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(continued inside back cover)

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WHO IS A PARAPSYCHOLOGIST?

Martin Johnson
University of Utrecht

The question is neither a new nor an original one, but the surging public interest and associated fringe groups seem to have made it relevant to pose the question. Is it possible to draw a clear line between the scientificly minded and professionally qualified parapsychologists and a non-qualified person presenting himself as a parapsychologist? Is there any advantage at all in trying to clear the jungle and of trying to help the public and the mass-media to distinguish between the two categories? Will there be any advantages for parapsychology at all in such an endeavour? Should there be a purge led by e.g. the Parapsychological Association (PA) or should we take a more tolerant attitude based on the conviction that the phenomena is ephemeral by its very character.

To be able to define a parapsychologist we may have to start by discussing and trying to define what is meant by parapsychology and parapsychological. Is there fundamental agreement on what is considered reliable data and what is not? That could constitute one line of departure. When we have managed to make those definitions and perhaps obtained a certain degree of concensus, then we may start to compare our notes as regards what we are willing to endorse as representative of our professional organization, the PA.

To the reader it may seem self-evident that a parapsychologist is a person who carries out parapsychological research. The following questions then have to be answered: what is meant by a) parapsychological? b) What is meant by research work? How is the work carried out and communicated? Are professional parapsychologists identical to the small, exclusive group of people who work full-time within the parapsychological field? Then again we have to ask ourselves what is meant by the parapsychological field? Does it include psychotronics, Kirlian photography, studies of 'bioplasma', transpersonal psychology, Backster's plant communication, healing, survival research, astrology, etc.? Further, are those who have obtained full membership of the PA 'the' parapsychologists? Are also those with an associate membership entitled to be considered as

'certified' parapsychologists in spite of the fact that comparatively few of those are engaged in research work? Are these two categories of research workers the only respectable 'parapsychologists'? Must a parapsychologist have a background in psychology? Evidently not, since a few persons who have a very good in-group reputation as very able research-workers have a background in physics and mainly concentrate their investigations on physical aspects of the paranormal.

Is it also a 'must' for a parapsychologist to have passed a higher academic degree? And if so, in which area? There are certainly no rules without exceptions. The requirement of a higher academic degree is falsified by one of the most productive and inventive parapsychologists who duly enjoys such a high in-group esteem that he has served as president of the PA. One should notice that subject-matter knowledge is what counts and an academic career is not the only way which to acquire such knowledge. If, however, we assume that a professional training as psychologist has to be considered as a conditio sine qua non for qualifying as a research worker in the paranormal field (has yet to be strictly defined), then we also have to ask ourselves what kind of background in psychology such a person should have. Should he be an experimental psychologist? Should he be an expert in the personality field? How about being a social psychologist? Perhaps specialization in human factors research (and system analysis) or in witness psychology would pay off; in the latter case not least when it comes to certain types of field-studies? Or should a parapsychologist restrict himself to laboratory work?

In this editorial, I am not going to suggest any possible concrete solutions to the questions I have just raised. The main purpose is one of indicating that we have to face a number of problems and to try to solve them as efficiently and correctly as we can. From what I have already indicated, the problems are not in fact unique and it is tempting to play with the idea that some kind of certification, authorized by the PA should be introduced, but then some valid criteria should have to be established beforehand. When should such a certification take place? At the beginning of one's career, by meeting certain criteria of knowledge and/or academic performance, or later, based on the person's judged accomplishment? At any rate the issue is heavily centred around the establishment of a number of criteria on which a certain degree of concensus must be reached. The main issue at stake seems to me to be: How can we distinguish between science and pseudoscience? Do we have to surrender to an entirely relativistic sociology of science? In that case almost nothing can be usefully done and almost nothing can be considered as misconduct. I agree with Thomas Kuhn that it is important to notice that 'whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discover what it values, what it

tolerates and what it disdains', but on the other hand we don't necessarily have to accept that the situation is as bad and relativistic within science that 'everything goes'. There is clearly a place for the logic of discovery as an instrument for progress in science as emphasized by Sir Karl Popper. There are pseudoscientific 'immunizing tactics' which can at least sometimes be easily distinguished and also there exist rather clear-cut criteria among scientists about the use and abuse of for instance statistics. Even when it comes to the question of interpreting research findings and communicating these findings there are certain rules for which good rationales can be given.

One reason why I decided to bring this matter up is, that as a member of the Membership Application Committee of the PA, I have been confronted with the problem of giving approval to, or rejecting an application. A definite improvement in the procedure has taken place during 1977: to avoid halo-effect, every member of the committee now receives a photocopy of the application and has to make his own decision independently of the other Committee members.

The whole problem brought up in this editorial is well recognized by the council of the PA. Committees have recently been established, one committee to write a position paper which provides guidelines for education in parapsychology; another committee to work out a position paper concerning professional and ethical standards with respect to the practical application, related to ostensible psi-phenomena. I certainly think that the PA has an important task to fulfill over a wide range of issues upon which I have hinted. With the expertise the PA undeniably has or can establish, the organization also has a responsibility to offer guidelines on definitions, methodological and educational standards and requirements within the field. But to be able to give definitions we have to face one of the real problems, namely that we still have to confess that even among the in-group members of the organization we are lacking a clear-cut concensus of what should be considered as paranormal and what should not. The evaluated results from a questionnaire which I recently issued to a number of leading parapsychologists underlines this fact.

A PK EXPERIMENT WITH A SINGLE BALL ROLLING ON A DECLINE

Haakon Forwald

The author presented in 1964 to the Seventh Annual Convention of the Parapsychological Association a paper with the title "A method for investigating PK without recourse to 'Statistical Proof'." It is shown in the paper that - in an experiment with an oak ball rolling on a slightly declined surface - the ball could be influenced by will or desire to roll to the side of the surface which was designated as the target side.

As doubts have been expressed about the correctness of this finding the author recently decided to try a further experiment with the same arrangement as previously used, and with identically the same oak ball. The experiment was carried out in March and April, 1977.

I chose to conduct a series of ball releases with the two surface sides A and B alternating as targets and to record the ball hits on barriers which were provided with a centimeter scale. The test unit was the run comprising five successive releases of the single ball with A as target, immediately followed by five releases with B as target. The run thus consisted of ten releases with the target order AAAAA BBBBB.

This order was maintained in the first three series of the experiment. For the last series (No. 4) the order was changed to BBBBB AAAAA. This inversion of the order was due to experiences in the first three series, in connection with the following argumentation.

In my previous cube experiments the Y-scale had its zero on the A-side. When A was the target the subject then wished for low values of Y. This would mean that he "pulled" the cubes toward himself, because he was regularly located on the A-side. In the ball arrangement, however, the scaling is opposite. A "pulling" effect corresponds to high scale values. These circumstances may suggest that I have a psychic preference to score better on low scale values than on high ones. As I have always got outstanding results in the first trial of the experimental unit it was natural to let the first trial in the ball experiments have the B-side (low scale values) as

the target, that is to say, the most promising target order should be BBBBB AAAAA. The outstanding results which were obtained in Series 4 seem to justify the above argumentation.

In my previous cube experiments the standard series comprised 300 cube falls (trials) and it was therefore practical to let the new ball series comprise 300 ball releases, which means 30 runs per series.

The experimental arrangement is shown in figure 1. It consists of a heavy writing desk, having an oak surface of 180 x 90 cms. provided with a copper plate of 120 x 90 cms., by one millimeter thickness, on which the ball rolls when released by an electromagnet controlled by a push button. The copper plate is very smooth and it looks very much like a mirror. The desk itself is placed on a concrete floor resting on a solid foundation. It is seen in the figure that the surface has a decline of about one in hundred, mainly in the direction from the A-side to the B-side, which involves that the extreme B-side is located about one centimeter lower than the extreme A-side. This has been accomplished by placing suitable distance pieces under the desk legs on the A-side. The decline ensures that the ball is kept in a continuous rolling state from the moment when it is released until it hits one of the barriers.

The decline and its direction have been checked by the release of a 5/8" ball bearing steel ball from many different points on the copper surface, taking its running time over a certain distance with a stop watch.

Considering the rotational inertia of the ball and its (very small) rolling friction, the decline could be calculated. It was found that the decline value stated above was fairly constant over the whole surface.

The release device is designed to give the ball a small but constant starting velocity on the surface. In its rest position the ball is held sideways between two pins with rounded ends, one of which is fixed, and the other one is attached to the anchor of the magnet. As soon as the ball is released by the magnet it falls over an edge and hits the copper surface two centimeters below the edge. According to marks on the surface it makes one jump of 0.6 cms. (horizontally) before it starts the rolling travel, which proceeds calmly and with a constant, or somewhat accelerated velocity until the ball hits the barrier. The velocity is so small that there is no difficulty to read the hit point on the barrier scale to the nearest centimeter. The time of movement (as taken with a stop watch in two "control" series, each of 50 releases) varies between 3 and 7.5 seconds. The corresponding barrier hit points are at about 30 and 180 cms.

The oak ball has a diameter of 18 millimeters and its weight is 2

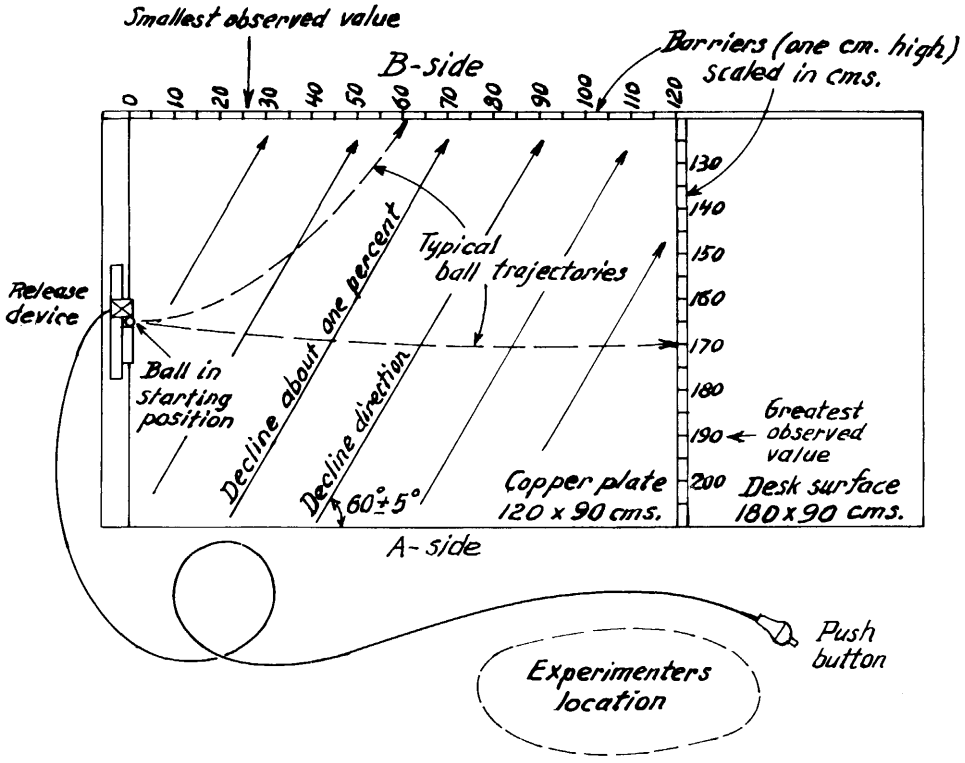

Smallest observed value
B-side
Barriers (one cm. high) scaled in cms.
0
10
20
30
40
50
60
70
80
90
100
110
120
130
140
150
160
170
180
190
200
Release device
Ball in starting position
Decline about one percent
Decline direction
Typical ball trajectories
Greatest observed value
Copper plate 120 x 90 cms.
Desk surface 180 x 90 cms.
60° ± 5°
A-side
Push button
Experimenters location

FIGURE 1

grams. Its surface is very smooth.

In total four series, each with 300 releases, were conducted in this experiment. When starting the work I was far from being sure that I could get any results, because I had not tried any PK experiments for the last 12 years or so. The results in the two first series were not promising. But the third series was definitely a success as conderns the results in the first position in the 5-release unit. The total results in the three series were also good, so I decided to carry out a confirmatory series (No. 4). This last series gave outstanding results which will be dealt with more at length in the sequel.

Table 1 and table 2 contain the evaluated data from an analysis of variance of the differences between averages with A and B

TABLE 1

Results in total series (each series of 300 trials)

Series	Averages of scores (cms.) Target A	Target B	A-B diff. (cms.)	Estimated var.diff. ($cm.^2$)	t_d	2P
1	104.03	96.83	7.20	2009	1.39	0.16
2	99.44	92.05	7.39	1958	1.45	0.15
3	107.51	100.43	7.08	1996	1.37	0.17
1+2+3	103.66	96.44	7.22	1990	2.43	0.015
4	104.50	85.49	19.01	1840	3.84	0.00013
1+2+3+4	103.87	93.70	10.17	1961	3.98	0.00008

as targets. Table 1 considers the total series (each of 300 trials) and table 2 the first position in the 5-release unit (each of 60 trials).

In the author's placement series with cubes the results in the first position have regularly been outstanding. The same characteristic has now been observed also in the ball experiments. The effect is predominant in Series 4 as is seen in table 3 which contains the evaluated data for each of the five release positions.

From the graph in figure 2 is also seen that parabolas can be fitted to the observed score values in Series 4, both for the averages with the A-side as target and for the averages with the

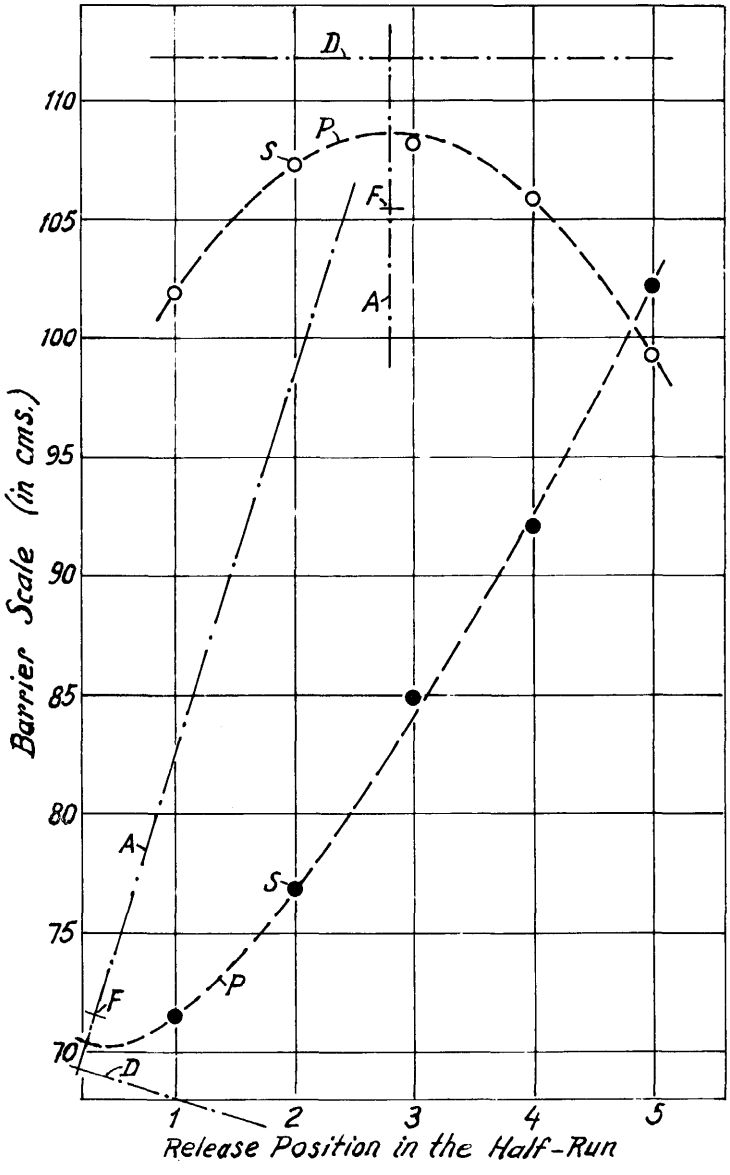

D
P
S
F
A
110
105
100
95
90
85
80
75
70
Barrier Scale (in cms.)
1
2
3
4
5
Release Position in the Half-Run

FIGURE 2

TABLE 2

Results in position 1 of series (each position of 60 trials)

Series	Averages of scores (cms.) Target A	Target B	A-B diff. (cms.)	Estimated var.diff. ($cm.^2$)	t_d	2P
1	104.67	82.93	21.73	1950	1.91	0.055
2	107.77	89.40	18.37	2315	1.48	0.135
3	123.17	93.17	30.00	1697	2.82	0.005
1+2+3	111.87	88.50	23.37	1985	3.52	0.00043
4	101.90	71.53	30.37	1062	3.61	0.00031
1+2+3+4	109.38	84.26	25.12	1780	4.61	0.0000045

B-side as target.

The correlations between the observed averages in Series 4 and the corresponding points of a parabola have been investigated. With the A-side as target the calculated data are: Correlation coefficient $r = 0.999091$, $t = 23.436$ (1 df), $2P = 0.0275$. The corresponding data with the B-side as target are: $r = 0.999151$, $t = 24.248$ (1 df), $2P = 0.0265$.

