion the poltergeist manifestations are a substitute for discharging tensions. So, too, are the grand mal attacks. This case will be commented more fully later.

Evaluation

The basic question is: has the psychopathological model been more adequately evaluated than before 1973? In our opinion the answer is no. Although all the authors emphasized the model, what we in fact get is a disheartening situation. First of all, in seven of the studies the evidence for psychopathology of the agent is meager or simply does not exist (Barrington, 1976; Eisler, 1975; Hastings, 1978; Pratt, 1978; Pratt and Palmer, 1976; Rogo, 1974; Zorab, 1973). The strange and unconditional endorsement of the model can be explained when we realize that all these authors, besides coming to the scene fully aware of the implications of the model (not 'blind'), do not even utilize the minimal requirements in protecting their results against artifact and bias. Any rigorous student of clinical research, from psychopathology to personality testing, knows that evidence obtained in these conditions is nearly worthless (Barber, 1976; Berger, 1977; Jones, 1977; Masling, 1965) (note 1).

In only one study (Palmer, 1974) at least an attempt was made to evaluate the model. But, and as pointed out before, the fact that the clinician was not 'blind' was a serious drawback. In addition, many researchers and clinicians will express doubt on the psychodiagnosis as the psychologist used five projective tests, which are very controversial to say the least (e.g., Lanyon and Goodstein, 1971; Masling, 1965; Pervin, 1975; Zubin, Eron and Schumer, 1965). It is

curious too that by Palmer's description this boy was hyperactive. But, as far as we know, nobody had talked of hyperactive children as typical poltergeist agents.

We think we can conclude this section of our paper with the statement that, if anything, the recent reports not only have not improved in methodology, but what is worse, there are clear indications that the psychopathological model is being accepted by many as an applied dogma or self evident fact. This situation, in our opinion, is not healthy from a scientific point of view. If we want to make real progress we must try to protect our studies from such obvious pitfalls (note 2).

The psychophysical model

Recently Roll (1977, 1978) has proposed that the central nervous system (CNS) is a mediator in poltergeist manifestations. According to Roll what happens is something like this: the agent, by a condition of despair or psychopathology, releases those problems by means of CNS eruptions and then in a poltergeist disturbance. The classic case is from someone who suffers from epilepsy and who releases his/her tensions by an attack. Roll thinks that in some cases these subjects substitute their somatic attacks by a poltergeist attack. In other words, the latter are substitutes for the medical symptoms. If this is so, then poltergeist incidents may be regarded as a special class of epileptic symptoms.

Before reviewing the evidence for this model, it is important to know that the epileptic theory is not new. Owen (1964) tried to confirm it in historical cases, but reached a totally negative verdict: "There is little or no evidence associating poltergeist activity with epileptic seizures" (p. 348).

Then, what new evidence has Roll uncovered to sustain this conceptualization? It is our purpose to discuss critically this new evidence.

The most important data, in Roll's judgement, is that he has found four agents with a 'diagnosis' of epilepsy. A review of these four cases will be rewarding:

a) In the case reported by McHarg (1973) we are simply informed that a 14 year old girl, from the age of 9 had an abnormal EEG complicated by generalized convulsions. No correlations between the attacks and the manifestations is mentioned. This fact is acknowledged by Roll (1978, p.179).

b) Pratt and Palmer (1976) inform that a 10 year old girl "had an earlier history" of abnormal EEG consisting of "potential epileptiform activity of a generalized nature" during photic stimulation. What are we to interpret by 'earlier history' is not said. Apart from this, there is no other data suggestive of epilepsy.

c) Solfvin and Roll (1976) presented a case with a 21 year old agent that began suffering from epileptic attacks (grand mal) in March 1974. In July of 1974 poltergeist outbreaks began in his house. Apparently there was an inverse correlation between the epileptic attacks and the poltergeist.

d) The last case is one reported by Thacher (1910) and Barrett (1911) where a boy at the time "was treated for epilepsy". It is not said when epilepsy began or if there was any correlation with poltergeist phenomena.

EVALUATION

We don't know how it can be said that the above cases support the epileptic or CNS theory. In case A, apart from the fact that the girl was epileptic (as far as four years), there is no correlation (positive or inverse) stated. As Roll has postulated that the poltergeist is a substitute for the somatic symptoms (1978, pp.180, 189), then this case cannot be used for an analysis of the theory.

Case B is interesting, insofar as it shows the way Roll interprets his evidence. In our opinion this case does not offer any evidence for the theory. The fact that years earlier the girl had some abnormal EEG activity is not necessarily indicative of epilepsy. It is a well known fact that about 10-20% of normal children display 'abnormal' EEG reading (e.g., Feuerstein, Ward and LeBaron, 1979; Harris, 1977). For example, in the Eeg-Olofsson (1970) study of 743 normal children between 1-15 years old, 15% had paroxysmal abnormalities at some time during the EEG, and 16% showed positive spike phenomena. In the

Stevens, Sachdev and Milstein (1968) study 28% of a group of normal controls displayed moderate to severe abnormalities. This fact is very important and not even mentioned by Roll when evaluating the credibility of the theory.

Case C is the only case reported in the literature where the 'predictions' of the theory are fulfilled. In favor of the theory we have an epileptic agent with recurrent attacks and an inverse correlation between them and the manifestations. At first sight this seems impressive, but not so when we are faced with the fact that the so called predictions are more accurately retrospective in nature as they follow precisely from this case.

Case D is very difficult to evaluate as it is an old case. As far as we know it does not give enough information to assess the theory.

In our opinion, a critical analysis of the cases only leave one where the epileptic theory may be the best explanation. In this condition all we can say is that the theory has been advanced in an over optimistic way, where enthusiasm and dubious correlations had substituted hard and controlled data. The over-enthusiastic way in which 'data' are forcibly made compatible with it is further illustrated by a comment by Roll (1978): "A fifth (subject), whose EEG was taken some months after the incidents, produced a short burst of such spikes" (p.172). As already said, this anomaly is rather frequent in the EEG of normal subjects. Undoubtedly, over-enthusiasm inclines Roll to see this as further 'evidence' and not even to mention possible artifacts in the EEG as a counter-explanation (e.g., Harris, 1977).

CONCLUDING REMARKS

It is somewhat unfortunate to say this, but in the last seven years (1973-1979) our scientific knowledge of poltergeist agents has not advanced as could be expected. All we get are reaffirmations, supported by very questionable ways, that the psychopathological model explains the manifestations. The data, as we have shown here and specially elsewhere (Martinez Taboas, 1977, 1980) does not support such optimistic conclusions.

On the other hand, a CNS or epileptic theory has been revitalized by

some. Although interesting and more easily to refute than the psychopathological model (Popper, 1968), it only has some very meager evidence in its favor, and obviously has been stated in an unduly optimistic manner.

We hope that in the years to come both theories will be evaluated more rigorously and that researchers will take more into account the many artifacts and pitfalls that can lead astray so many bright minds in this field.

NOTES

1) Jones (1977) summarizes this matter as follows: "Studies of the influence of the examiner on the results of psychodiagnostic testing indicate that the expectations of the examiner play a significant role in determining the outcome of testing. Further, the circumstances of testing and knowledge of 'extraneous' characteristics of the client also appear to play a role in the outcome of the diagnostic process. Different examiners both see different cues in the same patient and use the same cue to infer different things (p.4).

2) Recently a study by Rogo (1979) has come to our attention in which he claims that using an 'unbiased evaluation' and keeping the clinician (Dr. Gertrude Schmeidler) 'blind', he confirmed the personality patterns which are associated with poltergeist agents. It seems to us very odd how Rogo could claim that, as we are informed that not only Rogo himself administered the tests (three projective tests) but what is more puzzling is that Dr. Schmeidler "was informed that we are dealing with a poltergeist case". It is surprising that Rogo claimed that his clinician was blind when in point of fact she was not. Furthermore, as Dr. Schmeidler is in all probability familiarized with Rogo's writings and opinions of the family dynamics of poltergeists, all the more certain that we are that it is a misunderstanding to claim that the clinician was blind. In our opinion Rogo's recent case study does not add anything significant when compared with the other studies.

REFERENCES

- Barber, T.X. 'Pitfalls in human research', New York, Pergamon, 1976.
- Barrett, W.F. 'Poltergeists, old and new', Proc. S.P.R., 1911, 25, 377-412.
- Barrington, M.R. 'A poltergeist revived: The flying thermometer case again', J. S.P.R., 1976, 48, 293-297.
- Berger, M. 'Psychological testing', in Rutter, M. and Hersov, L. (eds) 'Child psychiatry: Modern approaches', London, Blackwell Scientific Publications, 1977, 306-333.
- Carrington, H. and Fodor, N. 'Haunted people', New York, E.P. Dutton, 1951.
- Eeg-Olofsson, O. 'The development of the electroencephalogram in normal children and adolescents from the age 1 through 21 years', Acto. Paediat. Scand., Suppl. 208, 1970.
- Eisler, W. 'The Bronx poltergeist', in Research in Parapsychology 1974, Metuchen, Scarecrow Press, 1975, 139-143.
- Feuerstein, M., Ward, M.M. and LeBaron, S.W.M. 'Neuropsychological and neurophysiological assessment of children with normal and behavior problems: A critical appraisal', in Lahey, B.B. and Kazdin, A.E. (eds) 'Advances in clinical child psychology: Vol. 2', New York, Plenum Press, 1979, 241-278.
- Fodor, N. 'The haunted mind', New York, New American Library, 1968.
- Guazzo, F.M. 'Compendium maleficarum', London, Frederick Muller, 1970.

Harris, R. 'The EEG', in Rutter, M. and Hersov, L. (eds) 'Child psychiatry: Modern approaches', London, Blackwell Scientific Publications, 1977, 334-358.

Hastings, A.C. 'The Oakland poltergeist', J. A.S.P.R., 1978, 72, 233-256.

Jones, R.A. 'Self-fulfilling prophecies: Social, psychological, and physiological effects of expectancies', New Jersey, Lawrence Erlbaum, 1977.

Jung, J. 'The experimenter's dilemma', New York, Harper and Row, 1971.

Lanyon, R.I. and Goodstein, L.D. 'Personality assessment', New York, Wiley, 1971.

Martinez Taboas, A. 'Algunas criticas y sugerencias acerca de la sicopatologia del poltergeist', Psi Comunicacion, 1977, 3, 67-74.

Martinez Taboas, A. 'The psychopathological model of poltergeist phenomena: Some criticisms and suggestions', Parapsychology Review, 1980, 11, 24-27.

Masling, J.M. 'The influence of situational and interpersonal variables in projective testing', in Murstein, B.I. (ed) 'Handbook of projective techniques', New York, Basic Books, 1965, 117-143.

McHarg, J.F. 'Poltergeist and apparitional phenomena affecting the family and associates of an adolescent girl with well-controlled epilepsy', in Research in parapsychology 1972, Metuchen, Scarecrow Press, 1973, 17-19.

Owen, A.R.G. 'Can we explain the poltergeist', New York, Garrett, 1964.

Palmer, J. 'A case of RSPK involving a ten-year-old boy: The Powhatan poltergeist', J. A.S.P.R., 1974, 68, 1-33.

Pervin, L.A. 'Personality: Theory, assessment, and research', New York, Wiley, 1975.

Pierce, H.W. 'RSPK phenomena observed independently by two families', J. A.S.P.R., 1973, 67, 86-101.

Popper, K. 'Conjectures and refutations', New York, Harper and Row, 1968.

Pratt, J.G. 'The Pearisburg poltergeist', in Research in parapsychology 1977, Metuchen, Scarecrow Press, 1978, 174-182.

Pratt, J.G. and Palmer, J. 'An investigation of an unpublicized family poltergeist', in Research in parapsychology 1975, Metuchen, Scarecrow Press, 1976, 109-115.

Rogo, D.S. 'Psychotherapy and the poltergeist', J. S.P.R., 1974, 47, 433-446.

Rogo, D.S. 'The poltergeist and family dynamics: A report on a recent investigation', Paper presented at the 22nd PA Convention, 1979.

Roll, W.G. 'The poltergeist', New York, Doubleday, 1972.

Roll, W.G. 'Poltergeists', in Wolman, B.B. 'Handbook of parapsychology', New York, Van Nostrand Reinhold, 1977, 382-413.

Roll, W.G. 'Towards a theory for the poltergeist', E.J.P., 1978, 2, 167-200.

Rosenthal, R. 'Experimenter effects in behavioral research', New York, Irvington, 1976.

Solfvin, G.F. and Roll, W.G. 'A case of RSPK with an epileptic agent', Parapsychology Review, 1976, 7, 20-21.

Stevens, J.R., Sachdev, K.K. and Milstein, V. 'Behavior disorders of childhood and electroencephalogram', EEG and clinical neurophysiology, 1968, 24, 188.

Stevenson, I. Are poltergeist living or are they dead?', J. A.S.P.R., 1972, 66, 233-252.

Thacher, G.A. 'The March poltergeist case', J. A.S.P.R., 1910, 4, 561-636.

Zorab, G. 'The Sitoebondo poltergeist: A firsthand account written soon after the events', J. A.S.P.R., 1973, 67, 391-406.

Zubin, J., Eron, L.D. and Schumer, F. 'An experimental approach to projective techniques', New York, Wiley, 1965.

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Editorial consultants: Volume 3, 1980 - 1981.

The editors of the European Journal of Parapsychology wish to express their appreciation to the following referees for their assistance in evaluating articles submitted to the European Journal of Parapsychology during the period of the now finished volume 3.

Dr. Susan Blackmore (University of Bristol, U.K.); Henk Boerenkamp (University of Utrecht); Dr. Rolf Ejvegaard (University of Linkoping, Sweden); J.M. Houtkooper (University of Amsterdam); Dr. Jurgen Keil (University of Tasmania, Australia); Dr. Brian Millar (University of Utrecht); W.G. Roll (Durham, U.S.A.); C. Sargent (University of Cambridge, U.K.); J. Solfvin (University of Utrecht); and Ms R. White (Dix Hills, U.S.A.).

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EUROPEAN JOURNAL OF PARAPSYCHOLOGY

VOLUME 4

NUMBER 2

MAY 1982

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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 as 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. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include names of authors, positions and other affiliations, and mailing addresses. Each table

(continued inside back cover)

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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 activity in 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 are considered more important than the level of significance of the outcome of the study. As a practical rule, we advise the potential contributor of an article to submit the design of his planned study before the work 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 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 dates, which are May 1st and November 1st.

ANALYSING SPONTANEOUS CASES: A REPLICATION
BASED ON THE RHINE COLLECTION

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Two previous studies (Schouten, 1979a, 1979b, 1981) presented quantitative analyses of two collections of spontaneous cases, the 'Phantasms' collection based on the cases as reported in 'Phantasms of the Living' (Gurney, Myers, and Podmore, 1886), and the collection of German cases selected by Sannwald (1961).

Spontaneous cases are experiences in the life of people which they find conspicuous because of their nature and their unexplained character, and to which they attribute a paranormal character. In general people are familiar with notions such as coincidence and have many experiences in their life which they consider coincidental. Nevertheless they single out one or a few experiences in their life and attribute a paranormal character to them even though to an outsider the experience might look like just another coincidence. This paranormal character implies that people feel that these experiences had a special meaning, and that they indicate an awareness of something happening in the outside world about which they had no normally sensory acquired information. It is of interest to note that such experiences are relatively rare. Not all people report having ever had a paranormal experience and of the people who do, the

This paper fulfils the publication policy of this journal.

majority reports only one or a very small number.

These experiences are the basis of concepts like paranormal, extra sensory perception, etc.. Thus, these concepts are subjectively derived concepts. Although people often use them in an explanatory sense, these terms should be considered as no more than just labels for certain types of experiences. It might well be that, as suggested by a term like extra sensory perception, an unknown communication process is the mechanism responsible for the occurrence of these experiences. However, other explanations are also conceivable. The occurrence of a phenomenon can never serve as a proof for the explanation of that phenomenon. Performing an unexplainable feat and stating that one does it by paranormal means does not provide any explanation for the observed phenomenon. A stage magician can bend a nail and tell his audience that he does it by exerting paranormal powers, and can then proceed bending another nail and explaining this time the phenomenon by stating that he used a trick. Both times the same phenomenon is observed, just the verbal explanations differ. The point is that in neither situation was any evidence presented for the veridicality of his explanation. Consequently, without further information there is no way to decide which statement is true. It is no use applying Ockham's razor rule to such a problem because this rule is not a method for discovering truth, it can never be an alternative for experimentation and research.

Research begins with learning as much as possible of a phenomenon by observing it, and then proceeding to experimentation and the testing of hypotheses. As a minimum requirement, these hypotheses to be tested should at least fit the observations and data we have on the phenomenon under investigation. However, most often one has to choose from more than one hypothesis which fits the observations, and when choosing which hypothesis to begin research with, Ockham's rule provides sound advice: start with the known before assuming the unknown. But this is basically a matter of research strategy, and does not guarantee at all that the hypothesis chosen thus will prove to be the correct one.

The study of spontaneous cases of which this paper is the third has been approached based on these ideas. Primarily it should be demonstrated that these experiences are characterized by common traits. The results of studies on spontaneous cases published before, which will be discussed in a following paper together with the implications of the results of the present series of analyses, are

suggestive that such is the case. The present series of analyses support this assumption. These studies distinguish themselves from previous work by a much stronger emphasis on quantitative analysis, particularly quantitative analyses of relationships between variables inherent to spontaneous cases.

If spontaneous cases are characterized by common traits, then the nature of these traits and especially the nature of the relationship between different variables should provide us with hypotheses for further investigation. A quantitative analysis of relationships between variables even enables testing the validity of some of these hypotheses on the present material. The fact that these cases are characterized by common traits also provides a legitimate basis for experimental research in parapsychology. It makes clear that in experimental parapsychology we are not dealing with just some anomalistic results of experiments difficult to make sense of when considered on their own, but with a phenomenon which is akin to a specific type of experience familiar to large numbers of people.

Even without experimentation, it is often possible to test the viability of certain hypotheses by examining if they fit known observations. Two classes of hypotheses, more extensively discussed in the previous publications, are considered in this series of analyses. First I investigate whether a number of 'known' factors which might influence the nature of the reports on spontaneous cases can be held responsible for the observed findings. These hypotheses are collectively called 'reporting effects'. A second type of hypothesis investigated is that the labeling of these experiences as 'paranormal' is largely determined by cultural factors. The study presented here together with the analysis of the 'Sannwald' collection (Schouten, 1981) mainly serves to judge the validity of that hypothesis. Hence the object of these two studies is to investigate to what extent the traits characteristic of the British spontaneous cases are also found in collections of spontaneous cases from other countries and periods, and in what way they deviate.

Like in the analysis of the Sannwald collection, in this study I will compare the characteristics of the 'Rhine' collection with the 'Phantasms of the Living' collection analyzed earlier (Schouten, 1979b). As in both previous publications I will restrict myself to presenting the results of the analyses carried out. The relationship between the results of all three studies and the results of analyses of spontaneous cases published before by others will be discussed in a

subsequent publication.

THE RHINE COLLECTION

The name 'Rhine collection' denotes the largest collection of spontaneous paranormal experiences existing today and has been built up by L.E. Rhine primarily during the forties, fifties and sixties of this century. Her extensive analyses of these cases are presented in various publications (Rhine, 1951; 1953a; 1953b; 1954; 1955; 1956a; 1956b; 1957a; 1957b; 1960; 1962a; 1962b; 1963a; 1963b; 1964; 1967; 1969; 1978). The collection includes over 10,000 cases, based on letters sent to L.E. Rhine, and are typed versions of the experience as related in the authors' letters. These cases are all classified by L.E. Rhine according to the classifications she has described in her publications. Examples are 'type of ESP', 'form of experience', etc.. The typed versions are kept in folders so that each folder contains cases of the same combination of classifications (for instance 'telepathy-hallucination-visual-etc.'). Assuming that the numbers on the folders indicating the number of cases the folder contained are correct - in view of the size of the collection I did not check on that - the collection available to me involved 10,772 cases. Apart from these cases, there are also folders containing cases which L.E. Rhine judged to be unsuitable for the collection, these cases are also excluded from these analyses.

The 'Rhine' collection consists of cases which were sent in and accepted "with or without supplementary validation if they seemed to be contributed in good faith and by apparently sane individuals" (Rhine, 1951, p.165-166). At the time the study was initiated, in 1948, about a thousand cases were in the collection. The number increased over the years to some fifteen thousand which were available in the sixties, after which counting and active collecting ceased (Rhine, 1969, p.230).

Rhine states that a criterion for inclusion in the case collection was: "that the incidence as reported seemed to be one in which a person had received objective information not available to the senses" (Rhine, 1962a). She did not try to obtain independent testimonies for the reported cases. Not only because of the mere impossibility of such a task in view of the size of the collection, but also because she felt that to do so would not confirm to the objective of her study.

This objective was to get suggestions or hypotheses as to the way psi operates and the method was to use case material in a search for such hypotheses. Hence the objective was not to use the material to prove the existence of ESP, which would make corroborative evidence as to the accuracy and reliability of the reports essential. Rhine argues that the method should fit the objective of the study and that the objective of her study was best served by incorporating in the collection as nearly as possible a representative sample of all the ways in which psi appeared to occur in life situations. To include only validated case reports would necessarily bias any collection, and thus render it less useful for what she had in mind. To counter the effect of the inclusion of unreliable reports she based her study on large numbers of cases on the assumption that "the larger the number of cases of a kind, the more likely - that they represented a valid process and that individual mistakes of testimony could be considered to be canceled out" (Rhine, 1970, p.154).

For the present analysis I took a sample of these cases (PK cases excluded) by taking from each folder 15% of the cases. Hence, all types of cases classified by L.E. Rhine are equally represented in this study. However, in order to ensure that the results of this study are as much as possible comparable to the results of the previous two studies, I did not follow L.E. Rhine's classification when scoring the cases but applied the criteria used when scoring the other collections. Although one can expect considerable overlap between the classification as adopted by L.E. Rhine and my classification, some differences in distribution of types of classification are unavoidable.

When discussing the nature of the 'Sannwald' collection (Schouten, 1981) I described various differences between that collection and the 'Phantasms' collection. Most of these differences are also valid when comparing the 'Rhine' and the 'Phantasms' collections. The 'Phantasms' collection dates from Victorian England, the 'Rhine' collection from the American forties, fifties and sixties of this century. Although this period includes the second world war, the number of war cases was less than 6% of the total number of cases I scored. Consequently, contrary to the analysis of the 'Sannwald' collection, in this study the low number of war cases do not warrant separate treatment.

As discussed in the previous paper there are reasons to believe that the cases from the 'Phantasms' collection were mainly acquired from the intellectual and well-to-do circles of the society. There are no

indications that the same would be true for the 'Rhine' collection, so apart from a difference in epoch and country we can also assume that the two collections contain people of different social levels. The difference observed between the 'Sannwald' and 'Phantasms' collections as regards the general opinion about the nature of spontaneous paranormal experiences (Schouten, 1981, p.13) can be considered also to apply when comparing the 'Rhine' and the 'Phantasms' collections. The same holds for the difference created by the importance the collectors of the 'Phantasms' collection attached to waking experiences (ibid, p.14). Therefore, waking experiences can be expected to play a less pronounced role in the 'Rhine' cases.

As in the 'Sannwald' collection, the cases of the 'Rhine' collection are a mixture of GESP and precognition cases, but also contain a fair number of cases - mainly precognition - in which the experience is not related to a different target person but to the percipient him or herself. From introductory remarks made by contributors when about to describe their experience, it can be concluded that many persons who sent in a case did so after reading an article or listening to an interview or lecture on parapsychology.

THE ANALYSIS

Like in the 'Sannwald' study it was decided in advance that:

- 1) the replication study would involve all analyses reported in the 'Phantasms' study with the exception of the analyses of 'best-witnessed' cases because the latter are not available in the 'Rhine' collection.
- 2) the differences between the collections regarding the inclusion of war and precognition cases will in principle be ignored when running the analyses. However, if differences in results between the two collections are observed it will be investigated whether these differences are attributable to the inclusion of precognition cases. A further split-up in war and non-war cases will not be made because of the small number of war cases in this collection.
- 3) findings will be considered confirmed when the results based on all cases or on the GESP cases separately agree with the results of the same analysis carried out for the 'Phantasms' study.

4) all analyses will be based on cases in which a target person different from the percipient has been involved. Results of analyses based on cases in which the experience is related to the percipient himself (except for a few clairvoyance cases nearly all precognitive) will be discussed in a section 'Further Analyses'.

As stated before, the collection which was available to me for scoring numbered 10,772 cases. In view of the time available I decided to try to score 15% of these cases. To ensure the selection of a representative sample I took from each folder 15% of the cases the folder contained, the latter as indicated by a number listed on the back of the folder. The selection of cases from each folder was not done in any systematic way, although usually I scored cases in roughly equal frequencies from first and second half of the folder. Very short reports numbering only a few lines were generally omitted because they lacked too much data. However, I did include a number of them from different folders in view of the analyses in which 'length of report' would be one of the variables.

A practical problem was posed by the fact that the typed versions did not list the date the case was sent in. Consequently in many cases I was unable to score the category 'time elapsed before reporting'. (For details of the scoring categories applied see Schouten, 1979a.) This introduced a bias as regards the scoring for this category. Often the difference between the date the experience took place and the date of sending in the case was expressed in the narrative verbally like 'a few weeks ago', or 'a few years ago'. With the scoring categories applied the very old cases and the recent ones could often be scored reliably whereas the intermediate cases could not. For instance when it was stated "during the first world war" the case could be scored without trouble in the category time elapsed before reporting >20 years. Similarly when it was stated "a few weeks ago" the case could be scored in the category time elapsed before reporting less than half a year. On the other hand cases in which it was stated that the experience took place "a few years ago" could not be scored reliably in one of the categories applied for time elapsed before reporting. Therefore this scoring category shows an excess of very old and very recent cases.

The total number of cases scored is 1630. It turned out that in 1238 cases the experience was related to a different target person, that in 325 cases the experience was related to the percipient, and that in 67 cases the experience concerned a general event. The categories scored

were in principle the same as those scored in the 'Phantasms' study, but two categories were added ('sex of person who reported the case' and 'war or non-war' case) and some categories which either could not be scored at all (for instance those related to testimonies from other persons) or which for practical reasons proved to be not usable for the intended analyses (for instance 'probability of the event') were omitted. Also for practical reasons I did not score the category 'focussing'. Precognition cases were distinguished and scored in such a way that different types could be isolated. If in a precognition case both the percipient and another person were involved in the event, the case was scored in a separate category, but was treated in the analyses as if the precognitive experience concerned the other person. Since it is rather difficult to distinguish between telepathy and clairvoyance cases for the analyses, the few clairvoyance cases have been added to the telepathy cases and all treated as telepathy cases. (Actually it would have been more proper to use in all these studies the term 'GESP' instead of 'telepathy' but to avoid adding confusion I will stick to the term 'telepathy'.)

The significance level is set at .01. Strongly significant effects ($p < .0001$) will be indicated by $p < .01$. All p -values given are two-tailed. Because of the large number of analyses carried out, far over a hundred, for reasons of space details are only presented for a limited number of analyses. In most instances in which the data of this study confirm the data of the 'Phantasms' study I will only present the chi-square value and p level. If not otherwise stated, the nature of the relationship observed in this study reflects the same type of relationship as observed in the 'Phantasms' data. In the chi-square analyses, in most instances levels of variables with cell frequencies of less than 5 have been omitted. In each analysis, which were all done by computer, only those cases where all variables relevant for that analysis could be scored were included. Therefore, due to the fact that for most cases some categories could not be scored because of lack of data, occasionally differences in totals of the numbers reported in the various tables will be noticed. To facilitate comparison with the data of the 'Phantasms' study, each analysis is accompanied by the number of the page where the corresponding 'Phantasms' analysis was reported or the number of the relevant table. Unless otherwise stated, all analyses reported in the following section are based on cases in which a target person other than the percipient has been involved.

RESULTS

The percipient

The proportion of female percipients (83.3%) is strongly different from the proportion of male percipients (16.7%), and much higher than observed in the 'Phantasms' collection (56.2% females versus 43.8% males). The nature of the difference, e.g. more female percipients compared to male percipients, confirms the data of the 'Phantasms' study ('Phantasms', p.414). However, the distribution of male and female percipients of the two collections differ to a significant degree ($\chi^2=148.8$, $df=1$, $p<.01$). When all cases in which a target person is involved are split up into telepathy and precognition cases the distribution of male and female percipients for the two types of cases appear not to differ ($\chi^2=1.8$, $df=1$). Hence we can conclude that the observed difference in proportion of male and female percipients between the two collections is not influenced by the incorporation of precognition cases in the 'Rhine' collection.

When the total number of males and females involved in these cases either as percipient or target person is considered another difference between the previous study emerges. In the 'Phantasms' collection significantly more males were involved in the collection ('Phantasms', p.414), in this collection significantly more females (1470 females versus 988 males).

As in the 'Phantasms' collection ('Phantasms', table 1) the higher proportion of female percipients is observed for all types of relationships (see table 1). Thus, the high frequency of female percipients can not be explained by assuming that it would be caused by a predominance of a special type of relationship involving female percipients.

However, the finding in the 'Phantasms' data that in the category 'children/parents' an exception was observed, e.g. a higher frequency of male percipients (sons) than of female percipients (daughters), is not confirmed by the data of the present study. Unlike the 'Sannwald' study where a trend suggestive of this effect was still observed not even such a trend is noticeable in the present data.

Another possible explanation might be that the excess of female percipients is due to an excess of female contributors of cases. In

TABLE 1
Male versus female percipients in different relationships

	male P		female P	
spouses	23	11.4%	178	88.6%
siblings	26	20.3%	102	79.7%
parent/children	33	11.8%	247	88.2%
children/parent	34	17.8%	157	82.2%
other family members	23	14.9%	131	85.1%

this collection like in the other ones ('Phantasms', p.415) the majority of the cases (85%) were sent in by the percipients. We could learn if women were more inclined to send in a case by comparing the number of male and female contributors not being percipients or target person. Contrary to the 'Phantasms' collection where males were the more active contributors of cases ('Phantasms', p.416) in the 'Rhine' collection females were significantly more active in this respect (for all cases including those without a target person: 121 females versus 41 males; $z=6.28$; $p<<.01$). This finding indicates that in this collection females have been more inclined to report a case than males. In other words, the collection was acquired from a female dominated part of the society. Undoubtedly this will have contributed to raising the number of female percipients.

A further difference is observed when studying the relationship between type of contributor and distribution of male and female percipients. In the former study ('Phantasms', table 2) no difference was observed in proportion of female percipients for the different groups of contributors. In the present study significantly more female percipients are observed in cases reported by the percipients compared to cases sent in by contributors not directly involved in the case. The data are presented in table 2. From this table it appears that the difference in proportion of female percipients between the different groups of contributors is quite substantial. The percentage of male

TABLE 2
Distribution of percipients in relationship
to type of contributor

contributed by	male P		female P	
percipient or target person	169	15.1%	947	84.9%
others	36	30.3%	83	69.7%
	chi-square=16.7, df=1, $p < .01$			

percipients is twice as large for the group of 'other' contributors. This implies that either the 'other' contributors were less inclined to send in a case when it concerned a female percipient, or that male percipients have been more reluctant to relate their experiences than female percipients. An indication of the validity of the first hypothesis can be found by investigating the relationship between male and female contributors not directly involved in the cases reporting on male and female percipients. It turns out that sex of contributor (not percipient and not target person) does not influence the distribution of male and female percipients (chi-square= .2, df=1). Consequently, the second hypothesis stated above seems a more acceptable candidate for explaining the observed difference in table 2. Thus it seems likely that the excess of female percipients in this collection is also strengthened by a reluctance on the part of male percipients to report their experiences.

Hence it can be concluded that in the 'Rhine' collection the proportion of female percipients is much higher compared to the 'Phantasms' collection. However, two reporting effects which were either absent or had an effect in the opposite direction in the 'Phantasms' collection have contributed to this phenomenon. In the 'Rhine' collection male percipients appear to have been less inclined to send in a case. Furthermore, judging from the difference between number of male and female contributors not directly involved in a case, the 'Phantasms' collection seems to be acquired from a male

dominated part of the society, whereas the 'Rhine' collection seems to be acquired from a female dominated part of the society.

The target person

The distribution of male and female target persons in this collection (64% male target persons versus 36% female target persons) does not differ from the distribution of target persons observed in the 'Phantasms' collection ($\chi^2 = .03$, $df=1$) ('Phantasms', p.417). It is a remarkable fact that despite the strong differences in proportion of male and female percipients in all three collections the proportion of male and female target persons is nearly the same. This implies that the increase of female percipients over the three collections analysed has not been accompanied by a similar increase in male target persons. Hence one could say that compared to female target persons the proportion of male target persons has decreased.

As was found in the previous study ('Phantasms', table 4) the distribution of sex of target person over the various types of relationships show that for each type of relationship a higher proportion of male target persons is observed (see table 3).

TABLE 3
Male versus female target persons in different relationships

	male target person		female target person	
spouses	178	88.6%	23	11.4%
siblings	86	66.7%	43	33.3%
parents/children	185	66.1%	95	33.9%
children/parents	93	48.7%	98	51.3%
other family	82	53.6%	71	46.4%
acquaintances	146	59.6%	99	40.4%

The only exception is formed by the category 'children/parents', in which female target persons (mothers) have a higher frequency than the males (fathers). This result also confirms the corresponding finding in the previous study.

When investigating possible reporting effects which might have influenced the observed higher proportion of male target persons, it was found that, as in the 'Phantasms' study ('Phantasms', table 5), no difference is observed when comparing the distribution of male and female target persons for the various groups of contributors (percipient or target person versus contributors not directly involved in the cases) ($\chi^2=3.0$, $df=1$). However, contrary to the results of the previous study ('Phantasms', table 6) a weak but significant relationship exists between sex of target person and seriousness of event ($\chi^2=16.1$, $df=3$, $p=.001$). Male target persons were slightly more involved in serious events than female target persons. However, when we compare the distribution of male and female target persons between the two collections for the different types of events (death, serious accidents, etc.) separately in none of the comparisons is a significant difference found. Therefore it seems safe to conclude that although this reporting effect might have slightly influenced the proportion of male target persons - as will be recalled serious events appear to have a higher likelihood of becoming the topic of a paranormal experience - the effect is so weak that it certainly can not explain the significant excess of male target persons.

The relationship between percipient and target person

The distribution of cases over the various types of relationships (see table 4) conforms to the same pattern as those observed in the 'Phantasms' collection, but when the actual frequencies are compared the two distributions differ to a significant degree ($\chi^2=36.7$, $df=3$, $p<.01$). The 'Rhine' collection includes relatively more cases between near relatives and fewer cases between friends and acquaintances. This difference is not due to the introduction of precognition cases in the 'Rhine' collection, because the difference in distribution of number of cases over the types of relationships between telepathy and precognition cases is not significant ($\chi^2=2.6$, $df=2$, strangers excluded).

In order to find out whether the high proportion of cases between

TABLE 4
Number of cases for different groups of relationships

	n	'Rhine'	'Phantasms'
close family	801	65.4%	51.6%
other family	154	12.6%	13.4%
acquaintances	250	20.4%	32.8%
strangers	20	1.6%	2.2%

near relatives is influenced by a reporting effect, e.g. that cases between near relatives have a higher likelihood of detection because it can be assumed that in general members of the same family have more frequent contact with each other, I also compared the distribution of all cases over the types of relationships with the same distribution for only death cases. The rationale behind this analysis is that I assume that the likelihood of the percipient's learning of the death of the target person will be by and large equal for all groups, independent of the type of relationship. Contrary to what has been observed in the corresponding analysis in the 'Phantasms' study ('Phantasms', table 8) the two distributions in the 'Rhine' collection differ to a significant degree ($\chi^2=26.9$, $df=3$, $p<<.01$) (see table 5).

From the data of table 5 it appears that for the category 'close family' the frequency of death cases is relatively lower than the frequency of all cases, while in the category 'other family' death cases are more prominent compared to other types of events. This suggests that this reporting effect is indeed related to a relatively higher frequency of non-serious cases between near relatives. In order to test this assumption I compared the distribution of serious events (death and serious accidents) and non-serious events (slight injuries, trivial, and positive) for the different types of relationships (see table 6).

TABLE 5

Number of cases for different types of relationships for all cases
and for cases involving the death of the target person

	all cases		death cases	
close family	801	65.4%	255	55.2%
other family	154	12.6%	100	21.6%
acquaintances	250	20.4%	104	22.5%
strangers	20	1.6%	3	0.6%

TABLE 6

Relationship between seriousness of event and
type of relationship

	death	serious accident	not serious
close family	256	276	271
other family	100	25	39
acquaintances	104	37	102

In my opinion the data presented in this table do not support the assumption stated above. Only in the 'other family' group fewer non-serious cases are observed. However, between the two largest types of relationships (close family and acquaintances) no difference in proportion of death and non-serious cases is found. It appears to be rather the high proportion of cases involving serious accidents which

creates the significance of table 5. It seems reasonable to assume that the likelihood of detecting cases involving serious accidents is rather equal for the three types of relationships. Hence an explanation for the high frequency of cases between near relatives based on the assumption of a higher likelihood of detecting a case between near relatives seems inappropriate. It appears that the physical well-being of the other is of more importance between near relatives than between members of other types of relationships. Thus it can be concluded that a reporting effect as stated above, assuming a higher likelihood of detecting a case between near relatives, only plays a minor role (it is clear that in the 'other family' category non-serious events do have a lower frequency). This conclusion does support the similar finding in the 'Phantasms' study. However, it remains unclear why the 'Rhine' collection involves so many cases related to serious accidents between near relatives compared to the other collections.

Another reporting effect supposes that more cases between near relatives are observed because the death of a near relative would be more impressive for family members than for outsiders and therefore more frequently reported. No evidence is found for such an effect. The number of death cases reported by family members or outsiders do not differ for the various types of relationships ($\chi^2=5.3$, $df=2$) ('Phantasms', table 9). A similar type of reporting effect assumes that percipients are more impressed by the death of a close relative and therefore report a higher number of (death) cases between near relatives. In the 'Phantasms' study it appeared that a significant effect of this type was observed but with an opposite influence on the data. Percipients were less inclined to report a case involving the death of a near relative ('Phantasms', table 10). The same trend is also observed in the 'Rhine' data, but the effect is not strong enough to reach significance ($\chi^2=7.7$, $df=2$, $p=.02$). Hence these reporting effects can not explain the higher number of cases observed between near relatives.

In table 7 the distribution is presented of number of cases for the various types of relationship within the category 'near relatives'. The general pattern of this table confirms the data of the 'Phantasms' collection ('Phantasms', table 11). However, there is one important difference between the two tables. From the 'Phantasms' data it appeared that within the category 'parent/child' the distribution of sons and daughters as target persons was strongly skewed in favor of the sons. In this collection the sons seem to have lost their favorite

TABLE 7
Number of cases between near relatives

main category	p	t	n
spouses	female	male	178
	male	female	23
parent/child	father	son	23
	father	daughter	10
	mother	son	160
	mother	daughter	87
child/parent	son	father	16
	son	mother	18
	daughter	father	78
	daughter	mother	79
siblings	brother	brother	16
	brother	sister	10
	sister	brother	69
	sister	sister	33

position, the proportion of sons being target person is 66.6%, nearly equal to the average proportion of male target persons (64%). Also, within the category 'child/parent' sons tended to report more experiences related to their parents than did daughters - a remarkable feature considering the general dominance of female percipients - which is no longer the case in the 'Rhine' collection. It looks as if the special mutual attachment between the sons and parents has somewhat cooled off. On the other hand, as stated before the higher frequency of mothers as target persons in cases between children and parents is also apparent in the data of this collection. We could conclude from the data of the 'Phantasms' collection that both the mother and the son played an exceptional role in the close family; in this collection only the mother can be considered as having retained such a special status.

The nature of the experience

The distribution of types of experiences differs strongly and significantly from the corresponding distribution based on the 'Phantasms' cases ('Phantasms', table 12). The data are presented in table 8.

TABLE 8
Distribution of types of experiences

intuitions	33.6%
dreams	41.7%
waking experiences	20.9%
others	3.8%

Waking experiences are strongly reduced in favor of intuitions and dreams. This phenomenon, which was also noted in the 'Sannwald' collection, most likely reflects a true cultural difference between the collections. However, it should be kept in mind as already stated when discussing the data of the 'Sannwald' collection that the collectors of the 'Phantasms' cases for several reasons had a preference for 'apparition' cases. Hence the actual difference in distribution of types of experiences between the cultures from which the cases were drawn is probably less than the present data suggest.

When comparing the distribution of types of experiences for telepathy and precognition cases separately, it turns out that all comparisons yield highly significant results (telepathy cases versus 'Phantasms': $\chi^2=169.4$, $df=3$, $p<<.01$; precognition versus telepathy: $\chi^2=96.8$, $df=3$, $p<<.01$). Hence it can be concluded that the distribution of types of experiences is different for the two collections, even when only telepathy cases are considered.

Male and female percipients do not differ regarding the distribution

of types of experiences ($\chi^2=5.1$, $df=3$) which confirms the similar finding from the 'Phantasms' study ('Phantasms', p.427). The same holds true for the relationship between type of experience and sex of target person ('Phantasms', p.427) ($\chi^2=5.6$, $df=3$). A difference between the outcomes of the two collections is observed, however, when the relationship between type of experience and time of the day the experience took place is considered ('Phantasms', p.427). In the 'Rhine' collection a significant relationship emerges ($\chi^2=44.8$, $df=8$, $p<.01$; intuitions, waking, and others - dreams excluded - against night, morning, afternoon, evening, and a fifth level 'by day'). As will be discussed later in the section 'Failures to replicate', this difference between the two collections is largely artifactual. In fact, both collections appear to show the same type of relationship between type of experience and time of the day.

When the distribution of cases over the time of day (again dreams excluded) for the two collections are compared, no difference is observed ($\chi^2=7.6$, $df=3$) ('Phantasms', table 13). Like in the 'Phantasms' collection, significantly more experiences are reported for the evenings and nights. This difference becomes even more pronounced when we deduct the 8 hours needed for sleep from the number of hours (14) applied for evening and night together. If dreams are also considered then of course by far the majority of experiences are associated with the dark hours. On the other hand, in most cases no time of the day is stated and it is conceivable that when a time of the day is stated the probability of all four levels - night, morning, afternoon, evening - being mentioned might not be equal. Unfortunately, the influence of this type of reporting effect can not be estimated due to lack of data.

The distribution of cases over the seasons does not deviate from an equal probability distribution ($\chi^2=10.5$, $df=9$) ('Phantasms', p.428). No relationship is observed between type of experience and season ($\chi^2=11.4$, $df=9$) ('Phantasms', p.428), and between time of day of the experience and season. These findings confirm the results of the 'Phantasms' study. But contrary to the data of the previous study the present data show a weak but significant relationship between type of experience (dreams excluded) and the percipient being indoors or outdoors ($\chi^2=16.0$, $df=2$, $p<.01$) ('Phantasms', p.428), due to the fact that in this collection waking experiences are strongly related with the percipient being indoors. This effect is independently significant for the telepathy cases

(chi-square=15.1, $df=2$, $p<.01$), but not for precognition cases (chi-square=2.9, $df=2$). Although the latter result is based on a rather small number of cases ($n=103$) it should be noted that in the precognition cases not even a trend to the same effect can be discerned.

Males and females do not differ in the distribution of number of experiences over time of day (chi-square=9.4, $df=4$), a result which confirms the result of the previous study ('Phantasms', p.429). The same applies to the observation that compared to females the males in Victorian England had a spontaneous experience relatively more often when outdoors ('Phantasms', p.429). American males show the same preference (chi-square=14.6, $df=1$, $p<.01$). In the former study no relationship was observed between social situation (percipient being alone or with others) and type of experience (dreams excluded, 'Phantasms', p.429). In the present data such a relationship does exist caused by the strong association between waking experiences and being alone. This difference is not due just to the larger number of cases in the 'Rhine' collection permitting a trend to reach significance, but is rather a true difference because even a trend in this direction is absent in the 'Phantasms' collection. In that study waking experiences were almost equally divided over the categories 'being alone' and 'being with others' (138 versus 119), while in the 'Rhine' collection the proportion is nearly 4 to 1 in favor of being indoors.

No relationship is observed between the variables type of experience and seriousness of event (chi-square=3.9, $df=2$). Such a relationship was observed in the data of the previous study ('Phantasms', table 14) due primarily to the very skewed distribution of death and non-serious cases in waking experiences. Although in the present data of all types of experiences waking impressions still yield the relatively highest proportion of death cases, the distribution is much less extreme. Here again I suppose that the interest of the collectors of the 'Phantasms' cases in apparitions of the dead and the dying and the disdain they had for the unimportant non-serious events has somewhat biased their collection and contributed to this discrepancy between the two sets of data. The results of the analyses concerning the relationship between type of experience and identification of target person and of event do confirm the corresponding results observed in the other collection ('Phantasms', table 15). Identification of the target person took place relatively more often in waking experiences while events are more often identified via dreams (chi-square=28.2, $df=2$, $p<<.01$).

The event

The distribution of events presented in table 9 shows largely the same pattern as observed in the 'Phantasms' collection ('Phantasms', table 16), a predominance of negative events and a virtual absence of events related to material damage.

TABLE 9
Distribution of events

	'Rhine'	'Phantasms'
death	37.7%	66.7%
serious accident	27.5%	12.5%
slight injuries	18.5%	8.7%
serious material	.3%	.5%
slight material	.6%	.2%
trivial	10.8%	10.0%
positive	4.5%	1.4%

However, when the actual frequencies for the two collections are compared a difference is observed (chi-square=141.9, df=4, $p < .01$). In the 'Rhine' collection significantly fewer death cases are included, but more cases involving serious accidents and slight injuries. When the distributions of events are considered for telepathy and precognition cases separately, both appear to differ significantly from the corresponding distributions based on the 'Phantasms' data (telepathy: chi-square=165.0, df=4, $p < .01$; precognition: chi-square=59.1, df=4, $p < .01$). Of even more interest is that the distributions for telepathy and precognition cases also differ very strongly from each other (chi-square=54.3, df=4, $p < .01$; material events excluded). From the frequencies observed it appears that precognitive experiences involve relatively more death cases (49.0% versus 32.9% for telepathy), but less non-serious events (22.5% versus

38.6% for telepathy cases). As stated before, it should be kept in mind that when results about precognition cases are reported unless otherwise stated it concerns only cases in which a target person different from the percipient has been involved.

In the 'Phantasms' study the relationship between seriousness of event and time elapsed before reporting was significant to the effect that non-serious cases appear to become more easily forgotten ('Phantasms', table 17). The same holds true for the 'Rhine' collection ($\chi^2=9.7$, $df=1$, $p<.01$) where the data reveal a similar trend. The finding that compared to death cases, non-serious cases are more frequently sent in by the percipients than by other contributors ($\chi^2=10.8$, $df=2$, $p<.01$) is also in agreement with the 'Phantasms' study.

The content of the experience

In 78.5% of the cases the target person was correctly identified by the percipient. This value is almost equal to the percentage observed in the 'Phantasms' study (81.7%) and the distributions of target persons identified and not identified for the two collections do not differ ($\chi^2=2.2$, $df=1$).

In 65.2% of the cases the event was correctly identified. Although the fact that the target person is identified correctly more often than the event confirms the corresponding finding from the previous study, the distributions of correctly and not correctly identified events for the two collections differ to a significant degree ($\chi^2=28.0$, $df=1$, $p<<.01$). When the same analysis is carried out for the cases of the 'Rhine' collection split up into telepathy and precognition cases, for telepathy cases the percentage of correctly identified events reduces to 59.5%. This seems a borderline case when having to decide whether this result confirms the data of the previous study since the associated χ^2 value of the difference between the 59.5% of this study and the 51.9% of the previous one drops to precisely the .01 level. For precognition cases the percentage of correctly identified events rises to 78.7%. Not surprisingly the distributions of identification of event for telepathy and precognition cases also appear to differ to a significant degree ($\chi^2=41.0$, $df=1$, $p<<.01$). Thus it follows that in precognition cases in which another target person is involved the event is

relatively more often correctly identified.

No difference is observed between male and female percipients regarding the proportion of correctly or not correctly identified target persons ($\chi^2=1.2$, $df=1$) and regarding their ability to correctly identify the event ($\chi^2=0.17$, $df=1$). Both results confirm the findings of the previous study ('Phantasms', p.436). No relationship was observed between conviction and sex of percipient ($\chi^2=0.34$, $df=1$). At least when applying the .01 level as criterion for significance this result can also be considered as confirming the previous study ('Phantasms', p.436). It should be mentioned, however, that both sets of data show male percipients reporting a higher proportion of conviction cases. A similar situation exists for the relationship between taking action and sex of percipient. However, this is also a borderline case as far as confirmation is concerned because the non-significant relationship observed in the 'Phantasms' data reaches again exactly the .01 level in the present data ($\chi^2=6.7$, $df=1$). Not surprisingly, the data from the two collections indicate that males are slightly more inclined to take action based on their spontaneous experience than females.

A difference between the two collections appears when analysing the relationship between taking action and the identification or not identification of the target person. Whereas this relationship is not significant in the data of the 'Phantasms' collection ('Phantasms', p.436), there is a significant tendency for action to be taken more frequently when the target person is identified in the cases of the 'Rhine' collection ($\chi^2=17.3$, $df=1$, $p<<.01$). On the other hand, the nature of the relationship between taking action and type of experience appears to be the same in both collections. In the 'Phantasms' collection ('Phantasms', table 20) intuition cases triggered action more often in the percipients, while with dream experiences this trend is reversed. In the 'Rhine' collection the relationship is again strongly significant ($\chi^2=102.3$, $df=2$, $p<<.01$) revealing the same pattern. Not unexpectedly, the similar type of relationship between conviction and type of experience ('Phantasms', p.438) is also found in the data of the 'Rhine' collection ($\chi^2=116.5$, $df=2$, $p<<.01$). Intuition cases are most often associated with a feeling of conviction, dream experiences least often.

The average number of details in the 'Rhine' collection is

substantially higher than the number of details observed in the 'Phantasms' collection ('Phantasms', p.439). A comparison shows 1.14 detail per case in the 'Rhine' collection versus 0.56 detail per case in 'Phantasms'. The higher number of details is partly due to the inclusion of precognition cases. The average number of details drops to 0.9 per case when only telepathy cases are considered. Nevertheless, even this value is significantly higher than the comparable value of the 'Phantasms' study. This is revealed by comparing the number of cases with 0, 1, or >1 details in the two collections ($\chi^2=37.6$, $df=2$, $p<.01$). However, a much stronger difference is observed when comparing in the 'Rhine' collection precognition and telepathy cases ($\chi^2=105.5$, $df=2$, $p<.01$). From the data it appears that precognitive experiences are much richer in detail than telepathic experiences.

No relationship is observed between number of details and time elapsed before reporting ('Phantasms', table 21, p.440). This is true regardless of whether time elapsed before reporting is dichotomized into <1 year and >1 year ($\chi^2=1.08$, $df=2$) or whether the same analysis is carried out for all time intervals longer than 1 year split up into the different scoring intervals ($\chi^2=4.6$, $df=10$). A similar result was observed when comparing length of report - in this study length of report was scored by counting the number of lines of each report - and time elapsed before reporting. Here length of report split up into 4 intervals and time elapsed before reporting into the intervals of <5 years; 5-10 years; 10-15 years; and >20 years ($\chi^2=10.1$, $df=9$). A weak but just significant relationship appears to exist between number of details and length of report ($\chi^2=17.4$, $df=6$, $p=.008$), the longer reports showing a tendency to relate to experiences involving more details. All these findings confirm the corresponding findings based on the analyses of the 'Phantasms' cases. In the 'Phantasms' study ('Phantasms', table 23) a strong relationship was observed between number of details and type of experience to the effect that dreams are associated with more details than intuitions. The same relationship is observed in the 'Rhine' collection but reaches here an extreme value of significance ($\chi^2=304.0$, $df=4$, $p<.01$) due, at least in part, to the size of the collection.

The relationship between number of details and seriousness of event ('Phantasms', table 24) from which it appears that in general non-serious events are more rich in details is also confirmed by the data of the 'Rhine' collection ($\chi^2=104.6$, $df=2$, $p<.01$). The

same applies to the observation from the previous study ('Phantasms', p.442) that male and female percipients do not differ in the average number of details in their experiences. For the 'Rhine' collection this analysis yielded an equally non-significant result ($\chi^2=0.7$, $df=2$). As stated before I consider this result as another indication that the difference in frequency of male and female percipients observed in all three collections is not due to a difference between the sexes in ESP ability, but must be related to some other factors.

In the analysis of the 'Phantasms' collection it could be shown that the number of details was not related to the identification of the target person, but very strongly to the identification of events ('Phantasms', table 25). Both findings are fully supported by the data of the 'Rhine' collection, and due to the larger number of cases in this collection the difference between significance level of the two analyses becomes even more pronounced. The results are presented in table 10.

TABLE 10
Relationship between identification of target person,
of event, and number of details

	number of details		
	0	1	>1
target person identified	412	221	308
not identified	115	73	69
	$\chi^2=4.3$, $df=2$		
event identified	233	219	351
not identified	301	91	37
	$\chi^2=222.6$, $df=2$, $p<<.01$		

It is of interest to note some further differences between identification of target person and of event. Conviction is positively

related to the identification of the target person. A similar type of relationship exists between identification of the target person and taking action. The probability that the percipient will take some action is higher in the case of identification. However, there is no such relationship between conviction and taking action with the variable identification of event. L.E. Rhine's hypothesis that conviction should be considered as a separate element is strongly supported by the finding that the feeling of conviction is independent of the correct identification of the event. But if we accept this hypothesis to be true, then what causes the relationship between conviction and identification of the target person? I will return to this problem in a following paper when discussing the results of all three analyses. Also, as observed in the previous study ('Phantasms', p.444) a significant relationship exists between number of details and conviction ($\chi^2=61.6$, $df=2$, $p<<.01$), indicating that in general conviction is stronger the fewer the number of details.

In the present collection distance has been scored in a slightly different way because distances within the U.S.A. are in general much larger than those in Europe from which the two collections analysed before originated. When scoring 'distance' in the present collection I included the category 'percipient and target person in different states'. For some analyses I combined the categories 'town' and 'same state' into one category termed 'near distance'; and the categories 'different state' and 'different countries' into one category 'large distances'.

The data support the finding reported in the previous study that distance is significantly related to seriousness of event ('Phantasms', p.444) ($\chi^2=41.0$, $df=1$, $p<<.01$), non-serious events being associated with near distances. A similar result is obtained when investigating the relationship between distance and type of relationship between percipient and target person ('Phantasms', p.445). As in the previous study, the data of the 'Rhine' collection show that cases involving large distances happen more frequently between near relatives ($\chi^2=29.4$, $df=2$, $p<<.01$). Not surprisingly, we also find a relationship between distance and number of details in the direction one would expect from the results mentioned above: cases involving large distances are associated with relatively less details ($\chi^2=49.1$, $df=6$, $p<<.01$). A similar trend was observed in the previous study ('Phantasms', p.445) but as was noted already the small number of non-serious cases in that collection prevented this effect from reaching a significant level.

For similar reasons, a further difference between the two collections is observed. In the 'Phantasms' collection no relationship appeared between distance and type of experience ('Phantasms', p.445). In the present data this relationship is significant ($\chi^2=32.2$, $df=6$, $p<.01$) to the effect that, contrary to dream and waking experiences, intuition cases are more strongly associated with shorter distances.

The last analysis reported in this section of the 'Phantasms' study concerns the relationship between sex of percipient and distance ('Phantasms', p.445). The result of the present study confirms what was observed in the previous study, viz. that male and female percipients do not differ in the proportion of cases involving different distances ($\chi^2=5.2$, $df=3$).

DISCUSSION OF THE FAILURES TO REPLICATE

By a very conservative estimate, if all analyses, exceptions observed in the 'Phantasms' collection, etc, are considered and the two 'borderline' cases rated as failures to replicate, one might conclude that of the 57 results discussed in this study, 19 did not confirm the corresponding results of the 'Phantasms' study. In this count two results discussed before have been left out, because it is disputable whether to expect confirmation on these issues: the distribution of males and females over all persons involved in the collection and the proportion of males and females in the group of contributors other than percipient or target person.

Fortunately, the confirmative strength of this study is actually better than the 19 out of 57 failures to replicate do suggest. When replicating an observed effect two aspects of the data are of importance, direction and magnitude of the effect. For these studies this implies that first, the analysis should or should not yield a significant result in correspondence with the outcome of the same analysis of the foregoing study and, if significant, it should reflect the same type of relationship. Secondly, the distributions based on the actual frequencies observed in the two collections should not differ. In the evaluation given above I counted an analysis confirmative only if it satisfied both conditions, otherwise the result was considered as a failure to replicate. Take for example the proportion of male and female percipients. In both collections there was a significant higher proportion of female percipients observed.

Nevertheless this analysis was counted as a failure to replicate, because the distributions of male and female percipients based on the actual frequencies differed for the two collections. If only type of relationship is considered, one would conclude that no result of the present study revealed a relationship in conflict with the data of the previous study. What occasionally happens is that in the data of both collections the same type of relationship appears but that in one collection, for instance because the total number of cases is substantially less, its strength is not sufficient to reach significance, while in the other collection it does. Also, as was already demonstrated in the discussion of the results of the 'Sannwald' study, some failures to replicate are mutually dependent. Consequently one essential difference between the two collections, such as the distribution of types of experiences, can lead to a number of failures to replicate for analyses including this variable.

In the following section, the analyses which were counted as failures to replicate have been grouped together according to their apparent associations.

1. Difference in type of experience.

As stated before in the present collection the proportion of waking experiences is much lower and the proportion of intuitions and dreams much higher than in the 'Phantasms' collection. This difference will necessarily influence the outcome of a number of analyses in which type of experience is included as a variable. In this respect the difference in proportion of waking experiences and intuitions is especially of importance, because for obvious reasons a number of analyses were carried out with dreams excluded. In the 'Phantasms' collection more than 5.6 times as many waking experiences as intuitions were reported. In the 'Rhine' collection this ratio is reversed. Intuition cases appear 1.6 times as often as cases involving waking imagery. Despite the fact that, as discussed before, the 'Phantasms' collection is probably strongly biased in favor of waking experiences, in view of the pronounced changes I consider the observed differences in distribution of types of experiences between the two collections to be a genuine cultural effect.

The failures to replicate which I consider to be related to the difference in distribution of types of experiences between the two

collections are as follows:

1.1 In the 'Rhine' collection a relationship between time of day and type of experience was observed, indicating a higher proportion of intuition cases for the mornings and afternoons. However, the reasoning as outlined in the corresponding section of the publication on the results of the analyses of the 'Sannwald' collection is also valid for this study. If we assume 8 hours of nighttime sleep, then we find that the average number of intuition cases is equally distributed throughout the 24 hours of the day (20.4 for daytime and 23.7 for evening and night). On the other hand, waking experiences are more strongly associated with the evenings and nights (an average of 9.8 per hour for daytime and of 24.0 for the dark hours). The same trend is clearly noticeable in the 'Phantasms' data. There the average number of waking experiences is 31.6 per hour of the evening and night and 8.5 per hour for daytime, whereas the corresponding figures for intuition cases are 3.5 and 2.2. Hence we can conclude that despite a difference in significance levels, the data of the two collections do not contradict each other on this issue.

1.2 The 'Rhine' collection also yielded a relationship between type of experience and the percipient being indoors or outdoors, while no such relationship was found in the 'Phantasms' collection. The data suggest that the significance is caused primarily by the extremely skewed distribution for waking experiences in the 'Rhine' collection. Waking experiences happen 8.5 times more often when the percipient is indoors than when outdoors, whereas for intuition cases this ratio is only 3.6:1. In the 'Phantasms' collection both types of experiences show nearly the same ratio between the percipient being indoors or outdoors (waking 3.5:1; intuition 3.3:1). No indication was found in the data which might help to explain this peculiarity of the 'Rhine' cases.

1.3 The next failure to replicate is probably related to the one mentioned in 1.2. In the 'Phantasms' collection no relationship was observed between the percipient being alone or not and type of experience. In the present collection such a relationship does exist. Here waking experiences do take place significantly more often when the percipient is alone. Hence the 'Rhine' collection presents a picture, in agreement with the former study, that waking experiences do happen more frequently in the evenings and nights, but also that waking experiences happen more frequently when the percipient is alone and indoors. In my opinion the latter two effects (alone and indoors) reflect a real difference between the 'Phantasms' and 'Rhine'

collections and consequently may be considered influenced by cultural factors. Since waking experiences dominated in the 'Phantasms' collection where similar trends were not found, it would not be correct to consider these differences as merely a consequence of the higher frequency of intuition and dream cases in the 'Rhine' collection.

1.4 In the 'Phantasms' study waking experiences were related most strongly to death cases. In the 'Rhine' collection a similar trend is observed, but due to the lower number of waking experiences the trend does not reach a significant level.

1.5 In the 'Phantasms' collection no significant relationship was observed between distance and type of experience. In the present collection intuitive experiences are significantly associated with cases involving short distances. Here again the difference is merely a matter of degree of strength. In both collections the rank order of association between type of experience and distance is the same (from short to long: intuitions, dreams, waking). Hence, it can be safely assumed that this failure to replicate is a direct consequence of the difference in distribution of types of experiences between the two collections.

2. Difference in type of event

The distribution of types of events displays the same trends as observed in the 'Phantasms' data, e.g. a predominance of death cases and other negative events and the virtual absence of cases concerning material damage. However, the 'Rhine' collection includes a significantly higher proportion of serious accidents and slight injuries but less death cases. This difference between the two collections is partly influenced by the preference of the collectors of the 'Phantasms' cases for apparitions.

2.1 As has been discussed before, there is a weak but significant trend in the 'Rhine' data indicating a relationship between seriousness of event and sex of target person. In both collections it appears that compared to the other types of events in the category 'serious accidents' males constitute the highest proportion of target persons. So, not surprisingly, the substantial increase of frequency of cases in this category of the 'Rhine' collection may be responsible

for this relationship reaching significance. Hence it appears that males and females have a nearly equal probability of experiencing all types of events except for serious accidents for which males seem to be more vulnerable than females.

2.2 The same applies to the observed difference in the 'Rhine' collection between distribution of number of cases over the various types of relationships for all cases and for only death cases. As discussed before, this difference is also caused mainly by the excess of serious accidents. From table 6 it appears that of all types of events the category 'serious accidents' is most strongly associated with the category 'close family'. Hence 'all cases' show a substantial increase in percentage for the 'close family' group by the inclusion of serious accidents. This inclusion, with its large numbers is primarily responsible for the significance observed.

2.3 Although the rank order remains the same, the distribution of cases over the types of relationships differ between the two collections. The 'Rhine' collection includes about 14% more cases between near relatives and about 12% fewer cases between friends and acquaintances. I suppose that this difference is also related to the observed difference in types of events because of the relationship between near family and serious accidents previously mentioned.

2.4 The average number of details is higher in the present collection. This is partly caused by the inclusion of precognition cases with their higher average number of details as compared to telepathy cases. But even when only the telepathy cases are considered the difference remains significant. We know that seriousness of event is strongly related to number of details, e.g. death cases are frequently without details whereas the reverse is true for non-serious cases. Consequently, the drop in proportion of death cases from 66.7% in the 'Phantasms' collection to 37.7% in the present data has undoubtedly contributed to the increase in the average number of details in the 'Rhine' collection.

2.5 From the present data it appears that when the target person of the experience was identified, action is taken more frequently. I suppose that this difference is also a consequence of the low frequency of death occurrences in this collection. When investigating the relationship between taking action and seriousness of event it turned out that in only 10% of the death cases was action taken, probably because the necessity for taking action no longer existed.

For the other types of events this percentage appeared to be significantly higher (38%).

Miscellaneous

3.1 The proportion of female percipients is substantially higher in the 'Rhine' collection than in the 'Phantasms' collection. However, the data clearly indicate that at least two reporting effects have contributed to this difference. In the first place it appears, when considering the contributors not directly involved in the cases, that in the 'Phantasms' collection males significantly more often contributed cases. The opposite is observed in the 'Rhine' collection. This fact, together with the information we have on how these collections were acquired and by whom, indicates strongly that the cases for the 'Phantasms' collection were mainly acquired from a male dominated part of the society while the cases for the 'Rhine' collection seem to come from a female dominated part of the society. Since most cases are contributed by the percipients, this difference must have influenced the proportion of female percipients in the direction observed. Another reporting effect absent in the 'Phantasms' collection but quite significant in the 'Rhine' collection indicates, as discussed before, that male percipients have been more reluctant to contribute cases than female percipients. The difference is quite noticeable. For cases reported by the percipients the ratio between male and female percipients is 1:5.6, but for cases contributed by others the same ratio is 1:2.3. The two reporting effects described above seem sufficiently strong to largely close the gap between the difference in proportions of female percipients between the two collections.

3.2 In the 'Phantasms' collection the son-parent relationship clearly had a different status. Despite the on-average higher number of female percipients sons more than daughters appear to have a spontaneous experience related to one of the parents. In a similar vein, in the spontaneous experiences of the parents the son was more often the target person than one would expect based on the average number of male target persons. In the 'Rhine' collection both phenomena have disappeared. I interpret this change as another cultural difference e.g. the difference within the family in position between sons and daughters has largely disappeared. Considering the different types of society, and especially the difference in general welfare between the

two periods, I feel that from a social psychological point of view independent arguments can be found which support this interpretation. If so, this discrepancy between the two collections supports the previous study's hypothesis that the probability of becoming either percipient or target person depends strongly on the nature of the emotional relationship between the persons involved.

3.3 Finally two borderline cases as far as confirmation is concerned were met. For telepathy cases, the difference in proportion of correctly identified events was slightly higher than the corresponding value for the previous collection. The other analysis concerned the tendency, observed in both collections, for males to be slightly more inclined to take action than females.

CONCLUSIONS

It is important to note that despite the observed differences between the two collections no relationships were observed which are at variance with the findings from the previous study. Also, the differences which were observed show in most instances an understandable relationship with each other. Therefore, the failures to replicate can be summarized as follows:

1. There is a strong difference between the two collections regarding types of experiences. In the 'Phantasms' collection waking experiences dominate; in the 'Rhine' collection dreams and intuitions are more frequent.
2. In the 'Rhine' collection fewer death occurrences but more slight and serious accidents are reported.
3. The proportion of female percipients is higher in the 'Rhine' collection. It has been demonstrated that two reporting effects contributed to this phenomenon.
4. The special status of the son-parent relationship observed in the 'Phantasms' data is not found in the present collection.

INTERACTIONS

Probably due to the much larger number of cases on which the present study was based, some relationships which showed a non-significant trend in the 'Phantasms' data became significant in this analysis. Consequently, the number of significant interactions increased. This is especially true for the relationship between distance and a number of other variables. The interactions observed are in many respects similar to those, described in the corresponding section on interactions of the 'Sannwald' study (see Schouten, 1981, figure 1, p.42). In fact the very same pattern emerges (see figure 1 of this study). The main differences are that group D is enlarged with the variable 'distance' and that in addition to the relationship between type of experience and number of details another link between the groups C and D is created by the significant relationship between seriousness of event and taking action.

A: In this group of variables it is not the interactions between them but rather the lack of interactions which create its importance. Despite the large differences in frequencies of male and female percipients and male and female target persons these two variables appear not to be related to most other variables. Moreover, the few relationships observed are, because of the weak strength of their association and because of their nature, totally unsuitable to provide an explanation for the discrepancies between number of males and females in the two roles. Sex of percipient is related to the percipient being indoors or outdoors, and a weak trend was observed to the effect that male percipients are more inclined to take action. Sex of target person appears to be weakly related to seriousness of event, males are somewhat more frequently involved in serious accidents and death cases. This lack of interactions, also observed in the two previous studies, is of importance since it proves that the explanation for the differences between males and females as percipients and target persons must be looked for elsewhere.

B: Serious accidents are strongly related to the near family, while death events are proportionally more frequent in the category 'other family'. Cases involving large distances are more frequent between members of the (near and other) family compared to friends and acquaintances. Also cases involving large distances are more frequently related to death and serious accidents.

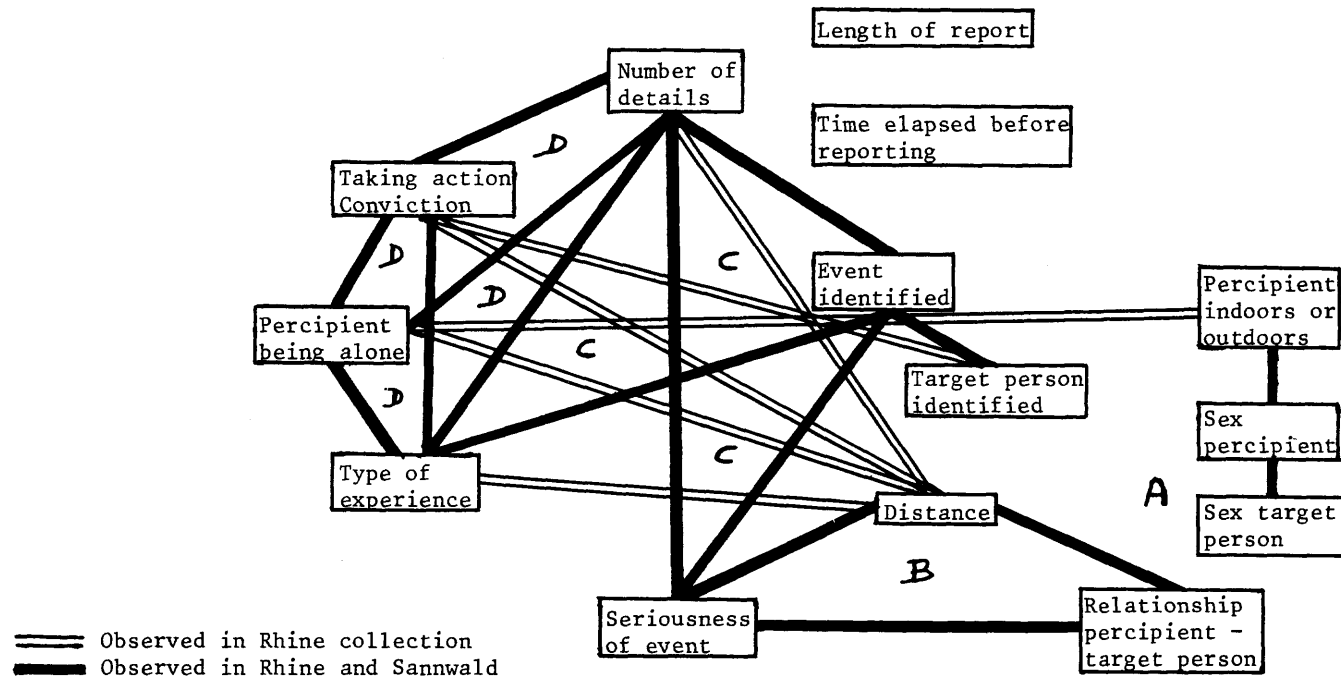


FIGURE 1
Relationships between variables

C: There appears a strong relationship between type of experience and number of details. In general dreams contain more details than intuition and waking experiences. Events are more frequently correctly identified via dreams, and number of details is positively related with correct identification of the event. On the average a higher number of details is associated with non-serious events. It is rather striking that in both replication studies, despite the strong interactions in group C no relationship is observed between seriousness of event and type of experience.

D: In general dreams contain more details than intuitions and waking experiences but carry less conviction. This is the probable reason why dreams are least often associated with action taken. Action is most often triggered by intuitive experiences. However, although there is a trend that less action is taken in the case of experiences with 2 or more details the relationship between number of details and taking action just fails to reach significance ($\chi^2=8.8$, $df=2$, $p=.012$). Compared to waking experiences, intuitions happen more often when the percipient is in the company of others. Also, experiences happening when in the company of other persons are more strongly related to conviction and taking action. There is a weak relationship indicating that the experience involves fewer details when the percipient had the experience while having company.

Cases involving large distances are more frequently based on dream or waking experiences and, compared to intuitions, happen more frequently when the percipient is alone. Distance is not related to conviction but there is a negative relationship between distance and taking action. This relationship is probably due to the fact that because of practical reasons large distances limit the possibility for taking action. Cases involving large distances are associated with less details.

FURTHER ANALYSES

Precognition cases in which a target person is involved

Both the 'Sannwald' study and the present analysis were carried out to explore to what extent cultural factors influenced the findings of the 'Phantasms' study. The 'Phantasms' collection contained only

telepathy cases. It was decided in advance that the replication studies would be based on both telepathy and precognition cases, provided that the target person was someone other than the percipient. Furthermore, it was decided that only when a difference among the collections occurred would the role of the inclusion of precognition cases be investigated. In the 'Sannwald' study, partly due to the relatively small number of cases, in none of such analyses did there appear a difference between characteristics of telepathy and precognition cases. However, the present study did yield some differences between telepathy and precognition cases. Also, as will be discussed in the following section, between precognition cases in which a target person is involved and precognition cases which concern only the percipient differences appear.

The main differences between telepathy and precognition cases (in which a target person is involved) concern the distribution of types of experiences and the distribution of types of events. No difference was observed in distribution of sex of percipient, in the reluctance of male percipients to report cases, and in the distributions of sex of target person. The reporting effect apparent in the telepathy data to the effect that males are more often involved in serious accidents than females is absent in the precognition data. For precognition cases the relationship between sex of target person and seriousness of event is clearly non-significant ($\chi^2=1.6$, $df=3$). An equally non-significant difference appears when investigating the distribution of cases (telepathy versus precognition) over the types of relationships ($\chi^2=2.6$, $df=2$, strangers excluded).

A strong difference emerges when comparing the distribution of types of experiences. The data are presented in table 11.

As L.E. Rhine already observed (Rhine, 1954) precognitive experiences more frequently involve dreams than do telepathic experiences. Another difference observed concerning the distribution of types of experiences is the relationship between type of experience and seriousness of event. In the 'Phantasms' collection a significant relationship was observed. For telepathy cases of the 'Rhine' collection this relationship is not significant, but for precognition cases it again reaches a significant level ($\chi^2=11.9$, $df=2$, $p=.003$). However, there is a difference, although not a conflicting one, between the nature of the relationship in the two sets of data. In the 'Phantasms' collection the significant relationship between type of experience and seriousness of event can be attributed mainly

TABLE 11

Distribution of types of experiences for telepathy and precognition cases in which a target person is involved

	telepathy		precognition	
intuitions	320	36.8%	95	26.0%
dreams	288	33.1%	228	62.3%
waking experiences	221	25.4%	37	10.1%
others	41	4.7%	6	1.6%

to the strong association between waking experiences and death. In the precognition cases of the present study the relationship can be attributed to the strong association between dreams and non-serious events.

Another major difference between telepathy and precognition cases appears when comparing the distribution of types of events. The data are presented in table 12.

It becomes clear from the data of table 12 that compared to telepathy, precognitive experiences are related more often to the death of the target person and less often to trivial and positive events. This finding is of interest considering the relationship observed between seriousness of event and type of experience. It has been shown before that in precognition cases dreams are significantly related to non-serious events. Nevertheless, despite the fact that the precognitive experience more often becomes conscious via dreams, precognition cases involve fewer trivial and positive events.