The 5 releases with A as target are independent of the 5 releases

TABLE 3

Position data in Series 4

Position	Averages of scores (cms.) Target A	Target B	A-B diff. (cms.)	Estimated var.diff. ($cm.^2$)	t_d	2P
1	101.90	71.53	30.37	1062	3.61	0.00031
2	107.23	76.80	30.43	1551	2.99	0.0027
3	108.16	84.87	23.30	2004	2.01	0.043
4	105.90	92.03	13.87	2360	1.10	0.28
5	99.30	102.20	- 2.90	2139	-0.24	0.85

with B as target, so the probabilities can be combined by Fisher's method, which gives a chi-square of $\chi^2 = 14.44836$ (4 df), 2P = 0.007. This figure suggests that the scores in the 5-release unit have a tendency to be grouped in accordance with a parabolic distribution. The finding presents a confirmation of earlier observations in psi position data, for instance in the cube experiments of the author (Forwald, 1952a, 1952b, 1972).

In figure 3 the A-B differences in the five positions are shown graphically and compared with the parabola. The correlation data are in this case: r = 0.999297, t = 26.659 (1 df), 2P = 0.024.

When conducting the experiment I had no idea about the progressing development of the parabolic position effect. I discovered the effect only after termination of Series 4, when I summed up the results in each of the five release positions. On the other hand, I could not avoid to observe the strong development of positive results in the total data in Series 4. This development seems to me - at least to some extent - to be due to the following circumstances.

I had observed already in the first three series that it could be of disadvantage to decide in advance the number of runs to be done during one and the same experimental occasion. I had for instance on three occasions specified to conduct 10 runs in succession. In two of these cases I got positive averages of about 11 and 16 cms., but in the third case a negative average of more than 17 cms. In Series 4 therefore I did not specify in advance the number of runs to be done on the occasion, but let the experiment proceed as long as the results were good. In this connection it is necessary to mention two conditions which were strictly attended to in Series 4:

1. Once begun, the run was always continued and concluded.
2. No recorded run was omitted from the investigation.

In psychological respect differences existed between the first three series and the last one. In Series 4 I kept my state of mind under stronger observation than previously before I decided to conduct a run. Already in the first three series I had experienced that I scored better when I was in a good mood. If being in a bad mood there could be heavy drawbacks if I got tempted to continue with the experiment.

A good mood can be attained in different ways, dependent on individual properties. I like good music and on one occasion I got very good results with the ball after having played Mahler's first symphony on my stereo set. I tried on another occasion to repeat this with another piece of music. But then the thing would not work. I recognized then that in the first case the intention to play music had come on spontaneously, without my reasoning about whether or not it would be useful for obtaining PK results. In the second

case, however, I would consciously make use of the music for the purpose of getting good PK results. This latter situation means a mental focusing on the object which is investigated, and it is well known in parapsychology that such a focusing may lead to psi-missing.

A further point in these two cases should be observed. In the first case real feelings were involved, but in the second case the intellect was at work.

It may be mentioned that Series 4 was carried out in 8 successive days. The total number of experimental days for the first three series was 15.

The two "control" series (each with 50 releases) were conducted at fast speed, without concentration on any target. An interesting point is that the averages in these series are comparatively great (111.53 cms. in the first control series and 108.71 cms. in the second series, with a total average of 110.12 cms.). The greatest average value arrived at for any series in table 1 is 107.51 cms., although conscious attempts were made in Series 1 to 4 to get high averages when A was the target.

Considering this report as a whole it may be concluded that a rolling oak ball can be influenced by PK to a similar degree as a wooden cube, and it is therefore doubtful if PK can be apprehended solely as a quantum-mechanical phenomenon.

SOME FURTHER INFORMATION ABOUT THE RELEASE DEVICE AND THE EXPERIMENTS WITH THE BALL

As shown in figure 4 the ball is held in its starting position by two edges and two pins. Both edges and one pin are fixed in relation to the desk. The second pin is fixed to the lever of the magnet, which holds the ball in position by means of a spring (not shown in the figure). For bringing the ball in position it is pushed between the fixed pin and the pin on the lever so that it comes into contact also with the two edges. In that way a fixed location of the ball relative to the desk is obtained.

When the magnet is energized the lever pin is momentarily withdrawn from the ball, which then drops down to the copper plate and begins its rolling movement.

In spite of the described fixed starting conditions of the ball its trajectories have turned out to be very different from one release to the other. This was experienced in the two control series, in which the barrier hit points varied between 31 and 184 cms. In each series 50 percent of the hits are below 120 cms. and 50 percent above this value. A significant observation is that the hit

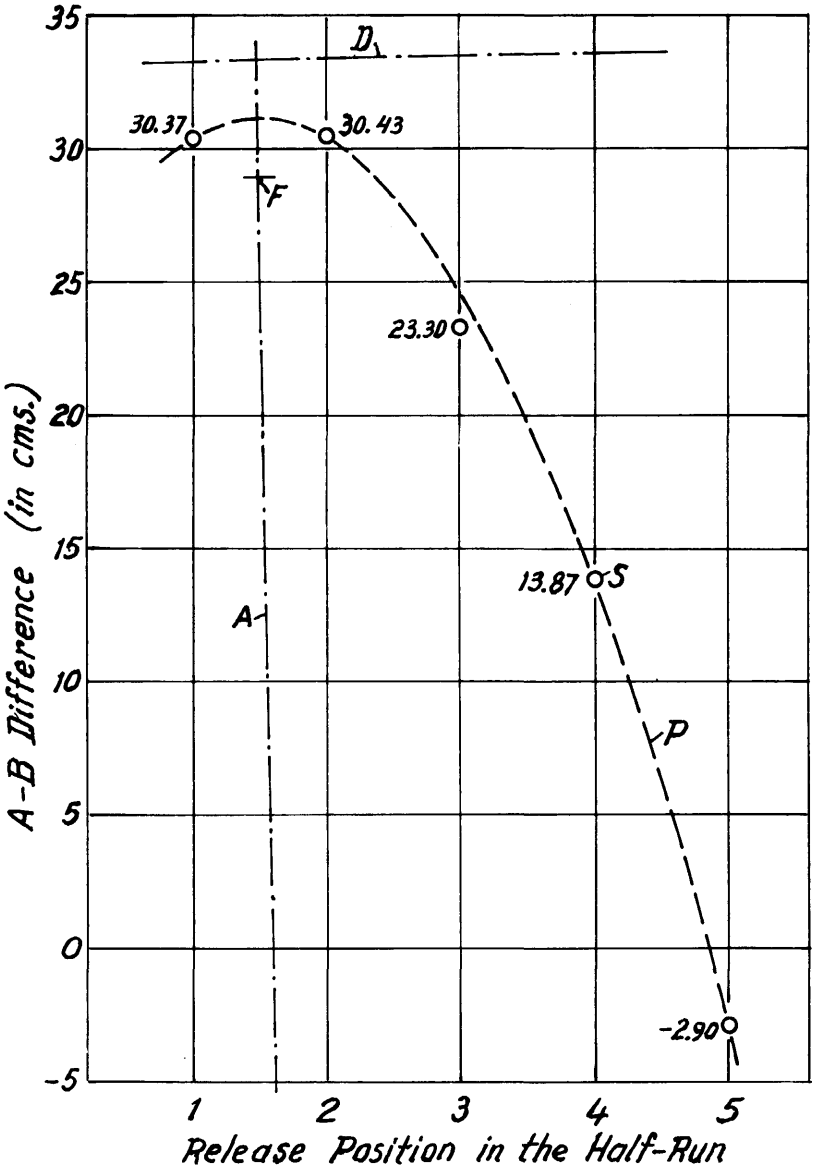

35
30
25
20
15
10
5
0
-5
D
30.37
30.43
F
23.30
13.87
S
A
P
-2.90
1
2
3
4
5
A-B Difference (in cms.)
Release Position in the Half-Run

FIGURE 3

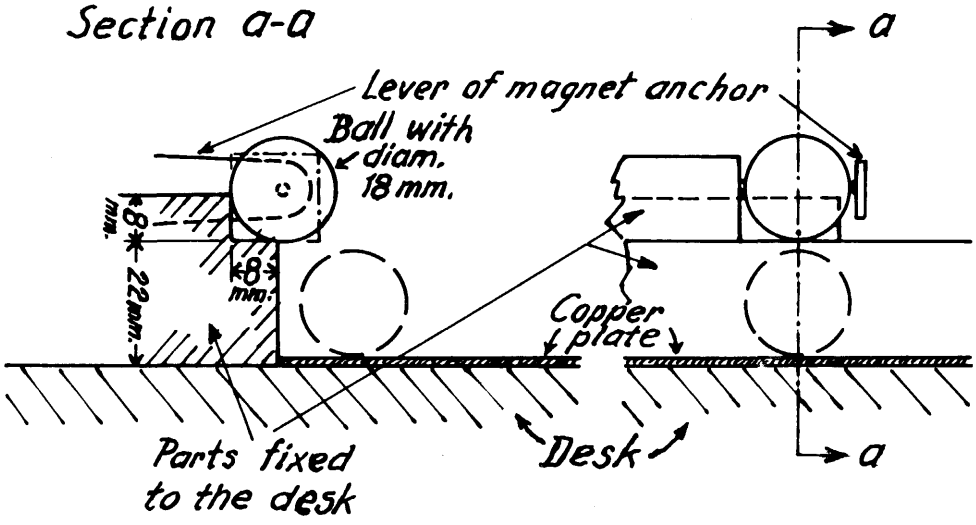

Section a-a
a
Lever of magnet anchor
Ball with
diam.
18 mm.
8 mm.
22 mm.
8
mm.
Copper
plate
Parts fixed
to the desk
Desk
a

FIGURE 4

points are evenly distributed within each of these two ranges.

One may ask: "Where does this uncertainty (in the trajectories) come from?" After some new investigations I am now rather sure that the uncertainty is caused by the ball, in connection with the circumstance that it has always been placed to start at random, i.e. in an arbitrary angular position. I just carried out a longer series of releases in which the ball was each time placed in the same angular position (this was possible by observing certain marks on its surface), but now the trajectories had a constant character, and the ball went each time to the same place on the scale. Thus it must be the ball which is biased, and not the release device. The fact that the hit points in the control series are very evenly distributed, gives bearing to the opinion that there is a high degree of randomness in the act of placing the ball to start. Therefore there is reason to assume that the same high randomness has occurred also in the "willed" series. The results in Series 4 consequently appear to have been caused by some sort of action on the ball during its rolling travel on the copper plane - viz. PK forces.

I think that we can neglect a hypothesis involving that I could each time unconsciously have placed the ball in such a position that the results in Series 4 were arrived at.

The hypothesis of the possible influence of air streams likewise seems questionable, e.g. due to the observed significant position effects.

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A COMMENT ON SCHMIDT'S MATHEMATICAL MODEL OF PSI

J.M. Houtkooper

In his article in the October, 1975 issue of the J.A.S.P.R. H. Schmidt formulates a mathematical model of the parapsychological phenomena (Schmidt, 1975). The relationship between psychokinesis, clairvoyance and precognition that follows from his formulation is in my opinion most interesting and fruitful for further theoretical developments.

Here, I would like to comment on the problem of the influence later observers of the results have on the outcome of an experiment. This 'Divergence Problem' is a serious consequence of the model that can be demonstrated by an extension of the mathematical treatment. More importantly, the divergence problem should be discussed without immediate recourse to an easy hypothesized solution. The divergence problem occurs because of the assumed space-time independence of psi, which is essential when explaining so-called causality violating effects. Therefore, the problem is inherent in all theories that assume psi effects to be brought about by the act of observation, independent of space-time relationships. We will call such theories observational theories. E.g. the theoretical approach by E.H. Walker (Walker, 1974) is to the same extent affected by the divergence problem as is Schmidt's formulation.

In his example of a coin-flipping experiment, Schmidt shows the expected probability of a hit ($\bar{P}_a$ or $E(P_a)$) in an experiment to be:

$$\bar{P}_a = \int_{-\infty}^{\infty} dS \; \frac{e^{-S^2/2\sigma^2}}{\sigma\sqrt{2\pi}} \cdot \frac{\theta_1 e^{S}}{1+\theta_1 e^{S}} \qquad \text{(his formula A7)}$$

He uses his formula to show that when σ (the standard deviation of the resultant psi strength S of all later observers) goes to infinity, $\bar{P}_a$ goes to $\frac{1}{2}$. (So the expected probability of a miss, $\bar{P}_b$, also goes to $\frac{1}{2}$). This means that the influence of the subject on the outcome of the experiment disappears. However, this apparent convergence to the value predicted by the null-hypothesis disappears

when we calculate the expected value of $\bar{P}_a/\bar{P}_b$. We obtain:

$$E(P_a/P_b) = \int_{-\infty}^{\infty} dS \, \frac{e^{-S^2/2\sigma^2}}{\sigma\sqrt{2\pi}} \cdot \theta_1 e^S = \theta_1 e^{\sigma^2/2} \qquad (1)$$

$E(P_a/P_b)$ goes to infinity when σ goes to infinity, while $\bar{P}_a/\bar{P}_b$ goes to 1. This unexpected result is clarified when we calculate the variance of P_a/P_b:

$$\mathrm{var}(P_a/P_b) = \theta_1^2 e^{\sigma^2}(e^{\sigma^2}-1) \qquad (2)$$

So this variance goes to infinity as well, when σ goes to infinity. To interpret these results in the case of a real psi experiment with a large influence of later observers is not so easy. To do this, we use a uniform probability density function (p.d.f.) of S, instead of the Gaussian p.d.f. used by Schmidt:

$$f(S) = \frac{1}{2C} \quad , \quad -C < S < C \quad ;$$

$$f(S) = 0 \quad , \quad \text{elsewhere} \qquad (3)$$

Here the half-width C plays the role of σ; the limiting cases $C \to 0$ and $C \to \infty$ are equivalent with the cases $\sigma \to 0$ and $\sigma \to \infty$. We can now calculate the expectation and variance of the probability of a hit as:

$$\bar{P}_a = \int_{-\infty}^{\infty} dS \, \frac{1}{2C} \cdot \frac{\theta_1 e^S}{1+\theta_1 e^S} = \tfrac{1}{2} + \frac{1}{2C} \ln\left\{\frac{\theta_1 e^C + 1}{\theta_1 + e^C}\right\} \qquad (4)$$

$$\mathrm{Var}(P_a) = E(P_a^2) - (E(P_a))^2 =$$

$$\frac{1}{2C}\left|\ln\left\{\frac{1+\theta_1 e^C}{1+\theta_1 e^{-C}}\right\} + \frac{1}{1+\theta_1 e^C} - \frac{1}{1+\theta_1 e^{-C}}\right| - \frac{1}{4C^2}\ln^2\left\{\frac{1+\theta_1 e^C}{1+\theta_1 e^{-C}}\right\} \qquad (5)$$

From (4): $$\lim_{C\to 0} \overline{P}_a = \frac{\theta_1}{\theta_1+1} \qquad (6)$$

and: $$\lim_{C\to \infty} \overline{P}_a = \tfrac{1}{2} \qquad (7)$$

These results are in accordance with Schmidt's formulae A8 and A9 respectively. We now calculate the limiting cases for the variance of P_a, from (5):

$$\lim_{C\to 0} (\mathrm{Var}(P_a)) = 0 \qquad (8)$$

and: $$\lim_{C\to \infty} (\mathrm{Var}(P_a)) = \tfrac{1}{4} \qquad (9)$$

The result (9) means that the standard deviation of P_a is $\frac{1}{2}$ in our experiment when the influence by later observers becomes infinitely large. This, combined with (7) forces the conclusion that in this limiting case P_a takes only the values of 0 and 1. That is, such experiments consist of only hits or only misses, while it cannot be predicted which will occur! This can be considered as the maximum degree of unpredictability that can exist for an experiment. The limiting case, C goes to infinity, is by no means unrealistic, as it can be approached by increasing the number of later observers, no matter how small their individual psi strengths are (but not zero).

Schmidt mentions a solution for this problem, by the assumption that only in a finite time interval after the experiment, influences are exerted by later observers. (Such in contradiction with the space-time independence of the basic Psi Axiom.) This easy solution has to be taken as just a soothing remark, without any ground, I am afraid. However, it seems to be testable: if you keep the results of your experiment secret longer than this critical time, the reproducibility should be better because later observers cannot influence the results. (The size of the critical time interval should fall within practical limits of say one hour to a few years,

in order to be demonstrable in a feasible experiment.) It seems that the space-time independence of the model is the source of the problem: Because the experimenter has no control over the influence of later observations, he has no control over an essential experimental condition and thus the results will be irreproducible.

As I see it, the divergence problem leads to a paradoxical situation: On the one hand, it predicts extremely irreproducible results and no experimenter would view this as at variance with the factual situation in parapsychological research (with a few exceptions, of course). On the other hand, this same prediction poses a serious methodological problem: As the outcome of an experiment would be completely unpredictable, observational theories are not falsifiable and therefore need modification. Whether the critical time interval proposed by Schmidt will be a fruitful modification cannot be told at the moment, but something should be done at this point to make observational theories viable.

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A BOUND FOR THE OBSERVATIONAL THEORIES OF PSI

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Several observational theories of psi are currently receiving considerable attention from serious students of parapsychology. Walker (1974) offered the first and still the most comprehensive theory of this type known to the author. He postulates a specific quantum mechanical formalism wherein acts of observation serve to determine in which of its possible states a system is actually found. Schmidt (1975) has achieved a highly compact mathematical representation by foregoing any explicit reference to the mechanisms involved. He postulates only that acts of observation modify probabilities according to a simple axiom. Although Schmidt himself may conceive of the fundamental mechanisms as being of a quantum mechanical nature, other authors have explicitly renounced this assumption. Mattuck (1976) presents a theory in which an observer acts to alter probabilities by organizing random fluctuations on a larger scale (e.g. thermal, traffic and muscular noise). The resulting effects are thus constrained by energy conservation but achieve a local reduction in entropy through the input of information from the observer. Donald and Martin (1976) conceive the observer's role as utilizing the order associated with an organized state in the future to bring about effects in the present. They posit this as the time-reversed analogue of normal behavior in which the order of the past is used in the present.