Not only does the distribution of events differ, but also the identification of these events. In precognition cases the event was correctly identified in 78.7% of the cases, a value which differs significantly from the 59.5% observed for telepathy cases ($\chi^2=41.0$, $df=1$, $p<<.01$). This finding is probably related to

TABLE 12
Distribution of events for telepathy and precognition
cases in which a target person is involved

	telepathy		precognition	
death	283	32.9%	180	49.0%
serious accidents	237	27.6%	101	27.5%
slight injuries	192	22.3%	35	9.5%
material	8	0.9%	3	0.8%
trivial	109	12.7%	24	6.5%
positive	31	3.6%	24	6.5%
chi-square=54.3, df=4, $p < .01$ (material events excluded)				

the previously observed phenomenon that dreams yield the highest proportion of correctly identified events. The same applies to the finding that dreams contain the highest proportion of details. Because precognition cases are strongly related to dream experiences, the distribution of number of details also differs strongly from the corresponding distribution for telepathy cases (see table 13).

When the distribution of cases over the various intervals of time between experience and event is studied, a rather skewed distribution is observed. It appears that in 33% of the cases the time interval is less than 24 hours, in 67% of the cases less than 2 weeks, and in 95% of the cases less than 1 year. Only 5% cases were observed in which the time between experience and event exceeded 1 year. Although no significant relationship appeared between type of experience and time between experience and event, it should be noted that nearly all cases in the latter category involved dream experiences.

TABLE 13
Distribution of number of details over cases for telepathy
and precognition in which a target person is involved

	telepathy		precognition	
0 details	451	51.8%	87	23.6%
1 detail	214	24.6%	96	26.1%
2 or more details	206	23.6%	185	50.3%
	chi-square=105.5, df=2, $p < .01$			

Cases in which the experience was related to the percipient

Because no difference was observed between male and female percipients in relation to any variable investigated (for instance type of experience, number of details, of correctly identifying the target person and event) it can be assumed that the difference in proportion of male and female percipients does not reflect a difference in ability to have spontaneous paranormal experiences. In the 'Phantasms' study, I have argued that this difference in proportion of male and female percipients can be explained by assuming that the roles of percipient and target person depends mainly on the emotional relationship between the two persons. If that assumption is correct, it follows that when no relationship is involved, e.g. in cases in which the experience is related to the percipient himself, the distribution of male and female percipients will change significantly towards a more equal probability distribution. In the 'Sannwald' study this prediction was confirmed by the data. In cases not involving a target person significantly more male percipients were observed. The same holds true for the data of the 'Rhine' collection. The proportion of male percipients in cases concerning the percipient himself is 33.2% compared to 16.7% for cases involving a target person. The difference between the distributions of male and female percipients for the two types of cases is strongly significant

(chi-square=42.6, df=1, $p < .01$) and in the predicted direction. In the 'Sannwald' collection it turned out that for precognition cases related to the percipient the difference between number of male and female percipients even decreased to a non-significant level. This is not so in the 'Rhine' collection, the value of 33.2% is too extreme for that. However, such was not to be expected because it was already demonstrated that two reporting effects have increased the number of female percipients for all cases in the present collection.

The distribution of types of experiences differs significantly from the corresponding distribution for all cases involving a target person, but is nearly equal to the distribution observed for precognition cases in which a target person was involved. Thus it can be concluded that as far as the distribution of types of experiences is concerned, the difference is created by the variable 'type of ESP', and not by the presence or absence of a target person. The latter does not apply to the difference in distribution of events. As previously described for cases in which a target person is involved there is a strong difference in types of events between precognition and telepathy. Precognition cases are more frequently related to death and less frequently to trivial and positive events. However, there is an even more extreme difference in distribution of events between precognition cases with and without a target person. The data are presented in table 14.

From the data presented in table 14 it becomes apparent that when the experience concerns the percipient himself serious events (death and serious accidents) are much less frequent, whereas non-serious events (slight accidents, trivial, and positive) are much more frequent. The same can be said when comparing precognition cases concerning the percipient with telepathy cases. Despite these large differences it is of interest to note that even when the event concerns the percipient himself, the proportion of purely material damage is still rather low. In fact, only 10 cases (3.1%) were observed in which serious material damage to the percipient was the subject of the experience.

As mentioned above these cases involve a higher frequency of non-serious events. Also no difference was observed between the two types of precognition cases as regards types of experiences. Thus not surprisingly in precognition cases concerning the percipient the average number of details is significantly higher (chi-square=52.8, df=2, $p < .01$), and extremely higher when compared with the telepathy cases (chi-square=293.8, df=2).

TABLE 14
Distribution of events in the different types
of precognition cases

		precognition cases			
		target person		no target person	
	death	180	49.0%	9	2.8%
serious	accidents	101	27.5%	51	15.8%
slight	accidents	35	9.5%	66	20.5%
	material	3	.8%	42	13.0%
	trivial	24	6.5%	120	37.3%
	positive	24	6.5%	34	10.6%
chi-square=278.5, df=5, $p < .01$					

The distribution of cases over the four intervals (<24 hours, <2 weeks, <1 year and >1 year) of time elapsed between experience and event is just as it was in the precognition cases with a target person very skewed with the high frequencies in the lower intervals. Of these precognition cases, 47% are observed in the 24 hour interval, 66% in the 2 week interval, 86% in the 1 year interval and consequently only 14% of the cases involved intervals longer than 1 year. However, a significant difference is observed when these distributions of time between experience and event are compared for the two types of precognition (chi-square=42.3, df=3, $p < .01$). For precognitive experiences related to the percipient there is a substantial higher proportion of cases in the first and last interval (<24 hours and >1 year). Contrary to what was observed in precognition cases involving a target person, cases related to the percipient yield a significant relationship between type of experience and time between experience and event (chi-square=58.0, df=6, $p < .01$). Almost all the cases involving long time intervals are dream experiences.

ABSTRACT

In a previous study a quantitative analysis of the 19th century's 'Phantasms of the Living' collection of spontaneous cases yielded various findings of interest. Strongly significant relationships between variables were observed demonstrating that although hundreds of different people unrelated to each other are involved, spontaneous paranormal experiences show common trends and can not be considered as a mere random collection of stories. It was also demonstrated that familiar phenomena, for example the higher proportion of cases between near relatives, can not be explained as reporting effects. Furthermore, cases supported by testimonies as regards the veracity of the experience and the event did not differ in characteristics from the non-witnessed cases.

Two replication studies have been carried out to investigate the extent these findings depend on cultural influences. The first replication study, based on a collection of German spontaneous cases from the first half of this century (the 'Sannwald' collection), confirmed most of the findings. Nearly all failures to replicate could be considered a consequence of two main differences between the two collections: a difference in distribution of types of experiences and a difference in distribution of types of events. In addition indications were found that the proportions of male and female percipients observed are influenced by a reporting effect related to the part of the population, male or female dominated, from which the cases were solicited.

The present investigation of the 'Rhine' collection has also been carried out to replicate the findings of the 'Phantasms' study and involves a similar quantitative analysis. This time, however, the collection was derived in the U.S.A. during the 1940's, 1950's and 1960's. It appeared that by the most conservative estimate of the 57 results discussed in this study 19 did not confirm the corresponding results of the original study. The confirmative strength of this study is actually better than these figures indicate. There was no instance, with the exception of the distribution of types of experiences, where these failures to replicate showed conflicting trends or patterns between the two collections. The main discrepancies between the two collections could be attributed to differences in the distributions of types of experiences and of types of events. A rather high proportion of female percipients was observed, but it could be demonstrated that

two reporting effects must have contributed to this phenomenon. Male percipients were more reluctant to send in cases than females and the cases were mainly acquired from a female dominated part of the society.

All analyses were based on telepathy and precognition cases in which a target person was involved. Contrary to the finding of the first replication study a few differences were observed between telepathy and precognition cases. The main one concerns the fact that precognitive experiences are strongly related to dreams. As was observed in the foregoing replication study, in precognitive experiences related to the percipient himself a significant higher frequency of male percipients is observed and less cases involving death and serious accidents.

ACKNOWLEDGMENT

I am indebted to Dr. L.E. Rhine for making her collection available for this investigation and to Dr. Ramakrishna Rao and the staff of the F.R.N.M. for their support during my stay at the institute when scoring the cases.

REFERENCES

- Gurney, E., Myers, F.H.W., and Podmore, F. 'Phantasms of the living'. London, 1886. Reprinted Scholar's Facsimiles, Gainesville, Florida, 1970.
- Rhine, L.E. 'Conviction and associated conditions in spontaneous cases', J.o.P., 1951, 15, 164-191.
- Rhine, L.E. 'Subjective forms of spontaneous psi experiences', J.o.P., 1953, 17, 77-114. (a)

Rhine, L.E. 'The relation of experience to associated event in spontaneous psi experiences', J.o.P., 1953, 17, 187-209. (b)

Rhine, L.E. 'Frequency of types of experience in spontaneous precognition', J.o.P., 1954, 18, 93-123.

Rhine, L.E. 'Precognition and intervention', J.o.P., 1955, 19, 1-35.

Rhine, L.E. 'The relationship of agent and percipient in spontaneous telepathy', J.o.P., 1956, 20, 1-33. (a)

Rhine, L.E. 'Hallucinatory psi experiences. I. An introductory survey', J.o.P., 1956, 20, 233-257. (b)

Rhine, L.E. 'Hallucinatory psi experiences. II. The initiative of the percipient in hallucinations of the living, the dying, and the dead', J.o.P., 1957, 21, 13-47. (a)

Rhine, L.E. 'Hallucinatory psi experiences. III. The intention of the agent and the dramatizing tendency of the percipient', J.o.P., 1957, 21, 186-227. (b)

Rhine, L.E. 'The evaluation of non-recurrent psi experiences on post-mortem survival', J.o.P., 1960, 24, 8-25.

Rhine, L.E. 'Psychological processes in ESP experiences. Part I. Waking experiences', J.o.P., 1962, 26, 88-111. (a)

Rhine, L.E. 'Psychological processes in ESP experiences. Part II. Dreams', J.o.P., 1962, 26, 172-199. (b)

Rhine, L.E. 'Spontaneous physical effects and the psi process', J.o.P., 1963, 27, 84-122. (a)

Rhine, L.E. 'Auditory psi experience: Hallucinatory or physical?', J.o.P., 1963, 27, 182-198. (b)

Rhine, L.E. 'Factors influencing the range of information in ESP experiences', J.o.P., 1964, 28, 176-213.

Rhine, L.E. 'Hallucinatory experiences and psychosomatic psi', J.o.P., 1967, 31, 111-135.

Rhine, L.E. 'Case study review', J.o.P., 1969, 33, 228-267.

Rhine, L.E. 'Dr. L.E. Rhine's reply to Dr. Stevenson', Letters and Comments, J.o.P., 1970, 34, 149-164.

Rhine, L.E. 'The psi process in spontaneous cases', J.o.P., 1978, 42, 20-33.

Sannwald, G. 'Parapsychischen Erlebnissen und Persönlichkeitsmerkmalen', Dissertation University of Freiburg, 1961.

Schouten, S.A. 'Analysing spontaneous cases'. Research Letter Parapsychology Laboratory, University of Utrecht, 1979, 9, 55-63. (a)

Schouten, S.A. 'Analysis of spontaneous cases as reported in 'Phantasms of the Living'', E.J.P., 1979, 2, 4, 408-454. (b)

Schouten, S.A. 'Analysing spontaneous cases: A replication based on the Sannwald collection', E.J.P., 1981, 4, 1, 9-49.

PSI EXPECTANCY EFFECTS IN PSYCHIC HEALING STUDIES
WITH MALARIAL MICE

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In a recent review of the parapsychological literature concerning psychic healing that is too extensive to repeat here (Solfvin, in preparation), the author has noted several suggestions that may be helpful in guiding research efforts with this interesting phenomena. First and perhaps most important is that experimental studies in this area have been quite successful in producing significant effects. While this area is wrought with greater problems of control than parapsychological work involving inanimate objects (cards, dice, etc.), the area of psychic healing, or psychokinesis (PK) on biological materials, appears to hold great promise for future studies.

One of the most promising features of this work is the unanticipated finding that there seems not to be a decline of the phenomena with increasing control as has often been assumed (Ehrenwald, 1977). The major outcome studies (Goddard, 1899; Finkler, 1980;1981; Leuret and Bon, 1957; Rose, 1954; Straunch, 1963; West, 1957) involving no controls other than the examination of pre- and post-treatment conditions of patients, show rather meager effects. Although a varying percentage of the patients do benefit, the results are readily

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interpretable in terms of inaccurate diagnoses, suggestion, spontaneous remissions or the effects of more standard treatments that are being simultaneously administered to the patients. More recent random sample surveys (Haraldsson and Olafsson, 1980; Attevelt, 1981) have found that about 70% of the patients report some improvement as a result of psychic healing treatments but these figures are well within the range of placebo effect estimates, particularly when the patients self report is involved (Shapiro, 1979). These observations suggest that patients do benefit from these treatments but they do not suggest parapsychological processes to any great degree.

However, the experimental studies of psychic healing or PK (psychokinesis) on biological materials, which incorporate more controls and more specific and objective measures, are surprisingly impressive in intra- and inter-experimenter reliability of results. Series of studies conducted by Grad (1967), Watkins and associates (Watkins and Watkins, 1971; Watkins, Watkins and Wells, 1973; Wells and Klein, 1972; Wells and Watkins, 1975) and Krieger (1972; 1973; 1975; 1976) showed consistently significant effects on a variety of target conditions in mice and humans by gifted and trained healers. Null (no date) and Heaton (no date) also reported significant results in shorter series. Numerous other studies (Barry, 1968; Nash and Nash, 1967; Nash, 1982; Haraldsson, 1973; Smith, 1972; Edge, 1980; Metta, 1972; Richmond, 1952; Rubile and Rauscher, 1979; Vasse and Vasse, 1950; Onetto and Elguin, 1972; Tedder and Monty, 1981) have shown significant effects of healing or PK treatments on a variety of animal materials, while Deamer (1979) and Kief (1973) have failed to do so. Snel (1980) and Kmetz (1979) have reported significant effects which, in retrospect, might have been due to artifacts in the handling of the cellular materials. Numerous others (see Solfvin, in preparation) have shown significant PK effects on gross motor behaviors (movement) of animal "targets".

The weakest studies are those involving plants. Only Vasse and Vasse (1950), Grad (1962; 1963) and McDonald et al (1976) have reported significant effects on plants while Knowles (1954), Lennep (1979), Pauli (1973), Hickman (1979) and Harari (1974) have reported no significant effects.

Although there are problems with many of these studies (discussed in the review) that prevent us from drawing strong conclusions concerning these effects on biological materials, it is clear that this area of investigation warrants far more attention than it has recently been

given. Perhaps the "Levy affair" has made us hesitant to explore any kind of studies with animals or biological materials. If this is so, it is indeed a shame, for it appears that we are retarding our own potential growth as a science by ignoring this potentially rich area of study.

Another point that is clear from my review is that the experimental studies, by and large, lack generalizability to real life situations. While this is a criticism that can be equally levelled at parapsychological research generally, it is particularly unfortunate with respect to studies of the possible effects of psychic healing treatments which have so much potential for practical application in society at large. Here is an area where parapsychologists, whose work is often adrift from real-life concerns, have the opportunity to demonstrate the value of our work to society. With a few exceptions, the existing literature contains little of value outside of our own journals.

Tunnell (1977) has pointed out that generalizability can be easily achieved in laboratory studies by attending to the naturalness of the setting, the treatment and the response aspects of the study. The bulk of the research with healers scores low on naturalness of setting and response, both of which are usually quite clearly artificial. Even the treatment cannot be considered truly naturalistic when the other two aspects are not. A further problem in healing studies is that the treatment is identified with the healer and very few healers (treatments) have come under test. The significant effects that are found with colonel Estabany, for example, tell us little about healing treatments generally.

Indeed, it is not psychic healing that is being studied but laboratory PK on biological materials, although the authors often assume incorrectly that the latter have direct relevance to the former. To study psychic healing we must construct naturalistic experiments in the sense that Tunnell has outlined, but we must also involve large samples of practicing healers or, better still, non-gifted subjects working in natural settings. Only in this way can our studies reflect on real-life settings.

A further aspect of non-generalizability is the degree to which the research in related, non-parapsychological areas is ignored in our studies. There are extensive and readily available literatures in medicine, pharmacology, psychiatry and psychology that could help us

to avoid the problems that were encountered by others in testing treatments decades ago. Even if we are only interested in PK and involve the biological material as simply a target object, we cannot escape the necessity of understanding the potential and often well-known artifactual influences that the living systems are subject to. If we wish to study psychic healing, we must keep in mind that in real-life situations there are usually several processes operating in concert with the actual treatment, such as the expectations of the healer and patient, the communications between them, the physical handling or touching that may occur, and the natural variability and self repairing aspects of the patient and the disease being treated. These topics are dealt with in non-parapsychological treatment literatures under such headings as placebo responses, gentling effects, iatrogenetic processes, spontaneous remissions and non-specific treatment effects. In the parapsychological literature these "other" factors are either systematically eliminated or simply ignored. If we are to gain an understanding of psychic healing in real-life situations we must, first of all, familiarize ourselves with what is known of these various influences in treatment situations and, secondly, systematically study their relationships with psychic healing treatments. It may turn out, as Knowles (1954; 1956) has suggested, that some of these non-parapsychological factors may complement or assist the psychic healing process, or may themselves have parapsychological components. By ignoring these relevant literatures we are not only depriving ourselves of a valuable resource but we are isolating ourselves from science at large. In reviewing the literature of psychic healing effects, one gets the distinct impression of this isolation.

There is another conclusion that is relevant to the current study that can be drawn from the parapsychological literature which is reinforced by the other literatures mentioned above. This concerns a logical and methodological issue that has been discussed in parapsychological circles for some years under the heading of experimenter effects, or psi experimenter effects. Its full impact for psychic healing studies, however, has not been fully understood. The inescapable conclusion that obtains for psychic healing research is that we cannot, with any degree of certainty, attribute the outcome of a successful psychic healing study to the treatment per se. Similar limitations on our ability to isolate a single "cause" for an observed "treatment" effect have been noted in psychiatry (Rosenthal and Frank, 1956), Pharmacology (Uhlenhuth et al, 1966), medicine (Benson and McCallie, 1979) and experimental psychology (Rosenthal, 1969). These

have in common the fact that the expectations of various persons involved in the assessment process are not independent of the results of the study.

In psychic healing studies, the problem is even worse than with the more usual forms of treatment. We are inferring some invisible process as the cause of the result. This unseen effector could as likely originate in the experimenter or in some personal quality of the healer as in the treatment per se. In fact, Stanford (1974) has argued persuasively that naturalistic psi effects are LIKELIER to come from a source other than the actual mental effort, ritual or procedure that is applied to the target materials. It is also possible that the outcomes of these studies is a product of some combination of several elements or persons in the situation. For psychic healing research this means that we are currently incapable of establishing a causal link between the healing treatment and the observed outcome.

This may not be so disastrous for our research efforts as it at first might seem. It may be just the push we need to turn our attentions away from the question of "who's doing it?" towards the research variables that we can productively investigate. If we fully assimilate the fact that it is beyond our current means to point a causal finger at a specific aspect (treatment, healer) of the psychic healing situation, we can begin to address some of the important questions regarding the situations and combinations of factors upon which successful healing treatments may depend. The current experimental series is an attempt to do just this.

Based upon the above considerations, it was decided to conduct an experimental series to test the hypothesis that the expectancy structure in a controlled laboratory test of psychic healing, might alone be sufficient to produce a significant (parapsychological) healing effect similar to those observed in studies where there is an actual healer. Controls must include multiple blinds that leave no room for sensory leakage, randomization of all variable factors, and a precisely defined and objective illness criterion.

In addition to its relevance to other research projects, the study is to be naturalistic for generalizability to real-life situations as well. In addition to the observance of Tunnell's (1977) three criteria, other considerations enter here. In order to generalize to the healer-patient interaction in the broadest sense, persons who are pursuing careers in the health care field (veterinary students) but

who claim no special abilities as psychic healers, were chosen for this study. Their task is to make regular visits to their "patients" (sick mice) and make careful observations of them, much the same as they would do in a veterinary hospital situation.

The illness model is the development of the malarial blood parasite (*babesia rhodaini*) in white mice, a well-known and naturally occurring condition in rodents. This particular illness model has been extensively investigated at the laboratory where this experiment is conducted. There is a standard objective measurement procedure available for determining the progress of the parasites in the host organisms.

The only manipulation in this series is of the expectations of the student assistants. In one sense, this becomes a study of expectancy effects. In a broader sense, because it is modelled after actual tests of psychic healing, it is a study of a possible contributing factor to the effects observed in such studies. With no healing treatment involved, but only the expectation that there is one, we can be reasonably certain that whatever results obtain will not be due to a healing treatment. In conformance with Stanford's (1974) model of spontaneous psi events, the students will not be consciously aware that they are anything more than assistants in this study, and they will be led to expect that certain treatments have been or are being administered by unseen agencies that have the means to accomplish the goal. Thus, significant results in this series would suggest that the expectancies held by an experimenter or assistant in an actual test of a psychic healing treatment could at least contribute to a successful outcome.

EXPERIMENT 1

SUBJECTS

The real subjects of this experiment are three veterinary students from the University of Utrecht. The author was invited to give a talk to the veterinary students society on "ESP in Animals" during November, 1980, just when this series of experiments was being planned. During the talk, a sheep-goat questionnaire (Appendix A) was distributed to the audience on which volunteers were requested to

assist me in some psychic healing experiments that I was conducting with laboratory mice. From the 14 students who volunteered I selected the two most positive sheep and two most negative goats (all yeses or all no's on the questionnaire) to assist me in this first experiment. All agreed to participate. However, through a miscommunication, one of the students (a sheep) went to the wrong building and we began the experiment with only the other three, the fourth group of mice being used as an unplanned and unhandled control.

Experimental Animals

The Animals were 48 female Balb-c white mice that were bred by the university for experimental purposes. They were born on the same day (14 days prior to the start of the experiment) and ranged in weight from 13.9 to 18.7 grams with a mean weight of 16.9 grams.

DESIGN

The manipulations in the experiment were of the expectations of the student assistants. All the mice were inoculated intraperitoneally with 0.1 milliliter of 1:10 stablate (*Babesia rodhaini*), the rodent version of the malarial blood parasite. Each student was assigned 12 mice housed in a single cage and was told that half of them would be inoculated with *Babesia* while the other half would receive a sterile injection. The students themselves randomized and marked their mice with either yellow (*Babesia*) or black (non-*Babesia*) markings to indicate which were which, thus this condition remained non-blind to them. In addition, the students were led to believe that half of each of these two groups would be receiving distant healing from a psychic healer, when in fact there was no such healer in the study. These expectations formed a balanced two-way analysis of variance that was fully randomized.

The students were not aware of which mice were supposed to be healed and which not, these being randomly assigned to the healing and control conditions by the author.

ILLNESS EXPECTANCY		
	Babesia	Non-babesia

	*	*
Heal	*	*
	*	*
	*	*

	*	*
Non-	*	*
Heal	*	*
	*	*

PROCEDURE

The student assistants met with the experimenter (E) on day 1 of the experiment at the veterinary pathology laboratory in Utrecht. A general instruction sheet was distributed for the students to read (see Appendix B) and after they have read it, it is read aloud to them by E to reduce the possibility of misunderstandings due to the language differences. The students are all native Dutch speaking and speak English as a second language.

Each student is then given a cage, a cage label and a notebook containing:

- 1.) a randomization instruction sheet;
- 2.) a pack of randomization envelopes;
- 3.) a set of weight recording sheets;
- 4.) an instruction sheet for reweighing;
- 5.) a large empty envelope and a pen; and,
- 6.) several blank pages for notes.

In addition, the work table in the center of the room was equipped with the 48 mice for this study, an electronic weighing scale (reading in milligrams), a polaroid camera and film, and black and yellow markers for marking the mice.

Each student arbitrarily picks 12 mice, one at a time, from the pool of 48 and puts them into his cage. The remaining 12 were designated as the control (since the fourth student didn't show up). E then demonstrated the procedure for randomizing, photographing, weighing and marking each mouse that is outlined on the instruction sheet (Appendix C). This procedure involves arbitrarily picking a mouse out of the cage, placing it on the scale and photographing it with the Kodak Instant Camera. The first (top) randomization envelope is opened while the photo is developing and the sticky label with the coding instructions is attached to the back of the photo. On this label is written a color (yellow or black) and a number (1 through 6) by which the mouse will be identified. The mouse is then weighed and the weight is recorded on the line on the weight record sheet corresponding to the code on the label (eg., black-5). The mouse is then marked with the appropriately colored marker with markings that will signify its number and returned to its cage.

The photo with the label is inserted into the empty randomization envelope and a mark is made on the outside of the envelope with the same color marker that was used to mark the mouse. This procedure is repeated until all the mice have been marked. The completed weight record sheet and the photos (in their envelopes) are inserted into the large envelope and left in the cabinet in the corner of the room for E to pick up later. The cage top is put back on the cage, the mice are checked, the food and water are checked, and the cage is returned to the specifically designated position on the storage shelf. All the other materials are put away in the cabinet in the corner and the student leaves the building.

When the students are certain that they understand the procedure, E leaves the building so that the randomizing will be carried out in his absence. The following morning, E returns to the building to retrieve the envelopes with the photos and weights and returns to his office (several miles away). This is the last visit that E makes to the experimental building for the remainder of the experiment. During the afternoon of this day (day 2 of the experiment) the mice are inoculated with babesia by laboratory staff members who are blind to

the nature of the experiment.

The day after the inoculations (day 3 of the experiment) the students return to the building where the mice are housed and reweigh them. An instruction sheet for this procedure is in their notebooks. They remove the cage from the shelf and place it next to the scale on the table. They are required to weigh the mice in a specific order (yellow-1, black-1, yellow-2, etc.) to eliminate the possible effects of selection. They have also been instructed to handle the mice gently and uniformly, and for each mouse they record observations of relevant behaviors such as activity level, biting, etc. The visit lasts about 45 minutes. Three days later, day 6 of the experiment, they come again to weigh the mice, and two days later (day 8) they make their final visit. After each weighing visit, the student removes the completed weight record sheet from his notebook and deposits it into a special storage container in the cabinet. This prevents the weights he has just recorded from being in his view when he makes his next visit.

Two days after the students' final visits, two members of the laboratory staff who are not otherwise involved with the experiment take a blood sample from the tail of each mouse and put it directly onto a microscopic slide along with the identification of the mouse. These blood samples are analysed for the percentage of red blood cells which have been destroyed by the babesia parasites. All the mice are then given a drug cure which arrests the disease and the mice are officially returned to the veterinary pathology group for their continuing studies of the immunization of mice against babesia. During the following week, all of the materials (notebooks, etc.) are picked up from the laboratory by E.

From the time of the inoculations until the taking of the blood samples, the mice are handled only by the students. The four cages are assigned to a special place on the shelf and are labelled so that other staff members will not touch them. In addition, the students are instructed not to disturb another student's cage and the weighing times are arranged so that only one student is in the "mouse room" at any given time (except for the initial randomization which they do simultaneously). The students are asked not to discuss the experiment with anyone during the experiment and particularly not with the other students involved in it.

During the following week, each of the students came to E's office for a post-experimental interview. The student was given another

sheep-goat questionnaire to complete and was verbally asked a few questions concerning the events during his handling of the mice. The weight record sheets were checked at this time to assure they were legible and the student was thanked for his help and told he would receive a copy of the results when this series was completed. The interviews were conducted before E had seen the blood count results.

RANDOMIZATIONS

The first randomization was to assign the mice to specific colors and numbers within their cages. For each student a set of 12 address labels with sticky backs was prepared with one of the twelve possible combinations of the two colors (yellow, black) and six numbers (1-6) written on each. These were sealed in 12 identical envelopes, shuffled well, clipped together and inserted into the notebook for the students to use during the randomizing procedure.

The second randomization was to assign the photos of the mice to the supposed "healing" and "non-healing" groups so that exactly half of the black and yellow marked mice from each cage would be assigned to each condition. When the photos of the mice were collected from the laboratory (day 3 of the experiment), E returned to his office and conducted the following procedure. For each student, the large envelope was opened and the photos, still in their separate envelopes, were placed in two piles according to the color marked on the outside of the envelopes. Each photo was inserted into a new envelope and sealed and each pile was shuffled ten or more times. Another person was brought into the room and, after E left, reshuffled the piles and dealt out two groups of 3 from each pile of 6. E returned to the room, placed the subpiles on the left side into a large envelope and labeled this "H" (healed) and the subpiles on the right side into an envelope labelled "NH" (non-healed). These envelopes were sealed and placed in a drawer in E's desk which remained locked until the results of the experiment were all in E's possession.

ANALYSIS

The criterion variable for this experiment is the percent of destroyed red blood cells in the blood samples. For each mouse, a drop

of fresh blood is smeared uniformly onto a glass microscope slide. After drying, the slides are treated with a tint to make the red blood cells stand out. This was done by two members of the veterinary pathology group staff who were not otherwise involved with the experiment. One of these two staff members "read" the slides in the following manner. A slide is put into a microscope with a special ocular to simplify the counting procedure. The ocular delineates the microscopic viewing field into two concentric circles, the outer one enclosing exactly ten times the area as the inner one. First, all the destroyed red blood cells in the outer circle are counted. Then the total number of red blood cells in the inner circle are counted (destroyed or not). This last number is multiplied by ten to approximate the number in the outer circle. This procedure is repeated three times for each slide, each time focussing on a different section of the blood smear. A percent is calculated from these averages.

The statistical analysis consists of a two-way analysis of variance with two levels of illness expectancy (I) and 2 levels of healing expectancy (H). This analysis is repeated with the initial weights of the mice added as a covariate.

A secondary analysis is planned for the sheep-goat breakdown. A sheep-goat score is derived from the belief questionnaire responses (Appendix A) by assigning one (1) point for each "yes" and one-half point for each "not sure" (or similar) response. Sheep are defined by a score of 3 or more while goats are those who score one or less. A 3-way ANOVA is planned to evaluate the sheep-goat variable including the two factors described above with the sheep-goat breakdown of handlers as the third factor.

HYPOTHESES

It was hypothesized that there would be a significant main effect for the healing (H) factor. If, additionally, the illness expectancy (I) factor shows a significant main effect, or there is a significant HxI interaction, this would support the contention that the expectations of the handlers contributed to the effect. On the other hand, a significant H effect but not I effect, with no HxI interactions, would not support the hypothesis that the expectancies were effective here. An I main effect in the absence of an H effect and with no significant interactions with H, would suggest the

operation of a sensory expectancy effect, a Rosenthal effect. If there are no significant main effects or interactions we would conclude that the expectancy treatment in this experiment did not show an effect on the babesia levels of the mice.

For the sheep-goat breakdown it is hypothesized that there will be a significant interaction between this factor and the healing expectancy factor (H).

RESULTS

When the results of the babesia analysis for the 48 mice were completed a cursory glance at them brought their normality into suspect. A preliminary analysis was therefore devised using nonparametric statistical procedures to circumvent any distributional problems. Using these procedures the results showed main effects for both the healing and illness expectancy factors beyond the $p=05$ level. Later, it was discovered that the techniques that were used to approximate the analysis of variance may have introduced dependencies between the scores of different groups and could have artificially altered the results. Moreover, there are inherent disadvantages to presenting "home-made" analyses that make it difficult for other researchers to adequately assess the results, so it was finally decided to return to the original analysis of variance that was planned for this data and which is customary with studies of this type. The first step was to have a closer look at the distribution of scores.

Closer inspection showed that the excess skew in the distribution was the result of the scores of one of the three handlers. A one-way ANOVA of the three handlers and control groups showed that they did not all come from the same population ($F=6.01$, 3 and 41 d.f., $p<.01$). This is due to handler J, whose mice showed significantly higher babesia levels than the other two handlers and control combined ($t=3.22$, 13.7 df, $p<.01$). When his scores are removed, the remaining two handlers's mice do not differ significantly from the control ($t<1$) nor from each other ($t<1$). The remaining two handlers' scores are within the limits of normality for conducting the ANOVA, as is handler J's mice alone. It was therefore decided to analyze handler J's group separately from the other two handlers using the planned analyses. Table 1 shows the results for handler J and Table 2 shows the results

for handlers M and T.

TABLE 1a
Analysis of variance table for Handler J
Babesia count by illness expectancy (ill, not ill)
by healing expectancy (healed, nonhealed)

SOURCE	SS	DF	MS	F	P
Illness expectancy	20.646	1	20.646	8.193	.021
Healing expectancy	9.258	1	9.258	3.674	.092
Illness x Healing	.030	1	.030	.012	.916
Residual	20.160	8	2.520	---	---
Total	50.094	11	4.554	---	---

In the tables we see that there are no significant effects for handlers M and T, but for handler J there is a significant main effect for illness expectancy and a suggestive trend for the healing expectancy factor. Both of these are in the direction opposite to the induced expectancies----that is, the supposedly ill mice had less babesia than the others and the supposedly healed mice had higher babesia levels than the nonhealed. The average babesia counts (in percent) are tabulated in 2x2 tables below to illustrate these effects.

The sheep-goat breakdown could not be statistically evaluated because of the distributional problems. Handlers M and T are sheep ("believers") and handler J is a goat.

TABLE 1b
ANOVA for handler J
covarying initial weights of mice

SOURCE	SS	DF	MS	F	P
Initial weights	.115	1	.115	.041	.846
Illness expectancy	21.053	1	21.053	7.490	.029
Healing expectancy	9.334	1	9.334	3.320	.111
Illness x Healing	.026	1	.026	.009	.926
Residual	19.667	7	2.811	---	---
Total	50.094	11	4.554	---	---

DISCUSSION

The only significant results in this experiment were contributed by the group handled by J. The significant illness expectancy effect suggests that the expectancy treatment had an effect on this student which resulted in differential babesia infestation in the mice which he handled. Although we cannot exclude the possibility that this occurred parapsychologically, it can be most parsimoniously explained as a social psychological (Rosenthal) effect mediated by the physical handling of the mice. This is further supported by the fact that handler J's mice showed generally higher babesia levels (significantly so) than those of the other two handlers and the unhandled control group.

A number of studies have demonstrated the deleterious effects of stressful handling on laboratory strains of rats (eg., Weininger, 1953; 1954). In the current experiment the illness model employed may be particularly susceptible to such stressful handling effects. The

TABLE 2a
 Analysis of variance table for handlers M and T
 Babesia count by illness expectancy (ill, not ill) and
 by healing expectancy (healed, nonhealed)

SOURCE	SS	DF	MS	F	P
Illness expectancy	.008	1	.008	.007	.935
Healing expectancy	1.166	1	1.166	1.029	.322
Illness x Healing	.393	1	.393	.347	.563
Residual	22.652	20	1.133	---	---
Total	24.219	23	1.053	---	---

blood parasite babesia rhodaini is a very effective agent. It spreads rapidly through the bloodstreams of all the mice and, if left untreated, will inevitably result in their deaths. The natural mechanisms in the body of the mouse are stretched to their limits and still are wanting. Any additional stresses on the mouse, such as being handled by humans, can only tax the system further and will result in the more rapid spreading of the parasite. In the current experiment, for example, we see that the unhandled control group showed lower babesia levels than any of the handled groups, a mean of 1.85% as compared to 4.62%, 2.08% and 2.55% for the handled groups. Apparently the best thing that one can do for the mice is to leave them alone.

In an experiment assessing the effects of the handlers expectancies, this creates a sort of double bind. To the extent that physical handling of the animals mediates the observed differences in parasitaemia, these differences are brought about by the worsening of one group relative to another. In this experiment, for example, it would seem natural for the students to handle the (supposedly) sick mice more gently than the supposedly normal ones. But, by the logic above, this would produce a difference between the groups that is

TABLE 2b
ANOVA for handlers M and T
covarying initial weights of mice

SOURCE	SS	DF	MS	F	P
Initial weights	.697	1	.697	.603	.447
Illness expectancy	.000	1	.000	.000	.997
Healing expectancy	1.313	1	1.313	1.136	.300
Illness x Healing	.240	1	.240	.208	.654
Residual	21.968	19	1.156	---	---
Total	24.219	23	1.053	---	---

TABLE 3
Average babesia counts for each condition by handler.

	Handler J		Handler M		Handler T	
	Ill	normal	Ill	normal	Ill	normal
H	3.9	6.4	2.2	2.7	3.2	2.1
NH	2.0	4.7	1.4	2.0	2.5	2.4

opposite to the induced expectations. Since all the mice are equally infected at the outset, more gentle handling of the supposedly sick ones would make for less stress in this group and therefore they would be better able to combat the parasite and would show lower babesia levels at the end of the experiment. The supposedly normal mice, handled with less TLC, would be more stressed and develop more babesia.

If handling effects were responsible in this way for the illness expectancy effect, then we might wonder whether the marginal healing expectancy effect might have been mediated by handling as well. It is interesting that handler J showed overall higher babesia levels in his mice and a significant illness expectancy differential, both presumably mediated by physical handling, as well as this trend towards a healing expectancy effect. The other two handlers showed relatively low babesia levels in their mice and neither of these two effects.

If handling played a role in the parapsychological effect, it could not have been the simple one depicted by the experiments with "laying-on-of-hands" (Grad, 1965; Smith, 1971; Krieger, 1972). It must have operated in a selective, goal-oriented manner as Stanford (1974) has suggested or as a psi-mediating motor activity as Rush () suggests. However, the role of handling in mediating these effects is beyond the scope of the current experiment and must be left here as a suggestion for future research.

Although the sheep-goat variable could not be statistically assessed in this study, it is interesting that handler J, whose mice showed all of the significant effects, was the only goat among the three handlers with the other two being sheep. This suggests another line of reasoning to fit these data which at first glance seems to be rather appealing. We might assume that handler J was negative or uncomfortable about the experiment or was eager to prevent the results that he was led to expect might occur. He might have used his psi, so to speak, to create a negative healing effect. This point of view, however, leaves open the question of the illness expectancy effect. Also against this viewpoint is the fact that handler J freely volunteered a considerable amount of his time to this project with no remuneration. Further, in his post experimental interview he stated that, while he didn't believe it was possible, he was quite open to seeing evidence to the contrary.

One other thing that came up in handler J's post-experimental interview is relevant here. When I asked him if he had enjoyed participating in this experiment he said yes. His reason was that being involved in this experiment gave him entry into the veterinary pathology research building which was normally closed (locked) to all but the staff members and workers. In retrospect, it is possible that he volunteered for this study in order to gain that access, since he knew when he volunteered where the study was to be conducted. He told me that on each of his three visits after the initial one, he first weighed his mice and then wandered about the building asking the veterinary researchers questions about their own projects. The image that this suggests to me is not one of a disbeliever who is attempting, consciously or otherwise, to negate the experiment, but rather of a person who isn't very interested in the experiment because he has other things on his mind. Such a situation could certainly result in a sloppy job on the handling of the mice. Although we cannot be sure on the basis of this one experiment, I rather lean towards the view that handler J's lack of interest in the experiment coupled with the expectations given to him by E, could have created the differential babesia levels in his group of mice via the stressful handling explanation above. Only further research, of course, can shed light on this issue.

CONCLUSION

The point of the current experiment is to test the hypothesis that expectancies similar to those that might be present in studies of psychic healing and to those that may occur in human health care situations, can have a measurable effect on the progress of organic illness in laboratory animals. To this question, we can answer "yes", qualified by the fact that only one of the three handlers in this study showed such an effect, and that this was in a direction opposite to the expectations given him by the experimenter, and that the handler was aware of which animals were supposed to be which, thus suggesting a sensory-based handling effect. There is also a suggestive trend towards an expectancy effect which cannot be explained entirely by the known sensory communication processes. Since both the illness expectancy effect and the (marginal) healing expectancy effect are opposite to the induced expectancy, and since both occur in the mice handled by one of the three handlers, there is a suggestion here that these two expectancy effects may be related to one another.

Finally, we note that to say that the "expectancy" treatments were partially effective with one handler, does NOT imply that this handler was necessarily the source of these effects. In fact, one of the points of this experiment is to demonstrate that significant effects observed in controlled studies of psychic healing or psychic healers cannot be localized to the healer alone. The best we can say is that the situation, of which the experimenter(s), assistants and (usually) the healer(s) and healing treatment(s) are parts, yields the results.

EXPERIMENT 2

The second experiment in this series was conducted in the same way as the first, with the following exceptions:

- 1) there were 5 student assistants (handlers) and one planned control;
- 2) a new batch of 72 mice were used in this study, the same age, sex and strain, and bred at the same facility as the first group;
- 3) the babesia stabilate comes from a new batch that had been prepared since experiment 1, therefore making it difficult to assess differences between overall babesia levels from experiment 1 to experiment 2, should this occur;
- 4) The babesia and non-babesia designations (used for inducing expectancies in handlers) were changed to "high" or "low" babesia.

This last item was adopted because one of the students in the first experiment had discovered that, contrary to what I had told him, mice with babesia could not communicate the parasite to ones which did not have it. This had been the rationale for having babesia and non-babesia mice being "healed"----to test the curative and preventative aspects of psychic healing. The students in this second experiment were therefore told that I wanted to test the effects of distant healing on very sick and slightly sick mice. The dosage of babesia for the latter, they were told, was so low that no noticeable illness would develop during the experiment but that we would be able to detect it only in the blood samples taken at the end.

SUBJECTS

The subjects were 5 veterinary students who had, like the assistants in experiment 1, come to the lecture on "ESP and Animals" and had volunteered to assist in the study. Only one of them was a strong disbeliever (all no's on the questionnaire in Appendix A), three of them believed in the possibility of the phenomena but had had no personal experiences with it, and one who was "not sure" on the belief questions but was certain she did not have this ability herself.

EXPERIMENTAL ANIMALS

The patients were 72 balb-c female white mice born at a university breeding facility on the same day (two weeks before the experiment). They ranged in weight from 16,5 grams to 23,4 grams with a mean weight of 19,75 grams.

DESIGN

Same as experiment 1, except the babesia and non-babesia groups are labelled high-babesia and low-babesia. These labels, of course, apply only to the expectancies induced in the student handlers.

The procedure, randomizations, analyses and hypotheses were the same as in experiment 1.

RESULTS

The distribution of the babesia counts was checked and found to lie within the bounds of normality (skew=-.281). A one-way ANOVA showed no overall differences between the means of the five handled groups plus control ($F < 1$). The planned analysis of variance was therefore conducted. The results are shown below in Tables 4 and 5.

The results show a significant main effect for the healing expectancy factor ($P < 0.05$) and a marginal trend for the illness expectancy factor ($0.05 < P < 0.10$). Both of these are in the direction

TABLE 4a
 Analysis of Variance for Babesia Counts
 by Illness Expectancy by Healing Expectancy.

SOURCE	SS	DF	MS	F	P
Illness expectancy	5.521	1	5.521	3.100	.084
Healing expectancy	10.086	1	10.086	5.663	.021
Illness x Healing	1.291	1	1.291	.725	.398
Residual	99.736	56	1.781	---	---
Total	116.633	59	1.977	---	---

of the induced expectancies, as can be seen from the average babesia counts in Table 5 below.

The sheep-goat factor was not evaluated because of a heterogeneity of variances between the handled groups (Cochran's $C = .3693$; $P < .025$). Handlers G, P and W are sheep while Handler E is a goat. Handler K scored between 1 and 3 on the sheep-goat rating scale and was not classified into either category.

DISCUSSION

The significant main effect for the healing expectancy factor suggests that so-called paranormal healing effects can be obtained in a well controlled study in which the actual healing treatment is replaced by the expectations that there is one being given. This effect is in the direction of the induced expectancy so that the supposedly healed mice do indeed seem to be "healed" in contrast to the supposedly non-healed mice. The most striking thing about the effect is its uniformity across the 5 handled groups. We can see by

TABLE 4b
Analysis of Variance covarying
initial weights of mice.

SOURCE	SS	DF	MS	F	P
Initial weights	15.363	1	15.363	9.686	.003
Illness expectancy	4.812	1	4.812	3.034	.087
Healing expectancy	7.765	1	7.765	4.896	.031
Illness x Healing	1.517	1	1.517	.957	.332
Residual	87.232	55	1.586	---	---
Total	116.633	59	1.977	---	---

Table 5 that without exception the supposedly healed mice in each cage (group) showed less babesia at the end of the study than the controls in the same cages. For two of the five handled groups, those of handlers P and E, this effect was (independently) marginally significant ($.05 < P < .10$).

This healing expectancy effect is definitely a parapsychological one in the sense that it cannot be entirely explained in terms of known sensory processes, since the "target" animals were not known by anyone until the end of the study. We have therefore produced a paranormal healing effect, or something that resembles a healing effect, in a well controlled laboratory study which cannot be attributed to a specific psychic healer or healing treatment. It must therefore be attributable to something else and that something else may be operating in other psychic healing situations as well.

In experimental studies of psychic healing treatments the experimenters may have reason to expect positive results. The healer may have performed well in pilot or screening trials, may have brought an impressive anecdotal case history of successful healings, or may

TABLE 5
Average babesia counts by condition by handler

	Handler G		Handler P		Handler E	
	Ill	normal	Ill	Normal	Ill	Normal
H	4.7	3.8	5.0	4.9	5.0	3.4
NH	4.8	4.4	5.3	6.3	5.7	4.1
	Handler W		Handler K			
	Ill	Normal	Ill	Normal		
H	4.6	3.3	4.0	3.9		
NH	3.7	6.0	6.5	3.6		

make a strong personal impression on one of the experimental staff members. The results of the current study, modelled after this situation, suggest that the expectation structure may be an important contributor to the results, regardless of what the healer does.

It is important to conceptualize the manipulations in this experiment as part of the "expectancy structure" of the situation, rather than focussing on the handlers as the specific causal agents. While we can be fairly certain that the expectation treatment was an important feature of the experiment, we simply cannot be sure what unseen processes were at work between the (verbal) administration of this treatment and the final outcome of the experiment. It may be, for example, that the experimenter's own expectations were a contributor

to the results. If E has a strong conviction that the expectation treatment administered to the student assistants will effect the outcome, then E's presentation of this verbal treatment to the assistants is likely to be more convincing than if E did not have this conviction. Further, the students' overt responses to E during their interactions and E's satisfaction with his verbal treatment presentation will almost certainly influence E's expectation for the outcome. Thus, there are a variety of psychological and social psychological processes working in concert in this situation which could contribute, separately or in combination, to the outcome.

This is precisely the situation that occurs in real-life healer-patient encounters, whether the treatment is a placebo, an active medication or a psychic healing. It is widely accepted (Frank, 1973; Feldman, 1963; Shapiro, 1979; Rosenthal, 1969) and has been empirically demonstrated in medical contexts (Uhlenhuth et al, 1959; 1966; Dohan et al, 1960) that the expectation structure of the treatment situation is an important determinant of the eventual course of the illness in the patient. While it has been assumed that known sensory processes account entirely for these effects, the current research suggests that, in some circumstances, there may be a parapsychological component as well.

The form of this parapsychological component suggested in this study deserves some comment because it is somewhat unusual with respect to the typical healer-patient situation. Most psychic healing studies, for example, are designed around the implicit or explicit PK model whereby the healer is supposed to be exerting a direct influence upon the biochemical processes in the living targets materials. Stanford (1974) has pointed out, however, that a viable model for PK effects requires an extrasensory "guidance" system to direct the assumed activities in order to produce the specific results. The parapsychological component under study in the current series is this extrasensory guidance system since it is not necessary to assume PK effects in order to explain the results. While we cannot be sure that PK was not involved, it is sufficient to assume that the manner in which the assistants handled the mice was extrasensorially guided, or perhaps that the animals themselves were extrasensorially affected by the random (hidden) assignments to various conditions. In short, we need only assume a communicative psi process to account for these results. At least one practitioner of psychic healing (as well as medicine) has suggested that this is indeed a better model for what actually occurs in the real-life treatment situation (Knowles, 1956).

This interpretation is supported by the fact that the illness expectancy effect in experiment 1 was significant for handler J and marginally significant for the whole group in experiment 2. Therefore, we see that when the assistants know which animals are which there is at least a strong tendency for the expectancy effect to come about, most probably by sensory means (handling). Therefore, if their handling of the animals were guided extrasensorially for the healing expectancy factor, we need not assume any PK effects to account for the results found there.

The sheep-goat variable could not be adequately evaluated here because of the small number of participants (a total of two goats) and because of the distributional problems. However, it seems unlikely that the sheep-goat variable effected the outcome in a simple way. In experiment 2, the only goat was handler E. The babesia counts for his mice (Table 5) show a quite strong illness expectancy main effect which is independently significant beyond the $p < .001$ level, and is in the direction of the induced expectancy. In addition, his mice showed a marginally significant healing expectancy effect ($p < .06$) which was also in the direction of the induced expectancy. This is similar to the pattern for handler J in the first experiment (also a goat) except that the directions of the effects are reversed. For this reversal there is at present no ready explanation.

For the three sheep in experiment 2, handler G's mice showed a marginally significant illness expectancy main effect ($p < .10$), handler P's group showed a marginally significant healing expectancy main effect ($p < .07$), and handler W showed a marginally significant interaction ($p < .10$). All of these were in the direction of the induced expectancies. The unclassified handler (K) showed no effects beyond (or close to) $p = .10$.

No conclusions can be drawn concerning the sheep-goat variable in this study except to say that the patterns of these results suggests the inclusion of this variable in future studies as a potentially interesting variable in psychic healing and expectancy studies.

CONCLUSION

This experimental series supports the hypothesis that the manipulation of the expectation structure in a controlled study of

psychic healing treatments on biological organisms can alone be sufficient to produce significant results similar to those observed when there actually is a healer. It is not clear from this study how the expectancy structure influenced the spread of malaria in the mice, nor whether the experimenter's expectations, the students assistants' expectations, or some combination of these produced the results. It is assumed that the illness expectancy effect, for which it was known which mice were (supposed to be) which, occurred as a result of differential handling of the mice, much the same as has been assumed for experimenter effects on animal responses in social psychological studies (Rosenthal, 1969). The significant healing expectancy effect found in the second study cannot be entirely explained this way because it was not known which mice were assigned to which group. This must therefore involve a psi "guidance system" in order to account for the results. It is suggested that this psi guidance system, a communicative process, may work in concert with the sensory (eg., handling) effects and other known processes to produce the final outcome. Thus, it is not necessary to assume any PK events to account for these results. It is further suggested that a parsimonious model for psychic healing in real-life settings need only involve this communicative process, sometimes involving extrasensory or psi aspects, working together with known psychological and social psychological influences on the target condition.

By the design of these experiments, the results may generalize to other healer-patient encounters as well. It is concluded that in animal health care contexts, there may at times be subtle social psychological and parapsychological processes in operation that influence the course of illness in the patients as a result of the expectancy structures of these situations. The size of these influences may be quite small and, in the case of serious illnesses such as malaria, may exert only negligible effects on the ultimate outcome. Further research is necessary to assess the size of these effects with other illness models and a variety of expectancy structures, both naturally occurring and induced. Further research is also needed to determine whether, and to what extent, these influences may operate in human health care situations.

The sheep-goat variable could not be statistically analyzed but the patterns of scores suggest that further research is warranted with this variable in healing and expectancy studies.

ABSTRACT

A review of the literature concerning the effects of psychic healing and PK (psychokinesis) treatments on biological systems suggests that this area may be a fruitful one for parapsychology. However, problems with inadequate controls and generalizability, together with the problems inherent in treatment effectiveness research generally, restrict the interpretability of the existing studies. To overcome some of these difficulties, an experimental series was conducted which explores the effects of an "expectancy structure" that may be found in laboratory studies as well as real-life healer-patient encounters, on the course of an organic illness. Veterinary students were given expectations regarding the illness level and "treatment" (absent healing from a successful psychic healer) of sick (malaria) mice which they were responsible for handling. Hemoanalyses showed that the actual illness conformed to the handlers expectations for both factors, even when the handlers could not have known by sensory means which mice were (supposed to be) which. The implications for laboratory tests of psychic healing, explanatory models for psychic healing in real-life settings and the size of the effect are discussed.

ACKNOWLEDGEMENTS

I am indebted to many persons, too numerous to list here, for valuable assistance with this project. I am especially grateful to Professor Martin Johnson, Brian Millar, Joop Houtkooper and Frans Snel for technical advice and assistance, and to the director and staff members of the Veterinary Pathology Laboratory (State University of Utrecht) for their cooperation.

REFERENCES

Attevelt, H. A Statistical Survey of the Patients of Paranormal Healers. Amsterdam: Nederlands Federation for Paranormal and Naturopathic Healers (NFPN), 1981.

Barry, J. PK on fungus growth. *Journal of Parapsychology*, 1968, 32, 55.

Benson, H. and McCallie, D.P. Angina pectoris and the placebo effect. *New England Journal of Medicine*, 1979, 300, 1424-1429.

Braud, W. Psychokinetic influence upon *E. coli* bacteria. Unpublished manuscript, University of Houston, 1974.

Braud, W., Davis, G. and Wood, R. Experiments with Matthew Manning. *Journal of the Society for Psychical Research*, 1979, 50, 199-223.

Deamer, D.W. Observations using biological materials. In J. Mishlove (Ed.), *A Month with Matthew Manning: Experiences and Experiments in Northern California during May-June, 1977*. San Francisco: Washington Research Center, 1979, pp. 48-52.

Dohan, F., Taylor, E. and Moss, N. The role of the surgeon in the prolongation of uncomplicated surgical convalescence. *Surgery, Gynecology and Obstetrics*, 1960, 111, 49-57.

Edge, H. The effect of laying on of hands on an enzyme: An attempted replication. In W.G. Roll (Ed.), *Research in parapsychology*, 1979. Metuchen, N.J.: Scarecrow, 1980.

Ehrenwald, J. Parapsychology and the healing arts. In B.B. Wolman (Ed.), *Handbook of Parapsychology*. New York: Van Nostrand Reinhold, 1977, pp. 541-556.

Feldman, P.E. Non-drug parameters of psychopharmacotherapy: The role of the physician. In M. Rinkel (Ed.), *Specific and Non-Specific Factors in Psychopharmacology*. New York: Philosophical Library, 1963.

Finkler, K. Non-medical treatments and their outcomes. *Culture, Medicine and Psychiatry*, 1980, 4, 271-310.

Finkler, K. Non-medical treatments and their outcomes, part 2: Focus on adherents of Spiritualism. *Culture, Medicine and Psychiatry*, 1981, 5, 65-103.

Frank, J.D. *Persuasion and Healing*. New York: Schocken Books, 1973 (revised ed.).

Frank, J.D. Faith that heals. *Johns Hopkins Medical Bulletin*, 1975, 139, 129-131.

Goddard, H.H. The effects of mind on body as evidenced by faith cures. *American Journal of Psychology*, 1899, 10.

Grad, B. A telekinetic effect on plant growth. *International Journal of Parapsychology*, 1963, 5, 117-133.

Grad, B. A telekinetic effect on plant growth II: Experiments involving treatment of saline in stoppered bottles. *International Journal of Parapsychology*, 1964, 6, 473-498.

Grad, B. A telekinetic effect on yeast activity. *Journal of Parapsychology*, 1965, 29, 285-286.

Grad, B. The laying-on-of-hands: Implications for psychotherapy, gentling and placebo effect. *Journal of the American Society for Psychical Research*, 1967, 61, 286-305.

Haraldsson, E. and Olafsson, O. A survey of psychic healing in Iceland. *The Christian Parapsychologist*, 1980, 3(8), 276-279.

Haraldsson, E. and Thorsteinsson, T. Psychokinetic effects on yeast: An exploratory experiment. In W.G. Roll, J.D. Morris, and R.L. Morris (Eds.), *Research in Parapsychology*, 1972. Metuchen, N.J.: Scarecrow, 1973, pp. 20-21.

Harari, S.B. Exploratory study of PK effects on the growth of seedlings. Unpublished manuscript, Psychical Research Foundation, Durham, North carolina, 1974.

Heaton, E. Mouse healing experiments. Unpublished manuscript. Foundation for Research on the nature of Man, Durham, North Carolina.

Hickman, J. Plant growth experiments with Matthew Manning. In J. Mishlove (Ed.), *A Month with Matthew Manning: Experiences and Experiments in Northern California during May-June, 1979*. San Francisco: Washington Research Center, 1979, pp. 74-76.

Kief, H. A method for measuring PK ability with enzymes. In W.G. Roll, R.L. Morris and J.D. Morris (Eds.), *Research in Parapsychology*, 1972. Metuchen, N.J.: Scarecrow, 1973.

Kmetz, J. PK experiments with cancer cells. Unpublished manuscript, Science Unlimited Research Foundation, San Antonio, Texas. (Cited in Braud et al, 1979).

Knowles, F.W. Some investigations into psychic healing. *Journal of the American Society for Psychical Research*, 1954, 48.

Knowles, F.W. Psychic healing in organic disease. *Journal of the American Society for Psychical Research*, 1956, 50, 110-117.

Knowles, F.W. Rat experiments and mesmerism. *Journal of the American Society for Psychical Research*, 1959, 53, 62-65.

Krieger, D. The response of in-vivo human hemoglobin to an active healing therapy by direct "laying-on" of hands. *Human Dimensions*, 1972 (Fall).

Krieger, D. The relationship of touch, with intent to help or heal, to subjects in-vivo hemoglobin values: A study in personalized interaction. *Proceedings of Ninth American Nurses' Association Nursing Research Conference*. New York: American Nurses' Association, 1973.

Krieger, D. Therapeutic Touch: The imprimatur of nursing. *American Journal of Nursing*, 1975, 75, 784-787.

Krieger, D. Healing by the "laying-on" of hands as a facilitator of bioenergetic change: The response of in-vivo human hemoglobin. *Psychoenergetic Systems*, 1976, 1(3), 121-129.

Lenington, S. Effects of Holy Water on the growth of radish plants. *Psychological Reports*, 1979, 45, 381-382.

Leuret, F. and Bon, H. *Modern Miraculous Cures*. London: Peter Davies, 1957.

McDonald, R., Hickman, J. and Dakin, H. Preliminary Physical Measurements of Psychophysical Effects Associated with Three Alleged Psychic Healers. San Francisco: Washington Research Center, 1976.

Metta, L. Psychokinesis of lepidopterous larvae. *Journal of Parapsychology*, 1972, 36, 213-221.

Nash, C.B. Psychokinetic control of bacterial growth. *Journal of the*

Society for Psychical Research, 1982, 51, 217-221.

Nash, C.B. and Nash, C.S. The effect of paranormally conditioned solution on yeast fermentation. *Journal of Parapsychology*, 1967, 31, 314.

Nash, C.B. and Nash, C.S. Psi-influenced movement of chicks and mice onto a visual cliff. In W.G. Roll, J. Beloff and J. McAllister (Eds.), *Research in Parapsychology*, 1980. Metuchen, N.J.: Scarecrow, 1981, pp. 109-110.

Null, G., Stone, C., Revici, E., Bryan, C. and Berman, V. An experiment in paranormal healing: Influence exerted by a parapsychological technique with cancerous ascites in mice. New York: Nutrician Institute of America.

Onetto, B. and Elguin, G. Psicoquinesis en la Tumerogenesis. Abstracted in *Journal of Parapsychology*, 1966, 30, 220.

Pauli, E. PK on living targets as related to sex, distance and time. In W.G. Roll, R.L. Morris and J.D. Morris (Eds.), *Research in Parapsychology*, 1972. Metuchen, N.J.: Scarecrow, 1973, pp. 68-70.

Richmond, N. Two series of PK tests on paramecia. *Journal of the Society for Psychical Research*, 1952, 36, 577-588.

Rose, L. Some aspects of paranormal healing. *British Medical Journal*, 1954, 2, 1329.

Rosenthal, D. and Frank, J.D. Psychotherapy and the placebo effect. *Psychological Bulletin*, 1956, 53, 294.

Rosenthal, R. Interpersonal expectancies: The effects of the experimenter's hypothesis. In R. Rosenthal and R. Rosnow (Eds.), *Artifact in Behavioral Research*. New York: Academic Press, 1969, pp. 181-277.

Rosenthal, R. and Rubin, D. Interpersonal expectancy effects: The first 345 studies. *The Behavioral and Brain Sciences*, 1978, 3, 377-415.

Rubile, B. and Rauscher, E. Effects on motility behavior and growth rate of salmonella typhimurium in the presence of Olga Worrell. In

W.G. Roll (Ed.), *Research in Parapsychology*, 1979. Metuchen, N.J.: Scarecrow, 1980.

Rush, J. Problems and methods in psychokinesis research. In S. Krippner (Ed.), *Advances in Parapsychology 1: Psychokinesis*. New York: Plenum, 1977.

Shapiro, A.K. The placebo effect in medical and psychological therapies. In S. Garfield and A. Bergin (Eds.), *Handbook of Psychotherapy and Behavior Change*. New York: Wiley, 1978 (second edition), pp. 369-410.

Smith, J. Paranormal effects on enzyme activity through "laying-on" of hands. *Human Dimensions*, 1972, (Spring-Summer), 15-19.

Snel, F. PK influence on malignant cell growth. Research Letter of the Parapsychology Laboratory, University of Utrecht, 1980, (August), number 10, 19-27.

Solfvin, G.F. Psychic healing: A review and commentary. In preparation.

Stanford, R. An experimentally testable model for spontaneous psi events. II. Psychokinetic events. *Journal of the American Society for Psychical Research*, 1974, 68, 321-356.

Straunch, I. Medical aspects of "mental" healing. *International Journal of Parapsychology*, 1963, 5, 135-165.

Tedder, W. and Monty, M. Exploration of long-distance PK: A conceptual replication of the influence on a biological system. In W.G. Roll, J. Beloff and J. McAllister (Eds.), *Research in Parapsychology*, 1980. Metuchen, N.J.: Scarecrow, 1981, pp. 90-93.

Tunnell, G. Three dimensions of naturalness: An expanded definition of field research. *Psychological Bulletin*, 1977, 84, 426-437.

Uhlenhuth, E., Cantor, A., Neustadt, J. and Payson, H. The symptomatic relief of anxiety with meprobamate, phenobarbital and placebo. *American Journal of Psychiatry*, 19659, 115, 905-910.

Uhlenhuth, E., Rickels, K., Fisher, S., Park, L., Lipman, R., and Mock, J. Drug, doctor's verbal attitude and clinic setting in the symptomatic response to pharmacotherapy. *Psychopharmacologia*, 1966, 9, 392-418.

Vasse, P. and Vasse, C. Influence de la pensee sur la croissance des plantes. *Nouvelle Serie*, 1948, (N.2), 87.

Watkins, G. and Watkins, A. Possible PK effects on the resuscitation of anesthetized mice. *Journal of parapsychology*, 1971, 35.

Watkins, G., Watkins, A. and Wells, R. Further studies on the resuscitation of anesthetized mice. In W.G. Roll, R.L. Morris and J.D. Morris (Eds.), *Research in Parapsychology*, 1972. Metuchen, N.J.: Scarecrow, 1973, pp. 157-159.

Weininger, O. Mortality of albino rats under stress as a function of early handling. *Canadian Journal of Psychology*, 1953, 7, 111-114.

Weininger, O. Physiological damage under emotional stress as a function of early experience. *Science*, 1954, 119, 285-286.

Wells, R. and Klein, J. A replication of a "psychic healing" paradigm. *Journal of Parapsychology*, 1972, 36, 144-149.

Wells, R. and Watkins, G. Linger effects in several PK experiments. In J.D. Morris, W.G. Roll and R.L. Morris (Eds.), *Research in Parapsychology*, 1974. Metuchen, N.J.: Scarecrow, 1975, pp. 143-147.

West, D.J. *Eleven Lourdes Miracles*. London: Duckworth, 1957.

APPENDIX A

Initial Questionnaire

Do you believe that it is POSSIBLE that in a controlled laboratory experiment, sick mice might improve as a result of:

- | | | |
|--|-----|----|
| a. a paranormal healer (magnetizer)
sending healing from a distance ? | YES | NO |
| b. a paranormal healer holding or
stroking the mice with their hands ? | YES | NO |
| c. a person (not a paranormal healer)
who prays or wishes for them to
get well ? | YES | NO |

Have you yourself had any experiences in which you felt that your prayers, wishes or thoughts may have:

- | | | |
|---|-----|----|
| a. helped a sick animal to get better ? | YES | NO |
| b. helped a sick person to get better ? | YES | NO |

If you had a pet that was sick, do you think
that your prayers, wishes or thoughts might
help it to get well again ?

YES NO

I am currently conducting experiments in which paranormal
healers are sending "healing" from a distance to sick mice.
I need assistants to handle and to weigh the mice. Each
assistant must make four (4) visits to the Veterinary
Pathology Institute (on Biltstraat) where the mice are
kept. Each visit will take about an hour.

Would you volunteer to assist me in this project ? YES NO

If yes, please complete the following:

Name.....

Address.....

Telephone number.....

APPENDIX B

Information Sheet for Handlers

This is an experimental study of psychic healing at a distance. A psychic healer who has shown significant results in preliminary experiments of this sort has agreed to participate in this series of tightly controlled studies. He will be sending psychic healing from a distance to some sick mice without having any physical contact with them. He will only have photos of the mice that are to be healed, just as in our preliminary work.

The subjects for this study are 72 white mice. These mice are involved in the continuing studies of an immunization procedure for the blood parasite *Babesia rhodani* which was developed and is being tested here. The mice will receive the parasite by injection tomorrow, after which you will be the only persons having contact with them for the remainder of the experiment. Next week, the mice will receive a drug cure which is 100% effective. That is, unless something unusual happens, they will all survive. Thereafter, they will be returned to the veterinary group for their continuing studies of the long-term effects of this immunization procedure.

You, as handlers, are an important element in this experiment. Previous studies of psychic healing have not taken sufficient account of the persons who handle the experimental animals. There is a considerable body of evidence to suggest that the beliefs and attitudes of these persons can have an effect upon the results of the experiment. Looking at it another way, you are the "middle-men" between the psychic healer and the mice, since you are the only persons having contact with mice while the healing is being done. For this reason, each of you has been especially selected for this experiment on the basis of your responses to the questionnaire on psychic healing.

Each of you will be responsible for one cage with twelve (12) mice. Six of the mice will receive a high dosage of *Babesia*, while the other six will receive a low dosage. This latter group may not develop any

noticeable signs of sickness during the experiment since the babesia will spread much more slowly. Half of the mice (3) in each of these two conditions will be receiving psychic healing from the healer, while the other half will not. You will not, of course, know which mice are receiving healing.

The experiment will test the following hypotheses:

1. the "healed" mice will develop less sickness than the control (unhealed) mice.
2. there will be a greater healing effect upon the high babesia mice than on the low babesia mice.

The degree of sickness is being assessed by the weights that you will record and by the analysis of a blood sample that will be taken just prior to the drug cure. (The blood sample will be taken by a trained technician)

Today, you must photograph, weigh and mark each of the 12 mice in your care. It is important that you carefully follow the detailed procedures that you will be given. When you are through, you mice will be assigned to a specific place in the next room (the rat room) where they will stay for the remainder of the experiment.

You must return to this building on Friday, Monday and next Wednesday to check on the mice and to weigh them. You may do this at any time that is convenient for you. However, I would like you to make all of these visits at the same time of day. If you would like to visit your mice on other occasions as well, I encourage you to do so, but you should not handle them except during the scheduled weighings. In addition, please be sure to make a note of any extra visits that you make in the notebooks that I am providing you.

One final thing. It is important that you concentrate all of your

attention only on your own mice. This means that you should not touch or look into another person's cage and you must not discuss anything about this experiment with anyone during this next week, and especially not with any of the other handlers.

PSI TRANSMISSION OF ANGER AND ITS ENHANCEMENT
THROUGH MEANING

Shulamith Kreitler
Hans Kreitler
Tel Aviv University

This study had two goals. One was to examine whether emotions can be transmitted through psi from agent to receiver. The other was to examine whether psi effects can be facilitated or intensified by producing in subjects a temporary dominance of personal-subjective meaning.

A study on the direct psi transmission of emotions presents a missing link in the traditional research domain of psi and emotions. Previous investigators noted relations between psi and emotions but in their studies treated emotion as a variable that merely interacts with psi (Carpenter, 1968, 1969, 1970; Nielssen, 1970; Osis and Carlson, 1971; Price, 1973; Schmeidler, 1971; Stevenson, 1970). Moss and Gengerelli (1967) tested directly the question whether emotion itself is transmitted by way of psi but their study yields inconclusive findings. Since their results consist of matching verbal reports of agents and receivers, and since due to personality dynamics these reports consist in a tangled mixture of sensory, cognitive, and emotional elements, it is impossible to ascertain whether it was direct emotional cues that formed the basis for the judgement of correspondence between agent and receiver. Yet, only if that was the case it can be concluded that emotions were transferred.

Our study was designed to clarify whether emotions can be

transmitted via psi. More specifically, since cognitive and experiential factors interact in emotions (Schachter, 1964), we distinguished methodologically between emotion as it may be conceived ideationally and emotion as it may be felt experientially. Thus we studied the effects of psi on these two aspects of emotions.

Focusing on the effects of meaning represents a continuation of our previous attempts to investigate the conditions facilitating the occurrence of psi (Kreitler and Kreitler, 1972, 1973, 1974a, 1974b). Since we view psi as a phenomenon with a potentially great importance for cognition (Kreitler and Kreitler, 1974a), and since meaning is so central to cognition that cognition itself has been defined as a meaning-processing system (Kreitler and Kreitler, 1976, Chapter 2), a study of the relations of psi to meaning is a requisite for promoting understanding in this domain. In particular, personal-subjective meaning, that is one of the two modes of meaning, seemed to us to be a factor that enables understanding and integrating many cognitive and apparently non-cognitive conditions affecting psi.

In order to explain the relations between psi and meaning that underlie this study it is necessary to describe, however briefly, some of the elements of our theory of meaning (Kreitler and Kreitler, 1976, chapter 2; Kreitler and Kreitler, 1983).

On the basis of extensive empirical studies we define meaning as a pattern of meaning values focused on some referent. The referent may be any stimulus, ranging from a word, a tone or a concept to a picture or a whole situation. Meaning values are cognitive contents that convey something about the referent in terms of any one of a specific set of meaning dimensions. Meaning dimensions are general classes or categories of contents. There are 22 meaning dimensions, for example, Function (e.g., when the referent is 'mop' the statement 'serves for cleaning' is a meaning value along the dimension Function), Locational Qualities, Feelings and Emotions, etc.. Meaning values may be expressed verbally or non-verbally, they may be concrete or abstract, true or false, interpersonally-shared or purely subjective.

The aspect of meaning that is important for the present study is the types of meaning. Types serve for characterizing the manner in which meaning values are related to their referent. There are four basic types: the attributive that characterizes the direct relation of a meaning value to a referent; the comparative that includes the

relations of similarity, dissimilarity, etc. (e.g., Love - the opposite of hatred); the exemplifying-illustrative that includes exemplification of the meaning through an instance, a situation or a scene; and the metaphoric-symbolic that includes communication of the meaning through metaphors, symbols, etc..

The types of meaning fall into two distinct modes: lexical interpersonally-shared meaning, defined through the attributive and comparative types, and personal-subjective meaning, defined through the exemplifying-illustrative and the metaphoric-symbolic types. This distinction is reminiscent of comparable distinctions drawn by Piaget (1948) (socialized versus egocentric meaning), Goldstein and Scheerer (1941) (abstract versus concrete), Ogden and Richards (1949) (symbolic versus emotive), Werner and Kaplan (1963) (lexicalized versus sense) and so on. Our distinction is based on empirical findings which showed that when subjects are required to communicate lexical meaning they use predominantly the attributive and comparative types while if they are required to communicate personal meaning they use predominantly the other two types.

The types of meaning play a specific role in cognition that is distinct from the role played by the dimensions of meaning. Whereas the dimensions determine performance in specific cognitive tasks (e.g., comprehension of text, storage in long-term memory), the modes or types of meaning represent basic cognitive processes.

In the present context our interest is focused on the personal-subjective meaning. This includes, on the one hand, the various kinds of concretizations, static images or dramatic dynamically evolving scenes, and, on the other hand, interpretive conceptualizations, metaphors and symbols proper that are complex metaphors resolving through imaginal means some conflict or contradiction (Kreitler, 1965). In a series of studies we were able to show that personal-subjective meaning is central for experiencing art and for creative performance in general. The latter conclusion is based not only on a correlational study between personal-subjective meaning and creative performance but also on actively inducing personal-subjective meaning in subjects and studying its effect on creativity (Kreitler and Kreitler, 1980).

We expected that an active induction of personal-subjective meaning in subjects would enhance the reception of psi messages by these subjects. The rationale for this assumption was twofold. First, a

temporal dominance of personal-subjective meaning seems to us to be the element common to many states or conditions found to be facilitating in regard to psi: hypnosis (Rao, 1966, pp.37-41); meditation (Osis and Bokert, 1971); sensory deprivation (Honorton, 1977), psychotherapy (Ehrenwald, 1947; Eisenbud, 1970); hypnagogic imagery state (McKellar, 1959; Myers, 1957); dreams (Ebon, 1966; Servadio, 1966; Van de Castle, 1977; Ullman, 1966); personal relevance of the material to the receiver (or alternately, personal concern on the part of the receiver) particularly in the case of spontaneous psi occurrences; forms of experience dominated by non-verbal (i.e., sensory-imaginal) processes rather than by verbal processes (L.E. Rhine, 1954); the use of visualization in the case of subjects whose thinking is dominated by vivid mental images (Stanford, 1969); and concern over mental state (Nielsen, 1956). It is to be noted that personal-subjective meaning is the only concept capable at present to encompass such a wide range of phenomena and states (see Discussion). Second, dominance of personal-subjective meaning may induce the subject to pay attention to possible psi cues, or at least not to expel them from his or her field of attention, even if they are illogical, incongruous with a present context or seem irrelevant and unexplainable. These effects may be expected because personal-subjective meaning is much less tightly structured and logically ordered than the common lexical interpersonally-shared sub-system of meaning. Attention to the incongruous and acceptance of the apparently illogical may be particularly facilitating in regard to psi since it has often been observed that psi resembles unconscious contents that have to be 'realized', 'elaborated', in short, processed in some form acceptable for the conscious mind (L.E. Rhine, 1953; Tyrrell, 1947). Our findings about the facilitating effects of psi messages that differ greatly from other contemporaneous, external or internal stimuli (Kreidler and Kreidler, 1972, 1973, 1974) point in the same direction.

In sum, the study was designed to test two hypotheses. One was that emotions (in our case, anger) can be transmitted via psi from agents to receivers in the form of experienced feelings or moods as well as in the form of conceptual messages. The second hypothesis was that an experimental induction of temporary dominance of personal-subjective meaning would enhance the effect of psi transmission, in our case, transmission of an emotional state. The two hypotheses have to be considered as independent although in this study they were investigated jointly.

METHOD

Subjects

The total number of subjects was 315. Of these 135 acted as agents and 180 acted as receivers. The subjects were not preselected in any sense. They were regular students at school and were invited to participate in a completely random manner. Also, the decision as to who would act as agent and who would act as receiver was met on completely random grounds. All the subjects were in the age range 12.3 - 14.2 years. Their mean age was 13.4 (s.d.=.26). The mean ages of the agents and receivers were 13.5 and 13.3 respectively. Children in the age range of 12 to 14 years were used as subjects since another study (Kreitler and Kreitler, 1983) showed that at this age the personal-subjective meaning is already well developed and clearly separated and distinguished from the interpersonally-shared meaning in the majority of children. The 135 agents included 66 boys and 69 girls, whereas the 180 receivers included 91 boys and 89 girls. All the 315 subjects were students of the seventh and eight grades in regular primary schools. Sixty of the subjects were students in one school, and 255 subjects were students in another school. The two schools were located in two different towns. The study was conducted in two schools in order to control for possible factors that might affect the findings in one school or another. The student population in the two schools was, however, comparable in socioeconomic status (mostly West and East European).