Despite their important differences these theories share the notion that acts of observation are crucial to the working of psi. Thus they also share the general deduction that the role of the experimenter is of extreme importance, as it is he who observes the overall results of an investigation and often even studies in detail the performance of every subject. Credence is lent to this view by the existence of experimenters with a remarkable record for publishing results showing evidence of psi (Kennedy and Taddonio, 1976; White, 1976). This argument seems particularly persuasive in studies which find overall significance even though no individual subject scores strongly. Thus one may be led to the conclusion that

observation of the results by the experimenter is an important effect even though each subject may have observed his own results previously.

The observational theories of Walker and Schmidt are formulated so as to be independent of distance and time. Recent experimental data directly suggesting the existence of retro-PK (Schmidt, 1976) support a model independent of spatial or temporal limitations. Much earlier work with GESP and precognition experiments also points to the same conclusion. This assumption has allowed Schmidt to give a convincing demonstration of the equivalence of PK, precognition and clairvoyance. The resulting unitary model is an important triumph for the observational theories and undoubtedly accounts for much of their current popularity.

The greatest difficulty with these theories also arises directly from the assumed independence of an observer's effects from the constraints of distance and time. For, if it is granted that the experimenter can affect the results by observing them after the subject, then what of all future observers? Unless their effects are somehow bounded, one cannot possibly control the essential experimental variables. A well-defined and reproducible experiment would therefore be impossible.

Schmidt (1975) has considered the effects of a large number of observers upon an experimental outcome. In particular, he assumed that the effects from observers were small and normally distributed about a mean of zero. He then deduced that when the number of observers grew without bound, the expected probability of a given outcome tended to $\frac{1}{2}$ regardless of its a priori value. Schmidt used this deduction to argue that the effects of later observers would tend to 'wash out' a PK effect exerted by a first observer; however he failed to point out that the $p = \frac{1}{2}$ limit corresponds to chance expectation only for the binary case considered. This result is clearly inconsistent with empirical evidence. Houtkooper (1977) has recently extended the Schmidt analysis by adopting the more tractable (though less realistic) assumption of a uniform distribution of observer effects. He finds that the variance of the probability of a given outcome tends to $\frac{1}{4}$ in the same limit. In other words experiments must result in either all hits or all misses, although we cannot say in advance which will occur. Thus the unbounded model not only predicts that a reproducible experiment is impossible, it also predicts that experiments with a very large number of later observers must be grossly unpredictable.

Now it is the business of scientific theories to make falsifiable predictions about experimental results. The observer theories meet this criterion of science by yielding deductions that are patently false in the light of the data at hand. Experiments do not yield all

hits or all misses. Indeed, parapsychological experiments rarely depart significantly from mean chance expectation even when the choice is not a symmetrical binary one. Consequently one must abandon these theories or else modify them so as to preclude catastrophic failure.

It is clear that any successful modification of the observational theories must remove their unbounded dependence upon future observers. It is possible to accomplish this by making some additional assumptions. One may, for instance, escape the specific mathematical difficulties outlined above by arguing that only a finite number of observers need be considered for a single experiment, and by emphasizing that their individual effects are apt to be very weak. Such assumptions treat the dilemma superficially, however, and leave the fundamental problems of space and time-independent models unchallenged.

Schmidt (1975) has suggested that systems might be insensitive to observations occurring after the passage of some finite time. This assumption by itself does not produce a bounded theory, however. (This is plain when one considers broadcasting results to a very large number of observers immediately after an experiment.) A time constraint is also difficult to reconcile with some experimental data, and it has distasteful theoretical implications. To remain internally consistent, it seems important for the observational theories to rely upon a bounding mechanism that arises from acts of observation themselves.

It is conceivable that a satisfactory solution to the bounding problem could be phrased in terms uniquely appropriate to the observers. One might define a psychological distance whose magnitude governs the strength of an observer's effect upon a given outcome. It is anticipated that this distance would often correlate with time or the number of previous observations, but this would not always be necessary. One difficulty would appear to lie in defining the psychological variable in a manner that permitted an independent measure of its magnitude.

Another way to meet these objectives is to suppose that a system is rendered less susceptible to further influence by each successive observation. If this should be the case, then it would be possible for an experimenter to have a non-negigible effect (as a second observer, say), yet for even an infinite number of subsequent observations to converge to only a small additional effect. This line of thinking is developed here, and some of its ramifications are discussed.

MATHEMATICAL DEVELOPMENT

It happens that the Schmidt formalism can be readily adepted to this

idea. We first institute a simplified notation and set

$$r = p/q. \tag{1}$$

The fundamental relation proposed by Schmidt (1975) is that an observer acts as a psi source of strength θ so as to modify this ratio of the probability of a hit to the probability of a miss according to the equation

$$r' = \theta r. \tag{2}$$

It is of interest to compute the change in this ratio caused by the observation. That is, one forms the difference

$$\Delta r = r' - r = (\theta - 1)r. \tag{3}$$

It is here that the decreased sensitivity of the system to successive observations will be introduced. It is proposed that Equation (3) be modified so the ratio is changed according to the inverse square of the observation number. That is, if the nth observation be made by a psi source of strength θ_n, then

$$\Delta r = \frac{(\theta_n - 1)}{n^2} r. \tag{4}$$

For brevity θ_n is broken into a sum of its active and inactive parts by setting

$$\theta_n = \psi_n + 1, \tag{5}$$

so that on the nth observation a simple relation is obtained:

$$\Delta r = \frac{\psi_n}{n^2} r. \tag{6}$$

The new ratio is found by adding the change to the previous value, so that

$$r' = \Delta r + r = (1 + \frac{\psi_n}{n^2})r. \tag{7}$$

In the case of N successive observations on a system of a priori ratio r_o, the cumulative effect is computed to be

$$r' = r_o(1 + \frac{\psi_1}{1^2})(1 + \frac{\psi_2}{2^2}) \ldots (1 + \frac{\psi_N}{N^2})$$

$$= r_o \prod_{n=1}^{N}(1 + \frac{\psi_n}{n^2}). \qquad (8)$$

As a limiting condition in the presence of only weak sources, this reduces to a model of additive effects with

$$r' = r_0(1 + \sum_{n=1}^{N} \frac{\psi_n}{n^2}), \quad \frac{\psi_n}{n^2} << 1. \qquad (9)$$

A particular weak source would therefore make a bounded contribution even for an unbounded number of observations. The absolute convergence of the function $\Sigma\, 1/n^2$ is an important reason for choosing this particular dependence.

To show that this theory is free from the divergences that plague the unbound theories, it is necessary to calculate the probable effects of random independent observations. It will be assumed that the ψ_n are distributed about a mean of zero so that

$$E\{\psi_n\} = 0. \qquad (10)$$

It is further assumed that they are independent and of variance σ^2 so that

$$E\{\psi_n \psi_m\} = \begin{cases} \sigma^2, & m = n \\ \sigma^2, & m \neq n \end{cases} \qquad (11)$$

The expected value can then be computed from Equation (8):

$$E\{r'\} = r_o \prod_{n=1}^{N} E\{1 + \frac{\psi_n}{n^2}\} = r_o. \qquad (12)$$

Thus random observation by a balanced population of psi hitters and psi missers does not alter the expected result. This would seem to be an important property for any serious theory of psi. It follows here directly from the assumption embodied in Equation (10).

The variance in r' can also be calculated without the need for specific distributional assumptions.

$$\text{Var}\{r'\} = E\{r'^2\} - r_o^2$$

$$= E\{r_o^2 \prod_{n=1}^{N}(1 + \frac{\psi_n}{n^2})^2 - r_o^2$$

$$= r_o^2 \prod_{n=1}^{N} E\{1 + \frac{2\psi_n}{n^2} + \frac{\psi_n^2}{n^4}\} - r_o^2$$

$$= r_o^2 \left| \prod_{n=1}^{N}(1 + \frac{\sigma^2}{n^4}) - 1 \right|. \qquad (13)$$

A well-behaved result has been achieved owing to the rapid convergence of the bounding function. A finite variance in the distribution of psi strengths leads to a finite variance in experimental behavior even when the number of observations tends to infinity. This invites certain deductions about experimental outcomes that were not possible with previous theories.

APPLICATION TO EXPERIMENTS

It is helpful to restrict the theory to a reasonable range of numeric values. To estimate the appropriate range one need only consider the rare observer who scores at the rate of 1 in 4 on an ESP task with mean chance expectation 1/5. Equation (2) provides a value

$$\theta = \frac{r'}{r} = 5/4,$$

so that from Equation (5) one finds

$$\psi = 0.2.$$

It is now argued that this value is sufficiently rare in the population as to represent a departure from the mean of at least two standard deviations. It then follows that

$$\sigma < 0.1$$
$$\sigma^2 < 0.01. \qquad (14)$$

Restricting σ^2 to values much less than 1 allows an accurate approximation of Equation (13) for application to experiments:

$$\text{Var}\{r'\} = r_o^2(1 + \sum_{n=1}^{N} \sigma^2/n^4 - 1)$$

$$= r_o^2\sigma^2 \sum_{n=1}^{N} 1/n^4. \qquad (15)$$

This variance is almost entirely contributed by the assumed random choice of the first observer. The subsequent terms sum to less than an additional ten percent even for infinite N.

An experiment to which these calculations would apply is obtained when randomly chosen persons flip an unbiased coin, record the result, and show it to many other randomly chosen observers before

passing the information on to the experimenter. The statistics describing this situation are calculated from Equation (12)

$$E\{r'\} = r_o = p_o/q_o = 1, \tag{16}$$

and from Equation (15)

$$\text{Var}\{r'\} = (1)^2\sigma^2(1.09) \sim \sigma^2 \tag{17}$$

This variance would, of course, be added to that arising from other sources. Consequently an experiment to check this prediction should be designed with sufficient sensitivity to reveal the effects of a reasonable value of σ^2. A carefully conducted mass screening experiment of the type attempted by Palmer et. al.(1976) would provide a direct way to estimate σ^2 in advance.

Variance effects should be greatly reduced when the first observer is fixed. In this instance, however, Equation (8) may be used to calculate experimental probabilities as altered by the fixed observer. (Here it is assumed that fixing the observer fixes the associated psi strength. More refined ways of associating a psi strength with an observer can undoubtedly be proposed.) If later observations influence the results randomly, then expressions analogous to Equations (12) and (15) can be readily derived. Again it is found that the mean is unaffected by a balanced population, and the associated variance is notably smaller owing to the absence of the first term in the summation. This may be considered a somewhat optimistic prediction for experimental research. It asserts that the performance of a strong psi source is not weakened by subsequent publication of the data to a randomly chosen group of observers. The case of repeated observations by the same psi source can be obtained from Equation (9). A justification for the approximation used there has been given in deriving Equation (14). It is noted that the sum of the inverse squares converges to something less than 1.7. Consequently repeated observations by a single source are expected to augment by roughly seventy percent the results achieved with only a single observation.

Schmidt (1976) has reported an experiment in which some of the results were observed by subjects four times. He obtained significantly increased scoring, but was unable to directly test his hypothesis that the PK effect would be four times that found on the singly observed trials. A sufficiently long and significant experiment should be able to distinguish between predictions based on the two models, however.

The ability of an experimenter to influence the scoring of subjects who witness their own results previously can also be

computed from Equation (8). In this instance, one fixes the strength of the second psi source and allows the first to vary, perhaps randomly, with each subject. The theory then gives predictions similar to those for a fixed first observer. The system's susceptibility to influence by the experimenter is only ¼ of that for the initial observer. Because the experimenter may observe many more trials than any single first observer, he must be considered a likely source of significant effects in the overall analysis. When subjects are randomly selected, the between-subjects variance in r' should again approximate that given in Equation (15) while the within-subjects variance should be notably smaller.

DISCUSSION

One possible way of bounding the observational theories has been developed here. Some ramifications deserve comment. There are also some difficulties with the present approach which should be plainly stated.

Undoubtedly a host of different formulae can be proposed to replace the inverse-square dependence posited here. It is to be hoped that experimental techniques will eventually allow one to choose amongst them. For the time being it appears that any formulation leading to simple, specific and convergent results must be welcomed.

A more difficult problem with the current theory is posed by the very notion of successive observations. A kind of implicit time dependence has been introduced here by serializing the observations. It remains to be shown, for instance, that an unambiguous generalization can be given to this notion when one considers observers separated by relativistic transformations. Unless a satisfactory definition (e.g. time-symmetric and relativistically invariant) can be found, this theory must eventually suffer the same criticisms applied to those formulations which rely upon "time's arrow" explicitly.

Serializing the observations as has been done here, also calls into focus the difficulties associated with defining what is meant by an "observation". It is not clear what specific acts, attitudes or contingencies are necessary for it to be said that an observation has occurred. The need for removing such ambiguities is not so pressing in theories that treat all observations equally. Here, however, in order to predict the effect of a particular observation, it is necessary to specify the number of predecessors. Consequently one must be clear about what constitutes a prior observation. The theory rewards this clarity by providing an experimental check upon

its validity: If the strength of a psi source is known independently, then the number of prior observations upon a system can be inferred by measuring the effect of this source upon the same system.

Finally, it should be noted that this theory could be refuted by several different kinds of experiments. Consequently it may contain reasons for its immediate rejection. In the author's view its most interesting prediction concerns variances in the ratio of hits to misses when psi sources are randomly chosen. If the theory be considered otherwise satisfactory, then this result allows an experimental check for the existence of psi as modelled in terms of non-zero sources. The theory assumes that such sources are distributed about a mean of zero. If it could be determined that their variance was also zero, this would be tantamount to a rejection of the notion that such effects exist. While it might be impossible to show that the variance was in fact zero, one could devise experiments to determine whether or not it exceeded an arbitrarily small value.

Conversely, if a variance attributable to this source were found, then it would constitute proof for the existence of psi. It is to be emphasized that the kind of experiment envisioned here does not require that any single subject, or group of subjects score consistently. Rather, each subject observes only a single trial, and the overall scoring is predicted to coincide with mean chance expectation. The spread in results is, however, predicted to be larger owing to the chance effects of psi sources sometimes pushing the scoring up and sometimes down. Undoubtedly methodological problems would arise in the course of such an investigation, but at least they should be quite different from those which have plagued experimental research to date.

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EMPIRICAL CONSTRAINTS UPON THE CHOICE OF MODEL FOR MIND/BRAIN RELATIONSHIP: A PROPOSAL

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Most contemporary philosophers and psychologists tend to regard the question of the mind/brain relationship as having either been settled in favour of Central State Materialism or as being essentially undecidable and metaphysical in nature. Many parapsychologists however, have regarded their studies as providing evidence for some form of dualism whereby minds are not wholly reducible to brains or their functioning.

There appear to be two sorts of evidence which could be regarded as constraining the choice of model for mind/brain relations, one sort being the "normal" evidence showing localisation of brain function, or correlations between brain function and conscious experience or behaviour, the other sort being the results deriving from research upon paranormal perception - telepathy, clairvoyance and precognition. Psi functions appear to transcend the limitations applying to brains. This has led to the hypothesis being made that psi functions may be due to the activities of non-physical minds.

Dualist positions of the Cartesian type (as opposed to "subtle body" types of dualism) have usually been subject to five main forms of objection. The first type of objection is that dualism arises from a conceptual confusion created by reificatory talk of "minds" when really "mind" talk is an indirect way of expressing quasi law-like statements about people's capacities, dispositions, habits and styles of behaviour. This form of objection applies to any form of dualism and was initially formulated by behaviourist psychologists and subsequently adopted by the philosophers of the logical behaviourist school.

The second objection is that it is not necessary to posit the existence of ghostly minds as all human information processing and conscious experience can be adequately accounted for by brain function.

The third objection is that giving mental events an ontological status independently of brain states produces "nomological danglers" - events which cannot be identified with observed entities or their states and which as a result would not fit in to the world as encompassed by science.

The fourth objection is that the concept of a non-physical mind entity is an empty notion, a metaphysical supposition not subject to empirical test.

The fifth objection is that interaction between a mind entity, which by definition is non-physical, and the brain - a physical object, must be impossible, because only physical entities can interact with other physical entities.

To maintain its feasibility any dualist position must be able to meet the five objections raised. Clarification of the dualist position may be aided by the use of the hardware/software distinction. In any information processing system the hardware is the physical "works", the software is constituted by the set of instructions - programs - which organise the hardware's information processing functions.

The nub of the parapsychologically generated case for dualism has been that humans show software functions - psi perception, which appears inexplicable by reference to their hardware - brains. These results provide an effective answer to objections one and two. Of course should a physicalist explanation of psi be proved, for example that psi perception is due to the reception and transmission of very low frequency radio waves, then the case generated for dualism by parapsychology would perhaps collapse.

The third and fourth objections are closely related. Dualists have to provide evidence showing that mind entities are inferred entities rather than being metaphysical. At present dualists can supply no empirically constrained description of the structure of mind entities, but the process of rescuing minds from having purely metaphysical status has already taken the first step as psi software functions apparently impossible for brains to perform have been shown to exist.

The fifth objection can now, I think, be met, and itself provides, indirectly, a form of test for dualism. The dualist picture which seems to be emerging is of man having two hardware items, brain and mind entity, both of which perform both hardware functions, casually interacting with the world, and software functions, processing information. The interface between mind entity and brain is mediated by ESP, allowing the mind to scan its associated brain, and PK, which allows the mind to control the brain. Beloff has recently stated this view (1) and E.H. Walker

has produced a detailed (2) set of hypotheses concerning mind/brain interaction. I am not qualified to comment upon his postulation of control by means of "hidden variables", nor his postulation of quantum mechanically coupled electron flow in the brain, but his proposal that PK is basically a Maxwell Demon function seems to be a credible answer to objection five. The mind's sole hardware function is posited as being PK, which does not involve an energy transfer from mind to matter, but an information transfer involving a reduction of the entropy of noisy energy containing systems so as to obtain work from them. PK on this view is thus both a software function in its informational aspect, and a hardware function in its causal aspect.

So far the parapsychological evidence tendered in favour of dualism has been mainly of software functions, but it is possible that very much more compelling evidence for dualism could be obtained from the mind entity's hardware function via PK.

The well established research utilising Schmidt types of PK indicating devices (3) has apparently shown that PK upon quantal processes is possible. One counterhypothesis to the conventional interpretation of results is that they are due to the effects of paranormally created changes in the electrical conductivity of the air surrounding the device's circuits. Paranormally created changes in the electrical conductivity of air have already been noted within the literature (4), (5). This counterhypothesis needs to be checked. But taking results at face value, there is an ambiguity present in the interpretations which it is possible to place upon work involving Schmidt devices which if clarified could possibly produce rather powerful evidence in favour of dualist views.

Supposing that the dualist scenario is true, it would not be surprising if the speed of operation of the mind entity were faster than the speed of functioning of the brain, whose speed of operation is rather well known. The interfacing between the mind entity and brain would proceed more smoothly, as error correction within the interfacing process would be more efficient if the mind entity could react more rapidly than the brain. If this hypothesis is true, it provides a test for dualism, because parapsychologists could show that people can exhibit (PK) hardware functions which exceed in rapidity responses that could be mediated by the brain.

The relevant ambiguity in the interpretation of results from Schmidt devices is that success in influencing them may be due to a dynamic form of PK which would satisfy the proposed test for

dualism, or it may be due to a static form of PK effect, which would not. The dynamic form of PK would involve the PK agent's mind entity monitoring the state of the Schmidt device and coordinating a very rapid PK "poke" at the noise generator within it so as to cause the noise generator to deliver a pulse at precisely the correct instant (which in the original device was a series of temporal "windows" only one microsecond long), so as to make the device latch in the target state. Such a dynamic form of PK would show that action within a one microsecond interval was possible, and one microsecond is some five hundred times shorter than the interval taken by one nerve impulse to cross one synapse (6).

The static interpretation of the results is that PK causes the overall probabilities of the device's operation to slightly shift, making it latch in the target state slightly more frequently than it normally would. Focusing and linger effects have already been noted with certain PK effects and the static interpretation would be compatible with this sort of effect (7).

What is needed to clarify the issue is a form of PK test which differentiates between static and dynamic effects. To this end modifications could be made to the Schmidt device so that the state designated as target was changed randomly very shortly just before each individual trial (something like one microsecond before each trial). This would still enable a dynamic form of PK effect to score at significant levels, but would prevent static effects from achieving significant scores unless the target selection process was itself biased, which could be checked.

However, even this modified test is still challengeable on the grounds that although the task appears to be a dynamic one, in reality it is static in that it is still to cause a state to occur which will as an end product register a hit. Viewed in this way perhaps all putatively dynamic PK tasks would seem to be static in character, and PK on this view would be a process where the increase in probability of occurrence of a given end state was what PK effects coupled with, regardless of the complexity or nature of the particular quantal route by which it was reached. Schmidt's work with differing machine configurations would seem to lend support to this interpretation (8).

I do not find the reduction of dynamic forms of PK task to static ones very convincing, for two reasons. Firstly, it must be trivially true that PK does increase the probability of occurrence of the target state - otherwise there would be no PK effect to argue about! Viewing the proposed dynamic form of PK task in any conventional cybernetic way must involve the assumption that the presentation of the assigned target state to the interfacing

mechanism very shortly before the device becomes open to PK influence requires a rapid target state updating to be done by the mind entity, otherwise a "normal" pulse from the noise generator would latch the gate before PK action occurred and results would be random.

Secondly, if PK effects originate from the interfacing mechanism between mind and brain, it is not surprising that machines which we regard as presenting complex PK tasks should be equally as easy to achieve given levels of scoring rate upon as relatively more simple devices, because either sort of device must be dwarfed in complexity by the enormous complexity of the neural circuits which are the more usual targets for the PK activity of the interfacing mechanism.

To the counterhypothesis that precognition could short circuit the procedure of targets being randomly chosen shortly before each trial, the only possible precaution would seem to be to increase the trial rate, so that even if the PK target had been foreseen, the interval available for the establishment of a PK effect upon the noise generator would be less than could be effectively utilised by neural components.

In summary, it has been suggested that the occurrence of the psi software functions of the mind entity are what has initially drawn parapsychologists to dualist views of the mind/brain relationship. Fine detail of the hypothetical mind entity cannot yet be offered, nor can any account yet be given as to why mind entities should be non-physical in nature, although theorists such as LeShan (9) would seem to be claiming that mind entities may lie on the other side of some dimensional barrier. Mind/brain interaction seems to be less problematical than has been thought and Walker's formulations have apparently offered a model for the interfacing mechanism. It is proposed that the evidence for the existence of mind entities be broadened so as to not only include perceptual psi functions apparently inexplicable on the basis of brain function, but also to encompass PK effects of dynamic type. Should dynamic types of PK effects showing faster action times than would be possible on the basis of neural functioning be shown to exist, this would constitute a valuable step towards the proof of the existence of mind entities as distinct from brains.

Finally, it is suggested that dualist models of mind/brain relationship could ease the conceptual discomfort caused by the existence of precognition because information drawn from the future by non-physical minds could be routed to brains without there being any need for an energy transfer from the future to that future's past. This would mean that causal problems encountered in models of precognition which involve energy transfer from the future to the present would perhaps be overcome by a dualist model of the

mind/brain relationship.

It is hoped that the foregoing discussion may stimulate research into dynamic PK effects which would satisfy the proposed test of dualism.

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SEX DIFFERENCES AND DOMINANCE EFFECTS IN A NOVEL CLAIRVOYANT PSI TEST

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INTRODUCTION

Sex differences in psi test results have been reported many times (e.g., Anderson & Gregory, 1959; Ballard, 1975; Busschbach, 1956, 1959, 1961; Freeman, 1963; Louwerens, 1960; White & Angstadt, 1963) but there are problems involved in considering the sex-difference literature. Firstly, we do not know how many studies failed to yield evidence of sex differences; we may be able to find out how many papers do not cite such differences but we do not know how many, if any, of those papers might have contained sex differences in scoring overlooked or not analysed for by authors. Secondly, it would not be easy to make a coherent story out of the sex-difference literature since rarely has this variable been systematically manipulated. In one of the few systematic explorations of sex differences, Krippner (1970) found that males showed significant psi-hitting as percipients in a GESP psi test whilst female percipients showed chance scoring. No sex differences were observed in the performance of agents. This finding, of course, undermines the old belief that women make better percipients, and men make better agents, in telepathy. It would therefore seem that women are more likely to remember their experiences as psi percipients, are more ready to admit to such experiences, or are less trustworthy observers than men and report lots of false alarms.

It is not my intention to review the literature on sex differences in psi tests here, since the form of the psi test used in the experiments to be discussed makes comparison with earlier findings

on sex differences difficult. Two papers, however, deserve some comment. Mihalasky (1970) found that 'dynamic' persons, if they are in a dominant subgroup involved in a group psi test, score better than 'nondynamic' persons from that same dominant subgroup, whilst in the nondominant subgroup nondynamic persons score better than dynamic persons. Several problems arise with regard to evaluating Mihalasky's report, which is only in abstract form. Firstly, since no data is given to support the validity of findings of the questionnaire used to separate Ss into 'dynamic' and 'nondynamic' persons we are entirely free to doubt such an interpretation of questionnaire results. Secondly, Mihalasky's definition of 'dominance' was not simply based on arithmetic considerations; sex of group leaders, inherent sex-typed nature of the group (e.g. for Mihalasky, PTA = female-dominated group, whilst management groups + male groups) and other factors contribute to dominance. The criteria for evaluating dominance are not strictly defined which seriously diminishes the value of Mihalasky's data.

Friedman et. al. (1976) presented data which touch more closely on the findings reported in this paper than any other paper cited here. They showed that dominant groups in mixed classes score higher on psi tests than do nondominant groups, and their criterion of dominance was simple: arithmetic dominance. This was the criterion used by myself and since the sex ratios in my experiments are similar to those given by Friedman et. al. (1976) it is of interest to see how their results compare with those reported here.

In the experiments to be described, three groups of Ss were tested, and each group had a strong (at least 2 to 1) imbalance of the sexes present. The test used was a novel binary-choice clairvoyant psi test using Christian names as stimuli. I will briefly give a rationale for the use of such a test. It has often been said that conventional ESP testing procedures are boring (i) because of their monotony and (ii) because the stimuli used (e.g., ESP cards) are dull, unemotional, etc. I have little sympathy with this view but I enquired of some friends what sorts of stimuli they would like to work with in psi testing, and two of them independently mentioned that names would be interesting stimuli. A test using names as stimuli would give one the opportunity to examine sex differences in psi scoring as well as being interesting for Ss. So the test employed here was developed, the Names Test (NT).

EXPERIMENT I: JULY 1976

SUBJECTS

Ss were 20 in number, 15 male and 5 female, giving a male:female ratio of 3:1. They were a mixture of undergraduate medical students, post-graduate psychologists, and members of staff of the Department of Psychology. A large majority of Ss were well known to E before the experiment and many of them had participated in previous experiments conducted by him.

TEST MATERIALS

An early version of the Names Test, NT1, was used. This version, given in Appendix I, contained 64 pairs of Christian names, the first 32 pairs being male names and the second 32 pairs being female names. The psi task was a binary-choice clairvoyant one: Ss were provided with a response sheet and a target envelope inside which was a target sheet which had one name from each and every name-pair underlined, in a random order: Ss job was to underline, on his response sheet, one name from each and every name-pair corresponding to the one which he/she felt was underlined on the target sheet.

Target binary random digits were used to specify target names, moving sequentially down the list of name-pairs. Odd-numbered digits were taken to code for the left-hand member of a name pair being the target, and even-numbered digits were taken to code for the right-hand member of a name-pair. Target digits were taken from the Cambridge Elementary Statistical Tables, a card-cutting procedure being adopted to code for an entry point to the tables. At the time of preparing the targets, the Departmental Computer was inoperative following a 'routine maintenance', so computer-generated random digits could not be used. The individual series of targets were, however, tested by frequency and runs-tests, as was the whole population of targets, and did not show significant deviations from an ideal random series.

The target sheets having been prepared, CLS flanked each one with two stiff sheets of card and then inserted the whole into a brown opaque envelope which was Sellotape-sealed. CLS then handed these envelopes to a confederate who shuffled them thoroughly and returned them to CLS, who was thus blind to target order during testing.

PROCEDURE

The 2o Ss were tested in a single group in a classroom of the Department of Psychology. Ss were seated at desks, and were given a sheet of instructions about the test, a response sheet, and a target package. Whilst this sounds very formal, the presence in the group of a large number of Ss who knew E well ensured that the testing atmosphere was, in fact, easy and informal.

When Ss had finished the test, CLS collected up all the response sheets and target packages, writing each Ss name on the appropriate target package. Data was double-checked by CLS.

Feedback to Ss was delayed and nonsystematic. CLS left Cambridge shortly after the experiment for a holiday and on his return sent out Xerox copies of results of the experiments to most of the Ss, informing others by word of mouth of results. In point of fact the written feedback to Ss was slightly erroneous: whilst the nature of findings was correctly given (see Results) CLS informed Ss that more Ss had been tested than was actually the case. This mistake arose from a confusion in computer-filing of data, CLS retrieving the wrong set of data from his file space. This mistake was only realised after the sheets had been sent out. This seems worthy of mention because of the critical role attributed to feedback by many parapsychologists (e.g. Schmidt, 1975; Tart, 1976). However, all Ss were correctly informed about their individual scores, and also of the qualitative nature of the results of the experiment.

RESULTS

Table 1 shows scoring rates on various categories of stimuli for different categories of S. (see next page)

Data was evaluated by binomial and t- tests. The following findings emerged:

All Ss

The overall deviation of +15 hits is not a significant deviation from chance expectancy, and scoring rates are not significantly different from chance expectancy on either male or female names; nor is the difference between them significant. However, there is significant psi-hitting on cross-sex names ($t=3.47$, $df=19$, $P=.006$, two-tailed (all P-values cited in this paper are two-tailed unless otherwise stated) or, by a binomial test, 16 Ss show an above-chance score on cross-sex names and 4 a below-chance score, $P=.012$). Also, there is significantly higher scoring on cross-sex than on

TABLE 1

RESULTS OF EXPERIMENT I

(a) All Ss (N=20)

Targets	Trials	Exp. hits	Sigma	Obs.hits	Devn.
All	1,280	640	17.89	655	+15
Male names	640	320	12.65	325	+5
Female names	640	320	12.65	330	+10
Own-sex names	640	320	12.65	305	-15
Cross-sex names	640	320	12.65	350	+30

(b) Male Ss only (N=15)

Targets	Trials	Exp. hits	Sigma	Obs. hits	Devn.
All	960	480	15.50	493	+13
Male names	480	240	10.95	234	-6
Female names	480	240	10.95	259	+19

(c) Female Ss only (N=5)

Targets	Trials	Exp. hits	Sigma	Obs.hits	Devn.
All	320	160	8.94	162	+2
Male names	160	80	6.32	91	+11
Female names	160	80	6.32	71	-9

own-sex names (t-test for correlated means yields t=3.09, df=19, P=.012; by a binomial test, 16 Ss show higher scoring on cross-sex than on own-sex names whilst 3 show the reverse picture, 1 S showing no difference: P=.004).

Male Ss

There is a suggestion of higher scoring on cross-sex than on own-sex targets (correlated t=2.23, df=14, P=.09; or, by a binomial test, 11 Ss score more highly on female than on male names, 3 score

more highly on male names, 1 shows no difference:binomial P<.06).

Female Ss

First, there is a suggestion that females show psi-hitting on male names (all 5 Ss show an above-chance score on male names; binomial P=.063: however, t=1.84, df=4, P>.10) and that they show higher scoring on male than on female names (all 5 Ss show this effect, binomial P=.063; but again correlated-t=2.24, df=4, P>.10).

Comments

CLS realised after the first experiment that the data do not allow one to discriminate between two types of psi effect: the results given might reflect one, or both, of the following: (i) Ss score better on cross-sex than on own-sex names, or (ii) male SS show an incline in psi scoring during the test whilst females show a decline. (i) certainly has a higher face validity than (ii) as an explanation of the results, but (ii) remains a possibility. (ii) as an explanation was eradicated in later work with the use of NT2, a form of the Names Test with a randomised order of name-pairs. For this reason, a detailed consideration of the results obtained in Experiment I will not be given here but a general discussion of all results obtained with NT1 and NT2 will be given at the end of the paper.

EXPERIMENT II: OCTOBER 1976

This experiment was also conducted using NT1. Whilst NT2 materials were being duplicated an opportunity arose to test a large group of Ss, and rather than pass up the opportunity CLS decided to use NT1, since use of that test at least permitted validation of the unpredicted findings of Experiment I.

Two subseries of the experiment were carried out within three days of each other, and since the size of the group tested in Subseries IIa was small (N=6) it was decided in advance of any knowledge of results to combine the data with that from Subseries IIb. Since the procedures involved in the two subseries differed slightly, however, they will be reported on separately.

SUBSERIES IIa

Subjects

6 Ss were tested, 5 male and 1 female. They were tested at an

informal pre-term meeting of the Cambridge University Society for Psychical Research, held in the basement premises of the Society. Whilst none of the Ss were known to E, there were few of them in the group, so the atmosphere during testing was informal.

Target Materials

These were exactly as in Experiment I. CLS had hoped for a larger group size than 20 in that experiment, and still had some spare target packages left over. Six of these were therefore used.

Procedure

Ss were invited to make their guesses as in Experiment I. When they had all finished, CLS collected in the guess sheets and then asked Ss to open their target packages and sign the target sheets inside. CLS rough-checked the data (which was double-checked later) and gave SS feedback about their scores.

SUBSERIES IIb

Subjects

49 Ss were tested, 32 male and 17 female. The ratio of males to females was in fact over 2 to 1 because of the presence of numerous officers of the C.U.S.P.R. (all males) during the test.

Test Materials

CLS had been told that the attendance at the meeting was likely to be approximately 30, so he prepared 40 sets of target packages using pretested computer-generated targets. These target series prepared were individually numbered 1 to 40, and CLS handed them encased in opaque envelopes and with stiff card-sheets flanking them as in Experiment I to a confederate who numbered them in an arbitrary way and returned them to E. A code was kept by the confederate of which envelope-numbers corresponded to their order as given to him by CLS, and this code was given to CLS at the end of the experiment. The confederate knew nothing of the target series, so at the time of testing no-one knew what the contents of any particular envelope were.

In addition to these 40 target packages, CLS happened by chance (?) to have in his brief-case the nine remaining target packages from Experiment I. These were also used.