Design

The design of the study was essentially a 4x3 two-factor design. The four levels of the one factor (factor A) reflected treatments of the agent whereas the three levels of the second factor (factor B) reflected treatments of the receiver. The levels of factor A were: preparation of the agent by subjecting him or her to an 8-minute induction of personal-subjective meaning before the psi-specific task (a1); administering to the agent an 8-minute task of sentence completions before the psi-specific task (a2); administering to the agent no task before the psi-specific task (a3); no agent participates at all (a4). The levels of factor B were: preparation of the receiver

by subjecting him or her to an 8-minute induction of personal-subjective meaning before testing for psi-specific effects (b1); administering to the receiver an 8-minute task of sentence completions before testing for psi-specific effects (b2); administering to the receiver no task prior to testing for psi-specific effects (b3).

It will be noted that levels a2, a3 and a4 of factor A, and b2 and b3 of factor B are included mainly as control conditions making possible the testing of the second hypothesis of the study about the facilitating effects of personal-subjective meaning on psi transmission. The task of sentence completions (levels a2 and b2) was designed to control for the possible effects of administering to the agent or receiver a cognitive task prior to the psi-specific task. It may be noted that the task of sentence completions resembles the induction of personal-subjective meaning in being an essentially cognitive task but differs from it in not being based on meaning elaboration.

Again, levels a3 and b3 were designed to control for the possible effects, whether facilitating or inhibiting, of administering to the agent or receiver any task, preparatory or not, prior to the actual psi-specific task.

Finally, level a4 was included in order to control for the effects of the agent as such.

Thus there were 12 groups of subjects. In each group there were 15 trials. In nine groups there were in each trial one agent and one receiver; in three groups only a receiver participated. The trials of each group were run under externally identical conditions. The only differences were those dictated by the design of the study.

The major hypotheses of the study were tested by an analysis of variance procedure coupled with comparison of means through the method developed by Scheffe (1953). We used the Scheffe method although our mean comparisons were of the a priori type, that is, less strict than the a posteriori comparisons and although the Scheffe method is, according to the authorities in the field (e.g., Winer, 1962) the strictest among the a posteriori procedures, the least sensitive for pairwise comparisons and by setting the highest levels for alpha maximizes the risk of a Type I error (i.e., rejecting the tested hypothesis when it is in fact true). The reason for our choice was the

decision to pit our hypotheses against the greatest odds, since running the risk of Type I errors seems to us far less risky than committing a Type II error, in science in general and in psi research in particular.

Procedure

Five individuals were involved in the trials of the study: an agent, a receiver, experimenter A who worked with the agent, experimenter B who worked with the receiver, and experimenter C who acted as a coordinator. None of these individuals was acquainted with all parts and aspects of the study. In order to prevent any uncontrolled effects each individual knew only about that part that was strictly indispensable for him or her to know.

All in all 12 different individuals were involved in the study as experimenters. There were five individuals (four female, one male) who acted in turn as experimenter A (with the agent) and five other individuals (three female, two male) who acted in turn as experimenter B (with the receiver). Two different individuals (one male, one female) acted as experimenter C. All the experimenters were students at Tel Aviv University, nine of them were undergraduate students and three have just completed their undergraduate studies. They were all in the age range of 22 to 26 years. They were recruited in the regular way experimenters for studies are recruited, they were not preselected in any way and were paid for hours of work as other experimenters are. None of the experimenters had participated before in any role in any psi experiment and none was chosen because of any particular interest or skill in psi. Before starting work they were interviewed as other experimenters are about their general interests which turned out to be of the type common for undergraduates (partly applied work, partly business and partly research). None of the experimenters knew the schools in which the study was conducted, any of the teachers in these schools, or any of the individuals who served as subjects.

Each experimenter participated in the study only in one role (as experimenter A or B or C). The assignment of experimenters to any one of the three roles was completely random. It was done in the following way: before the study began it was decided arbitrarily that if an individual would select a three-digit random number in the range of 101-333, that individual would act as experimenter A, if an individual

would select a three-digit random number in the range of 334-666, that individual would act as experimenter B, and if an individual would select a three-digit random number in the range of 667-999, that individual would act as experimenter C. After nine months the rule was changed in regard to new experimenters so that individuals who selected numbers in the range 334-666 were assigned the role of experimenter A, those who selected numbers in the range 667-999 were assigned the role of experimenter B, and those who selected numbers in the range 101-333 were assigned the role of experimenter C. An experimenter was asked to select a random three-digit number in the first meeting with him or her before the details of the task were discussed.

The experimenters differed in the number of trials in which each participated. The five experimenters who worked with the agents participated in 38, 36, 17, 24 and 20 trials, respectively; the five experimenters who worked with the 180 receivers participated in 39, 41, 35, 49 and 16 trials, respectively. We used several experimenters in order to control for the possible effect of particular experimenters. The experimenters participated in different numbers of trials for reasons that had nothing to do with the experiment proper. The major reason was that being students the foremost commitment of the experimenters was to studying. Thus, they were not always free to participate in experimental work because sometimes they had to prepare exams or term papers. Another reason was that due to the low payment rates common for experimenters their motivation to continue this work for protracted periods of time may waver. Thus, one experimenter left after a certain number of trials because unexpectedly she was offered a more profitable work in another part of the country. Yet, raising fees for these experimenters contrary to custom would have set them in a particularly advantaged class relative to experimenters in other studies and would have introduced an undesirable systematic selection factor characterizing the experimenters in this study.

The study was conducted so that each of the 180 trials was numbered and the corresponding instructions relevant for the experimenters were put into separate envelopes. Each experimenter was assigned several trial cases (i.e., envelopes) randomly according to a system of random numbers. This procedure ensured first, that the assignment of any experimenters to a particular trial would be random, and second, that trials of all 12 conditions would be conducted in random order. Thus, there would be no possibility for interactions between experimenters and conditions. Further, no experimenter would be in a position to

figure out either the structure of the study or the number and kind of trials of which it consists.

In trials in which no agent was to participate the envelope for experimenter B contained merely the instruction to accompany the subject assigned the role of agent immediately back to his or her classroom. This procedure was adopted in order to prevent experimenters A and C from identifying certain trials as perhaps different from others.

As mentioned, the determination of who would act as agent and who as receiver was completely random. In order to ensure secrecy, the agents and receivers for a particular set of trials were drawn from different classes. For every trial it was determined randomly from which class the receiver would be drawn and from which one, the agent. The randomization was performed by experimenter C several days before the trials were conducted. Experimenters A and B got the lists of subjects whom they had to invite for the trials only on the day on which the trials were to take place. There were no refusals on the part of the subjects to participate in the study. Following the usual procedure for inviting subjects for an experiment, the experimenter would go up to the predesignated class and invite the particular subject to come with him or her. From this point on the procedure followed in regard to the subject who acted as agent differed from that followed in regard to the subject who acted as receiver. Table 1 presents an overview of the different stages in the procedures and their coordination.

Procedure in regard to agent

The experimenter entered together with the subject the predesignated room. The experimenter invited the subject to sit down, seated himself at his side, introduced himself and explained by way of introduction that the subject was invited to participate in a study and that the study involved the performance of several different tasks. The first task was then introduced without further delay. In the case of those subjects who had to undergo a preparatory stage, the first task consisted - according to the design - either of induction of personal-subjective meaning (see 'Induction of personal-subjective meaning') or of sentence completions (see 'Sentence completion task'). In either case the preparatory stage lasted for exactly eight minutes.

In the case of those subjects who had to undergo no preparation, the experimenter pretended to be busy looking for some papers apparently necessary for the experiment, asked the subject to make himself or herself comfortable until everything was ready and thus waited unobtrusively until eight minutes passed. At the end of the preparatory stage the experimenter evaluated (if that preparation was at all administered) on a 3-point scale the degree to which the subject immersed himself or herself in the personal-subjective meaning (a large, medium, small degree), and on five separate 4-point scales the degree of tension, fear, gaiety, sadness, and anger evident in the agent's mood and behavior.

Mood evaluations of this kind, here and elsewhere in the experiment, were designed to provide a measure for the interpersonally evident effects of the mood of subjects and also to disguise from the experimenter the goal of the study.

The next task, identical for all agents, consisted of enacting psychodramatically a scene designed to evoke anger. Anger was the emotion selected for this study first, because pretests with 13-14 year olds showed anger to be the least frequent emotional state, and second, because anger has clearcut direct manifestations that make it easy to measure it operationally.

In order to avoid the effects of possible communication of the contents of the psychodramatic scenes among the subjects, and also in order to control for the effects of a particular scene we used interchangeably five different scenes. As an additional precaution against spreading information about the scenes we used as agents students from different classes and different grades that do not know each other. Each agent got only one scene for enactment. The assignment of scenes to agents was done on a completely random basis. The number of trials in which each scene was used was: 29, 28, 28, 30, and 20, respectively. The enactment of the psychodramatic anger-evoking scene lasted precisely 2 minutes.

Then the subject was instructed to 'transmit' the anger to another person. There were two reasons for this instruction. The first and major one was that this instruction made it possible to preserve the 'anger' of the agent for 3 full minutes without the triggering of processes designed to decrease the evoked anger. Secondly, we expected these instructions to enhance the psi effect in line with a previous study (Kreitler and Kreitler, 1973) that showed 'transmitting' a

message to be superior in its effects to merely thinking about a message, at least in some experimental conditions. The precise instructions were: "And now you are still angry and in addition you transmit your anger to another person. Let that other person feel the great anger you feel; let him or her feel it truly, with all their body". If the subject asked about who should feel the anger, the answer was "Never mind who; just somebody who is not here". If the subject asked about how to transmit the anger, the answer was "Do it as you wish; feel the anger and also feel that you transmit it". The duration of these questions and answers never exceeded 10-12 seconds. The stage of feeling and transmitting anger lasted for 3 minutes. The experimenter was instructed to avoid talking to the subject during this stage so as to let him or her concentrate on the anger or transmission or both.

When the 3 minutes were over, the subject was told that "all this was just a game; we wanted to study how children get angry, but now you should try to stop being angry.". The subject was asked to rest for a couple of minutes while the experimenter made some notes. The period of waiting lasted for exactly two minutes. Then the experimenter checked by questions and observation whether the subject was still angry. If that was the case the experimenter discussed with the subject the anger-provoking scene and the causes of the anger and arranged for the enactment of a short improvised psychodramatic scene designed to free the subject from the anger. According to the teachers' reports after debriefing, the attempt to dissolve the induced state of anger proved to be fully satisfactory. In no case the teachers noted anger, aggression or any unusual emotional reactions in the subjects after the experiment or later. Incidentally, this makes the psychodramatic method of dissolving emotional reactions recommendable for studies of this kind. All subjects were then asked the following questions. The first question was: Did you feel anger during the scene? (or: Were you angry during the scene?) If the subject answered positively, he or she was further asked: How much anger did you feel? (or: How angry were you?): Very angry, medium, or a little angry? The responses of subjects to the two parts of this question were evaluated in terms of four degrees: the subject reports feeling no anger, a little, medium, a lot of anger. The second question was: Do you think that you have succeeded in transmitting your anger to someone else? If the subject answered positively, she or he was further asked: How much have you succeeded to transmit: a lot, medium, or a little? The responses of subjects to the two parts of this question were evaluated in terms of four degrees: the subject

reports a little, medium, or a large success in transmission. At the pretest stage for this study we experimented with two further questions about the identity of the person(s) to whom anger was transmitted and about the method used by the subject in transmitting, but we dropped them since they yielded no information and seemed to embarrass the subjects. After answering the questions, the subject was thanked and dismissed with the request not to tell about the experiment anything for several weeks until the study was over.

After the subject left the experimenter summarized his or her notes and impressions by checking responses on the following scales of evaluation: Degree to which the agent expressed anger in the psychodrama through verbal means, through behaviors or through movements; overall degree to which subject seemed to feel anger during the scene; the agent's seriousness of attitude toward the enactment of anger; degree to which the agent immersed himself or herself in psychodramatic enactment; and the agent's seriousness of attitude toward the task of feeling and transmitting anger.

Procedure in regard to receiver

The experimenter entered together with the subject a predesignated room. The experimenter invited the subject to sit down, sat at his or her side, introduced himself or herself, and explained by way of introduction that the subject was invited to participate in a study and that the study involved the performance of several different tasks. The first task was then introduced without further delay. In the case of those subjects who had to undergo a preparatory stage, the first task consisted - according to the design - either of induction of personal-subjective meaning (see 'Induction of personal-subjective meaning') or of sentence completions (see 'Sentence completion task'). In either case it lasted for exactly eight minutes. When no preparation was required, the experimenter pretended to be busy looking for some papers apparently necessary for the experiment, asked the subject to make himself or herself comfortable until everything was ready and thus waited unobtrusively until eight minutes passed.

At the end of the preparatory stage the experimenter evaluated (if that preparation was at all administered) on a 3-point scale the degree to which the subject immersed himself or herself in the personal-subjective meaning (a large, medium, small degree), and on

five separate 4-point scales the degree of tension, fear, gaiety, sadness, and anger evident in the receiver's mood or behavior.

The next task identical for all receiver subjects was called 'an exercise in concentration'. This stage included the training for concentration and ended with giving instructions for emotion reporting. The experimenter seated himself or herself alongside the subject somewhat to the side, so that they did not face each other, and while changing his or her sitting position, activated a tape recorder hidden in a case under the table. Then the experimenter gave the following instructions: 'Sit comfortably. Feel at ease. Close your eyes if you feel like it. And now breathe 10 times quietly (Pause for breath). And now concentrate on your body, try to sense your body, feel all its parts (Pause of 10 seconds). And now start thinking about yourself (Pause of 10 seconds). Think about your feelings (Pause of 10 seconds)'. The experimenter was instructed to space these instructions over 90 seconds, to talk little, to produce a pleasant atmosphere, to speak in a low voice with pauses so as to enable the subject to concentrate on himself or herself, and end the two-minute period with these instructions: 'When I give you the sign "Now" please start telling all that you feel, tell everything, absolutely every feeling that comes up, even if you are uncertain, even if it is very weak or unclear; tell me every feeling even if it is illogical or unexpected; describe every feeling you have as precisely as possible. Go on talking and tell all you feel until I say "Enough". If you have any questions please ask them now because it is not good to stop later'.

The stage of emotion reporting lasted for precisely three minutes. The experimenter did not interfere and if the subject asked anything the answer was: 'I will answer later'.

After exactly three minutes, the experimenter said 'Enough', and introduced immediately the next task which consisted of sentence completions (this stage should not be confused with the Sentence Completion Task that served as a preparatory stage for agents of level a2 and receivers of level b2). The experimenter said: 'I have another request from you. I will read out to you several sentences, each sentence separately. The sentences are incomplete and you will tell me how to complete them. There are no wrong or right answers here. You will simply tell me how it seems best to you to complete the sentences'. There were eight sentences and they were presented in random order, different for each individual. The sentences were read

and not presented in written form in order to enable the subjects to provide the completions that seemed to them adequate without dropping those that they did not know or were not sure how to write down. The incomplete sentences were selected from a larger pool on the basis of pretests which showed that these sentence beginnings triggered in at least 50% of the 30 pretest subjects completions reflecting emotions. The incomplete sentences were: The boy felt; The girl felt; He did it because; She did it because; The most unpleasant feeling is; The strongest feeling is; The feeling that is felt most frequently is; It is undesirable

After this the experiment proper ended but the experimenter was instructed to improve the subject's mood if necessary by talking with him or her and making some jokes before thanking the subject and dismissing him or her. The reason for this was to dispel any feelings of anger that might remain in experimental subjects. Again, as in the case of subjects who served as agents, the inquiry conducted after debriefing showed that the teachers did not note anger, aggression, or any unusual emotional reactions in any subject after the experiment or later.

Immediately after the subject left the experimenter checked his or her evaluations on 4-point scales about the degree of tension, fear, gaiety, anger, and sadness evident in the subject's mood and behavior after the experiment. The experimenter also checked responses on the following evaluation scales: Degree to which the subject succeeded in concentrating during the concentration exercise on himself or herself, degree of tension evident in the subject's mood and behavior in the course of emotion reporting, and the seriousness of the subject's attitude toward the task of reporting emotions.

Coordination and precautions

As shown in table 1, the procedures followed with the agent and the receiver were coordinated precisely in time. Exactly the same procedure was followed with the receiver when there was no agent.

Agent and receiver stayed in separate rooms, located in one school in two different floors of the same building and in another school, in

TABLE 1
Outline of the procedure

Duration	Procedure with agent	Procedure with receiver
1 min	Entrance and seating	Entrance and seating
8 min	Preparation (meaning or Sentences or none)	Preparation (Meaning or Sentences or none)
2 min	Psychodramatic enactment and instructions for transmitting anger	Concentration exercise and instructions for emotion reporting
3 min	Feeling anger and transmitting it	Emotion reporting
2 min	Resting	Sentence completions
1-2 min	Mood improvement if necessary; exit	Mood improvement if necessary; exit

two different buildings about 50 meters apart.

Neither experimenters A and B nor the agents and receivers were in contact with each other or knew of each other's participation in the experiment. At the same period when the experiment was carried out at least five other experimenters were busy conducting different experiments in the same school (but not with the subjects of this study) and all were instructed to keep their work secret from other experiments presumably in order to prevent the spread of rumors based on misunderstandings that might affect the further course of the study on which they worked.

In the course of the experiment proper it was experimenter C who

coordinated temporally the work of the two experimenters without their being aware of it. In order to cover up his or her role as coordinator, the same experimenter pretended to be busy at the same time coordinating different studies. In the course of the experiment, experimenter C stayed outside guarding against any possible communication between the experimenters or subjects. Experimenter C was the only experimenter who knew that the study dealt with ESP but he or she did not know anything else about the contents of the experiment.

In order to check whether the temporal coordination between experimenters A and B was satisfactory, they were asked to note down the exact time (hour, minute, second) when they started each stage of the experiment. Their watches were checked by experimenter C every hour. Trials in which there were deviations in time greater than two seconds in any of the stages were cancelled. There were only two such trials in the whole experiment.

After the whole experiment was over, the tapes of the emotion-reporting stage were given for transcription to a person who was completely ignorant of the experiment and its purpose.

It is of special importance to note that neither of the experimenters A and B nor the subjects who acted as agents or receivers knew that they participated in an ESP experiment. Experimenters A were led to believe that the study dealt with the impact of psychodrama on emotional processes, after different kinds of prior preparation of the subject. Even the instruction to 'transmit' the emotion was explained as a means of intensifying the evoked emotion. In addition, the central role of anger in the experiment was disguised by telling experimenters A that the experiment dealt with five emotions (anger, fear, gaiety, sadness, and jealousy) and that he or she were chosen in order to participate in those trials that were devoted to the study of anger. Experimenters B were led to believe that the study dealt with the process of the unfolding of emotion under conditions of concentration and introspection, after different kinds of prior preparation of the subject. The strict adherence to an exact schedule of time and the notation of the actually followed time schedule were explained to experimenters A and B as necessary for the study of the minutest details of emotional processes.

Induction of personal-subjective meaning (see note 1)

The applied procedure differed from previous applications (Kreitler and Kreitler, 1980) in the choice of stimuli designed not to affect mood appreciably in any specific direction, and in the limitation to eight minutes. The purpose of the procedure was to produce in the subjects a dominance of personal-subjective meaning (i.e., a salience of the exemplifying-illustrative and metaphoric-symbolic types of relation) for the duration of the experiment. The procedure included three parts: basic instructions for the subjects focused on eliciting the communication of personal-subjective meaning to some hypothetical other, using any forms of expression; a list of guiding questions for the subject designed to help the experimenter to stimulate any aspect of personal-subjective meaning that did not occur at all or with sufficient clarity in the subject's spontaneous response; and a list of stimuli (i.e., Summer, Television, Car, Birthday), the first two of which were administered to all subjects, and the latter two only if the eight-minute period was not over. A pleasant permissive atmosphere was preserved all through.

Sentence Completion Task (see note 1)

The same task was used for agents (level a2) and receivers (level b2) as a control in the stage of preparation. There were 106 incomplete sentences (e.g., On the floor; The flowers; etc.) chosen out of a pool of almost 200 as those that were easiest to complete and evoked a majority of completions low in emotional tone. These criteria were designed to prevent interference of this task with the ensuing stages of the experiment. The sentences were read aloud (slowly, for eight minutes) in order to make for a similarity in procedure with the induction of personal-subjective meaning. The subjects were instructed to complete each sentence as they saw fit, there being no true or false answers. The experimenter wrote down the responses. The atmosphere was pleasant and permissive. Most subjects completed 50 - 60 sentences.

Psychodramatic scenes (see note 1)

The five psychodramatic scenes were designed to evoke anger in 13 - 14 years olds. They were selected on the basis of pretests for their anger-provoking potential from a larger pool of scenes constructed in line with suggestions made by pretest subjects 13 - 14 years old. The experimenters were trained on all scenes and in the procedures of leading the subject into psychodramatic enactment. Scene A put the subject in a situation of having been accused and punished by a teacher for disturbing in class without having done so and having no chance of defending oneself. Scene B put the subject in a situation of having been pushed out of a line and thus missing a bus on a morning on which it was particularly important to be in school on time. Scene C put the subject in a situation of not having been picked up by friends for a party in which one wanted to participate. Scene D put the subject in a situation of not having been given the chance to prove knowledge when it was particularly important to do so. Scene E put the subject in a situation in which several frustrations occur in a sequence (i.e., on the bus on the way to school someone forces him or her to get up despite being tired, someone treads on his or her foot, being the target of a diatribe against 'modern youth', etc.).

Variables and measures

The major dependent variables were four variables reflecting anger:

- 1) The number of direct references to anger in the stage of emotion reporting, defined as words expressing directly feelings of anger in the receiver, i.e., 'I feel angry', 'I feel anger', 'There is anger in me', 'I am about to burst' (common Hebrew slang expression for anger), and any variations for these using common synonyms for 'anger' and 'to feel'. Each of the mentioned instances was counted as one reference;
- 2) The number of indirect references to anger in the stage of emotion reporting, defined as words expressing anger attributed to another person, general pronouncements about anger, and metaphoric-symbolic references to anger, e.g., 'I think about my father who was angry when he heard what my aunt said', 'It is funny how important anger is in life', 'I was just thinking about a bomb falling on a town, a bomb is like a clenched fist';
- 3) The sum total of direct and indirect references to anger in the receiver's emotion reporting;
- 4) The number of references to anger in the sentence completions following the

emotion reporting. The maximum number could be eight. Due to the nature of the task and the brevity of the responses no distinction between direct and indirect references could be made here. Only unambiguous references to anger were counted.

Variables 1 - 4 were evaluated on the basis of the recorded material by individuals who knew nothing about the goals or procedure of the study. The material was presented to four psychologists who were asked, each independently, to code it in line with the above described criteria. The intercoder correlations were all in the range of .95 - .99.

In addition to these four dependent variables we tested our hypotheses also in regard to the the following variables that were merely of secondary importance relative to the four major ones, first because they were based on evaluations by the experimenter and not on direct behavioral evidence, and second because two of the three referred to evaluations of mood after the crucial stage of transmission.

These secondary variables were: aI. Evaluations of anger in receivers after the stage of emotion reporting, or an allied variable: aII. Difference in the evaluation of anger in receivers before and after the stage of emotion reporting; bI. Evaluation of gaiety in receivers after the stage of emotion reporting, or an allied variable: bII. Difference in the evaluation of gaiety in receivers before and after the stage of emotion reporting; c. Evaluation of tension in receivers in the stage of emotion reporting.

As noted earlier, these evaluations were made in the course of the experiment by the experimenters. We tested the reliability of evaluations of this kind by pretests in which two different experimenters evaluated independently the mood of subjects who got instructions comparable to those given the receivers. These pretests showed that if the evaluations on 4-point scales are combined into two clusters, i.e., scale points 1 and 2 (low) versus 3 and 4 (high), the interjudge reliability rises to .99. Therefore, this is the procedure we adopted in coding the evaluations by the experimenters in this study.

Finally, we examined the relation of the psi effects to several variables, such as the gender of the agent, the gender of the receiver, individual experimenters, evaluations of the anger, sadness,

fear and gaiety of the agent prior to the psychodramatic enactment, etc. (all these variables are listed at the end of the section 'Results').

RESULTS

Before the data were subjected to the major analyses, preliminary analyses were made in order to examine whether the twelve groups of the study differed in any of the following variables that could have affected the results: In regard to the receivers, separate evaluations of their level of tension, fear, gaiety, sadness, and anger prior to the experiment, and again, in regard to the agents, separate evaluations of their level of tension, fear, gaiety, sadness, and anger prior to the experiment. The 12 groups did not differ in any of the five variables concerning receivers, and the 9 groups in which an agent was present did not differ in any of the five variables concerning agents. It may be of particular interest to note that concerning the major variable anger the grand mean of the receivers prior to the experiment was 1.13 ($F=.299$, $df=2$, $p=.985$), and that the grand mean of the agents prior to the experiment was 1.18 ($F=.22$, $df=3$, $p=.988$). Further, no receiver or agent got a high rating (scale points 3 or 4) on anger prior to the experiment. These findings indicate that any anger evidenced by the receivers in the stage of emotion reporting or after it may be attributed safely to the experimental manipulations.

The first hypothesis of the study was that emotion can be transmitted from agent to receiver both as a feeling and as ideational contents. Tables 2 and 3 present the data and findings relevant for testing this hypothesis. Table 2 shows that for all examined variables - the four major dependent variables and the five secondary variables based on evaluations - there was a highly significant main effect due to the agent. For all the major variables the effect was at the .001 level. Does this imply that all the four states of the agent (i.e., two different kinds of preparation, no preparation, no agent) differed significantly from each other in their effectiveness, or perhaps that one state was significantly more effective than the others? The answer to this question is given in the right hand part of table 3. This table shows unambiguously that the groups for which an agent was present (groups a, b, and c) did not differ from each other at all, but that each of these groups differed significantly from the group

for which no agent was present (i.e., group d). This is true for all nine variables. Hence the conclusion is that the existence of an agent is responsible for the occurrence of psi effects in receivers. The only thing about an agent that makes a difference is whether there is an agent or there is none. Whether the agent undergoes some kind of preparation (by meaning induction or sentence completions) or not and if so, which preparation, are factors of no consequence.

In sum, an emotion such as anger can be transmitted from agent to receiver as actual feelings reported introspectively (i.e. direct references to anger in emotion reporting), as ideational contents (i.e., indirect references to anger in emotion reporting), references to anger in sentence completions, and as mood swings evident to an outside observer in the form of increased tension in the course of introspecting, as well increased anger and decreased gaiety after introspecting.

The second hypothesis was that preparation of the receiver through an induction of personal-subjective meaning would enhance the psi effects relative to an irrelevant preparation through sentence completions or no preparation at all. Tables 2 and 3 show that in regard to the four major dependent variables the different states of the receiver contributed differentially to the results. The overall effect is again highly significant ($p=.001$ in all cases). There are two exceptions in regard to the secondary variables based on evaluations. The different states of the receiver do not differ significantly in regard to: the evaluation of gaiety after emotion reporting as well as difference in gaiety between before and after reporting, and the evaluation of tension in the course of reporting. Thus, the states of the receiver differ significantly in their effectiveness when the six direct, indirect and evaluated measures of anger are concerned.

However, in order to find out in which respect the states of the receiver differ, it does not suffice to examine the means of the three receiver groups presented in table 4. Indeed, all the comparisons indicate the superiority of the group that got preparation through personal-subjective meaning, but these means reflect the data of all receivers, regardless of whether there was an agent or not.

In order to get a direct answer to the question about the relative effectiveness of preparation through meaning, we performed another series of analyses of variance (2×3). The factor relating to the agent

TABLE 2
Analyses of variance (4x3) of the effects
of agent and receiver on variables reflecting anger

	Agent	Receiver	Agent x Receiver	Explained	Residual	Total
	df=3	df=2	df=6	df=11	df=168	df=179
Variable: Number of direct references to anger in stage of emotion reporting.						
Mean square:	25.622	69.272	6.650	23.210	2.240	3.528
F:	11.44	30.93	2.97	10.36		
Significance:	.001	.001	.009	.001		
Variable: Number of indirect references to anger in stage of emotion reporting.						
Mean square	23.532	82.756	12.793	28.442	2.406	4.006
F:	9.78	34.40	5.32	11.82		
Significance:	.001	.001	.001	.001		
Variable: Total number of direct and indirect references to anger in stage of emotion reporting						
Mean square:	97.324	299.622	37.830	101.654	6.357	12.213
F:	15.31	47.13	5.95	15.99		
Significance	.001	.001	.001	.001		
Variable: Number of references to anger in sentence completions after stage of emotions reporting						
Mean square:	57.852	32.089	1.874	22.634	2.271	3.523
F:	25.47	14.13	.82	9.96		
Significance	.001	.001	n.s.	.001		

TABLE 2 (continued)

	Agent	Receiver	Agent x Receiver	Explained	Residual	Total
Variable: Evaluation of anger in receivers after stage of emotion reporting.						
Mean Square	9.576	5.539	.709	4.006	.452	.671
F:	21.17	12.24	1.57	8.85		
Significance:	.001	.001	n.s.	.001		
Variable: Difference in evaluation of anger in receivers before and after emotion reporting.						
Mean square:	9.487	5.939	.887	4.151	.575	.795
F:	16.49	10.32	1.54	7.21		
Significance:	.001	.001	n.s.	.001		
Variable: Evaluation of gay mood in receivers after stage of emotion reporting.						
Mean square:	21.087	.067	.215	5.880	.375	.714
F:	56.17	.18	.57	15.66		
Significance	.001	n.s.	n.s.	.001		
Variable: Difference in evaluation of gay mood in receivers before and after stage of emotion reporting.						
Mean square:	29.074	.206	1.524	8.798	1.083	1.557
F:	26.86	.19	1.41	8.13		
Significance:	.001	n.s.	n.s.	.001		
Variable: Evaluation of tension in receivers in the stage of emotion reporting.						
Mean square:	3.428	1.239	1.017	1.715	.660	.724
F:	5.20	1.88	1.54	2.60		
Significance	.002	n.s.	n.s.	.004		

TABLE 3
Comparison of means between subgroups of agent
and between subgroups of receiver

Agent				Receiver			
Means	F	Significant		Means	F	Significant	
	df=3/176	group			df=2/178	group	
		differences				differences	
Direct references to anger in reporting							
a: 2.09		a-d		A: 2.65		A-B	
b: 1.78	8.13	b-d		B: .73	24.87	A-C	
c: 1.42	p<.0001	c-d		C: .85	p<.0001		
d: .35							
Indirect references to anger in reporting							
a: 1.75		a-d		A: 2.75		A-B	
b: 1.80	6.41	b-d		B: .68	26.56	A-C	
c: 1.71	p=.0004	c-d		C: .75	p<.0001		
d: .31							
Total of references to anger in reporting							
a: 3.84		a-d		A: 5.38		A-B	
b: 3.60	9.04	b-d		B: 1.41	33.42	A-C	
c: 3.13	p<.0001	c-d		C: 1.61			
d: .64							
References to anger in sentence completions							
a: 3.00		a-d		A: 3.13		A-B	
b: 2.93	22.25	b-d		B: 1.73	10.03	A-C	
c: 2.69	p<.0001	c-d		C: 2.07	p<.0001		
d: .62							

Note: The table presents the means of the four agent groups (a. preparation through meaning, b. preparation through sentence completions, c. no preparation, d. no agent), and the three receiver groups (A. preparation through meaning, B. preparation through sentence completions, C. no preparation). All group comparisons are by the Scheffe method.

TABLE 3 (continued)

Agent			Receiver		
Means	F	Significant	Means	F	Significant
	df=3/176	group differences		df=2/178	group differences
Evaluation of anger after reporting					
a: 2.42		a-d	A: 2.40		A-B
b: 2.31	18.45	b-d	B: 2.02	8.99	A-C
c: 2.15	p<.0001	c-d	C: 1.80	p<.0002	
d: 1.40					
Difference in evaluation of anger before and after reporting					
a: -1.29		a-d	A: -1.28		A-B
b: -1.15	14.67	b-d	B: -.87	8.05	A-C
c: -1.04	p<.0001	c-d	C: -.67	p=.0004	
d: -.27					
Evaluation of gaiety after reporting					
a: 1.38		a-d	A: 1.55		
b: 1.15	57.55	b-d	B: 1.58	.09	
c: 1.20	p<.0001	c-d	C: 1.62	n.s.	
d: 2.60					
Difference in evaluation of gaiety before and after reporting					
a: .75		a-d	A: .55		
b: 1.07	26.73	b-d	B: .60	.13	
c: 1.00	p<.0001	c-d	C: .48	n.s.	
d: -.64					
Evaluation of tension in reporting					
a: 2.82		a-d	A: 2.52		
b: 2.87	5.05	b-d	B: 2.70	1.72	
c: 2.73	p=.0022	c-d	C: 2.80	n.s.	
d: 2.27					

TABLE 4

Analyses of variance (2x3) of the effects of agent and receiver on variables reflecting anger and comparisons of receiver effects

Source of variation	Mean square	Df	F	Means	F	Significant group differences
Direct references to anger in reporting						
Agent	66.85	1	30.06*	a: 3.38		a-b
Receiver	69.27	2	31.14*	b: .84	31.66	a-c
AgentxReceiver	19.58	2	8.80*	c: 1.07	p<.0001	
Explained	48.91	5	21.99*			
Residual	2.22	174				
Total	3.53	179				
Indirect references to anger in reporting						
Agent	70.42	1	29.87*	a: 3.62		a-b
Receiver	82.76	2	35.11*	b: .75	38.94	a-c
AgentxReceiver	35.47	2	15.05*	c: .89	p<.0001	
Explained	61.37	5	26.04*			
Residual	2.36	174				
Total	4.01	179				
Total references to anger in reporting						
Agent	280.22	1	44.86*	a: 7.00		a-b
Receiver	299.62	2	47.96*	b: 1.60	51.52	a-c
AgentxReceiver	109.87	2	17.59*	c: 1.97	p<.0001	
Explained	219.84	5	35.19*			
Residual	6.25	174				
Total	12.21	179				
References to anger in sentence completions						
Agent	171.14	1	72.33*	a: 3.89		a-b
Receiver	32.09	2	14.50*	b: 2.18	13.04	a-c
AgentxReceiver	5.10	2	2.30	c: 2.55		
Explained	49.10	5	22.19*			
Residual	2.21	174				
Total	3.52	179				

+ p<.05; * p<.001

Note: Column 5 gives the means of the three receiver groups (a. preparation by personal-subjective meaning, b. preparation by sentence completions, c. no preparation) in all cases when there was an agent.

TABLE 4 (continued)

Source of variation	Mean square	Df	F	Means	F	Significant group differences
Evaluation of anger after reporting						
Agent	27.11	1	60.00*	a: 2.73		a-b
Receiver	5.54	2	12.26*	b: 2.18	13.32	a-c
AgentxReceiver	1.62	2	3.59+	c: 1.98		
Explained	8.29	5				
Residual	.45	174				
Total	.67	179				
Difference in evaluation of anger before and after reporting						
Agent	27.11	1	47.47*	a: -1.62		a-b
Receiver	5.94	2	10.40*	b: -1.00	11.53	a-c
AgentxReceiver	1.98	2	3.47+	c: -.87	p<.0001	
Explained	8.59	5	15.04			
Residual	.57	174				
Total	.79	179				
Evaluation of gaiety after reporting						
Agent	62.02	1	167.33*	a: 1.27		
Receiver	.07	2	.18	b: 1.24	.12	
AgentxReceiver	.56	2	1.50	c: 1.22		n.s.
Explained	12.65	5	34.14*			
Residual	.37	174				
Total	.71	179				
Difference in evaluation of gaiety before and after reporting						
Agent	84.14	1	77.92*	a: .87		
Receiver	.21	2	.19	b: .95	.20	
AgentxReceiver	2.02	2	1.86	c: 1.00	n.s.	
Explained	17.85	5	16.40*			
Residual	1.09	174				
Total	1.56	179				
Evaluation of tension in reporting						
Agent	9.87	1	15.19*	a: 2.78		
Receiver	1.24	2	1.91	b: 2.78	.20	
AgentxReceiver	2.15	2	3.30+	c: 2.87	n.s.	
Explained	3.33	5	5.12*			
Residual	.65	174				
Total	.72	179				

*Notable
this is
direct*

was collapsed into two levels (i.e., agent present versus no agent present) since these were found to be the only levels of the agent that made a difference in regard to psi effects (table 3). The factor relating to the receiver was left unchanged. The results of the analyses of variance (table 4) correspond completely to those reported in table 2: the main effects for agent and receiver are highly significant for all four major variables and for the evaluation variables reflecting anger. (Again, in regard to gaiety and tension, only the agent main effect is significant). These results allow us to perform the necessary mean comparisons between the states of the receiver (see the left-hand side of table 4) These comparisons indicate clearly and unambiguously that for all variables reflecting anger, psi effects were significantly stronger in receivers who were prepared by personal-subjective meaning than in receivers who got preparation by sentence completions and in receivers who got no preparation at all. The two latter receiver groups do not differ from each other.

An examination of the means in table 4 reveals something about the magnitude of the effect. In the three major dependent variables (direct, indirect, and total references to anger) receivers prepared by personal-subjective meaning manifested psi-effects 3 to 4 times more frequently than the other receivers. In the rest of the anger variables (in sentence completions and evaluations) personal meaning enhanced the effect only 1 to 2 times over the level in the other receiver groups.

It is of interest to note that even within the receiver groups that got preparation through personal meaning, those receivers who succeeded to immerse themselves more completely in personal meaning attained better psi results than the rest (table 5). As may be remembered, immersion in personal meaning was judged on the basis of the meaning responses the subjects gave at the end of the stage of preparation. The difference between those subjects who immersed themselves more in personal meaning and those who immersed themselves less is about 2 to 3 times higher frequency in respect to direct and indirect references to anger in emotion reporting and 1 to 2 times higher frequency in regard to the other variables. This finding lends further support to the major finding about the facilitating effect of preparation through personal meaning on psi transmission.

The findings cited up to now show that there are main effects due to the agent (i.e., agent present versus no agent present) and to the

TABLE 5
Comparisons between receivers who immersed themselves
in personal meaning and those who did not

Variable		n	x	s.d.	t	p
Direct references to anger in reporting	Hi	19	5.00	2.24	-4.33	<.001
	Lo	26	2.19	2.08		
Indirect references to anger in reporting	Hi	19	5.53	2.65	-4.79	<.001
	Lo	26	2.23	1.97		
Total of references to anger in reporting	Hi	19	10.53	3.72	-6.00	<.001
	Lo	26	4.42	3.10		
References to anger in sentence completions	Hi	19	4.58	2.01	-1.97	.055
	Lo	26	3.38	2.00		
Difference in evaluation of anger before and after reporting	Hi	19	-1.53	1.02	-.57	n.s.
	Lo	26	-1.69	.93		

receiver (i.e., preparation through personal meaning versus preparation through sentences or no preparation). But what about possible interactions? Table 2 shows that in regard to direct, indirect and total number of references to anger in emotion reporting there are significant interactions between agent and receiver. A separate analysis of the interactions showed that the transmission of anger from the agent was clearly enhanced when receivers got prior preparation through personal meaning. The same conclusion is supported by the interactions cited in table 4. These indicate that the best combination for psi is when an agent is present and when the receiver got prior preparation through induction of personal meaning. Separate analyses were made in order to check further possible interactions or effects due to the particular state of the agent. For example, we subjected the data to 2x2 analyses of variance (i.e., Agent: preparation through meaning versus preparation through sentence completions plus no preparation; Receiver: preparation through meaning versus preparation through sentence completions plus no preparation). As expected, these analyses showed only a main effect due to the receiver (for all variables the effect was significant at the .001 level) but no main effect due to the agent and no significant interactions. Further comparisons through t-tests corroborated these findings.

Up to now we have presented only findings referring directly to the two hypotheses of the study. It may be of interest to present now findings about further related questions. First, what is the relation between direct and indirect references to anger? It may be remembered, that we considered the first as a manifestation of a transmission of feeling and the latter as a manifestation of a transmission of ideational contents concerning the affect. The product-moment correlation coefficient between these two variables across all subjects in the 9 groups when an agent was present is $r=.58$ ($n=135$, $p<.01$). Thus, there seems to be a fairly high positive correlation between them. Hence, if a receiver manifested one effect he or she also had a comparable tendency to manifest the other effect. This finding corresponds well with the view that emotion is an experiential-cognitive phenomenon. The two effects are probably merely two forms of manifesting the same phenomenon. A receiver may use the one or the other and more often both (number of receivers who used only direct references was 32, only indirect 28, both 66, and none 9).

Second, how did psi affect the extent of talking in emotion reporting and the other emotional states of the receivers as evaluated

by the experimenters: fear, and sadness? We examined the effects on the number of words spoken by the receivers since this could possibly be an indicator of change in general emotional arousal. For a similar reason we examined the effects on fear and sadness. If these moods change too then the effects of psi in this study cannot be viewed as specific to anger but rather as reflecting a shift in general emotional reactivity. A 3x4 analysis of variance on each of these three variables separately (in regard to fear and sadness both the level after emotion reporting and the difference in level between before and after emotion reporting were analyzed) showed that none of the main or interactive effects was significant. Thus, psi transmission of anger seems to be specific to anger, does not produce a general emotional effect manifested in fear or sadness, and does not affect amount of words spoken while reporting emotions.

Third, is the demonstrated psi effect dependent on any of the environmental or contextual variables assessed in this study? Thus, the relation of psi to the following variables was examined: Variables related to the receiver - gender of the receiver, the particular experimenter who stayed with the receiver, evaluations of tension, fear, sadness, gaiety, and anger prior to the experiment, evaluation of the depth of concentration, and evaluation of the seriousness of attitude toward the task of emotion reporting; variables related to the agent - gender of the agent, the particular experimenter who stayed with the agent, the particular psychodramatic scene (one out of the available five) that was enacted, evaluations of tension, fear, sadness, gaiety and anger prior to the experiment, evaluation (by experimenter) of the agent's seriousness of attitude toward the enactment of anger, evaluation (by experimenter) of the agent's seriousness of attitude toward feeling and transmitting anger, evaluation (by experimenter) of the degree to which the agent seemed actually to feel anger during the scene, evaluation (by agent) of the degree to which the agent actually felt anger, evaluation (by agent) of the degree to which the agent felt he or she succeeded to transmit the anger, evaluation (by experimenter) of the degree to which the agent made acts and movements denoting or expressing anger, and evaluation (by experimenter) of the degree to which the agent got immersed in the psychodramatic enactment. The relation of psi to these variables was examined by comparing psi manifestations in each of the dependent variables separately of groups defined by these variables. For example, when the examined variable was gender of the agent, psi effects of receivers who acted when there was a male agent were compared with psi effects of receivers who acted when there was a

female agent. In the case of evaluations, subjects that got low evaluations (i.e., scale ranks 1 or 2) were compared in psi with subjects that got high evaluations (i.e., scale ranks 3 or 4). When the differentiating variable was a response on the part of the subjects, the effects of those who responded in one way were compared with those who responded in the alternative way. Whenever the comparisons were between two groups, t-tests were used, whenever the comparisons were between more than two groups, one-way analyses of variance coupled with multiple range comparisons by the Scheffe procedure were used. None of the mentioned variables differentiated significantly between subjects who had more and those who had less psi manifestations in terms of any of the dependent variables.

DISCUSSION

The major results of the study concern the psi transmission of emotion and the facilitating effects of induction of personal-subjective meaning.

In regard to emotion, the study showed clearly that an emotional state as anger can be transmitted from an agent to a receiver. The effect is highly significant. It may take place between any two individuals located at a distance from each other, who do not know each other and are unaware of each other's presence or participation in the experiment. The psi transmission occurs between regular individuals under regular everyday conditions.

The transmission may become manifest in the receiver as an actual feeling, as an ideational content or most often in both of these forms. Since emotion is known to be the product of changes in arousal coupled with the proper cognitive elaboration (Schachter, 1964) the transmission of the cognitive contents alone may also be taken as evidence of transmission of the emotion, no less than the effects on the tension level of the receiver while in the stage of emotion reporting. Another manifestation of the transmission of the emotion as ideational contents is the effect on the receiver's associations in the sentence completion task following the emotion reporting. The transmission of the ideational contents of the emotion also accounts for the specificity of the emotional transmission. As was shown, the transmission affected anger and the related mood of gaiety but not fear, sadness or the general activity level as manifested by amount of

speaking. Our study showed that the experiential and conceptual manifestations of the transmitted emotion are correlated positively. The two forms of manifestation probably reflect merely differences in input elaboration that may depend on temporary situationally-determined conditions such as the availability of external cues stimulating one form or another, or on more personality-bound traits, such as cognitive style.

The transmission of emotion does not seem to depend on factors such as the depth and authenticity of the agent's feeling, the agent's intention to transmit, the actual feeling of anger on the part of the agent, the expression of anger by the agent through visible movements, the trigger or cause for the agent's assumed anger (i.e., which psychodramatic scene was enacted), the experimenter who was with the agent or with the receiver at that time, gender of all concerned, and the mood of the agent and of the receiver prior to the experiment. These findings indicate that the transmission is independent of a host of situational and contextual conditions. Hence, the transmission of emotions could be expected to occur under conditions of daily life. Perhaps it could account at least partly for the largely unexplained fluctuations in emotions and moods to which human beings are so often subjected.

In general terms the study showed that the transmission of emotions by psi (and perhaps the psi transmission of other messages too) depends on the state of the receiver, i.e., on whether the receiver is under the impact of personal-subjective meaning, but does not depend on anything that characterizes the agent apart from the basic fact of whether an agent is present or not. Hence, it seems at present more promising to concentrate attempts to enhance psi transmission by exploring facilitating conditions on the part of the receiver rather than on the part of the agent.

Two things remain unclear, however, concerning the transmission of emotions via psi: duration of the effect and its range. Our findings do not allow clear conclusions about the duration of the effect. Although the task of sentence completions performed by the receiver after emotion reporting was administered after completion of the psychodramatic enactment on the part of the agent and was clearly affected by the transmission, one cannot be sure that the agent has completely overcome by that time the anger that had been evoked. Quite on the contrary. Many agents still manifested some anger at a stage of the experiment that may have still affected the receivers. The same is

true in regard to the mood evaluations after the experiment. In addition, it is possible that the experimenter's evaluation may not have been free from the effects of listening to the receivers' emotion reporting in the previous stage. Finally, in regard to both the sentence completions and the mood evaluations, the possibility of a spread of effect could not be discounted. The anger evident in the behavior of the receivers in the stage after the actual transmission may have been due to a chain reaction concerning anger or to a conclusion on the part of receivers that their former anger still exists, or to a mere association. Thus, it remains to be investigated how long the effect of emotion transmission lasts or may become manifest.

Another theme for further inquiry is the range of the effect. Our findings relate to anger only. It remains to be shown whether the conclusions hold for other emotions, too.

The second major group of findings in this study focuses on the effects of personal-subjective meaning. It was shown that induction of personal-subjective meaning in the receivers enhances the manifestation of psi transmission up to 3 to 4 times more than under regular non-facilitating conditions. The magnitude of the enhancement is even greater (2 to 3 times more) in receivers who have immersed themselves particularly well in personal meaning as compared with those who immersed themselves less. Thus, when one compares the receivers who immersed themselves well in personal meaning with receivers who got no such preparation (compare tables 2 and 3) the differences in the means assume the magnitudes of 6 : 1 (i.e., direct references: $5.00 : .84 = 5.95 : 1$) or 7 : 1 (i.e., indirect references: $5.53 : .75 = 7.37 : 1$; or total of references: $10.53 : 1.60 = 6.58 : 1$).

The enhancing effect of personal-subjective meaning was observed in regard to the manifestations of feeling, of ideational contents, associations in the sentence completion task and evaluation of anger afterwards. It is noteworthy that only the induction of personal-subjective meaning in the receivers had this facilitating effect on psi but not an induction of personal-subjective meaning in the agents.

The finding about the facilitating role of personal-subjective meaning in regard to psi is of great importance because of the magnitude of the effect that the induction produces, its generality in

respect to subjects and measures of psi, and the fact that the induction can be easily produced under regular circumstances. But no less important is the theoretical contribution of the demonstration about the role of personal meaning. As noted in the introduction, personal meaning is a factor that explains a broad range of phenomena related to psi better than other suggested conceptualizations. Let us repeat: it seems to be the element common to such disparate phenomena as psychotherapeutic contexts, hypnosis, hypnagogic states, meditation, sensory deprivation, dreams, events of personal relevance, or tendency for nonverbal expression. Now, Honorton (1977), for example, suggested that psi functioning is enhanced by 'an internal attention state', that exists 'when the receiver is in a state of sensory relaxation and is minimally influenced by ordinary perception and proprioception' (ibid, p.466). Yet, this conceptualization does not fit psychotherapy, the effects of personal relevance or visualization tendencies. Others (Rao, 1977) have emphasized the overriding role of the personal or collective unconscious, of nonverbal channels, subliminal perception, contact with other realities, and so on. Each of these conceptualizations fits a part of the data. It seems to us, however, that dominance of personal-subjective meaning is common to all of these and partly underlies them.

Further, the concept of personal-subjective meaning may also account for several apparent inconsistencies in findings relating to the mentioned variables. One of these concerns the personal significance of the messages apprehended via psi. If personal relevance of the messages is so important, why is it that psi operates so often in regard to nonsense events of no personal concern whatsoever, or why does it function under experimental conditions in regard to personally irrelevant targets such as Zener cards? Another inconsistency concerns the instability of the effects exerted by conditions shown to be facilitating in regard to psi. Take hypnosis, for example. There are enough reports linking hypnosis and psi, but there are also many findings that show the hypnotic state not to be superior to other states in facilitating psi (Pratt, Rhine, Smith, Stuart and Greenbaum, 1966, pp. 266-273; Rao, 1966, pp. 37-41). A third inconsistency concerns the state of consciousness assumed to be favorable to psi. It is often claimed that concentration and reduced alertness favor psi (Honorton, 1977). If so, why is it that caffeine increases performance and drugs decrease it or do not at all affect it (Rao, 1966, pp. 33-34; Rhine, 1935, p.169)? Or again, if an altered state of consciousness (Tart, 1973) is such an important factor, it would be

difficult to account for the many spontaneous psi occurrences in daily life when receivers go about their business in an apparently normal state of consciousness.

These inconsistencies remain unresolved if it is assumed that the crucial factor is the mental state itself, as for example, hypnosis or dreaming. But if we identify the crucial element as dominance of personal-subjective meaning, the mental or circumstantial states assume the role of background contexts that merely create conditions favorable for the emergence of personal-subjective meaning. Thus, if the latter actually becomes dominant during that state, psi may be facilitated, if it does not, it will not. Further, dominance of personal-subjective meaning often involves concern over egocentrically relevant issues, but not only over issues of this kind. Hence, the frequently observed operation of psi in regard to themes or matters devoid of personal significance or the no less frequently observed absence of psi in regard to themes or matters endowed with great personal significance. The former would occur when personal-subjective meaning dominates, the latter, when it does not.

More generally, the concept of personal-subjective meaning helps to bridge the gap between the general expectation that psi is related to a special state of consciousness and the results of experiments that lend this expectation only partial support. On the basis of several studies we venture the hypothesis that personal-subjective meaning is a mode of cognition that differs from the lexical mode. The difference is mainly in the manner in which cognition operates under the dominance of personal-subjective meaning and not in the contents or issues with which it deals, its level of arousal, the level of its intellectual performance, or the characteristic degree of personal concern. The difference in manner of operation involves the use of exemplifying instances, situations and scenes including concretization, sensory means, imagery, nonverbal expression, sensitivity to the whole-character of gestalts and to the perceptually given, and the use of the metaphoric-symbolic mode including symbols, metaphors, sometimes illogical combinations, and receptivity to a reality beyond the common, interpersonally-shared. It is by no means accidental that many of these descriptive terms are reminiscent of terms often used in regard to psi (Rao, 1977).

In sum, the dominance of personal-subjective meaning produces an altered state of cognition but not necessarily of consciousness (Kreitler and Kreitler, 1983). The two partly overlap but are not to

be identified. Confusing the one with the other has certainly not helped to produce greater clarity in regard to factors facilitating psi.

Our above-stated assumption about the role of personal-subjective meaning raises two issues for further research. First, our study showed the facilitating effect of personal-subjective meaning on the psi transmission of an emotion. It is of crucial importance for our assumption to show that personal-subjective meaning facilitates also the transmission of non-emotional messages.

The second issue refers to the manner in which personal-subjective meaning operates so as to facilitate psi. Several possibilities present themselves. Personal-subjective meaning may directly enhance receptivity for psi transmission; or it may increase attention to psi cues; or it may decrease the chances that psi cues after having been noticed will be discarded as trivial or illogical; or it may increase the chances for letting psi cues intrude upon the scene of consciousness; or it may help to manifest psi cues by providing adequate means of expression for such contents. The list may not be exhaustive. It merely serves to illustrate the fact that if our suggestion is valid, it may not only help to resolve several inconsistencies in the broader field of psi research but will in its turn produce at least as many new questions to be resolved by further studies.

ABSTRACT

The goals of the study were to examine whether emotions can be transmitted via psi from agent to receiver, and whether a temporary dominance of personal-subjective meaning in receivers would enhance the effects of psi transmission. The subjects were regular 7th and 8th grade students, 135 agents and 180 receivers. The design was 4x3 (Four states of the agent: preparation by meaning, by a control task, no preparation, no agent; three states of the receiver: preparation by meaning, by a control task, no preparation). The transmitted emotion, anger, was evoked in the agents through psychodramatic enactment. The effects of psi were tested by means of references (direct references that reflect feeling and indirect ones that reflect ideational contents) to anger in introspective emotion reporting by the receivers, references to anger in sentence completions, and by means

of evaluations of tension, anger, and other moods. The main findings were that anger was transmitted from agent to receiver, and that induction of personal-subjective meaning in receivers greatly increased the effects of psi. Both effects were highly significant. Further, it was shown that the psi effect did not depend on the kind of preparation the agent got, the depth of feeling by the agent, prior moods of agent or receiver, etc.. The discussion is focused on the implications for emotion transmission in daily life, and on the role of personal-subjective meaning as a mode of cognition that enables integrating findings about psi and resolving various paradoxes about psi occurrence.

NOTES

1) The complete set of instructions could not be presented in the paper because of space limitations. It can be obtained from the authors upon request.

REFERENCES

Carpenter, J.C. 'Two related studies on mood and precognition run-score variance', *J.o.P.*, 1968, 32, 75-89.

Carpenter, J.C. 'Further study on a mood adjective check list and ESP run-score variance', *J.o.P.*, 1969, 33, 48-56.

Carpenter, J.C. 'The differential effect and hidden target differences consisting of erotic and neutral stimuli', *J. A.S.P.R.*, 1971, 65, 204-214.

Van de Castle, B. 'Sleep and dreams'. In B.B. Wolman (ed) 'Handbook of parapsychology', New York, Van Nostrand Reinhold, 1977.

Ebon, M. 'Parapsychological dream studies'. In G.E. von Grunebaum and R. Cailliois (eds) 'The dream and human societies', Berkeley and Los Angeles, University of California Press, 1966.

Ehrenwald, J. 'Telepathy and medical psychology', London, Allen and Unwin, 1947.

Eisenbud, J. 'Psi and psychoanalysis', New York, Grune and Stratton, 1970.

Goldstein, K. and Scheerer, M. 'Abstract and concrete behavior: An experimental study with special tests', Psychological Monographs, 1941, 53, No.2.

Honorton, C. 'Psi and internal attention states'. In B.B. Wolman (ed) 'Handbook of parapsychology', New York, Van Nostrand Reinhold, 1977.

Kreitler, S. 'Symbolschoepfung und Symbolerfassung: eine experimentalpsychologische Studie', Basel-Munich, Reinhardt, 1965.

Kreitler, H. and Kreitler, S. 'Does extrasensory perception affect psychological experiments?', J.o.P., 1977, 36, 1-45.

Kreitler, H. and Kreitler, S. 'Subliminal perception and extrasensory perception', J.o.P., 1973, 37, 163-168.

Kreitler, H. and Kreitler, S. 'ESP and cognition', J.o.P., 1974, 38, 267-285. (a)

Kreitler, H. and Kreitler, S. 'Optimization of experimental ESP results', J.o.P., 1974, 38, 383-392. (b)

Kreitler, H. and Kreitler, S. 'Cognitive orientation and behavior', New York, Springer Publishing Co, 1976.

Kreitler, H. and Kreitler, S. 'Art, meaning, and reality'. Invited Address American Psychological Association Convention, Montreal, 1980.

Kreitler, S. and Kreitler, H. 'Meaning and personality', New York, Academic Press, 1983. (in press)

McKellar, P. 'The mental images which precede sleep', J. A.S.P.R., 1959, 53, 23-27.

Moss, T. and Gengerelli, J.A. 'Telepathy and emotional stimuli: A controlled experiment', Journal of Abnormal Psychology, 1967, 72, 341-348.

Moss, T. and Gengerelli, J.A. 'ESP effects generated by affective states', J.o.P., 1968, 32, 90-100.

Myers, O.H. 'Images in the mind', J. A.S.P.R., 1957, 51, 62-73.

Nielsen, W. 'Mental states associated with success in precognition', J.o.P., 1956, 20, 96-109.

Nielsen, W. 'Relationships between precognition scoring level and mood', J.o.P., 1970, 34, 93-116.

Ogden, R.M. and Richards, I.A. 'The meaning of meaning', London, Routledge and Kegan Paul, 1949.

Osis, K. and Bokert, E. 'ESP and changed states of consciousness induced by meditation', J. A.S.P.R., 1971, 65, 17-65.

Osis, K. and Carlson, M.L. 'The ESP channel - open or closed?', J. A.S.P.R., 1972, 66, 310-320.

Piaget, J. 'Language and thought of the child', Routledge and Kegan Paul, 1948.

Pratt, J.G., Rhine, J.B., Smith, B.M., Stuart, C.E. and Greenwood, J.A. 'Extra-sensory perception after sixty years', Boston, Bruce Humphries Publishers, 1966.

Price, A.D. '"Subject"s control of imagery, "agent"s mood and position effects in a dual-target ESP experiment', J.o.P., 1972, 37, 298-322.

Rao, K.R. 'Experimental parapsychology: A review and interpretation', Springfield, Ill., Charles C. Thomas, 1966.

Rao, K.R. 'On the nature of psi', J.o.P., 1977, 41, 294-351.

Rhine, J.B. 'Extra-sensory perception', Boston, Humphries, 1935.

Rhine, L.E. 'Subjective forms of spontaneous psi experiences', J.o.P., 1953, 17, 77-114.

Rhine, L.E. 'Frequency of types of experience in spontaneous precognition', J.o.P., 1954, 18, 93-123.

Schachter, S. 'The interaction of cognitive and physiological determinants of emotional state'. In L.Berkowitz (ed) 'Advances in experimental social psychology', Vol.1, New York, Academic Press, 1964.

Scheffe, H. 'A method for judging all contrasts in the analysis of variance', *Biometrika*, 1953, 40, 87-104.

Schmeidler, G.R. 'Mood and attitude on a pretest as predictors of retest ESP performance', *J. A.S.P.R.*, 1971, 65, 324-335.

Servadio, E. 'The dynamics of so-called paranormal dreams'. In G.E. von Grunebaum and R Caillois (eds) 'The dream and human societies', Berkeley and Los Angeles, University of California Press, 1966.

Stanford, R.G. '"Associative activation of the unconscious" and "visualization" as methods for influencing the PK target', *J. A.S.P.R.*, 1969, 63, 338-351.

Stevenson, I. 'Telepathic impressions: A review and report of thirty-five new cases', *Proceedings A.S.P.R.*, Vol. 29, 1970.

Tart, C. 'States of consciousness and state-specific sciences'. In R. Ornstein (ed) 'The nature of human consciousness', San Francisco, W.H. Freeman, 1973.

Tyrrell, G.N.M. 'The modus operandi of paranormal cognition', *Proceedings S.P.R.*, 1947, 48, 65-120.

Ullman, M. 'An experimental approach to dreams and telepathy: Methodology and preliminary findings', *Archives of General Psychiatry*, 1966, 14, 605-613.

Werner, H. and Kaplan, B. 'Symbol formation', New York, Wiley, 1963.

Winer, B. 'Statistical principles in experimental design', New York, McGraw-Hill, 1962.

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PRECOGNITION TESTING WITH FREE-RESPONSE TECHNIQUES
IN THE GANZFELD AND THE DREAM STATE

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Whilst there have been many GESP and clairvoyance studies which have used free-response test techniques, there have been relatively few precognitive studies of this type. In the domain of application of the "ESP-conducive conditions" (Braud, 1978) - i.e., hypnosis, progressive muscular relaxation, ganzfeld isolation, the dream state, and so on - there have been very few precognitive studies.

Perhaps the best known of these latter studies are the two run with Malcolm Bessent as the subject at Maimonides, looking at dream precognition (Krippner, Ullman and Honorton, 1971; Krippner, Honorton and Ullman, 1972). Each of these studies comprised eight experimental nights on which Bessent was asked to dream about a future event, based on the content of an art print. The probability of the outside judges matching his dream material to the correct target was 1 in 8 since seven dummy controls were included together with the genuine target for their judgements. In each of the series Bessent scored five direct hits, five times chance expectation, an obviously highly significant performance.

Dunne, Warnock, and Bisaha (1977) reported significant ($P < .04$) psi-hitting from a single subject tested over six precognitive ganzfeld sessions. Contemporaneous GESP testing also showed significant psi-hitting ($P < .01$).

Rogo (1977) gave each of four subjects five test sessions in a ganzfeld free-response precognition task. Overall scoring was insignificantly negative, but Rogo noted post-hoc that the sum of the individual CR-square values was significant, suggesting high variance. Although post-hoc, the magnitude of the effect is quite large here.

From these few studies one may infer that the precognitive paradigm is a variable one, but that it might only be expected to yield significant overall effects if selected subjects are used, since the two single-subject studies were successful and the four-subject study was not.

In this paper we report the results from a pilot study using precognitive free-response testing with a procedure advocated by one of us previously as a simplifying one. A body of data, much of it highly statistically dubious and almost none of it repeatable, suggests that incidental participants (e.g., randomizers, data checkers) can influence the outcome(s) of psi experiments in addition to experimental personnel (i.e., those people who actually encounter the subjects). If this is the case then there is a simple expedient for dealing with these effects: get rid of the extraneous personnel. However, this still leaves the experimenter. Since (s)he is indispensable (computerized testing notwithstanding) we need to get rid of the subject(s). This sounds extremely bizarre but is quite feasible if one makes the experimenter the subject. It is perfectly possible for one individual to play all the roles in an experiment; subject, experimenter, randomizer, etc., with appropriate design of experiment. However it is desirable for political reasons to conduct such experiments in parallel, i.e. with two experimenters each carrying out their own experiment under similar conditions. This is the approach used in the present study.

This pilot study was split into two halves, one half conducted by Sargent and one half conducted by Harley. The methods used will be discussed separately and the results discussed together since data pooling was pre-planned.

METHOD

SUB-SERIES 1: GANZFELD PRECOGNITION (SARGENT)

A total of 24 test sessions were carried out. The first 12 were run in 1979 but the study was temporarily shelved because of other and more pressing experimental commitments. The remaining 12 sessions were conducted early in 1980.

The procedure for each session was as follows. I first set myself up in an audio-visual ganzfeld in a quiet and secluded experimental studio. A visual ganzfeld was created using a 60-watt red light source diffused through halved ping-pong balls sellotaped over the eyes, using cotton wool to seal off the edges of the visual field. White noise from a white noise generator (note 1) was relayed to headphones through an Armstrong 521 or JVC JAS8 amplifier. I relaxed on a comfortable mattress and after putting on the headphones said "Session number X starting now". The entire session was recorded using an Akai tape deck.

I determined the session duration. During the session I reported my feelings, impressions, imagery, etc., and when I wished to stop said "Stop now", and removed the ping-pong balls and headphones. This "optional stopping" procedure (a new twist for an old phrase!) had been used before with success (e.g., Ashton, Dear, Harley, and Sargent, 1981) and is particularly useful with experienced subjects who sometimes tend to get bored with fixed session durations of 30 or 35 minutes.

After the session I transcribed the taped material on to standard transcript forms and, using a stopwatch, recorded the entire session duration to the nearest second. Next I randomly generated a set of four diverse pictures to act as the "judging pool" for the session (note 2). This selection was made randomly using the Rand Corporation (1955) tables, with the proviso that no set should be used for two successive sessions.

I then judged the correspondences between the session material and the four pictures in the judging set. The four pictures were ranked first through fourth for order of correspondence with the transcript material (first rank for best correspondence, fourth rank for worst).

Finally the four pictures were also given ratings between 0 (absolutely no correspondence with the transcript) and 99 (perfect correspondence). The judging outcomes were recorded on standard forms.

When the judging had been completed the target for the session was finally generated. I used the Rand Corporation (1955) tables to randomly cut a deck of 20 sealed envelopes and then to select one envelope from that deck. Each of these envelopes was opaque and contained a slip of paper, flanked by two sheets of stiff card, which read "A", "B", "C", or "D". In the deck of 20 there were five envelopes which contained A-slips, five which contained B-slips, and so on. The original order of envelopes in the deck was randomized by someone otherwise unconnected with the experiment (note 3). When the original deck was prepared a number of replacements was also prepared so that after each trial a replacement could be effected to maintain the closed deck using an envelope as aged as the original so that no cues could be seen (use of the Rand tables rather precludes this problem in any case). After the envelope was withdrawn from the deck it was opened and the identity noted. A replacement was inserted into the deck at a random place determined by the Rand tables and the deck thoroughly shuffled. The target identity was recorded on the record sheet.

Finally, before each session I completed a pre-session questionnaire and, immediately after each session (and before transcribing the session material) I completed a post-session questionnaire. These data were collected for purely exploratory reasons and the questionnaires are not reproduced here. They were the same as those used in experiment V reported previously (Sargent, 1980).

SUB-SERIES II: DREAM PRECOGNITION (HARLEY)

A total of 20 test sessions was carried out, commencing just before the second set of 12 sessions in the ganzfeld subseries and finishing just afterwards. I used the dream state rather than the ganzfeld as an ESP-conducive procedure because I am a fairly fluent dream recaller and at the time I was finding that the ganzfeld was not particularly successful at altering perception, imagery, etc..

For most of the 20 experimental nights I decided in advance to attempt to dream and recall my dreaming although sometimes I dreamed

spontaneously and recalled the dream material clearly. On these experimental nights I wrote down my dreams in brief form during the early hours of the morning if I happened to wake, and then completed a written account in the morning when I woke up. I then completed a questionnaire concerning the nature of the dream, the form and content of the recalled dream(s). This was for exploratory purposes and the questionnaire is not reproduced here for space reasons (note 4).

On coming in to the laboratory later in the day I first randomly chose a set of four pictures from the total pool used in the ganzfeld subseries and judged correspondences between the dream material and the four pictures in the set. If there was more than one separate dream on any given night, all dreams were scored against the pictures: the unit of analysis was one night's recalled dreams in total. I ranked the four pictures first through fourth for order of correspondence with the dream material (first rank for best correspondence, fourth rank for poorest) and also rated them on a 0-99 point scale as in the ganzfeld subseries.

When judging had been completed I first took the dream pictures and the dream transcript to Sargent's office and left them there for him. In this subseries he acted as an independent judge of the data. Only my scorings were to be used in the final data analysis, but we were interested in examining judging reliability and comparing the scorings. When this had been done I randomly generated a target for the session using exactly the same procedure as was used by Sargent in the ganzfeld subseries. However, I utilized my own deck of target-specifying envelopes.

DATA ANALYSES AND PREDICTIONS

The first planned analysis was of overall scoring level. This analysis was made on the pooled data from the two subseries. Both direct hit rate and the sum of ranks measure (Solfvin, Kelly, and Burdick, 1978) were used in analysis. One of us has subsequently argued (Sargent, 1980) that the latter is preferable on logical grounds if one is talking about psi-optimization but at the time this study was planned neither analysis was designated as the primary one.

No prediction was made about the outcome(s) of these analyses. Obviously we hoped for a high scoring rate similar to that observed in

previous studies but since this was a first precognitive study, and others had only been successful with selected subjects, we did not feel that high scoring could be predicted.

The first prediction made was that the scorings by Sargent and Harley for the dream data would show significant agreement. From the rating data for each session a Z-score was computed for each judge thus:

$$Z = \frac{(\text{rating for target}) - (\text{mean of all ratings})}{(\text{standard deviation of all ratings})}$$

It was predicted that Z-scores from Harley's and Sargent's judgements would show significant agreement. This is not a test of any psi effect, of course, merely an examination of the reliability of judgements.

The second prediction was made for the ganzfeld data alone. It was predicted that Z-scores from these sessions should correlate positively and significantly with session duration. This follows from the results of an experiment previously reported (Ashton, Dear, Harley and Sargent, 1981).

The third prediction was also made for the ganzfeld data alone. From the findings from a previous study (Sargent, 1980, Experiment V) it was predicted that scoring should be superior later in the ganzfeld session rather than earlier. This was examined by looking at the "judging matrix" generated in judging each session. In judging data each of us took each response and scored it against the four pictures in the judging pool using an arbitrary but consistent scoring scheme. An analysis can be effected by dividing the matrix into two halves and totalling the number of points scored by the correct picture (target) and the three non-targets. The former can be divided by the number of responses to yield two "signal scores" S1 and S2 (from first and second halves of the matrix) and the latter can be summed across the three pictures, divided by three, and again by the number of responses to yield two analogous scores N1 and N2 (N for "noise" here). The hypothesis of superior scoring later in the session can be examined by scrutinizing the mean value of the parameter $((S2-N2) - (S1-N1))$. The mean value of this parameter should, by chance alone, be zero. If there is superior scoring later in the session $(S2-N2)$ should be higher than $(S1-N1)$ and so the mean value of the difference should be positive. This may be tested by using a single-mean t-test for the

difference scores, which is functionally identical to using a correlated-means t-test on the (S-N) pairs.

Data from the dream subseries was also tested for time change although here no predictions were made about the outcomes of the analyses.

The material from the questionnaires was collected for purely exploratory reasons and will not be discussed here.

All analyses reported in the following section were pre-planned unless otherwise reported and all P-values are two-tailed unless otherwise stated.

RESULTS

Overall scoring

The 24 sessions from the ganzfeld subseries yielded the following rank distribution: 1st,10; 2nd,4; 3rd,5; 4th,5. The 20 sessions from the dream subseries yielded the following distribution: 1st,8; 2nd,3; 3rd,2; 4th,7.

The pooled distribution thus yielded 18 direct hits (41%), seven second ranks, seven third ranks and 12 direct misses. The ranks sum is 101, an insignificant deviation of -9 from MCE. However, the 18 hits is a significantly greater number than the 11 expected by chance. The two-tailed P for obtaining a deviation of ≥ 7 hits here is .022. Hence there was a significant excess of direct hits in the present study. Also (but this is not independently significant) the fact that there are 30 direct hits and direct misses pooled, as against 22 to be expected by chance, suggests significantly high variance ($Z=2.26$, $P=.024$). This effect however is largely due to the dream data where 75% of the sessions were direct hits or direct misses (exact binomial $P=.041$).

Judge reliability

For the dream data the judgements made by Harley and Sargent showed a significant positive agreement or $r_s = +0.78$ ($t = 5.29$, $df = 18$, $P < .0001$, one-tailed). There is an excellent agreement here.

Sargent's scorings were not as useful as Harley's: Sargent scored seven direct hits in the 20 trials against Harley's eight and only 13 direct hits and direct misses combined as against 15 scored by Harley. Thus if Sargent's judgements had been used neither the direct hit rate nor the variance effect would have been significant.

The two judges also generated $((S2-N2)-(S1-N1))$ scores but these intercorrelated only at $+0.33$, which is insignificant and disappointing. However later work has shown much higher agreements for scoring this parameter.

Temporal trends in scoring: 1

Post-hoc it was noted that both subjects tended to improve in scoring during the individual subseries. Sargent scored three hits in his first 12 trials (chance) but seven in the next 12; Harley scored three hits in his first 10 trials and five in the next 10. Thus the first 22 trials, with six hits ($MCE = 5.5$) were pure chance whilst the last 22 showed 12 hits, a highly significant (exact $P = .0029$) scoring rate of 54% hits.

Temporal trends in scoring: 2

In the Sargent ganzfeld data overall there was an utterly insignificant duration of session/ESP Z-score correlation of $+0.05$. However, for the first 12 sessions, which showed slightly below-chance scoring (sum of ranks of 31), the duration/ESP correlation was -0.62 which is significantly negative ($t = 2.49$, $df = 10$, $P < .05$); for the last 12 sessions, which showed significantly above-chance scoring (seven direct hits and a sum of ranks of 22) the correlation was $+0.64$ which is significantly positive ($t = 2.64$, $df = 10$, $P < .025$). Since these are Spearman correlations there is no direct way of examining the

significance of the difference between them.

The remaining prediction to be tested against the data was supported. The mean value of $((S2-N2)-(S1-N1))$ was significantly greater than zero in the ganzfeld data ($t=1.97$, $df=.23$, $P<.05$, one-tailed).

The same analysis was applied to the Harley dream data where it was found (for Harley's judgements, but not for Sargent's) that the mean value of $((S2-N2)-(S1-N1))$ was significantly negative ($t=2.13$, $df=19$, $P<.05$). The ganzfeld incline and the dream decline do not contradict each other for reasons to be discussed shortly.

DISCUSSION

The overall hit rate of 41% was significantly above the chance mean of 25% ($P=.022$). However there were also 12 direct misses in the data. This suggests high variance since the number of hits and misses was significantly greater than the 50% to be expected by chance. However the direct hitting is probably more reliable than the high variance effect (which is not independent of it) for one reason. The high variance is almost entirely due to the Harley dream data which has independently significant high variance. The direct hit significance, however, is just as clearly dependent upon very similar direct hit scoring rates by the two subjects (42% by Sargent and 40% by Harley). Indeed these two hit rates are so similar that if one took one of them as the true empirical hit probability one would be able to predict perfectly the scoring rate of the other subject.

So it may be concluded that the altered-states procedures are capable of being applied to precognitive testing successfully. We must first point out that the term "precognition" is not being used here to denote any process or "type" of ESP. We used the term "precognition" only in an operational sense to denote an experimental procedure in which a target is generated only after a response is made. The second point that needs to be made is that we might be regarded as selected subjects and thus the usefulness of the altered state procedures in optimizing precognitive performance with unselected subjects remains to be demonstrated. This seems correct to us and we are currently running a study with unselected subjects as Phase 2b of this part of our research program (see below).

The strong positive agreement between the two judges in scoring the Harley data shows that subjectivity of judging need not be a major problem in dealing with free-response data. We have found comparable agreements in other data. For example, in the 20-session dream clairvoyance study run by Harley, Harley and Sargent agreed at +0.73 for overall scoring. In a ganzfeld study with 16 experienced subjects, their Z-scores correlated at +0.72 with Harley's scorings (Sargent, Bartlet, and Moss, 1981). In a 48-session dream study, Sargent and another judge (Gerald Matthews) agreed at +.70 in their scorings. Good agreements can be obtained but they do require judges trained in techniques of judging as we train all our judges.

The correlational analyses in the Sargent ganzfeld data for effects of session duration showed no significant overall effect, but two independently significant ones: a significant negative duration/ESP correlation when the overall scoring was negative and a significant positive duration/ESP correlation when the scoring was significantly positive. Since both correlations are significant one can hardly ignore them on the grounds that they are post-hoc. The obvious speculation would be that magnitude of deviation from chance correlates significantly with duration: with long duration ESP effects are much more likely to occur but their direction is uncertain. This speculation would be in line with the view expressed by Palmer (Palmer, Bogart, Jones, and Tart, 1977) that the "altered state" induced by the ganzfeld (which will be stronger with longer duration) influences magnitude of deviation from chance whilst other factors influence the direction of deviation.