Procedure

The test was administered following an introductory lecture on parapsychology given by the C.U.S.P.R. President, A.D. Cornell, and a brief chat on experimental parapsychology given by CLS in his role as Research Director of the C.U.S.P.R. People present at the lecture were invited to take part in a psi test and those who were willing to do so were given a response sheet, a target package and instructions about how to do the test. Silence was asked for during the test, which was untimed. When Ss had all finished, response sheets were collected up by CLS and four officers of the C.U.S.P.R. who checked that target packages had not been opened, and that Ss had-as instructed-put the number of their target package on their response sheet. When all response sheets had been collected up, Ss were asked to open their target packages, take out the target sheet, and sign it. This was done to minimize the possibilities of E-fraud; target names were boldly underlined in dark blue felt-tip and CLS could not have changed them after the experiment.

The fact that Ss had recorded the number of their target package on their response sheet precluded any possibility of Ss 'swopping around' target sheets in order to get high scores. The nine Ss who received target packages from Experiment I (which were unnumbered) were carefully selected so that they were seated well apart, and were watched carefully so that they could not swop targets.

Data from one S was discarded. This S had taken part in Subseries IIa and her retest data could not legitimately be included in the Subseries IIb sample (the data supported the hypotheses being investigated in both cases, from this Ss tests).

Predictions made about outcomes of Experiment II

The following predictions were made about the outcomes of this experiment:

(i) For all Ss, there will be significant psi-hitting on cross-sex names.
(ii) For all Ss, there will be significantly higher scoring on corss-sex than on own-sex names.
(iii) For male Ss, there will be significantly higher scoring rates on female than on male names, and the P-value of this finding will be lower if the data is evaluated by a binomial test than if it is evaluated by a t-test.
(iv) For female Ss, there will be significant psi-hitting on male names if the data is evaluated by a binomial test.

(v) For female Ss, there will be significantly higher scoring on male than on female names if the data is evaluated by a binomial test.

(i) and (ii) follow directly from the significant findings of Experiment I. (iii)-(v) are justified in view of the low, though not significant, P-values (<.06,=.063, and =.063 respectively) found in Experiment I regarding these effects which reflect, with a small number of SS and the binomial test, a quantitatively sizeable effect which would be expected to reach significance with an N over 2½ times larger than the N in Experiment I.

These five predictions are not, of course, all independent.

RESULTS

Raw data are shown in table 2. One comment on them is in order here. One male S underlined both members of five male-name pairs. CLS was thus faced with the problem of what to do with this data. I consider that data should be discarded from psi experiments only when absolutely necessary, for to do otherwise evokes the selection-of-evidence spectre. It was not apparent that this Ss data need necessarily be entirely useless. It could clearly be used to test prediction (i) since the responses on female name-pairs were valid. It could also be used to test predictions (ii) and (iii) when the binomial statistic was used, if one of the following was the case:

(a) The score on the 27 remaining male-name-pairs was higher than the score on the 32 female name-pairs; or

(b) The score on the 27 remaining male name-pairs was at least 6 hits lower than the score on the 32 female name-pairs.

If either of these turned out to be the case, then the outcome of the five 'rogue trials' could not affect the Ss relative scoring on male and female names, so use could be made of his data. In fact, this S showed 14 hits on the 27 male name-pairs where responses were valid, and 21 hits on the 32 female name-pairs. Therefore, irrespective of the outcome of the 5 rogue trials, this S would have scored higher on female than on male names. His data is included also in the grand totals for the group shown in table 2.

The fates of the predictions made are as follows:

(i) Confirmed, just, by a t-test (t=1.73, df=53, P=.045, one-tailed) but not by a binomial test (31 Ss above-chance on cross-sex names, 4 at chance, 19 below:CR=1.56, P<.07, one-tailed).

(ii) Confirmed by a binomial test (35 Ss in predicted direction, 19 in opposite direction CR=2.04, P=.022, one-tailed) but this result fails to reach significance when a t-test is used (correlated t=1.58, df=52, P=.061, one-tailed). The t-test uses data from only 53 Ss.

TABLE 2

Results of Experiment II

(a) All Ss (N=54)

Targets	Trials	Exp.hits	Sigma	Obs.hits	Devn.
All	3,451	1,725.5	29.37	1,759	+33.5
Male names	1,723	861.5	20.75	846	-15.5
Female names	1,728	864	20.78	913	+49
Own-sex names	1,723	861.5	20.75	858	-3.5
Cross-sex names	1,728	864	20.78	901	+37

(b) Male Ss only (N=37)

Targets	Trials	Exp.hits	Sigma	Obs.hits	Devn.
All	2,363	1,181.5	24.31	1,207	+25.5
Male names	1,179	589.5	17.17	576	-13.5
Female names	1,184	592	17.20	631	+39

(c) Female Ss only (N=17)

Targets	Trials	Exp.hits	Sigma	Obs.hits	Devn.
All	1,088	544	16.49	552	+8
Male names	544	272	11.66	270	-2
Female names	544	272	11.66	282	+10

It is worthy of mention at this stage that the P-values obtained from t- and binomial tests differ. The fact that prediction (i) is supported by a t- test but not by a binomial test reflects the fact that the problematic S, whose case we have noted, scored 21 hits on cross-sex names, a deviation of +5 which is a not insignificant contributor to the total group deviation of +37 on cross-sex names, and he contributes heavily to the t-test result. On the other hand, his data could not be included in a correlated-

means t-test of prediction (ii) since we have no way of knowing what his 'true' score on male names would have been on male targets: but he does contribute to the binomial result, contributing to the fact that the binomial thus gives a lower P-value than the t-test does.

To move on:

(iii) Confirmed. Binomial result: 27 Ss in predicted direction, 10 in opposite direction; CR=2.63, P=.0044, one-tailed). In fact, the P-value obtained from a t-test is similar although, as predicted, it is slightly larger: t=2.55, df=35, P=.0075, one-tailed.

(iv),(v): Not confirmed and trends are in opposite direction to the prediction.

Finally, although this was not predicted since the P-values obtained in Experiment I did not warrant such a prediction, males showed significant psi-hitting on female names (binomial:23 Ss above-chance, 4 at chance, 10 below:CR=2.09, P<.04; t-test, t=2.56, df=36, P=.015).

The picture that emerges is that the majority group (males) show the predicted effects quite clearly, and being in the majority they are enough to 'carry the group' as regards the group predictions. Females show essentially no psi effects at all. Please note that the data from this experiment are easily explicable in terms of decline effects rather than interactive effects of Ss sex and sex of stimuli but this point, as stated earlier, will be discussed when all data has been presented.

EXPERIMENT III: MARCH 1977

SUBJECTS

Ss were 50 undergraduate psychologists in a group tutored by Dr. Alan Gauld at the University of Nottingham. 16 were male qnd 34 female, giving a female:male ratio in excess of 2 to 1. They were tested following a brief introduction to the test by CLS; these students were complete novices to parapsychology, and CLS briefly introduced the concept of clairvoyant psi and described the test, saying that research conducted with it in Cambridge had provided some evidence for the operation of clairvoyance.

TEST MATERIALS

The NT2 version of the Names Test was used here (see Appendix II). This test has a randomised order of name-pairs on it, and CLS also exchanged some of the more uncommon names on NT1 for commoner ones.

The reason for this will be discussed in more detail later, but briefly I wished to ensure that a reasonable proportion of Ss found their own names somewhere on the NT2 list.

A third source of random numbers was used in this experiment; the RAND Corporation random digits (RAND, 1965). An entry point to the tables was obtained by a card-cutting procedure, and 45 sets of 64 consecutive digits were read off, converted into binary random digits, and transcribed onto target sheets. The target sheets were individually numbered 1 through 45 and flanked by two stiff sheets of card, the assemblage then being inserted into an opaque brown envelope and Sellotaped up. CLS then passed the 45 target packages in a known order to a confederate who arbitrarily numbered them with a recorded code and passed them back to CLS. As in Experiment II Subseries B, this procedure ensured (i) that CLS was blind to target orders during testing, and (ii) when Ss opened their target packages (see Procedure) they could not swop targets around.

PROCEDURE

The class assembled to find that CLS had set out on desks 45 sets of target packages plus two response sheets and a carbon sheet. Ss were instructed to make carbon copies of their guesses and also to mark the number of their target envelope clearly on their guess sheet. When all Ss had finished guessing, CLS, AG, and three helpers collected up all the carbon copies of the guess forms and checked all the target packages for any tampering. Ss were then allowed to open their target packages and check their own data, which was rechecked later by CLS.

As stated, N=50 but only 45 target series were used. CLS had been advised by AG to expect up to 30 persons, so he prepared 45 target packages which turned out to be insufficient. Rather than exclude five persons who wished to take part, five pairs of Ss were allowed to 'share' a target package (CLS did have spare response forms and carbons). The resultant stacking effect, with 45 target sequences and 50 Ss, is quite negligible.

PREDICTIONS ABOUT OUTCOMES OF EXPERIMENT III

No predictions about the outcomes of this experiment were made since this group was the only female-dominated group tested.

RESULTS

Raw data are shown in table 3.

TABLE 3

Results of Experiment III

(a) All Ss (N=50)

Targets	Trials	Exp.hits	Sigma	Obs.hits	Devn.
All	3,200	1,600	28.28	1,532	-68
Male names	1,600	800	20.00	759	-41
Female names	1,600	800	20.00	773	-27
Own-sex names	1,600	800	20.00	783	-17
Cross-sex names	1,600	800	20.00	749	-51

(b) Ss subdivided by sex

Male Ss (N=16)

Targets	Trials	Exp.hits	Sigma	Obs.hits	Devn.
All	1,024	512	16.00	496	-16
Male names	512	256	11.31	253	-3
Female names	512	256	11.31	243	-13

Female Ss (N=34)

Targets	Trials	Exp.hits	Sigma	Obs. hits	Devn.
All	2,176	1,088	23.32	1,036	-52
Male names	1,088	544	16.49	506	-38
Female names	1,088	544	16.49	530	-14

The group as a whole shows some fascinating trends in scoring. The total group deviation of -68 hits is significant if evaluated by a t-test ($t=2.40$, $df=49$, $P=.02$, two-tailed) but is completely insignificant if evaluated by a binomial test (19 Ss above chance, 4 at chance, 27 below: $CR \tilde{=} 1$). The reason for this may be seen in table 4 and in figure 1. Figure 1 shows that the form of the

group scores approximates more to a bimodial distribution than to a normal one: however, calculation of the a statistic (Pearson & Hartley, 1966 p. 207) shows that the value of a (mean deviation/ standard deviation) has a value of 0.8317 which represents a departure from normality not even significant at the .1 level of probability. Thus we have no grounds for arguing that the data shown does not approximate to a normal distribution and our use of the t-test in assessing the significance of the group deviation of -68 hits is correspondingly not invalidated. (See figure 1 next page.)

Table 4 shows that the psi-hitters and psi-missers in the group do differ in the magnitude of their deviations from chance. This is obvious from the gross difference between the values of P obtained from t- and binomial tests noted above, but is formally demonstrated with the use of the chi-square statistic in table 4.

TABLE 4

Magnitude of deviations from MCE

shown by psi-hitters and psi-missers

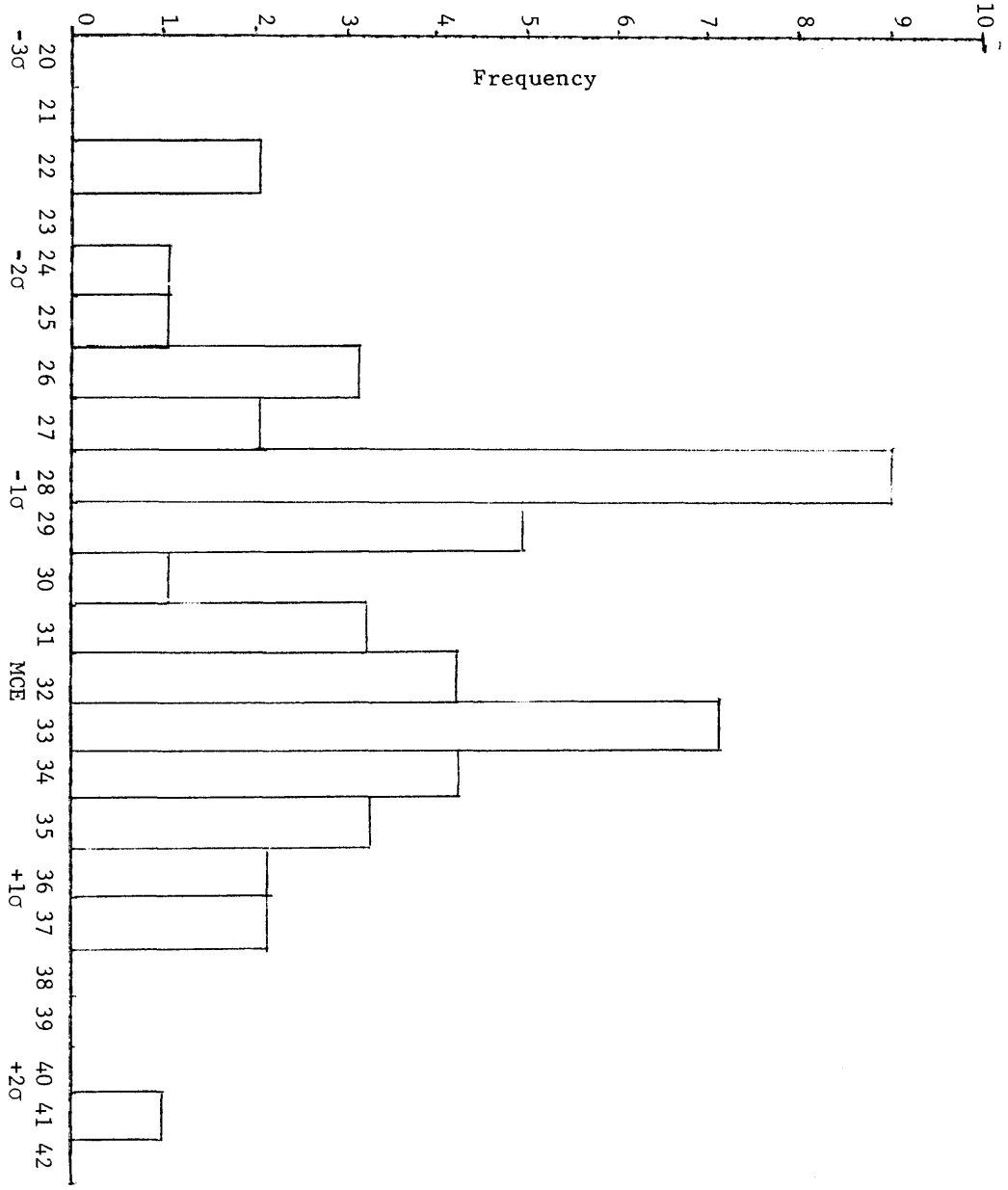
	Hitters (N=19)	Missers (N=27)
Score ≤ 3 points from MCE	14	8
Score ≥ 4 points from MCE	5	19

χ^2 (continuity-corrected) = 8.24
P=.0082, two-tailed.

We might infer that Ss tend to fall, in the main, into one of two categories: weak psi-hitters and strong psi-missers. If this inference is valid, we might expect that test-retest reliability with these Ss should be good. Shortly after CLS had collected up all his data and departed, Dr. Gauld carried out a completely different psi test with his group (see Appendix III). Whilst his experiment did not yield any firm results of its own, results from the AG test yielded a statistically significant correlation with

FIGURE 1

Distribution of scores in Experiment III



results from the CLS test. So, given the psi-missing by the group reflected in the reasonable P-value (=.02) obtained with the t-test, the results from the chi-square test applied, and the AG retest data, we have some grounds for inferring the existence of operative psi in the performance of this group on NT2.

Considering a breakdown of this scoring, we find that the group shows significant psi-missing on cross-sex targets (t=2.24, df=49, P=.034). The absolute values of the deviations on these categories were, respectively, -51 and -41 hits. Which effect is the primary one? Let us examine data breakdown by sex of Ss.

Males show no psi effects whatsoever. Females show significant psi-missing overall (t=2.33, df=33, P=.026) and this is solely due to very significant psi-missing on male names (t=2.92, df=33, P=.0062).

It should be noted that application of binomial tests would not have yielded any significant P-values here.

COMMENTS ON EXPERIMENTS I-III

Do we have reasons for inferring the existence of operative psi from the data here? The answer is yes. Taking Experiment I, given that findings were not predicted as such, from the seven analyses conducted (not all of which were independent) two yielded significant results. Since these two were interdependent, let us argue that we extracted one significant P value from six analyses. Given that the P value was .004, this outcome was suggestive. The second experiment gave three significant confirmations out of five, again all interdependent; but the P-value of 0.0044 presented for the predictions made about the scoring rates of male Ss on male and female names (i) carries the experiment, and (ii) is a replicative finding also. In Experiment III, we have good evidence for the operation of psi from the overall psi-missing, as well as from the unusual total distributions of scores and some confirmation of the psi abilities of the group from Dr. Gauld's retest data. Whilst it is not easy to give a formal estimate of the significance of the data reported it is reasonable to argue that psi effects were present during the experiments.

Before attempting to theorize about the implications of the data, it is interesting that the effects observed seem to be robust in the face of, and not systematically affected by, variations in experimental procedure in the form of changes in sources of random digits and feedback differences.

DISCUSSION

It is clear that the data from Experiments I and II could be

explained in terms of sex-linked incline and decline effects: however, since the data from Experiment III could not be explained in such manner, I shall not go any further into the explanations which might be given.

Let me offer a hypothesis and attempt to defend it: the hypothesis is that sex of S interacts with sex of target name, and that this may reflect itself in (a) significantly different scoring rates on cross-sex and own-sex names, and/or (b) significantly extra-chance scoring on cross-sex names. This hypothesis concerns the roles of Ss sex and sex of stimuli: it is also clear that group-role (dominant/nondominant) affects scoring, but this variable is not being considered yet.

The data from Experiments II and III could be explained simply in terms of extrachance scoring effects occurring on male names: there would then be no necessity to invoke an interactive effect of Ss sex and sex of stimuli on psi scoring. However, this hypothesis could not explain the data from Experiment I where it is plain that the only results remotely approaching significance concern Ss scores on own and cross-sex stimuli. It follows from this, and what has gone before that an elaborate explanation of the findings for the whole group data from the three experiments could be cooked up using incline effects in males plus a general tendency to score higher on female targets to explain the results observed: but, on the grounds of parsimony such an explanation seems far-fetched (it should be noted that, in Experiment III, no tendency for males, or females, or all Ss to show either declines or inclines was observed).

It would appear then that the hypothesis tentatively offered earlier would seem to be the best explanation of the data presented, as regards the effects of Ss sex and sex of stimuli on psi scoring. We now turn to the effects of group role, and what has been observed in Experiments II and III suggests that it is the dominant group which shows the psi effects: the nondominant group shows essentially no psi effects at all. Friedman et.al. (1976) reported that dominant-subgroup Ss score higher than nondominant subgroup Ss; here, it seems that dominant-subgroup Ss score better than nondominant subgroup Ss. Friedman et.al. reported a directional difference: mine seems to be a variance difference. However, in Experiment I what was observed was clearly a whole-group effect: the dominant and nondominant-subgroup Ss both contributed to it. This weakens the strength of the conclusions drawn from Experiments II and III regarding the effects of dominance, and it is worth considering why the discrepancy arose.

In two respects the group in Experiment I differed from those in

Experiments II and III; in size and in knowledge of E. Size of group is almost certainly an important variable in psi tests, although I must admit that I know of no reports which have systematically compared the effects of group size on psi. Also, most of the Ss in Experiment I knew me quite well, whereas in Experiments II and III virtually none of the Ss knew me (in Experiment III the group was, of course, totally unknown to me). I am well aware that the comments made upon these points are mere post-hoc offerings, but there is some evidence that, at least on one point to which I have referred, familiarity with E and with testing (most Ss in Experiment I had done psi tests before: again, this was not true of the Ss in Experiments II and III) can affect results (Sargent, in preparation (3)).

Finally it is possible that sex of Ss and group role might interact, since Experiments I and II (males in majority) showed insignificant, but reasonably clear trends towards, psi-hitting, whereas in Experiment III (females in majority) there was significant psi-missing. However, the data by no means necessarily point to an interaction between sex and dominance on group scoring; but they do suggest it as a distinct possibility.

These conclusions are offered cautiously, and whilst a host of related experiments can be imagined to explore the role of various independent variables it is felt that accumulation of data from over 120 Ss in three experiments warrants a first report. Some possibilities for further work are suggested below.

(1) Specific Names

Obviously, certain names present on the list will be more familiar to Ss than others: it might be of interest to ask Ss to tick those names which are familiar to them as being those of family, friends, etc. Two names on the list may, however, be expected to be noted by all Ss: their own name (if present) and E's name. It was decided before any of the experiments were conducted that 100 protocols would be needed to be collected before an adequate analysis of scoring trends on own-name could be undertaken, and as yet this criterion has not been met. Neither has it been met for E-name, because I was introduced to his group by Dr. Gauld as 'Mr. Sargent' and my first name was not given to them. This group will form an interesting control group when analysis of scoring trends on E-name is undertaken; any strongly extra-chance pattern of scoring on it by this group might suggest an E-psi effect. One problem would arise with evaluating the data pertaining to scoring on E-name: scoring trends on it would have to be compared with scoring on other rare names (none of the 124 Ss tested here had my name and nor have I ever met anyone else with my first name).

(2) Group vs. individual tests

Varying the proportion of females and males in group testing would obviously be important in future work, but every parapsychologist knows that large groups available for testing are a blessing which arises all too infrequently and he must be grateful for the opportunity to test whichever groups he can as and when they become available to him.

Tests with individual Ss would also be illuminating. I have commenced a study using the names test with individual Ss but it will not be complete for some time since N has been preset at 50 to allow comparison of results with those obtained from Experiments II and III conducted earlier with groups. At the time of writing (June 1977) 22 Ss have been tested and it is not expected that the research will be completed before October, since student Ss are largely absent during the summer. It might be premature to comment on the results at this stage, but as yet no sex differences of any sort have emerged in the data.

(3) Experimenter Effects

All the experiments reported here were conducted by a male E, and in Experiments II and III respectively the S.P.R. hierarchy was exclusively male and Dr. Gauld, whose class was being tested, is also male. Different results might have been obtained by a female E, who might (for example) have found psi-hitting with a female-dominated group rather than psi-hitting as I did. Clearly, on this point I can make no further contribution, since even if I used female Es the investigator, myself, would be male and investigators (as opposed to experimenters; see Barber, 1976) can affect results of experiments (Kennedy & Taddonic, 1976; White, 1976).

The experimenter-psi hypothesis also needs comment. The problem of why some Es are "successful" and others not is most acute in the context of group experiments with unselected Es since these are not often successful. The results from Experiment III most strongly suggest a subject psi factor, because of the odd distribution of scoring and the AG retest data. I might also add that in the case of Experiment III I was not confident of obtaining any results at all, since I had risen after little sleep on the morning of the experiment and had suffered a long train journey to Nottingham and consequently felt unwell, tired and most unconfident when I arrived there.

The following conclusions are offered from research with the Names Test to date: 1. sex of S affects psi scoring on the Names test in

interaction with 'sex' of stimulus name; 2. this interaction appears most strongly in the dominant subgroup, dominance being defined arithmetically; 3. the possibility exists of an interaction between dominance of subgroup and psi such that a male-dominated group tends to psi-hitting whilst a female-dominated group tends to psi-missing.

ABSTRACT

A total of 124 Ss were tested in three separate group experiments using a binary choice clairvoyant psi task which uses Christian names as stimuli. Data is presented to show that (1) Ss sex interacts with 'sex' of target name to affect psi scoring; (2) this interactive effect is strongly facilitated by Ss being a member of the dominant subgroup present during the testing; (3) the possibility exists of an effect of dominant subgroup composition on overall group scoring, since males in the majority was correlated in two experiments with marginal psi-hitting whilst females in the majority (one experiment) was correlated with significant (P=.02) psi-missing.

The relationship between these results and other work on group dominance effects is briefly discussed.

ACKNOWLEDGMENTS

I am grateful to Dr. Alan Gauld for the opportunity to test his group in Nottingham, and for his generous provision of the data summarized in Appendix III. I am indebted to the Perrott-Warrick Studentship for continuing support of my researches.

APPENDIX I

Name-pairs used in NT1 test

1. Brian-Graham	2. Tony-Robert	3. Sean-Charles
4. Rex-Mike	5. Malcolm-Keith	6. Linus-Rick
7. Dick-John	8. Burt-Gary	9. Neil-Julian
10. Judas-Edward	11. Philip-Steve	12. William-Dave
13. Andy-Nigel	14. Mark-Carl	15. Edwin-Jim
16. Alex-Neil	17. Patrick-Christopher	18. Frank-Rod
19. Mich-Anthony	20. Justin-Richard	21. Kevin-Paul
22. Trevor-Adrian	23. Oliver-Barry	24. Colin-James
25. Nicolaus-Harry	26. Alec-Michael	27. Andrew-Gerald
28. Arnold-Jonathan	29. David-Tom	30. Fred-Peter
31. Arthur-Walter	32. Bill-Raymond	
33. Anne-Vanessa	34. Erica-Dinah	35. Sue-Jacqui
36. Madeleine-Vicki	37. Teresa-Maggie	38. Susannah-Imogen
39. Sarah-Barbara	40. Yvonne-Valerie	41. Geraldine-Brenda
42. Alice-Sally	43. Carol-Diane	44. Eileen-Dora
45. Patti-Liz	46. Joan-Mae	47. Tina-Dorothy
48. Doreen-Janine	49. Christine-Anna	50. Hannah-Mandy
51. Jane-Laura	52. Andrea-Patricia	53. Val-Wendy
54. Sophie-Helen	55. Nicki-Joyce	56. Linda-Amanda
57. Jo-Charlotte	58. Angela-Margaret	59. Britt-Mary
60. Virginia-Elizabeth	61. Edna-Susan	62. Marilyn-Emelia
63. Maria-Inga	64. Zoe-Caroline	

APPENDIX II

Name-pairs used in NT2 test

Numbers in brackets indicate the name-pair used in NT1 carried over into NT2; some of them were reversed in name-order (e.g. Brian-Graham became Graham-Brian). New, or partly new, name-pairs are given in full.

1. (33) 2. (34) 3. (35) 4. Rachel-Vicki 5. (1) 7. (37) 8. (3) 9. Geoffrey-Rex 10. (5) 11. Rick-Simon 12. (38) 13. (7) 14. (39) 15. (40) 16. (41) 17. (42) 18. (8) 19. (9) 20. (10) 21. (11) 22. Hugh-Ian 23. (13) 24. (43) 25. (44) 26. (45) 27. (14) 28. (46) 29. (47) 30. (48) 31. (15) 32. (49) 33. Alex-Bernard 34. (51)

(cont.)

35. (17) 36. (18) 37. (50) 38. (52) 39. Karen-Wendy 40. (19)
41. (20) 42. (22) 43. (23) 44. (24) 45. (25) 46. (26) 47. (54)
48. (55) 49. (27) 50. (28) 51. (56) 52. (29) 53. (57) 54. (30)
55. (58) 59. (31) 60. (32) 61. (62) 62. (63) 63. (64) 64. (21)

Choice of names for NT1 was purely E's personal choice. Choice of names for NT2 reflected incorporation of commoner names as found in testing with NT1.

APPENDIX III

Results of Dr. Gauld's experiment

This experiment was a classroom replication of the experiments reported on by Broughton (1976). Ss were tested in four conditions, left or right hand/relaxed or distracted (see Broughton, 1976): an agent was used, separated from S by a screen or in a cubicle. An E watched over the experimental pairs (agent and subject). Targets were taken from the RAND Corporation tables (RAND, 1955) and were given to the agent by E, who was given instructions by Dr. Gauld on preparation of an entry point to same. 44 of the Ss from Experiment III also took part in this experiment.

It should be noted that testing conditions were not so tight that psi effects could definitely be inferred from data which deviated significantly from MCE. Only minimal details will be given of the experiment here.

Calculation of a linear regression coefficient of scores on the CLS test with scores on the AG test gave LR=+.356, t=2.47, P=.018, two-tailed.

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A PRELIMINARY STUDY OF PRECOGNITION IN THE GANZFELD[1]

D. Scott Rogo

INTRODUCTION

Recent research has indicated that ESP signals can be easily detected when the subject undergoes internally-focusing procedures such as dreaming (Krippner & Ullman, 1970); muscular relaxation (Braud & Braud, 1974); or sensory isolation (Honorton & Harper, 1974; Braud, Wood & Braud, 1975). Of these various techniques one of the most widely used methods of internal focusing has been by the use of continual ganzfeld stimulation. Using this procedure subjects are cut off from normal access to sensory input by listening to homogenous acoustical stimulation such as white-noise while also staring into a homogeneous visual field induced by covering the subject's eyes with transluscent hemispheres as he stares into a uniform light source. Employing these techniques subjects often report mental imagery, and recent research has suggested that these images may be mediating vehicles for the detection of psi-signals (Honorton & Harper, 1974; Terry, 1974; Braud, Wood & Braud, 1975).

Several ganzfeld designs have been employed including both GESP designs (Honorton & Harper, 1974; Terry, 1974; Braud, Wood & Braud, 1975) and clairvoyant procedures (Stanford & Neylon, 1974), the latter, however, not showing the strong results of the GESP design. Based on the hypothesis that psi represents a unified process there seems no a priori reason that the ganzfeld could not be adapted for the elicitation of precognitively-mediated imagery. In this study a preliminary approach to this problem was made.

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[1]Thanks are due to Mr. Michael Smith of the Division of Parapsychology and Psychophysics; Maimonides Medical Center who aided in this study; to Mr. Charles Honorton for suggestions in the design and appraisal of the results; and to the Parapsychology Foundation who funded the project.

METHODOLOGY

For this study 4 subjects were used, all of whom had previous experience in the ganzfeld and who had strongly habituated to it. All 4 subjects had shown some indication of success during more conventional ganzfeld testing and each contributed five sessions spread over separate days for a four week period.

At the onset of the session the experimenter (S.R. in all cases except when he acted as a subject with M.S. as his experimenter) led the subject into a sound attenuated booth, seated him in a reclining chair and adjusted the transluscent spheres over the subject's eyes until he reported a homogeneous visual field ignited by staring into a red light in front of him. The experimenter would then adjust the white noise level on earphones until S. reported it at a comfortable level. When the ganzfeld was stabilized the Ss were given the following precognitive instruction set:

> "In this experiment we want you to think out loud. Report all of the images, thoughts, and feelings which pass through your mind. Do not cling to any of them nor create images. Just observe them as they go by. After the session I will select a picture and show it to you. Please give yourself a suggestion, right now, that you will image the picture you will see right after the session. (Pause)
> "Keep your eyes open as much as possible throughout the session and allow your consciousness to flow through the sound that you will hear through the headphones.
> "You should take a couple of minutes to get as comfortable as possible. Release all conscious hold of your body and allow it to relax completely. When you begin observing your mental processes, start thinking out loud. Continue to share your images, thoughts, and feelings with us throughout the session."

After relaying the instructional set, the experimenter left the booth, sealed it and monitored S's mentation over a speaker for from 20-25 minutes. (3 of the 4 subjects gave immediate mentation and thus were allowed 20 minute periods. One subject, though, took several minutes to adapt to the ganzfeld and so was given 25 minutes of ganzfeld stimulation.) After the session Ss were instructed that they could remove their headsets but not the hemispheres. At that time the experimenter re-entered the booth and read over to S his mentation for the period. S was then asked to code his mentation as to whether he felt the target incorporated or lacked any combination of ten components (based on Van de

Castle & Hall's Content Analysis of Dreams): color, activity, mythical characters, animals, human characters, artifacts/ implements, food, body parts, architectural features, nature features. Thus, each session consisted of S making 10 binary decisions with a mean chance expectation of 5 correct responses for the session. Ss also made confidence calls on those decisions he felt especially strong about. (See Honorton, 1975.)

After the coding, the experimenter left the booth and generated a target (from a pool of 1024) by the use of the binary target selecter section of a RNG apparatus in use at the Maimonides Medical Center. By pressing a specific switch either a red or green light would illuminate. Ten choices were made with red lights signifying inclusion of a specific category and a green light signifying the absence of the category. This gave a binary coding which designated a target in the pool which was then compared to the subject's own coding and qualitative content of his mentation report.

RESULTS

Of the 4 subjects, three scored below mean chance expectation, two to a significant degree (for each subject CR=1.98, p=.048 two tailed). Only one subject, the experimenter (S.R.), scored above chance but not to a significant degree (CR=1.13, n.s.) Calculating an overall CR by pooling all sessions does not show an overall significant psi-missing effect (87 hits from an expected 100 hits, p=.067) but the absolute deviation from chance calculated by taking the sum of the x^2s is significant (Σx^2=9.76, 4df; p=.045). Analyses of the confidence calls were purely at chance level. (See table next page.)

DISCUSSION

The fact that the predominant effect of this study was psi-missing calls for comment. So far, psi-missing is an effect not usually associated with psi-optimization designs although one experimenter did find such an effect (Parker, 1974). Most evaluation of the psi-missing phenomenon have been devoted to card-guessing studies (Rhine, 1952; Rhine, 1969) and Louisa Rhine has applied the concept to spontaneous cases (Rhine, 1965).

Because psi-missing is believed to be influenced by interpersonal relationships, this discussion will briefly outline the types of interactions which occurred in the lab during this four week experiment. Two groups of hypotheses will be discussed: hypotheses based on ESP functioning to miss the target, and PK based hypotheses which could have effected the random generator during target selection.

TABLE OF TABULATED RESULTS

Subject	Scores (MCE per session=5)	$\bar{x}$	Confidence Calls hits/trials	CR	p value (two tailed)
F.P.	5,5,5,2,5	4.4	3/6 (n.s.)	- .8	n.s.
C.H.	3,5,2,5,3	3.6	6/16 (n.s.)	-1.98	.048
M.S.	5,4,4,2,3	3.6	5/12 (n.s.)	-1.98	.048
S.R.	7,5,5,5,7	5.8	6/12 (n.s.)	+1.13	n.s.
Totals	87 (MCE=100) p=.067	4.35	20/46 (n.s.)	Σx^2=9.76, 4 df, p=.045+	

+The use of this statistic is partially post-hoc as it was suggested as another means of analyzing the results during the course of the experiment.

ESP HYPOTHESES

Two hypotheses can be offered to explain these psi-missing trends: (1) a genuine psi-missing effect generated by subjects gearing their spontaneous imagery away from the target or, (2) extrasensory noise from extraneous ESP signals.

(1) Psi-missing has been known to occur as a product of interpersonal relationships. That is, psi-missing often occurs when subjects are uncomfortable with the experimenter or the experiment. In the above experiment all subjects were co-workers with the experimenter during the 4 weeks of the experiment and this gave ample time for relationships to build up subtly towards a psi-missing set when the actual experiments were run. Working in conjunction with these subjects naturally leads to varied interpersonal relationships, especially when a visiting researcher is introduced into a laboratory. Psi-missing was a topic often discussed during these four weeks which also could have led to a "psi-missing" set for the subjects. In fact, there was a bit of good-humored badgering on this topic. Since we used multiple sessions, subjects were aware of how they were scoring. It could

easily have been that they realized a downward trend early in the experiment. This could have been purely artifactual. Nonetheless, subjects could have then used ESP to avoid the target in order to reinforce the trend. All of these points would not relate to the experimenter (myself) who would be immune to this type of psi-missing tendency due to the vested interest in the outcome of the experiment, and the experimenter was the only subject to score in the positive direction.