Finally there are the analyses of change in scoring within the session. The Sargent ganzfeld data showed significant within-session inclines. This was predicted. The Harley data showed (unpredicted) significant within-night declines. These findings do not contradict each other. The reason for expecting the ganzfeld incline is simple. Honorton's model of ganzfeld psi-optimization states that sensory habituation is one major cause of the optimization. Since this habituation is time-progressive, ergo so must ESP scoring be. The logic is simple; whilst Honorton has not derived this prediction himself others have (Stanford, 1979; Sargent, 1980). It is quite clear that this logic applies to the ganzfeld procedure but not to the dream procedure. In the Harley data the effect can be seen to arise because dreams which Harley woke early (between 3 and 5) and recalled tended to fit the target particularly well. Thus the decline may be due to any one or more of a number of factors which have no direct analog in

the ganzfeld paradigm.

In conclusion, this study was a successful first approach to exploring precognition in altered-state settings. Three psi hypotheses were tested against the data, of which two (overall hitting and temporal incline in the ganzfeld data) were supported whilst one was not (duration/ESP correlation in the ganzfeld data). Inter-judge reliability for scoring the dream data was excellent. The next stage in this research will use two experimental approaches. In Phase 2a of the study the present authors will replicate the present study with an increased number of trials. In phase 2b a study with unselected subjects, using the ganzfeld procedure, will be undertaken. Data from these studies will hopefully further our understanding of precognitive performance in the altered state procedures and of whether unselected subjects can show significant effects in such settings.

ACKNOWLEDGEMENTS

Carl Sargent gratefully acknowledges financial support from the S.P.R. at the time this study was conducted.

NOTES

Note 1: It may be of interest to readers to know that white noise cannot be taped efficiently even with excellent professional tape-recorders. There is no substitute for a true white noise source.

Note 2: The total number of sets available was not constant since we always build up our pool. The minimum number of sets in the pool was 25 and the maximum 47.

Note 3: This action does represent the only intrusion into the design by anyone other than the subject/experimenter. This could be rectified by use of appropriate random number generating apparatus, which we did not possess when this study was run.

Note 4: A copy of the questionnaire can be obtained from me upon request.

ABSTRACT

As a first exploration of precognition in "altered states" the authors acted as single subjects in parallel free-response studies, Sargent completing 24 ganzfeld sessions and Harley completing 20 sessions of a precognitive dreaming study. Transcript material was scored against four alternative pictures and rank judgements and ratings recorded. Targets were only generated after judgements had been made.

The 44 sessions produced 18 direct hits, a significant ($P=.022$) scoring rate of 41%. Sargent scored 42% hits, Harley scored 40%. There were also 12 direct misses in the data, suggestive of high variance. The latter effect was independently significant in the Harley dream data ($P=.041$).

Sargent's scorings of the dream data, undertaken for comparative purposes, correlated at +0.78 with Harley's ($P<.001$), showing that subjectivity in judging need not be a major problem with free-response data.

Contrary to prediction, the duration of the ganzfeld sessions did not correlate positively with scoring. However in the first half of the data (scoring below chance) the correlation was significantly negative whilst in the second half of the data (scoring significantly high) the correlation was significantly positive.

As predicted the ganzfeld data showed significantly superior scoring in the later stages of a test session than in the earlier stages using a judging matrix for analysis. Similar analysis of Harley's data showed a significant decline effect, reflective of the fact that the dreams he recalled when awaking early in the morning (3-5 AM) tended to match the target particularly well.

The implications of the findings, with particular reference to further research now in progress, are discussed.

REFERENCES

- Ashton, H.T., Dear, P.R., Harley, T.A., and Sargent, C.L. 'A four-subject study of psi in the ganzfeld', J. S.P.R., 51, 1981, 12-21.
- Braud, W.G. 'Psi conducive conditions: Explorations and interpretations'. In: B. Shapin and L. Coly (eds), 'Psi and States of Awareness', New York, Parapsychology Foundation, 1978, 1-41.
- Dunne, B.J., Warnock, E. and Bisaha, J.P. 'Ganzfeld techniques with independent rating for measuring GESP and precognition'. In J.D. Morris, W.G. Roll, and R.L. Morris (eds), 'Research in Parapsychology 1976', Metuchen, N.J., Scarecrow Press, 1977, 41-43.
- Honorton, C. 'Psi and Internal Attention States'. In, B.B. Wolman (ed), 'Handbook of Parapsychology', New York, Van Nostrand Reinhold, 1977, 435-472.
- Krippner, S., Honorton, C. and Ullman, M. 'A second precognitive dream study with Malcolm Bessent', J. A.S.P.R., 66, 1972, 269-279.
- Krippner, S., Ullman, M. and Honorton, C. 'A precognitive dream study with a single subject', J. A.S.P.R., 65, 1971, 192-203.
- Rand Corporation 'One Million Random Digits', Glencoe, Ill., Free Press, 1955.
- Rogo, D.S. 'A preliminary study of precognition in the ganzfeld', E.J.P., 1, no. 3, 1977, 60-67.
- Sargent, C.L. 'Exploring psi in the ganzfeld'. Parapsychological Monographs no. 17. New York; Parapsychology Foundation, 1980.

Sargent, C.L., Bartlet, H.J. and Moss, S.P. 'Response structure and temporal incline in ganzfeld GESP performance'. Paper presented to 24th Annual Convention of Parapsychological Association, Syracuse, August 1981.

Solfvin, G., Kelly, E.F., and Burdick, D.S. 'Some new methods of analysis for preferential-ranking data', J. A.S.P.R., 72, 1978, 93-109.

Stanford, R.G. 'The influence of auditory ganzfeld characteristics upon free-response GESP performance', J. A.S.P.R., 73, 1979, 253-272.

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PARAPSYCHOLOGY FROM THE POINT OF VIEW OF MODERN PHYSICS

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INTRODUCTION

Modern physics is based on the recognition that 1) the description of physical phenomena principally depends upon the nature and state of measuring instruments and observers (note 1) interpreting the results of measurements; 2) the set of all possible measuring instruments can be divided into reciprocally complementary sub-sets such that it is impossible to simultaneously perform measurements by instruments of different sub-sets; 3) for instruments of each of these sub-sets their own reality exists; there is no reality independent of the kind of measuring instruments (see, for example, Albert Einstein, 1905, 1953; Peter Gabriel Bergmann, 1942; Nils Bohr, 1928, 1934, 1935; P.A.M. Dirac, 1958; W. Heitler, 1956; Werner Heisenberg, 1930; Leonard J. Schiff, 1955; A. Messiah, 1961). For example, an electron has a co-ordinate but not the corresponding linear momentum for one class of measurements; and it has linear momentum but not the corresponding co-ordinate for the other class of measurements.

To consider measurements performed on human beings with the purpose of providing information on their states, thinking processes, etc., it must be taken into account that a man A can himself measure his state and thinking processes and interpret the results of these measurements, i.e. he is at the same time a measuring instrument and an observer of himself, which is impossible in the world of elementary

particles. An elementary particle is too primitive, and so it can only be an object of measurements.

Thus, the set of measuring instruments (together with observers) necessary to measure the state of a man A consists of the man A himself and all other men B_i . A man B_i can measure time, all co-ordinates, configurations a.o. geometrical and mechanical parameters of A, as well as some physical, chemical and physiological quantities of A. He can also obtain some information on the thinking processes of A from speaking with A, from psychological tests, from EEG, etc., but such information is very limited and not exact. Maximum information on his thinking processes can be provided by A himself, if he is concentrating on them. But at the same time, he deprives himself of information about the time, co-ordinates, his configurations and other geometrical, physical, chemical and physiological quantities, which are measurable by all B_i . A similar situation occurs when A is sleeping (and dreaming) or is in a hypnotic state. It would be correct to label 'meditation' all such states of A in which he is able to obtain maximum information about his own thinking processes. We see that in our case, as well as in quantum mechanics, all measuring instruments are divided into two incompatible classes: 1) the man A himself, and 2) all other men B_i . In quantum mechanics, for example, these are 1) instruments measuring co-ordinates and 2) those measuring the corresponding linear momentum. These two classes (or sub-sets) of measuring instruments are reciprocally complementary. This principle of complementarity in quantum mechanics was formulated by Nils Bohr. In his lectures (Nils Bohr, 1933, 1937), Bohr mentioned arguments to the effect that this principle and the uncertainty principle of Heisenberg are also valid for biological systems. An uncertainty principle represents relations between errors of measurements of the same object, performed simultaneously by two reciprocally complementary measuring instruments: for example, measurements of the co-ordinate and the corresponding linear momentum of an electron (in quantum mechanics) or measurements of the state of a man A performed simultaneously by A and by B_i (in our case).

If A is in a state of meditation, his reality does not contain his localization in space and time, but it contains, for example, his dreams and other things produced by his brain. It is not less real than for all B_i the localization of A in space and time and other parameters measurable by B_i are real. This means that for A his reality exists and for all B_i another reality exists. Any discussion about which reality is more real is nonsense, like the discussion of

which reality an electron has - a co-ordinate, or the corresponding component of the linear momentum. If being in a meditation state A 'sees' that he is in another place (maybe in another country) in an epoch other than the one a B_i has determined from his measurements, it is still the reality for A; the data (the reality) of B_i do not exist for him. This means that the clairvoyance, the 'memory' of events of hundreds of years ago, meetings with persons who are in other towns or even other countries, etc., represents consequences of uncertainty and complementarity for human beings.

In reality the situation is more complicated than described above. If man A concentrates his attention on a certain subject, he cannot provide information on other subjects. If he pays attention to two or more subjects, he provides incomplete information about each of them. I.e. there is complementarity and uncertainty for different groups of thinking-process measurements.

It is well known that the existence of an uncertainty principle (or principles) for micro-systems leads to the description on the ground of algebra of non-commuting operators acting on elements of space (wave functions), i.e. to the quantum theoretical description. Based on the considerations described above, we can suppose that also for human beings a kind of quantum theoretical description is valid. Consider the consequences of it.

HUMAN WAVES OF DE BROGLIE

The wave function of A determined from quantities measurable by the man A (i.e. in A-representation) would be delocalized in space and time if A is in a meditation state. The amplitude of probability for A to be in another space and another time (than those measured by B_i) would therefore be non-zero. Let there be two men, A_1 and A_2 , in meditation states. The set of variables in this case corresponds to the set of quantities measurable by A_1 or by A_2 - in contrast to the case of two electrons, in which the set of variables includes the linear momenta and spins of both particles; the difference is a result of the fact that the state of these electrons is measured by a 'third body', but the measuring instrument (and observer) for the $A_1 + A_2$ system is either A_1 or A_2 . The delocalization of the wave functions of A_1 and A_2 can lead to their overlapping in a certain region in space and time. In this region there can be an interaction of the wave

functions of A_1 and A_2 as between two quantum fields. If this interaction exists, the thinking processes of A_1 can influence the thinking processes of A_2 and vice-versa. For example, there can arise in the brain of A_2 thoughts existing in the brain of A_1 . This is telepathy. On the basis of our considerations we can state that: telepathy is realized by (human) waves of De Broglie, represented by the above-described wave functions. Thus, telepathy is not connected with any kind of radiation, and the velocity of telepathic propagation is not limited by the velocity of light and can even be infinite; also, screens that shield electromagnetic waves are not effective as protection against the telepathic influence of one person on another. These two consequences of our theory can be used to check it: 1) telepathy between human beings separated by cosmic distances can permit us to determine the velocity of telepathic propagation and to show whether it is limited or not by the velocity of light; 2) experiments with screens can show whether or not they protect from telepathic influence.

Using the analogy with quantum mechanics, we can predict that such an interaction between the A_1 and A_2 human waves of De Broglie (responsible for telepathy) would be a strong one, provided that there is some similarity between the A_1 and A_2 'thinking apparatuses'. Just as a photon emitted by an atom has the highest probability of being absorbed by another atom if its electronic structure (distances between electronic levels) is the same as that of the first atom, telepathic propagation has a greater probability of being 'absorbed' by a similar thinking apparatus. The notion 'structure of thinking apparatus' does not refer to the microscopic physico-chemical structure of the brain, but to the memory and its content, to thoughts circulating in the brain, and to the typical reactions to different kinds of information obtained by A or thoughts arising in his brain. The other name for this 'structure of thinking apparatus' is 'personality'. Telepathy is more probable between individuals with similar personalities, or more precisely, between similar personalities, because one human body can contain different personalities (Pascual Jordan, 1960) having different amplitudes of probability. So a telepathic propagation can be received not only by the dominant (i.e. the one with the biggest amplitude of probability) personality of A_2 , but also by one of his personalities having a small amplitude of probability, provided that it likes the dominating personality of A_2 . Individuals (like Wolf Messing in the USSR) who are able to 'read thoughts' of different persons have comparable probability amplitudes of their different personalities; possibly none

of the personalities of such a man is the dominant one. In everyday life, such a man would be very strange and probably considered mentally ill by contemporary psychiatrists who cannot reconcile themselves to the fact that somebody may have two personalities ('split personality' is for them a symptom of serious mental disease), to say nothing of cases in which he has more than two personalities. For such a man, 'voices' which he hears can be produced by the interference of his different personalities and be a symptom of mental disease, if he is not able to establish that this is their source, and not the outside world. But 'voices' can also be telepathic propagations from different sources, and his talking about them cannot be considered a symptom of mental disease, even if his stories seem very strange to us.

Just as a non-stationary perturbation of a quantum mechanical system can provoke its transitions from one state to others, the telepathic influence on a man A_2 , depending on time (non-stationary), can provoke his transition from an initial state with one dominating personality to a final state with another dominating personality, i.e. a change of his personality. This means brainwashing can be accomplished by using telepathy.

THOUGHTS

Consider a set $a = \{a_i\}$ of elements of thoughts a_i . There is no condition limiting the set of all possible a_i ; we also are not interested in their nature. Consider a subset $b_r = \{a_{rj}\}$ of the set a .

Definition: We call a subset $b_r = \{a_{rj}\}$ a thought if and only if: 1) b_r is ordered in some way, and 2) between elements a_{rj} , or between different subsets $c_{qr} = \{a_{qrj}\} \subset b_r$, at least one relation exists besides the above mentioned relation of order (notice that this is not obligatory between each two elements or sub-sets).

Different sub-sets b_r can also be elements of thoughts of a higher rank. If a_{rj} are elements of thoughts b_r , and b_{qr} are elements of thoughts c_q , we shall call c_q thoughts of higher rank than b_r and shall write it so:

$$R(b_r) < R(c_q) \quad (1)$$

To introduce thoughts of higher rank, we can use a thought (or thoughts) of lower rank as relations demanded by the definition of 'thought'.

SPACE OF STATES

Define a topology τ in the set $\mathfrak{A}$, defined as the set of all possible thoughts. Define also an empty set 0 . It is evident that the sum or the crossing of any number of thoughts is also a set of thoughts $\mathfrak{A}$ or 0 . In other words, the sum and the crossing is contained in τ . Thus, all conditions that are contained in the definition of the topology of a set are accomplished for τ (G. Kelley, 1955; P.S. Alexandrov, 1948; A.N. Kolmogorov & S.V. Fomin, 1957, 1963, 1968; J. Dugunji, 1976).

To consider a system of two men A and B, the topological set $\mathfrak{A}_{A+B} = \mathfrak{A}_A \vee \mathfrak{A}_B$ must be used, i.e. the sum of topological spaces of A and of B. The intersection of any sub-set of $\mathfrak{A}_A$ with any sub-set of $\mathfrak{A}_B$ is empty. The same thought, for example, "Today is a holiday" existing in the brains of both A and B is considered as two different thoughts (Today is a holiday) and (Today is a holiday). The topology of $\mathfrak{A}_{A+B}$ is the sum of the topologies of $\mathfrak{A}_A$ and $\mathfrak{A}_B$, and therefore $\mathfrak{A}_{A+B}$ is a topological space.

A set $\ddot{b}$ of A's thoughts, which can be measured simultaneously, is called a complete set of thoughts if and only if each of the other thoughts of A, which also can be measured by A simultaneously with all the thoughts contained in $\ddot{b}$, can be represented as a function of the thoughts contained in $\ddot{b}$. We use the word 'function' in its general set-theoretical meaning. If the last condition is prevented from being satisfied by the elimination from $\ddot{b}$ of one of its elements, then $\ddot{b}$ is called the minimal complete set. In the following text the word 'minimal' is omitted and it is supposed that only minimal complete sets are being considered.

A topological space cannot be used for the representation of states of the system - man A - because it contains no quantitative characteristic which can be interpreted as a probability. We represent each state of A by a point in a certain metric space. This point should be a function (in general meaning) of the complete set $\ddot{b}$. In each metric space the distance $\rho(\psi_1, \psi_2)$ between each two points ψ_1 and ψ_2 exists, is positive for each two different points, and is equal to

zero if and only if these points coincide (A.N. Kolmogorov and S.V. Fomin, 1957, 1963, 1968; P.S. Alexandrov, 1948). We shall identify $\rho(\psi_1, \psi_2)$ as the relative probability of the states 1 and 2 of the system A with the probability amplitudes ψ_1 and ψ_2 respectively. If we fix one state with the probability amplitude ψ_0 , then $\rho(\psi, \psi_0)$ can be considered as the probability of each state ψ .

Represent a sub-set of the set $\mathfrak{A}$ by an operator acting on points in the metric space of states. It would be natural to represent each thought by an operator. Then the number of operators necessary to represent a complete set of thoughts will be less for thoughts of higher rank than for ones of lower rank. Operators representing sub-sets of the topological space that cannot be measured simultaneously do not commute. To sub-sets of operators, representing different complete sub-sets of the topological space which cannot be measured simultaneously, correspond different representations like in quantum mechanics (for example, co-ordinate and linear momentum representation), but an important difference exists in our case. In quantum mechanics, two successive transformations, one from the co-ordinate representation to the linear momentum representation and one back to the co-ordinate representation, are equivalent to a unit transformation, i.e. to the case when no transformation was done. This means also that the product of these two transformations is the unit operator, and therefore each of them is the inverse operator for the other. The physical cause of this is the fact that there are two different instruments to measure the co-ordinate and to measure the linear momentum, but the interpretation of results of measurements of the co-ordinate as well as of the linear momentum is performed by the same observer. In our case of A and B representations of the state of the man A, we have two different observers - one for the A-representation and the other for the B-representation; they are the men A and B respectively. It cannot be stated that any information about A obtained and interpreted by A himself must be identical to the same information obtained and interpreted by A but thereupon sent to B, reinterpreted by B, sent back to A and reinterpreted by A. Either some uncontrolled change or loss of information is possible at each transformation from one representation to another. Thus, it cannot be stated that the A and B representations are equal. It cannot be required that each operator of transformation from one representation to another have the inverse operator.

Define a basis J in a topological space of thoughts which are measurable simultaneously, i.e. in a certain sub-space of the whole

space of thoughts (see, for example, J. Dugunji, 1976). Then each thought in this sub-space can be represented as a sum of a number of sub-sets of J , but not as a more complicated function. It can be expected that the same statement would be valid also for operators each of which corresponds to a thought that is a sub-set of J .

ON A MATHEMATICAL DESCRIPTION OF MEASUREMENT

To measure his own thinking process, A uses stable ordered sets of thoughts. The variety of such sets of thoughts depends on the personal experience of A , his education and cultural level, and on the material structure of his brain as determined by heredity and other factors. The structure of this variety limits the capacity of A to measure his thinking process. For example, a jurist has no possibility of measuring thinking concerned with problems of modern physics, because he does not have conveniently available stable sets of thoughts. If there were only one stable (ordered) set of thoughts, the state of A would be represented by a point (in the above described metric space) corresponding to the probability amplitude, which is non-zero only for one stable set of thoughts. Notice that in this case it should be checked whether this point really belongs to this metric space or whether it is a distribution (Loran Schwartz, 1950-1951) and then require a generalization of the space of states. This first set creates (by any means - logical or not) a new set of thoughts, and then the state of A will be represented by a point in the metric space of states, in which the probability amplitude is non-zero only for these two stable sets of thoughts. This process can be continued. This is the 'extremistic' description of the measurement; in reality, the probability amplitudes of other sets of thoughts would be small, but not strictly equal to zero. We shall call the sets of thoughts described above 'trial sets of thoughts'. These trial sets of thoughts can also include the information provided by the sense organs of A and interpreted by A , as well as the information from A 's memory, etc.. This information can be considered as the input which transforms a program existing in A 's brain to a stable ordered set of thoughts. This first trial set of thoughts measures his thinking process, just as a photon in the 'spectroscop of Heisenberg' measures the state of an electron: it creates a new stable ordered set of thoughts, and in this way creates the state of A represented by a point in the metric space of states. If A is in a pure state (only for certain sets of thoughts are probability amplitudes non-zero) an interaction between A

and B can provoke the transfer of thoughts from B to A only if these thoughts are sub-sets of the sets of these sets of A's thoughts with non-zero probability amplitudes. If A does not measure his state, the probability amplitude is non-zero for the whole topological space of thoughts, and so a wide class of thoughts can be transferred from B to A. The appearance of a new thought in A's brain (for example, transmitted from B) in this case means that a new state of A would be created with a non-zero probability amplitude only for one ordered set of thoughts, the last of which is the new one, i.e. the new thought is 'captured' by the end of an ordered stable set of A's thoughts, and this new set of thoughts becomes the trial set of thoughts.

If we represent the state of A as a linear combination (or more complicated function) of states with a different (but fixed for each of them) number of thoughts in each set, we shall obtain for our case representations similar to the second quantization and configurational representation of the multi-particle problem in quantum mechanics (V. Fock, 1932). Then telepathy could be represented as the emission and absorption of thoughts at transitions between states with different numbers of thoughts in their sets, and these transitions could be described by using operators of appearance and annihilation of thoughts. This would be a quantum theory of thought radiation.

PERSONALITY

Personality can be defined as the set of points in the individual's metric space of states representing all his possible states.

His behaviour, his reaction to signals received by his organs of feelings, etc., are determined by the relative probability amplitudes of his states with different sets of thoughts. Therefore, his typical behaviour and thinking are determined by the positions of points representing different states in the metric space of states.

To introduce a basis in this metric space (A.N. Kolmogorov and S.V. Fomin, 1957, 1963, 1968; P.S. Alexandrov, 1948), the set of points under consideration can be represented as a sum of these basic sets. Each of these basic sets can be interpreted as a personality, and therefore each personality can be represented as a sum of basic personalities as predicted by Jordan (P. Jordan, 1960). The observed effects are described by quadratic forms of probability amplitudes,

and this leads to the interference between different basic personalities of A. Usually one of these basic personalities has the highest probability amplitude. Remember that only relative probability amplitude was defined, and therefore it is possible that different ψ_0 (see section 'Spaces of states') correspond to different dominating personalities. We will not discuss this interesting problem here. Notice only that it is very important whether there exists a point (or a set of points) that is distinguished from other points and can be naturally accepted as ψ_0 , i.e. it is very important to find the type of symmetry of the personality.

ON THE THEORY OF THE BRAINWASHING

If A receives signals from B who is speaking or emitting telepathic information and seeks to convince A that his (i.e. B's) statement is right, B is interested in whether A is concentrating on the subject of the discussion, i.e. in whether A is in a state with sets of thoughts corresponding to the logical schema (common for A and B) of thinking and of the control when each set of thoughts corresponds to this logic and to the input information. This means B is interested in whether A is in a state with a non-zero probability amplitude only for a certain sub-set of all possible thoughts. But in the case when B wants to thrust his opinion onto A, i.e. to brainwash him, he is interested in whether A is in a state with the same probability amplitudes for all possible sets of thoughts. Then the first trial set of thoughts in A's brain will be obtained from B. But subsequent thoughts can be produced in A's brain without B's wishes, because a sub-set of thoughts would be generated by A (i.e. with a probability amplitude higher than those of other sub-sets) that includes the above mentioned first trial set of thoughts. But this second sub-set of thoughts can contain statements that the first sub-set (the statement of B) must be rejected. To avoid such a result, B would have to continually seek to equalize the probability amplitudes of all sub-sets of A's thoughts, i.e. such that no trial set of thoughts would be produced by A himself. To achieve this purpose, B could utter many unconnected phrases, each of which would become a first trial set of A's thoughts, just because there will be many sets of thoughts in A's brain with comparable probability amplitudes. If at the same time B says to A (or transmits to him telepathically) any set of thoughts that is identical or very similar to one of the possible sets of thoughts generated by A himself (in terms of the distance between points in the metric space

of states with and without this set of thoughts of B), its probability amplitude can be made to dominate the amplitudes of all the other sets of A's thoughts because at each step B artificially keeps A in a state in which the amplitudes of probability of all sets of thoughts are approximately equal except for one, thus preventing A from checking B's statements.

CONCLUSIONS

The principal result of the present work is to show that telepathy is accomplished by interactions of the human waves of De Broglie rather than by a kind of radiation. The theory proposed in this work can be checked experimentally. The first test could be to examine the influence of screens or other obstacles to the propagation of electromagnetic or other radiation. Our theory predicts that telepathy can exist even between two persons separated by metallic screens, earth, or water. Good and unambiguous experiments on telepathic communication between one man on the earth and the other in a submarine deep under water could be considered a serious test of the proposed theory. The second test could be telepathy experiments when the distance between two persons is extremely great - the ideal would be to use cosmic distances - so that it would be impossible to explain the communication by electromagnetic or other radiations. The third test (admittedly very difficult to execute) could be the measurement of the time of telepathic propagation between two persons. The velocity of the propagation of a radiation cannot be more than the velocity of light, so if the velocity of telepathic propagation is faster than the velocity of light it would confirm our mechanism of telepathy, because the propagation of the (human) wave of De Broglie is not connected with the transfer of the matter and its velocity is not limited by that of light.

The views described in this paper can be applied not only to human beings, but also to any system which has a variety of states and wide enough transitions between them to be considered an automaton: animals, plants, electronic computers, and perhaps certain kinds of inorganic natural objects (including cosmic objects). Possibly the above described telepathic interactions of amplitudes of probability (waves of De Broglie) can be accomplished between objects of different kinds (human beings - animals; human beings - plants; etc.). A non-living macroscopic object has an enormous number of states and can

be considered as an automaton. It is natural to try to describe its interactions with other physical bodies in terms of information transferred from one physical body to another (including the objects under consideration) instead of by the usual descriptions in terms of forces and fields of forces. A change of the state of a physical body would then be defined in terms of its reaction to the information obtained from other bodies. But the information that, for example, a pencil on the table must change its place or orientation can be obtained not only from another physical body; it can also be obtained telepathically from a human being. Thus, psychokinesis can be represented as telepathy between a human being and a physical (non-living) body and described, as written above, in terms of a topological space of thoughts and a metric space of states. Of course, it is only the hypothesis which needs the development of the approach proposed in the present paper to be checked.

NOTES

Note 1: The observer can be a human being or a computer. In physics, measuring instruments and observers are considered simply as physical bodies, but the taking into account of specific properties of the observer as an automaton, the duration of the interpretation process, the duration of the output of information from an observer and its input to another automaton-observer, accompanied possibly by an uncontrolled loss and change of the information, etc., can change the physical transformation laws. For example, it can make questionable the validity of Lorentz transformations for living systems.

REFERENCES

Alexandrov, P.S. 'Introduction to the general theory of sets and functions' (in Russian), Moscow-Leningrad, 1948.

Bergmann, Peter Gabriel 'Introduction to the theory of relativity', 1942.

Bohr, Niels, Nature, 1928, 121, p.580.

Bohr, Niels, *Nature*, 1933, 131, p.421.

Bohr, Niels 'Atomic theory and the description of nature', Part 2, London, 1934.

Bohr, Niels, *Physical Review*, 1935, 48, 696.

Bohr, Niels 'Biology and atomic physics', Physical and biological memorial congress of Louigi Galvani, Bologne, November 1937.

Dirac, P.A.M. 'The principles of quantum mechanics', Oxford, Clarendon Press, 1958.

Dugunji, James 'Topology', London, Allyn and Bacon, 1976.

Einstein, Albert, 'The meaning of relativity', Princeton, 1953.

Einstein, Albert *Annalen der physik*, 1905, 17, p.891.

Fock, V., *Zeitschrift fur physik*, 1932, 75, p.622.

Heisenberg, Werner. 'The physical principles of the quantum theory', Chicago, 1930.

Heitler, W. 'Elementary wave mechanics', Oxford, Oxford University Press, 1956.

Jordan, Pascual, *International Journal of Parapsychology*, 1960.

Kelley, G. 'General topology', New York, 1955.

Kolmogorov, A.N., Fomin, S.V. 'Elements of the theory of functions and functional analysis', Rochester, Gray Lock Press, 1957, 1963.

Messiah, A. 'Quantum mechanics', Volume 1 and 2, Amsterdam, 1961.

Schiff, Leonard I. 'Quantum mechanics', McGraw-Hill Book Company, Inc., New York, 1955.

Schwartz, Loran 'Theorie des distributions', Volume 1 and 2, Paris, 1950, 1951.

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FIRE-IMMUNITY AND FIRE-WALKS: SOME HISTORICAL
AND ANTHROPOLOGICAL NOTES

Giovanni Iannuzzo

In a paper in the E.J.P., Carlos Alvarado (1980) pointed out some aspects of the 'transference' of psychic abilities. Reviewing some historical experiences, Alvarado wrote: "The medium D.D. Home not only showed apparent immunity to fire, but it was claimed that he could 'transfer' his ability to others".

Many other observations and comments have been made about the problem of 'fire-immunity' in the history of psychical research. The phenomenon has been sometimes observed in mediumistic states (Crookes, 1874; Dunraven, 1924; Home, 1921) but, as is well known to students of parapsychology, connected with the mediumistic claims there have been a few magic and religious practices observed and reported by anthropologists, ethnologists and psychical researchers. These are called 'fire-walks'.

In the strict sense of the term, fire-walking is an historical religious phenomenon observed in Asia, America, Indonesia, Oceania, Eastern Europe and Africa. It is clear that the fundamental aspect of fire-walking is the fire-immunity that is shown by the subjects.

Many reports are available about this phenomenon. Andrew Lang (1900-1901) wrote about a fire-walking ceremony that he observed in India. The ceremony is called 'Siri Mariamman Kovil' and it was observed also in the Mauritius Islands and Trinidad. Thompson (1898) and Ockem (1894) attended a fire-walking ceremony in the Fijian Islands.

It was reported that the natives passed through a circular furnace that was made white-hot. Its diameter was 25-30 feet. Near the edges Ocken recorded a temperature of 114 degrees Fahrenheit and in the middle a temperature of 282 degrees Fahrenheit; this value is, however, imprecise because the thermometer's maximum recording was 282 degrees Fahrenheit. Ocken hypothesized that the real temperature in the middle could have been 400 degrees Fahrenheit. All the men who passed through the furnace were uninjured. It seems that some thus could transfer their capacity to others; as a matter of fact, Ocken and four other Europeans passed through the furnace and they also were uninjured.

Price (1936) and Brown (1938) carried out five fire-walking experiments. Kuda Bux - an Indian subject who was studied by Price - could walk on burning coals (400 degrees Fahrenheit).

A large number of studies were carried out in Greece. In this country there exists a religious sect, the 'Anastenaridi', whose origins are wrapped in mystery and legend. It is said that about 1257 in Kosti, a northern village of Greece, the little church of the village took fire. The countrymen heard cries coming from the church and thought that they could be caused by the icons that cried for help. Some peasants threw themselves into the flames and rescued the icons.

Thereafter, the rescuers became the proprietors of the icons and obtained the fire-immunity. This popular tradition, and this 'power', were handed on from father to son. This is the legend.

A number of studies have been carried out about this religious sect and its fire-immunity claims. Tanagras (1953, 1956), Dimantoglou (1952, 1953) and others (a review is available in Cassoli, 1958) have reported their researches and speculations. In 1958 Cassoli, supported by a grant from the Parapsychology Foundation, observed the phenomenon in Greece and published his report in an Italian medical review (Cassoli, 1958). He saw men and women who passed through a furnace (and they were uninjured). In the Cassoli report it is suggested that the fire-immunity could be 'transferred' to others. The observations and comments of Cassoli are very suggestive.

There exist a number of reports about the 'fire-walking'. De Vesme (1928), Leroy (1931), Sayce (1933), Thomas (1934), Gibson (1952), Eliade (1966), Gaddis (1967), Kenn (1949), Salvadori (1946), De

Martino (1949), Van der Bie and Phoa (1956) and others have reported their observations and made speculations, reviews, comments. Zorab, in his bibliographical guide (1957) reported 15 papers about this topic. A brief review of these works, and a note on their relevance for the knowledge of paranormal factors in anthropological accounts, are available in Van De Castle (1977).

All these reports suggest that suggestion, increased motivation or decreased resistance in the subject who is supposed to receive the 'fire-immunity power' should be considered. It does not seem that the paranormal hypothesis suggested by Alvarado (1980) can be considered. A hypothetical PK action by the medium (or the man believing in the phenomenon) on the target (the fire) seems improbable. It might reasonably be argued, however, that a possible psi-conductive effect could exist, but the available observations are too insufficient and imprecise to justify such an explanation.

REFERENCES

- Alvarado, C. 'On the transference of psychic abilities: a historical note', *E.J.P.*, 1980, 3, 209-211.
- Brown, S.T. 'Three experimental fire-walks', University of London, Council for Psychical Investigation, Bulletin IV, 1938.
- Cassoli, P. 'La pirobazia in Grecia (Le Anastenaria)', *Minerva Medica*, XLIX, 1958, 77, 3679-3686.
- Crooks, W. 'Researches in the phenomena of spiritualism', London, James Burns, 1874.
- De Martino, E. 'Il mondo magico', Torino, Boringhieri, 1949.
- De Vesme, C.B. 'Histoire du spiritualisme experimental', Paris, Meger, 1928.

Dimantoglou, M.J. 'Pyrovassie et incombustibilite', Rev. Met., 1952, 22, 82-89.

Dimantoglou, M.J. 'La pyrobatie en Grece', Rev. Met., 1953, 23, 9-19.

Dunraven, Earl of 'Experiences in spiritualism by D.D. Home', Proc. S.P.R., 1924, 35, 1-285.

Eliade, M. 'Shamanism: archaic techniques of ecstasy', Princeton, Princeton University Press, 1966.

Gaddis, V.M. 'Mysterious fires and lights', New York, David McKay, 1967.

Gibson, E.P. 'The American indians and the fire-walk', J. A.S.P.R., 1952, 44, 149-153.

Home, Mme D. 'D.D. Home: his life and mission', London, Kegan Paul Trench Trubner, 1921.

Kenn, C.W. 'Firewalking from the inside', Los Angeles, Francklin Thomas, 1949.

Lang, A. 'The fire-walk', Proc. S.P.R., 1901-1902, 15, 2-15.

Leroy, O. 'Les hommes salamandres. Recherches et reflexions sur l'incombustibilite du corps humain', Paris, Deschee de Brouvet, 1931.

Ocken, T.M. 'An account of the Fiji fire-ceremony', Transaction of the New Zealand Institute, 31, 1894.

Price, H. 'A report on two experimental fire-walks', University of London, Council for Psychical Research, Bulletin II, 1936.

Salvadori, R. 'Le passeggiate sul fuoco', Milano, Bocca, 1946.

Sayce, R.V. 'Fire-walking ceremony in Natal', Man, 1933, 2, 33.

Tanagras, A. 'Firewalkers of modern Greece', Tomorrow, 1956, 4, 4, 73-79.

Thomas, E.S. 'The fire-walk', Proc. S.P.R., 1934, 62, 202-309.

Thompson, B. 'South of Yearn's', Edinburgh and London, 1894.

Van De Castle, R.L. 'Chapter in 'Handbook of parapsychology', B.B. Wolman (ed), New York, Van Nostrand Reinhold, 1977.

Van der Bie, G.J. and Phoa, K.L. 'Over de vuurlopers van Tjiluar', Tijdschrift voor parapsychologie, 1956, 24, 9-12.

Zorab, G. 'Bibliography of parapsychology', New York, Parapsychology Foundation, 1957.

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LETTERS TO THE EDITOR

From Rex G. Stanford: Comments on 'In Defense of Extroverts' by Adrian Parker

It is standard practice for editors to require authors of journal articles to modify, prior to publication, any conclusions which are not justified by the data reported. This is all the more true in the case of unjustified conclusions which have serious implications. I must, therefore, express surprise and dismay that the editors of the 'European Journal of Parapsychology' did not require that Adrian Parker eliminate or drastically modify his claim that Carl Sargent's results in his report on personality differences of psi-conducive and psi-inhibitory ganzfeld experimenters were 'obtained by fitting the data to the hypotheses' (E.J.P., 3, 4, p.377). The facts discussed by Parker simply do not warrant such a charge, and every such fact could have been discussed without bringing in such an accusation.

These remarks are in no way intended to disparage legitimate criticism or to attempt to lessen the impact of the critical observations made in Parker's paper. It is one thing to point out what one believes are errors in the paper, but it is quite another to go beyond those errors by imputing motives which cannot possibly be justified by the mere existence of the errors or supposed errors. Whenever such an imputation is not justified by the facts to be published, and especially when that imputation has serious and possibly hurtful consequences, it should not be published.

I must also point out some factual errors in Parker's paper which are related to my own work. In his effort to justify re-classifying me as a psi-inhibitory experimenter, Parker makes errors in commenting upon my statistical analyses and outcomes in a recent ganzfeld study (Stanford, 1979); see Parker's p.376 and p.378, note 2. (He also fails to list my paper in his references;

consequently, his reader may have difficulty in discovering his errors.) He incorrectly alleges that my analysis of variance results were nonsignificant and that, therefore, my subsequent 'secondary effects' analyses were 'questionable'. Here are the facts: I did two ANOVAs (one for partial transcripts, one for total transcripts) to test an a priori hypothesis of a specific kind of interaction between the character of the auditory ganzfeld stimulation and the interruption variable. That hypothesis was central to the study and was derived from Walker's theory (1975) and my conformance model (1978). Both ANOVAs revealed a significant interaction of the kind hypothesized, and subsequent tests were used, fully legitimately, to pinpoint the effects related to the four means involved in each of those interactions.

In an apparent, but confusing and unclear, reference (p. 376) to the aforementioned study, Parker refers to 'nonsignificant overall results'. Given the 2x2 nature of the experimental design of that study, I am not at all sure what he means. (Three of the four conditions were in at least one respect not traditional ganzfeld sessions; indeed, none of them were, if we consider their very short length.) Partial and total transcripts were evaluated separately for reasons of the hypothetical structure of the study, and in our single condition which was most comparable to traditional ganzfeld sessions, the former showed significant, overall psi-hitting, but not the latter.

It may well be, however, that all this has no bearing upon whether I should be classified as a psi-conducive ganzfeld experimenter. First, as was clearly stated in the paper, I personally tested only 25% of the subjects; my research assistant tested the other 75%. I am not sure that I gathered enough data to be meaningfully classified. (I would add, though, that statistical analyses showed that my assistant and I had comparable success rates and would remind the reader that there was, overall, significant psi hitting for partial transcripts with uninterrupted noise.) I certainly do not think that the label psi-inhibitory experimenter can be justified. But, given the small amount of data which I personally gathered and the nontraditional length of our sessions, any classification of me relative to such breakdowns seems doubtful. That conclusion is especially strong because the other two ganzfeld studies which I have published with Neylon (1975) and with Schmitt (1978) involved no subjects tested by me. So, my opinion, for what it is worth - it is at least well

informed - is that it is not a particularly meaningful exercise to include me in such an analysis.

There is little basis for making a judgment at present about the relationship of experimenter personality as assessed by paper-and-pencil tests and performance of subjects in ganzfeld. This is a very tricky area for several reasons. These include the small number of experimenters involved, insufficient agreement about how to classify experimenters, a greater ease of classifying some experimenters as contrasted with others relative to psi favorability (e.g., some have reported more studies than others), and sundry other difficulties and uncertainties (e.g., the number of tests used for overall significance by a given experimenter).

REFERENCES

Schmitt, M. and Stanford, R.G. 'Free-response GESP during ganzfeld stimulation: The possible influence of menstrual cycle phase', J. A.S.P.R., 1978, 72, 177-182.

Stanford, R.G. 'Toward reinterpreting psi events', J. A.S.P.R., 1978, 72, 197-214.

Stanford, R.G. 'The influence of auditory ganzfeld characteristics upon free-response ESP performance', J. A.S.P.R., 1979, 73, 253-272.

Stanford, R.G. and Neylon, A. 'Experiential factors related to free-response clairvoyance performance in a sensory uniformity setting (Ganzfeld)', R.I.P., 1974, 89-93.

Walker, E.H. 'Foundations of paraphysical and parapsychological phenomena'. In L. Oteri (ed.) 'Quantum physics and parapsychology' (Proceedings of an international conference held in Geneva, Switzerland, August 26-27, 1974.) New York: Parapsychology Foundation, 1975.

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From D. Scott Rogo:

I would like to comment on Alfonso Martinez Taboas and Carlos S. Alvarado's "Poltergeist agents - A review of recent research trends and conceptualizations" which appeared in the November 1981 issue of the E.J.P..

The authors are critical of my 1974 PA brief in which I demonstrated that each member of a poltergeist family, when given psychodiagnostic tests, will show personality characteristics which we normally associate with the primary agent alone. The authors criticize this report particularly because I claimed that the diagnosing clinician (Dr. Gertrude Schmeidler) was 'blind' to the case. The authors seem rather confused about my use of the term 'blind'. As I state in the report, Dr. Schmeidler knew full well that she was dealing with a poltergeist family. The use of the term refers only to the fact that she was 'blind' to which of the three sets of data was that of the adolescent in the family. Under the 'poltergeist pathology' bias, which the authors elucidate, Dr. Schmeidler should have been biased to 'see' typical pathology in one set of data but not in the others. Dr. Schmeidler was also blind to the fact that I was testing a specific hypothesis - that all the family members would show a similar pattern of denied, repressed hostility and aggression. This she did, indeed, find - even though such a finding was counter to what her 'supposed' parapsychological bias would have suggested to her. I am sorry if the authors have taken a more structured connotation of my use of the term 'blind'.

I must also object when the authors say that my study, in their

opinion, "does not add anything significant when compared with" other such cases where projective tests have been employed. Once again let me point out that my study is the first, and only one, in the literature which sought to empirically demonstrate a particular theory, 'The family interaction theory', about poltergeist aetiology.

I think the authors are also very unfair to Dr. Palmer's 1974 J. A.S.P.R. report, in which the psychologist on the case was 'blind' in the sense that she didn't know she was dealing with a poltergeist victim. The authors write that "the fact that the tests were of the projective type, and that the psychologist knew that unexplained movement of objects had been associated with the boy, are serious drawbacks to this study." I think the authors are artificially weakening the significance of the case. As Palmer writes in his report, although the psychologist was aware that unexplained object movements had occurred around the boy, "she had no special interest in or knowledge about RSPK, and there is no reason to expect that she had any preconceived ideas about the psychodynamics involved in such cases".

The authors are correct in stating that psychologists tend to 'see' psychopathology where it often doesn't exist. But there exists a wide variety of psychopathological traits within the codex of clinical psychology. The fact that this psychologist, unfamiliar with the standard 'repression hostility' theory of poltergeist behavior, should find this typical pattern upon testing the subject is indeed significant.

Apart from these quibbles, I would like to congratulate the authors on their paper. Indeed, they have shown that the psychopathological model of the poltergeist is not as well established as many parapsychologists believe it to be, and that more data - gathered under tighter conditions - should be collected. But I would also add one word of warning. I don't know how many investigations the authors have personally conducted into the strange world of the poltergeist. Having conducted many such investigations over the last several years, I have discovered that the type of psychopathology we associate with the poltergeist - denied feelings of hostility, low verbal skills, suppressed anger - are so blatantly obvious that one doesn't really even need to rely on formal diagnostic tools to demonstrate it.

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**EUROPEAN
JOURNAL
OF
PARAPSYCHOLOGY**

VOLUME 4

NUMBER 3

NOVEMBER 1982

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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 as 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. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include names of authors, positions and other affiliations, and mailing addresses. Each table

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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 activity in 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 are considered more important than the level of significance of the outcome of the study. As a practical rule, we advise the potential contributor of an article to submit the design of his planned study before the work 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 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 dates, which are May 1st and November 1st.

A FEW REFLECTIONS ON THE S.P.R. 1882-1982 AND
P.A. 1957-1982 JUBILEE CONFERENCE

Much could be written and commented upon this Centenary-Jubilee Conference. I will restrict myself here to just a few comments, partly because circumstances did not make it possible for me to attend the joint meetings for more than a couple of days.

The very idea of a joint arrangement was a natural one. The council of the P.A. consequently decided in favour of doing homage to the British S.P.R., the first of the seriously intended organizations of its kind, by holding its 25th Annual Convention in England. A more appropriate site for the meetings than Trinity College, Cambridge, could not have been found. For historical, esthetic, and many other reasons, Trinity College is perhaps the world's most famous college - in addition, perhaps, the most beautiful. The organizing committee did by and large a good piece of work. It consisted of Tony Cornell, Bernard Carr and Carl Sargent. The program committee included John Beloff (chairman), Anita Gregory, Julian Isaacs, John Palmer, Ian Stevenson and myself.

It is worth noticing that no seriously intended parapsychological meeting has ever been attended by so many persons. More than 300 reportedly registered. It was also probably a longer meeting than any previous one. A check on the number of papers, symposia and workshops also suggests a record. It may be worth mentioning as well, that for the first time observers from the People's Republic of China attended an international parapsychological meeting. For good or evil, there were a great number of parallel sessions, to the sorrow of those who wanted to attend all the presentations; in the long run, if parapsychology develops as a science, this is an untenable notion.

A new interesting feature was the fact that the critics of parapsychology for the first time at a P.A. Convention, officially got a chance, first to air their criticisms and, secondly, to meet counter-criticism. The title of the session was: 'The Case for Criticism'. The meeting was well attended and chaired by Dr. D.J. West. The contributors and their contributions were as follows: Christopher Scott 'Why Parapsychology demands a Skeptical Response'; Piet Hein Hoebens 'The Hume Game'; Susan Blackmore 'Prospects for Psi Inhibitory Experimenters'; and Ray Hyman 'Does the Ganzfeld Experiment Answer the Critic's Objections?'. Later an open discussion was introduced by Marcello Truzzi. A somewhat heated exchange took place over which I would suspect each party claimed victory. For my part, I believe that the critics as well as the defenders of parapsychology scored several good points but also made some mistakes, mainly because the emphasis was too much on trying to prove themselves right and the antagonist wrong. However, the lasting impression is that a dialogue between reasonable critics and reasonable parapsychologists may have been opened, a dialogue that in the best of cases may turn out to be seminal and important both for parapsychology and its critics. The lesson to the parapsychologists seems to me to be that some of the criticisms must be seriously considered, and to the opposite camp, that all the research workers in the parapsychological field are by no means incompetent scientists and fanatics.

As a member of the program committee of the joint proceeding, I experienced several types of problems which may be worthwhile to ponder upon. Undeniably, the criteria for accepting or rejecting a contribution are hard to establish, but they should be worked on. I did feel that with the combined meeting there was a clear-cut tendency to lower the requirements of the P.A. standards which have traditionally been followed in recent years. Admittedly, the weakest contributions were accumulated in the so-called workshops. According to my view, parapsychology as a scientific community would have been better served if some presentations had been rejected, which I at least felt caused embarrassment. Be that as it may. Another issue worth pondering upon is whether the P.A. should continue to use more lax standards from papers submitted from countries outside the mainstream of parapsychology. Should we try to establish a special committee for rendering editorial know-how and language skills (in the writing up of papers) to people not used to the format and requirements of the P.A.? Another sign that I think one could observe is that the range of opinions regarding the ideal of science that we should maintain within the P.A. has probably increased in recent

years. This range of opinions may soon start to create considerable tension. One scenario is that it could lead to a crisis regarding the unity of the P.A.. How can these problems be constructively discussed and remedied? Are some structural changes of the organization needed? What should such possible changes be?

The joint meeting was in many ways a stimulating experience, but the very fact that a hundred years of research endeavour has not been sufficient to fully convince even the moderate critics should tell us that the situation calls for hard and constructive work, not for wishful thinking or euphoria. Predictably, a backlash came after the heydays of the seventies, a period when quite a number of parapsychologists overstated our case and even showed a lack of responsibility, judgment and taste. In that wake we have to combine our efforts both to do constructive research, based on relevant criticism, and to defeat rather skilled but imbalanced criticism, reflected in J.E. Alcock's book, just to mention one of the recent criticisms of parapsychology.

Martin Johnson

STUDIES OF THE EFFECTS OF MENTAL HEALING AND EXPECTATIONS
ON THE GROWTH OF CORN SEEDLINGS

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In previous work with mice (Solfvin, 1982) it was found that statistically significant effects on the health of the mice could be observed in a controlled study with only the expectation that a mental healing treatment was being administered (no actual treatment). A further experiment with the same illness model was underway but aborted due to technical problems. Subsequently, the veterinary pathology group (University of Utrecht), with whom these studies were co-conducted, concluded their own research with this illness model. Therefore, the author decided to conclude this series and seek an alternative illness model with which to continue this line of work.

After considering various possibilities for a suitable illness model, it was decided to explore the germination and early growth of corn (maize) seeds which were presoaked in a saline solution. It was hoped that explorations with plant growth might shed some light upon the mixed results reported by different experimenters in the past (see Solfvin, 1982). Working with healers, Grad (1963, 1964), McDonald et al (1976) and Miller (1972) have reported significant effects for the 'healing' treatments on the growth of the plants, while Harari (1975), Hickman (1979) and Knowles (1954) have found no effects. With

This paper fulfils the publication policy of this journal.

non-healers ('gifted' PK subjects or otherwise) significant effects have been reported by Barry (1968a, 1968b, 1971), Brier (1969), Grad (1967), Haraldsson and Thorsteinsson (1973), Loehr (1959), Nash and Nash (1967), Nicholas (1979) and Vasse and Vasse (1948, 1950, 1954). Lenington (1979), and Pauli (1973) have reported null results and Shafer et al (1982) found significant effects in the opposite direction (controls grew taller than experimental 'treated' plants) which they interpreted as psi-missing. Moreover, at least one well controlled series with plant growth has shown significant results that were attributed to the 'laying-on-of-hands' treatment by a well known mental healer (Grad, 1963, 1964). Thus, it is important to know if the manipulation of the expectancy structure without the actual treatment could also produce such effects.

There is some indication that this might be so from a series of experiments conducted to test the effects of a radionics device on crop yield (De la Warr, 1970). A small plot of ground was planted with treated seed while another identical plot was planted with untreated seed. After several successful tests of this sort, another one was run the same way except that this time, unbeknownst to the assistants who monitored the crop, the "treated" seeds were never really treated by the radionics technique. Nonetheless, a significant advantage was again registered for the supposedly treated seed.

In addition to the potential for shedding light on other plant growth studies, there are two main issues raised by the mouse studies that need to be addressed in the current series. The first is the issue of the possible relationship between the two expectancies given to the handlers--- the one being sensorially available to the handlers (illness expectancy) and the other not (healing expectancy). In a post-hoc analysis (not previously reported) scores were assigned to each handler reflecting the degree to which his or her mice showed each of these effects. The scores were simply the square roots of the F-values for the respective factors (main effects) from the analysis of variance tables. Each score was prefixed by a sign indicating the direction of the difference--- a plus sign (+) indicated a difference in the direction of the induced expectancy while a minus sign (-) indicated the direction opposite to the induced expectancy. The Pearson correlation between these two expectancies for the 8 handlers is $r=0.673$ which is marginally significant ($.05 < p < .10$). It is planned to follow up this interesting possibility in the current series.

The second issue left unsettled in the mouse studies was the role played by the beliefs of the handlers. It is hoped that with larger N's for both plants and assistants we will be able to assess this variable in the current series.

Dr. H. Konings, a plant physiologist at the Botany Laboratory of the University of Utrecht, agreed to act as advisor and consultant for this series as well as to make available the special materials and techniques that would be required. Under Dr. Koning's guidance, a month-long series of pilot studies was conducted to test the effects of temperature, moisture, depth of planting, soil type and various combinations of saline concentration and durations of soaking on the growth of the seeds. A standard planting procedure was developed which is used throughout the following experiments.

EXPERIMENT 1

Subjects

While the pilot studies were in progress, I gave a talk on general parapsychology to a small group of Dutch ladies who were studying conversational English. One of the ladies told me that her husband was a "magnetizer" (a term commonly used in Holland to describe a mental or laying-on-of-hands healer). Subsequently, she and her husband agreed to participate in this plant growth study, he as a "healer" of the seeds and she as a caretaker for their subsequent growth. They are both retired and could devote a considerable amount of their attention to these studies. Both are highly motivated to do so since they are quite involved with philosophic and scientific discourses regarding so-called paranormal phenomena.

While Mr. K believes that he is gifted with a healing ability, he does not think of himself as a powerful healer. He expressed some doubt about whether his treatment would show any effect upon the plants for this reason. In his work with human patients his healing treatments involve, first, a diagnosis by a divining rod passed over the body together with intuitive impressions he might receive, and, secondly, a laying-on-of-hands procedure applied to the area of the problem. Mr. K does not claim to know how this procedure works but feels there might be some special energy radiating from his hands or

body when he is doing his treatments. He discovered his ability as a teenager and later developed it through practice with friends and co-workers who were sick or in pain. (footnote 1)

Materials and Preparations

On the evening before the experiment begins a handful of maize (corn) seed (Cultivar Caldera 535) is taken from a large sack containing several thousand. From these, forty normal looking (undamaged) seeds are selected, washed in tap water to remove the fungicide and placed in a shallow dish. A 2% saline solution (table salt and tap water) is prepared and poured over the seeds so that all seeds have at least one centimeter of solution covering them. The seeds are gently stirred and then left to soak overnight (11.5 hours) at room temperature (20 C.) In the morning the seeds are rinsed well with tap water and dried with a paper towel, at which point they are ready for planting. The use of the saline solution, as opposed to an identical procedure using only tap water, was found to retard the growth of the plants in height (25 cm) and weight (.10 grams), but does not effect their successful germination.

For this study a commercial planting box is employed. ("Pop-up Pots"---a self watering propagator and window box. Registered design 974459. Patent number 131659. Manufactured in Great Britain by Poly-Gard). The 24 inch by 4 inch unit contains 36 separated cells which, when filled with sand, draw water from a holding tank below by capillary action. This avoids the problem of someone having to pour water directly onto the seeds or plants. A transparent plastic cover is also provided which limits the exchange of air with the surroundings. There is a thermometer supplied with the unit that can be placed near the soil level inside the cover.

Dry construction grade sand of uniform consistency (medium fine) is lightly packed into the 36 cells and levelled several hours before the seeds are planted in it.

Design of the study

There are two experimental manipulations in this study which

parallel those in the mouse studies (Solfvin, 1982). The first is the expectancy given Mrs.(and Mr.) K that some of the seeds are "sick" (soaked in salt) and some "normal" (soaked in water). As is the mouse studies, the information about which plants are which is totally nonblind. There is a diagram of the planting box on the first page of the notebook that is left with them for recording the daily observations. The S (salt soaked) and P (plain water soaked) seeds are clearly marked on this diagram. They are planted in a distinct checkerboard arrangement which makes their placements identifiable at a glance.

The second manipulation is the actual healing of the seeds by Mr. K. This is, of course, confounded by Mrs. K's expectations that this treatment will be effective. The healed and unhealed seeds are randomly assigned to a repetitive pattern in the planting box which assures balanced representation throughout the 36 cells. These positions were not known to Mr. and Mrs. K. The planting of the seeds was done, nonblind, by E.

Randomizations

The seeds were randomized into the four groups (sacks) before arriving at the K's house. The sample of 40 soaked and dried seeds were shaken in one lightly closed hand and one of them drawn with the other hand, reinspected for damage and put into pile 1. This was repeated, incrementing the piles sequentially, until the four groups of 9 seeds were formed. The remaining seeds were discarded.

The randomization to positions in the planter was done at the K's house while they were not present. Since the planter is divided into three separate boxes of 12 (2 rows by 6 columns), four slips of paper designating the four groups of seeds were shuffled and drawn three times. the order of this sequence was used to assign the four groups of seeds to the twelve positions in the first section of the planter. This order was repeated for the remaining two sections. The seeds were randomly drawn from the four sacks and planted into the positions indicated by the pattern.

Procedure

In the mid-afternoon, the preprepared planter and seeds are brought to the home of Mr. and Mrs. K, the participants in this experiment. E also brings a notebook containing daily record sheets, a cardboard ruler for measuring plant heights (in the planter), a jar of extra sand, and a plastic device for planting seeds at a uniform depth. The seeds are in four sacks, two with white tabs and two with green tabs. Mr. and Mrs. K. are told that the white-tabbed sacks contain the salted seeds while the green-tabbed ones contain plain (unsalted) seeds. The latter, they are further told, should grow faster than the former. Mr. K is given one sack of each color to "heal". After he has done so, E asks the two of them to leave the room while the seeds are planted in a special order in the planter.

The seeds are then planted by E, assuring that they are the same depth (0.3 cm) and orientation (pointy end down, longest cross-sectional axis parallel to the long dimension of the planter). The sand is levelled over the seeds, adding extra sand as needed, and Mr. and Mrs. K are asked to come back into the room. The planter is then carefully moved to a place where it will stay for the remainder of the experiment and levelled with a carpenter's level in both directions. The place is on a wooden shelf directly above a heating unit and about 20 centimeters from a permanently closed window. A thick newspaper is used to shield the planting box from direct sunlight. Finally, 600 ml of tap water is poured into the holding tank, the thermometer put into place and the cover installed.

Mrs. K acts as caretaker for the plants during the four days of growing. Her task is to check on their progress twice a day. When no shoots are showing she need only look through the transparent cover and record the temperature. After the shoots begin to appear she must also remove the cover and measure the heights above soil level. These measurements are recorded, along with the temperature and time of day on the daily record sheets in the notebook.

Four days later, E returns to their house to pick up the planting box and other materials and brings them directly to the botany laboratory. The plants are immediately harvested, measured, weighed and put into bottles for subsequent drying (in an oven) and reweighing (dry weights). A maximum of three hours elapses from the time of picking the plants up to the completion of the initial measurements (fresh). Thus, there is negligible growth during this time even under the most optimal conditions (0.3 cm in height).

Analyses

The plants are harvested one at a time from the left to the right of the planter. Immediately upon harvesting, the total height and nodal height are measured (see Diagram 1) to the nearest millimeter with a standard plastic rule. In this experiment, this process was done by E in his home and each plant, after measuring, was placed into one of four plastic sacks each containing about 10 ml of tap water. These four sacks, coded to represent the four conditions for the 2x2 factorial design, were brought to the botany laboratory where a trained technician weighed (blind) the roots and shoots and recorded them along with the group code. The plant parts were put into numbered bottles, dried (80 C for 24 hours) and reweighed by the same technician to obtain the dry weights.

(In all subsequent experiments the measurements of the heights and fresh weights are done simultaneously to avoid transporting the harvested plants in sacks.)

The statistical analysis is a two-way analysis of variance (fixed effects model) with two levels of illness expectancy (salted, non salted) and two levels of healing treatment (healed, non healed). This analysis is conducted for each of the 6 measures described above. The significance level is set at the five percent level.

Hypotheses

There are two rather general goals of this first experiment. Since there is no reliable precedent available to estimate the possible size of effect of expectancy or mental healing treatments with this particular illness model it was hoped that this study would establish one. Of particular interest is the question of whether a single planting box with three plants, split into four groups (2x2 factorial) would be sufficient to detect the subtle effects on the plant growth. Thus, the fact that this was E's first experiment in this plant series after much preparation and after successful mouse studies, the fact that this was Mr. and Mrs. K's first experiment of this sort, and the fact that the handler of the plants (Mrs. K) was certainly an enthusiastic believer with high expectations for success, should combine to provide optimal results if there is anything to be found

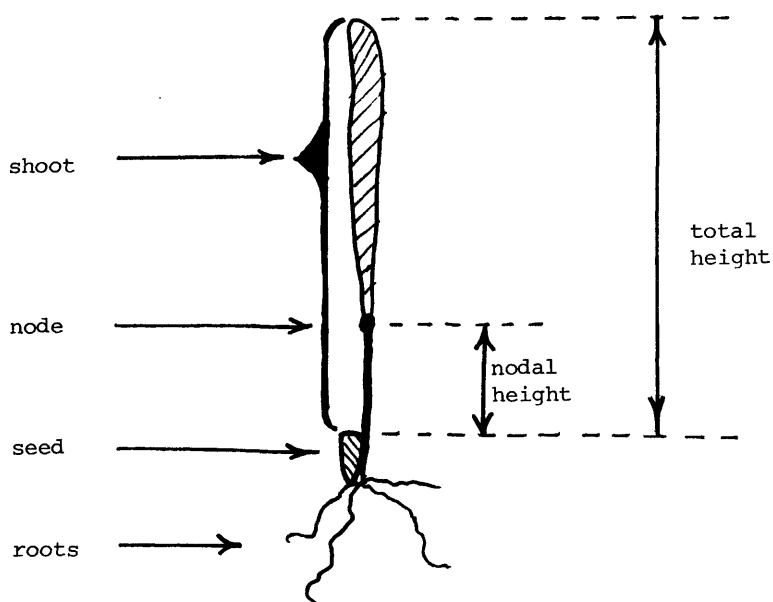


Diagram 1: The parts of the corn seedling

here. Even the non-blind planting and measurement of plant heights (by E) were intentionally included on the grounds that there might be handling effects involved that should be included in these initial estimates of the size of possible effects.

The second goal of the experiment was to explore the various measurement parameters available for assessing the growth of a seedling. It was planned that this first study should allow for a selection of growth parameters that will be most sensitive to the sorts of effects that we are interested in.

It is therefore hypothesized that there will be significant effects for both the illness expectancy and healing expectancy, either main effects or an interaction. Significant effects of any sort in this study can be attributed to normal sensory motor mediation on the part of E, Mr. or Mrs. K, or some combination of them. However, it is assumed that the size of the effects noted here will be an adequate estimate of the possible effects in future studies where sensory processes are systematically eliminated.

There is no specific hypothesis concerning the several measures used. It is assumed that the results of this experiment will provide a basis for the selection of a subset of the measures in future studies.

RESULTS

The results are summarized in the tables below.

DISCUSSION

The results show an interactional pattern. One of the four cells in the 2x2 design, the 'plain-healed' cell, shows more growth than the other three cells. This interaction is significant for both the fresh and dry weights of the roots, but the tendency is clearly present throughout the 6 measures. The fact that the node tends to be smaller for this group is not inconsistent with greater growth since on healthy plants that have adequate radiation (sunlight), the nodal height is often shorter than on less healthy plants.

TABLE 1
Experiment 1
Summary of ANOVA's for Weights and Heights

Source	DF	Roots		Shoots		Heights	
		fresh	dry	fresh	dry	nodal	total
		F	F	F	F	F	F
Salt	1,31	9.359c	3.754a	.349	1.730	3.822a	.084
Heal	1,31	2.606	1.438	1.608	2.104	.514	.494
SxH	1,31	6.034b	5.006b	1.363	2.872	1.227	.958

a: $p < .10$; b: $p < .05$; c: $p < .01$

TABLE 2a
Experiment 1
Average Heights by Condition

	Nodal Height		Total Height	
	Salt	plain	Salt	Plain
Heal	1.94	1.42	5.98	6.48
Nonheal	1.88	1.73	6.09	5.81

The interactional pattern suggests that both expectancy manipulations were effective here, although it is not clear how or on whom they operated. It is clear, however, that a single planting box with 36 plants divided into four groups should be sufficient to

TABLE 2b
Experiment 1
Average Weights by Condition

	Plant roots				Plant shoots			
	fresh		dry		fresh		dry	
	salt plain		salt plain		salt plain		salt plain	
Heal	.295	.542	.027	.047	.642	.730	.038	.049
Nonheal	.329	.353	.031	.030	.635	.571	.039	.035

observe significant differences in growth that are due to subtle and, possibly, non-sensory factors.

Secondly, it would appear that the weight of the root structure (fresh and dry) is most sensitive to these subtle influences. However, due to the difficulties of weighing the roots accurately, and due to the fact that all the measures reflect the same tendency, it is suggested that the fresh and dry weights of the total plant (roots and shoot) might be more conservative and sufficiently sensitive to any potentially interesting effects.

Finally, the success of the healing expectancy in this study leaves open the possibility that this effect was an actual treatment effect due to Mr K's magnetizing the seeds. It could also have been due to Mrs. K, in a nonsensory manner, or to E in a sensory or nonsensory manner. It could even have been due to a combination of these or other factors. It was decided to conduct the next experiment so as to experimentally separate these possible causal factors from one another.

EXPERIMENT 2

This experiment is exactly the same as experiment 1 from the point of view of Mr. and Mrs K. However, there are several major modifications of which they were (I believe) unaware, as well as some minor changes to tighten up the procedure.

Preparations

A total of 80 seeds were soaked in saline solution for 11.5 hours and 72 of them were randomized in the same manner as in experiment 1 into eight groups of 9. These were put into sacks with color coding so that there were two identical groups of four sacks. These two groups were carried in the left and right pockets of my coat to the K's house.

Two planters were prepared with sand and both were put into my car. One of these was brought into the K's house.

Procedure

Arriving at the K's home, I told them that it was best that we not discuss the specifics of the previous results until we had concluded the entire series. I did, however, tell them that the results showed a quite clear effect for Mr. K's treatment. I asked him to do exactly the same thing that he had done before with these seeds.

We then repeated the procedure we had previously used. However, when the K's left the room for me to plant the seeds, I put the four groups of seeds that they had seen back into my pocket. From another pocket, I took the other set of seeds and I planted these in the planter.

After leaving the house, I drove immediately to the botany laboratory where I planted the original set of seeds in the second planter. This planter was maintained in the germination room at the laboratory under constant temperature, humidity and lighting. Dr. Konings acted as caretaker for this planter and made regular checks and measurements. He was aware that half the seeds were actually

healed, but not which ones, and he also was told that half were salted and half not in the known checkerboard planting pattern. Both planting boxes were planted with the same planting arrangements with respect to the four groups.

Analyses

Both planters were harvested by E and Dr. Konings, working together, with both blind to the healing condition. The same eight measurements were made for each plant. However, it was decided to focus on the overall growth parameters, as suggested in the first experiment. Thus, the hypotheses were tested on the total fresh and dry weights (roots plus shoot) of the plants. The same statistical analysis as in the first study was planned, applied independently to each of the two planters.

Hypotheses

This was actually two experiments. For the planter (1) in the K's home, the only manipulations were of expectations. It was hypothesized that there would be either a.) main effects for both expectancies (salt and healing) or , b.) an interaction between the two. The healing expectancy here was designed to exclude an entirely sensory-motor explanation.

For the botany laboratory planter (2), a main effect for the healing treatment in the absence of an illness (salting) expectancy effect would suggest that the healing treatment itself was effective. This would not necessarily require a parapsychological explanation since Mr. K actually held the seeds in his hand while he healed them. On the other hand, main effects for both factors would suggest that the expectations may have been effective here, for the healing treatment effect as well as for the illness expectancy.

RESULTS

The results for planter 1 (in the K's home) are presented in the

Table 3 in summary form.

TABLE 3
Experiment 2
Summary of ANOVA's for Planter 1

Source	DF	Total ht F	Nodal ht F	Root wt F	Shoot wt F	Plant wt F
Salt	1,29	.420	.364	.250	.195	.014
Heal	1,29	.196	.127	.619	.826	2.149
SxH	1,29	.014	.720	3.920a	.537	.552

a: $p < .10$

TABLE 4
Experiment 2
Average Growth Measures for Planter 1

	Nodal Height salt plain		Total Height salt plain		Root Weight salt plain		Shoot Weight salt plain		Plant Weight salt plain	
Heal	1.44	1.36	4.17	4.21	.153	.119	.180	.182	.333	.301
Nonheal	1.54	1.41	4.65	4.76	.153	.192	.218	.225	.370	.418

TABLE 5
Experiment 2
Summary of ANOVA's for Planter 2

Source	DF	Nodal Height F	Total Height F	Root Weight F	Shoot Weight F	Plant Weight F
Salt	1,31	.130	.000	.561	.278	.490
Heal	1,31	4.120a	.190	.256	.297	.313
SxH	1,31	.000	.006	.868	1.056	1.086

a: $p < .10$

TABLE 6
Experiment 2
Averages of Growth Measures for Planter 2

	Nodal Height		Total Height		Root Weight		Shoot Weight		Plant Weight	
	salt	plain	salt	plain	salt	plain	salt	plain	salt	plain
Heal	.313	.333	6.61	6.56	.271	.260	.276	.256	.546	.516
Nonheal	.433	.456	6.86	6.93	.248	.330	.257	.312	.505	.642

DISCUSSION

There are no significant effects for either planter on the fresh and dry plant weight measures (roots plus shoot). This suggests that neither the treatment of Mr. K, nor the expectancies of the other persons involved was effective in this experiment. However, there are three interesting features of the subsidiary measures that deserve mention.

The first interesting feature is a marginally significant interaction ($p < .06$) in planter 1 that is found only in the root weights. The second feature is a marginally significant main effect ($p < .06$) for the healing treatment which occurs only in the nodal heights of the plants. These two features could be seen as simply random fluctuations in the many measures or as slight, but meaningful, effects of the treatments. The third feature suggests another interpretation which points toward an artifact that may be present in other studies of healing effects on plant growth as well. This is a significant interaction amongst the four cells of the design for the weights of the seeds after harvesting. The seeds in the healed group for example, were heavier than the seeds in the nonhealed groups, and were therefore likely to have been heavier to begin with. Could the size of the seeds before planting be creating artifactual differences in plant growth?

During the pilot studies this topic had been discussed but since, in botanical studies, it is known that the size of the seeds is not consistently related to the rate of growth of the plant it was not considered necessary to preweigh the seeds. The randomization of them should have been sufficient to control for this factor. However, since these seeds were being soaked in saline solution prior to planting it is possible that seeds of different sizes are differentially effected by the salt treatment, due to different absorption characteristics (footnote 2). It was decided to check on this possibility in experiment 3.

EXPERIMENT 3

Preparations

Thirty-six seeds were selected in the usual manner, weighed on an electronic scale to the nearest milligram, soaked in saline solution (as before) and placed into individual paper packets with the weights written on them. These were randomly divided into four groups as before and put into four sacks. The weight of each seed is used as its identification number and these were recorded, by group, on a sheet of paper. A set of 36 slips of paper is prepared with each having the weight (ID number) of one of the seeds on it. A person not otherwise involved with the experiment separates these into the S and P groups. The S seed slips are shuffled and randomly drawn for sequential assignment to the S positions of the planter while the P seed slips are likewise assigned to the P positions in the planter. In this way a diagram of the planting box is prepared with each of the 36 positions or cells containing a unique seed weight. This diagram is given to E for the actual planting which will be done at the K's home. The remaining listings of group membership are kept by the randomizer until the end of the experiment. In this way, E will remain blind to which seeds are 'healed' and which not during the planting.

Procedure

After Mr. K has healed the two healing groups, the seeds are put into a single pile on the table. These are mixed with the numbers hidden so that E (or the K's) do not know which are the ones Mr. K has healed. Then E draws the seeds one by one and plants them in the predetermined order by number (weight).

The plants were allowed to grow for five days instead of four since the plants in experiment 2 were rather small and quite variable in growth.

Analyses

The plants were harvested at the botany laboratory with the help of Dr. Konings. The measures used were the two heights (nodal and total) and the fresh and dry weights of the plants. Separate root and shoot weights were not measured.

To the ANOVA used in the earlier experiments, seedweight was added as a covariate. It was planned to assess the linear (Pearsson) correlations of seedweight with each of the measures and to produce scatter diagrams for visual inspection of the relationships.

RESULTS

The results of experiment 3 are presented in tables 7 and 8.

TABLE 7
Experiment 3
Summary of ANOVA's. Covarying Seedweight

Source	DF	Nodal Height F	Total Height F	Fresh Weight F	Dry Weight F
Salt	1,28	1.222	2.957	1.477	3.698a
Heal	1,28	.426	.287	.212	.003
SxH	1,28	1.209	.563	.212	3.758a

a: $p < .10$

DISCUSSION

The marginally significant ($.05 < p < .10$) main effect for illness

TABLE 8
Experiment 3
Average Growth Measures by Condition

	Nodal Height		Total Height		Fresh Weight		Dry Weight	
	salt	plain	salt	plain	salt	plain	salt	plain
Heal	1.63	1.59	6.66	7.24	1.06	1.07	.232	.205
Nonheal	1.25	1.66	5.31	7.39	.86	1.10	.191	.211

expectancy found in the dry weights of the plants would seem to be interesting here since it is in the expected direction and is supported by a nonsignificant tendency in the same direction in the heights and fresh weights of the plants. Examination of the average heights (Table 8) suggests that the effect is due to retarded growth in the salted-unhealed group relative to the other three conditions. The seedweights show only a small linear correlation ($r=.22$; n.s.) with plant dry weight and this is controlled for by covarying it in the ANOVA.

However, close examination of the results shows that the seed weights for the same group (salted-unhealed) average only 227 mg as compared to 267, 249 and 252 for the other groups. In fact, 4 of the 9 seeds in this group weighed less than 205 milligrams prior to soaking. The smallest one (169 mg) never germinated and was excluded from the ANOVA while the other three produced stunted plants averaging 3 cm in height.

By comparison, only two other seeds below 205 mg occurred in the other three plant groups, one in the plain-healed group and the other in the plain-unhealed group. Thus, of 6 seeds in this study which were under 205 mg, 4 were randomly assigned to the salted-unhealed group which subsequently showed stunted growth. Removal of these 6 plants from the analysis yields non significant results.

This is an important finding. In view of it we must question whether the results in experiments 1 and 2 might have been due to this artifact. Unfortunately, since we do not have the seed weights to check, we cannot exclude this possibility. It is particularly interesting that the results in experiment 1 were also due to one cell in the 2x2 design showing enhanced growth relative to the others. This makes it more likely that the artifact noted here may have operated there as well.

EXPERIMENTS 4 AND 5

These studies will be described together because they were conducted simultaneously. The first was conducted entirely at the botany laboratory and involved the manipulation of the expectancies of a student who planted, grew, harvested and made all measurements on one planting box of 36 plants. The other part (experiment 5) is a control experiment involving two psychology students who each did the same with one planter kept in their homes but without expectancy conditions. The seeds for all three were weighed, soaked, reweighed and assigned to the students from a single batch.

Subjects

For experiment 4 the subject, G, is an advanced student at the botany laboratory who was working with Dr. Konings. Dr. Konings asked G if he would assist me in my experiments and G agreed. This student was experienced in all of the techniques that are required in conducting the study.

For experiment 5, two advanced psychology students (M and H) who were studying with E were requested to run this control. They were earlier trained (by E) in the procedures for conducting these studies and were fully aware of the expectancy manipulations which are the real purpose of them.

Preparations

For these studies, 120 seeds were weighed, soaked and reweighed with both weights being recorded on the individual seed packets. Eighty of the seeds were soaked in saline solution and 40 in plain tap water. Thirty-six of the salt-soaked seeds with presoaking weights between 230 and 300 milligrams were selected out for experiment 4 and assigned alternately by weights to four groups of 9. These were put into four envelopes marked with SA, SB, PA and PB. Two more groups of 18 were formed from the salted seeds and two groups of 18 from the plain water soaked seeds for experiment 5.