Another hypothesis which could account for the result could be what I label 'extrasensory noise'. As is well known in the concept of 'filter theory' (Broadbent, 1958) information gets into consciousness based on intensity, pitch, spacial localization, etc. It may very well be that extrasensory influences are subject to similar principles. That is, that during an ESP test the mind must filter out random extrasensory signals in order to focus in on the required task at hand. Precognitive signals may be very weak ones and a subject initiating ESP may latch on to random extrasensory influences which are more easily available. This could cause displacement, as for example when during a dream telepathy experiment the subject picked up not the target but what the agent was reading peripherally (Ullman, Krippner with Vaugan, 1973). It might also cause psi-missing since the ESP orientation is away from the actual target.

During the precognitive ganzfeld, there was some qualitative evidence that subjects were by-passing the target in order to latch onto images generated by previous subjects. This could be chance and is certainly not a quantifiable observation. Nonetheless, some of these cases of extrasensory noise were rather impressive. For example, for one subject the precognitive target was an aerial night scene of a volcanic eruption. The subject's mentation did hit on the target, though weakly: "A desert scene with mountain in background, looks like a red glow or giant foundry on side of mountain. Not a volcano, but Vulcan. Norse God who forged and Loki, the mischevious God." During the next session the following subject gave no correspondence to the target but reported: "Have imagery of fire coming out of a mountain top and get feeling of Hawaii."

These types of effects were numerous and they lead me to believe that subjects may be confused by several lines of extrasensory information from which they were unable to sort out the required precognitive message. ESP researchers eventually may have to learn to control extrasensory noise levels as we have been doing in psi-conducive state research in our attempt to control sensory noise levels.

PK HYPOTHESES

Since the determination of targets was based on making ten choices on a random generator which was being used also for PK testing, the question arises as to whether the psi-missing was a product of experimenter PK. Of the 4 subjects used, I chose the targets knowing full well the subjects' codings for 3 of them, and all of these subjects scored in the negative direction. When I acted as subject with Michael Smith as experimenter and did not generate the target no psi-missing occurred.

Why should psi-missing be a product of experimenter PK in this experiment?

In the original design of the experiment the target selection was based on a card shuffling plus random cut procedure. A RNG had just been introduced into the laboratory but since these machines seem sensitive to PK I had not wished to use it. Nonetheless, due to the suggestion of Charles Honorton this machine was used to generate the target over my objection that it could be PK-influenced thus weakening the design of the experiment. The resulting psi-missing effect may thus have been an involuntary PK-based protest. (There is one piece of evidence for experimenter PK. During my stay at Maimonides I was looking over several targets in the target pool and one struck me as particularly offensive, yet appealed to me at the same time setting up a typical approach-avoidance conflict. In fact, I spent several minutes looking at the target. I had been examining many targets but this was the only one to really strike me emotionally. The very next ganzfeld session (the next day) this same target was selected after I had generated ten binary choices on the generator. This struck me as a possible PK effect on the target selection at the time.)

CONCLUSIONS

If anything this experiment demonstrates the difficulty in setting up a proper precognitive experiment. In traditional ganzfeld work it was found that a transference relationship developed between subject and experimenter (and perhaps the agent) which no doubt aids in the ESP transfer and detection. Precognitive signals may be too weak in the ganzfeld since no agent is employed as a 'landmark' or direction to the source of the information (Tyrrell, 1947). As in many precognitive tests, PK may also be a factor and the design of this test did not control sufficiently for PK influences on the target choice. The possible types of psi effects discussed above may actually have been working in conjunction to offset results. It is hoped that the presentation of this data will enable experimenters

to more fruitfully design precognitive ganzfeld tests in the future.

ABSTRACT

Four subjects each contributed five precognitive ganzfeld sessions, with instructions to image about a target they would see immediately after the session. The target material consisted of pictures from a pool of 1024, each consisting of any combination of, or lack of, ten features. Each session consisted of the subject coding his imagery according to these ten categories after a period of 20-25 minutes under continual acoustical and visual ganzfeld stimulation. After the session and coding the experimenter generated a target from the pool by generating a coding index on a binary target section of a Random Number Generator. Of the 4 subjects, 2 psi-missed to a significant degree ($p=.048$ two-tailed for each subject) while the other two subjects scored non-significantly. The absolute deviation from chance for the series (calculated by summing the x^2 s of all subjects) was significant ($p=.045$, two tailed). It is suggested that these results could either be due to a genuine ESP psi-miss effect or experimenter PK on the target selection.

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RELATIONSHIPS BETWEEN PK AND PHYSIOLOGICAL VARIABLES IN A GIFTED SUBJECT

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The study of physiological correlates of parapsychological phenomena can be approached in two different (though complementary) ways. First, parapsychological phenomena can be regarded as being a form of behavior (here to be defined as the complex of activities in the physiological system of the organism, elicited or directed by specific stimuli, internal or external to the organism). Regarded in this way, parapsychological phenomena can be studied in a way analogous to research on other aspects of behavior, such as the physiological correlates of thinking (e.g. the study of evoked potentials in relationship to thinking by Chapman, 1973) or the physiological correlates of hallucinations (e.g. the analysis of the electromyogram in relationship to hallucinations by Hefferline e.a., 1973). Basic to this approach is the idea that the phenomena studied, such as thinking or hallucinations, which are apparently of a 'mental' kind, do have behavioral (physiological) correlates which can be detected and studied by means of a systematic research strategy. Thus, the studies of these 'mental' phenomena have in common a body of knowledge concerning the activities of the physiological system. If parapsychological phenomena are regarded as comparable to the mental phenomena mentioned above, the approaches used to study these would be available for use. As the studies mentioned are basically successful in their approach, it would be worthwhile to examine in what way their methods can be also useful to parapsychological research.

The second way to study the relationship between parapsychological phenomena and physiological events might be called 'process analysis'. In this approach little specific is assumed about the physiological variables involved. Rather, several important physiological variables are studied in relationship to the occurrence of a parapsychological phenomenon in order to detect any connection that might appear. Needless to say, a systematic connection is hoped

for.

Of course, in actual research, aspects of both approaches will play a role. The reason for studying the physiological correlates of parapsychological phenomena is that as we begin to attribute psi events to other observables, physiological or physical, we begin to gain at least statistical or predictive control over the phenomena. Whatever the explanation of psi processes will turn out to be, it may be assumed as a working hypothesis that these processes are reflected in the activities of the organism, most probably in the information processing activities of the central nervous system.

Previous work with physiological measures in parapsychology can be classified according to the two approaches mentioned above. Belonging to the behavioral approach studies can be considered which involve physiological measures as dependent variables and which use a model analogous to the studies of subliminal perception and perceptual defence phenomena (see Dixon, 1971).

They assume that information may enter the organism and produce behavioral and physiological effects without entering 'awareness'. Using the EEG, plethysmogram or GSR as physiological response to a physically remote, supposedly paranormal stimulus, several studies have been carried out, with on the whole encouraging results (e.g. Tart, 1963; Sanjar, 1969; Schouten, 1976).

The other major role for physiological variables is set out in studies of physiological correlates of ESP and PK in what has been called here the process analysis approach. Most of these investigations have focused on 'state' correlates of ESP performance. This activity was stimulated by a paper by White (1964), which reviews at length self-descriptions of several exceptional ESP performers.

Important factors in the production of a successful ESP response turned out to be reduction of sensory input, deep relaxation and active, persistent re-direction of attention to the internal environment. Studies of physiological correlates of PK are hard to find. We have to mention the studies of Averill & Rhine (1945) and Rhine e.a. (1945) in which subjects underwent physiological changes produced by respectively alcohol and caffeine. The results of these studies with dice were that alcohol reduced the PK performance whereas caffeine improved it. However, the supposed physiological changes in these studies were not measured. In experiments with PK on the resuscitation of anaesthetized mice Watkins e.a. (1973) found changes in the physiological variables of the subject during successful PK series, viz. decreased pulse amplitude and increased heart rate, respiration rate, irregularity of respiration, alpha block in the occipital EEG and muscle tension in the skeletal muscles.

However, this work does not seem to be replicated. This limited effort on PK studies surprises us, as PK performance can be measured

quantitatively as the objective response of a physical system.

In the present study, a systematic search is attempted for physiological correlates of PK, as exerted on an electronic random number generator. The study can be characterized as mainly a process analysis oriented approach, yet it contains behavioural aspects, as will be exposed in the discussion.

EXPERIMENTAL PROCEDURE

The occasion to carry out this experiment was the visit of Matthew Manning, well known as a PK gifted subject, to Amsterdam in April, 1975.

We have already reported on the complete series of experiments performed on that occasion (Bierman, De Diana and Houtkooper, 1976). One of these experiments was carried out with the purpose of studying the relationship between PK scoring on an electronic RNG and the physiological state of the subject. The experimental setup described by Bierman and Houtkooper (1975) was used with the following modifications: The runs were automatically started with a fixed time interval between runs. The series consisted of 32 runs with the target directions according to the scheme: control-left-control-right. Runlength was the same for all runs: 15 seconds, with a fixed pause before each run of a slightly longer duration (about 16,5 seconds). During experimental runs (those with left or right target direction), the subject was directed to influence the RNG so that the line appearing on the display screen would deviate in the assigned direction. During control runs he would only look at the screen without trying to exert an influence.

The trials generated by the RNG during the run were divided into invisible trials and visible trials. The visual feedback contained only the latter, while both types of trial were recorded. Simultaneous to this series of runs, physiological signals were recorded, viz. the EEG occipital, the ECG and a respiration signal derived from chest impedance measurement.

ANALYSIS

For each run the deviation from chance expectancy in the number of trials corresponding with each target alternative was calculated as a z-score (standard normal score). This was also done separately for the visible and invisible trials. For the experimental runs, the z-scores were supplied with the appropriate sign, according to whether the deviation was in the target direction or in the opposite direction. This results for each experimental run in a $Z_{pk,vis}$ for the visible trials and a $Z_{pk,inv}$ for the invisible trials.

The physiological signals were analyzed by computer for each run and for each pre-run period. From the signals we calculated the values of several parameters derived from the EEG, ECG and respiration signal. These are the following: In the ECG, the R-peaks of the PQRS complex were detected and the time intervals between peaks were measured. From these the mean time interval (pulse interval) and the standard deviation (pulse interval variability) were calculated.

The respiration signal was quite difficult to process because of contamination, mainly by the ECG. We used a combination of analog and digital filtering techniques; the analog filter with a cut-off point at 4 Hz and the digital filter having its first zero at 1.3 Hz, in the neighbourhood of the mean heart frequency. In the filtered signal we detected peaks and valleys by a peak detecting algorithm: True peaks were required to rise above the last valley by at least 15% of the last peak-valley amplitude. The number of respirations calculated in this way is quite small in each section so that the variance will be large in the obtained respiration data.

The following parameter were calculated: the mean of the inspiratory time, the mean of the expiratory time and the mean respiration amplitude.

The EEG was sampled for the first 14 seconds of each section. The Fast Fourier Transform was applied to obtain a power spectrum for each section. The values of the following parameters were calculated: the total energy in the spectrum, the mean frequency of the spectrum, the energy in the delta band (0.5 - 4 Hz), the energy in the theta band (4 - 8 Hz), the energy in the alpha band (8 - 13 Hz) and the energy in the beta band (13 - 25 Hz).

RESULTS

As previously published (Bierman, De Diana and Houtkooper, 1976) we obtain for the overall PK scoring a deviation of -162.5 from chance in the 348051 trials of the experimental runs, with $z = -.65$, indicating non-significant psi-missing. The next step is to look at the z-score per run. These z-scores are squared to obtain a measure of the extremeneness of the deviation from chance, without regard to the direction of the deviation. The sum of the squared z-scores is a measure of overall scoring extremeness, which is chi-square distributed under the null hypothesis. For all 32 runs in this experiment we obtain $x = 50.29$ with 32 degrees of freedom, with $p < .025$. This post hoc analysis has bearing on the prospect in the following analysis of the possible relationship between physiological variables and PK scoring: With some extra variance present, the possibility exists that this is due to a varying PK influence which is related to changes in the physiological state of the subject. The

physiological variables chosen are averaged for the four kinds of period during the experiment, viz. before and during control and experimental runs, see table 1. The number of instances of each condition is 16. In table 1 differences are apparent between the mean levels of the physiological variables in different conditions. To test these differences we apply the Wilcoxon matched pairs signed ranks test, resulting in the z-scores given in table 2. As

TABLE 1

Mean level of physiological variables in different conditions

Variable	Pre-control	Control run	Pre-exper.	Exper. run
Pulse-interval(ms)	761	759	759	738
Pulse-int.var.(ms)	63	58	82	50
Inspiratory time(ms)	2092	2454	1959	2444
Expiratory time(ms)	1459	1843	1986	2313
Respiration ampl.	2355	2810	2871	2763
EEG total energy	20110	20736	20355	21034
EEG mean frequ.(Hz)	8.95	9.00	8.95	8.61
Delta band energy	5167	4954	4826	5266
Theta band energy	4505	4565	4775	5104
Alpha band energy	4279	4374	4432	4067
Beta band energy	4848	5172	4815	4889

a measure of the overall physiological changes we give the sum of the squared z-scores in each case.

However, even with seemingly only randomly fluctuating physiological variables one may expect a correlation between these and PK scoring. In this case the subject experiences by chance a physiological state which is less than conducive to the incidence of positive PK scores. In a previous report on the same experiment (De Diana and Houtkooper, 1976) we presented some correlations between single physiological variables and PK scores. We shall here look for a complex of physiological variables which predict the PK scores. To achieve this, we shall apply a stepwise multiple regression analysis using the program in the SPSS package (Nie e.a., 1975).

By this method, variables are entered successively into a regression

TABLE 2

Tests of difference of level of phys. var. between conditions (Wilcoxon matched pairs signed ranks test)

Variable	Control run vs. exp. run (z)	Pre-exp. vs. exp. run (z)	Pre-contr. vs. contr. run (z)	Pre-run vs. run (z)
Pulse-interval	-0.67	-0.83	+0.21	-0.58
Pulse-int.variab.	-0.93	-2.69	-1.03	-2.71
Inspiratory time	+0.10	+1.24	+0.75	+1.64
Expiratory time	+0.05	-0.21	+1.34	+0.79
Respiration ampl.	-0.57	-0.21	+0.57	+0.22
EEG total energy	+0.57	+1.50	+0.62	+1.40
EEG mean frequency	-1.55	-2.07	+0.21	-1.33
Delta band energy	+0.98	+1.34	-0.57	+0.58
Theta band energy	+1.96	+0.62	+0.31	+0.69
Alpha band energy	-1.34	-1.55	+0.36	-0.93
Beta band energy	-0.98	+0.05	+1.03	+0.79
Σz^2	11.94	20.67	5.83	17.07

equation which forms a predictor of the dependent variables. We use as dependent variable the PK score (z-score) of the invisible trials. This is done to avoid contamination by the visual feedback of the physiological state of the subject. The result of this procedure (see table 3) is a highly significant predictor of the PK score, in which five variables take part. This significance however is not corrected for the fact that the five best variables are selected out of eleven. The difficulty of evaluating this is circumvented by correlating the predictor with $Z_{pk,vis}$, the PK score on the visible trials. This independent test on the consistency of the predictor results in a product moment correlation coefficient of $r = +.427$ with a one-tailed $p < .05$.

DISCUSSION

The picture which emerges from the above results consists of three parts: 1: The PK data; 2: The physiological data; and 3: The relation

TABLE 3

Results of a stepwise multiple regression analysis applied to the relationship of the PK score of the invisible trials with the physiological variables during the experimental run

Variable	Corr. coeff. with $Z_{pk,\ inv.}$	Entered in the regression equation	Normalized regression weights
Pulse-interval	.067	x	.386
Pulse-int.variab.	-.357	x	-.735
Inspiratory time	-.002		
Expiratory time	.346		
Respiration ampl.	.449		
EEG total energy	-.443	x	-.603
EEG mean frequency	-.431		
Delta band energy	.219	x	.210
Theta band energy	-.346		
Alpha band energy	-.362		
Beta band energy	-.572	x	-.426

Multiple R = .919
Test of the regression equation: $F_{5,10} = 10.94$ ($p < .001$)

between both.

1. The PK data.

The overall effect is negative and nonsignificant, while the z-scores of individual runs contain somewhat extreme scoring. The calculated chi-square over all 32 runs is 50.32 with $p < .025$. This means there is extra variance in the z-scores of the runs possibly indicating the occurrence of a mixture of psi-hitting and psi-missing in this experiment. These variations in performance may be correlated with a physiological factor. From the extra variance we expect a correlation coefficient of up to .60, which would have a two-tailed $p < .01$ if occurring only in the 16 experimental runs. Here we feel the use of invisible trials needs some comment. If one adheres strictly to an observational theory of psi, the use of the PK scores on the invisible trials is difficult to justify. Since the subject does not observe these trials, there will be no effect and therefore no relationship between PK scores and physiological

variables will be possible.

We feel that, lacking a suitable alternative method of analysis, this theoretical stringency has to be relaxed. Moreover the development of an observational theory is still far from complete, while the present assumptions following from it restrict the possibilities of testing the relationship between physiological variables. Since we come tentatively to the conclusion that a relationship with the invisible trials exists, either the observational theory does not hold in the present form, or the scores are due to an experimenter effect.