Randomizations

Six index cards were prepared so that three had the letter A, and three the letter B written on one side. These were shuffled blindly by an assistant who recorded the resulting sequence of A's and B's in the first six S locations on the diagram of the planter (section 1 of the planter). The cards were reshuffled and this procedure repeated for section 2 and then section 3 of the planter. The P locations were similarly assigned A's and B's. Thus, each of the 36 cells in the diagram had either SA, SB, PA or PB written in it.

The same assistant then flipped a coin to assign the letters A and B to the numbers 1 and 2. This was recorded on a card and sealed in an envelope. Independently, E flipped a coin and assigned the numbers 1 and 2 to "healed" and "not healed", recorded this on a card and sealed it in an envelope. Both cards remained locked in E's desk until the results from G were completed.

Procedure

One prepared planting box, four envelopes with 9 seeds each and instructions and recording sheets were brought to the botany laboratory. The two psychology students accompanied and assisted E there. G was given general instructions on planting the seeds and on the measurements he was to make. He was told that half the seeds had been soaked in saline solution and half in plain water, and that half

of each kind were "healed" by a paranormal healer who had shown excellent results in pilot studies. He was given four large envelopes, marked SA, SB, PA and PB, as well as a diagram of the planting arrangement for them. He was instructed to begin with the upper left corner of the planter, draw a seed from the appropriate envelope (according to the diagram), record the seed numbers (weights) from the packet onto the diagram, and plant the seed in the planter. When all of the seeds were planted the planter was to be set in a permanent spot in the germinating room, water added and the thermometer and cover set in place. This was accomplished after E and his students left the laboratory.

During the week, G made daily checks and on the fourth day, harvested, measured, dried and reweighed the plants and left the completed results with Dr. Konings for E to retrieve.

Back at the psychology laboratory, the psychology students were given similar materials to take home to plant and grow the seeds. However, the S and P seeds they were given were actually soaked in saline or plain water, respectively and there was no healing expectancy.

Analyses

The plant measurements involved were the nodal heights, total heights and fresh and dry weights of the plants (whole plants with seed). Experiment 1 was to be analyzed as the earlier experiments, this time covarying both the pre- and post-soaking seedweights.

For the control experiment, the object was to determine whether both pre- and post-soaking seedweights were necessary and if not, which was the better predictor. It was also hoped to estimate the relations between seedweight, salt treatment and plant growth and their stability across two different environments, the homes of M and H. For this, scatter diagrams, correlation matrices and multiple regression techniques were planned as search measures.

RESULTS

The results from experiment 4 are presented in Tables 9 and 10, below.

TABLE 9
Experiment 4
Summary of ANOVA's. Covarying Seedweight

Source	DF	Nodal Height F	Total Height F	Fresh Weight F	Dry Weight F
Salt	1,30	.397	3.466a	.081	.956
Heal	1,30	.497	.049	.026	.577
SxH	1,30	2.116	1.172	.252	.092

a: $p < .10$

DISCUSSION

The hypotheses concerning the effects of the expectation structure upon the growth of the seedlings were not confirmed in this experiment. The marginally significant tendency ($p < .075$) on the salt expectancy factor is found only in the heights of the plants and is assumed to be due to chance fluctuations.

The control study confirmed that the salting treatment is effective at stunting the growth of the plants but showed also that the manner in which it does so is dependent upon other factors most likely related to environmental conditions. Both H and M show a decrease in height of about 25-30% due to the salting, as well as a 15-20% drop in the heights of the nodes. For M, the salted plants were far more

TABLE 10
Experiment 4
Average Growth Measures by Condition

	Nodal Height		Total Height		Fresh Weight		Dry Weight	
	salt	plain	salt	plain	salt	plain	salt	plain
Heal	.91	.94	6.6	7.1	1.01	1.13	.272	.295
Nonheal	.94	.87	7.6	6.2	1.13	1.05	.295	.296

variable in height ($sd=26.2$) than the unsalted ones ($sd=14.2$), but for H the variability in height was not effected by the salt treatment (SD's of 12.4 and 10.1 for salted and unsalted). Only H's plants show a substantial decrease in fresh weight due to salting (1.03 grams to 0.87 grams). M's salted plants weighted only slightly less than the unsalted ones (1.04 versus 1.00 grams). M's plants also grew considerably taller than H's for both salted and unsalted seeds. By comparison, G's plants were taller than M's and H's salted ones, (averaging 7.1 cm versus 6.5 and 5.3 cm), but not appreciably heavier than M's (1.06 grams).

There is little uniformity amongst the correlations for the two seedweights with the growth measures for M and H for salted and unsalted seeds. The strongest correlation is between both seedweights and the weight of the plants, but this drops to $r=.21$ (n.s.) for M's salted group for presoaking seedweight and to $r=-.01$ for post soaking seedweight. For the other groups, this correlation is above .70 for both seedweights. Total height and nodal height show no consistant relationship with either seedweight. The two seedweights correlate beyond $r=.90$ for M, H and G, so we can assume that they are interchangeable for our purposes. Thus, it is redundant to weigh the seeds twice.

These mixed results suggest that the early growth processes of corn

seedlings are highly variable and are apparently subject to a variety of influences that are not entirely understood. The botanical consultant for these studies, Dr. Konings, suggested that these results might be due to the differential environments in the following way. For seedlings grown in an optimal environment, the seed size will not be a factor in the plant growth but when the environment is marginal, seedsize may become a significant factor. By extension, we can speculate that the salting of the seeds prior to planting may increase the importance of subtle environmental factors that would not have a noticeable influence on the growth of normal (unsalted) plants.

From the statistical standpoint, these results bring into question the assumptions of normality of distribution, equality of variances and even the most appropriate measurement scale for these parameters. It seems clear that a single planting box with 36 plants is subject to variations that are too great and too uncontrollable to make an adequate statistical assessment of the possible subtle effects of expectations, sensory or otherwise. At a minimum, a larger sample is necessary to work with this illness model and that is the intent of the next experiment.

EXPERIMENT 6

This final experiment is an attempt to replicate the mouse studies (Solfvin, 1982). The results of the previous experiments in this series suggest that a larger scale experiment is necessary to adequately assess whether the expectancy treatments can influence the growth of the salted maize seeds. The often high variability and the question of the adequacy of randomization techniques to control for artifacts when small samples are involved make this necessary. Moreover, it was desired to closely match the setting of veterinary students who volunteered to watch over organisms which they were in training to do professionally. It was also important to conduct a study involving sufficient numbers of both sheep and goats to assess this factor, and to have sufficiently uniform conditions to correlate the two kinds of expectancy effects across the group of handlers. In order to give more impact to the healing expectancy condition, a 'bogus' healer was to be brought into the laboratory to perform a bogus prayer ritual over the appropriate seeds. This bogus healer was

actually a friend of E's who was raised in Chile and had witnessed the activities of Chilean native healers, or 'machis', many times in his life. He did not believe that there was anything 'paranormal' about these treatments but agreed to pretend to be a machi with great powers for the purposes of this experiment.

Subjects

Eleven advanced students at the botany laboratory were assembled (by Dr. Konings) and given a brief talk by E on the experiment to be carried out. E asked for volunteers who would watch over and measure some maize plants during the following week. A brief questionnaire similar to the one used in the mouse study was administered and the students asked to complete it. All eleven students volunteered for the experiment.

There are 6 females and 5 males. Two of the females share one planting box, therefore there are 10 experimental units. There are 4 goats (3 males and the 2-female team) with the remainder being sheep. Three of the sheep reported a personal experience in which they felt that they had helped a sick plant, animal or person to get better through their thoughts or prayers. Only one of the six believed she had a green thumb.

Preparations

From a large pool about 50 corn seeds were drawn, weighed and arranged in a row on a table in order of increasing weights. Seeds from the larger pool were then visually matched against this size scale and only those which appeared to be between 220 and 300 milligrams were selected. A total of about 400 seeds was collected in this way. These were soaked in a 2% saline solution at room temperature (21 C) for 9 hours, washed in tap water and dried on a paper towel. They were then brought to the psychology laboratory for weighing and randomizing by four persons not otherwise involved in the experiment. Three hundred sixty (360) seed packets were prepared, half green and half yellow, and numbered 1 through 180 for each color. These were put into an open plastic container and mixed well. S randomly drew a packet from the container while, simultaneously, B

drew a seed from the pool. B weighed the seed on an electronic scale and called out the weight to S. S wrote this weight on a record sheet on the line corresponding to the number and color of the packet. Seed and packet were then placed on the table and M or H took them, inserted the seed securely in the packet and placed this on another table. When the 360 packets had been used, the individual packets were put into envelopes in groups of 18 by number. Envelope A1 contained seeds 1 through 18 in green, envelope A2 contained 1-18 yellow, envelope B1 contained 19-36 green, and so forth. The envelopes containing the green seeds were put into one cardboard box and the envelopes with yellow seeds were put into another cardboard box. Both boxes were closed.

Ten planting boxes were prepared with sand at the psychology laboratory. These were brought to the botany laboratory in a van and arbitrarily placed on the work tables where the botany students would assemble. After checking and correcting the sand levels, the planting boxes were marked sequentially with the letters A through J. The students were assigned the code letters A through J alphabetically by their family names. (For the two person team, the first name on the questionnaire was used for alphabetizing). These letter designations were used to assign planting boxes and seed envelopes to the students.

Procedure

After the students were given instructions regarding the procedures, the "healer" was introduced to the students and he proceeded to perform a simple prayer ritual over one of the two boxes of seeds. When he was finished, E brought the "healed" box to the corner of the room, opened it and poured the envelopes into the other box. These were mixed up with the other envelopes. E then distributed the envelopes to the students (2 each) by code letter.

The students then opened their two envelopes and put the seed packets in two piles in front of their planter, one pile of greens and one pile of yellows. Each student begins by arbitrarily taking one of the yellow packets, recording the number on the packet in the first (leftmost) cell of the planting diagram, and plants the seed in that cell. Planting depth is 0.5 cm and all the students are instructed to plant the seeds in the same orientation. The greens and yellows are alternated until all the seeds have been planted and then the planter

is carried to the germinating room, about 50 meters away from the work room. One shelf in the germination room is reserved for the ten planters and they are arranged on it with Dr. Konings assistance, to assure uniform spacing and lighting. Each student pours 600 milliliters of water into the holding tank of his planter and the thermometer and cover are installed.

During the week, each student is asked to check on his planter daily, recording the date, time and temperature in the planter on each visit and making marks in his notebook diagrams to indicate which plants are sprouting. When the plants are large enough, he must also measure the height of each one to the nearest half centimeter and record these in his notebook.

On the fourth day, the students harvest, measure and weigh the plants and place the plant parts in numbered containers (paper dishes) ready for drying. The completed notebooks with all the student's recordings is then returned to Dr. Konings for E to retrieve.

Analyses

The students measure the total height and nodal height of each plant and weigh the roots and then the shoot with the seed. The roots are put into one drying cup and the shoot and seed in another. The cups are prenumbered. E then puts the cups into the oven for 24 hours and makes the dry weighings.

The statistical analysis consists of a 3-way ANOVA with two levels for illness expectancy (salted, plain), two levels for healing expectancy (healed, non healed) and two levels for belief (sheep, goat). This is conducted using total plant fresh weight, dry weight and height, in each case covarying the preplanting seed weight. The partial weights (roots, shoot plus seed) and the nodal height will be analyzed for exploratory purposes. Individual 2 factor ANOVA's, as in the previous experiments, will be produced for each handler separately to assess individual effects using the same main measures indicated above.

In addition, a score will be assigned to each handler for each expectancy condition that is based upon the F-value for the respective main effects from the individual ANOVA's. The square root of this

value will be used and the direction of the effect will be indicated by prefixing this with a plus or minus sign. A plus sign will indicate a difference in the direction of the induced expectancy and a minus sign indicates the direction opposite the induced expectancy. A Pearson correlation will be calculated for the two expectancy scores across the handlers.

Sheep and goats are determined as in the mouse experiments (Solfvin, 1982) on the basis of the responses on the pre-experimental questionnaire. The questionnaire was adapted from the one used in the mouse studies. Students scoring 3 or more are considered sheep while scores of 1 or less are designated as goats. All 11 students fit into one of these categories with 6 being sheep and 5 goats. Two of the goats worked together on a single planting box and are considered as a single subject for this experiment.

Hypotheses

It is hypothesized that there will be a main effect for the healing expectancy condition, a main effect for the illness (salting) expectancy condition, or an interaction between them.

For the belief variable, it is hypothesized that there will be an interaction of this variable with the healing expectancy, the illness expectancy or both.

For the correlation between the expectancy scores, a positive correlation is hypothesized based on the mouse experiments but a significant negative correlation would also support the contention that these two factors are related.

RESULTS

The results of the 3-way ANOVA, in summary form, are presented below in Table 11. The average growth measures for each condition (cell) in these ANOVA's are presented in Table 12.

TABLE 11
Experiment 6
Summary of ANOVA's. Covarying Seedweight

Source	DF	Total Height	Fresh Weight	Dry Weight
Salt(S)	1,293	.801	1.411	2.084
Heal(H)	1,293	.693	1.466	.004
Belief(B)	1,293	55.311d	6.289b	3.165a
SxH	1,293	.170	.052	.035
SxB	1,293	.220	.007	.063
HxB	1,293	.265	.297	.660
SxHxB	1,293	.984	.517	.794

a: $p < .10$; b: $p < .05$; d: $p < .001$

DISCUSSION

All of the three hypotheses for this experiment were rejected. The only significant difference found was for the "belief" variable. The plants that were grown by the sheep were significantly smaller than the plants grown by the goats. The size and generality of this difference assures us that it is not a chance fluctuation in the natural growth variability of the plants but the question of what caused this difference is another matter.

It is possible that the difference was due to psychological factors mediated by handling of the plants and planting boxes. The degree of roughness with which the students handled the plants was not controlled for in this experiment. This result is somewhat reminiscent of the mouse studies reported earlier where one handler (a goat) showed significantly higher babesia levels in his mice than did the other two handlers (both sheep) in that experiment. It was suggested there as well that handling may have played a role and that either his

TABLE 12
Experiment 6
Average Growth Measures by Condition

	SHEEP						GOATS					
	Total		Fresh		Dry		Total		Fresh		Dry	
	Height		Weight		Weight		Height		Weight		Weight	
	slt	pln	slt	pln	slt	pln	slt	pln	slt	pln	slt	pln
Heal	3.6	3.6	717	706	238	235	4.7	5.0	788	790	250	243
Nonheal	3.9	3.7	735	716	238	231	5.3	5.0	834	798	250	240

Note: Heights are in centimeters; weights are in milligrams

"goatishness" or his lack of interest in the experiment may have been instrumental.

On the other hand, the artifactual problems that have been noted throughout this plant series raise doubts as to the meaning of such a significant difference as found here. It was pointed out in the discussion of experiments 4 and 5 that plants under stress may be more liable to artifactual influences. For example, the compaction of the sand in the planting boxes or the amount of water that comes up through the capillary tubes to the cells may exert unusually large influences on these salt-treated seeds, especially during this early growth period.

In this experiment there is some evidence of such uncontrolled variations. Five of the students (3 sheep, 2 goats) reported a total of 7 "dry cells", cells which appeared to be dry relative to the other cells in the planting box during the first and second days of the experiment. Of these seven cells, three never germinated and while the four that did achieved an average height of 1.4 centimeters, quite stunted compared to the other plants. While the plants from these cells were eliminated from the statistical analysis, they suggest that

other plants which were not eliminated may also have received differential amounts of moisture which may have had relatively large effects upon their growth. Thus, it is possible that this artifact coupled with other environmental and handling factors and with coincidental matching of the stunted plants with the sheep, could account for the sheep-goat effect observed here.

We cannot eliminate the possibility that this difference has psychological import but, because of the illness model, we also cannot exclude the artifactual explanations. Moreover, since there is no precedent for such an effect as this in the literature and since it wasn't hypothesized, this finding is taken as further evidence of the problematic nature of the specific plant illness model chosen here.

Finally, the correlations between the two "expectancy scores" (see analysis section) for the handlers were calculated, despite the fact that it is assumed that these are dependent upon significant results for the main factors. No significant correlations were found between the two expectancies for either the height, fresh weight or dry weight of the plants.

OVERALL DISCUSSION AND CONCLUSIONS

The results of this series of experiments with the illness model suggest that the early growth of corn (maize) seeds which have been soaked in saline solution is a poor model for assessing the effects of the expectation structure, sensory or otherwise. It may be that there are no such effects on plant growth but we cannot even be certain of that. In the first place the natural variability in the heights and weights of the plants may be too great for these apparently subtle effects to manifest statistically. Moreover, the possibilities of artifactual interference by a variety of environmental factors suggests a good deal of caution in interpreting positive results in experiments of this sort, even when large samples and careful randomization procedures are employed.

In every experiment in this series there has been at least one marginally significant result which might have been interpreted as suggestive of the expectancy or mental healing effects which are the focus of this study. However, the series as a whole has shown no

consistent pattern which could raise our confidence that they were the result of a predictable process. Moreover, alternative explanations for these results can be found in the variety of artifacts that are suggested by the data.

The sheep-goat effect in the final experiment in this series is an example. The effect may have been due to psychological factors but the point is that we simply cannot be sure. Strong F-values in an analysis where these artifacts are at least possible cannot raise our confidence that the F-value has meaning regarding the sorts of effects we are researching. Further, it is concluded on the basis of this series that we are not likely to achieve interpretable results with this plant growth model in the future, because some of the difficulties seem inherent to the model itself.

There is a deception in the apparant simplicity of using plant growth models to assess the effects of mental healing or psychokinesis. In fact, the growth of plants, especially during the germination and seedling periods, involves a complex network of chemical-physical processes which are not yet entirely understood. A plant has a more intimate relationship with its environment than an animal does. There is a constant interchange of energy and matter occurring between the plant and the soil and air. Recent studies of the effects of auxins (Evans, Mulky and Vosper, 1980) and ethylene (Konings and Jackson, 1979) on the growth of plants illustrate the intricate processes that are detectable only with sophisticated instrumentation and years of study. Even these treatments have variable effects depending upon a variety of endogenous and exogenous factors. In some circumstances auxin or ethylene will promote growth while in others they will retard it. With plants, perhaps more than with other living organisms, we are seeing the delicate and intricate balance of nature in its fullest sense.

With "ill" or stressed plants, such as when the seeds have been treated with saline, we must ask what effects the stress has on this balance. In Greenway and Munn's (1980) review of the mechanisms of salt tolerance in nonhalophytic plants (footnote 3) we find further indications of the contention raised in experiments 4 and 5 that stressed plants become more sensitive to environmental influences than normal plants. For example, when nonhalophytes are grown in saline waters their growth is inhibited but the inhibition is less in high humidity. Thus we must wonder whether even the respiration and perspiration of a person who regularly handles the saline treated

plants in our mental healing experiments might be yet another source of artifact. We of course do not know whether this is even possible, but how can we be sure that it is not?

This is a common thread that runs through treatment effect research with diseased or stressed organisms. Stressed plants, like stressed persons and animals, become sensitive to a variety of stimuli to which they are normally robust. Plant illness models, because plants are so dependent from moment to moment upon environmental factors which are difficult to control, must be used in this research with great caution.

For future research with expectancies, mental healing and psychokinesis it is suggested that animal illness models in well-controlled experiments may be less problematic than the plant growth models. If further research with plants is to be conducted, it should be preceded by extensive pilot work to determine the distributions of the various measures of plant growth and the effects of environmental factors on these distributions. At least three pilot or control studies should be conducted for every experimental one. Nonparametric statistical evaluation should be used as further assurance against unanticipated distributional problems. Matching is to be preferred to randomizing for assigning seeds or plants to the conditions as well as for assigning the planting boxes to positions in the room or area where they will be grown. Handling of the plants during their growing periods is to be eliminated or carefully controlled. Unless convincing evidence to the contrary is supplied, we must assume that the handling of the planting containers at any time during the experiment can exert an artifactual influence, even when the handler is blind to the experimental conditions.

It is further suggested that future studies of the effects of expectancies, mental healing treatments or psychokinesis on plant growth processes should avoid the highly variable early growth periods. It may be more suitable to focus on the growth of matured, healthy plants instead. In this way, problems associated with the quality and weight of the seed and the cellular differentiation and rooting processes going on in these early stages may be avoided. Even in this case, though, the above mentioned recommendations for plant growth studies should be observed.

FOOTNOTES

1. It was my impression that Mrs. K is quite certain that her husband has a special gift for healing. She seemed more confident than he that the seeds could be effected. Both of them expressed an attitude of open-mindedness towards the outcome and seemed more interested in the prospect of learning something from these experiments than of 'proving' something.

2. This was suggested by Prof. Dr. M.U. Johnson of the Parapsychology Laboratory, University of Utrecht, The Netherlands.

3. Halophytes are the native flora of saline waters.

ABSTRACT

A series of six experiments was conducted to assess the possible effects of mental healing treatments and the expectations that such treatments were administered on the growth of corn seedlings. The first experiments showed results suggestive of treatment and expectancy effects. However, further experimentation turned up several artifactual and methodological problems inherent to the 'sick' plant model. These problems are discussed and it is concluded that they confound the interpretation of the results of the existing studies of mental healing or psychokinesis treatments applied to plant materials. Recommendations are made for future studies with the plant growth model.

ACKNOWLEDGMENTS

I am deeply indebted to Prof. Dr. M.U. Johnson and Dr. H. Konings for their insightful criticisms and comments and for their general support during this series. Thanks also to Brian Millar and Sharon Solfin for their suggestions at crucial points and to Mieke Spronk and Henk Evers for their many hours of weighing seeds and setting up the planting boxes. A special thanks goes to Mr. and Mrs. K for their

participation and to the botany students who assisted in this project.

REFERENCES

- Barry, J. "PK on fungus growth", J.o.P., 1968, 32, 55. (abstract)
- Barry, J. "General and comparative study of the psychokinetic effect on fungus culture", J.o.P., 1968, 32, 237-243.
- Barry, J. "Journal d'un parapsychologue", Evraux, France: Editions et Publications Premieres, 1971.
- Brier, R.M. "PK on a bio-electrical system", J.o.P., 1969, 33, 187-205.
- De la Warr, M. "Plant experiments - series 2", Newsletter of the Radionics Centre Organization, summer, 1970, 1-72.
- Evans, M.L., Mulky, T.J., Vesper, M.J. "Auxin action on proton influx in corn roots and its correlation with growth", Planta, 1980, 148, 510-512.
- Grad, B. "A telekinetic effect on plant growth", International Journal of Parapsychology, 1963, 5, 117-133.
- Grad, B. "A telekinetic effect on plant growth II", International Journal of Parapsychology, 1964, 6, 473-498.
- Grad, B. "The laying-on-of-hands: implications for psychotherapy, gentling and placebo effect", J. A.S.P.R., 1967, 61, 286-305

Greenway, H., Munn, R. "Mechanisms of salt tolerance in nonhalophytes", *Annual Review of Plant Physiology*, 1980, 31, 149-190.

Haraldsson, E., Thorsteinsson, T. "Psychokinetic effects on yeast: an exploratory experiment", *Research in Parapsychology* 1972, Metuchen, Scarecrow Press, 1973, 20-21.

Harari, S.B. "A pilot study of the effects of psychically treated saline solution on the growth of seedlings", Unpublished manuscript, Psychical Research Foundation, Durham, North Carolina, 1975.

Hickman, J.L. "Plant growth experiments with Matthew Manning", In J. Mishlove (ed) *A Month with Matthew Manning: Experiences and Experiments in Northern-California during May-June, 1977*. San Francisco: Washington Research Center, 1979, 74-76.

Konings, H., Jackson, M. "A relationship between rates of ethylene production by roots and the promoting or inhibiting effects of exogenous ethylene and water on root elongation", *Zeitschrift fur Pflanzenphysiologie*, 1979, 92, 385-397.

Lenington, S. "Effects of holy water on the growth of radish plants", *Psychological Reports*, 1979, 45, 381-382.

Loehr, F. "The power of prayer on plants", New York, Signet, 1959.

McDonald, R.G., Hickman, J.L., Dakin, H.S. "Preliminary studies of three alleged 'psychic healers'", *Research in Parapsychology* 1976, Metuchen, Scarecrow Press, 1977.

Miller, R.N. "The positive effect of prayer on plants", *Psychic*, 1972, 3, 24-25.

Munson, R.J. "The effects of PK on rye seeds", J.o.P., 1979.
(abstract)

Nicholas, C. "The effects of loving attention on plants", New England Journal of Parapsychology, 1977, 1, 19-24.

Pauli, E. "PK on living targets as related to sex, distance and time", Research in Parapsychology 1972, Metuchen, Scarecrow Press, 1973, 68-70.

Shafer, M., McBeath, M., Thalbourne, M., Phillips, P. "PK experiments with two special subjects", Unpublished manuscript, McDonnell Laboratory, Washington University, St. Louis, Missouri, 1982.

Solfvin, G. "Psi expectancy effects in psychic healing studies with malarial mice", E.J.P., 1982, 4, 2, 159-197.

Vasse, P., Vasse, C. "Influence de la pensee sur la croissance des plantes", Nouvelle Serie, 1948, 2, 87.

Vasse, P., Vasse, C. "Influence de la pensee sur la croissance des plantes", Nouvelle Serie, 1950, 12, 223.

Vasse, P., Vasse, C. "Influence de la pensee sur la croissance des plantes", Nouvelle Serie, 1954, 28, 62.

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MATHEMATICAL INFERENCE STRATEGIES VERSUS PSI:
INITIAL EXPLORATIONS WITH THE PROBABILISTIC PREDICTOR PROGRAM

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Classical testing procedures in parapsychological research involve (a) the randomization of a sequence of numbers or items (the target sequences); (b) responses by a percipient as to what he believes the order of the target sequence is in the lack of any known sensory cues (the calls); (c) a comparison of the number of correspondences between corresponding temporal calls and targets in the target and response sequences (scoring); and (d) a computation of the probability of getting the obtained or a greater number of such correspondences or hits, given the a priori chance probabilities of a hit on any particular trial and the standard assumptions made for the statistical tests used.

While it has long been recognized that a given percipient's response sequence is usually quite non-random, this lack of randomness in the response sequence is irrelevant to testing the null hypothesis of no information transfer about the target sequence, via ESP, to the percipient. Randomicity of the target sequence, on the other hand, is a necessary assumption for the standard statistical tests used to be valid, so it has generally been believed that lack of randomness in the target sequence may invalidate the results of our statistical tests.

The effects of non-randomicity in the target sequence may manifest

in either of two ways. First, consider the case where there is no feedback given about individual target identity to a percipient before he or she has completed all of his calls to a target sequence from a uniformly biased source. It is possible that if a particular percipient's global response biases just happen to match the biases in the target sequence, a 'statistically significant' hit score will result, but this is not due to any ESP but rather to what we, in retrospect, realize is an incorrect specification of the a priori probability of hits. Control scoring can usually handle this global bias problem. In the second case, if feedback of target identity is given to the percipient after each call, we can postulate that an occasional percipient may gradually assess what the biases in the target sequences are by keeping track of what previous targets have been and adjust his response pattern accordingly, that is, he may develop a mathematical inference strategy, thus artifactually inflating his score and invalidating the standard statistical test of the null hypothesis (note 1). The specifics of this second possibility have not been adequately explored to date in the literature.

RANDOMICITY

To assess the practical significance of a mathematical inference strategy for artifactually inflating hits, we need to examine the concept of randomness more closely. Randomness is conventionally taken as meaning a lack of order, but this definition can be misleading without further examination. It is generally agreed among mathematicians (see, for example, Chatian, 1975) that you may take any sequence of numbers generated by a truly random generator, and in retrospect, construct a mathematical algorithm that, when expanded, gives the exact set of numbers seen in this particular sequence. The sequence may be any length, from half a dozen digits to millions. While constructing the algorithm becomes much more complex for longer sequences, it can, in principle, be done. We postdict the sequence. This seems to imply that the sequence was not generated in a random fashion after all, but by a complex but completely deterministic and lawful generator whose operating characteristics are described by the particular algorithm.

The catch, however, is that the algorithm you have constructed for a sequence of N numbers will not predict subsequent numbers obtained from the same random source on trials $N+1$, $N+2$, etc. with any greater

accuracy than would be predicted by chance. To put it in more general terms, we may come up with a set of concepts which seem to account for the generation of any observed set of events or numbers in retrospect, but while we can always imagine such concepts, they do not necessarily have anything to do with the actual mechanism of generation of the events or numbers. This is the mathematical version of the psychological concept of rationalization: we can always imagine patterns in retrospect. The concept of randomness being a lack of order or pattern per se then is not really accurate, as some kind of pattern can always be projected in retrospect.

The more important property that we imply by randomness in parapsychological (and general scientific) research is that randomness means lack of predictability. Given a sample of numbers of length N from a random number generator (RNG), if your generator is truly random, your knowledge of these N numbers does not help you predict the output on further trials $N+1$, $N+2$, etc., with greater than chance expectancy. This lack of predictability is what is crucial for parapsychological testing. Given feedback to the percipient about previous target sequences, if we see any above chance scoring we want to be able to attribute it to some kind of ESP, rather than to some kind of mathematical inference strategy which the percipient might have worked out, consciously or unconsciously, from the sample of target numbers presented to him to date. Postdiction, of course, is not prediction.

Ordinarily, if the target sequence used in an experiment meets the conventional tests of randomness we usually assume non-predictability. These conventional tests of randomness would usually be a test for equiprobability of each possible target number (target singlets), and of serial independence of target generation, measured by tests of the equiprobability of each possible sequential pair of numbers of the target sequence (target doublets), each possible sequential triplet (target triplets), etc.. Given the usual experimental ways of generating random numbers, such as thorough shuffling of cards or electronic RNGs, where we do not have any theoretical reason to suspect higher order patternings at the triplet, quadruplet, and higher levels, testing for singlet and doublet bias is ordinarily considered sufficient. Pseudo-random generators require more complete testing, and are rarely used in parapsychological research.

REJECTING DATA

Naturally we should always strive to have acceptably random target sequences, but suppose we carry out a parapsychological experiment and discover, in retrospect, that some of our target sequences were significantly nonrandom by the above-mentioned tests of equiprobability of singlets and doublets? A frequent reaction is that such lack of randomness of the target sequence somehow totally invalidates the experiment, so all the data are lost. Psychologically, this is an understandable reaction, for parapsychologists have often been subjected to such severe (even if often unfounded) methodological criticisms that they have become hypersensitive to departure from accepted experimental requirements.

We will argue that we need not necessarily reject data from experiments where a small degree of target non-randomicity is retrospectively discovered. We suggest conducting a powerful, empirical test of the degree to which this non-randomicity might allow a mathematical inference strategy by the percipients to account for results. If the results attainable by this test are considerably smaller in magnitude than those that were actually obtained in the experiment, and if the remaining data are significantly extra-chance, it is legitimate to argue that ESP was operating. We shall describe the algorithm we have developed for such a test, which we call the 'Probabilistic Predictor Program', and apply it to the results of an actual experiment.

MATHEMATICAL INFERENCE STRATEGIES

In an experiment where a percipient receives trial by trial feedback, and where there are small but statistically significant biases in the target sequence, how would he go about making use of these biases to raise his hit score? To simplify things, let us assume that the percipient never uses any kind of ESP, but only mathematical inference. Let the target sequence consist of the numerals zero to nine. We will postulate that our random generator contains small biases, such that one or more singlet targets have a probability slightly greater than 10%, and that some doublets, triplets, etc., may have biases resulting from singlet biases and/or independent biases in addition to those resulting from a reflection of singlet biases (note

2).

For his first call, our percipient's response must be a pure guess: he has no information yet about the target RNG to make any estimate of its biases from. Having made his guess response, he obtains feedback and finds out what his first target was.

To make his second call, his estimate of the biases in the target RNG must be based on a sample size of one, and we would not expect him to be able to make a very useful estimate of small target sequence biases from such a small sample. As he sees more and more of the target sequence, we can imagine him more or less accurately remembering whether some particular target singlet or singlets seem to appear very frequently, whether some particular target doublets seem to appear very frequently, etc.. By the time he has a sample of say, 100 targets, he can make a better estimate of possible biases in the target sequence than when he had a sample of one. By the time he has a sample of 1,000 targets, his estimates are even better, etc. (ignoring for the sake of simplicity, the possibility of forgetting and confusion in his memory). The larger the number of trials he has had feedback on, the more adequately he might be able to work out some sort of strategy where he might usually call, say, a five, because so far he has observed fives occurring with a frequency of two-tenths rather than one-tenth. He might supplement his singlet strategy by observing and deciding that threes were frequently followed by sevens, etc.. If his estimations of the biases in the target source are accurate he can, in principle, score above mean chance expectation.

Note that the potential success of such a mathematical inference strategy is related to the degree of bias in the target RNG and the accuracy of the percipient's perception of these biases. If there are very strong biases, such as a particular singlet target having a probability of one-half, and all the other possible singlets having much smaller corresponding probabilities, or if, for example, twos are always followed by ones, then our percipient should be able to begin scoring significantly above chance with a relatively small sample of the target sequence, say, after 25 or 50 trials. If, at the other extreme, the departures from an equiprobability and serial independence model in the target sequence are very small, say, one or two singlet targets having probabilities of .15 rather than .10, it would take a much longer sample of the target sequence to begin to get any accurate idea of these departures, and it is quite possible that forgetting and confusions in memory over very long samples of the

target sequence might effectively rule out any practical use of such a mathematical inference strategy for many percipients. We shall examine this in more detail below.

In attempting to estimate how much percipients in general might be able to actually use some sort of mathematical inference strategy, we get into a murky area. At one extreme, we find rare cases of mathematical or memory prodigies or idiot savants who can do very striking and unusual things with very complex numbers, which leads us to think that almost any kind of conceivable mathematical computing ability might be possessed by some person somewhere. Balanced against this extreme possibility is the fact that in almost all cases of people we actually come in contact with, they can seldom remember more than 10 digits or so accurately. The limit of the 7-digit telephone numbers, which are difficult for quite a number of people to remember, is a practical example of this. Thus in practice we would expect most percipients to have considerable difficulty keeping very accurate track of even the singlet distribution of the target sequence, much less the doublet, triplet, and higher order distributions, after more than a few dozen or so trials. Actual experimental data on what people can judge about large masses of serial, numerical data would, of course, be useful in estimating human capacities more precisely.

INADEQUACY OF CLASSICAL BIAS TESTS

Before considering our predictor program, it is important to note that standard tests for non-randomicity of sequences do not necessarily indicate how reliably a mathematical inference strategy could provide significant results in an ordinary length experiment. This will be illustrated by the following examples. We will use only singlet level bias, but the same arguments hold for higher level biases.

Suppose a percipient works in an experiment involving guessing the numbers one to ten, and he makes a total of 200 responses. We shall deliberately use a biased target source, such that outputs 1, 2, 3, 4, and 5, all have a probability of .15, while the outputs 6, 7, 8, 9, and 10 have a probability of .05, instead of all targets being equiprobable. The observed distribution of targets in the first half of our experiment (100 trials) would look like this, deliberately giving it a perfect reflection of the target bias pattern for

simplicity of illustration:

TARGETS	1	2	3	4	5	6	7	8	9	10
FREQUENCY	15	15	15	15	15	5	5	5	5	5

The standard Chi-square test for equal frequency of observed targets would tell us that this sample of 100 is not from a random source, as we get a Chi-square of 25.00 with 9 degrees of freedom, $p < .01$, one-tailed.

Suppose our percipient takes the first 100 trials to catch on to this singlet bias pattern, so that while he has only scored the 10 hits expected under our assumed equiprobability model in the first 100 trials, he will use a mathematical inference strategy, based on his new knowledge, for the remaining 100 trials of the experiment. His best strategy is to guess a 1, 2, 3, 4, or 5 on every trial. It does not matter whether he picks one of the high five and always guesses it, or randomly alternates among the high five: we would expect him to score about 15 hits in the second 100 trials. If we assess the significance of this score under our assumption of equiprobability, we compute a CR of 1.67, $p < .05$, one-tailed. For the whole experiment of 200 trials, we now have $(10+15)=25$ hits with an associated CR of 1.18, which, while not reaching statistical significance, might suggest to an experimenter that something was happening.

If the experiment was longer than 200 trials and the bias pattern and mathematical inference strategy were consistent, the percipient could obviously attain conventional levels of significance as he went further. For 300 trials, for example, we would have $(10+15+15)=40$ hits, for a CR of 1.92, $p < .05$, one-tailed. We shall stay with a 200 trial experiment for now, however, to illustrate certain points.

Now consider another target source with a quite different sort of bias, where we observe the following distribution of targets in the first 100 trials:

TARGETS	1	2	3	4	5	6	7	8	9	10
FREQUENCY	24	10	10	8	8	8	8	8	8	8

This generator is highly biased toward producing ones, with no other large biases. The Chi-square test for equiprobability of singlets for

this distribution gives a value of 22.40, $p < .02$, one-tailed. If we mistakenly assumed that the magnitude of the Chi-square values reflected the degree of predictability of these target sources for a mathematical inference strategy, we would think this target source was equally or slightly less predictable than the source in the previous example. We would be quite wrong.

Again assume that it takes our percipient the first 100 trials to catch on to the bias, so he only scores 10 hits in the first 100. Now he follows the optimal strategy of calling a one for every one of the remaining 100 trials, and scores about 24 hits! For the second 100 trials alone, this gives a CR under the equiprobability model of 4.67, $p < .0001$, one-tailed. For the whole experiment of 200 trials, we have $(10+24)=34$ hits, with a CR of 3.30, $p < .0005$, one-tailed.

For equal Chi-square values in tests of bias, two sequences may differ enormously in usefulness for a mathematical inference strategy. Further, there will be far more possible bias patterns of less usefulness for a mathematical inference strategy than there will be highly useful ones for a given Chi-square value: there are many ways to rearrange the bias pattern in our first example without giving it the single number peak bias pattern of our second example that is so useful in a mathematical inference strategy.

Consider a third example where the following frequencies of targets are observed in the first 100 trials:

TARGETS	1	2	3	4	5	6	7	8	9	10
FREQUENCY	18	10	9	9	9	9	9	9	9	9

The standard Chi-square test of equiprobability tells us that this is not a biased sequence, for Chi-square is only equal to 7.20, which would occur more than half the time by chance alone. Yet if our target source is really biased toward ones in this way, and our percipient decided to call all ones in the second 100 trials of the experiment, he could make 18 hits there, for a CR of 2.67, $p < .01$, one-tailed, and a total of $(10+18)=28$ hits for the whole experiment, $CR=1.89$, $p < .05$, one-tailed. It is especially interesting to note that the entire sequence of 200 targets for this percipient, with the bias continuing through the second 100 trials, still does not show any significant bias: Chi-square=14.40, $p < .10$, one-tailed.

We may conclude the following for standard Chi-square tests of bias. For long to infinite length experiments:

(1) lack of significance Chi-square values in bias tests probably indicates lack of significant predictability by a mathematical inference strategy; and

(2) the presence of significant Chi-squares in bias tests indicates some degree of predictability by a mathematical inference strategy, but the magnitude of Chi-square does not indicate the degree of predictability.

For short to moderate length experiments of the type frequently carried out, however:

(3) a significant Chi-square indication of bias does not necessarily indicate that a significant overall score can be obtained through a mathematical inference strategy;

(4) the magnitude of the obtained Chi-square is a very poor measure of the magnitude of results that can be obtained with a mathematical inference strategy; and

(5) a mathematical inference strategy may produce significant results from a biased source which does not appear to be significantly biased by Chi-square evaluation.

The shortcomings of standard Chi-square measures of bias in realistic length experiments further illustrate why more direct measures of predictability by mathematical inference need to be developed.

THE PROBABILISTIC PREDICTOR PROGRAM

Our interest has been in carrying out an extreme test of the hypothesis that human percipients might use mathematical inference strategies to capitalize on non-randomities in a target sequence instead of using ESP, so we set out to develop a mathematical inference strategy which, as far as we can tell, would be far superior to anything we could expect from a typical or even an exceptional human being. Our strategy program, for example, (a) never forgets or get confused about what previous targets in a target sequence to date have been; (b) has precise and accurate ways of estimating such things as whether, e.g., five to seven occurrences of one particular doublet in the first 100 trials should carry more weight as to what to call than a 15% proportion of a specific singlet appearing; (c) never gets tired or becomes prone to error; and (d) is not intellectually or

emotionally attached to a previous successful strategy, but constantly updates itself against new information from the target sequence.

We cannot claim that the human mind could not have some unknown, fantastic computing powers that might be superior to our inference strategy, but we do claim that our procedure looks to be far more powerful than anything we could reasonably expect to find in a typical human being. Until a more powerful mathematical inference strategy is actually modeled and demonstrated to be superior to ours, the procedure we present below constitutes an extremely powerful test of the hypothesis that percipients might artifactually inflate their hit scores through a mathematical inference strategy.

We have named the mathematical estimator strategy we have developed the 'Probabilistic Predictor Program' (PPP), and have implemented it on the Control Data Corporation Model 6400 computer at the University of California Berkeley campus. Its basic mode of operation on each trial is to (a) consult storage registers about what targets have been on all trials previous to the current one; (b) compute the probabilities of the observed target frequencies to date given an assumed equiprobability and sequential independence model; (c) use the largest deviation from this model to estimate the most likely possible biases in the target RNG and respond accordingly; and (d) update all these steps with the feedback on target identity for that trial in preparation for the next trial. We shall illustrate the operation in detail for seven trials, using an exceptionally biased source which generates only the sequence

12341234123412341234123..... etc.

even though we assume an RNG which generates the digits zero through nine independently and with equal probability. Using such an exceptionally biased source for our example allows the features and power of the PPP to be demonstrated in a few trials.

Trial One

Since there is no data in the PPP storage registers about the target distribution, except the assumption that any of the target digits zero to nine are equally probable, the PPP consults an RNG program within the computer and produces a digit from zero to nine as its first response. It might, for example, make a response of five. It now consults the appropriate storage register to pull out the first digit

of the target sequence, the equivalent of immediate feedback to a human percipient in a comparable situation. In our example the first target was a one. The PPP scores its response against the target, in this case a miss, and it notes in its singlet storage register that the digit one has occurred once in a total of one trial.

Trial Two

In order to estimate possible deviations from the assumption of every target digit having an equal probability of .1 and serial independence, from this trial on, the PPP begins consulting its storage registers for information about the target sequence on previous trials. For the second trial, the only register with any data in it is the singlet register, and it tells the PPP that one has been the most frequent singlet to date. Because it will be necessary in later trials for making decisions, although it is not really necessary on this second trial, the PPP computes the exact cumulative, one-tailed binomial probability of the most frequently observed target to date (a one in this case) having appeared one time in one trial, given the assumption of a probability of .1 for any possible digit. In this case the exact binomial probability is .1, one-tailed. The general decision rule now followed by the PPP is to always use the most significant departure from the equiprobability and serial independence model as the estimator of the most likely target to come up next in a target RNG that is actually biased. The lowest probability observation, the maximum departure from the model among several comparisons, is the most important. We have only one figure this time, namely the probability of .1 for a one, so the PPP responds with a one as its call for the next target.

The PPP now consults the second digit of the target series for its immediate feedback. In our example this digit was a two. The PPP again scores a miss, stores the fact that a two has occurred once in the singlet storage register (which is already storing the fact that a one has occurred once), and also stores the fact that a two has followed a one in a total of one (doublet) trial in the doublet register.

Trial Three

The PPP again consults its storage registers. In its singlet register it finds that both a one and a two are tied for the highest frequency of occurrence, namely each at one occurrence in two trials. The exact binomial probability of any one specific number occurring one or more

times in two trials, when the assumed probability of any particular number occurring is .1, is .19, one-tailed. The PPP consults then the doublet register to see if there is any relevant information about doublet frequencies, given that a two has just occurred on the previous trial. While the doublet register indicates that, given a one, a two has followed in one (doublet) trial, it has no relevant information about previous occurrences given a two as the first pair of a doublet. The call decision will have to be based solely on information from the singlet register. Since both one and two are tied for frequency of occurrence (and probability of occurrence) to date as singlets, the PPP randomly chooses one of them; for our example, the number one is its response.

The PPP now consults the third digit of the target series for immediate feedback, which for our example is a three. Its response of one was a miss and is scored appropriately. The fact that a three has occurred is stored in the singlet register, the fact that a two has been followed by a three is stored in the doublet register, and the fact that a one, two has been followed by a three is stored in the triplet register. Our target and response sequence to date is as follows:

Targets:	1	2	3
PPP Responses:	5	1	1

Trial Four

On consulting the singlet register, the PPP finds that the target digits one, two, and three have occurred with equal frequency. The exact binomial probability of any particular digit occurring one or more times in three trials, given our assumptions, is .27, one-tailed. There are no relevant doublet registers and no relevant triplet registers. Since the digits one, two, or three all present equally improbable departures from the model, it randomly chooses one of them, a two in this case. The PPP then consults the fourth digit of the target series for its immediate feedback. The target was a four. A miss is scored; a four is stored as having occurred once in the singlet register; a three, four is stored as having occurred in the doublet register; a two, three, four is stored as having occurred in the triplet register; and a one, two, three, four is stored as having occurred in the quadruplet register. Results to date look as follows:

Targets:	1	2	3	4
PPP Responses:	5	1	1	2

Trial Five

The PPP consults the storage registers and finds that one, two, three, and four have all shown an equal frequency of occurrence, and computes that the exact binomial probability of any particular digit having occurred one or more times in four trials, given an assumed probability of .1, is .34, one-tailed. There are no relevant doublet, triplet, or quadruplet registers, so since the singlet targets one, two, three, and four are equiprobable a random choice is made, a four in this case. The fifth digit of the target series is consulted for feedback: it is a one in this case, and the PPP response of four is scored as a miss. The fact that a one has just occurred is stored in the singlet register and high level strings to date are stored in high level registers, as previously illustrated, to the quintuplet level now. Our targets and responses to date are as follows:

Targets:	1	2	3	4	1
PPP Responses:	5	1	1	2	4

Trial Six

The way more complex decisions are made by the PPP is illustrated as we reach the sixth trial. The PPP first consults the singlet storage registers, and finds that one particular singlet target has occurred more frequently than any others, namely a one has occurred twice in five trials. There are no ties for the most frequent singlet to date. Given our assumed probability of .1, the exact binomial probability of any particular digit having occurred two or more times in five trials is .08, one-tailed. There is now relevant information in the doublet storage register: given a one, a two has followed it on one occasion in a total of four (doublet) trials. Assuming that any particular doublet has a probability of .01 (.1 times .1, given our model of serial independence), then the exact binomial probability of a particular doublet having occurred one or more times in four trials is .05, one-tailed. There are no relevant triplet, quadruplet, or quintuplet indications.

The PPP computes that the probability of .05 is a greater departure from the assumed model than a probability of .08, and so uses the

doublet information to make its decision: a one has occurred which was followed by a two before, a one has occurred again, so a response of two is given. It now consults the target storage register for feedback as to the identity of the sixth target, which is a two in our example, given our repeating target sequence, and it scores this as a hit. It then records the occurrence of a two in the singlet register, of a one, two in the doublet register; of a four, one, two in the triplet register, of a three, four, one, two in the quadruplet register; of a two, three, four, one, two in the quintuplet register; and of a one, two, three, four, one, two in the sextuplet register. Our results to date are as follows:

Targets:	1	2	3	4	1	2
PPP Responses:	5	1	1	2	4	2

HIT

While in principle one could go on constructing higher registers above the sextuplet level, this obviously involves considerable increases in computer space used and computational time, so we made a practical decision to not construct higher registers than the sextuplet one.

Trial Seven

The PPP consults its singlet register and finds that the target singlets one and two are tied for highest frequency of occurrence. The exact binomial probability of any particular digit occurring two or more times in six trials, given our assumed model, is .11, one-tailed. Consulting the doublet register, given that a two has been the previous target, a two has been followed by a three once before. The probability of one particular doublet occurring one or more times in six (doublet) trials is .06, one-tailed. There is also a relevant triplet, namely given a one, two, a three has occurred before. The exact binomial probability of any particular triplet occurring one or more times in five (triplet) trials is .01, one-tailed. There are no other relevant storage registers for this trial.

Since the p value of .01 represents the greatest departure from the equiprobability and serial independence model and is thus our best estimate of possible biases in the target sequence, the PPP chooses to respond on the basis of the triplet register with a three. The extreme probability values that are quickly generated by doublet and higher order biases make the PPP capable on capitalizing on short term sequential biases, as well as long term biases. Consulting the target

register for feedback on the identity of the seventh target, we find that it is a three so a hit is stored, appropriate entries are made in the appropriate registers, etc.. Our results to date are as follows:

Targets:	1	2	3	4	1	2	3
PPP Responses:	5	1	1	2	4	2	3
						HIT	HIT

There is no need to continue to illustrate the functioning of the PPP at this point, as all the basic principles have been outlined. Notice that for this highly biased target sequence of a repetition of the series 123412341234..... the PPP has started hitting by the sixth trial and will make a perfect hit score from this point on.

APPLYING THE PPP TO EMPIRICAL DATA

Two kinds of sources for the generation of sequences to be tested by the PPP have been used to date, both drawn from an experiment (Tart, 1975; 1976a; 1977c), the first Training Study (TS), designed to assess the effects of immediate feedback on GESP performance (note 4). One source of sequences was the target sequences used in that experiment, which were generated by an electronic RNG; the other was the response sequences of the 10 percipients in that experiment. These 10 percipients were preselected for ESP talent from a much larger pool.

The electronic RNG was of the 'electronic roulette wheel' type, where an oscillator operating at a frequency of approximately five megahertz drives a zero-to-nine counter over and over again. The length of time that the experimenter holds down a pushbutton determines how long the oscillator is connected to the counter, and thus the final output that the counter stops at. Since the speed of the oscillator/counter combination is at least four orders of magnitude faster than human reaction time, as well as much faster than random variations in pushbutton pushing performance we would expect (from factors like variations in neural firing times and muscular reaction patterns) that the output of this type of generator would be random, with each output equiprobable and the outputs serially independent. A more complete description of the RNG is given in the original publications (Tart, 1975; 1976), although the circuit diagram there is incorrectly drawn: anyone wishing to build this RNG should contact C.T.T..

The zero-to-nine output of the RNG was displayed on a seven-segment Litronix Data Lite 10, which was then read by an experimenter and copied down on a data sheet. Occasional transcription errors might occur, although we would expect them to be rare for such a straightforward task. Responses of the percipients were also recorded manually on the data sheets by the experimenters, so occasional transcription errors were also possible here. We do not believe any significant number occurred, however, for the number of hits in a run of 25 trials was recorded automatically on a electro-mechanical counter, and the hit total of this counter had to be equal to the number of hits recorded by hand on the data sheet.

We would expect the response sequences of the percipients to be far more biased than anything generated by an electronic RNG, especially in terms of sequential biases, so they will constitute an especially interesting test sequence for the PPP.

The experimental design called for each of the ten percipients to make exactly 500 responses (in 20 runs of 25 trials each), but the number of usable targets for PPP analysis of target and response sequences varied slightly from this nominal 500 in a number of cases. On a few occasions the written record of a particular trial or response was ambiguous, so it was deleted. Also, the percipients sometimes used the Pass option (note 5) on the Ten-Choice Trainer and did not make a response to a particular target, so we would have a target digit but no response digit. Although the percipients did not receive feedback on Passes, we conservatively allowed our PPP to use the data obtained from Pass trials as part of its estimation strategy, thus giving the PPP a small advantage (the larger the N, the better the estimate of biases) that the percipients did not have.

RESULTS

In this analysis of PPP performance on target sequences, as well as the following one on guess sequences, we present only the results when the PPP was operated with a memory span of six, that is, it based its decisions on the singlet, doublet, triplet, quadruplet, quintuplet, and sextuplet levels, as this was the most powerful operation of the program in principle. We shall compare performance with shorter length memory spans than the sextuplet later.

The performance of the PPP on the target sequence of the ten percipients is shown in table 1, along with the degree of singlet and doublet bias in the target distributions, as assessed by the magnitude of the appropriate Chi-square tests for equal frequency and serial independence. The target source sequences are ordered from the highest to the lowest level.

As table 1 shows, the target sequences of two percipients, P5 and P3, showed statistically significant singlet bias at the level. The primary source of this bias, by inspection of the raw data, was a lack of XX doublet, i.e., a lack of 11s, 22s, 33s, etc.. Only one of the ten target sequences (for P3) is significant for general doublet bias if the XX doublet lack contribution is deleted. Retrospective questioning revealed that this XX bias was due to experimenter error. Pushing the button to activate the RNG did not have any kind of obvious mechanical snap to the switch, the switch just got more resistant to pushing toward the end of its travel. When an experimenter pushed the switch, let it out, and saw that the target was the same as on the previous trial, an XX doublet, he sometimes thought that he had not pushed the switch in far enough to activate the RNG, so he pushed it again, thus leading to a systematic depletion of the XX doublets. Triplet bias was not tested for, as an N of 500 is far too low to meet the assumptions for a valid Chi-square test.

Although doublet bias that is independent of singlet bias is presented in table 1, it is important to note that for the PPP any high doublet bias is potentially useful for prediction, regardless of whether it is significant independently of singlet bias or only a reflection of it.

The fourth column of table 1 is the mean number of hits per run of the PPP, when the expected chance value is 2.50. Although N (given in the last column) was nominally 500, it was slightly high or low in each case due to occasional extra trials being run by an experimenter, incomplete data, or passes by the percipient, so the means presented in the fourth column were adjusted to a standard run length of 25 trials to facilitate comparison. The fifth column presents the Z-score for hitting of the PPP program. As can be seen, the PPP was able to score significantly above chance on three of the target sequences, but not on the other seven. Only one of these three was a distribution that showed significant singlet or doublet bias by the standard Chi-square test.

Table 2 presents the performance of the PPP on the response

TABLE 1
Performance of Probabilistic Predictor Program
on target sequences

Sequence Source	Singlet Chi-square	Doublet Chi-square	Mean Hit per Run+	Z	N++
P5	38.81	99.08	3.32	2.50*	495
P3	35.53*	137.70*	2.55	.15	510
P4	17.04	97.37	2.95	1.40	525
P2	16.34	107.84*	2.78	.84	513
P14	15.08	104.70*	2.95	1.30	500
P1	13.47	87.81	3.30	2.46*	530
P32	12.12	76.23	3.05	1.70*	508
P17	11.83	101.43	2.30	-.60	501
P11	5.13	75.66	2.48	-.30	501
P7	2.64	93.32	2.48	-.10	528
MCE=2.50					

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

** Doublet tests, given the observed singlet distribution.

+ These means have been adjusted to a standard run length of 25 trials.

++ N was nominally 500, but is slightly higher or lower in each case due to incomplete data or Passes by percipients. The mathematical transformations which base means and Ns on 25 trial runs and 500 trial series facilitate comparison, but make it difficult to retrieve raw data from this and subsequent tables. Researchers needing raw data should contact C.T.T..

sequences of the percipients. We expect human beings to show much more biased response patterns than an electronic RNG, and this is certainly the case.

As in table 1, the response sequences are ordered from highest to lowest singlet bias magnitude. The PPP was able to score as high as

TABLE 2
Performance of Probabilistic Predictor Program
on response sequences

Sequence Source	Singlet Chi-square	Doublet** Chi-square	Mean Hit per Run+	Z	N++
P5	146.47*	122.42*	4.90	7.23*	510
P11	105.53*	129.23*	3.89	4.15*	501
P4	79.53*	120.68*	4.26	5.37*	522
P17	71.00*	89.29	4.20	5.07*	500
P1	65.47*	94.80	3.44	2.87*	523
P32	48.39*	131.19*	2.81	.91	499
P14	40.88*	119.03*	2.80	.89	500
P3	39.35*	131.35*	3.28	2.34*	511
P7	28.88*	108.67*	2.90	1.19	500
P2	15.00	90.27	2.77	.81	515
MCE=2.50					

* $p < .05$, one-tailed

** Doublet tests given the observed singlet distribution.

+ These means have been adjusted to a standard run length of 25 trials.

++ N was nominally 500, but is higher or lower in each case due to incomplete data or extra Pass trials.

4.90 hits per run, against a mean chance expectation of 2.50, for a corresponding Z of 7.23. The PPP could significantly predict response patterns for six of the ten percipients.

We would expect that the degree of hitting by the PPP would be roughly proportional to the degree of bias by the Chi-square measures, but only roughly, for the reasons discussed earlier. For the target sequences alone, $r = +.40$ for the Z of PPP performance versus singlet bias, which is not statistically significant for an N of ten. For the response sequences, the corresponding $r = +.89$, which is significant at

the .001, one-tailed level for an N of ten. The correlation is low in the target sequences, for example, as a quite unbiased (by Chi-square) sequence for P1 turns out to be significantly predictable. Similarly, one of the less biased response sequences, that of P3, turns out to be significantly predictable, but the correlation is not so attenuated for the response sequences as a whole because of the wider range of bias in these sequences.

The imperfect correlation of PPP performance with Chi-square measures of degree of non-randomicity is to be expected. Chi-square measures reflect the overall degree of deviation of the observed target distributions from a model of equiprobability and sequential independence. Much of the kind of deviation from the model that would contribute to raising the Chi-square, however, would not be of any use in an inference strategy aimed at making hits. Much of the significance of a Chi-square test of singlet distribution, e.g., might come from the fact that half of the possible targets were too infrequent, given the model, but in making predictions you want to know what is the more frequent singlet, so you can give it as a response. At the doublet level, the lack of the XX doublets observed in the target sequences contributes greatly toward making the Chi-square test significant, but knowing not to call X when X has just appeared is much less useful information than knowing, as an example, that if X has appeared you ought to call Y because the observed frequency of Y following X in previous trials has been much higher than expected. The target distribution for P1 is a good illustration of this: neither the singlet nor doublet Chi-square measures of lack of randomicity approaches statistical significance, but the PPP managed to significantly predict this sequence.

The PPP was designed to be sensitive not only to relatively consistent biases to a target sequence at various levels, but also to pick up short-lived, but potentially important, higher order biases. The compilation and consultation of doublet through sextuplet registers is particularly important in creating this sensitivity of the higher order and transient biases such as might occur from temporary malfunctioning of the RNG. To empirically examine the actual usefulness of potential higher order biases in this empirical data, we modified the PPP so it could be run separately with memory lengths that included only the singlet level, the singlet plus doublet levels, etc., from the singlet through the sextuplet levels. Table 3 presents the performance of the PPP at each of these six memory levels, tabulated separately for target and response sequences. The number in

the body of the table is the Z-score of the PPP performance.

TABLE 3
Effects of various memory lengths on the performance
of the Probabilistic Predictor Program

Sequence Source	Target Sequences Memory Length						Response Sequences Memory Length					
	1	2	3	4	5	6	1	2	3	4	5	6
P1	2.5	2.5	2.5	2.5	2.3	2.5	2.6	2.7	2.9	2.9	2.9	2.9
P2	.1	.3	.3	.8	.8	.8	1.1	.5	.4	.7	.7	.8
P3	-.1	-.3	.1	.1	.1	.1	1.6	2.3	2.5	2.3	2.5	2.3
P4	1.1	1.1	1.2	1.2	1.4	1.4	4.2	5.2	5.4	5.4	5.4	5.4
P5	2.2	2.2	2.3	2.5	2.5	2.5	6.9	7.5	7.2	7.2	7.5	7.2
P7	11.0	-1.3	-.6	-.1	-.3	-.1	.3	1.2	.6	.9	1.2	1.2
P11	.6	.1	.4	-.3	-.3	-.3	3.0	3.3	3.7	4.2	4.2	4.2
P14	1.6	1.3	1.6	1.6	1.3	1.3	1.6	1.1	.9	.9	.9	.9
P17	-.8	-.9	-.9	-.8	-.6	-.6	5.2	4.8	4.9	4.9	5.1	5.1
P32	1.4	1.8	2.1	2.0	1.7	1.7	1.2	1.2	1.2	.9	.9	.9
Mean CRs												
	.76	.68	.90	.95	.89	.93	2.77	2.98	2.97	3.03	3.13	3.09

Note: Figures in body of table are standard Z-scores

As can be seen, there was considerable variation in how much success the higher level registers added across various sequences. Consider the three target sequences (for P1, P5, and P32) where the PPP was able to score significantly: in two of these three cases the PPP had essentially gained its significance at the singlet level with very little added at higher levels. For some of the target and response sequences, the addition of higher level decision making actually resulted in some decrease in performance, as, for example, in the target sequence from P11. One effect of adding higher decision making levels, which carry higher weight, is that erroneous, chance fluctuations that suggest higher level biases can keep the PPP from

responding to a singlet bias if that is the only bias that is actually in the sequence. That is, the power of PPP which makes it sensitive to real higher order biases also makes it somewhat sensitive to being thrown off by artifactual indication of biases of this sort, just as human beings might be.

The differences between the various memory lengths are small, and generally not well suited to formal statistical analyses, but we present them primarily for their suggestive value. They do suggest that as far as the target sequences of this experiment are concerned, most of the predictive power that might be used in an estimation strategy is available at the singlet level, with only a slight amount added by higher level biases. For the ten target sequences in general, there is actually a non-significant (by t-test for related pairs) drop when the doublet memory length is added in, and a rise by the time the triplet and higher levels are used. The gain from the singlet to the triplet level is suggestive ($p < .10$, one-tailed), but in general, differences between adjacent levels are non-significant. Even when higher level registers are consulted, inspection of the PPP output shows that almost all of the hits arise from singlet level decisions. We interpret these results as suggesting that high level biases are of little consequence in this particular data. While it is always conceivable that a very high level bias (greater than the sextuplet level) might exist, we regard this as quite unlikely, for not only can we not envision a mechanism whereby an electronic RNG of this type would generate such a bias, but we also think it would be quite likely that if such a high level bias existed, components of it might very well show up as lower levels (sextuplet level or less) bias that would be detected and capitalized on by the PPP.

DID PERCIPIENTS IN THE FIRST TRAINING STUDY INFLATE THEIR SCORES WITH A MATHEMATICAL INFERENCE STRATEGY?

The question we shall now deal with is the degree to which percipients in the first TS might have used mathematical inference strategies to increase their hit scores. Three hypotheses will be examined: (1) no mathematical inference strategies were used, all the above chance-scoring was due to ESP; or (2) all of the above chance-scoring was due to (unconscious) mathematical inference strategies, with no need to invoke ESP; or (3) percipients may have used both ESP and some degree of mathematical inference strategies to

increase their hit scores.

How do scores of percipients and the PPP compare?

Table 4 presents the mean hit/run and associated Z-scores that the percipients and the PPP (memory span of six) (note 6) made on the target sequences of the feedback experiment. The PPP had more targets to work with than the percipients, for reasons discussed earlier, giving it a small advantage and so making our comparison more rigorous. Overall, the PPP made 577 hits in 5,136 trials for a Z of 2.95 and an associated $p=.003$, two-tailed. In terms of means, the PPP averaged 2.82 hits per run of 25, compared to the value of 2.50 per run expected by chance. The percipients, on the other hand, made a total of 722 hits in 5,000 trials for a Z of 10.46 and an associated $p=2 \times 10^{-25}$, two-tailed. In terms of means, the percipients averaged 3.61 hits per run instead of the chance-expected 2.50. As far as peak results per target sequence are concerned, the PPP's best Z-score was 2.50, but five of the ten percipients had Z's of 4.17 and higher, with the highest at 11.03. Obviously the percipients performed enormously better than the PPP.

The relationship between percipient performance and PPP performance is illustrated in figure 1. As can be seen, the two performances are often highly divergent. The best percipient, for example, had a Z of 11.03, while the PPP could only score at the quite chance Z of .15 on her target sequence. The only case where the percipients's and PPP's scores are essentially equal was a case of chance level scoring for both. Overall, the Z's of the percipients and PPPs were uncorrelated ($r=+.08$, n.s.).

The enormous differences in performance magnitude of the actual percipients and the PPP strongly argue against hypothesis two, that all of the percipients' scoring was due to a mathematical inference strategy. A further argument against this hypothesis is the complete lack of correlation between the magnitudes of PPP and actual percipient performance.

Hypothesis three was that the results might be due to a mixture of ESP and mathematical inference strategies. Insofar as any other mathematical inference strategy we have been able to conceive to date would have been even less efficient than the PPP, however, this leaves

TABLE 4
Performance of real percipients versus
the Probabilistic Predictor Program

Sequence Source+	PPP		Percipients	
	Mean hits per run	Z	Mean hits per run	Z
P5	3.32	2.50*	5.15	7.90*
P3	2.55	.15	6.20	11.03*
P4	2.95	1.40	4.05	4.62*
P2	2.78	.84	4.00	4.47*
P14	2.95	1.30	2.85	1.04
P1	3.30	2.46*	3.90	4.17*
P32	3.05	1.70	2.35	-.45
P17	2.30	-.60	2.95	1.34
P11	2.48	-.30	1.95	-1.64
P7	2.48	-.10	2.70	.60
Means	2.82		3.61	
Mean chance expectation	2.50		2.50	

+ listed in order of decreasing singlet bias

* $p < .05$, one-tailed

even more than at least two-thirds of the results attributable to ESP. Suppose we consider, nevertheless, that results were due to this mixture; we can then use the PPP results as an empirical estimate of the true probability of a hit by ESP after mathematical inference results are subtracted out, and recalculate the significance of the ESP results. The results are only trivially different from the original results.

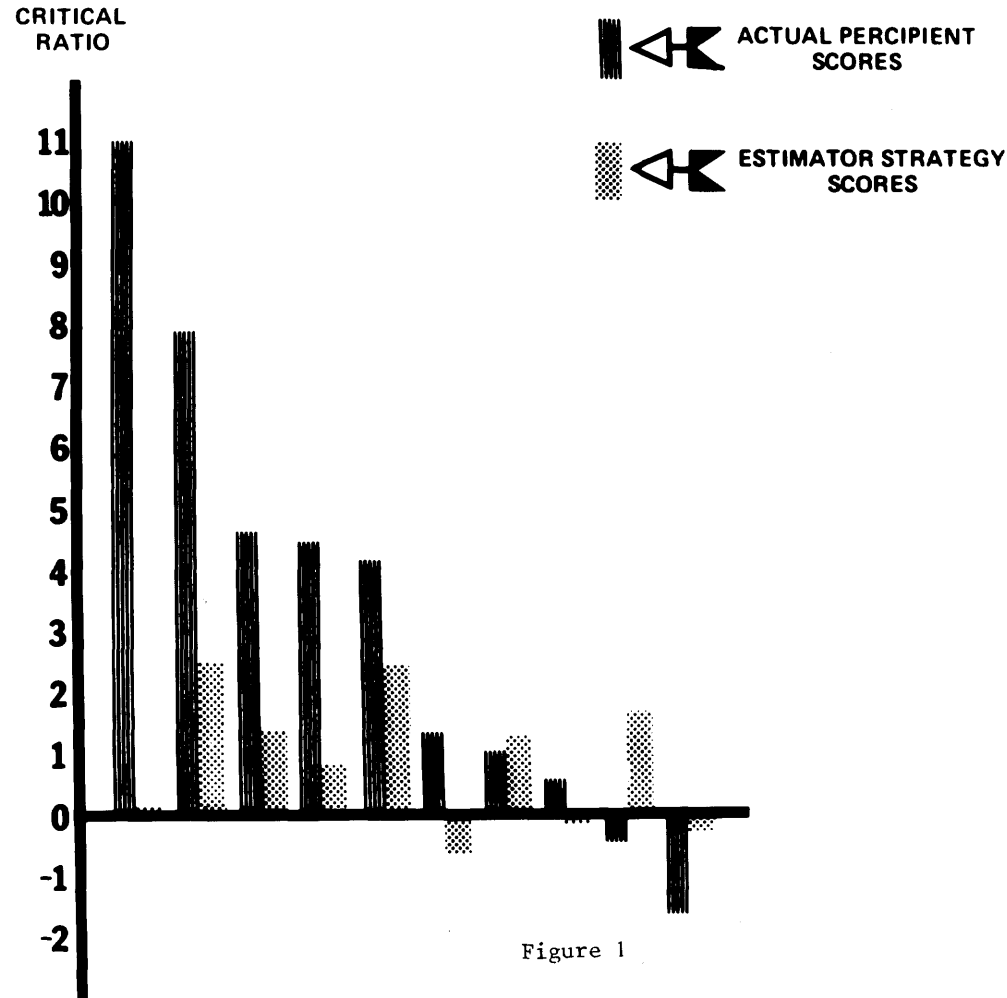


Figure 1

CONCLUSIONS

Given our discussion of the ways in which conventional statistical measures of bias fail to adequately deal with the question of predictability of a target sequence, and our demonstration that the PPP deals much more adequately with this issue, we propose that the PPP (or some superior version of it) be adopted as the standard measure of predictability, and be used in all psi experiments in which percipients receive feedback on target identity before the end of the experiment. To make this more feasible, one of us (C.T.T.) is working on developing a simplified version of the PPP that will run on an Apple II microcomputer, since that computer is apparently being adopted as a standard in parapsychological laboratories.

For the first TS whose data have been examined here, the analyses strongly support the hypothesis that ESP is the best way to account for the hitting, with mathematical inference strategies playing little or no significant part in scoring. This conclusion is, of course, specific to the particular PPP strategy used here. While it may be possible for someone to develop a more powerful mathematical inference strategy, it is not clear to us how this could be done, and we present our results as a challenge to others to develop and empirically demonstrate a superior strategy.

We believe our results are important for parapsychological research in general, as well as having application to general psychological studies of human decision making. For example, there may be some parapsychological studies with useful results that have not been submitted for publication because of a retrospective discovery of some degree of non-randomicity in their target sequences. We suggest that rather than an all-or-none reaction to non-randomicity, experimenters with such data reevaluate them along lines similar to those used here (note 7). Useful data may be forthcoming.

ABSTRACT

With the increasing use of immediate feedback of target identity in parapsychological research, the question of departures from randomness (equiprobability and serial independence) in target generators becomes important, as it is possible that some percipients

might identify such departures and develop a mathematical inference strategy for predicting targets, thus artifactually inflating their scores. The key aspect of randomness of relevance is not lack of pattern per se, but the predictability of the generator. It is shown that standard Chi-square tests of randomness are poor measures of predictability in short to moderate length experiments. A direct approach to the predictability of a possibly biased target source has been developed, the 'Probabilistic Predictor Program' (PPP), which is probably much more powerful than most human percipients could be. The operation of the PPP is described in detail. The PPP is then applied to both the target and response data of Tart's first Training Study, where some small departures from randomness were found in the electronically generated target sequences and, of course, in the percipient generated response sequences. The PPP was found to occasionally score significantly on the target sequence, but far less successfully than the actual percipients did. The more biased response sequences were predicted quite significantly by the PPP. Examination of the internal displacement scoring patterns of the PPP was also compared with the patterns of the actual percipients and found to be drastically different. For these two reasons, it was concluded that use of mathematical inference strategies of the PPP sort could have only accounted for a trivial portion of the extremely high target scoring of the percipients in the first Training Study. While we should normally strive for completely random target sequences, the PPP is offered as a powerful approach to the question of predictability when departures from randomness do occur, and can be of use in working with other experimental data.

NOTES

1. We may further subdivide the second category of feedback after calls into situations with closed decks and those with open decks. The closed deck situation has recently been discussed by Thouless (1977). We will not consider it further here, but confine our attention to the open deck situation.

2. If an RNG has significant singlet bias, this will automatically affect doublet, triplet, etc., distributions. A singlet excess of 7s, for example, will produce excesses of 71s, 72s, 73s, etc.. A Chi-square test for equiprobability of all observed doublets, where

the expected frequencies in each cell are based on the square of the assumed, unbiased singlet hit probability, will usually give significant results, as a result of such singlet bias. An appropriate Chi-square test for doublet bias that is independent of singlet bias would use expected frequencies in each cell that were calculated from the marginal totals.

3. This section draws heavily on material published in the January 1978 'Journal of the American Society for Psychical Research' (pp. 50-53) and is reproduced here by permission of the Journal.

4. The first Training Study has generated considerable critical comment (Gardner, 1977; Gatlin, 1978a; 1978b; 1979; O'Brien, 1976; Stanford, 1977a; 1977b) and response (Tart, 1976b; 1977a; 1977b; 1978; 1979) concerning a variety of issues. This paper deals only with the question of mathematical inference strategies as hypotheses for explaining the results of that study, and the reader should see the above references for other considerations.

5. Percipients did not receive feedback as to target identity on these Pass trials, and Passes were not counted in the formal scoring of hits and misses in evaluating the number of total hits obtained. Another minor difference was that this version of the PPP broke ties by choosing the lowest of the tied numbers, rather than randomly selecting among them, but this should not have any important effects.

6. The output of the PPP with a memory span of six is used here since this was theoretically expected to be the most sensitive to higher order biases, but, as an inspection of table 3 will show, the pattern of results reported here would have been the same if any other memory span results had been used.

7. We regret we cannot offer the use of our PPP program to other researchers, but our present version is quite expensive to run. The principles of operation, described above, should allow implementation of the program on other computers by skilled programmers.

ACKNOWLEDGMENT

We wish to thank Laura Dale, Aaron Goldman, J. Gaither Pratt, and Gertrude Schmeidler for helpful suggestions in preparing the final version of this paper.

REFERENCES

- Chaitin, G. 'Randomness and mathematical proof', Scientific American, 1975, 232, 47-52.
- Gardner, M. 'ESP at random', New York Review of Books, August 14, 1977.
- Gatlin, L. 'Correspondence: Comments on the critical exchange between Drs. Stanford and Tart', J. A.S.P.R., 1978, 72, 77-81. (a)
- Gatlin, L. 'Correspondence: Reply to Dr. Tart', J. A.S.P.R., 1978, 72, 294-296. (b)
- Gatlin, L. 'A new measure of bias in finite sequences with application to ESP data', J. A.S.P.R., 1979, 73, 29-43.
- O'Brien, D. 'Review of application of learning theory to ESP performance by C.T. Tart', J.o.P., 1976, 40, 76-81.
- Stanford, R. 'The application of learning theory to ESP performance: a review of Dr. Tart's monograph', J. A.S.P.R., 1977, 71, 55-80. (a)
- Stanford, R. 'The question is: Good experimentation or not?' J. A.S.P.R., 1977, 71, 191-200. (b)

Tart, C.T. 'The application of learning theory to ESP performance', New York, Parapsychology Foundation, 1975.

Tart, C.T. 'Learning to use extrasensory perception', Chicago, University of Chicago Press, 1976. (a)

Tart, C.T. 'Correspondence: Reply to O'Brien's review of Application of learning theory to ESP performance', J.o.P., 1976, 40, 240-246. (b)

Tart, C.T. 'Toward humanistic experimentation in parapsychology: A reply to Dr. Stanford', J. A.S.P.R., 1977, 71, 81-102. (a)

Tart, C.T. 'Psi and science', New York Review of Books, 1977, October 13. (b)

Tart, C.T. 'Toward conscious control of psi through immediate feedback training: Some considerations of internal processes', J. A.S.P.R., 1977, 71, 375-408. (c)

Tart, C.T. 'Correspondence: Reply to Dr. Gatlin', J. A.S.P.R., 1978, 72, 81-87.

Tart, C.T. 'Randomicity, predictability, and mathematical inference strategies in ESP feedback training experiments', J. A.S.P.R., 1979, 73, 44-60.

Thouless, R. 'The effect of information given to the subject in card guessing experiments', J. A.S.P.R., 1977, 49, 429-433.

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BEYOND REDUCTIONISM - RECONSIDERED
HOW TO LINK PHYSICS AND PSYCHOLOGY

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The problem of reductionism is as old as Western philosophy and starts with the idea of the ancient Greek philosophers who thought that the whole of nature is composed of simple elements. Empedokles (492-432) believed that world consists of four elements: earth, air, fire and water and a quinta essentia which was reserved for the eternal stars. Everything consisted of different mixtures of these elements. Leukippos (550) and Demokritos (460-370) invented the idea that the elements are very tiny particles which could not be divided further. They called them atoms. The property of any object was described by the property of the atoms. In philosophy these ideas are designed by the word 'materialism'. Beginning with Dalton (1766-1844) and Avogadro (1776-1856) it turned out that the atomistic hypotheses could be applied with great success in chemistry and physics, and its enormous success had a great impact on the philosophy of science. It led to the so-called program of reductionism, and a full reductionist would claim that if we could describe the properties of atoms or elementary particles comprehensively we should necessarily be able to describe every phenomenon in biology or even in psychology in terms of physics. This position held by many scientists is often called ontological reductionism (Nickles, 1980).

It is clear that the problem of reductionism is of essential importance to parapsychology, since this subject deals with a special sort of alleged phenomena where a 'direct linkage' (whatever that means) between psychological and physical variables is asserted. Thus there emerges a problem which we call the problem of coherent levels of description (Bauer and von Lucadou, 1979). Obviously modern experimental psychologists do not regard themselves as forced to deal with the mind-body problem. They believe psychological phenomena and their correlations can be described in a rather sober 'one-level language' (Hubner, 1980). On the other hand neither do physicists feel any such commitment and believing in (ontological) reductionism they have enough complicated problems in their field without it.

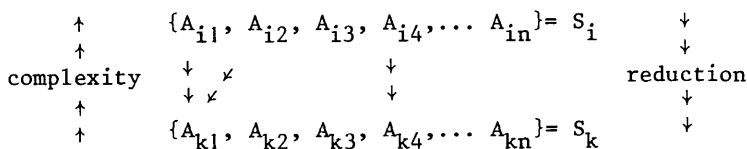
Reductionism has led to a lot of important results in molecular biology and physiology, but on the other hand it could never be proved as applicable in totality. The well-known mind-body problem is still unsolved (Bunge, 1979) and the counter movement against reductionism continues to gain supporters, such as the well-known science writer A. Koestler (Koestler, Smythies and Raymond, 1970). The discussion about reductionism affects the foundations of science and philosophy, and its traces can be found in the well-known debate between the Popperian and Kuhnian views. It is true that working scientists tend not to be affected by it and generally adopt a pragmatic attitude. We will try to show what consequences this attitude will have on science in general and the field of parapsychology in particular.

CLASSICAL REDUCTIONISM

As already mentioned in the introduction the usual form of reductionism which we will call classical reductionism (or reductionism by interdependence) is grounded in the idea that the whole world is governed by a few simple physical laws which can at least in principle explain every phenomenon.

Formally the program of classical reductionism can be presented in the following way: natural sciences consist of several levels of description which show a different degree of complexity. Note however that the term 'level' used here does not necessarily mean a language level in the sense of meta-language in logic. Thus

physics is simpler than chemistry and psychology is more complex than biology but in every discipline there are still a lot of sub-levels. Reductionism claims that every theory which describes a certain level can be reduced to the theory of the level below. This can be done by certain logical operations. To demonstrate this let us assume two theories S_i and S_k of different levels i and k . A theory can be expressed by a set of theoretical expressions which we call A_{ij} and A_{kj} . Essentially we can find three operations which we call elimination, consolidation and transformation to describe the process of reduction (Nickles, 1980).



Elimination (1)

$$A_{kj} \Leftrightarrow (A_{k1} \wedge \neg A_{im})$$

example: A witch is only a neurotic person, witches do not really exist.

Consolidation (2)

$$A_{kj} \Leftrightarrow (A_{i1} \Leftrightarrow A_{km})$$

example: Light is an electromagnetic wave.

Transformation (3)

$$A_{kj} \Leftrightarrow ((\forall_m A_{im}) \Leftrightarrow A_{k1})$$

example: Ideal gases and CO_2 are nothing but examples for van der Waals equation.

Thus, all statements of the level S_i can be expressed by elements of level k . From this point of view it is necessary that the level

k contains a complete set of expressions which allow one to deduce all expressions A_{ij} . This idea proved very successful in the natural sciences, where it was found possible to reduce such fields as acoustics and thermodynamics to classical mechanics and to unify optics and electrodynamics. Later the theory of classical mechanics was seen to be a special case of the theory of special relativity which again could be derived from the general theory as a special case. In the realm of microscopic phenomena quantum theory was very successful and seemed to explain the work of chemistry in principle, leading to new disciplines such as quantum chemistry and molecular biology, which developed with enormous success. But as well as in empirical sciences there was a similar trend towards reductionism in logic and mathematics (Meschkowsky, 1964).

It was the old idea of an *ars magna* imposed by Raimundus Lullus which led to Hilberts program of a complete arithmetic theory of mathematics which he called 'Metamathematik, based on the algebraic formalism for the Aristotelian logic introduced by G. Boole (1812-1864) and on Cantor's set theory.

However the first doubts concerning this program of reductionism came from the mathematicians themselves, since, B. Russell showed that a general set theory could lead to antinomies and K. Godel (Godel, 1931) proved that Hilberts program was not possible in principle. Soon there were speculations whether this situation could have any consequences for the reductionistic program in the empirical sciences. To understand this point it is convenient to give a little example of Godel's theorem which provides some intuitive understanding of the problems involved:

Let us suppose that it was possible to establish a complete theory S of a physical system say S'' . This theory should be proved by empirical data and suppose that the theory is totally algebraic. So the theory will be represented by a set of algebraic expressions, say S' . Since the theory shall be complete it must contain the theory of algebraic expressions too. So it will consist of the set S' and another set $S^{\sim}$ whose elements of course should also be algebraic expressions. For this a procedure called Godel numbering or Godelization can be used:

$$\begin{array}{cc} \text{system} & \text{theory} \\ S'' & \longrightarrow S' \cap S^{\sim} = S \end{array}$$

As a result the theory S consists of a set of algebraic expressions $A_i(\xi)$ which may be defined within the set of natural numbers.

$$\xi \in N$$

$$S = \{ A_1(\xi), A_2(\xi), \dots, A_n(\xi) \}$$

$A_i(\xi) \in S$ means that A_i is a valid expression for all natural numbers $\xi \in N$

$\neg A_i(\xi) \in S$ means that A_i is not valid for at least one natural number.

$$\exists \xi \in N: A_i(\xi) \text{ false}$$

According to our supposition S contains all derivable or provable algebraic expressions. Now let us write down a very simple algebraic expression which is defined by pure algebraic means, thus:

$$\neg A_m(m)$$

This looks very respectable since it contains only accepted signs, and since it is such a well-defined algebraic expression we would expect that it is an element of S which contains all derivable algebraic expressions.

Unfortunately we must discover that this is not the case. Thus $\neg A_m(m)$ is no element of S . To prove this suppose $\neg A_m(m)$ was an element of S . This would include that it is equivalent to one of the expressions $A_1(\xi), A_2(\xi), \dots, A_n(\xi)$ say A_i .

$$\neg A_m(m) \in S$$

$$\neg A_m(m) \Leftrightarrow A_i(m)$$

But $A_i(m)$ means that A_i is true for all $m \in N$. So let us take $m = i$. We thus get $\neg A_i(i) = A_i(i)$ which is contradictory. Therefore our supposition must be wrong and it is shown that $\neg A_m(m)$ is no element of our theory even though it is well defined. Thus it follows that something must be wrong and K. Godel has proven that the deficiencies lies within the theory S . His theorem states that S cannot be complete in principle. This incompleteness theorem does not mean that the theory S is wrong or could contain contradictory expressions. Thus if a theory S is supported by empirical facts there is no danger that the theory could be inconsistent for formal reasons. Incompleteness however means that

there is always a possibility that expressions could be formulated in a correct and well defined way without any possibility of being derived within the context of the theory S . This may be a problem for a mathematician but not for an empirical scientist. He will ask by experiment whether the theoretical expression in question is right or wrong.

Up to now this constitutes no refutation of reductionism, because the theory S can be extended to a new theory $S+$ where the results of the critical experiments can serve as a new axiom and this procedure can be iterated arbitrarily. On the other hand if experimental results lead to a theoretical (algebraic) expression which is not derivable in the system S a new theory must be found which contains the experimental results as derivable expressions. This situation seems typical of the empirical sciences where always some experimental results exist which do not fit into the present theory.

But Godel's theorem says more than this. If a new theory $S+$ is found this will also be incomplete and so present the same dilemma. This is unfortunate because a reductionist must be certain his basic (say physical) theory is complete in principle if he wants to explain more complex systems (say in biology) on this basis. Otherwise one would expect that the deficiencies of the theory could accumulate to such an amount that the reductionistic program is impracticable.

Several approaches have been made to avoid this problem. B. Russell for instance proposed to restrict the notion of sets in set theory and L.E.J. Brouwer proposed to inhibit certain methods within mathematics, but none of these approaches could gain general agreement among the mathematicians.

A less rigorous argument prevalent in science is that one has to avoid reflexive or self-referential expressions explicitly (Wandschneider, 1975) (our example $A_m(m)$ is such a reflexive expression), and also be careful if one changes from one level of description to a meta-language and vice versa. But it is dubious whether this demand can really be fulfilled (Kleinknecht, 1976; Pittioni, 1976). We will consider this point in the third section.

Throughout this historical period when formal arguments against reductionism were put forward there arose a lot of problems in the

field of physics (which still remain unsolved) that seem to refute the concept of reductionism. Maxwell tried to reduce electromagnetism to mechanics without success. Conversely Einstein tried to find a field theory to unite gravity and electromagnetism. Quantum theory led to a proliferation of elementary particles, a host of divergence problems and the inability to be combined with general relativity. Even quantum chemistry leads primarily to a set of heuristic approximations and rules-of-thumb (Rosen, 1978). So one can hardly say that on the microscopic scale there exists a simple one-level theory which could serve as a basis for reductionism. Moreover one gets the impression that the more sophisticated experimental methods get in any field or on any level, the more complicated the theory becomes, and with further questions left open. Thus, even if great success were attained in the field of elementary particle theory it is most doubtful if it would help to elucidate the problems of say biology and psychology.

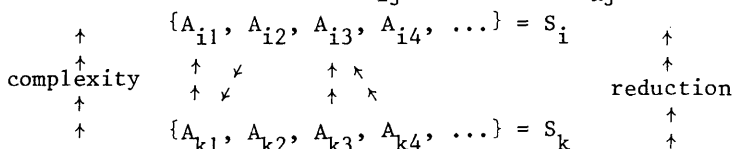
Another argument against reductionism is often mentioned: For example, many concepts which are used in psychology seem to have no counter-part in physics, and this led Wolfgang Buchel (Buchel, 1975) to argue that the concept of information, even if it is interpreted as negentropy, loses its genuine purpose. The hardware side of a computer, for instance, can be described by physics, but there is no possibility of deciding whether a given piece of information processing makes any sense or not. Doubts about reductionism also emanated from the philosophy of science. Thomas Kuhn illustrated by many examples that Poppers idea of reductionism was more a post hoc rationalization than an adequate model of the real socio-historical process of the development of science, even if one concedes that in practice the notion of Kuhns scientific revolution is a similar exaggeration as Poppers falsification principle. For these reasons we may conclude that the empirical sciences rest upon a rather dubious philosophical basis.

A NEW NOTION OF REDUCTIONISM

Dubious thing this basis may be it does not imply that chaos reigns. Since working scientists were not irritated too much by the philosophical problem which reductionism poses they have

adopted a pragmatic attitude which was not by any means worthless. It led to the ingenious approach called general systems theory (von Bertalanffy, 1968) where it was shown that certain theoretical models which were developed in quite different fields of science showed a similar mathematical structure. For instance the same differential equations which describe hydrodynamical problems can describe sociological problems too. In this paper we can only summarize some essentials of this approach. Systems theory has led to a new understanding of the role of empirical data and theories (models). Every object of description denoted as 'system' is only defined within the context of actual interest. Thus a property of an object per se does not make any sense. An object is viewed as having different aspects which are more or less relevant in relation to certain problems. The aspect chosen depends on the interests of the describer. Every system has an author. From this point of view the problem of reductionism acquires a generalized meaning, depending on the interests of the author, and one could reasonably demand that in system theory reductionism should also yield a unification of different fields of research. Thus the classical form of reductionism is incorporated into the generalized notion which we call 'reductionism by correspondence'. If we define reductionism by correspondence as being required to concentrate information, in the sense that less information is necessary to explain a greater amount of data, nothing is said about the direction of reduction. Thus if we describe very complex systems it may be appropriate to 'reduce' the properties of simple elements to the properties of the composed system. Formally we can describe reductionism by correspondence in the following way:

Again let us assume that there exist two different models S_i , S_k of a natural system which represents two hierarchical levels. H.A. Simon (1962) has shown that systems of given complexity can only be described in a rational way if they are described in terms of hierarchical levels. Again we denote the theoretical expressions by A_{ij} respectively A_{kj} .



We can now find three operations to attain reduction, which we call idealization, interpretation and classification.

idealization (1') $A_{ij} \Leftrightarrow \neg A_{im}$ (isolation, fractionation,
generalization)

interpretation (2') $A_{ij} \Leftrightarrow A_{kl}(\xi), \xi = A_{im}$

classification (3') $A_{ij} \Leftrightarrow ((\forall_m A_{km}) \Leftrightarrow A_{in})$

Idealization (1') means that certain features within the system S_i will be regarded as irrelevant for the general description and will be negated. This can be performed in different ways. Thus one can isolate a certain element out of its context. An example is to regard the neuron as a McCulloch-cell. Another way is called fractionation, which means that the composed element is split in two smaller parts, so certain connections may be ignored. Generalization means that different elements of the same level are put together and their differences are ignored. Thus isolation, fractionation and generalization are different features of idealization because all of them lead to a loss of what may be regarded as irrelevant information.

To explain the term 'interpretation' is more difficult. It can be illustrated by the following figures:

	00000	00LL000	LLOLL
	00L00	00LL000	LLOLL
L =	00L00	= 00LL000	= LLOLL
	00LL0	00LLLL0	LL000
	00000	00LLLL0	LLLLL

What we see here is a sign called a super-sign which is constructed by other signs. We interpret it as a sign but we are not forced to do so. So we can interpret our formula given above:

$$A_{ij} \Leftrightarrow A_{kl}(\xi) \quad \text{with} \quad \xi = A_{im} \quad (2)$$

$\downarrow \qquad \qquad \downarrow \qquad \qquad \downarrow$

A super-sign is a sign composed by a sequence of signs

$\uparrow \qquad \qquad \uparrow \qquad \qquad \uparrow$

Information is negentropy of a data-processing system

Such an interpretation may be problematic because if we asked for the meaning of it, it may become misleading. Another example

is the following designation of a colour:

red (written in blue letters)

blue (written in red letters)

Clearly we see that formula (2') can lead to reflexive or self-referential expressions and therefore to undecidable or underivable expressions in the Gödel's sense.

Most scientists seem to believe that such expressions can easily be avoided but this is not true, as we will see. Often the existence of such self-referential expressions is not obvious. D.R. Hofstadter (1981) gives a nice example:

Can you translate this english sentence into a german one?

which looks naive unless you do try to translate it!

The notion of interpretation plays an important role within the foundations of quantum theory. There have been many attempts to reduce (in the classical sense) quantum theory to classical mechanics. This has led to the so-called different interpretations of quantum theory but none of them has been successful (Jammer, 1974). In a previous paper (von Lucadou, Kornwachs, 1980) we tried to demonstrate that from the point of view of system theory all these different interpretations emerge from a system-theoretical property of quantum mechanics and that none of these interpretations could falsify any other. All are valid within their terms of reference. Thus quantum theory is a good example of a theory which cannot be reduced in the classical sense but can be reduced by correspondence. It also illustrates the notion of interpretation. Again let us consider formula (2'):

$$A_{ij} \Leftrightarrow A_{kl}(\xi) \text{ with } \xi = A_{im}$$

This represents the usual way used in quantum theory to extract quantum mechanical formalism from the classical one. (This is part of the so-called principle of correspondence and is justified by Ehrenfests theorem). In classical mechanics the state of a system is described by the Hamiltonian which is a function of coordinates of position and momentum.

For example: $H = \frac{1}{2m} p^2 + U(x)$

To get the correct quantum mechanical formula which describes the

equivalent quantum mechanical system, the variables X and P have to be replaced by the corresponding operator:

$$\begin{array}{ll} S_k & X \rightarrow \hat{X} = X \\ & P \rightarrow \hat{P} = \frac{\hbar}{i} \frac{\partial}{\partial x} \end{array} \quad S_i \text{ (space representation)}$$

So we now get the correct quantum mechanical Hamiltonian which is now an operator:

$$\hat{H} = \frac{\hbar^2}{2m} \frac{\partial^2}{\partial x^2} + U(x)$$

The change from the function H to the operator $\hat{H}$ is expressed in formula (2'). A_{kl} may be a function than A_{lm} is an operator, because its variables are functions. The physical interpretation however is the same. It is called energy.

It is important to note that the classical system S_k and the quantum mechanical one S_i are not equivalent. Since $\hat{H}$ depends on X a reduction of variables has occurred. This however means a loss of information. Thus we can conclude that a reduction by interpretation can also lead to a loss of information and to self-referential expressions.

A complete system theoretical discussion of the well-known measurement problem in quantum theory shows the same loss of information. To get a measurement in quantum theory the projection operator P_o must be replaced by the statistical operator W_o which means that all terms with mixed states $\langle \rho_i | \rho_j \rangle$ will be ignored (see Mittelstaedt, 1966, for details). The important point is that this loss of information emerges from a voluntary action of the systems author. Mittelstaedt says the observer thus renounces information, but this is however irrelevant from a pragmatic point of view. It would thus appear improbable, since these apparent deficiencies of a system description are deficiencies in principle (there exists no system without author) that a 'better' quantum theory without any measurement problem could exist.

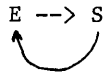
Finally, we have to consider a third operation of reduction by correspondence. We call it 'classification'. Formally it is very similar to the term 'transformation' in classical reductionism, but the meaning is quite in contrast. In empirical sciences it is customary to search for typical patterns of phenomena, which seems

to be an heuristical method but which has yielded successful results in biology and psychology. However, its worth is ambiguous because it often suggests relations with which there is no correspondence in the systems described, and it may thus lead to an increase of information, i.e. concerning alleged states of the system which either may be irrelevant, or non-existent. It is interesting to see that the intuitive human way of getting knowledge of everyday life is of this kind. It is the well-known 'Gestaltwahrnehmung'. In comparing the new notion of reductionism by correspondence with the classical one, one may feel at first glance that the former contains more problems than the latter. But let us try to demonstrate the advantages.

CONSEQUENCES OF THE SYSTEM THEORETICAL PARADIGM

We have described the arguments against classical reductionism which are all related to the problem discussed in the previous section. On the other hand there is no discipline in the field of science which does not use reductionistic methods, at least partially, and where the more general concepts of reductionism by correspondence cannot be applied. Moreover we cannot imagine that an analytical and empirical science makes any sense without trying to minimize information in the sense mentioned. First of all one should realize that the concept of reduction by correspondence is purely pragmatic which means that only aspects which are related to a given model or an actual system can play a role. There is no absolute space, no absolute time, there is nothing absolute. The object per se is irrelevant. Every system has an author and the author pursues his own interests within the system. This is a straightforward and modest position which takes into account the limitations of scientific method and excludes all metaphysical, spiritualistic or mystical explanations. So this seems to suggest a position from which to reconsider the problems of parapsychology. We feel this approach is a powerful one and that its power comes from awareness of its limitations. Without being able to prove this in every detail we can say that most of the usual arguments against classical reductionism emerge from extrapolations of scientific findings to an absolute value. (William James called this 'terrible nothing-buttery'). But if we always describe the limitations of a scientific rule explicitly this cannot happen.

As a consequence we can understand that a quantum mechanical system cannot be described like a classical one or that a very complex system cannot be described like a simple system. The rules which governs the reduction by correspondence of one system to another are not watertight as in the classical case because they always allow the occurrence of undecidable expressions or meaningless statements or a loss of information in the description. Sadowsky (1974) has shown that this statement is even true for the fundamental definitions of system theory itself. There are a lot of different definitions of general systems which make use of concepts like set and element. Since the system must be described by its elements it is necessary to describe the elements first. The elements however are only describable as components of the system. So it is necessary to describe the system first.



Sadowsky has found several similar paradoxes which we will omit here. Nevertheless we can recognize that the structure of these paradoxes is in the same way self-referential as formula (2') or the formula we got in the second section. This led to the paradoxical situation that system theory is self-consistent in relation to its paradoxes. The usual way to eliminate these paradoxes is again a pragmatic one. It assumes that every knowledge which can be obtained from a system is only an approximation. This approximation can be enhanced, but this has to be done at a later time. In this way an historical aspect comes into the story and the self-referential structures break into a sequence of approximations.

$$E1 \rightarrow S1 \rightarrow E2 \rightarrow S2 \rightarrow E3 \dots$$

A proof that the sequence of approximations is converging to an optimal description or to the 'absolute truth' however cannot be given. But this knowledge is quite consistent with the awareness of the limitations of the scientific method mentioned above. But the historical aspects may always be applied (Vollmer, 1975), and this is the reason why we feel there is no sufficient justification to use it as a means to solve logical paradoxes. Fortunately there remains another interpretation of the problem which is straightforward and which is also a pragmatic one. We

have just stated that all knowledge is only an approximation without any criterion to give a measure of its accuracy. This however means that we only can describe systems with a certain probability. There is nearly no field of science where statistical methods may not be applied. It is used in quantum physics as well as in psychology. Most scientists have assumed, however, that this is only an indication of lack of 'proper' methods or 'precise' knowledge. For a long time this was the opinion in classical thermodynamics and such probability was called accidental.

Most psychologists today believe that the probabilities which they calculate are accidental. Quantum theory was the first field where the notion of probability was regarded as objective. Prigogine et al (1979) has shown that this is true for thermodynamics too. Our interpretation of system theory led to the assertion that the only proper description of natural systems is a probabilistic one and from a pragmatic point of view this is actually true. The distinction between accidental and objective probability loses its sense. We are therefore driven to the assumption that natural systems can only be described as probabilistic systems.

DOES GENERAL SYSTEM THEORY IMPLY A NEW UNDERSTANDING OF PARAPSYCHOLOGY?

We mentioned in the introduction that the problem of reductionism seems to be more crucial in parapsychology than in any other field of science. It is not very surprising that most theoretical attempts to explain alleged parapsychological phenomena start on the basis of physics (von Lucadou, Kornwachs, 1979). Some workers believe that only a fundamental new understanding of space, time and causality can solve the enigma of psi (Rosen, 1981). Others (Ruderfer, 1968; Dobbs, 1971; Firsoff, 1967, 1976) hold out hopes for new elementary particles or new dimensions and interactions in physics which could serve as basis for a psi-model.

Viewed from the point of system theory it is rather improbable that new findings in the field of high-energy physics could lead to theoretical advances in the field of biology and physiology. Actually the greatest success of system theory is not in the field

of elementary particle physics or cosmology but in computer science, biology and the social sciences. Here a new interdisciplinary approach called synergetics (Haken, 1981) led to an understanding of a huge class of phenomena.

This enormous success even motivated some scientists to make the optimistic claim that we have reached the starting point of a new scientific revolution which is comparable with the renaissance (Jantsch, 1979; Prigogine, 1979).

The advantage of this approach is that it is not necessary to search for a new substratum of the phenomena but to discover new structures and new mathematical methods to describe them (Haken, 1980). It would go far beyond the scope of this paper to demonstrate this in detail.

There has also been a similar development in parapsychology, as shown by the 'observational terms' of E.H. Walker (1979) and others (Millar, 1978). The essence of this approach is that it does not change well established physics too much but merely gives a new interpretation of it. Some very recent models contain the system theoretic approach very explicitly (Ruttenberger, 1979), and here we wish also to mention the work of J.T. Culbertson (1979), A.N. Chester (1981), D.F. Lawden (1981) and our own approach (Kornwachs, von Lucadou, 1979; von Lucadou, Kornwachs, 1980).

It has not been our purpose to discuss the different models in detail, which we feel represent only the first steps in what must be still considered to be an uncertain direction. Nevertheless it appears that certain perplexing problems in parapsychology can be understood in principle from this point of view.

The concept of 'pragmatic information' as it was suggested by E. von Weizsacker (1974) may serve as an example. By means of this it is possible to understand in principle the decline-effect and the displacement effect observed in psi testing, and on a more general basis the elusiveness of psi-phenomena. Moreover, the model allows for experimental tests, which will actually be performed (Weiner and Bierman, 1981).

In this paper we have thereby tried to show that reductionism need not be dismissed as a mechanistic and dogmatic method which

is unable to deal with psychic phenomena. Properly applied reductionism could be a way for parapsychology to become integrated within the rest of science. As D.R. Hofstadter remarked: Reductionism does not get rid of mystery or beauty or communion with nature - if anything it enhances it (Hofstadter, 1981).

ABSTRACT

We try to show that in spite of the great success in many disciplines the program of reductionism has failed its genuine purpose. System theory however has yield a new concept of reductionism which we call reductionism by correspondence and which may imply a new understanding of parapsychology.

The crucial operations of reductionism by correspondence are called idealization, interpretation and classification. They are used to optimize the description of a system. Nevertheless they lead to certain deficiencies which cannot be avoided in principle. We are therefore driven to the assumption that natural systems can only be described as probabilistic systems. From this point of view nothing is said about the direction of the reduction.

ACKNOWLEDGMENT

This paper was presented at the 11th International Parascience Institute Conference at Birkbeck College, London, September 1981.

REFERENCES

Bauer, E. and von Lucadou, W. 'Methoden und Ergebnisse der Psychokinese-Forschung', in Condrau (ed) Transzendenz, Imagination und Kreativität. Die Psychologie des 20. Jahrhunderts. Band XV, Kindler, Zurich, 1979, 546-559.

von Bertalanffy, L. 'General system theory', New York, 1968.

- Buchel, W. 'Naturphilosophische Überlegungen zum Leib-Seele-Problem', *Philosophia Naturalis*, 15, 1975, 308-343.
- Bunge, M. 'The mind-body-problem in an evolutionary perspective', *Ciba Foundation Symposium 69, Experta Medica*, Amsterdam/Oxford/New York, 1979, 53-63.
- Chester, A.N. 'A physical theory of psi based on similarity', *Psycho-energetic Systems*, 1981, 4, 89-111.
- Culbertson, J.T. 'The spacetime structure of mental images, Part I, II, in Krippner S.: *Psychoenergetic Systems*, Gordon and Breach, New York/London/Paris, 1979, 51-113.
- Dobbs, A. 'The feasibility of a physical theory of ESP', in Smythies, J.R. (ed): *Science and ESP*, Routledge and Kegan Paul, London, 1971, 225-254.
- Firsoff, V.A. 'Life, mind and galaxies', Oliver and Boyd, Edinburgh/London, 1967.
- Firsoff, V.A. 'Can psychic phenomena be fitted into the framework of physics', *Parascience conference*, London, 1976.
- Gödel, K. 'Formal unentscheidbare Sätze in der Principia Mathematica', *Monatshefte für Mathematik und Physik*, 38, 1931, 173-198.
- Haken, H. 'Synergetics and the problem of selforganization', in Roth, G. and Schwegler (eds): *Selforganizing systems*, Campus, Frankfurt/New York, 1979.
- Hofstadter, D.R. 'Gödel, Escher, Bach: An eternal golden braid', Basic Books, New York, 1979.

Hofstadter, D.R. 'Ein Streifzug durch die absurden und tiefgrundigen Regionen der Selbstbezugs', Spektrum der Wissenschaft, Marz, 1981, 6-10. Translation from Scientific American, January 1981.

Hofstadter, D.R. 'Personal remark in a letter to Prof. W. Gauger, Berlin. By courtesy of W. Gauger.

Hubner, P. 'Einfuehrung in die Methodenlehre der Psychologie', Wissenschaftliche Buchgesellschaft, Darmstadt, 1980.

Jammer, M. 'The philosophy of quantum mechanics', Wiley, New York, 1974.

Jantsch, E. 'Die Selbstorganisation des Universums', Hanser, Munchen, 1979.

Kleinknecht, R. 'Bemerkungen uber die vermeintliche Losung des Antinomienproblems', Zeitschrift fur allgemeine Wissenschaftstheorie, VII/1, 1976, 124-126.

Koestler, A., Smythies, J.R. and Raymond 'Das neue Menschbild', Wien/Munchen/Zurich, 1970.

Kornwachs, K. and von Lucadou, W. 'Psychokinesis and the concept of complexity', Psychoenergetic Systems, 1979, 3, 327-342.

Lawden, D.F. 'Separability of psychophysical systems', Psychoenergetic Systems, 1981, 4, 1-10.

von Lucadou, W. und Kornwachs, K. 'Parapsychologie und Physik', in Condrau, C. (ed): Transzedens, Imagination und Kreativitat. Die Psychologie des 20. Jahrhundert. Band XV, Kindler, Zurich, 1979, 581-590.

von Lucadou, W. and Kornwachs, K. 'Development of the system theoretic approach to psychokinesis', E.J.P., 3, 3, 1980, 297-314.

von Lucadou, W. and Kornwachs, K. 'On the structure of observational theories', Parascience Conference, London, 1980.

Meschkowsky, H. 'Wandlungen des mathematischen Denkens', Vieweg, Braunschweig, 1964.

Millar, B. 'The observational theories: a primer', E.J.P., 2, 1978, 303-332.

Mittelstaedt, P. 'Philosophische Probleme der modernen Physik', Bibliographisches Institut, Mannheim, 1966.

Nickles, T. 'Reduktion/Reduktionismus, in Speck, J. (ed): Handbuch wissenschaftstheoretischer Begriffe, UTB Vandenhoeck, Gottingen, 548-553.

Pittioni, V. 'Kritik an Wandschneiders Konstruktion', Zeitschrift fur allgemeine Wissenschaftstheorie VII/1, 1976, 127-132.

Prigogine, J. 'Vom Sein zu Werden', Piper, Munchen, 1979.

Rosen, R. 'Fundamentals of measurement and representation of natural systems', North Holland, New York, 1978.

Rosen, S.M. 'The concept of the infinite and the crisis in modern physics', Parascience Conference, 1980.

Ruderfer, M. 'Correspondence', J. A.S.P.R., 62, 1968, 84-86.

Ruttenberg, A.J. 'Introduction to the general system basis of psychoenergetics', in Krippner, S. (ed): *Psychoenergetic Systems*, Gordon and Breach, New York, 1979, XVII-XXVI.

Sadowsky, N.N. 'Problems of a general system theory as a meta theory', *Ratio* 16, 1974, (engl.ed.), 33.

Simon, H.A. 'The architecture of complexity', *Proceedings of the American Society for Philosophy*, 106, 1962, 467-482.

Vollmer, G. 'Evolutionare Erkenntnistheorie', Hirzel, Stuttgart, 1975.

Walker, E.H. 'The quantum theory of psi phenomena', *Psychoenergetic Systems*, 1979, 3, 259-299.

Wandschneider, D. 'Zur Elimination des Godelschen Unvollständigkeitsproblem im Zusammenhang mit dem Antinomienproblem', *Zeitschrift für allgemeine Wissenschaftstheorie*, VI/1, 1975, 65-83.

Weiner, D.H. and Bierman, D.J. 'Toward a definition of observation: A test of the effects of its information specificity and meaningfulness on PK', *Research Brief P.A. Convention*, 1981.

von Weizsäcker, E. 'Erstmaligkeit und Bestätigung als Komponenten der pragmatischen Informationen', in von Weizsäcker, E. (ed): *Offene Systeme*, Klett, Stuttgart, 1974.

PSI AS NATURE

Keith Harary

A NEW DISCIPLINARY MATRIX

Most approaches to psi research have assumed that the laws of nature are known and that psi refers to certain interactions that impinge upon a non-psychic universe. Psi may also be approached from the perspective that all natural laws are not known and that there are actually psi aspects of nature. This approach represents a paradigm-shift, or what Kuhn (1970) would more correctly term a new disciplinary matrix for psi research.

This new approach assumes that certain laws govern natural relationships and that observers do not perceive all of nature because they exist within it. As a result, observers perceive nature as not usually functioning psychically and consider psi relationships to be the results of interactions that influence nature to permit psi to occur.

In fact, psi relationships may be operationally defined as certain aspects of nature's normal functioning that observers do not usually notice. Observers base their view of nature upon their usual perceptions and define certain less frequently perceived relationships, which do not fit in with this limited perspective, as extraordinary occurrences.

To an extent, therefore, psi relationships are observer-defined because they are those aspects of nature, or physical reality,

which observers are not usually aware of. These psi aspects of nature may be somehow different (in ways that are not yet understood) from the more frequently observed non-psi aspects of nature, but they are considered here to be an integral part of nature's lawful functioning.

DEFINITION OF TERMS

In the disciplinary matrix offered here, 'psi relationships' refer to certain aspects of nature that are not usually perceived by observers. Conversely, 'non-psi relationships' refer to aspects of nature which observers usually do notice. It is understood that both psi and non-psi relationships interpenetrate nature.

'Nature' refers simply to the organized system that contains all that exists. Observers, and everything else in nature, are considered to have psi and non-psi aspects, or those aspects which are in psi or non-psi relationships with the rest of nature.

The aspects of observers that respond to psi and non-psi aspects of nature may be referred to as 'psi-senses' and 'non-psi senses', respectively. By definition, psi senses are not typically noticed by observers since these aspects of the observer belong to that part of nature of which observers are not usually aware.

'Psi processes' refer to ways in which lawful psi relationships are aspects of interactions in nature that may not directly involve observers. 'Observer psi processes' are ways in which observers respond to, and interact with, psi aspects of nature.

'Psi observers' refer to aspects of nature which are capable of being aware of, and willfully responding to, their own psi relationships with the rest of nature. 'Psi detectors' refer to those aspects of nature which are not consciously aware of, or capable or willfully responding to, their psi relationships with nature but which simply express their own participation in ongoing natural processes through their interactions. Psi observers who are not conscious of, and willingly responding to, their psi relationships are essentially functioning as detectors.

WHAT IS PSI?

Psi processes are viewed as principles of interaction that permeate nature, rather than as autonomous energetic influences that effect the outcome of relationships, or as independent realities superimposed upon the universe that observers are familiar with. From this perspective, if observers could perceive nature objectively, they would see that psi exists as an inherent lawful aspect of one reality that they usually only partly perceive, not as a supernatural force or a separate reality.

Why would observers usually perceive and respond to certain aspects of nature and not to others? The fact that psi relationships are somehow different from non-psi relationships, in ways that are not presently understood, simply defines that which observers respond to and does not explain influences that lead observers to respond to nature in the way that they do.

The aspect of the observer that responds to psi may be a usually unconscious process, as in Stanford's PMIR (1974). As has already been suggested, it may be a part of that aspect of nature which the observer does not usually perceive.

CULTURAL CONDITIONING

Why may observers generally not be consciously aware of an important part of their own interaction with nature? A possible explanation may be found in cultural conditioning. Many cultures perpetuate a more or less limited view of everyday reality by conditioning observers to consciously attend only to certain aspects of nature. Once a certain cultural frame of reference is in effect, it tends to reproduce itself by creating reality-loops based upon limited descriptions of nature which lead to limited perceptions (Carroll, 1956). Thus the origins of the human observer's response to psi in nature may lie in the origins of culture.

If this is true, observers from certain cultures might be more psi-responsive than others. This may explain why certain observers would tend to function more at the level of unconscious detectors

(PMIR) than as truly psi-conscious observers.

Non-human forms of life may respond more directly to psi aspects of nature because their psi senses may not be subject to cultural interpretation. It is not clear, however, whether these life forms would be responding as psi observers or as detectors.

If the psi attentiveness of human observers is influenced by their cultural conditioning, it may be possible for these observers to learn to attend to their psi senses by overcoming cultural biases and paying attention to psi aspects of nature.

IS PSI GOAL-ORIENTED?

Traditional approaches to psi have assumed what Stanford (1977) terms a 'psychobiological' or 'cybernetic' perspective in which psi is viewed as a step-by-step transmission of information or influence by organisms, that permits psi to occur through observer feedback assisted by psi perceptions. Stanford argues that cybernetic models do not hold up because they do not explain why observer psi responsiveness is apparently not influenced by the observed complexity of natural processes. Instead, Stanford's Conformance Behavior model suggests that psi functioning is actually not cybernetic, but goal-oriented. Stanford is probably correct in suggesting that the traditional cybernetic approach needs to be revised, but his suggestion that psi processes are goal-oriented represents the total abandonment of a perspective that may still have some useful components.

Perhaps a broader definition of cybernetic interactions is required. The old cybernetic perspective assumed that psi senses interacted with non-psi aspects of nature. The disciplinary matrix offered here provides a more internally-consistent view in which psi-senses observe and interact with psi aspects of nature and in which the apparent non-psi complexity of an aspect of nature may not necessarily be equated with its participation in psi relationships. If observers perceive only the changes in non-psi aspects of reality that are the result of the cybernetic interactions on a psi process level, these changes will indeed appear to be goal-oriented.

Psi processes are considered to be isomorphic to non-psi processes in nature because both are different interpenetrating aspects of the same continuous reality. Using non-psi observer processes, for example, if an observer decides to drink from a bottle, he must first notice the bottle and his non-psi relationship to it, then influence his non-psi relationship with the bottle in order to achieve the desired goal.

By the same token, an observer's psi response may be contingent upon his noticing his psi relationship with the psi aspect of that with which he wishes to interact, and upon his influencing that relationship through observer psi-processes assisted by feedback from psi senses.

Observer psi interactions may have consequences for non-psi aspects of nature since both psi and non-psi processes are viewed as interpenetrating aspects of one continuous reality. Observations of psi aspects of nature may also be translated into information about non-psi aspects for the same reason. Displacement effects, apparent analytical and associative interpretations of psi information, and other observer difficulties, may possibly be explained as distortions in the translation process between psi and non-psi aspects of reality.

NEW EXPERIMENTAL APPROACHES TO PSI INFLUENCES

If psi processes permeate nature, it may be possible to measure psi aspects of relationships that do not directly involve observers. Future research may explore ways in which psi detectors may reach beyond observer limits to enhance and augment the observer's awareness of psi processes. Conceivably, any natural interaction may potentially function as a psi detector. If psi processes are an aspect of all interactions, it may not be possible for an absolutely random process to occur in nature because what observers have traditionally considered to be random processes may actually contain psi components. For example, there may be underlying biases in what observers have considered to be random events. There is some indication in the work of G.S. Brown (1953; 1957) and others that such biases may have already been discovered.

If this is correct, and if it is possible to link the apparently random processes of random events generators to information components that are discernable to observers, it may be possible to develop devices that will express psi processes to psi observers as recognizable psi information. This might be accomplished by using a random event generator to select bits of information from computer programs that contain possible descriptive elements of individuals or geographical locations. The lists of elements thus obtained might provide accurate descriptions of target persons or sites because the apparent randomness of the generator may contain psi aspects that would influence the selection process.

In experiments that explore observer responses to psi processes (psychokinesis) the observer is not viewed as standing apart from nature and create separate influences upon it. Instead, the observer is viewed as an active participant in ongoing psi relationships who influences nature by first becoming aware of psi processes and then responding to them. Understanding and developing methods for measuring psi processes themselves will help us to comprehend the observer's response to these processes. This awareness may also lead to the development of devices that amplify and direct the psi aspect of natural interactions.

Experiments should also be done to examine possible psi components of the target selection process in free-response studies. Targets may be chosen, in free-response studies using random event generators, which bear some recognizable relationship to the observers for whom they are selected.

EXISTING EXPERIMENTAL APPROACHES TO PSI SENSES

How may human observers become more consciously aware of their own psi senses? Many existing experimental studies have relied upon an 'altered states of consciousness' approach to observers in an attempt to make them more attentive to psi aspects of nature. These studies, such as Ganzfeld experiments by Honorton and others (e.g. Honorton and Harper, 1974), have attempted to reduce non-psi input by blocking out local environmental stimuli from psi observers. This approach recognizes the need to make observers more attentive to psi but assumes that psi is an internal observer

process, rather than an interpenetrating aspect of all states of awareness and reality.

Altered states of consciousness experiments may filter out certain types of environmental input (by replacing it with other types of external input), but, if psi is an interpenetrating aspect of nature, both externally and internally focused states may include psi and non-psi components. It is not, then, possible to use altered states to filter out psi versus non-psi halves of the universe. Rather psi information may be available for discrimination in any state of consciousness. Altered states of consciousness experiments, such as the Ganzfeld, may simply provide observers with a forum in which to report interpenetrating psi and non-psi aspects of their imagery.

Admittedly, a large body of data seems to support the notion that altered states are psi-conducive (summarized in Palmer, 1978). Significant scoring by psi observers in altered states experiments may, however, be the direct result not of altered states manipulations but of other factors that may influence observer attentiveness, confidence, general orientation to the possibility of psi functioning, or other variables (Rogo, 1976).

The efficacy of altered states manipulations for improving observer psi awareness is difficult to assess in many experiments because these experiments have not used control groups. It would be necessary to compare observer psi responses in altered versus normal states in order to conclude that altered states are especially psi-conducive.

L. and W. Braud (1974) reported a study in which they compared observer psi responses in relaxed versus tensed states of consciousness. This is different from comparing responses in experimental versus control conditions because all observers in this study were manipulated. This study is still important because observers in the relaxed condition achieved significantly higher psi scores than those in the tensed condition, despite the fact that both groups of observers reported sharing similar beliefs in the possibility of psi functioning and in the psi-conducive interpretation of their respective experimental conditions. A positive correlation was also found between the degree of observer-reported and electromyographically-recorded relaxation and level of observer psi functioning.

In the Braud study (as in other altered states experiments) the experimenters were not blind to experimental conditions and may have communicated a strong belief in the psi-conducive interpretation of relaxed states. This experimenter effect (Kennedy and Taddonio, 1976) could have influenced observer confidence and willingness to respond to psi aspects of their awareness in various conditions. Observers who most accepted the psi-conducive interpretation of relaxed states might be more suggestible than other observers, and might therefore also be more likely to report and experience significant degrees of relaxation. The experimenter effect would probably occur at the subtle level of interaction and would therefore not be easily recognizable. Observers in all conditions might wish to satisfy the immediately perceived demands of the experimenters and might consequently report that they believe in psi and in their own ability to function psychically in their respective conditions.

To further complicate matters, a strong cultural bias exists (as expressed, for example, in the mass media) in favor of the psi-conducive-states interpretation of psi functioning. This bias reinforces the idea that psi functioning is strange and difficult to accomplish. Observers who are faced with any state manipulation in psi experiments are given this same message. Under these circumstances, most observers would probably find it easier and more enjoyable to be relaxed and might therefore be motivated to provide better reports of their imagery in this condition than in a tensed condition. If such a message were not communicated to observers, in the very design of psi experiments themselves, observers might just as easily respond to their psi senses in normal states of consciousness.

Experiments by Warcollier (1948), Puthoff and Targ (1976), and others (e.g. Dunne and Bisaha, 1978, 1979; Jahn et al, 1980) suggest that altered states are not necessary precursors to observer psi discrimination. Some of these studies have focused upon having observers recognize associative and analytical interpretations in their responses. These attempts to elicit and examine psi functioning in normal states appear to be at least as successful as altered states approaches have been.

Altered states approaches have given observers the message that they are not responsible for consciously discriminating psi aspects of their awareness from non-psi aspects. Instead, in

experiments such as the Ganzfeld, observers have been told to simply report all of their imagery and associations in a free-wheeling fashion and to expect that psi information will emerge in their reports on its own.

Perhaps those observers who are convinced to believe that altered states are psi-conducive permit themselves to be attentive to their psi senses in altered states experiments. These observers may be as attentive to psi senses in normal states when they are convinced that these states are also psi-conducive. Certain experimental conditions may be more convincing, in this regard, than others, just as certain observers may be easier to convince of their ability to function psychically under certain conditions.

Altered states may indirectly help some observers to identify psi processes by loosening the grip of cultural conditioning upon their experience of reality, so that they may become better able to respond consciously to psi aspects of nature in any state of awareness. Altered states may simply make it easier for some psi observers to provide undifferentiated reports of their imagery, which would be expected to contain some percentage of psi information. Others might find altered states manipulations to be a distraction, or an intrusion, which might explain reports of 'psi-missing' associated with pronounced altered states in some experiments (e.g. Parker, 1975; Palmer et al, 1977). Certain altered states may actually flood psi observers with too much input from their unconscious by increasing psi observers' analysis of, and free-association to, internal experiences.

NEW APPROACHES TO PSI SENSES

Systematic comparisons of observer responses in altered versus normal states may lead to an understanding of ways in which observer mentation may be influenced by internal and external input and may lead to the conclusion that altered states do not help observers to discriminate psi or non-psi aspects of nature any more effectively than do normal states. If altered states of consciousness approaches do not actually enhance the ability of observers to discriminate psi processes, what approach would be more productive? Perhaps research efforts should be focused upon defining special characteristics of psi aspects of imagery and

upon using this awareness to increase the percentage of psi information content in observer mentation.

Research may be conducted which will be aimed at understanding the specific ways in which particular observers respond to psi aspects of nature at conscious and unconscious levels. For example, research should be done to explore symbolic, linguistic and cognitive differences between psi and non-psi aspects of observer mentation. Psi aspects of observer mentation may be expressed in special ways and may feel somehow experientially different, in any state of consciousness, from non-psi aspects of awareness.

Observers may be in the best cognitive position to discriminate psi information among their own images. Some experimenters have considered this possibility and have, therefore, asked observers to compare their own responses with target pool contents. The sorting out of psi components of observer mentation has, however, often been an after-the-fact proposition in altered states experiments, wherein observers (or judges) have been presented with target pools to compare with transcripts of observer mentation at the end of experiments. Comparisons have been made between all observer images and all target pool possibilities. This is quite a different approach from helping observers learn to recognize which aspects of their imagery represent psi components, and then comparing these specific aspects to the target pool possibilities in order to discriminate which was the actual target.

Since there is, by definition, a difference in the way that observers respond to psi and non-psi aspects of nature (i.e., they actually pay more attention to one than to the other), there may be some discernable difference between these aspects of observer mentation that can only be recognized, by observers themselves, at the moment in which they are actually responding to their psi senses. Giving observers the responsibility for identifying psi aspects of their awareness, while they are responding to an experimental target, may help observers learn to overcome possible analytical or associative distortions in their imagery and respond directly to discernable differences between psi and non-psi observations.

Experiments that give observers the message that it is not

possible for them to tell the difference between psi and non-psi processes may deny the real psi observation capabilities of observers and may thereby destroy their confidence and their motivation for attending to psi processes at a conscious level. This message may be communicated implicitly by telling observers to provide undifferentiated accounts of all their images, by asking observers to consider all aspects of their imagery equally in the judging process, or by having persons other than the observer attempt to compare observer mentation to target pool possibilities. Giving observers the responsibility for consciously discriminating psi aspects of their awareness on an ongoing basis may provide the kind of motivation that is necessary for more successful and replicable psi experiments.

Research on the relationship between psi senses and creativity should help us to understand ways in which observers may translate their psi responses into recognizable imagery. Highly creative observers might, for example, be more consciously aware of their psi senses than less creative observers because they might be better able to form intelligible images out of their psi impressions. Preliminary research (e.g. Moss, 1969) has already been done in this area and should continue. Observers who are most able to pay attention to subtle details of non-psi aspects of nature may also be more attentive to psi aspects of their awareness.

Other research should explore ways in which observer psi processes may have been influenced by cultural conditioning. Cross-cultural studies will be important in helping to develop this understanding. At present, relatively little cross-cultural research (e.g. Van de Castle, 1974) has yet been carried out. Personality correlates of psi functioning may only be properly understood within a cultural frame of reference.

Within cultures that condition observers to not notice psi aspects of nature, some observers may be more open to the possibility of psi functioning than others and so may function more effectively in psi experiments. Some of these observers may be more responsive to experimenter suggestions of success, while others might be less attached to prevailing cultural definitions of reality. These observers might be inherently more psi responsive than others and so may reject cultural definitions of reality because these definitions do not coincide with their own

experiences. Others might be more prone to question cultural values and conditioning in general and so may relate to psi as simply another area in which they will form their opinions through firsthand experimentation and experience. Preliminary studies of personality correlates of psi functioning have been done (e.g. Schmeidler and McConnell, 1958; Palmer, 1971, 1972) and should continue.

Research should also be conducted with different types of observers in order to develop an understanding of the nature of psi functioning in various species. Certain very simple species might function as psi detectors while more complicated species might function as psi observers.

CONCLUSION

The suggested approach to psi research that has been offered here may lead to a better understanding of psi functioning in observers and detectors. Among the implications of this approach are the possibility of increasing the reliability of observer psi responses, and developing devices that are capable of manifesting and interacting with psi aspects of nature. The scientific community's response to the disciplinary matrix which has been presented may help determine the directions that psi research will take in the future.

ABSTRACT

A new disciplinary matrix for psi research is presented. This alternative approach treats psi as a process which permeates nature, rather than as an ability that is confined to the interactions of organisms or as an external influence upon their environment. This approach suggests that, to understand psi, we must develop methods for directly measuring natural processes that we do not yet fully understand and for understanding our own responses, and the responses of other types of observers and detectors to these processes.

ACKNOWLEDGMENT

The author would like to express appreciation to Dr. J. Palmer, Dr. H.E. Puthoff, Dr. R. Targ, Ms. C. Irwin and Ms. L. Vilenskaya for their comments and suggestions, and to SRI International and the Parapsychology Research Group for their support in the preparation of this paper.

This paper was presented at the 1981 Convention of the Parapsychological Association, Syracuse University, August 1981.

REFERENCES

Braud, L.W. and Braud, W.G. 'Further studies of relaxation as a psi-conductive state', J. A.S.P.R., 1974, 68, 229-245.

Brown, G.S. 'Statistical significance in psychical research', Nature, July 25, 1953, 172, 154-156.

Brown, G.S. 'Probability and statistical inference', New York, Longman's Green and Co., 1957.

Carroll, J. (ed) 'Language, thought, and reality', New York, John Wiley and Technology Press, Massachusetts Institute of Technology, 1956.

Dunne, B.J. and Bisaha, J. 'Multiple channels in precognitive remote viewing', Research in parapsychology 1977, Metuchen, N.J., Scarecrow Press, 1978, 146-151.

Dunne, B.J. and Bisaha, J. 'Precognitive remote viewing in the Chicago area: A replication of the Stanford experiment', J.o.P., 1979, 43, 17-30.

Honorton, C. and Harper, S. 'Psi mediated imagery and ideation in an experimental procedure for regulating perceptual input', J. A.S.P.R., 1974, 68, 155-158.

Jahn, R.G., Dunne, B.J., and Jahn, E.G. 'Analytical judging procedure for remote perception experiments', Paper presented at the Twenty-third Annual Convention of the Parapsychological Association, Reykjavik, Iceland, August 1980.

Kennedy, J.E. and Taddonio, J.L. 'Experimenter effects in parapsychological research', J.o.P., 1976, 40, 1-33.

Kuhn, T.S. 'The structure of scientific revolutions' (second edition), Chicago, University of Chicago Press, 1970.

Moss, T. 'ESP effects in "artists" contrasted with "non-artists"', J.o.P., 1969, 33, 57-69.

Palmer, J. 'Scoring in ESP tests as a function of belief in ESP. Part I: The sheep-goat effect', J. A.S.P.R., 1971, 65, 373-408.

Palmer, J. 'Scoring in ESP tests as a function of belief in ESP. Part II: Beyond the sheep-goat effect', J. A.S.P.R., 1972, 66, 1-26.

Palmer, J., Bogart, D.N., Jones, S.M., Tart, C.T. 'Scoring patterns in an ESP ganzfeld experiment', J. A.S.P.R., 1977, 71, 121-145.

Palmer, J. 'Extrasensory perception: research findings', *Advances in parapsychological research*, 2, New York, London, Plenum Press, 1978, 59-243.

Parker, A. 'Some findings relevant to the change in state hypothesis', *Research in parapsychology* 1974, Metuchen, N.Y., Scarecrow Press, 1975, 40-42.

Puthoff, H.E. and Targ, R. 'A perceptual channel for information transfer over kilometer distances: Historical perspective and recent research', *Proceedings IEEE*, 1976, 54, 329-354.

Rogo, D.S. 'Research in psi-conductive states: Some complicating factors', *J.o.P.*, 1976, 40, 34-35.

Schmeidler, G.R. and McConnell, R.A. 'ESP and personality patterns', New Haven: Yale University Press, 1958.

Stanford, R.G. 'An experimentally testable model for spontaneous psi events. Part I: Extrasensory events', *J. A.S.P.R.*, 1974, 68, 34-57.

Stanford, R.G. 'An experimentally testable model for spontaneous psi events. Part II: Psychokinetic events', *J. A.S.P.R.*, 1974, 68, 321-356.

Stanford, R.G. 'Are parapsychologists paradigmless in psi land?', In B. Shapin and E. Coly (eds) *The philosophy of parapsychology*, New York, Parapsychology Foundation, 1977.

Van de Castle, R. 'An investigation of psi abilities among the Cuna indians of Panama', In A. Angoff and D. Barth (eds) *Parapsychology and anthropology*, New York, Parapsychology Foundation, 1974, 80-97.

Warcollier, R. 'Mind to mind', New York, Collier Books, 1953 (originally published 1948).

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SOME NOTES ON PARAPSYCHOLOGY IN CHINA

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In September, 1981, the Chinese magazine 'Ziran Zazhi' (Nature Journal) published an article entitled "Some Experiments with the Moving of Objects through Exceptional Functions of the Human Body". Though the authors had some striking findings to relate, their work was no more sensational (or well-publicized) than many other reports that had been circulating in China since the spring of 1979.

Following upon the 1976 death of Mao Tse-tung and the subsequent deposition of the so-called Gang of Four, the late 1970's had seen the gradual restoration of something like normal academic and scientific life after the long period of disruption which had begun with the 'Cultural Revolution' in 1966. Aspects of this restoration included the reinstatement of full-fledged university education, resumption of publication in the case of many journals which had been inactive for ten years and more, and a positive re-evaluation of the role and function of scientifically trained specialists as opposed to the previous ideal of the politically fervent all-round man whose technical knowledge might be quite limited. On the whole, these trends were encouraged and supported by top-level Party and government circles; in view of the country's acute need for economic reconstruction and modernization, it seemed inevitable that more open study and discussion would have to be tolerated, even in fields which had recently borne the label of 'foreign bourgeois science' or 'bourgeois idealism'. Nevertheless, Chinese scientists, students, and editors remained cautious. A significant minority of the nation's leaders continued to oppose the newly emerging academic and scientific freedom, and the possibility of a radical Maoist

backlash could not entirely be discounted. As the experience of previous political campaigns had shown, such a backlash, if it came, could have literally fatal consequences for those who had committed themselves in print to opinions later judged politically unacceptable.

Such was the background against which parapsychology, surprisingly enough, began raising its head as a legitimate science in 1979. The public emergence of parapsychology is usually dated from March, 1979, when the influential provincial newspaper 'Sichuan Daily' published a report claiming that a 12-year-old boy had been found capable of 'reading' written material with his ears without the aid of visual perception. Nationwide media soon picked up on the story, and similar stories began to come in from other areas of China. The pros and cons were discussed for weeks; the 'believers' suffered a temporary setback in May, 1979, when the authoritative 'People's Daily' criticized the phenomena as illusory. Subsequent reports denounced the purported aural reading as nothing but a clever feat of parlor magic. But positive eyewitness reports continued, and in November, 1979, a conference was organized in Peking at which hundreds of children were examined and their cases discussed. The resulting testimony seemed to suggest that at least some of the cases were genuine.

In 1980, 'Nature Journal' organized conferences and demonstrations at which subjects, mostly children, are said to have performed successfully a wide variety of paranormal operations including eyeless reading, distant clairvoyance, psychic diagnosis of physical conditions, etc.. A brief and on the whole favorable account of the results appeared in the English-language magazine 'China Reconstructs' for January, 1981.

In May, 1981, at a conference in Chongqing (Chungking), it was decided that a Chinese Society for Human Body Science should be established to put the study of parapsychology on a well-coordinated scientific basis. Speeches held at the Chongqing conference and published in 'Nature Journal' in July, 1981, made repeated mention of some of the central concerns of Chinese parapsychologists after their first two years of public research and writing. These same concerns recur as the background to the article 'Some Experiments with the Moving of Objects...', as even a cursory glance at the following abstract will show. Certain subtleties in the phrasing and even in the punctuation of the

article can probably be read as indicative of the self-image (or self-presentation) of serious Chinese parapsychology anno 1981. A brief review of these points may contribute to a more meaningful reading of the abstract.

First of all, the Chinese writers stress the material side of their research. There are repeated calls for the recording of objective, concretely replicable data. In the case of Chinese parapsychologists, this caution probably implies more than just the normal rigors of conscientious scientific experimentation. Operating in the overall ideological context of a Communist country, Chinese scientists are wary of formulations that might smack of 'idealism'. They are anxious to view their research as an extension of the established sciences rather than as any sharp theoretical departure therefrom. It is very significant that the Chinese consistently refer to what the West calls parapsychology by the term 'renti teyi gongneng', meaning 'exceptional functions (or extraordinary capacities) of the human body'. The terminological anchoring in bodily functioning seems to suggest a materially accessible basis for research design, and for the eventual emergence of theoretical hypotheses that might not be too incompatible with existing neurological or brain-physiological models.

Another obvious feature is the interest in the native Chinese tradition. There is frequent reference to possible links with the theoretical tradition of Chinese medicine (the internal energy-circulation described by acupuncturists, etc.) and with the traditional practices collectively referred to as 'qi-gong' or 'energy work', in which various meditational techniques are correlated with supposed energetic processes both within the body and in the surrounding environment. The interest in Chinese medicine and 'qi-gong' is quite inevitable. Anyone growing up in China would have heard innumerable stories, claims, and rumors in these areas from childhood on, and the fact is that very large numbers of people apparently do derive positive benefits from the associated meditational, athletic, and dietetic practices. There is probably also a streak of national pride involved here. Many Chinese would like to break through the typical Western assumption that only the Western medical and physiological concepts are realities, and that the Chinese tradition in these matters is a kind of Curiosity Shop of makeshift techniques catering to the less educated. The Chinese are eager to systematize their

traditional empirical data and, if at all possible, to develop relevant theoretical models that could be of revolutionary scientific impact on a worldwide scale.

Their interest in the national tradition, however, poses a serious problem for Chinese researchers in parapsychology. Put quite simply, the theoretical rationales of Chinese medicine and 'qi-gong' have always gone far beyond what could be called a purely material basis. This problem has been felt by the Chinese government, for example, in the propagation of 'tai ji' (t'ai chi) and the other traditional martial arts. Much as the government encourages the populace to study such practices as 'tai ji', an ideological problem is involved, since the more the student advances in these arts the more he necessarily moves in the direction of such legendary techniques as the 'seeing out of the back of one's own head', the supposed bundling of energy within one's body and then sending it out through objective space with drastic physiological effects on the target person, etc.. In practice, official training manuals often emphasize the visible, bodily postures and movements involved while shying away from clear statements on the exact status or meaning of the more subtle concepts handed down by tradition. A frequent ploy in this connection is to admit the existence of the traditional lore of paranormal feats, and then to round off the discussion with a quick pat statement to the effect that further scientific research is needed to determine the reality value of these aspects.

In any case, the Chinese experimenters seem intrigued by their national tradition while not knowing quite how to deal with it from an acceptable scientific or ideological point of view. Indeed, in the very first paragraph of the article on the 'moving of objects', the authors refer to the states of appearance and disappearance, respectively, as 'you' and 'wu'. In traditional Chinese metaphysics whether Buddhist or Taoist, these are stock terms meaning 'manifestation' and 'non-manifestation', or 'creation' and 'non-creation'. In the People's Republic, of course, such philosophical concepts have tended to be regarded as 'superstitious' or 'idealist'. The authors of the article abstracted here, while using these terms, place them within quotation marks.

Since the publication of 'Some Experiments with the Moving of Objects...', the parapsychology debates seem to have continued in

China. In February of this year, the Chinese Academy of Sciences held a public hearing on this subject. In the words of a summary which appeared in 'China Reconstructs' in June, "the reported cases were analyzed, and the great majority shown to be unfounded ... The day after the hearing, February 25, 'People's Daily' summarized criticisms made, reviewed the news coverage of the past several years, and said that in its opinion there was no solid evidence for the existence of paranormal powers. Nevertheless, the debate goes on in scientific and lay circles, and experimentation continues regarding what has become a highly emotional issue".

LITERATURE

He Chongyin, 'Renti teyi gongneng yi nian lai yanjiu jinzhan he zhanwang' (Progress in research on exceptional functions of the human body over the past year, and future prospects), in 'Ziran Zazhi' (Nature Journal) 4:7, July 1981, 492-496.

Nie Chunrong, 'Tuidong renti kexue yanjiu' (Promote research in human body science), in 'Ziran Zazhi' (Nature Journal) 4:7, July 1981, 489-491.

'The Parapsychology Controversy', in 'China Reconstructs' 31:6, June 1982, 51.

Zheng She, 'Parapsychology, Is it Real?', in 'China Reconstructs' 30:1, January 1981, 50-51.

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ABSTRACTS OF CHINESE REPORTS ON PARAPSYCHOLOGY

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Lin Shuhuang et al. 'Some Experiments with the Moving of Objects through Exceptional Functions of the Human Body'

Several different types of experiments are described in which youthful subjects succeeded in moving a material object from one location to another without apparent use of direct physical means. During each of these experiments, the object seemed to 'disappear' (quotes in original) for periods of up to several tens of minutes, subsequently 'reappearing'. The experimenters do not regard this as a true material 'disappearance', but rather as the temporary transfer of the object concerned into an 'yitai' ('altered state' or 'alternate state') which is inaccessible to normal sensory perception or to presently known detection procedures.

The experiments, which were carried out in late 1980 and early 1981, are described under three headings:

- 1) Experiments with the tracking of electromagnetic wave signals;
- 2) Experiments with photosensitivity; and
- 3) Experiments with transcendence of spatial-physical limitations.

1. Experiments with the tracking of electromagnetic wave signals

In each of these experiments, the object was a miniature radio transmitter, which was initially placed (usually) in a pocket or

on the person of the subject, a young girl. During a process which lasted typically several tens of minutes, the girl then caused the transmitter to be moved to a different location within the experimental area. Observers were present from beginning to end; in addition, the signal produced by the transmitter was monitored continually by electronic means. A typical experiment would include the following successive stages:

- 1) Subject declares object has 'disappeared'. Visual inspection fails to detect object. Signal emitted by transmitter still detectable, but subject to marked instability.

- 2) Subject feels object has 'passed into her body', describing a course ending 'in the brain'. Subject is agitated and complains of physical discomfort. Visual inspection fails to detect object. Signal amplitude, as monitored, strongly irregular including momentary total disappearances of signal.

- 3) Subject says object has gone 'out of the brain'; feels physically somewhat relieved. Transmitter's signal undetected by monitoring systems.

- 4) Signal returns, at first sporadically, then steadily.

- 5) Subject designates location at which object can be recovered; confirmed by subsequent search.

2. Experiments with photosensitivity

In these experiments, the object was a light-sensitive film or paper. The subjects were two young girls. Typically, the object was placed in a lightproof bag attached to the clothing of one of the subjects. The subjects, consciously co-operating, then caused the object to be transferred across the room into a second lightproof bag. Subsequent developing showed the recovered object not to have been affected by light; comparable control material was heavily affected after even momentary exposure to the obtaining light conditions. As in the experiments with the radio transmitter, transfer of the object was preceded by subjective reports of its having passed out of the original bag and into the brain of the subject, etc..

In one replication, the object proved not to have been transferred into the second lightproof bag itself, but to have been placed between this second bag and its outer lightproof wrapper.

3. Experiments with the transcendence of spatial-physical limitations.

These experiments were rather varied, but their overall import was similar to that of the experiments described above. The objects used included a small metal nut placed into a sealed film cassette case, a small piece of aluminum inside a sealed cloth bag, a pocket watch, etc.. The procedures were similar to those described above. Successful transfer of the objects (without breaking the seal of the film cassette case) was effected.

An advanced practitioner of the Chinese 'qi-gong' energy-meditation techniques claimed that when the object was 'going out' into the 'disappeared' state, and returning, he sensed 'bundles of radiance' leading from or to the subject.

CONCLUSIONS

Some subjects seem able to put material objects into an 'altered state' which is inaccessible to normal sense perception or other search procedures, and subsequently to move the objects back into other than their original location. Though these processes are not susceptible of perception by random observers, they can be sensed and concurrently described with some accuracy by the subjects, or by attendant 'qi-gong' adepts. The movements show a certain correlation with the subjects' mental processes or train of thoughts; they are, however, not entirely subject to deliberate control.

The movement of an object into or out of the 'altered state' cannot be a form of ordinary three-dimensional movement. Mechanical and biological processes seem to continue uninterrupted in the 'altered state' (this was shown by placing into the 'altered state', and then recovering, running watches and fruit flies, respectively). Signals emitted by a radio transmitter become greatly diminished in amplitude and/or disappear while the transmitter is in the 'altered state'. Light-sensitive material in the 'altered state' is unaffected by normal light.

The experimenters surmise that an object is energetically and

cognitively 'isolated' from the outside world while in the 'altered state', and that this state cannot be satisfactorily explained by existing theories.

SOURCE

Lin Shuhuang et al. 'Renti teyi gongneng yiwu de ruogan shiyan' (Some experiments with the moving of objects through exceptional functions of the human body), in 'Ziran Zazhi' (Nature Journal) 4:9, September 1981, 652-661. Thirteen authors are listed. They are associated with four institutions: (1) Peking Normal College, (2) Qinghe Secondary School, Peking, (3) Xiyuan Hospital, Institute for Chinese Medicine, and (4) National Defence Science Commission.

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LETTERS TO THE EDITOR

From Adrian Parker: In replying to Rex Stanford's letter (E.J.P., 1982, 4, 2, 277-280) could I first make the recommendation to the editors that they offer the author of the initial publication the right of reply before publishing the letter in question. This is the usual procedure in scientific journals and in this case it would have reduced the necessity for a further six months extension in debating a paper which was published in this journal nearly two years ago.

Stanford's main point is that I have done Carl Sargent an injustice by concluding that the personality differences from his study of Ganzfeld experimenters were "obtained by fitting the data to the hypotheses". In fact the offending phrase is lifted from a sentence which prefixes it with qualifiers and is thereby much less accusatory in its original context. The sentence reads: "From these considerations it would seem that without overstating the matter Sargent's research here amounts to little more than a dressed up case study in which results are obtained by fitting the data to the hypotheses". These 'considerations' included the arbitrary selection of experimenters, their arbitrary classification into groups, and the use of one's own personality profile along with the data. As well as the editors of the E.J.P., the paper was shown to other parapsychologists prior to publication, none of whom had any objections on the grounds of it exceeding the limits of decorum.

Nevertheless, I do think that Stanford has made an important point about professional rhetoric, which may have wider relevance. The tone of my article was set by that of Sargent's earlier papers and was an exercise at meeting him on this level. My present

feeling, now two years later, is that there is little to be gained from this type of confrontation and it is easy to loose sight of the issues involved. Indeed, I would have felt happier about Stanford's criticism had he been even handed in the direction of his comments.

Stanford's second point concerns my assessment of his auditory Ganzfeld study. The study is complex and it was difficult to go into a detailed analysis in what was a footnote to the paper. I assessed the study as non-significant with respect to replicating the original Harper and Honorton findings of an overall effect, since the results for the total uninterrupted pink noise condition were non-significant. Subsequent to Stanford's letter, it would now seem that the analysis for partial transcripts also fails to reach significance (Stanford, personal communication 1982).

Finally, as regards to the basic issue itself, of whether or not there is a genuine personality difference between successful and non-successful Ganzfeld experimenters, I think it is clear from the remainder of Stanford's letter that we are agreed the present data do not support any positive conclusions.

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From the editors: After this rejoinder by Dr. Parker we consider the debate on this issue finished.

ACKNOWLEDGMENT

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**EUROPEAN
JOURNAL
OF
PARAPSYCHOLOGY**

VOLUME 4

NUMBER 4

MAY 1983

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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 activity in 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 are considered more important than the level of significance of the outcome of the study. As a practical rule, we advise the potential contributor of an article to submit the design of his planned study before the work 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 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 dates, which are May 1st and November 1st.

DR. LOUISA E. RHINE, 'FIRST LADY OF PARAPSYCHOLOGY' PASSES AWAY

Another great loss has hit the parapsychological community. Dr. Louisa E. Rhine passed away on March 17, 1983, at the age of 91 years. She had been ill for only a few days. Married for about sixty years to Dr. J.B. Rhine (1895-1980), Mrs. Rhine was almost as much a part of the history of parapsychology as her husband.

Dr. Rhine distinguished herself as a creative and leading parapsychologist, not only through her very extensive studies of spontaneous cases, but also in laboratory studies, not least in the area of psychokinesis. She was much encouraged by her husband to specialize in the collection of spontaneous cases, and her 'Duke-collection' reached an impressive size (In 1973, it included 12659 ostensible ESP cases and 178 PK cases). She made a serious attempt to obtain a better understanding of the mental processes involved in the ostensible paranormal phenomena occurring in real life. By penetrating classifications and by conjectures about the meaning of the cases, she tried to generate new hypotheses of value for the laboratory study of psi. She generously shared her thoughts with other research workers interested in the same area of research. Our laboratory was privileged to get access to her collection for a study carried out by Dr. Sybo A. Schouten. She clearly ranked as one of the greatest authorities in the world on spontaneous cases. She was a surprisingly productive research worker, even at the end of her long life.

Most of her scientific contributions were published in the 'Journal of Parapsychology', the total which exceeded twenty papers. Among these papers, the following ones should especially be considered as important contributions: 'Placement PK Tests with Three Types of Objects' (Journal of Parapsychology, Volume 15,

Number 2, 1951); 'Conviction and Associated Conditions in Spontaneous Cases' (15, 4, 1951); 'Subjective Forms of Spontaneous Cases' (17, 1, 1953); 'Relation of Experiences and Associated Events' (17, 3, 1953); 'Frequency of Types in Spontaneous Precognition' (18, 2, 1954); 'Precognition and Intervention' (19, 1, 1955); 'Relationship of Agent and Percipient in Spontaneous Telepathy' (20, 1, 1956); 'Hallucinatory Psi Experiences' (published in three parts: 20, 4, 1956; 21, 1 and 3, 1957); and 'Toward the Understanding of Psi-Missing' (29, 1965). She also wrote a contribution to the 'Handbook of Parapsychology' (edited by B.B. Wolman, Van Nostrand Reinhold, New York, 1977, 59-80) entitled 'Research Methods with Spontaneous Cases'. The published books are: 'Hidden Channels of the Mind', 1961; 'ESP in Life and Lab', 1967; 'Mind over Matter', 1970; 'Psi. What Is It?', 1975; and after the death of her husband 'The Invisible Picture', 1981.

It should also be mentioned that Louisa Rhine functioned as a co-editor/consultant for the 'Journal of Parapsychology' from 1937 until her death. From the time of the death of her husband, she functioned as Executive Director of the F.R.N.M.. In 1980, she served as President of the British S.P.R.. She finished her long, productive life by completing a manuscript on a book that will shed further light on her life together with J.B. Rhine. This manuscript was handed over to the publisher just a few weeks before she died from a heart-attack.

The story of J.B. and Louisa Rhine is more than a fascinating story of love, dedication to each other, and dedication to certain ideas. It is also the story of much creative and extremely hard work. Furthermore, it is the story of more than 60 years of intensive discussion or dialogue with each other. Starting with religious, dogmatic issues; they soon drifted away from formal religiosity, becoming fascinated to the point of obsession with 'the scientific method'. Their life work mainly constituted a heroic attempt to try to show that psi phenomena could be successfully studied and at least partly understood by the 'scientific' approach. Nonetheless both the Rhines firmly believed that the investigation of psi phenomena had great philosophical (and metaphysical) implications.

Louisa Weckesser and Joseph Banks Rhine met when J.B.'s family moved to Chio. Rhine's family became neighbours to Louisa's family. Both had a religious background. Louisa's family was the

only one in the vicinity belonging to the strict and disciplined Mennonites. Louisa and J.B.'s first intellectual exchange centered around religious beliefs and dogma. If man did not have a free will, how could he then be made responsible for his actions? It is quite interesting to ponder how this issue remained a central one to the Rhines for the rest of their lives. Their deep-rooted interest in PK was probably very much related to their long-lasting and burning interest in the mind-body problem, since PK could have implications for the understanding and interpretation of the interaction between mind and body. Louisa married J.B. Rhine in 1920, and both took their academic degrees in botany (B.S., M.S., and Ph.D.) at the University of Chicago. A long-lasting companionship as man-wife and fellow-researchers started. They were both tremendously fascinated by the method of the natural sciences, which they both found generally to be 'objective' and reliable. The ideology of science which they maintained thereafter was clearly moulded by the 'scientism' they were exposed to and appreciated early in their careers. Odd as it may seem, the Rhines became somewhat impressed by Ernst Haeckel's materialism as well as by J.B. Watson's behaviourism. During their time at the University of Chicago, their heroes were clearly the pioneers in science, like Madame Curie, Albert Einstein, Ivan Pavlov, etc..

What turned them from their materialistic outlook was their acquaintance with the writings and thoughts of three great vitalists: the philosopher Henri Bergson, the biologist Hans Driesch, and finally William McDougall, who later on came to play such a crucial role in their lives and careers. Without the influence of these three vitalists, it is questionable whether they would ever have felt ready to turn to 'psychical research', an area of inquiry they were to profoundly affect. The rest of the illustrious careers of the Rhines is probably too well known to deserve discussion. It is common knowledge to readers of any parapsychological journal.

Louisa Rhine's impact on her husband's thinking must have been both extensive and varied, but not so easy to assess. However, I consider it a fascinating and important challenge to the historians of science and ideas to make a contribution to that assessment. There are still important witnesses alive and the great collection of correspondence now deposited at Duke University, should facilitate the task.

I was always impressed by Louisa Rhine's ability to listen to a debate, and suddenly, with the perfect timing, bring the discussion down to earth by making some very relevant points. She maintained that ability even at a very advanced age. Mrs. Rhine, besides being a versatile scientist, had many excellent human qualities. She functioned as a sensible and tactful mediator between J.B. Rhine and those around him. That task was certainly not always an easy one. She also functioned as a mother-figure for a couple of generations of parapsychologists who had the privilege to work at or visit the Parapsychology Laboratory, subsequently the Institute for Parapsychology. She shall also be remembered for her great hospitality and great ability to make people feel at home at their farm. She was an excellent cook. Her cherry-pie was the best I ever had!

My wife and I have many reasons to mourn the Rhines, and Mrs. Rhine just as much as J.B. Rhine. We became utterly moved by their hospitality and courtesy, even during the first weekend we spent in North Carolina in July of 1963. They came and picked us up at the Westover Apartments in Durham and took us out for sightseeing and dinner. On their way to pick us up, they had made a detour to pluck some delicious blackberries for us!

Mrs. Rhine dedication was not only to science and to J.B. Rhine. She was also a warm, understanding and supportive mother and grandmother. Surviving her are a son, Robert E. Rhine of Mexico City, Mexico; three daughters, Dr. Sally Feather of Durham, N.C., Elisabeth E. Rhine, who lived with her mother, and Rosemary Rhine of Portland, Oregon; two sisters, Sylvia Newcombe of York, Pa., and Miriam Whaley of Philadelphia, Pa.; one brother, Dr. Eldon Weckesser of Doylestown, Ohio; nine grandchildren and one great-grandchild.

The most appropriate way I can think of to commemorate Mrs. Louisa E. Rhine, the First Lady of Parapsychology, as Dr. Rao so aptly has named her, seems to me to give financial support to the Rhine's creation: the F.R.N.M. and its Institute for Parapsychology (P.O. Box 6847, College Station, Durham, N.C. 27708, U.S.A.), to ensure that the important work they started will continue.

Martin Johnson

IN MEMORY OF LAURA A. DALE

Karlis Osis
A.S.P.R.

Laura Abbot Dale, the distinguished editor of the 'Journal of the American Society for Psychical Research', died on February 2nd, 1983. Born on August 22nd, 1919, Laura grew up in New York. Her educational background focused on the humanities with an emphasis on music. After her close friend's untimely death, Laura experienced psychic phenomena first-hand and began her active involvement in parapsychology at the A.S.P.R.. She came to the Society at a time when it was undergoing a kind of rejuvenation under the influence of Gardner Murphy, and a new breed of researcher was emerging, e.g., Jan Ehrenwald, Jule Eisenbud, Ernest Taves, Montague Ullman. Laura herself played an essential role in this renaissance, as evidenced by the 16 journal articles which bore her name as author and co-author. Her research interests ranged from the analysis of spontaneous cases to experimental work, e.g., 'A Series of Spontaneous Cases in the Tradition of 'Phantasms of the Living'' (J. A.S.P.R., 1951, 85-101), 'The Psychokinetic Effect: The First A.S.P.R. Experiment' (J. A.S.P.R., 1946, 123-151). Laura also co-authored a parapsychological source book with Rhea White, as well as one with Gardner Murphy.

The life work of Laura Dale was editing the Journal and Proceedings of the A.S.P.R., which she did from 1944 through 1982, with a brief interruption in the mid-'sixties. As an editor, she developed remarkable competence and skills, which won her the high

esteem and appreciation of parapsychologists all over the world. In the last two decades, the Journal grew from 172 pages (in 1961) to 382 pages (in 1981); because of her efforts this publication rose to the top in the field.

Laura Dale was almost invisible to outsiders but within parapsychology she was one of the most influential figures of the last three decades. Single-handedly, Laura managed the Journal of the A.S.P.R., determining its standards and preferences. In other words, she had a very large say concerning which lines of research had easy access to publication and therefore helped determine the dominant thrust of American parapsychology. She admired the high standards and the conservative outlook of Eleanor Sidgwick, who similarly shaped the direction of early S.P.R. publications.

Laura did not hesitate to heavily edit the papers accepted for J. A.S.P.R., even when the authors were luminaries of their day. Papers dealing with the type of research she appreciated came out of the editorial process greatly improved. She had an admirable zest for work. Besides editing, she did all the chores of typing manuscripts and correspondence. In spite of this very heavy load, Laura still found time to give a helping hand to editing such important books as those produced by Gardner Murphy, Gaither Pratt, Ian Stevenson, Montague Ullman, and the Wolman 'Handbook of Parapsychology'. So it was also in this respect that she was an invisible but vital part of modern parapsychology.

Laura's parapsychological side was balanced by a rather specialized hobby: breeding dogs. She received many trophies for her own beautiful boxers and was a highly sought after judge of dog shows in the United States. In this sense, Laura Dale was a citizen of two worlds quite foreign to one another; she had many friends both among parapsychologists and dog breeders. Her occasional weekends at dog shows would invigorate Laura so that she would return to her Long Island home renewed and ready to again take on her efforts in parapsychology, which heavily taxed her energies and sometimes her patience.

Parapsychology would not be the same today without the invaluable contribution made to it by Laura Dale. Without her touch, many papers and books would have been less comprehensible and sloppier both in grammar and research standards. Many parapsychologists will long remember her with thankful hearts.

ESP AND "HYPNOTIC IMAGINATION" : A GROUP FREE-RESPONSE STUDY

John Palmer
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Ivo van der Velden

There is a relatively large amount of experimental evidence suggesting that subjects under hypnosis, in comparison to subjects in the waking state, generally perform better on both restricted-choice (Honorton & Krippner, 1969; Sargent, 1978) and free-response (Honorton, 1972; Honorton & Stump, 1969; Parker & Beloff, 1970) tests of ESP. This relationship, if genuine, could reasonably be attributed to the relaxed and focussed state of consciousness induced by most hypnotic induction procedures, to increased confidence resulting from the acceptance of explicit or implicit suggestions of success included in the induction, or both. There is other research, independent of hypnosis, which suggests that both altered states (Honorton, 1977) and belief in the possibility of ESP occurring under the conditions of the test (Palmer, 1971) are associated with more positive and/or significant scoring on ESP tests.

Research also suggests that, at least in free-response ESP tests, the superior performance of hypnotized subjects is restricted to those who score high on measures of hypnotic susceptibility (Honorton, 1972). This of course makes logical sense, as these are the subjects

Note: This paper fulfills the publication policy of this journal.

most likely to experience the intended effects of the hypnotic induction. However, hypnotic susceptibility has not been demonstrated to relate to free-response ESP in states other than hypnosis (Palmer & Lieberman, 1976). It has shown a significant positive relationship to scores on conventional ESP card tests in one pair of experiments (Stanford, 1972), but not in two others (Honorton, 1969; McBain, Fox, Kimura, Nakanishi, & Tirado, 1970).

The present experiment was conducted primarily to screen volunteer subjects for participation in further research. We wished to select subjects on the basis of a trait which we have labelled "hypnotic imagination" and defined as the capacity to experience and identify with cognitions in response to hypnotic suggestions. Although it is closely related to hypnotic susceptibility, it is not identical to it. For one thing, we are not interested in behavioral or ideomotor responses to suggestions, only cognitive responses. Secondly, we are not interested in hypnotic susceptibility insofar as it reflects responsiveness to authoritative directives or "demand characteristics". In keeping with this principle, our approach to hypnosis has been a very nonauthoritarian one.

We chose to define hypnotic imagination operationally by three separate instruments, each representing a different aspect of the construct.

1. Tellegen Absorption Scale (TAS). The TAS is a true-false questionnaire which measures in a straightforward manner the capacity of a person to become absorbed in fantasy (Tellegen & Atkinson, 1974). We chose this measure because we are particularly interested in the capacity of subjects to identify with what the hypnotist suggests, and it seems reasonable to assume that persons who can do this easily in daily life will, assuming they are also suggestible, be best able to do it in an experiment. The TAS correlates moderately with standard measures of hypnotic susceptibility and is orthogonal to standard measures of neuroticism and extraversion (e.g., Kiddoo, 1979; Tellegen & Atkinson, 1974). It has EEG correlates suggesting that high scorers are better able to block out competing mental activity when focussing on a task than are low scorers (Davidson, Schwartz, & Rothman, 1976). It has been reported to correlate significantly and positively with scores on a free-response ESP test in one study (Irwin, 1981).

2. Creative Imagination Scale (CIS). The CIS is a hypnotic susceptibility scale which has nonauthoritarian wording and which is

very much oriented toward how suggestions are experienced (Wilson & Barber, 1978). It can be administered with or without a preceding hypnotic induction. We chose to give the scale without a prior induction for two reasons. First, we were already using an induction for other purposes incompatible with the CIS. Second, we wanted to assess our subjects' capacity to respond to suggestions independently of their capacity to enter a "trance" state.

The CIS correlates moderately with more traditional measures of hypnotic susceptibility (Monteiro, MacDonald, & Hilgard, 1980; Wilson & Barber, 1978).

3. Hypnotic Depth Scale (HDS). The HDS is a homemade scale designed to provide a simple, straightforward measure of the depth of "trance" subjects could obtain in response to a "live" induction by the hypnotist (I.V.) who will be working with subjects in the future research. It consists of subjects reporting a number from one to ten representing the deepest trance they achieve during the induction. Although inspired by the depth scale proposed by Tart (1970), it differs in that subjects do not give their response during the hypnosis itself and it is not locked to a particular time in the hypnosis. Whereas it still is a measure of the deepest state of hypnosis the subject enters during the hypnotic induction, for purposes of the research we were interested in it primarily as a reflection of the subject's capacity to enter hypnosis (i.e., as a trait measure).

We planned to select for the future project people who obtained high scores on all these measures. It was our philosophy that these scales would compensate for each others' weaknesses and collectively cover each of the subsidiary characteristics which together comprise what we mean by "hypnotic imagination".

This context provided us an excellent opportunity to explore the correlation between hypnotic imagination and ESP. Thus a free-response ESP test, with different targets for each subject in each of our groups, was administered following the hypnotic induction, while subjects were still under hypnosis. We predicted that subjects who scored high on our collective measures of hypnotic imagination would show more ESP on the test than would subjects who scored low. However, since research on ESP in altered states seems to suggest that unidentified situational factors may determine the direction of the relationship (Palmer, 1979), we predicted that the direction would be

dependent upon the overall level of scoring on the ESP test: if the grand mean was positive, the correlation would be positive; if the mean was negative, the correlation would be negative.

Finally, we saw a convenient opportunity to test an extrapolation from the controversial "observational theories" (Millar, 1978). If, as these theories suggest, psi occurs at the time of feedback or "observation" of the results, and if hypnosis is psi-facilitatory, it then would seem to follow that subjects who observe or concentrate upon their target picture (after the "test") while hypnotized should score better than subjects who observe and concentrate upon their target only in the waking state. We tested this hypothesis by having half of our groups observe the target under hypnosis whereas the other groups did not. However, for practical reasons, all subjects had their first observation of the target in the waking state. We do not consider this in any way to be a crucial test of the observational theories, as the design is based upon assumptions beyond those made by observational theorists.

Summary of Predictions

We made the following three formal predictions:

1. Our three measures of hypnotic imagination -- TAS, CIS, and HDS -- will correlate positively and significantly with one another.
2. There will be a significant positive correlation between hypnotic imagination and ESP if the overall mean ESP score is above chance. If the mean is below chance, the correlation will be negative.
3. Among subjects who score above the mean on the HDS during rehypnotization, those who observed the target picture under hypnosis will score significantly higher on the ESP test than those who did not if the overall mean ESP score (for all subjects) is positive. If the overall mean is negative, the relationship will be the reverse.

METHOD

Subjects

The subjects were 150 volunteers, the great majority of whom responded to solicitations on University bulletin boards and the University student newspaper requesting subjects for a research project involving hypnosis and psi. Most were university students and only a few appeared to be over 40 years of age. There were 85 males and 65 females.

Setting

The experiment was conducted over a period of three weeks in a series of 12 group sessions, with eight to 16 subjects in each. All sessions were conducted in classrooms in the experimental psychology building at the University of Utrecht. Six of the sessions had to be conducted in a room facing a busy street and thus traffic noises could not be excluded. The other six sessions were held in a quieter room. Three of the sessions were held in the afternoon, all in the less quiet room. The remaining sessions were conducted in the evening. In all sessions, subjects were seated at long tables, two abreast, in cushioned, straight-back chairs with armrests. We succeeded in creating an informal, relaxed atmosphere in all the sessions, and most everyone seemed to enjoy the experiment.

Hypnotic Induction

The hypnotic induction was conducted orally by I.V. Although the content of the suggestions was standardized, their presentation and emphasis varied slightly from session to session, depending upon the general response of the group.

The induction, which lasted approximately 20 minutes, began with suggestions of eye fixation and closure. A permissive, Eriksonian approach was used throughout. Subjects were given the illusion of choice about entering trance and told that everyone has their own pace

and can choose which suggestions to use for entering or deepening trance. Suggestions were given for feelings of heaviness, warmth, and relaxation, all parallel with breathing, as well as suggestions of the dual sensations of drowsiness and attention. It was suggested that bodily distractions (e.g., eyeblinks) and outside noises could help one go deeper into trance.

Trance was formally deepened by developing the metaphor of a road. The counting technique was employed in conjunction with having subjects imagine moving along the road. A confusion technique was also used, built around multiple meanings of the word "way" (e.g., "on your way", "your own way"). Motivation to reach the endpoint was enhanced by a combination of delay and making attainment of the end seem increasingly attractive.

Summary of Predictor Scales

1. Tellegen Absorption Scale. We used a 37-item revised version of the original TAS. The great majority of subjects completed the scale at home prior to the sessions, with instructions to fill it out alone in an environment free from distractions. Five subjects filled out the scale during their sessions or afterwards.

2. Creative Imagination Scale. The CIS was modified slightly for appropriateness to the Dutch language and recorded on tape by I.V.

We also attempted to include an amnesia item on the end of the CIS by suggesting to subjects that they would forget the CIS exercises until told by I.V. that they could recall them. However, the responses indicated that so little amnesia in fact occurred that we decided not to analyze this measure.

3. Hypnotic Depth Scale. During the deepening phase of the hypnotic induction, subjects were asked to imagine travelling along a road to deeper and deeper hypnosis, each point on the road corresponding to a number from one to ten. "1" was defined as "the normal waking state" and "10" as "the deepest trance you can imagine ever to attain". At the end of the induction, subjects were told to remember the number corresponding to the farthest point on the road they reached during the hypnosis and to write it down, when requested to do so, following the deduction. The HDS was administered twice, once in conjunction

with the initial hypnotization (HDS1) and once with the rehypnotization (HDS2).

4. Attitude Scale. A nine-item "sheep-goat" scale, adapted from Palmer (1973), was used to assess subjects attitudes about psi as represented by Palmer's four definitions, as follows:

- SG1 : Attitudes toward psi tests in general and this test in particular (2 items)
- SG2 : Attitudes toward the existence of various psi phenomena (4 items)
- SG3 : Previous psi experiences (2 items)
- SG4 : Estimate of performance on the present test (1 item)

ESP Test

Target Pool. The target pool consisted of 60 magazine photographs depicting mostly outdoor scenes and reflecting varying degrees of complexity, action, and emotionality. Each photograph was reproduced twice. The photographs that actually served as targets were mounted on approximately A4 size (21 x 31 1/2 cm.) pieces of heavy cardboard. Smaller prints, 13 x 18 cm., were available for judging.

The pool was divided by J.P. into 15 sets of four each, in such a way as to maximize the diversity of content and emotionality within each set.

Selection of Targets. Two persons not otherwise associated with the experiment each selected the target sequences and prepared the target envelopes for six of the 12 sessions. Which selector did which sessions was determined in a quasi-random manner based upon J.P. shuffling a set of playing cards, but this sequence had to be modified slightly to adapt to the selectors' schedules.

Twelve pages of random numbers from the RAND (1955) tables were each placed inside small manila envelopes by J.P., shuffled, and then consecutively numbered. Each selector was asked independently to pick a number from one to 12, which then determined which page he would use for target selection. (They did not pick the same number). This procedure was intended to allow for a possible selector effect on the

target distribution, and thus possibly on the ESP scores as well.

The selectors were instructed to generate a target sequence of 15 numbers from one to four for each session, beginning at the upper left of the page and moving across. They then determined a separate sequence of 15 subject numbers by shuffling a deck of playing cards numbered one to 15. Thus, the first of the 15 numbers from the random number table identified which picture from Set 1 was to be the target, and this target was assigned to the subject with the first number in the sequence of subject numbers resulting from the shuffling. The selectors recorded these sequences of target and subject numbers in duplicate on prepared forms, one for each session, which they retained until that session had been completed.

The selector then prepared the target envelopes. These were manila envelopes which had taped to their faces two sheets of blank paper on which subjects were to record their impressions. The bottom sheet was specially treated so as to reproduce what was written on the top sheet, like carbon paper. The selector placed each target picture, face forward, inside the envelope with the appropriate subject number written in the upper left corner of the blank sheet. A piece of black construction paper was then placed in front of the picture inside the envelope, and the envelope was sealed. The envelopes were then rearranged in consecutive order by subject number and given to J.P. prior to the session.

Test Procedure. Immediately prior to the hypnotic induction, J.P. or an experimental assistant handed each subject one of the 15 envelopes, beginning with the top one. Thus, for example, if there were only ten subjects in a session, only those envelopes with subject numbers one through ten were utilized. As no one at the sessions knew which pictures were in which envelopes, or even which sets they came from, which subject received which envelope was of no importance.

Immediately following the hypnotic induction, the following instructions were given by I.V.:

You are now deeply relaxed and find yourself in a state in which you are completely open for the experiences to come, experiences you can have especially on this level of consciousness. Now you can have that special sensation for which you started this hypnosis. Now you have access to that particular capacity that

people have hidden somewhere deep inside themselves to become aware without senses, to see without eyes, hear without sound, or feel without touching. In a moment I will ask you to concentrate on the envelope lying in front of you, to turn your entire attention to the contents of the envelope, to penetrate with your mind into the envelope, where there is a picture, and in the next five minutes impressions will come to you. They may be images, thoughts, feelings, or sounds that relate to the picture in the envelope. These impressions can come to you in many ways; you have your own ways, your own ideas about this. Maybe you imagine yourself as if you were leaving your body to have a look into the envelope, or maybe imagine being able to look with X-rays. In this hypnotic state, however deep, you have the capacity to concentrate very much on this task, and you have access to this deeply concealed human skill. You can get the feeling that you can do it, you may even get the conviction of the correctness of your impressions. Now, let the impressions come to you during the next five minutes. If you wish to place your hands on the envelope, please do so. After these five minutes you will have a very strong memory of this experience and you will be able to remember it in every detail until long after the hypnosis. Later you can pick up my voice again.

Subjects were then given as much time as they needed to record their impressions on the blank sheets on the target envelopes, either in words or in drawings. This period generally lasted about ten minutes. Subjects were then told to record their HDSI ratings in the lower right corner of the sheet, rip off the top sheet, and hand it in.

Observational Manipulation. Following the completion of the ESP test, subjects were instructed to remove the target pictures from their envelopes. In half of the sessions, which ones determined by the same quasi-random process used to assign the selectors to sessions, the subjects were immediately rehypnotized by I.V. They were then asked to open their eyes and, while still hypnotized, to concentrate on any similarities they could find between the target picture and their impressions, using the carbon copies of their response sheets as reminders of the latter. In the other sessions, subjects were told to concentrate on such similarities prior to the rehypnotization. The rehypnotization in these sessions was without reference to the targets, and I.V. spoke continuously to keep subjects' attention riveted to him. Subjects kept their eyes closed throughout.

The procedure for the rehypnotization, except for those aspects directly related to the manipulation, was the same in the two conditions and lasted between five and ten minutes. In both conditions, subjects concentrated on the picture for about two minutes.

Following deduction, the subjects were asked to record their HDS2 ratings in the lower right corner of the envelope before handing it in. They were allowed to retain the carbon copies if they wished.

Judging. The selectors periodically deposited their duplicate target lists with Dr. Martin Johnson (M.J.), who, after all sessions had been completed, separated the sheets, removed the column of target designations from the carbon copies, and handed these carbon copies to a recorder. The recorder then prepared the rating sheets, each of which had written in the upper left corner the session and subject number of one of the subjects. The recorder's task was to write in the upper right corner of each rating sheet the number of the set of photographs from which the target picture for that subject was drawn, using the carbon copies as a guide. The rating sheets had been prepared in triplicate, one set going to each of the two judges. The remaining set was given to M.J., who checked the rating sheets against the originals of the target lists to be certain the recorder had made no mistakes.

I.V. solicited two acquaintances to serve as independent judges. One was a social psychology graduate student interested in dream symbolism and the other was a creative arts therapist, so both were somewhat conversant with creative human imagery of the type reflected in the ESP test.

Each judge was mailed a package consisting of the following materials:

1. A complete set of copies of the subjects' response sheets, with the HDS ratings removed.
2. A corresponding set of rating sheets as described above.
3. Envelopes containing reproductions of the pictures for approximately half of the picture sets.
4. A form for recording the ratings in duplicate. This form was to be retained by the judge in case the completed rating sheets became lost in the mail.
5. A return envelope for mailing back the response sheets.

6. A set of judging instructions, including "Tips for Judging", which were very similar to the instructions presented in the appendix of Palmer, Khamashta, and Israelson (1979).

The materials were divided up and placed into 15 manila envelopes, such that each envelope contained the rating sheets and response sheets of subjects whose targets were drawn from one of the 15 sets of pictures. Thus one judge rated first all the response sheets associated with Set 1, then all those associated with Set 2, etc. The other judge completed the task in reverse order.

Specifically, the judge was requested to place the pictures from Set X in front of him in order and then to survey all the response sheets assigned to that set, in order, looking for similarities as described in the instructions. The judge was then asked to go through the response sheets a second time, this time rating each picture in the set from 0 to 20, according to the closeness of its correspondence to the subject's response. Thus each response sheet contained four ratings, one for each picture in the set.

Since each judge received only half of the picture sets, they had to exchange sets midway through the judging. We instructed them, however, not to discuss their ratings with each other until all ratings had been completed and the materials mailed back.

Sequence of Events

When subjects contacted us initially, we mailed them a copy of the TAS. Later we recontacted them, usually by phone, and a convenient time was arranged for their participation in one of the group sessions. Thus assignment to the sessions was not strictly random, but it was not intentionally biased on any factor that would be expected to covary with variables of interest in the research.

Each session began with brief welcoming and orienting remarks by J.P. and I.V. This was followed by taped presentation of the CIS. After this, I.V. answered questions about the CIS, presented a brief standardized talk about the nature of hypnosis and its relation to parapsychology, answering any questions subjects had about these topics. This talk, which generally lasted about half an hour, was followed by the hypnotic induction and ESP test as described above.

After the completion of the ESP test per se but before the rehypnotization, subjects filled out the attitude scale. The rehypnotization was followed by a short break and refreshments, after which J.P. briefly discussed the future aspects of the project. J.P. and I.V. then answered questions about the hypnosis and ESP test as time allowed. The sessions lasted about two hours and 40 minutes.

All scales, instructions, and didactic material relevant to the tests were presented in Dutch.

RESULTS

Predictor Variables

Between-Session Effects. Each of the 12 sessions was in some respects an experiment unto itself, and some of the groups tended to take on "personalities" of their own. Thus we first conducted one-way ANOVAs to determine if there were any between-session differences among the means on our various predictor scales. The only significant effects we found were linear trends ("decline effects") for the HDS, both for HDS1 ($F(1,134) = 7.95$; $p < .01$) and HDS2 ($F(1,131) = 7.80$; $p < .01$). Although only the latter trend was supported by a significant overall between-groups effect, we decided it would be prudent to adjust all the HDS scores for these apparent declines. We accomplished this by computing regression equations for each HDS measure as a function of sessions, and then adding or subtracting from each score the difference between the grand mean and the mean predicted by the regression equation for that session. The adjustments proved to be quite modest in effect, and the two revised scales both correlated $>.95$ with the original versions.

There also were significant differences among the 12 sessions regarding the proportion of subjects of each sex ($\chi^2(11) = 25.03$; $p < .01$). There was no intention to assign subjects to sessions on the basis of sex, and we do not know why this effect occurred.

Hypnotic Imagination. The means and standard deviations of our measures of hypnotic imagination are presented in Table 1. Although we have no normative data for the 37-item version of the TAS, our data

conform well to data we have seen from the earlier version on American samples (e.g., Monteiro et al., 1980), and its distribution was normal. Compared to normative data reported by Wilson and Barber (1978), our CIS yielded a dearth of scores at the upper end of the scale, resulting in a compressed standard deviation and a modest but significant negative skew ($z = 2.32$; $p < .05$). HDS1 was normally distributed, but HDS2 had a significant positive skew ($z = 1.98$; $p < .05$).

TABLE 1
Means and Standard Deviations of Hypnotic Imagination Scales

	N*	M	SD	Range**
TAS	146	23.36	7.23	0-37
CIS	148	19.97	7.23	0-40
HDS1	146	4.96	2.16	0-10
HDS2	143	3.26	2.15	0-10

Note: *) Sample sizes are sometimes less than 150 due to missing data.
**) Refers to possible range of scores.

In confirmation of Prediction 1, the intercorrelations among TAS, CIS, and HDS were all significantly positive. The correlation between HDS1 and HDS2 was, not surprisingly, larger than the others, and may serve as a rough estimate of the reliability of the HDS viewed globally. These correlations are presented in Table 2.

As planned, we computed a "Hypnotic Index" (HI) by converting the raw scores on TAS, CIS and HDS1 to standard scores and taking the average. We did not include HD2 because of the different procedures of

TABLE 2
Intercorrelations among Predictor Variables*

	TAS	CIS	HDS1	HDS2	SG1	SG2	SG3	SG4
TAS		.46**	.39	.30	.19	.35	.36	.17
CIS	.47		.46	.33	.24	.30	.32	.28
HDS1	.40	.47		.72	.16	.24	.26	.26
HDS2	.31	.33	.71		.16	.17	.19	.19
SG1	.18	.26	.16	.18		.33	.09	.17
SG2	.37	.32	.24	.18	.31		.35	.25
SG3	.37	.31	.24	.19	.08	.38		.27
SG4	.17	.23	.24	.18	.15	.26	.24	

Note: *) Pearson rs on right of diagonal, Spearman rs on left. All are positive.

**) p values: .05: $r \geq .17$; .01: $r \geq .23$; .001: $r \geq .28$

rehypnotization in the different sessions. HI, which gives equal weight to all three measures, was intended to be our specific operational definition of hypnotic imagination for this experiment. HI was normally distributed with a mean of zero and a standard deviation of one.

Attitude Scale. A factor analysis supported the structure of the attitude scale as described in Methods, except that the two items defining SG1 did not correlate with each other ($r = +.07$) or form a factor. This most likely was because one item referred to the present (free-response) test whereas the other item referred to psi tests in general, using card tests (restricted-choice) as the example. We thus decided to use only the former item as the measure of SG1.

The item measuring SG4 revealed very little variability, most

subjects preferring to be noncommittal about predicting how well they had done on the ESP test. Obviously, any confidence in psi ability induced by the hypnosis did not carry over into the post-hypnotic period, although one would not necessarily expect it to.

The means and standard deviations of these subscales are presented in Table 3. Their correlations with each other and the hypnotic imagination scales are presented in Table 2. Correlations tended to be significant, positive, and slight to moderate in magnitude.

TABLE 3
Means and Standard Deviations of Attitude Scale Components*

	N	M	SD	Range
SG1	149	3.19	0.75	1-5
SG2	150	15.86	2.75	4-20
SG3	133	8.06	1.35	2-10
SG4	140	2.82	0.69	1-5

Note: *) See notes to Table 1 above.

Observational Manipulation. The protocol for the experimental condition was not adequately carried out in one group, so we decided prior to analysis to remove this group from any analyses involving the observational manipulation. T-tests were performed to determine if the remaining subjects in the two conditions differed on any of the predictors. We expected that HDS2 might be relatively lower in the experimental condition, because subjects had to open their eyes during the rehypnotization. There indeed was a trend in this direction, but it did not reach significance ($t(129) = 1.58$), and similar trends were

noted with the other hypnosis-related variables. The difference for one of these, the CIS, actually achieved significance ($t(134) = 1.99$; $p = .05$), but this strikes us as not particularly noteworthy given the large number of analyses conducted.

As expected, the identity of the target selector had no effect on any of these variables.

ESP Analyses

ESP Scores. The ratings sent back by one judge showed insufficient variability for scoring, as did those of another judge who was explicitly instructed to distribute his ratings more widely. Finally, a second judge was found who completed the task properly. Our final two judges, a clinical psychologist and a social psychology graduate student, both completed their tasks conscientiously.

Because for both judges the variability of ratings differed from set to set, ESP Z-scores for each judge were computed by subtracting the mean of the four ratings for each response sheet from the rating given to the target and dividing by the standard deviation of all the ratings given by the judge to pictures in that set. The Z-scores of the two judges were then averaged to obtain the subject's ESP score.

The Z-scores of the two judges correlated $+0.57$, which in our experience is rather good for this task. However, the distribution of these scores was leptokurtic with a moderately extreme positive skew. The mean was -0.08 , which did not differ significantly from chance (i.e., 0) by single-mean t-test.

Relations to other Variables. Because of the nonnormal distribution of the ESP scores, nonparametric tests were used whenever possible. The Spearman correlation between ESP scores and the Hypnotic Index was $+0.08$, which is nonsignificant and in the opposite direction from that predicted.

Among subjects whose ratings on HD2 exceeded the mean of 3.25, the mean ESP score of those who observed their target pictures under hypnosis was $+0.07$. The mean of the other high-HD2 subjects was -0.00 . Although in the predicted direction, the difference was not

significant by U-test.

Cutoff points were determined for each of the four attitude scales (SG1-SG4) so that scores could be dichotomized into "sheep" and "goat" categories. They were selected prior to analysis of the ESP data, the intent being to approximate as closely as possible the cutoffs used by Palmer (1973) while including enough subjects in the "goat" categories to allow analysis. Nonetheless, the number of goats tended to be small in most cases: SG1:18; SG2:16; SG3:46; SG4:34. None of the four "sheep-goat" differences were significant by U-tests, the "goats" actually scoring slightly higher than the "sheep" on the crucial Criterion 1.

DISCUSSION

The significant findings in this experiment all involved intercorrelations among the predictor variables. The construct validity of the TAS and CIS in relation to hypnotic susceptibility was supported directly by the moderately strong positive correlations of both scales with the ratings of hypnotic depth, and indirectly by their moderately strong correlation with each other. These results also provide cross-cultural generality of findings obtained primarily with American samples.

The various components of the Hypnotic Index also correlated positively with the four measures of belief in psi. As one would expect, the correlations were generally strongest for the two subscales (SG2 and SG3) that reflected beliefs not linked to the test situation. However, these differences are slight and may not be reliable. In any event, one can say that subjects whose scores reflected a strong capacity for "hypnotic imagination" reported a stronger degree of belief in the reality of various psychic phenomena and were more likely to claim having had such experiences themselves than were subjects low on this trait. They also were more confident about their own performance on the FR ESP test (SG4), but the fact that this correlation was lower for the TAS than for the scales completed at the test session suggests that this may be at least in part a halo effect: viz., "I succeeded in the first tests, so I will probably succeed on the ESP test as well."

As for the ESP findings (or, more precisely, the lack of findings),

not too much can be said. For those who consider the existence of psi anomalies to be at issue and who base their judgment of the likelihood of their existence at least in part on how frequently evidence for them appears in controlled investigations, this experiment clearly counts on the negative side of the ledger.

For those who think that sufficient evidence already exists for these anomalies or who consider their existence to be a valid working hypothesis, it is reasonable to ask what factors other than the non-existence of psi might have contributed to the failure to obtain evidence of psi in this experiment. One point of possible relevance is that most of the evidence for a relationship between ESP and altered states, including hypnosis, comes from experiments in which subjects were tested individually. The group studies, all of which according to my knowledge used meditation as the altered state, have produced relatively disappointing results (e.g., Osis & Bokert, 1971; Roll & Solfvin, 1976). It is reasonable to suppose that the potentially distracting nature of the group setting could have adversely affected the level of internal focussing necessary for optimal psi performance, even among highly susceptible subjects in hypnosis. This is a hypothesis for future research. In the present experiment, the distracting atmosphere was accentuated by ambient traffic noise, etc., which we could not entirely eliminate.

In anticipation of these disruptive factors, we neither predicted nor expected overall significant above-chance scoring. However, we did expect that subjects who scored high on hypnotic imagination would be better able to cope with these distractions, and partly for this reason would show significantly greater evidence of ESP on the test. This expectation was not confirmed.

Of course, other factors besides those noted above could have been responsible for the disappointing results, but further speculation would not seem appropriate at this juncture.

ACKNOWLEDGMENT

An earlier version of this paper was presented at the 25th Annual Convention of the Parapsychological Association and Centennial Conference of the Society for Psychical Research, held in Cambridge, England. Special thanks are due to our student assistants, Wim Kramer

and Annemarie van der Meer, who contributed greatly to the project by contacting subjects, helping in the conduct of the experimental sessions, and processing the data. Our thanks also to Brian Millar and Jerry Solfvin for serving as selectors, to Bernard Verlaan for serving as recorder, to Dr. Martin Johnson for checking the rating sheets, and to Arjan Eleveld and Ton Jansen for serving as judges.

ABSTRACT

In connection with sessions to screen potential subjects for future research, the relationship between "hypnotic imagination" (the capacity to experience and identify with cognitions in response to hypnotic suggestions) and free-response ESP performance was explored. The subjects were 150 volunteers, each of whom participated in one of twelve group sessions of eight to 15 subjects. During the sessions, subjects were hypnotized and asked to spend five minutes attempting to gain impressions of magazine pictures selected randomly for each individual. To test a prediction based on the Observational Theories, subjects in half of the sessions received feedback of their targets after having been rehypnotized, while the other subjects received feedback out of hypnosis.

Hypnotic imagination was operationally defined as a composite of three measures: the Tellegen Absorption Scale, the Creative Imagination Scale, and a self-rating of the maximum hypnotic depth achieved during the initial induction. It was predicted that this "Hypnotic Index" would correlate positively with ESP scores to a significant degree. Subjects also completed a nine-item questionnaire concerning belief in psi.

Significant positive correlations were found among the three measures of hypnotic imagination, as well as between these measures and each of four subscales derived from the belief questionnaire. None of the predictions involving ESP were confirmed, and there was no evidence of psi in the data.

REFERENCES

- Davidson, R.J., Schwartz, G.E. & Rothman, L.P. 'Attentional styles and the self-regulation of mode-specific attention: An electroencephalographic study', *Journal of Abnormal Psychology*, 1976, 85, 611-621.
- Honorton, C. 'A combination of techniques for the separation of high- and low-scoring ESP subjects: Experiments with hypnotic and waking-imagination instructions', *J. A.S.P.R.*, 1969, 63, 69-82.
- Honorton, C. 'Significant factors in hypnotically induced clairvoyant dreams', *J. A.S.P.R.*, 1972, 66, 86-102.
- Honorton, C. 'Psi and internal attention states', in Wolman, B. (ed) 'Handbook of parapsychology', New York, Van Nostrand Reinhold, 1977.
- Honorton, C. & Krippner, S. 'Hypnosis & ESP: A review of the experimental literature', *J. A.S.P.R.*, 1969, 63, 214-252.
- Honorton, C. & Stump, J.P. 'A preliminary study of hypnotically-induced clairvoyant dreams', *J. A.S.P.R.*, 1969, 63, 175-184.
- Irwin, H.J. 'The absorbing experience of being out of the body', *J.o.P.*, 1981, 45, 160-161. (abstract)
- Kiddoo, K. 'Personality, cognitive style, and imagination related behaviors', Paper presented at the meeting of the American Psychological Association, San Francisco, 1979.

McBain, W.N., Fox, W., Kimura, S., Nakanishi, M. & Tirado, J. 'Quasi-sensory communication: An investigation using semantic matching and accentuated affect', *Journal of Personality and Social Psychology*, 1970, 14, 281-291.

Millar, B. 'The observational theories: A primer', *E.J.P.*, 1978, 2, 304-332.

Monteiro, K.P., MacDonald, H. & Hilgard, E.H. 'Imagery, absorption, and hypnosis: A factorial study', *Journal of Mental Imagery*, 1980, 4, 63-81.

Osis, K. & Bokert, E. 'ESP and changed states of consciousness induced by meditation', *J. A.S.P.R.*, 1971, 65, 17-65.

Palmer, J. 'Scoring in ESP tests as a function of belief in ESP. Part I: The sheep-goat effect', *J. A.S.P.R.*, 1971, 65, 373-408.

Palmer, J. 'ESP scoring as predicted from four definitions of the sheep-goat variable', in Roll, W.G. et.al. (eds), 'Research in parapsychology 1972', Metuchen, Scarecrow Press, 1973, 37-39.

Palmer, J. 'Extrasensory perception: Research findings, in Krippner, S., (ed) 'Advances in parapsychological research', Vol. 2, New York, Plenum, 1979.

Palmer, J., Khamashta, K. & Israelson, K. 'An ESP ganzfeld experiment with Transcendental Meditators', *J. A.S.P.R.*, 1979, 73, 333-348.

Palmer, J. & Lieberman, R. 'ESP and out-of-body experiences: A further study', in Morris, J.D. et.al. (eds) 'Research in parapsychology 1975', Metuchen, Scarecrow Press, 1976, 102-106.

Parker, A. & Beloff, J. 'Hypnotically-induced clairvoyant dreams: A partial replication and attempted confirmation', J. A.S.P.R., 1970, 64, 432-442.

RAND Corporation. 'A million random digits with 100,000 normal deviates', Glencoe, Free Press, 1955.

Roll, W.G. & Solvvin, G.F. 'Meditation and ESP', in Morris, J.D. et.al. (eds) 'Research in parapsychology 1975', Metuchen, Scarecrow Press, 1976, 92-97.

Sargent, C.L. 'Hypnosis as a psi-conductive state: A controlled replication study', J.o.P., 1978, 42, 257-275.

Stanford, R.G. 'Suggestibility and success at augury: Divination from "chance" outcomes', J. A.S.P.R., 1972, 66, 42-62.

Tart, C.T. 'Self-report scales of hypnotic depth', International Journal of Clinical and Experimental Hypnosis, 1970, 18, 105-125.

Tellegen, A. & Atkinson, G. 'Openness to absorbing and self-altering experiences ("absorption"), a trait related to hypnotic susceptibility', Journal of Abnormal Psychology, 1974, 83, 268-277.

Wilson, S.C. & Barber, T.X. 'The Creative Imagination Scale as a measure of hypnotic responsiveness: Applications to experimental and clinical hypnosis', American Journal of Clinical Hypnosis, 1978, 20, 235-249.

APPLYING DAILY EXPERIENCES IN EXPERIMENTAL ESP RESEARCH

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It would appear that in many if not most spontaneous cases the paranormal experience as such does not differ from similar daily experiences. The experience has no independent characteristics of its own. For example, a dream with a paranormal character is not different from other dreams. According to Rhine (1978), conviction, i.e. the feeling that an experience is of a paranormal character, must be considered as a separate element which is not related to the experience itself. From this it follows that if the case is the first one in the life of the subject, a paranormal character often is attributed to the experience after the subject has learned of the corresponding external event.

Thus a meaningful correspondence is unexpectedly observed between a subjective experience and a certain external event. Such an observation is likely to create confusion in the person involved. The confusion is understandable because on the one hand the correspondence between the experience and the event more or less forces the person not to consider its occurrence as coincidental, while on the other hand the apparent relationship does not fit the familiar cause-effect model of reality. Besides this confusion as such, the reported experience in spontaneous cases most often refers to some dramatic event concerning another person, as we know from several previous analyses, e.g. Schouten (1979, 1981, 1982). Especially after being confused once by the

observation of a correspondence between such an experience and some outside event with a dramatic content, a subject might later give special attention to a daily experience which has something in common with the first one. The subject might be apt this second time to attribute a paranormal character to this attended experience before rather than after he learns of the external event. The similarity between the two experiences might be more or less specific: for example, a similarity in type of experience (e.g. dream) or a similarity in informative content (e.g. death). Thus instead of relating an attended external event to an experience, as in the first case, the subject might relate an attended experience to an external event.

Once having related an external event to the attended experience, whether or not it is objectively justified, a subject might gradually develop a tendency to attribute a paranormal character to an ever widening range of attended experiences before he learns of the external events, while the similarity between these later experiences and the first one becomes less and less specific. This generalization process is likely to take place when the subject is able to relate external events to the attended experiences in a subjectively meaningful way.

These attended experiences will be called 'striking experiences', because the subject presumably has learned to discriminate, on the base of some common characteristic, those experiences which have the highest probability of corresponding to certain external events. It is of interest to learn whether the relatively striking character of certain daily experiences of subjects who report many spontaneous experiences might be used as a predictor of the paranormal nature of such experiences.

Consequently, an experimental method was designed for evaluating the striking daily experiences of subjects who repeatedly report spontaneous cases and report at the same time that they often attribute a paranormal character to striking experiences before they have learned of the external events. The method involves the recording and assessment of striking daily experiences by means of what will be called the Striking Experience Test (SET), in order to compare them to certain previously determined external events, which serve as targets. It is also of interest to learn which types of experiences are selected by these subjects as sufficiently striking to attribute a paranormal character to them

before learning of the external event.

This type of research can be considered as lying between the two main lines of research in parapsychology: laboratory techniques like the Ganzfeld, e.g. Honorton (1977), and the analyses of spontaneous cases, e.g. Schouten (1979). In laboratory research, all experiences, whether corresponding to the external event or not, are represented in the evaluation. Thus only experiences in a particular set of circumstances defined by the experimental laboratory setting are involved. In the analyses of spontaneous cases, on the other hand, experiences in a wide range of circumstances defined by daily life are evaluated. But only experiences which in fact correspond to external events are represented in the evaluation.

The experimental method presented here attempts to study experiences in a wide range of circumstances defined by daily life, in such a way that all experiences are represented in the evaluation. The only restriction is on the event itself, which is chosen to be one of a set of 36 pictures. Such studies can enhance our insight in how potential paranormal experiences are embedded in the psychological functioning of subjects in real-life situations.

THE STRIKING EXPERIENCE TEST

SUBJECTS

The Parapsychology Laboratory is regularly approached by people who report frequent spontaneous paranormal experiences. Because of their confusion and the feeling of not knowing what to make of them, they often ask for some kind of objective evaluation. The SET was partly developed to serve as an attempt to answer that request. In a first interview, details about the paranormal experiences are collected. If these experiences appear to be of an either telepathic or clairvoyant character, the subject is invited to take part in the SET. If the subject consents, a second interview is arranged for discussing the details of the test.

We started asking subjects to take part in the SET in the

beginning of 1982. This paper includes the results of all subjects who participated during 1982. All 18 subjects who we invited, fourteen males and four females, participated. The age of these subjects ranged from 20 to 60 years, with a mean of about 35 years.

METHOD AND PROCEDURE

The SET is developed around a set of pictures, numbered 1 to 36, which were selected to cover different types of human experiences like perceptions, memories, imaginations and dreams, in such a way that for any experience at least one picture could be found with some relation to that experience. Using the set of pictures as the target set, we are able to generate artificially an external event by drawing one picture from this set. If a subject who knows the set of pictures in a similar way as he is aware of the pool of external events to which his recurrent spontaneous experiences might be related in normal life would get striking experiences which contained information about some external event, he might be able to relate the correct picture to these experiences. So before the start of the experiment, we presented the set of pictures to the subject. In order to establish a memory image of each picture, we first asked the subject to write down an objective description of each picture. Secondly, he had to write down his personal associations to each picture. These notations were kept by the subject during the experiment, and a copy was given to the experimenter. Thirdly, we asked the subject to rate each picture on the three components of the semantic differential: activity, potency and evaluation (Osgood, 1957).

The experimental procedure was as follows. The test was run during a period of 10 days, during which there was no contact between subject and experimenter. Each day the experimenter randomly, as defined by a computer program, took a picture from the set of pictures and concentrated for five minutes on this picture, while reading the notations the subject had made for that particular picture. The selection of targets during the ten days was with replacement. Thus a particular picture could serve more than once as a target during the ten-day period. The subject was asked to write down his most striking experiences each day in a diary, in such a way that an outsider would be able to understand

what had happened in the experience. At the end of the day he had to take his diary and to try to relate each noted experience with one or more of the pictures. He also had to record the numbers of the picture or pictures related to the experience. At the end of the ten-day period, he was to send the diary to the laboratory. After receiving the diary, the experimenter contacted the subject to evaluate the results.

No restrictions were placed on the kinds of striking experiences. All experiences were to be categorized later as dreams, imaginations, memories, perceptions, or 'picture experiences'. The category 'picture experiences' was made up of those experiences in which one or more of the pictures were autonomously and very prominently active in the mind of the subject. Under 'perceptions' were grouped those experiences in which the subject reported seeing, hearing or feeling something or somebody, in the normal meaning of the word of perception. Under 'memories' were grouped those experiences which referred to something that had actually happened in the life of the subject; for example, "While reading I thought about the scrap yard in the village where I was born and especially about the fat son of the owner". Under 'imaginations' were grouped the experiences which referred to something that did not actually happen in the life of the subject (fantasies and hallucinations); for example, "I thought about my mother having the queen's crown on her head", or "I swear I saw a cross behind him". The category 'dreams' does not need further explanation.

Some restriction was placed on the number of striking experiences. The subject was advised that the number of such experiences per day should be in the range of one to six. The subject was also told not to make any effort to have experiences with respect to the test; he should live as he normally would.

THE TARGET SET

The 36 pictures were selected to represent different aspects of reality in such a way that it would be possible to relate any type of experience to at least one of the pictures.

Besides being a specific picture, referring to one specific item

of reality, each picture was defined in terms of its conceptual and affective meaning. This means that the set of pictures was composed such that each of the pictures referred to the same conceptual or affective aspect of reality as did a specified number of other pictures of the set.

a. Conceptual meaning

The conceptual meaning of each picture was defined by the experimenter on three different levels of abstraction, called the high, medium and low levels. For example, the picture 'red colour' is equivalent on the low level of abstraction to the pictures 'yellow colour' and 'orange colour', all three referring to the concept 'warm colours'. On the medium level of abstraction, the picture 'red colour' is equivalent to the pictures yellow, orange, blue, green and purple colour, all six referring to the concept 'colours'. On the highest level of abstraction, the picture 'red colour' is equivalent to all five other colour pictures and all six pictures representing a geometric shape, all twelve referring to the concept 'abstract pictures'. In table 1 the target set is presented in terms of the conceptual meaning of the pictures.

b. Affective meaning

The affective meaning of each picture was defined by the subjects. As mentioned before, the subjects had rated each picture on each of the three components of affective meaning on a binary scale, as representing respectively activity or passivity, strongness or weakness, pleasantness or unpleasantness. Activity, potency and evaluation scores for each picture were computed by adding the number of subjects who rated the picture as 'active', 'strong' or 'pleasant'. In order to check the reliability of these scores and to check whether subjects taking part in the experiment might deviate in their rating of the pictures from subjects who do not say that they have frequent spontaneous paranormal experiences, a control group of subjects was employed to obtain a comparison set of ratings and scores. Scores for the two groups appeared to be significantly related, using Spearman's rank correlation coefficient. (Activity: $r = .56$; potency: $r = .53$; evaluation: $r = .77$; $df = 34$; $p < .001$ in all three cases). Hence we may conclude that the groups did not differ in their ratings of the affective meaning of the pictures and that the scores are reliable. We thus added the scores of the experimental and control

TABLE 1
The conceptual meaning of the pictures in the target set

Abstraction: low	Abstraction: medium	Abstraction: high
Warm colours	Colours	Abstract pictures
Cold colours		
Angular shapes	Geometric shapes	
Circular shapes		
Wild animals	Animal context	Concrete pictures
Domesticated animals		
Parts of human body	Human context	
Groups of human subjects		
Faces 'en face'	Human faces	Facial pictures
Faces 'en profile'		
Masks	Human-like faces	
Dolls		

subjects for each picture for each of the components of affective meaning. A picture was classified as neutral as regards a component of affective meaning when the total score did not deviate significantly from the score which was to be expected if the ratings had been assigned randomly. Using the exact binomial test and a one-tailed probability of $p < .05$ as a criterion, each picture was classified as 'significant' or 'neutral' with respect to each component of affective meaning. In table 2 the target set is presented in terms of the distribution of numbers of pictures representing the positive, neutral or negative value of each of the components of affective meaning.

Thus, if a significantly extreme number of subjects had rated a picture as passive, that picture would be classified as passive on the activity component. If about half of the subjects had rated the same picture as strong, the picture would be classified as

TABLE 2
The distribution of affective meaning of the target set

Activity	active	15	neutral	15	passive	6
Potency	strong	11	neutral	20	weak	5
Evaluation	pleasant	12	neutral	16	negative	8

neutral on the potency component. For example, the picture 'green colour' was classified as passive, neither strong nor weak, and pleasant. The picture 'red colour' was classified as active, strong, and pleasant. The definition of the pictures in terms of different levels of conceptual meaning and in terms of different components of affective meaning enabled us to evaluate the possibility that the experiences of the subjects contained information about the target on more general conceptual or affective levels as well as on the specific content level.

EVALUATION

We asked the subjects to keep a diary of different striking experiences each day. The subjects related one or more pictures to each of these experiences. This set will be called the response set. If the target picture for the day was among the response set the experience is called a 'direct hit experience'. Since the subject might relate the same picture to different experiences, each different striking experience can lead to a hit. Evaluations were based on empirical probabilities by simulating the results of the 18 subjects 10,000 times by means of a Monte-Carlo procedure. This was done by feeding the targets as observed, the number of experiences of each subject for each day, and the number of pictures which the subject related to each experience into a computer program which exactly replicated what the 18 subjects did, except for the fact that the choices of pictures were now

made on the basis of a random number generator (RNG). Thus probability distributions were generated for all the evaluations mentioned below.

1. Evaluation of direct hits

The hit experiences as defined above were counted and the one-tailed probability for this observed number was read from the probability distribution. This evaluation was done for each subject and for all subjects together. The following evaluations apply for all subjects together.

2. Evaluation of time-dispersion hits

The response sets for all experiences for all ten days of each subject were scored against the targets for all ten days for the subject.

3. Evaluation of category-hits

As shown in the description of the target set, each picture is defined in terms of three levels of conceptual meaning and in terms of three components of affective meaning. As far as the conceptual meaning is concerned, we scored for each level of abstraction the conceptual meaning of each picture of the response set against the conceptual meaning of the target picture. We did the same for each component of affective meaning; i.e. we scored for each component the affective meaning of each picture of the response set against the affective meaning of the target picture. For example, when the picture related to the experience was 'green colour' and the target was 'cows', the experience would not be a hit on any of the conceptual levels. On the affective level the picture 'green colour' was defined as passive, neither strong nor weak, and pleasant. The picture 'cows' was defined as passive, weak, and pleasant. The experience thus would be considered as a hit on the activity component, not as a hit on the potency component, and again as a hit on the evaluation component.

4. Evaluation of content of the experiences

In this evaluation, we studied whether perhaps only certain types of experiences score significantly, for example if dreams score significantly while 'picture experiences' do not. With the

Monte-Carlo procedure, we generated the probability distribution for the number of hits for each of the types of experience defined. In the same way, we studied if experiences with positive, negative or no concomitant feelings scored differentially. Likewise, we studied whether experiences in which a particular type of person was involved scored significantly.

RESULTS

Randomness of targets and pictures related to the experiences

Over 180 days, the expected number of times for each picture being selected as a target is 5. The targets appear to be equally distributed over the experiment. (Chi-square=41.2; df=35; n.s.). The targets also appeared to be randomly selected in terms of the levels of conceptual meaning (low level: chi-square=10.42, df=11; n.s.; medium level: chi-square=1.72; df=5; n.s.; high-level: chi-square=.59; df=2; n.s.). The same applies for the targets expressed in terms of affective meaning (activity: chi-square=.03; df=2; n.s.; potency: chi-square=2.74; df=2; n.s.; evaluation: chi-square=5.34; df=2; n.s.).

The total number of experiences is 524, an average of about 3 experiences per day. In total 864 pictures were related to these 524 experiences, an average of 1.65 picture per experience. The response bias for individual pictures was relatively low (chi-square=56.6; df=35; $p < .05$). The expected number of times for each picture being selected is 24. From a binomial test applied to the frequencies of the selected pictures, it appeared that only four of the 36 pictures deviated significantly ($p < .05$, one tailed) from the expected value, three being selected too frequently and one being selected too infrequently. Apparently, the subjects did not consider the experiment as a guessing task.

No preferences were observed in terms of conceptual meaning (low level: chi-square=14.83; df=11; n.s.; medium level: chi-square=3.13; df=5; n.s.; high level: chi-square=.40; df=2; n.s.) or affective meaning (activity: chi-square=3.77; df=2; n.s.; potency: chi-square=4.65; df=2; n.s.; evaluation: chi-square=3.44; df=2; n.s.).

Evaluation for direct hits

The 18 subjects reported 524 experiences, of which 35 were direct hits. The mean number of pictures related to the direct hit experiences was 1.97. The mean number of pictures related to the other experiences was 1.63. By means of the Monte-Carlo procedure, which simulated the experiment 10,000 times, we found the one-tailed probability of finding 35 hit experiences or more to be .018. This marginally significant result is attributable, however, to the scoring of one subject, the first subject taking part in the SET. This subject reported 36 experiences and scored 9 hits, distributed over 5 days. Applying the Monte-Carlo procedure to the experiences of this subject, the result proved to be highly significant: $p=.0001$. Eliminating the data of this subject left only 26 hits in 488 experiences, which is nonsignificant: $p=.21$.

Evaluation of time-dispersion hits

Comparing the responses for each experience with all targets of the ten-day period for each subject, we found no significant time-dispersion effects. The distribution of hits, expressed in distance of time (number of days forwards and backwards) between target and experience, is presented in table 3.

TABLE 3
Distribution of time-dispersion hits

Distance in days forwards and backwards									
9	8	7	6	5	4	3	2	1	0

Observed number of hits									
6	11	10	19	24	24	34	25	45	35

One-tailed probabilities in the Monte-Carlo procedure									
ns	ns	ns	ns	ns	ns	ns	.014	ns	.018

Besides the marginally significant high number of direct hits, we found a marginally significant low number of hits for two days

forwards or backwards. As we made ten such distributions for this analysis, one such a spurious effect is to be expected.

Evaluation of category-hits

Comparing the responses for each experience with the target for that day either in terms of conceptual meaning or in terms of affective meaning did not yield any results of interest.

Evaluation for content of the experience

The three characteristics of the experiences studied were type of experience, concomitant feelings in the experience, and involvement of another person in the experience.

a. Type of experience

As explained before, experiences were classified as 'picture-experiences', perceptions, memories, imaginations and dreams. The distribution of hits for each type of experience is presented in table 4.

TABLE 4
Distribution of hits related to type of experience

Type of experience:				
Picture experiences	Perceptions	Memories	Imaginations	Dreams
100	201	92	91	40

Observed number of hits				
8	13	4	8	2

One-tailed probability in the Monte-Carlo procedure				
ns	.050	ns	.032	ns

From these results, it appears that imaginations and perceptions were responsible for the marginally significant overall result.

But when the high scoring subject is excluded, no differential scoring between different types of experiences is observed.

b. Concomitant feelings

The experiences were also classified on the basis of the type of concomitant feelings in the experience. Feelings were classified as positive, negative and no feelings. The 'picture experiences' were excluded in this analysis, as only 'no feelings' were associated with these experiences. The distribution of hits according to each type of feeling associated with the experience is presented in table 5.

TABLE 5
Distribution of hits related to type of feeling in experience

Type of feeling in the experience:		
Positive	Negative	No feeling
27	84	313
Observed number of hits		
0	7	20
One-tailed probability		
ns	ns	.029

From these results, it appears that experiences not associated with positive or negative feelings were responsible for the marginally significant overall result. When the high-scoring subject was excluded, however, no relationship was observed between scoring and type of feeling associated with the experience.

It must be noted that although experiences without feelings were reported most frequently, if feelings were associated with the experience they were most often of a negative nature.

Involvement of another person

Experiences were classified on the basis of the type of person involved in the experience. These persons were categorized as relatives, acquaintances, well-known personalities, strangers, or as no others involved. In this analysis the 'picture experiences' were again excluded, as only 'no others' were involved in these experiences. The distribution of hits for each category is presented in table 6.

TABLE 6
Distribution of hits related to the involvement of another person

Person involved in the experience:				
relative	acquaintance	known personality	stranger	no person

20	100	28	64	212

Observed number of hits				
0	4	2	3	18

One-tailed probability in the Monte-Carlo procedure				
ns	ns	ns	ns	.002

From these results, it appears that experiences involving no other persons were responsible for the marginally significant overall result. We found in this case that these experiences, relative to the others, scored marginally significantly, even when we exclude the high scoring subject ($p=.037$).

FURTHER ANALYSES

In this section some observations and post-hoc analyses are presented:

1. The subject who had scored significantly in this experiment was

asked to participate in the test for another ten-day period. This time only 2 hits in 32 experiences were obtained, a nonsignificant result.

2. Most subjects expressed before the experiment their confidence in the SET as an adequate instrument to evaluate their experiences in an objective way. After being informed about their nonsignificant numbers of hits, several subjects, not knowing that other analyses were also carried out, themselves suggested that we run time-dispersion and category-hit analyses. This suggests that these subjects were aware of such possibilities for different ways of analyzing the experiences. Perhaps they use the same strategy when evaluating their spontaneous daily life experiences. That is, they might be prone to look for events that match some of their experiences to which they attribute a special meaning, and they might try different ways of matching these experiences with events. This could result in several 'spontaneous paranormal experiences'.

3. In the course of the year, it became apparent that subjects tended to score a hit more often on the first day of the ten-day period. To evaluate this impression, the distribution of hits of all subjects over the ten-day period was compared with the number of experiences for each day. The results are presented in table 7. Of the 35 hits, 11 were obtained on the first and 7 on the last day of the ten-day period. The distribution resembles the familiar U-shape pattern of scoring. The numbers of hits for these two days differ significantly from their expected values. Even after eliminating the data of the high-scoring subject, still over half of the hits, 14 out of 26, were obtained on the first and last day.

4. An analysis of content characteristics of the experiences for the different subjects revealed strong individual differences. Strong individual preferences were found, for instance, with respect to the proportions of externally attended experiences (perceptions) versus internally attended experiences (memories, imaginations, dreams). ($\chi^2=182.10$; $df=17$; $p<.001$). We considered individual subjects as having a significant preference for a specific mode of experience, if their distribution for that mode differed significantly at the .01 level (one-tailed) from the distribution for all subjects. Eight subjects displayed a significant preference for a specific type of experience. Two of them appeared to have a preference for internally attended experiences, the others for externally attended experiences. A significant correlation was observed between proportion of

TABLE 7
Distribution of hits over the ten-day period

Days	1	2	3	4	5	6	7	8	9	10
Number of experiences of all subjects										
	63	67	56	51	55	49	46	46	46	45
Number of hits of all subjects										
	11	1	1	3	2	3	3	2	2	7
One-tailed probability in the Monte-Carlo procedure										
	.0001	ns	ns	ns	ns	ns	ns	ns	ns	.004
Number of experiences without high-scoring subject										
	57	62	50	46	50	46	43	45	45	44
Number of hits without high-scoring subject										
	7	1	1	2	1	2	1	2	2	7
One-tailed probability in the Monte-Carlo procedure										
	.01	ns	ns	ns	ns	ns	ns	ns	ns	.001

'picture experiences' and proportion of internally attended experiences ($r_s=.59$; $t=2.92$; $df=16$; $p=.01$). Thus it seems possible to characterize most subjects in terms of tendency to direct attention to either internal or external events.

Six persons deviated from the group norm with respect to feelings associated with the experiences. Four subjects reported a significantly high number of experiences of an emotional nature, whereas two subjects were characterized by reporting experiences without concomitant feelings.

Three subjects reported significantly more often than other subjects experiences in which other persons were involved, and

three subjects showed a preference of the opposite nature, i.e. in their experiences other persons figured less often.

When subjects were rank ordered on strength of preference for each of the characteristics, type of experience, concomitant feelings, and involvement of other persons in the experiences, the rank orders appeared to be unrelated. Thus it can be concluded that no relationship exists among the preferences for the various characteristics.

The high-scoring subject more often reported internally attended experiences in which no other persons were involved. His experiences did not deviate from the average of reported experiences as regards associated feelings.

DISCUSSION

It appears that the observed correspondences between experiences and external events do not deviate strongly from what could be expected by chance. Only the results of the first subject are noteworthy. In interpreting these results, we are in a more or less similar position to that of a person who feels confused by a spontaneous paranormal experience. One feels inclined to attribute meaning to the results of the first subject, but one feels unable to provide any satisfactory explanation. The same applies to the post-hoc findings presented above. What is the reason that the first subject scored so extremely and for all subjects the first day proved to be the optimal day of the ten-day period? The results of such post-hoc analyses seem comparable to what happens to a subject who had a striking experience and observes certain events which seem related in a meaningful way. They create confusion.

Nevertheless, some clear conclusions can be drawn from the analyses of this experiment. Firstly, it appears that the subjects felt the test to be appropriate. That they took their experiences seriously and used them as the basis for selecting pictures is also demonstrated by the observed low response-bias in the selection of pictures. In addition, no subjects attributed their failure to obtain significant results to the pictures used. Secondly, we can conclude that if the significant results obtained

are based on psi, the absence of time-dispersion effects is suggestive of an all-or-none model. The same suggestion follows from the absence of category-hit effects. Neither conceptual nor affective 'dispersion' between experience and external event is present. Thirdly, the results suggest that the experiences which resulted in hits had no distinctive characteristics. Both externally and internally attended experiences resulted in direct hits. The same applies for experiences with and without concomitant feelings, and for experiences with and without another person involved.

A final conclusion seems to be that it is possible to differentiate subjects as regards the characteristics of the experiences they report. Some subjects seem to be more oriented toward the external world and report mainly perceptions. With others experiences associated with feelings dominate, or experiences in which no other persons are involved. Data from more subjects will probably allow us to make further distinctions, for instance between subjects who predominantly report experiences with negative or positive feelings respectively.

The fact that subjects apparently have specific individual preferences for certain types of experiences of sufficiently striking character to be considered as of potential paranormal nature is suggestive that in ESP occurrences type of experience is individually determined. This is in agreement with findings of studies of spontaneous cases. Perhaps these preferences are influenced by the characteristics of the first spontaneous paranormal experience. This question only can be answered in subsequent research.

ABSTRACT

An experimental method called the 'Striking Experience Test' (SET) was devised for subjects who report frequent spontaneous paranormal experiences. The SET can be considered as an experimental method lying between the analysis of spontaneous cases and experimental techniques such as the Ganzfeld. The 18 subjects who participated in the experiment during 1982 ranged in age from 20 to 60 years. Subjects were required to record during a ten-day period their most striking daily experiences. Striking

experiences were those experiences to which the subject assigned a high probability of correspondence to certain external events on the basis of his recurrent spontaneous experiences. The experiences for each day were compared with a target picture randomly selected by the experimenter from a set of 36 targets. To achieve this, the subject compared each experience with the target set and selected one or more pictures which represented the information in the experience. The pictures were also defined in terms of conceptual and affective meaning in order to evaluate the possibility of correspondences between targets and experiences on more general levels.

The experiment yielded 524 experiences. A total number of 35 hit experiences was found for all subjects. This result proved to be marginally significant ($p < .02$) when the experiment was simulated by means of a Monte-Carlo procedure. However, only the first subject scored a significant ($p = .0001$) number of hits. Eliminating the results of the high-scoring subject, the overall result of the experiment was not significant. Neither time-dispersion effects, that is correspondences between experiences and target pictures for different days, nor category-scoring effects, that is correspondences between experiences and targets in terms of conceptual and affective meaning of the pictures, were observed. Finally, no differential scoring was observed between the types of experiences classified as dreams, imaginations, memories, and perceptions. It is concluded that if the marginally significant results observed can be attributed to psi, they are suggestive of an all-or-none process. Another conclusion is that no differential scoring is observed between the types of experiences classified as dreams, imaginations, memories, and perceptions.

ACKNOWLEDGMENT

The authors gratefully acknowledge their indebtedness to dr J. Palmer and dr Sybo A. Schouten for their critical appraisal of the article.

REFERENCES

Honorton, C. 'Psi and internal attention states', in B. Wolman (Ed.) 'The handbook of parapsychology', New York, van Nostrand-Reinhold, 1977, pp 435-472.

Osgood, C.E., Suci, G.J. and Tannenbaum, P.H. 'The measurement of meaning', University of Illinois Press, Urbana, 1957.

Rhine, L.E. 'The psi process in spontaneous cases', J.o.P., 42, 1978.

Schouten, S.A. 'Analysis of spontaneous cases as reported in 'Phantasms of the living'', E.J.P., 1979, 2, 4, 408-454.

Schouten, S.A. 'Analysing spontaneous cases: A replication based on the Sannwald collection', E.J.P., 1981, 4, 1, 9-49.

Schouten, S.A. 'Analysing spontaneous cases: A replication based on the Rhine collection', E.J.P., 1982, 4, 2, 113-158.

HYPNOSIS AND PSI
TOWARDS AN INTERACTIONAL EXPLANATION

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An association has existed for a long time between psi and hypnosis. Even before Barrett (1918) described his experiments with hypnosis and what would now be called GESP, Janet and Richet were conducting work on hypnosis at a distance Thouless, 1972). In two excellent reviews published nine years apart, both Van de Castle (1969) and Palmer (1978) came to the conclusion that there existed a definite relationship between hypnosis and ESP.

There have been various attempts to explain this relationship. These ranged from increased confidence of success following hypnotic suggestion (Palmer, 1978) to the hypothesis that hypnosis produces a quiet state of mind in which ordinary distractions are eliminated so that ESP impressions can be more readily perceived (Tart, 1977).

The latest of these attempts at explanation is one by Wilson and Barber (in press). They found that subjects who score high on scales of hypnotic susceptibility tend to fantasize more than those who score low, and that those 'fantasy-prone' people have psi experiences more often than other people. The conclusion is that it is not hypnosis per se that gives rise to psi experiences, but rather that both hypnosis and psi experiences tend to be found in the kind of person who has a history of profound involvement in fantasy.

These explanations, following the norm set by both parapsychological and hypnosis research, focus on the subject and virtually exclude the influence of the hypnotist(s) or experimenter(s). There is growing evidence, however, that a less limiting view of hypnosis is one that sees it as a particular type of relationship that exists between hypnotist and subject rather than as some condition of the subject (Fourie, 1980; Fourie and Lifschnitz, 1982; Haley, 1963, 1973). Without going into the technical details - it is done elsewhere (Lifschnitz and Fourie, in preparation) - the following axioms explain what is called the interactional model of hypnosis.

1. Hypnotic behavior is involuntary behaviour. Hypnotic behaviour is distinguished from other behaviour by the fact that the very way in which the behaviour is exhibited as well as the subject's verbalizations about the behaviour (both during hypnosis and thereafter) qualify it as involuntary. When hand-levitation takes place, for instance, it is done in such a way that the subject indicates that the hand is not lifted, but that it lifts by itself. In general, hypnosis researchers concur that hypnotic behaviour is involuntary and have done so since the time of Bernheim (Weitzenhoffer, 1980).

2. In hypnosis both hypnotist and subject communicate paradoxically. When a subject behaves but qualifies the behaviour as not of his/her own volition, he/she is communicating paradoxically. In hand-levitation the message from the subject can be read as: 'I am lifting my hand, but I am not lifting it'. By the same token the hypnotist communicates paradoxically as well. He/she suggests to the subject to perform certain acts, but not to perform them deliberately, such as to keep the eyes closed, but to try to open them, or to keep the hand still and to observe it lifting. A relationship in which both parties communicate in this way is called a paradoxical relationship (Haley, 1963).

3. The hypnotist is the initiator in the relationship. He/she actively takes the lead and tells the subject what to do. The hypnotist is also the one who starts communicating paradoxically. A relationship in which one person leads and the other follows is called a complementary relationship (Haley, 1963). Hypnosis

therefore can be seen as a paradoxical relationship encompassed within a larger complementary relationship. The existence of a complementary relationship is a prerequisite for the establishment of a paradoxical relationship, otherwise the subject might comment on the hypnotist's paradoxical statements or simply get up and leave. Hypnosis cannot occur if the hypnotist is not in the leading position. Even when it seems that the subject is taking the initiative, he/she does so with the permission of the hypnotist, who then retains the lead.

4. The process of establishing first a complementary and then a paradoxical relationship is one of negotiation. The hypnotist takes into account the subject's idiosyncratic style of communication, his/her needs and likes, in negotiating for the establishment of a particular type of relationship. For instance, if the subject does not like to follow the hypnotist's lead, he/she will indicate that in subtle verbal and non-verbal ways. The hypnotist will then probably indicate that he/she wants (or allows) the subject to take the lead. If the subject does take the lead then, he/she is conceding to the hypnotist's request, so that in effect the hypnotist retains the initiative. The negotiation process also takes place when the hypnotist tries to define a paradoxical relationship. The subject might follow either of the two conflicting suggestions in an involuntary way, while the hypnotist might wish the one rather than the other. The task of the hypnotist is then to negotiate, in a paradoxical way, for compliance with the instruction in mind. Much of this negotiation, at least from the side of the subject, takes place on an unconscious level, and consists of subtle interpersonal manoeuvring by both parties.

5. When the paradoxical relationship is established, its scope may encompass any number of hypnotic phenomena. If only a few such phenomena can occur within the bounds of a particular paradoxical relationship, the relationship can be called a narrow one; if a large number of phenomena is possible in the relationship, the relationship is wide. It has been suggested elsewhere (Fourie, in press; Fourie and Lifschnitz, 1982) that width of the paradoxical relationship is an apt conceptualization to explain that which is possible within the bounds of a particular hypnotic relationship. Once a paradoxical relationship exists, that is, once the subject

starts behaving involuntarily, the relationship is widened by a continued process of negotiation. For instance, the hypnotist might find that, if the subject is requested to open the eyes while staying in hypnosis, the subject might feel uncomfortable and might attempt to come out of the hypnosis. It might then be feasible to let the subject, in hypnosis, read from a book or write something, so that the mind is occupied when the eyes are open. It is conceivable that, if the mind is kept occupied, the subject might accept the idea of having the eyes open while in hypnosis. This is an example of the type of negotiation that can take place in order to widen the relationship, in this case to include, within the bounds of the relationship, the possibility that the subject can stay hypnotized with eyes open.

This interactional view of hypnosis has two clear implications for a study of parapsychology. The first is that it affords a different perspective on the established ESP-hypnosis relationship. The second is that it sheds a new light on psi research as a whole. It is the aim of this paper to bring these implications to the fore.

PSI BEHAVIOUR AS INVOLUNTARY BEHAVIOUR

In a previous publication (Fourie, 1980; 1982) I have shown that psi behaviour is involuntary behaviour. People, be they sensitives or not, are not able to produce psi phenomena on order. Even if they regularly show psi behaviour and are able to create favourable conditions so that they can get themselves into the correct frame of mind for psi to occur, the actual occurrence if psi is outside of their control. This characteristic of psi has led me to the widely accepted conclusion that the psi process is an unconscious one, that is one that is outside of the person's conscious control. In a sense subjects in psi experiments can but observe what happens just as a hypnotized subject can but observe a hand lifting. Psi behaviour therefore is involuntary behaviour.

Involuntary behaviour, which Haley (1963) calls symptomatic behaviour - a term which, because of its negative connotations will not be used here - manifests in a paradoxical relationship of some sort. As was seen, involuntary behaviour (which, as used

here, excludes reflexive behaviour) is by nature a paradoxical communication. It indicates that the person is behaving but denying that it is he/she that is behaving. Such a communication defines the relationship in which it occurs as a paradoxical one, that is, one in which paradoxical communication occurs. All paradoxical relationships are not called hypnosis, however. They occur *inter alia* in some religious rituals, in faith and spiritual healing and in families with disturbed members (Watzlawick, Weakland and Fish, 1974) - from the latter Haley's (1963) term.

HYPNOSIS AND PSI

Although there are vast differences in the form or content of different types of involuntary behaviour, from the point of view of their paradoxical nature they are of the same type. From this perspective there is no theoretical difference between psi behaviour and, say, hypnotic hand-levitation or hypnotic amnesia. Therefore it seems logical that psi can occur within the bounds of the paradoxical (hypnotic) relationship in the same way as any other involuntary behaviour. That does not mean that psi behaviour is always hypnotic, although that is the case when it occurs within hypnosis. What it does mean, is that both hypnotic behaviour and psi behaviour are of a type called paradoxical behaviour.

Just like hypnotic behaviour, psi behaviour is elicited within a paradoxical relationship by means of a process of negotiation. One of the tenets of this paper is that the negotiation is evident in most (if not all) psi-enhancement techniques recorded in the literature.

The word negotiation implies a process that differs from situation to situation, taking into account contextual variables, the interactional styles of both hypnotist and subject, hypnotist and subject's idiosyncratic needs, wants, likes and dislikes, and the particular ways in which both parties negotiate. There can be very little about this process that can be called standard, except for the basic aim of creating a paradoxical relationship. Unfortunately hypnotic induction is still thought by many to be a question of running through the right 'pattern' with very little regard to situation-specific variables.

If such a 'standard' view of hypnosis still exists in the hypnosis fraternity, it is small wonder that parapsychologists generally use hypnosis in a similarly limiting way in their experiments. There have been the exceptions, though. One of these was Milan Ryzl. Of his work Tart (1977, p.109) says:

"This is probably the most sophisticated research that has been done to date on hypnosis and psi. A common fallacy of many other studies was to assuming that because a person was in a hypnotic d-ASC (discrete altered state of consciousness) and was told he had psi faculties, he automatically had them. Ryzl assumed, more realistically, that although the hypnotic state might be favourable to manifesting psi, it was still necessary to develop that potential within the hypnotic state."

If one reads Ryzl's (1971, 1978) own description of his work, it is easy to see that the way in which he helped his subjects to develop their psi potential, constituted a very sophisticated negotiation process.

To date Ryzl's hypnotic training method could not be successfully replicated. The replication studies, in the very act of trying to follow Ryzl's outline, made the outline a standard procedure, not realizing that it was not the procedure itself that enhanced psi performance, but the negotiation process that underlied the procedure. By trying to follow Ryzl's procedure too closely, no room was left for the negotiation process.

The most well-known of these replication studies was the one carried out by Beloff and Mandleberg (1966). The following extract from their report shows how 'standard' (non-negotiable) their procedure was:

"Each subject came once a week for 10 weeks for a one-hour session. When the subject was comfortably settled in his easy chair our first job was to get him securely hypnotized. Standard methods of induction were used ..." (Beloff and Mandleberg, 1966, p.232).

In this approach, other than in that of Ryzl (1971) there seems to have been little that was negotiable. Ryzl certainly did not confine the training to one hour per week for ten weeks. He trained his subjects until he (and they) were satisfied with the results.

My own attempt (Fourie, 1977) to replicate Ryzl's work failed as well, probably for the same reason, although it was less standard

than that of Beloff and Mandleberg in that the number and length of training sessions were not fixed. Also I failed to establish the complementary relationship firmly enough.

The thesis of this paper is that psi behaviour is involuntary behaviour, which like all such behaviour occurs within a paradoxical relationship. Seeing that hypnosis is a paradoxical relationship, it follows that psi should, at least theoretically, be possible to elicit in hypnosis. And what is true of hypnosis probably is true of other altered states of consciousness as well. For instance, in administering a drug to a subject, the experimenter defines their relationship as complementary, paving the way for paradoxical interaction.

THE PSI-CONDUCTIVE SYNDROME

If psi manifests within a particular type of relationship, it should be possible to distinguish between those situations (relationships) in which psi seems to occur more frequently and those in which psi does not occur as frequently. This is precisely what William Braud (1975) did. He came to the conclusion that the literature suggests:

"... the existence of two 'states' or 'syndromes'. One of these states appear to facilitate manifestations of the psi process ... The other state seems to interfere in some way with the psi process (Braud, 1975, p.142). He distinguished seven major characteristics of what he called the psi-conductive syndrom. They were:

- (a) The subject is physically relaxed.
- (b) Physical arousal or activation is reduced.
- (c) Sensory input and processing is reduced.
- (d) There is a focus on internal rather than external processes.
- (e) Right hemispheric functioning is increased.
- (f) There is an altered view of the nature of the world.
 - The unity of 'things' are emphasized
 - Time is illusory
 - Sensory experience is relatively unimportant
 - 'Good' and 'evil' are illusory
- (g) Psi is momentarily important.

Although Braud himself again emphasized the role of the subject - the characteristics of the psi-conductive syndrome are all stated in terms of the experience of the subject - it is not difficult to see these characteristics as pointing toward the existence of a paradoxical relationship. It does require a shift in viewpoint, however. For instance, physical relaxation in relationship terms means much more than just that the subject relaxes his muscles. It means that the experimenter tells the subject that he should sit in a particular chair and that he should relax his muscles in a particular way. Braud had his subjects listen to and follow tape-recorded instructions for progressive relaxation. This type of experimenter-subject interaction defines their relationship as complementary with the experimenter in the superior position. Moreover, progressive relaxation, as first employed by Jacobson (1938), involves alternatively tensing and relaxing muscle groups. From an interactional point of view this embodies a particular paradox, namely that in order to relax, muscles must be tensed. After Braud's subjects were relaxed in this way, they "... attempted to gain psi impressions of coloured art reproductions ..." (Braud, 1975, p.143). It is not reported how they were instructed to make such attempts, but it is probable that the instructions involved two elements, namely

- (a) a request to obtain information on the reproductions, and
- (b) an embargo on the use of sensory means.

This amounts to a 'do it, but don't do it' paradox technically identical to the 'lift your hand, but don't lift it' paradox employed in hypnotic hand-levitation. In both instances the option of involuntary behaviour is left wide open to the subject: in hypnosis to lift the hand while denying responsibility for it, and in Braud's study (as in most other psi experiments) to obtain information while similarly denying responsibility for such action.

This argument can be seen to apply to most of the other characteristics of the psi-conductive syndrome mentioned by Braud. For instance, when arousal is reduced by means of biofeedback, or when sensory input is restricted by means of a ganzfeld procedure, the complementary relationship is firmly established, especially since the apparatus and setting enhance the status of the experimenter. In the instructions to obtain information by ESP following these procedures, the 'do it, but don't do it' paradox probably features again.

The last of the characteristics of the psi-conductive syndrome mentioned by Braud, namely that psi should be momentarily important, is of particular interest from an interactional point of view. By making the information to be obtained important to the subject, the 'do it, but don't do it' paradox is strengthened. It then becomes a 'do this important thing, but don't do it' paradox. This is a good illustration of how the experimenter can strengthen the base of his/her negotiation for the inclusion of a particular involuntary act into the scope of the paradoxical relationship. Of even more interest is the obvious connection between this characteristic of the psi-conductive syndrome and Stanford's (1975) PMIR model. Investigation of this model has shown that subjects can use psi to obtain important information without knowing that they do, and without knowing that they are supposed to do so (Tart, 1977). This raises the possibility that the 'do this important thing, but don't do it' paradox can be picked up by means of psi as well as by more explicit means. This is of course a speculation and it is an open question whether subjects partaking in an experiment run by known parapsychologists could be ignorant of the possibility that an 'innocent' word-association test could involve psi experimentation.

From the foregoing it is apparent that, from experimental reports emphasizing the subject rather than the experimenter-subject relationship, it is difficult to come by the particular paradoxes present in the situation as well as by the particular ways in which the experimenter(s) negotiate with the subject(s) for the inclusion, into the scope of the paradoxical relationship, of the particular type of involuntary behaviour desired. While the particular paradoxes and ways of negotiating thus remain speculative, it nevertheless seems though at least some characteristics of what can be called a psi-conductive syndrome can be seen as pointing toward a paradoxical relationship as the basis of psi behaviour.

This must not be taken to mean, however, that once a paradoxical relationship exists, psi automatically emerges. In the same vein it does not mean that hypnotic amnesia, for instance, necessarily is present whenever the paradoxical relationship called hypnosis exists. The emergence of psi behaviour, like that of amnesia, is something that has to come about through negotiation and depends inter alia on the subject's willingness to consider the possibility of exhibiting such behaviour - see Schmeidler's

sheep-goat effect (Schmeidler and McConnell, 1958).

Successful ways of negotiating, in a paradoxical way, for the emergence of psi are noted throughout the literature, although most often their negotiating nature is not realized when the experimenter-subject relationship is de-emphasized.

THE EXPERIMENTER EFFECT

Another aspect of psi research that needs consideration here is the so-called experimenter effect. This effect is most clearly demonstrated when two experimenters carry out identical experiments and one of them obtains significant results, while the other does not. Critics of parapsychology have used this effect and the resulting irreproducibility of many studies of psi as 'proof' that significant psi results are due to experimental artefacts - see, for instance Alcock's (1981, p.124) remark: "Rather than giving careful consideration to the possibility that those who obtain positive results do so because of flaws in the carrying out of the experiments, parapsychologists have come to view the experimenter effect as a manifestation of the psi-ability of the experimenter and thus, in a sense, vice becomes virtue".

Such critics, and unfortunately many parapsychologists as well, do not seem to realize that the experimenter effect is probably one of the most important discoveries made to date in parapsychology specifically and psychology in general. It is the one discovery that can lead parapsychological research out of the forest of confusion (where every tree was apt to be seen as some revolutionary new psi-enhancing technique) into the open, where the relationship aspects of the wood can be seen in perspective. Hopefully, the experimenter effect will help parapsychologists and their critics alike to realize that psi phenomena are relationship phenomena rather than manifestations of mystical 'powers'. For, if laboratory psi comes about as a result of negotiation within a paradoxical relationship it is to be expected that no two experimenter-subject relationships can ever be identical. Neither can identical negotiations take place. In fact, by trying to keep psi-enhancing techniques identical in two experiments, the negotiation aspect is denied completely, with negative findings as the result.

While investigation of the experimenter effect is a step in the direction of considering the broader context in which psi manifests, it is still not a study of the characteristics of the experimenter-subject relationship, a relationship that Swann (1979, p.225) describes as "... of extraordinary, if not basic, importance ...". The theoretical shift that this paper proposes, therefore, is from emphasis on the characteristics, intrapsychic state of behaviour of any one of the interacting parties to emphasis on the relationship between the parties: System C, as Riggs (1978) calls it. What follows is an attempt to look at the data on the experimenter effect from a relationship point of view.

There have been two main avenues of approach in research on the experimenter effect, namely efforts to identify experimenter characteristics that differentiate between 'successful' and 'unsuccessful' experimenters (e.g. Parker, Millar and Beloff, 1977), and attempts to isolate instances where the experimenters themselves used psi to influence experimental outcomes (e.g. Broughton, 1977). This second line of investigation is interesting. It seems that it is indeed possible for experimenters unwittingly to use their own psi to obtain the results that they wish to obtain (Kennedy and Taddonio, 1976). This is not surprising since it must be remembered that both subject and experimenter find themselves in a paradoxical relationship with each other, a relationship which could lead to psi behaviour in both of them. However, the experimenter can easily simultaneously be found in another paradoxical relationship as well, namely with his superiors and colleagues in the organization in which the research is taking place. In this instance the 'do it, but don't do it' paradox can consist of

- (a) strong pressure (possibly tied to promotion or tenure or grants) to obtain positive results, and
- (b) an embargo on the use of sensorimotor channels to obtain the desired results - see the Levy scandal of some years ago (Rhine, 1974).

From an interactional point of view, then, psi behaviour by the experimenter makes sense.

The first line of inquiry mentioned above into the experimenter effect, namely that of differentiating between 'successful' and 'unsuccessful' experimenters on the basis of individual characteristics, is interesting, but again emphasizes only one of

the two parties in the experimenter-subject relationship. Most experiments that were done along this line involved the manipulation of the behaviour of the experimenter(s). For instance, Honorton, Ramsey and Cabibbo (1975) tested subjects in two groups on a PK task. One group was tested by an experimenter instructed to be friendly and supportive, while the other group was tested by an experimenter that was instructed to be unfriendly. Each of the two experimenters tested subjects in each group. As expected, the group tested in a friendly way exhibited significant psi-hitting, while the other group showed significant psi-missing.

This type of investigation comes closer to considering interactional variables, although it still concentrates mainly on one of the two parties, in this case the experimenter, and it does so in a very vague way. Friendliness, for instance, is hardly a clear description of the negotiation process. Negotiation involves taking into account all possible givens, from situational and contextual variables to subject-specific variables and variables specific to the particular experimenter-subject relationship. Thus, while 'friendliness' might be essential to get a specific subject to produce psi-hitting at a specific time, it might keep another subject or the same subject at some other time from doing that. Negotiation thus is relationship- and situation-specific. Research could probably be directed more profitably at negotiation characteristics rather than experimenter characteristics.

RSPK, HAUNTING, AND OTHER 'SPONTANEOUS' PHENOMENA

An interactional view of psi fits well with what is known about recurrent spontaneous psychokinesis (RSPK) or poltergeist activity.

One of the main findings of research in this area is that RSPK usually occur in the presence of a specific person, called an agent (Whitton, 1978). Over the years a substantial volume of research has been conducted into the characteristics of such agents. It seems as though these people, usually teenagers (Roll, 1978), often experience high levels of frustration which they are unable to vent in an acceptable way either verbally or non-verbally (Rogo, 1975). The frustration is mostly a result of

aggressive feelings brought on by family interaction and it is implicit family and cultural rules that forbids expression of the anger and frustration. This build-up of frustration with the simultaneous embargo on expression thereof, constitutes a paradoxical situation similar to that existing in hypnosis or in families with disturbed members. (In fact Rogo (1980) mentions a case in which a 16 year old girl became a RSPK agent after her brother suffered a psychotic episode.)

Faced with such a paradoxical situation, the agent has the usual three options for a response. He/she can comment on the paradoxical nature of the situation, but that would mean expressing at least some of the frustration. Alternatively the agent can leave the situation, but that would mean leaving the family, which often is impossible. The only option then left is to express the anger/frustration in such a way that responsibility for it is simultaneously denied. This can be achieved in a variety of ways, depending on what would be acceptable to the system (i.e. the family, community, etc.). Drug or alcohol abuse may serve the purpose, because the addiction could be held responsible for active or passive aggressive episodes. Or the person may develop something like kleptomania, which again he/she cannot control. On the other hand, if the system is inclined to take paranormal phenomena seriously, the frustrated person might become a RSPK agent.

In recent years practitioners of family psychotherapy have come to the conclusion that, although one member of the family might be presented as the person with the problem, usually the problem is caused and maintained by the total family pattern of transactions and role expectancies (Freeman, 1977). This might be the case with RSPK as well. On the basis of an analysis of one particular RSPK case Rogo (1980, p.109) came to the conclusion that "... poltergeist are ... reactions to a total family situation and ... it might be unwise to conceptualize a RSPK outbreak as necessarily a psychic eruption revolving around a central agent." The role of the family interrelationships in RSPK might therefore be a fertile field of further investigation.

The difference between hauntings and RSPK cases have often been thought to lie in the fact that no specific agent seems to operate in hauntings, while such an agent is a characteristic of RSPK. However, in the light of the possibility that the total family

dynamics could underlie RSPK, the difference between these two types of phenomena might fade away. In fact, Rogo (1980) suggests that, when RSPK is caused by total family interaction, it might resemble haunting very closely.

Over the years a large number of studies have been done on so-called spontaneous psi occurrences, that is, instances where psi occur outside of a laboratory or experimental setting. By and large these studies have concentrated on the psi experiences themselves; often these experiences were very carefully analysed (e.g. Stevenson, 1970). Only rarely have the analyses so much as touched on the patterns of interaction surrounding the psi experiences, though.

An exception is a study by Schwartz (1971) of telepathic occurrences in his own family. Although the study is presented from a psychoanalytic point of departure, and by a family member itself, the two elements of paradox and the complementary nature of the relationship between the parents and the children, emerge clearly.

It can be hypothesized that, were careful interactional studies to be made of more instances of 'spontaneous' psi, the two elements of complementarity and paradox will stand out as will the interpersonal effects of the psi behaviour, because interpersonal behaviour always has such effects (Fourie, 1980). This would mean unravelling the complicated webs of interpersonal transactions which occur in families and other systems, a process similar to that family therapists undertake.

CONCLUSION

What has been presented here represents a theoretical shift from an individual- or subject-centered view of psi to a relationship oriented one. It is less limiting than traditional intrapsychic conceptualizations in that it takes into account the total observable situation or context in which psi occurrences take place, be it inside or outside of the laboratory. This is in contrast to traditional conceptualizations which often rely on vague and unobservable intrapsychic concepts, such as the subconscious, the ego, and other hypothetical constructs

(MacCorquodale and Meehl, 1948). As such it affords a more parsimonious way of conceptualizing that which can be observed to happen when psi takes place.

The interactional view of psi does not deny subjective experience and the value of models which utilize intrapsychic conceptualizations, though. It simply prefers to limit itself to observable interaction patterns.

When the interactional approach to psi sees it as involuntary behaviour evoked within a paradoxal relationship by means of a process of negotiation, it makes no effort to explain the mechanics of the psi process. It concerns itself with what happens rather than with how it happens. As such it has no argument with models such as that of Tart (1977) which endeavour to explain the workings of so-called psi channels. An interactional model of psi can be a useful complement to such mechanistic models and as such it opens up a comprehensive research field.

ABSTRACT

One of the similarities between hypnosis research and psi research is an emphasis on the behaviour and the intrapsychic experiences of the subject to the virtual exclusion of the dimensions of the hypnotist-subject or experimenter-subject relationship. This paper presents a theoretical shift in that the focus is placed on the hypnotist-subject relationship rather than on one of the participating parties only. This relationship is analysed from a communications point of view and similarities with the experimenter-subject relationship in psi research are pointed out. From this perspective a tentative model of psi as involuntary behaviour evoked within a particular type of relationship by means of a negotiation process, is formulated.

REFERENCES

Alcock, J.E. 'Parapsychology. Science or magic? A psychological perspective', New York, Pergamon Press, 1981.

Barrett, W.F. 'Psychical research', London, Williams and Norgate, 1918.

Beloff, J. and Mandleberg, I. 'An attempted validation of the 'Ryzl technique' for training ESP subjects', J. S.P.R., 1966, 43, 229-249.

Braud, W.G. 'Psi-conducive states', J. of Communication, 1975, 25, 142-152.

Broughton, R. 'An exploratory study on psi-based experimenter and subject expectancy effects', in Morris, D.J., Roll, W.G. and Morris, R.L. (eds) 'Research in parapsychology 1976', Metuchen, Scarecrow Press, 1977.

Fourie, D.P. 'An attempted revival of the Ryzl training method', in Morris, J.D., Roll, W.G. and Morris, R.L. (eds) 'Research in parapsychology 1976', Metuchen, Scarecrow Press, 1977.

Fourie, D.P. 'Psi: An interactional perspective', Parapsychological J. of South Africa, 1980, 1, 44-62.

Fourie, D.P. 'Hypnosis and psi: Taking stock', Parapsychological J. of South Africa, 1982, 3, 17-27.

Fourie, D.P. 'Width of the hypnotic relationship: An interactional view of hypnotic susceptibility and hypnotic depth', Australian J. of Clinical and Experimental Hypnosis, in press.

Fourie, D.P. and Lifschnitz, S. 'Hypnosis: Depth or width?', Perceptual and Motor Skills, 1982, 54, 906.

Freeman, D.S. 'The family systems practice model: Underlying assumptions', Family Therapy, 1977, 4, 57-65.

Haley, J. 'Strategies of psychotherapy', New York, Grune and Stratton, 1963.

Haley, J. 'Uncommon therapy. The psychiatric techniques of Milton H. Erickson', New York, Norton, 1973.

Honorton, C., Ramsey, M. and Cabibbo, C. 'Experimenter effects in extrasensory perception' J. A.S.P.R., 1975, 69, 135-150.

Jacobson, E. 'Progressive relaxation', Chicago, University of Chicago Press, 1938.

Kennedy, J.E. and Taddonio, J.L. 'Experimenter effects in parapsychological research', J.o.P., 1976, 40, 1-33.

Lifschitz, S. and Fourie, D.P. 'A pragmatic approach to hypnosis - theoretical postulates', in preparation.

MacCorquodale, K. and Meehl, P.E. 'On a distinction between hypothetical constructs and intervening variables', Psychological Review, 1948, 55, 95-107.

Palmer, J. 'Extrasensory perception: Research findings', in Krippner, S. (ed) 'Advances in parapsychological research. Volume 2. Extrasensory perception', New York, Plenum Press, 1978.

Parker, A., Millar, B. and Beloff, J. 'A three-experimenter ganzfeld: An attempt to use the ganzfeld technique to study the experimenter effect', in Morris, J.D., Roll, W.G. and Morris, R.L. (eds) 'Research in parapsychology 1976', Metuchen, Scarecrow Press, 1977.

Rhine, J.B. 'Comments: A new case of experimenter unreliability', J.o.P., 1974, 38, 215-225.

Riggs, B.C. 'System C: An essay in human relatedness', American J. of Psychotherapy, 1978, 32, 379-392.

Rogo, D.S. 'Parapsychology. A century of inquiry', New York, Taplinger, 1975.

Rogo, D.S. 'The poltergeist and family dynamics: A report on a recent investigation', in Roll W.G. (ed) 'Research in parapsychology 1979', Metuchen, Scarecrow Press, 1980.

Roll, W.G. 'Understanding the poltergeist', in Roll W.G. (ed) 'Research in parapsychology 1977', Metuchen, Scarecrow Press, 1978.

Ryzl, M. 'Hellsehen und andere parapsychische Phaenomene in Hypnose', Genf, Ramon Keller, 1971.

Ryzl, M. 'ASW-Experimente die erfolgreich verlaufen. Der sechste Sinn auf dem Pruefstand', Genf, Ariston Verlag, 1978.

Schmeidler, G. and McConnell, R. 'ESP and personality patterns', New Haven, Yale University Press, 1958.

Schwartz, B.E. 'Parent-child telepathy. Five hundred and five possible episodes in a family', New York, Garrett Publications, 1971.

Stanford, R.G. 'An experimentally testable model for spontaneous psi events. I. Extrasensory events', J. A.S.P.R., 1974, 68, 34-57.

Stevenson, I. 'Telepathic impressions. A review and report of thirty-five new cases', Charlottesville, University Press of Virginia, 1970.

Swann, I. 'On the subjective nature of psychic research, the subject-experimenter relationship, and the psychic type of personality', in Tart, C.T., Puthoff, H.E. and Targ, R. (eds) 'Mind at large', New York, Praeger Publishers, 1979.

Tart, C.T. 'Scientific studies of the psychic realm', New York, Dutton, 1977.

Thouless, R.H. 'From anecdote to experiment in psychical research', London, Routledge, 1972.

Van de Castle, R.L. 'The facilitation of ESP through hypnosis', American J. of Clinical Hypnosis, 1969, 12, 37-56.

Watzlawick, P., Weakland, W.H. and Fisch, R. 'Change. Principles of problem formation and problem resolution', New York, Norton, 1974.

Weitzenhoffer, A.M. 'Hypnotic susceptibility revisited', American J. of Clinical Hypnosis, 1980, 22, 130-146.

Whitton, J.L. 'Psychology and the poltergeist. A review', Parapsychology Review, 1978, 9, 8-10.

Wilson, S.C. and Barber, T.X. 'The fantasy-prone personality: implications for understanding imagery, hypnosis and parapsychological phenomena', in Sheikh, A.A. (ed) 'Imagery: Current theory, research and application', New York, Wiley, in press.

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IMPROVING PSYCHOKINESIS PERFORMANCE
THEORETICAL AND METHODOLOGICAL NOTES

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In 1966, I proposed (Tart, 1966) that the usual procedure for forced-choice testing in ESP experiments, namely delaying any feedback about individual target identity until after a run was completed or even indefinitely, constituted an extinction paradigm rather than a learning paradigm. Thus the frequently reported decline effect made sense from a learning theory approach: percipients were being systematically extinguished. More positively, the theory proposed that for motivated percipients with some initial ESP talent, the provision of immediate feedback of results on each trial could not only eliminate the decline effect, but could allow learning, so ESP performance could improve with time and come under voluntary control.

In an extensive modeling of internal processes involved in trying to learn to use ESP with immediate feedback (Tart, 1977), I suggested that what a percipient must basically try to do is find some aspect or aspects of his ongoing stream of experience that correlate with successful use of ESP. It is necessary to postulate some kind(s) of discernible experiential qualities associated with successful ESP functioning for learning to be possible, and the generally high success rate associated with confidence calls lends empirical support to this postulate. Discerning such aspects of experience would not, of course, be an easy task. Most people have had no training at trying to observe their ongoing experience, and

numerous ordinary thought processes constitute irrelevant and distracting noise. Further, there is a formidable problem which was discussed in the earliest presentation of the theory (Tart, 1966) in connection with the concept of the 'talent threshold', and amplified in the later modeling of internal processes (Tart, 1977). This is the problem of noise and confusion created by false positives, hits due to chance alone.

In any forced-choice ESP task, where the a priori probability of a hit is p , a proportion p of the trials will yield hits due to chance. Every one of these false positive hits will be associated with some psi-irrelevant aspects of ongoing experience, just as hits due to actual functioning of ESP will hopefully be associated with aspects of ongoing experience that are regularly and lawfully related to the proper functioning of the ESP process. Thus each false positive hit can generate a false hypothesis about some aspect of experience possibly being associated with ESP operation, can falsely reward the percipient, and can generally distract and confuse him or her. False positive hits mean that there is some extinction procedure inherent in any forced-choice tests, despite the provision of immediate feedback.

If a percipient has no demonstrable ESP ability to begin with, all his hits are false positives, associated with irrelevant aspects of experience, so he has nothing to learn. For a percipient with a little psi ability, there is a mixture of genuinely psi-mediated hits and false positive hits. Extinction may be slowed down for a time, insofar as the percipient efficiently makes use of relevant cues from psi-mediated hits to learn some control over his ESP abilities, but the confusion caused by false positive hits may eventually sap his motivation and inhibit learning, with extinction being a final outcome. For a percipient with high ESP abilities, on the other hand, many of his hits will be psi-mediated, giving him the opportunity to form and test useful hypotheses about aspects of his ongoing experience that are correlated with successful use of ESP. Once such genuine correlations are learned, the percipient may then opt to make calls only when his experience shows the characteristics he now knows to be psi-favorable; and/or he may deliberately try to induce such psi-favorable aspects of experience.

The 'talent threshold', as originally postulated, meant some initial level of psi talent brought to the immediate feedback

training sessions that was high enough so the learning process predominated over the extinction process inherent in any forced-choice testing procedure. The talent threshold is not an absolutely fixed level of ESP talent. Higher levels of motivation, e.g., can keep a percipient working at discriminating relevant and irrelevant aspects of experience and possibly learning even if their initial level of talent is somewhat lower than the talent threshold might need to be for percipients in general. Further considerations of this sort are discussed elsewhere (Tart, 1977).

EMPIRICAL SUPPORT FOR THE LEARNING THEORY

My learning theory application will be adequately tested when a reasonable number of initially highly talented percipients have been given immediate feedback training, training that they are motivated to work at for a time. Given the rarity of highly talented percipients, both in my own work and in parapsychological research generally, it may be years before the usefulness or lack of it of the learning theory application has been adequately tested. Thus the theory is in a kind of limbo as regards percipients being able to learn improved ESP ability by immediate feedback training, although it has some empirical support.

The evidence is rather strong, though, that the provision of immediate feedback largely eliminates decline effects in ESP testing. This literature has been reviewed elsewhere (Tart, 1976).

LEARNING TO USE PK

Although my original application of learning theory focused primarily on ESP, almost all aspects of it apply directly to PK. If any potentially discernible aspects of experience are associated with successful use of PK, a PK agent receiving immediate feedback of his or her results should be able to form hypotheses about what these correlations are, test these hypotheses, and eventually show much greater success at PK, either by waiting for the right internal condition to make his tries, and/or by inducing PK-favorable aspects of experience at will.

I mentioned above that the learning application hypothesis has not been adequately tested for ESP learning, partly because percipients have almost always been run without immediate trial-by-trial feedback, partly because of the rarity of talented percipients. At first glance, PK studies might seem to have provided a good data base for testing the learning hypothesis, as immediate feedback has been the norm rather than the exception. In dice studies the agent immediately sees how the dice fall; in electronic random number generator (RNG) PK studies there is usually some sort of continuous feedback display operative. Empirically, however, decline effects are common and incline (learning) effects very rare in published PK research, according to Stanford (1977).

On closer inspection, however, the PK literature to date does not provide an adequate data base for testing the learning theory application. The level of psi talent shown by agents in laboratory PK studies has generally been far too low.

I am in the process of reviewing the PK literature. To date I have computed psi coefficients (the proportion of time psi was presumably operating after the chance baseline hits have been factored out: see Timm, 1973) for the most successful conditions of 33 PK studies involving mechanical PK tests (mostly dice) (note 1). I have used only experimental condition results that were statistically significant ($p < .05$) for the presence of PK hitting beyond baseline expectation (not just for differential effects). I have also computed psi coefficients for 35 similarly successful PK studies on electronic RNG's (note 2). Combining both types, over 90% of these studies show psi coefficients (note 3) less than the 10% that I have empirically estimated as the mean talent threshold for learning to begin in ESP studies (Tart, 1976). Indeed, the vast majority (57%) of psi coefficients shown in the laboratory studies fall in the 1% to 3% range. Thus it is not surprising that declines (extinction) have been quite common in laboratory PK research.

I would have liked to do an analysis comparing psi talent levels in the reported studies with degree of decline or lack of it, but wide variations in completeness of reporting and method of computing decline effects produce too few studies to allow a fruitful analysis.

INSTRUMENTAL CONDITIONING VERSUS AVERSIVE CONDITIONING

The learning theory application I have proposed for increasing psi ability is a form of instrumental or operant conditioning. We take a behavior that exists only in infrequent and poorly developed form (the initial, non-zero level of psi functioning a percipient or PK agent brings to the training), and try to increase and refine it by providing informational feedback. Insofar as the percipient or agent is motivated to succeed, feedback of correctness also constitutes a reward.

Recently Broughton, Millar, and Johnson (1981) introduced a different learning paradigm, aversive conditioning, in an attempt to increase PK ability. Using an electronic RNG PK task, they delivered strong, unpleasant electrical shocks to their four agents when they obtained poor scores. No overall significant indications of PK were found, and even the most liberal interpretation of their results would show no more than tiny possible indications of PK.

Since none of the four would-be agents of the study had ever shown, to my knowledge, psi coefficients approaching the 10% level in previous laboratory psi studies, it is not surprising that no clear PK was found in this study, much less indications of learning. The results are in accord with those predicted from my learning theory application, albeit trivially so.

More importantly, while the authors of this study were familiar with my learning theory application, I do not agree with their extension of it to use of aversive conditioning. In an aversive conditioning paradigm, we begin with a well-developed behavior which we consider undesirable, and we attempt to decrease its frequency of appearance with applying punishment (aversive stimulation) whenever the person shows it. In trying to learn ESP or PK ability, to the contrary, we have an almost totally undeveloped behavior. It is unclear how applying punishment to suppress the behavior of not using psi will allow the subtle learning needed to recognize experiential correlates of psi.

Punishment might increase motivation to learn psi, but, given my extended model of internal processes involved in learning psi (Tart, 1977), I would suspect that the specific fear,

anticipation, and generalized unpleasant arousal occurring in the aversive conditioning situation would greatly increase the 'noise level' of irrelevant mental processes, and so make instrumental learning of increased psi ability extremely difficult. It is hard to search for subtle internal experiences when your mind and body are cringing in anticipation of the next severe shock!

Thus I do not recommend the use of aversive conditioning procedures for enhancing psi.

CONCLUSION

Insofar as there are any potentially discernible experiential correlates of the use of PK, the provision of immediate feedback of results should allow an agent to learn more effective control of his or her PK ability. The same sorts of considerations advanced in connection with using immediate feedback training to enhance ESP ability apply to its use to enhance PK, such as the need for the agent to be motivated to learn enhanced PK and, very importantly, to possess a fairly high level of initial PK ability. This level is roughly estimated, by analogy with ESP results, to correspond to a psi coefficient of about 10%.

I believe much of the impasse in attempts to control and understand PK arises from the same source as the impasse in attempts to control and understand ESP, namely that the manifestations of ESP and PK we usually see in the laboratory are of too small a magnitude to be readily separated from noise. If immediate feedback training leads to strong, reliable manifestations of laboratory PK, then research on understanding and controlling PK will take a quantum leap forward.

NOTES

1. I want to thank Barry Boatman for the library work he carried out for me that underlies this literature survey.
2. I am indebted to Ed May of SRI International for the raw data he used in an extensive study of electronic RNG PK, from which I have extracted the above 35 studies by my success criterion.
3. Psi coefficients are ordinarily expressed as decimals, but I shall use percentages for readability. Thus a psi coefficient at

.10 is called 10%, and indicates psi being used on 10% of the trials that would otherwise show misses by chance alone.

ACKNOWLEDGMENT

A shortened version of this paper was presented at the combined 1982 meetings of the Society for Psychical Research and the Parapsychological Association, held in Cambridge, England.

REFERENCES

Broughton, R.S., Millar, B.M., and Johnson, M.U. 'An investigation into the use of aversion therapy techniques for the operant control of PK production in humans', *E.J.P.*, 1981, 3, 317-344.

Stanford, R. 'Experimental psychokinesis: A review from diverse perspectives', In Wolman, B. (Ed) 'Handbook of Parapsychology', New York, Van Nostrand Reinhold, 1977, 324-381.

Tart, C.T. 'Card guessing tests: Learning paradigm or extinction paradigm?', *J. A.S.P.R.*, 1966, 60, 46-55.

Tart, C.T. 'Toward conscious control of psi through immediate feedback training: Some considerations of internal processes', *J. A.S.P.R.*, 1977, 71, 375-408.

Tart, C.T. 'Learning to Use Extrasensory Perception', Chicago, University of Chicago Press, 1976.

Timm, U. 'The measurement of psi', *J. A.S.P.R.*, 1973, 67, 282-294.

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COOPERATION VERSUS COMPETITION: IN DEFENSE OF
RATIONAL ARGUMENT IN PARAPSYCHOLOGY

Gerd H. Hövelmann

"When two ideas are conflicting,
all the powers of demagogy are
applied"
(Fleck, 1980, p.158)

INTRODUCTION

In a paper I wrote in September 1979 I attempted to analyze the verbal behavior of scientists engaged in communication with each other within the framework of their specific branch of science on its problems. Taking samples from the parapsychological literature, I tried to demonstrate that discussions very often fail to come up to the required standards of rational discourse. When in December 1980 that article appeared in the German 'Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie' (Hövelmann, 1980) the editors of the 'European Journal of Parapsychology' kindly offered to reprint an English version of it. However, since translation of the paper was delayed for various reasons and since, during these past four years, my views, especially on the problems I treated in the theoretical part of that paper, have slightly changed, I suggested that I write this enlarged and considerably revised English version of my original article for publication in the 'E.J.P.'.

As far as I can see, in the history of their field only very few parapsychologists have expressed concern about the sometimes acrimonious style of debate among them (note 1). The few exceptions I came across can be counted on the fingers of one hand. Thus, in an attempt to provide "some guiding principles" (some of which I do not agree with, by the way) to "enhance the beneficial aspects of criticism in parapsychology", Stevenson and Roll (1966, p.347) emphasized:

"... the critic inside the field has a greater responsibility than one approaching it from the outside; the views of an expert command more attention than those of a casual observer ... Well-founded criticism serves the important function of clearing away spurious data. Mistaken criticism can cause great harm ... A well-publicized attack, even if unfounded, may discourage needed support, not to mention its victim, the investigator."

Similarly, John Palmer (1981, p.11) commented on a recent controversy:

"The interactions at times seemed more like confrontations, and one only hopes that the interesting theoretical issues which underlie the two positions will not take a back seat to the kind of ad hominem backbiting which characterizes our relations with so many critics outside the field"

Finally, to give one further example, Rex Stanford (1982, p.277) addressing editors of parapsychological periodicals, recently remarked:

"It is one thing to point out what one believes are errors in (a) paper, but it is quite another to go beyond those errors by imputing motives which cannot possibly be justified by the mere existence of the errors or supposed errors. Whenever such an imputation is not justified by the facts to be published, and especially when that imputation has serious and possibly hurtful consequences, it should not be published."

These are all the papers I found clearly condemning unfair, polemical exchanges within the field. Below I will quote two further statements by parapsychologists arguing in favor of

polemic and sharp criticism.

The present paper is in no way intended to disparage legitimate criticism in parapsychology. On the contrary, I believe that much more competent and responsible inside criticism is urgently needed. In my view, however, abandoning the rules of rational discourse (a) is entirely superfluous, since it does not improve or add anything desirable to sound criticism and (b) may have detrimental consequences to our efforts to establish parapsychology as a legitimate scientific discipline.

I fully agree with Wiegand (1978, p.25), who holds that "oral and written scientific communication among scientists within a specific branch of science" constitutes "a largely neglected field of research" (note 2). So far there exists just one pertinent study dealing with the style of (oral) argumentation, on the occasion of a convention of linguists (Geier, Keseling, Nehrkorn and Schmitz, 1977) (note 3). Not only the 'external' linguistic communication between scientists and laymen (which here is meant to include scientists from other, non-neighboring disciplines) but also - and even more importantly - the 'internal' exchange of ideas frequently does not seem to function in such a way as to reliably secure attainment of the respective aims of communication. On a closer inspection of conflicts in the realm of scientific communication, the common picture of the rational, objectively and unemotionally arguing scientist who uses language in order to attain a level of knowledge as sophisticated as possible or to impart results of his research in an appropriate way, proves to be highly questionable and requiring correction. In what follows, I will try to show what scientific disputes in the realms of intra-scientific activity do, in fact, look like. It will become evident that, partly for reasons of carelessness and partly as a result of tactical considerations, basic rules of rational arguing are frequently invalidated. To demonstrate this, I will take samples from two disputes published in the parapsychological literature of recent years, since - to borrow Sybo Schouten's words (personal communication, December 19, 1980) - "parapsychologists are frequently involved in scientific debates and, in my opinion, should set an example as regards style of debate."

HUMAN ORIENTATIONAL ACTIVITY AND RULES OF RATIONAL ARGUING

In the original version of this paper, I adopted the concept of orientational activity from the work of the Soviet psychologist, Galperin (1967; 1980). In the meantime, it has become plain to me that Galperin's understanding of orientational activity contains several serious shortcomings which are mainly due to his background in orthodox Marxian philosophy, to an inadequate 'representation theory', to his adherence to a naively realistic theory of knowledge, and especially to his belief that cultural developments can be adequately described in terms of evolutionary theory (note 4). Since I continue to find the term 'orientational activity' quite suitable, I will retain it here and briefly elaborate my own thoughts on how it should be used.

If someone tries to cope with an unknown situation of whatever kind, he not only 'behaves' in a particular way; he rather 'acts' by applying orientations and distinctions he has acquired in linguistic contexts, or by forming new orientations or drawing new distinctions relative to his background of acquired orientations and distinctions which, in turn, are constituted by his elementary practice of life. We may thus speak of an 'orientational activity' that is indispensable for the conduct of our lives.

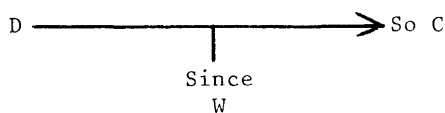
Now the mutual dependence of theory and practice implies that science itself is to be regarded as a 'practice-stabilizing theory'. Science - viewed as such a practice-stabilizing theory in so far (in the form of actions answering one's purposes) it serves to provide practical orientations - always refers to concrete practices which, in turn, themselves result from successful mediations of theory and practice. Scientific theories are to be defined methodologically as well as teleologically: on the one hand, their historical development is determined by methodological considerations and by the requirements of changing data bases; on the other hand, however, the historical development of scientific theories is also (and on a more basic level) determined by the proper consideration and estimation of purposes and aims which lie beyond the methodological viewpoint (note 5) and which many people wrongly regard to be aims completely 'exterior' to science. While the methodological aspect concerns a theory's adequacy in a formal sense, viz. the substantiation of a set of scientific propositions, the teleological aspect directly judges of a theory

and its applications in terms of the justifiability of practical orientations and purposes. Thus, it is not enough only to show that a theory has practical applications; rather, it must be demonstrable that these applications can be justified. (cf. Hövelmann, 1982, p.132). A basic precondition of attempts at such a justification is that it must be possible to carry on rational dialogues about practical orientations (rules for actions, purposes, aims, etc.) on which the historical developments of theories depend. This is the point from which the problems we are mainly concerned with in this paper originate. Strictly speaking, the crucial problem is that not all practical orientations are common orientations and that without common orientations practice becomes chaotic!

A specifically human means of orientational activity is represented by the use of language; thus, human orientational activity can be described as linguistically mediated activity. In a cooperative interaction, the various actors concerned take their bearings with regard to particular tasks (no matter whether these tasks are present, future, or even potential ones) by way of mutually communicating, for instance, their earlier experiences with similar tasks, assessments of relevant situations, required previous knowledge, necessary instructions, proposals for solving the tasks under consideration, fears of potentially undesirable consequences that may turn up, suggestions for future actions, alternatives, applications, or related tasks, etc.. As everybody knows by experience, such proposals, suggestions, and deliberations frequently result in considerably different opinions among the actors involved. To be able to deal with such a situation in an appropriate way, certain procedures for comparison and proper assessment of different opinions are required. These procedures essentially consist of an argument in support of, say, the claim (suggestion, plan, hypothesis, strategy, etc.) 'p' and of another argument in support of the claim '-p' (to be read 'non-p'). According to Toulmin (1958, esp. chapter III), it is essential that "keeping our eyes on the categories of applied logic - on the practical business of argumentation, that is, and the notions it requires us to employ - we must ask what features a logically candid layout of arguments will need to have" (ibid, p.95). Toulmin asks what is involved in establishing conclusions by the production of arguments, and, in the course of his investigation, he finds that the respective discussants are following certain rules of argumentation: in support of their

claims or conclusions (C), by which they have committed themselves to certain obligations and whose merits they are seeking to establish, they appeal to certain facts or data (D) as a foundation for their claims. Then they have to demonstrate the bearing on the claims or conclusions (C) of the data (D) they have provided. Presenting a particular set of data (D) as the basis for some specified conclusions (C) commits them to the use of (justificatory) warrants (W), which correspond to practical standards or canons of argument.

"We now have the terms we need to compose the first skeleton of a pattern for analysing arguments. We may symbolise the relation between the data and the claim in support of which they are produced by an arrow, and indicate the authority for taking the step from one to the other by writing the warrant immediately below the arrow:



." (ibid, p.99)

Although I do not agree with Toulmin's more far-reaching conclusions regarding the foundation of formal logic, and although his system of rules for argumentation has been developed much further in later years (esp. cf. Berk, 1979; Gethmann, 1979; Gethmann ed., 1980; Lorenzen, 1969), this first (and still rather primitive) pattern for analyzing arguments is sufficient for the purposes of the present paper.

Now in factual discussions, frequently incomplete arguments are to be found; that is, parts of the necessary arguments are left out or are merely presupposed. Defective arguing can become undetectable or even be intentionally veiled in that way. In ideal argumentation, on the other hand, the various actors argue explicitly and according to the rules, or are at least able to provide explicit and regular arguments upon request. "If (our) claim is challenged," writes Toulmin (1958, p.97) "we must be able to establish it - that is, make it good, and show that it was justifiable." That is to say, that in an ideal, cooperative argument every participant has to be able (and is obliged) to

correctly, completely, and unambiguously inform his partner(s) of his view of the objects or situations in question and of the aims and purposes he is trying to attain. Dissension, thus, must not result from the (intentional or unintentional) incorrect use of the rules of arguing, but only from different knowledge or different opinions about the matter in question. It should be recorded, moreover, that the linguistic means necessary for adequate (as well as those necessary for defective) argumentation are teachable and learnable.

Modern societies (whether in the eastern or in the western hemisphere), and especially modern science, are usually dealing with problems much too complex and difficult to be solved by single individuals. Therefore, these problems are divided into sub-problems, and in science, for instance, sub-tasks are delegated to smaller groups of scientists doing collective or team work (note 6). To keep such collective work of human beings in general and of scientists in particular functioning, it must necessarily be cooperative in character. This cooperative work of scientists aims at the joint production and use of knowledge, technologies, etc.. Especially in scientific contexts and discussions, the mutual acceptance of this cooperative principle also serves the communication of and agreement on matters relevant to the respective tasks and problems. Cooperative argumentation is characterized by the use of certain linguistic means such as: to evaluate, to oppose, to doubt, to substantiate, to repeat, to explicate, to interpret, to ascertain, to direct, to refute, to confirm, to corroborate, to question; by the formulation of statements or objections, and by making revisions and pointing to differences, discussants try to elaborate sound, justifiable, and mutually acceptable formulas or propositions which, they hope, can serve a useful purpose for future research or theory-building. In factual communication, however, "there are always only certain degrees of the cooperative principle mutually accepted and realized" (Henne and Rehbock, 1979, p.23). A new aspect comes to the fore instead: competition. Linguistic means characteristic of this competitive aspect are: to warn, to praise, to blame, to censure, to appeal, to ridicule, to embarrass, to cut out rivals, to prefer, to admonish, to call rivalling opinions nonsensical, crazy, or unacceptable without sufficient specifications, to hold back relevant information, etc..

As I indicated above, so far there is only one investigation of

the style of (intra-disciplinary) oral communication among scientists (Geier, Keseling, Nehr Korn and Schmitz, 1977). The authors try to find out basic characteristics of conflicts in communication which are caused by competitive arguing strategies as applied by participants of a scientific convention. Since, according to Holzkamp (1978, p.137), the cooperative character of collective work is independent of whether the cooperating scientists know each other, and since even scientists who live spatially and temporally far apart can nevertheless be in a cooperative relationship to each other, it is legitimate to subject written discussions to a comparable analysis. The specific character of such scientific debates calls for a pragmatic analysis which should be able to point to particular patterns of such debates in the light of the rules for rational argumentation. We may point out the following idealized patterns of rational argumentation which are to be found in scientific discussions:

1. A statement, claim, thesis, etc., put forward in an article (book, etc.) may be contested or restricted by:
 - a) mentioning empirical counter-examples;
 - b) demonstrating that the author failed to consider previous research findings, methodological considerations, relevant and reliable findings of other disciplines, precautions against fraud or artifacts, etc.;
 - c) explicating the applied procedures and demonstrating deficiencies in the study's design or defects in the evaluation (statistical calculation, etc.) of the material;
 - d) demonstrating violations of commonly accepted fundamental logical, methodical, or methodological rules of research.
2. A statement, claim, thesis, etc. put forward in an article may be supported by:
 - a) mentioning further empirical findings (usually results of comparable experiments);
 - b) referring to corresponding intra- and inter-disciplinary research findings and demonstrating that adequate precautions have been taken;
 - c) explicating the applied procedures and demonstrating the adequacy of the study's design and the evaluation of the material;
 - d) demonstrating that commonly accepted rules of research have not been violated.

This list is incomplete, of course, but it may suffice here.

COOPERATION AND COMPETITION IN PARAPSYCHOLOGICAL WRITING

The findings so far are compatible with the rules of rational argumentation and with the cooperative principle. This impression changes fundamentally, however, if we go into the details of argumentation and take a closer look at the formulation of single arguments. In the original German version of this paper (Hövelmann 1980, p.148-150), I took 37 samples from 17 contributions to the parapsychological literature to demonstrate that arguments put forward by parapsychologists in intra-disciplinary debates and controversies very often fail to measure up to the required standards of rational discourse (note 7). In the present paper, I will restrict myself to taking samples from the literature on two controversies which has been published in recent years. The first of these controversies is the discussion between Stephen Braude and Lila Gatlin on the value and acceptability of Gatlin's information-theoretic approach (Braude, 1979a; 1979b, Gatlin, 1979), the second is the discussion (and its complications) on the use of univariate versus multivariate experimental designs between Carl Sargent and Adrian Parker (Sargent, 1979; 1980b; Parker, 1980; 1981). It should be explicitly noted here that this study (as was the earlier one) is not intended to embarrass those people from whose writings I took my samples! On the contrary, I would hold that very similar patterns are to be found in the writings of most people in any field of inquiry, and a close inspection of my own publications (especially of those dealing with the philosophy of the natural sciences and the philosophy of language) would certainly reveal the same deficiencies as I am going to point out here. I had initially intended to conceal the parties involved, but it would have been very easy for regular readers of the parapsychological literature to find out from which publications my samples were taken. And reformulating the passages in question in order to make them untraceable would have certainly brought on the reproach that my examples are fabricated to fit them to the goals of the paper, and that deficiencies of that kind were probably not at all to be found in the literature. So I left names in.

Regrettably, the Parapsychological Association's very useful 'Ethical and Professional Standards for Parapsychologists' (Parapsychological Association, 1980) does not explicitly deal with the problems we are concerned with here. But interestingly,

while Braude and Gatlin do not talk about the style of their exchange, both Sargent and Parker express themselves on the value of polemical exchanges in their writings. Thus, Sargent says:

"Reading Paul Feyerabend has tended to increase my aggressiveness and taste for polemic and sharp criticism, and doubtless I will have offended some people with some comments (Sargent, 1980a, p.vii).

In a similar way, Parker (1980, p.23) reveals:

"One of the more positive trends identifiable in modern parapsychological research is a tendency towards polemic. Clearly the open exchange of viewpoints is a healthy sign in any developing science"

However, in a later reply to Stanford's (1982) criticism of Parker (1981), the latter concedes (Parker, 1982, p.403-404):

"Stanford has made an important point about professional rhetoric, which may have wider relevance. The tone of my article was set by that of Sargent's earlier papers and was an exercise at meeting him on this level. My present feeling ... is that there is little to be gained from this type of confrontation and it is easy to lose sight of the issues involved."

I will now turn to the samples I have taken from the publications mentioned above and discuss them afterwards (note 8):

- 1) "Apparently Dr. Braude has not heard of the genetic code" (Gatlin, 1979, p.323).
- 2) "Dr. Gatlin's reply to my paper represents no conceptual advance over her original paper, and in fact retains all of its major errors. I rather despair of convincing her of this, but perhaps the preceding remarks will have clarified for others what I take to be at issue between us" (Braude, 1979b, p.330).
- 3) "To caricature Parker, we are being invited by the guru to adopt certain approaches merely on his say-so, and the guru seems not to know what those approaches might be expected to produce for us" (Sargent, 1979, p.12).
- 4) "... although (it was) intended by him to be at my detriment

..." (Parker, 1980, p.23).

5) "Sargent is particularly derisive here ..." (Parker, 1980, p.25).

6) "Parker manages to misread my comments and indulge in his favorite trick ..., whilst failing to ..., and thinking that he has disproved a point. Alas! (Sargent, 1980b, p.26).

7) "Parker has an excellent ability to eliminate such flawed experiments only when the data from them contradict his hypotheses and views!" (Sargent, 1980b, p.26-27).

8) "Sargent's article begins in his customary style with a barrage of criticisms and negatively toned value judgements, which in substance amount to little more than a re-statement of the shortcomings already mentioned in my 1977 study" (Parker, 1981, p.374).

9) "...it would seem that without overstating the matter Sargent's research here amounts to little more than a dressed up case study in which results are obtained by fitting the data to the hypotheses" (Parker, 1981, p.377).

10) "I am afraid that Dr. Gatlin has not understood my criticisms ..." (Braude, 1979b, p.325).

11) "Parker misunderstood the results" (Sargent, 1979, p.12).

12) "Dr. Gatlin also fails to grasp my point about ..." (Braude, 1979b, p.329).

13) "Dr. Gatlin thus does not escape my criticism by her sudden and irrelevant appeal to ..." (Braude, 1979b, p.327-328)

14) "I am also unsatisfied with Dr. Gatlin's attempt to explain ..." (Braude, 1979b, p.329).

15) "Neither am I impressed by Sargent's other argument ..." (Parker, 1980, p.24).

16) "Far from having answered the major points I raised in my critique of his paper, Parker has shifted his ground and avoided some of them whilst ignoring others" (Sargent, 1980b, p.26).

17) "If he really believes both these things, he must be a master at eliminating cognitive dissonance" (Sargent, 1980b, p.27).

18) "... her 'theory' is simply form without substance" (Braude, 1979b, p.326).

19) "A second, important, point which Parker ignored has been made ..." (Sargent, 1979, p.11).

20) "Parker ... argued (totally illogically) that ..." (Sargent, 1979, p.12).

21) "I think it is fair to say that Parker did not consider carefully whether ..." (Sargent, 1979, p.12).

22) "It is ... rather tedious that the major points which Sargent

brings ... were already answered by me at the time ..." (Parker, 1980, p.23).

23) "... surely he has misrepresented Beloff ..." (Parker, 1980, p.24).

24) "I find this to be utterly contrived! It is moreover hardly consistent with ..." (Parker, 1980, p.25).

25) "Parker ... attacks the theoretical basis of Ganzfeld on dubious grounds" (Sargent, 1980b, p.26).

26) "Had Parker read my paper he would have noted that ..." (Sargent, 1980b, p.26).

27) "Further confusion is shown when Parker considers ..." (Sargent, 1980b, p.27).

28) "Parker's 'explanation' is arrived at by default" (Sargent, 1980b, p.27).

29) "I am afraid this is all we can really expect from the sort of research Parker urges us to commit (and that, I think, is the only appropriate word to use)" (Sargent, 1980b, p.28).

30) "... this error is simply a version of Dr. Gatlin's error of supposing ..." (Braude, 1979a, p.191).

31) "Dr. Gatlin's position is as unintelligible or otherwise unacceptable now as it was originally ..." (Braude, 1979b, p.326).

32) "... what Dr. Gatlin now says ... is true ...; but this truth is useless for her purposes" (Braude, 1979b, p.326).

33) "I cannot understand how she can ..." (Braude, 1979b, p.327)

34) "More curious still is that Sargent ..." (Parker, 1980, p.24).

35) "... when Parker says this, he cuts his own throat by later arguments" (Sargent, 1980b, p.26).

36) "Parapsychologists have a right to know that not everyone in the academic world shares Dr. Braude's negative view of my work" (Gatlin, 1979, p.322).

37) "It would seem that I am not alone in my misreading of Parker's comments!" (Sargent, 1980b, p.27).

From these 'Konfliktindikatoren' (indicators of conflicts), as Wiegand (1978, p.34) calls them, the following characteristics and features become evident which, of course, are interrelated and frequently change into one another:

a) Disputants are no longer primarily concerned with the problems to be solved but, instead, express themselves on the personal qualities of their respective adversaries. The person of the 'partner' in the communicating process (and the term 'partner' here proves to be a rather euphemistical one) is attacked while

the differences of opinion, which one pretends to be discussing, become secondary. Disputants try to discredit and ridicule their mutual 'partners' and to create the impression that they may be totally incapable of doing acceptable scientific research. In short, arguments pro reo are replaced by arguments ad hominem. One tries to 'proscribe' and 'eliminate' one's respective opponent; minor mistakes are "chalked up and sharply criticized" (Barzun, 1961, p.90). Compare indicators 1 through 9.

b) The opponent is reproached for failing or even for being unable to comprehend which problems are at issue or what kind of criticism has been put forward. In addition, one's own achievements and faculties are directly or indirectly shown off. By way of representing themselves in a light as favorable as possible, disputants try to profit by the controversy. Such publications frequently serve for making oneself known especially to the 'popes' of the scientific discipline one is associated with. Rational argumentation is repressed in favor of one's striving for personal gains, scientific reputation and approbation. Stubborn insistence upon insufficiently substantiated claims is frequently to be found. Compare indicators 10 through 17.

c) The various tasks which are still to be mastered are not placed in the foreground, and no attempt is made at pointing out what the actual problems are (and why those are the actual problems); disputants frequently prove unable to view problematic questions from different angles, instead - in case the opponent holds inopportune views - violations of supposedly unquestionable 'paradigms' are imputed. The opponent is reproached for having made methodological mistakes or for generally lacking methodological rigour. By means of categorical ex cathedra arguments, opponents are told what they are allowed (or forbidden) to do in science; dogmatic phrases are advanced; rules are imposed concerning what one may or may not think, assume, or say; opponents are reproached for having violated the dictates of scientific carefulness which, by definition and according to public charge, they ought to adhere to as scientists. Compare indicators 18 to 29.

d) Disputants appeal to ordinary reason and common sense. Obvious errors are pointed out and a lack of appreciation is expressed for the steps the opponent has taken and of the conclusions he has

drawn. As a consequence, formulations like the above "I cannot understand how she can ..." are to be found; challenges are declared completely nonsensical or rejected as being unacceptable on a priori grounds. Disputants claim to be speaking in the name of common sense (which is viewed as a universal and uniform category), while the respective opponents are regretfully regarded to be severely misled at best. Compare indicators 30 through 35.

e) Statements of other scientists (who are not involved in the dispute in question) which can be construed as supporting one's own views are explicitly welcomed. Applying some force they are combined with one's own opinions, and disputants do not fail to shed a favorable light on their own thoughts and work that way. If need be or if convenient, the entire argument of the outside scientist is not considered but just those parts which seem to fit in with the disputants' respective arguments and goals. The better the reputation of the uninvolved scientist, the greater the chances that his words will be misused in the manner described. Compare indicators 36 through 37.

DISCUSSION AND SOME ADDITIONAL THOUGHTS ON RATIONAL AND MORAL ARGUMENTATION

It is important to note that the above indicators 1 through 37 cannot unequivocally be attached to the patterns of argumentation 'a' through 'e'. Therefore, one should refrain from making generalizing, drawer-like classificatory statements. At best, these patterns can serve as more or less useful heuristic aids; moreover, clear conceptual distinctions cannot always be drawn. It is rather difficult, therefore, to come to a firm quintessence. However, the following interpretation seems obvious and reasonable to me.

The dispute with a 'partner' in the process of intra-disciplinary communication is sometimes understood as a sort of fight. Frequently, the rules of rational argumentation are violated, though only rarely are they completely disregarded. "The proof of truth or untruth is not as decisive as is personal triumph and the defeat of the (scientific) opponent" (Geier, Keseling, Nehr Korn and Schmitz, 1977, p.77). Thus, in order to win the debate, the most important strategies obviously are to demonstrate as many mistakes, absurdities, and weaknesses as

possible in the opponent's thoughts and works, to show that he is unable to master scientific tools and methods, or that he is violating commonly accepted rules of scientific research to an alarming degree. Pointing out that the opponent holds opinions which, alone with the aid of common sense - whatever that may be - can be shown to be invalid, exposes him to ridicule and may considerably undermine his professional reputation. "Various cliches and stereotypes, customary phrases, commonly used reply patterns play an important role (Leont'ev, 1974, p.23).

On the other hand, finding a 'scientific truth' (or making important discoveries or inventions) only advances one's own career if one is able to find (make) them by oneself. If one fails to achieve this, for whatever reasons, the only way to increase one's scientific reputation is to prove that one can at least demonstrate in the work of other scientists that one is able to distinguish important aspects from less important ones, to point to inconsistencies, to successfully dispute proposed explanations, to spread commonly accepted truths, and to reject false claims. On the one hand, reliable scientific findings are at issue in disputes of this kind. On the other hand, every scientist engaged in the process of intra-disciplinary communication endeavors as an individual to strengthen or improve his own position in his respective discipline. This necessarily leads to the unpleasant fact that scientists' readiness to cooperate with their colleagues in a responsible way is permanently usurped by an omnipresent desire to further their own careers. The scientist is thus confronted with an inner conflict: on the one hand, he realizes that scientific knowledge can hardly be achieved by solo attempts and that he is thus dependent on cooperative teamwork. But he knows quite well, on the other hand, that real cooperative attempts at gaining scientific knowledge are rather detrimental to his personal interests.

What, then, are we to make of all this? Is there a way of prophylactically preventing these latent conflicts in communication from actually breaking out from the beginning? There is no such way, I fear, that could guarantee this. I believe, however, that we need not rest upon our oars and that some additional guiding rules for rational communicating, which I am going to offer now, might be helpful (for the following, cf. Kambartal, 1974; Kamlah and Lorenzen, 1973; Lorenzen, 1969, esp. p.73-89.). Let us start with purposes (or synonymously with aims):

a purpose can be defined as the request to maintain or bring about a certain situation (S) which is defined by the description of the situation (DS). If we characterize the performative act of requesting by means of a preceding exclamation mark, a purpose can be symbolized in the form of '!S'. Following this, a way of speaking about interests and needs can be methodically introduced: that a person (P) or a group (G) is 'interested' in a situation S means that P or G work toward the production of S either explicitly by a request or implicitly by orientating their practices according to the purpose '!S'. 'Interests' of P or G which are not derived from other 'interests' may be called the 'needs' of P or G. Now the purpose '!S' does not determine exactly any single action that is suitable to transform a given situation into another situation for which DS is true; that is, the purpose '!S' does not produce unambiguous instructions for a certain action ('!a'). It is thus evident that a (conditional or absolute) request to maintain or bring about a situation S can only be propositional in character and, furthermore, that observance of such requests necessarily depends on the results of joint deliberations on the adequacy or desirability of '!S'. (Otherwise we would speak of orders rather than of requests). Now that is fine in theory, but - as we have seen above - this can lead to considerable practical problems which are caused by the collision of the factual interests of the various persons or groups involved in the joint deliberation. By definition, collisions of interests occur when a person, or especially a group concerned do not avail themselves of ways of action (a) which allow for a pursuit of all of their respective interests; this is because the enforcement of certain of the person's or group's interests (e.g., career) - which means of action (a) may ever be chosen - always leads to a deferment of other interests (e.g., desire to cooperate) of the person or the group (or of single group members).

The underlying interests are usually encountered in the form of private orientations (including agreed upon group orientations), which implies that little allowance is made for the respective interests of other persons, groups, or group members. Therefore, the above joint deliberations have to be 'unprejudiced', 'unconstrained', and 'non-persuasive' in character, in order to take the interests of all persons and groups into consideration. A dialogue is to be called 'unprejudiced' if, in order to make the joint deliberations successful, everyone involved is ready to let the theoretical and practical orientations which underlie his own

actions be questioned until a joint orientation has been elaborated. A dialogue is to be called 'unconstrained' if no agreeing or rejecting acts of speech occur which are caused by (threats of) sanctions. Finally, a dialogue is to be called 'non-persuasive' if agreements or rejections are not obtained by way of an appeal to un-questioned pre-orientations, which is made against one's better judgment (note 9). Following these definitions, we are able to determine what kind of dialogue or argumentation should be called 'rational': it is only those dialogues or arguments which are unprejudiced, unconstrained, and non-persuasive. In such arguments, well-founded and justified orientations or purposes can be agreed upon and modified, if necessary, in consideration of new arguments put forward by additional discussants (group members, etc.). Since it would be short-sighted to believe that a canon of justified and well-founded interests, orientations, and instructions for actions could, once and for all, be established, the process of communication (in science as well as in everyday life) must always be open to new arguments based on different interests, orientations, etc.. This also implies that, on principle, the process of communication can not be safeguarded against misuse for private purposes, (That would only be possible if the above rules of rational argumentation were violated, which itself would mean a misuse). Therefore, any well-founded system of rules of action (including those for argumentation), elaborated in rational dialogues by means of rational arguments, is to be viewed as the result of a development which can always be subjected to revision. Such revision, in turn, should again be made by way of rational arguments, for which the following paraphrase of what Mittelstrass calls his 'Methodical Imperative' (Mittelstrass, 1974, p.203) should be adopted as a normative rule: Act (talk, argue, cooperate, ...) in such a way that, with regard to other participants' interests and orientations, your contributions to an unprejudiced, unconstrained, and non-persuasive argument can serve as a rational means for the attainment of jointly agreeable aims!

CONCLUDING DISCUSSION OF SOME CRITICAL COMMENTARIES

The original German version of this paper has provoked a number of pertinent comments and criticisms, (very brief) discussions of which may be of interest to the readers of the present version.

Besides the reproach that I might have embarrassed those parapsychologists from whose writings I took my samples (I have already answered this point in the first part of section III, above), the following partly interrelated criticisms were made: (1) Rhetorical, polemical, and sharp arguing may serve a useful purpose for the furtherance of discussions in the field and of the field as a whole, and it may elicit some useful ideas which otherwise would not have been put forward; (2) the value of this study is very limited since no systematic screening of the parapsychological literature and subsequent statistical analysis of the relative frequency of polemically styled exchanges have been attempted; (3) scientists from other disciplines and especially critics of the field who frequently view parapsychology as a rather chaotic enterprise conducted by would-be scientists may find their prejudices confirmed; and (4) though this paper is well-intended, it is a little superfluous since no one would care about my admonitions anyway. Interestingly, no one argued that there were no such deficient arguments in parapsychology (Geier et al. have frequently been confronted with this objection, although they, too, provide many samples from oral exchanges).

Ad (1): I confess that I completely fail to comprehend the justification for this objection. What would we be able to reach by using unfair and polemical arguments which we could not equally well (or probably better) attain by way of rational argumentation? Certainly, polemical criticisms, say, of experimental conditions may lead to a strengthening of such conditions in future experiments. But would a criticism devoid of ad hominem attacks be less effective? I do not believe it would. And what useful ideas have been provoked which otherwise would not have come to light? Besides this claim being unfalsifiable, I do not know of any pertinent example.

Ad (2): I do not think that we need systematic screening and statistical evaluation of the literature to be able to point to deficient argumentation. Moreover, I had not intended to do a representative descriptive scientific-sociological study of how parapsychologists interact. This would have only lead - to caricature this approach - to statements of the type: "Mr. A. is significantly more impertinent when talking to Mr. B than when discussing with Mrs. C, while he is particularly obliging when exchanging views with Ms. D who, in turn, ...". I do not need to elaborate further on this, I think. A study of this kind certainly would not help us solve any problems - improving communication,

that is.

Ad (3): I have two things to say here: firstly, I have repeatedly emphasized that deficient argumentation is not unique to parapsychology, because the underlying interests and orientations are very similar across scientific disciplines. The problems we confront in parapsychology are thus not different from the problems in other fields, at least in this respect. Secondly, this objection can be construed as supporting the purpose of this study rather than as questioning it: if we engage in rational argumentation of the kind I have sketched above, scientists of other disciplines will no longer be able to have their prejudices confirmed that way.

Ad (4): This is a very serious objection. Of course, we cannot know a priori but only by experience whether a study of the present type will really serve any useful purpose; and we will not even be able to know this by experience if we do not try. From this point of view, the fourth objection is unreasonable; whether, in addition, it is unfounded, only future exchanges in parapsychology will show. To me, the recent compilation of the 'Ethical and Professional Standards for Parapsychologists' (Parapsychological Association, 1980) seems to be a healthy sign. I am still rather confident.

NOTES

1. As in the earlier paper (Hövelmann, 1980), I will not be dealing here with outside criticisms of parapsychological research or theories.
2. Throughout this paper, translations of quotes from German texts are my own.
3. This was true in the summer of 1979, at least. There may have been some follow-up studies; despite thoroughly searching the literature (in May 1983), however, I was unable to locate any.
4. Elsewhere, I have dealt at considerable length (though not in relation to Galperin) with the third and fourth of these shortcomings especially. Cf. Hövelmann (1983a, 1983b).
5. This does not necessitate the claim that scientific theories

are mainly historical coincidences. For a discussion of parapsychologists' reception of the Kuhnian views of the development of science and scientific theories see Hövelmann (1983c).

6. For the purposes of the present paper, it is irrelevant whether these sub-tasks are explicitly delegated to research groups or whether these groups are (relatively) free to choose their own respective tasks.

7. In this respect, parapsychology does not form an exception. Very similar deficiencies of argumentation are to be found in the literature of any other scientific discipline.

8. I am not going to deal with the problems which have been at issue in these controversies.

9. An appeal to pre-orientations of other persons is made 'against one's better judgment' if it can be clearly inferred from other words or actions of the person making such an appeal that he himself does not accept these unquestioned pre-orientations.

REFERENCES

- Barzun, J. 'Pathologie des Intellekts', Dusseldorf and Vienna: Econ, 1961.
- Berk, U. 'Konstruktive Argumentationstheorie', Stuttgart-Bad Cannstadt, Fromman-Holzboog, 1979.
- Braude, S.E. 'Objections to an information-theoretic approach to synchronicity', J. A.S.P.R., 1979, 73, 179-193. (a)
- Braude, S.E. 'Correspondence: Reply to Dr. Gatlin', J. A.S.P.R., 1979, 73, 325-330. (b)
- Fleck, L. 'Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv', (Ed. by L. Schäfer and T. Schnelle). Frankfurt/M., Suhrkamp, 1980.

Galperin, P.J. 'Die Entwicklung der Untersuchungen über die Bildung geistiger Operationen', in Hiebsch, H. (ed) *Ergebnisse der sowjetischen Psychologie*, Berlin (GDR), Akademie-Verlag, 1967.

Galperin, P.J. 'Zu Grundfragen der Psychologie', Cologne, Pahl-Rugenstein, 1980.

Gatlin, L.L. 'Comments on 'Objections to an information-theoretic approach to synchronicity'', *J. A.S.P.R.*, 1979, 73, 320-325.

Geier, M., Keseling, G., Nehr Korn, M. and Schmitz, U. 'Zum Beispiel: Argumentieren. Ein Beitrag zum Verhältnis von synchroner, ontogenetischer und historischer Rekonstruktion', in Baumgärtner, K. (ed) 'Sprachliches Handeln', Heidelberg, Quelle und Meyer, 1977, 69-108.

Gethmann, C.F. 'Protologik. Untersuchungen zur formalen Pragmatik von Begründungsdiskursen', Frankfurt/M., Suhrkamp, 1979.

Gethmann, C.F. (ed) 'Theorie des Wissenschaftlichen Argumentierens', Frankfurt/M., Suhrkamp, 1980.

Henne, H. and Rehbock, H. 'Einführung in die Gesprächsanalyse', Berlin, New York, de Gruyter, 1979.

Hövelmann, G.H. 'Kooperation und Konkurrenz im wissenschaftlichen Schrifttum der Parapsychologie', *Z. f. Parapsychologie*, 1980, 22, 143-156.

Hövelmann, G.H. 'Reality, relevance, and responsibility', *Zetetic Scholar*, 1982, 10, 131-133.

Hövelmann, G.H. 'Sprachkritische Bemerkungen zur evolutionären Erkenntnistheorie', *Zeitschrift für allgemeine Wissenschaftstheorie/Journal for general philosophy of science*, 1983, 14, in press. (a)

Hövelmann, G.H. 'Natur und Kultur - oder: Die Wiedergeburt einer verfehlten Sprachauffassung', in 'Aufsätze und Reden der senckenbergischen naturforschenden Gesellschaft, 1983, in press. (b)

Hövelmann, G.H. 'Against historicism', Paper submitted for presentation at the 26th Annual Convention of the Parapsychological Association, Madison, N.J., August 1983. (c)

Holzkamp, K. 'Sinnliche Erkenntnis - Historischer Ursprung und gesellschaftliche Funktion der Wahrnehmung', 4th rev. edition, Frankfurt/M., Fischer Athenäum, 1978.

Kambartel, F. 'Moralisches Argumentieren. Methodische Analysen zur Ethik', in Kambartel, F. (ed) 'Praktische Philosophie und konstruktive Wissenschaftstheorie', Frankfurt/M., Suhrkamp, 1974, 54-72.

Kamlah, W. and Lorenzen, P. 'Logische Propädeutik. Vorschule des vernünftigen Redens', 2nd improved and enlarged edition, Mannheim, Vienna, Zürich, Bibliographisches Institut, 1973.

Leont'ev, A.A. 'Psycholinguistik und Sprachunterricht', Stuttgart, Berlin, Cologne, Mayence, Kohlhammer, 1974.

Lorenzen, P. 'Normative Logic and Ethics', Mannheim, Zürich, Bibliographisches Institut, 1969.

Mittelstrass, J. 'Das normative Fundament des Sprache', in Mittelstrass, J. 'Die Möglichkeit von Wissenschaft', Frankfurt/M., Suhrkamp, 1974, 158-205.

Palmer, J. '1980 Parapsychological Association Convention', Parapsychology Review, 1981, 12, 1, 8-11.

Parapsychological Association 'Ethical and Professional Standards for Parapsychologists', (p.19), First edition, December 1980.

Parker, A. 'Correspondence', Parapsychology Review, 1980, 11, 3, 23-26.

Parker, A. 'In defense of introverts: a critical note on supposed personality differences between Ganzfeld experimenters', E.J.P., 1981, 3, 373-379.

Parker, A. 'Letter to the editor', E.J.P., 1982, 4, 403-404.

Sargent, C.L. 'A holistic methodology in psi research',
Parapsychology Review, 1979, 10, 5, 11-14.

Sargent, C.L. 'Exploring psi in the Ganzfeld', Parapsychological
Monographs, No. 17, New York, Parapsychology Foundation, 1980. (b)

Sargent, C.L. 'Correspondence', Parapsychology Review, 1980, 11,
3, 26-28. (b)

Stanford, R.G. 'Comments on 'In defense of extroverts' by Adrian
Parker', E.J.P., 1982, 4, 277-280.

Stevenson, I. and Roll, W.G. 'Criticism in parapsychology: an
informal statement of some guiding principles', J. A.S.P.R., 1966,
60, 347-356.

Toulmin, S.E. 'The uses of argument', Cambridge, Cambridge
University Press, 1958.

Wiegand, H.E. 'Kommunikationskonflikte und Fachsprachengebrauch',
in 'Sprache der Gegenwart, Bd. XLVI. Jahrbuch 1978 des Instituts
für Deutsche Sprache', Dusseldorf, Schwann, 1978, 25-58.

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LETTERS TO THE EDITOR

From Alfonso Martinez-Taboas and Carlos S. Alvarado:

We would like to respond to D. Scott Rogo's criticism of our poltergeist paper published in the May 1982 issue of this journal.

Rogo argues that we did not understand his use of the word 'blind', which simply means that Dr. Schmeidler did not know which of the three sets of data sent to her were those of the adolescent in the family of the case. Contrary to Rogo's impression we did understand clearly the intention of the evaluation. Our point is that a careful reading of this report discloses some pitfalls that severely limits the usefulness of calling it 'blind' or 'unbiased'. For example, Dr. Schmeidler not only knew that the tests were from persons involved in a poltergeist, but also, it is highly probably that she had read about Rogo's ideas of family interaction in poltergeists (Rogo, 1974, 1979). This is precisely the sort of contamination that researchers in clinical psychology attempt to control by keeping the person to measure some variable totally blind as of the task involved, type of study and topic, etc., since contaminating variables known to affect psychological tests can influence the results artifactually.

Other problems with Rogo's studies are: (1) the use of projective tests, highly dubious on reliability and validity (see Martinez-Taboas, 1980), and (2) that Rogo himself administered the tests which may be a serious contaminating variable in the results.

We agree that Rogo's study may be considered important for its pioneering attempt to test for family interaction in poltergeist aetiology. However, we think its importance lies in drawing attention to a concept and the possibility of testing it, but not in the way it was conducted and the conclusion reached from it.

Rogo also claims that we were 'very unfair' to John Palmer's (1974) report. This came to us as a surprise as we described it as "the most serious recent attempt to evaluate this model". Rogo's main point here is that in this case the psychologist was blind. We quote him, "she didn't know she was dealing with a poltergeist victim". But it is Palmer himself who contradicts Rogo's statement when he says that the psychologist "knew that unexplained movement of objects had been associated" with the boy. In his report Palmer tries to diminish the importance of this pitfall arguing that there is no reason to expect that she was particularly interested or had knowledge about poltergeists. Unfortunately, the subjective assurances and reflections that Palmer offers cannot be substitutes for a completely blind evaluation. Again, we think that our criticisms of this report were balanced and far from being "very unfair".

Rogo finishes his letter stating that, in his experience, psychological problems in poltergeist agents "are so blatantly obvious that one doesn't really even need to rely on formal diagnostic tools to demonstrate it". Our answer to this curious statement is simple: parapsychology is a scientific field where this type of 'personalistic' arguments have to be substituted by scientific appraisals. We have argued in our papers that in some cases not only the evidence for psychopathology was nil, but more important, that in practically all of the cases where it was reported to occur we found methodological pitfalls that severely and drastically reduce the value of the conclusions reached by its authors. We have to remember also that this type of 'personalistic' argument has been used repeatedly to defend many false interpretations and data, not only in parapsychology, but in such diverse fields as geology (Gillespie, 1959) and clinical psychology (Jones, 1977), to name a few fields. Since we know that Rogo is totally convinced of the validity of the psychopathological model of poltergeists, it is not far-fetched to think as a likely possibility that his 'observations' and 'experiences' are based on biased and inferential ways of looking at the subject. Again, there is abundant data that supports the notion that if you expect to see pathology you will interpret even presumably 'normal' conduct as consonant with your biases (e.g., Achenbach, 1978; Chapman and Chapman, 1982; Jones, 1977; Langer and Abelson, 1974; McCoy, 1976). We don't think that poltergeist researchers are exempt from this powerfull and well known pitfall.

In our previous papers we have argued for a better methodology in the evaluation of poltergeist agents; perhaps this letter has provided an opportunity for us to make clear some of our previous criticisms. We are glad that in his letter Mr. Rogo strongly supports this point. However, we do not agree with his optimistic evaluation of the above mentioned studies, and consider that a better and more productive choice than the defense of previous studies is to be fully aware of the many pitfalls that can be found in this type of research, and try to circumvent them through the use of more controlled methodology.

References

Achenbach, T.M. 'Psychopathology of childhood: Research problems and issues', *Journal of Consulting and Clinical Psychology*, 1978, 46, 759-776.

Chapman, L.J. and Chapman, J. 'Test results are what you think they are', in Kahneman, D., Slovic, P. and Tversky, A. (eds) 'Judgement under uncertainty: Heuristic and biases', New York, Cambridge University Press, 1982.

Gillespie, C.C. 'Genesis and geology', New York, Harper and Row, 1959.

Jones, R.A. 'Self-fulfilling prophecies: Social, psychological and physiological effects of expectancies', New Jersey, Laurence Erlbaum, 1977.

Langer, E.J. and Abelson, R.P. 'A patient by any other name: Clinician group difference in labelling bias', *Journal of Consulting and Clinical Psychology*, 1974, 42, 4-9.

Martinez-Taboas, A. 'The psychopathological model of poltergeist phenomena: Some criticisms and suggestions', *Parapsychology Review*, 1980, 11, 2, 24-27.

McCoy, S.A. 'Clinical judgements of normal childhood behavior', *Journal of Consulting and Clinical Psychology*, 1976, 44, 710-714.

Palmer, J. 'A case of RSPK involving a ten-year-old boy: The Powhatan poltergeist', *J. A.S.P.R.*, 1974, 68, 1-33.

Rogo, D.S. 'Psychotherapy and the poltergeist', J. S.P.R., 1974, 47, 433-446.

Rogo, D.S. 'The poltergeist experience', London, Penguin Books, 1979.

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PA CONVENTION 1983

The 26th Annual International Convention of the Parapsychological Association will be held from Monday, August 8th, 1983, to Saturday, August 13th, 1983, in Madison, New Jersey, U.S.A., at the Fairleigh Dickinson University. Activities start Monday evening. The convention will consist of reports on theoretical issues and current scientific research throughout the world on all varieties of psi phenomena.

The Program Committee consists of Dr. Richard Broughton, Chairman, Dr. Gertrude Schmeidler and Dr. Michael Thalbourne.

For further information, contact Laura Knipe and Donna McCormick, 1983 P.A. Convention, c/o American Society for Psychical Research, Inc., 5 West 73rd Street, New York, N.Y. 10023, U.S.A..

Editorial consultants: Volume 4, 1981-1983.

The editors of the European Journal of Parapsychology wish to express their appreciation to the following referees for their assistance in evaluating articles submitted to the European Journal of Parapsychology.

Dr. Eberhard Bauer (University of Freiburg, Germany); Drs. H.G. Boerenkamp (University of Utrecht); Dr. R.S. Broughton (F.R.N.M., U.S.A.); Dr. Op t'Hof (University of Amsterdam, The Netherlands); Dr. J.M. Houtkooper (University of Amsterdam, The Netherlands); Prof. Dr. J.A.M. Kooy; Dr. B. Loefqvist (University of Lund, Sweden); Dr. W. von Lucadou (University of Freiburg, Germany); Dr. B. Millar; Dr. J. Palmer (University of Utrecht); W.G. Roll (P.R.F., U.S.A.); G.F. Solfvin.