2. The physiological data.

The changes in mean level of the physiological variables from one condition to the next, as given in tables 1 and 2, indicate the behaviour of the subject, viz. his reaction to the test situation. The possible causes of any change in physiological state are of interest here. The following list may give an idea of the possibilities:

a) The subject tries to exert an influence during the experimental runs (effort).
b) The subject exerts himself, but he also enters a PK conducive state.
c) The functioning of Pk causes a physiological reaction, e.g. like exhaustion.
d) The existence of feedback causes perceptive changes, e.g. like photic driving or a change of attention.
e) The contents of the feedback cause emotive changes dependent on success or failure.
f) The contents of the feedback cause the subject to change his effort.
g) Unrelated mental or bodily processes cause physiological changes.

From the Σz^2 values we conclude that the most sizeable change occurs between the pre-experimental run period and the experimental run, although it is not much above the level of the random fluctuations.

This change consists most notably of a lowering of both the pulse interval variability and the mean frequency of the EEG. The cause of this change may be the effort of the subject, or equally a perceptive change due to the presence of feedback. The corresponding changes for the control runs instead of the experimental runs are not significant at all. Also, the change between control runs and experimental runs could be attributed to chance. Hence, the only change present seems to indicate a specific reaction to the start of the experimental run. This change should be compared with the findings of Watkins e.a. (1973) mentioned earlier. Their increase

in heart-rate corresponds with our shortening of pulse-interval ($z = 0.83$) and there is also agreement on the decrease of energy in the alpha band ($z = 1.55$). Our results disagree with the increase in respiration they registered, but not significantly so, while our respiration data have large variances due to the short durations of the experimental sections. However inconclusive this comparison may appear to be, we note that these changes can be considered as behaviour on the part of the subject stimulated by the test situation. This behaviour may turn out to be situation-specific as well as subject-specific and not directly related to the functioning of PK.

3. The relation between PK scoring and physiological variables.

The results of the stepwise multiple regression analysis indicate a consistent complex of variables related to PK scoring on the invisible trials.

The statistical treatment was contrived afterwards to test the general hypothesis on this experiment, that there is a relation between PK scoring and the physiological state of the subject. The present method seems to be a rather straightforward realization of this hypothesis when considering the behaviour of the PK scores and the physiological variables by themselves. Moreover, the correlation of the predictor with the PK scores of the visible trials constitutes a cross-validation on the complex of variables found. However, the physiological variables are related to the visible trials by the possible emotive reaction of the subject to the feedback. This means that cross-validation is hindered by contamination related to the visible trials. Nevertheless, the correlation between $Z_{pk,\ vis}$ and the predictor of $Z_{pk,\ inv}$ turns out to be significant ($p < .05$).

This convergent evidence brings us to the conclusion that the predictor found is truly related to PK, and no mere artifact. When interpreting this relationship, we note that the PK scores contain positive as well as negative scores so that a high value of the predictor corresponds with highly positive PK scores and low values of the predictor with highly negative PK scores. Thus it cannot be said that the predictor is indicative of a more or less PK conducive state. Rather, the predictor is indicative of a state in which positive scoring, or in the opposite state negative scoring, is preferentially occurring. The variables in the predictor have moreover a relationship with the variables that show a change from pre-experimental period to experimental run. The strongest related variable, pulse interval variability, shows low values when positive PK is occurring, while it lowers significantly at the start of the experimental run.

Less clearly perhaps, the relationship of PK with the energy in

the delta and beta bands (see table 3) is in line with the lowering of the EEG mean frequency at the start of the experimental run (tables 1 and 2). These are indications that the systematic changes in the physiological state during the experiment are related to the state corresponding with positive PK found in the experimental runs only. Referring to the list of possible causes of physiological changes given earlier, it seems most likely that item 2, the subject exerts an effort and at the same time enters a PK conducive state, is indeed occurring, if "PK conducive" is taken as conducive to positive PK scores.

From the gifted subject that cooperated in this experiment, such behaviour can be expected. On the other hand, his overall performance is as unsatisfactory as that of most ordinary subjects. It remains to be seen whether or not the same results with physiological correlates could be obtained with unselected subjects, who admittedly have no experience of consciously exerting PK, but who, curiously enough, do not have any difficulty in applying their "will" in the experimental setting. Therefore it seems worthwhile to replicate the present experiment, possibly with unselected subjects and possibly with some modifications. In this respect we have already mentioned the invisible trials. Results on these seem to be in contradiction with the observational theories. Other questions that remain to be answered are: To what degree are our results subject-specific and situation-specific; what is the role of the time-scale of the physiological changes involved and do observed physiological changes precede, follow or are they concomitant with the occurrence of PK? The methodology of this type of research should be developed further in order to be able to attempt answering these questions.

ABSTRACT

Physiological correlates of PK performance were studied in an experiment in which a gifted subject, Matthew Manning, tried to influence an electronic random number generator. At the same time physiological signals (EEG, ECG and respiration) were recorded. Overall PK scoring was not significant. However, in the 16 experimental runs there is apparently a consistent relationship between a complex of physiological variables and the PK scoring on the invisible as well as on the visible trials. The variables in this complex seem to be related to the behaviour of the subject in the test situation, in the form of physiological changes at the start of the experimental runs. The findings, tentatively summarized as the occurrence of a PK conducive physiological state in the subject, are discussed in the light of methodological problems encountered in this research.

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ANNOUNCEMENTS AND COMMENTS

THE SECOND INTERNATIONAL SPR CONFERENCE ON PSYCHICAL RESEARCH

PRELIMINARY NOTICE AND CALL FOR PAPERS

Following the success of the 1st International Society for Psychical Research Conference last April, a 2nd International Conference will be held, in conjunction with the Cambridge University Society for Psychical Research, at Trinity College, Cambridge, from Tuesday 28th March to Thursday 30th March 1978. Accommodation and meals will be available in Trinity for those who require them.

Booking forms and other information will be sent out with the December 1977 Journal but interested overseas persons may write for prior information to Dr. B.J. Carr (ref. SPR Conference), L2 Nevile's Court, Trinity, Cambridge. There will be reduced registration fee for members and associates of the SPR.

Any person may submit a paper for possible presentation at the Conference. Papers may be on any aspect of psychical research and they should aim to be presented in not more than 30 minutes to allow time for discussion. There will be no research briefs and no simultaneous sessions during the Conference. Lots of opportunity will be provided for informal interchange of views.

Summaries of proposed papers must reach the Programme Committee by 1st January 1978. They should be sent to C.L. Sargent (ref. SPR Conference), Psychological Laboratory, Downing Site, Tennis Court Road, Cambridge. The summaries should be double-spaced and should not exceed 300 words. Authors of papers which are accepted will be informed by the end of January.

RESEARCH PROJECT BIOCOMMUNICATION FOUNDED AT THE TECHNICAL UNIVERSITY BERLIN

Pertaining to the motion of the Research Group Parapsychology which closed down in 1976 the Technical University Berlin has established an interdisciplinary Research Group Biocommunication. This group

will work on examining the phenomenon of telepathy theoretically and experimentally with help of statistical methods. For this purpose a new research method has been developed that is based on using today's newest data-processing facilities. The research group consists of statisticians, physicists, physicians, electronics specialists, psychologists. The time established has been set for 3 years.

Forschungsinstitut für Parapsychologie / Psychotronik e.V.
Postfach 3310
1000 Berlin 30

G. ZORAB. "PARAPSYCHOLOGICAL DEVELOPMENTS IN THE NETHERLANDS." E.J.P. 1, 3, 57-83.

We received from the Dutch Society for Psychical Research a comment on the above-mentioned article objecting towards the way Mr. Zorab depicts the history of parapsychology in the Netherlands. Readers who are interested can acquire a copy of these comments from

Secretariaat
Studievereniging voor Psychical Research
Eemwijkplein 16
Voorburg
The Netherlands.

CORRECTION OF LAST ISSUE NUMBER

On the cover of the last issue (May 1977) the volume number has been incorrectly given as volume 2, part 1. It should be volume 1, part 4. The present issue is the first part of volume 2.

CORRECTION TO "CORTICAL AROUSAL AND PSI: A PHARMACOLOGICAL STUDY" (E.J.P., 1, 4, 72-79)

I should like to correct one error in this paper, which arose in typing and which stood uncorrected in the published paper since proofs were not sent to me before publication. On p. 78 'barbiturate' should read 'depressant drug': diazepam is not a barbiturate but a

benzodiazepine currently replacing barbiturate in medical practice.

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CONTINGENT NEGATIVE VARIATION AS AN INDEX OF PRECOGNITIVE INFORMATION

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The contingent negative variation (CNV) is a slow, surface-negative electrical brainwave studied in experiments which emphasize the association (contingency) of two successive stimuli. It was first described by Walter et. al. (1964) who used the term 'expectancy' to describe the primary psychological variable underlying its development. In the present study it is used as a physiological index of information about a future event. The merit of this kind of parapsychological experiment is that no consciously mediated response is sought from the subject. Instead, the brainwave is examined for evidence of paranormally derived information. The principal disadvantage of the method lies in the difficulties associated with recording and analysing the CNV data. This paper describes in detail some novel strategies used to cope with these demands in the present experiment.

At least one previous series of experiments have been done in which the CNV was assessed for its usefulness to parapsychology (Levin & Kennedy, 1975; Kennedy, 1977). No significant results were obtained. Grey Walter himself (1970) was the first to suggest the appropriateness of such an investigation, and the a priori strength of the idea seemed sufficient to warrant at least an additional experiment. It might also be noted that the past few years have seen a marked increase in the quality and sophistication of the psychophysiological laboratory facilities available for parapsychology experiments. This is expecially true at the University of Utrecht. Thus it recently became feasible to conduct a very careful experiment, improving upon the earlier attempt in design, instrumentation and analysis.

In the usual investigation of CNV, the first stimulus serves as a warning or preparatory signal. It is followed a few seconds later by the second or 'imperative' stimulus which (usually) requires that some definite response be made by the subject. The negative

Note: This publication fulfils the publication policy as stated on the page opposite this page.

shift in the electroencephalogram (EEG) during this interstimulus interval is the CNV. Tecce (1972) has reviewed early research relating the form and size of this shift to psychological processes in man. He concluded that attention to the anticipated occurrence of the imperative stimulus is the essential variable governing the magnitude of the CNV.

In more recent years at least two distinct component waves have been identified by the use of longer interstimulus intervals (Loveless & Stanford, 1975). Functional and topographic differences allowed Rohrbaugh, et.al. (1976) to distinguish between an early wave elicited by the warning stimulus and a later wave identified with preparation for a motor response. The later wave is maximal over the motor cortex contralateral to the responding hand and shares critical features with the motor readiness potential first described by Kornhuber and Deeke (1965).

The earlier wave has maximal negativity over the frontal regions and represents a response to the warning stimulus. Positive waves of a similar time course are elicited at parietal sites. These latter responses have been studied extensively as indicators of stimulus relevance. A critical review of this research has been published by Näätänen (1975). It is known that evoked responses of this latency can be influenced by stimulus meaning, as distinct from such sensory parameters as intensity or form (Sutton, et.al. 1967).

BACKGROUND

It is to be noted that the precise waveforms obtained in slow potential experiments are highly individualized. Signals derived from one subject under two experimental conditions can be expected to show more similarities than signals obtained from two subjects under the same condition. Consequently there are important advantages to experimental procedures which emphasize comparisons across conditions within subjects. These advantages are generally to be weighed against the hazards of offering to many experimental conditions to each subject. Typically a dozen or more artifact-free trials are needed within each condition if a reliable estimate of the CNV signals are to be formed.

It may be conjectured that a similar tradeoff exists in the way in which several experimental conditions influence performance on a parapsychological task. The presence of a few psychologically similar conditions may be desirable if they introduce benificial demand characteristics. Correct responses may then often be expected within the experiment as a whole, and no great distinction

need be made between psi-mediated trials and those of other types. Thus it is hoped that the several experimental conditions will contribute to a psychological environment in which the subject has 'permission to score' on the psi task. This advantage is once again balanced by the boredom of long experimental regimens and the seeming affinity of psi for novel situations. Thus both technical and psychological factors recommend a procedure with few experimental conditions for each subject.

EXPERIMENTAL DESIGN

The present experiment is organized as a precognition or psychokinesis study. It differs from most other parapsychology experiments in several ways. First, no consciously mediated response or guess is required. The analysis is entirely concerned with examining a subject's preparation for the imperative stimulus as reflected in his brain waves. Second, the subject is given a reaction time task and a psychological set that strongly condition him to focus upon sensory input. This may be considered a disadvantage, as there is a widely held belief backed by experimental data that paranormal phenomena are more likely to be manifest in situations which minimize the importance of sensory input by focussing a subject's attention internally (see, for instance, Honorton 1977). Finally it may be noted that the present study is a highly automated one. All data collection and delivery of stimuli are under the programmed control of a medium-sized laboratory computer. As they are gathered the data are stored on magnetic discs for subsequent multivariate statistical analysis on the same machine.

The study was designed to contrast two situations which are quite dissimilar in terms of the response expected from a subject. In one instance he glimpses a picture of a person opposite his sex and responds as quickly as possible by pressing a button. In the other instance the picture is of a person of his own sex, and no motor response is made. These two types of pictures (projected transparancies) serve as the imparative stimuli for a CNV study.

The warning stimuli are also projected transparancies, each of which provides one of three distinct levels of information about the imparative stimulus which follows it. These slides show a stylized face smiling, frowning, or with no mouth at all. They are equalized for overall brightness, and are projected briefly (about 150 milliseconds). A smiling face implies that an imperative stimulus depicting a person of the opposite sex will follow, so that the subject should prepare himself to make a rapid motor response

upon its presentation. A frowning face means that the imperative stimulus will depict someone of the same sex so that no motor response will be required. A face without a mouth may be followed by either sex.

One third of the warning slides are of high contrast so that the subject is fully informed about the imperative stimulus which follows. One third of the warning slides are of very low contrast so that the subject is less well informed about the imperative stimulus. The brightness of the projected image is adjusted to yield a scoring rate of 75% in a preliminary forced-choice guessing test using only the low contrast smiles and frowns. Thus these stimuli are said to be of a low information level. The final third of the warning slides are also of low contrast but show no mouth so that they provide no information about the imparative stimulus to follow. Trials using these warnings constitute the parapsychological or 'ESP' portion of the experiment.

A set of six CNV's are estimated for each subject. These correspond to the two kinds of imperative stimuli with each of the three levels of information. Previous research (Kennedy 1977) made it seem unlikely that a multivariate test would reveal significant differences between the two ESP conditions. Consequently the differences found at the other levels of information are used to generate a specific univariate hypothesis about the differences to be expected for the ESP trials. In this way the analysis is tailored to the kinds of CNV differences that seem relevant to each individual subject. Including an intermediate level of information allows a measure of the consistency of the CNV differences as information is decreased and the subject becomes uncertain about the response he must make.

The low information trials also serve to establish a psychological context for the ESP trials. Subjects are often uncertain about whether a given warning slide provides information or not as well as being uncertain about the sex of the person to be shown in the imperative stimulus. Nevertheless, the subject knows that some information is often provided by such slides as he has been able to guess them correctly more often than chance would predict. Consequently the instructions encourage him to prepare his response based upon whatever clues he might have.

A total of 160 trials are given in four runs of 40. A circular tray holds the slides in a predetermined order. In fixing this sequence a shuffling procedure based upon the FORTRAN subroutine RANDU is used to intermix the types of warning stimuli. These slides are apportioned as follows: 7 high contrast smiles, 7 high contrast frowns, 7 low contrast smiles, 7 low contrast frowns, and 12 low

contrast faces without mouths. These slides are placed in alternate positions in a tray with a total capacity of 80 slides. An appropriate imperative stimulus is paired with each warning slide which shows a smile or a frown. This slide immediately follows the warning slide in the tray. Two imperative stimuli (one depicting a person of each sex) are associated with each warning slide which shows no mouth. These immediately precede and follow it in the tray. A Schmidt-type random number generator employing a high frequency noise diode is used to determine which of these slides is shown. This choice is made immediately after the warning slide is projected. Ordinarily the projector is stepped forward after each slide is shown. A proper choice by the random number generator, however, causes the computer to move the projector backwards to the preceding slide. If this occurs, the projector is advanced three steps following this presentation instead of the usual one. The entry point in the tray is randomly determined at the start of each run in order to provide a modicum of variety to the sequence of warning stimuli. The imperative stimuli depict a number of different individuals in a variety of poses in order to make the experiment as interesting as possible.

Considerable care is used in shielding the subject from sounds made by the projector which could inform him of the direction of its movement. A rear projection system is employed that allows the projector to be located two rooms away from the subject. The windows in both intervening walls are doubly glazed to improve sound attenuation. With the doors closed, no projector sounds were audible to the laboratory staff when seated in the subject's chair. As an added precaution, however, a white masking noise was introduced to the subject's room. This has the extra benefit of aiding concentration by covering extraneous noises that may arise from the building or street. It is to be noted that the type of imperative stimulus selected in the case of the ESP trials depends not only upon the direction of projector movement, but also upon the type of imperative stimulus which immediately precedes this warning slide in the tray. This latter fact is usually determined by the preceding warning slide, but in case it also showed no mouth the situation is determined by the trial which preceded it, and so on.

A small red lamp glows near the center of the screen throughout the experiment to provide a place for the subject to fix his gaze. Weerts and Lang (1973) found this technique helpful in reducing eye movements during the interstimulus interval. A timing diagram showing the events for each trial is given in figure 1. The period between trials is approximately seven seconds, varying slightly from trial to trial.

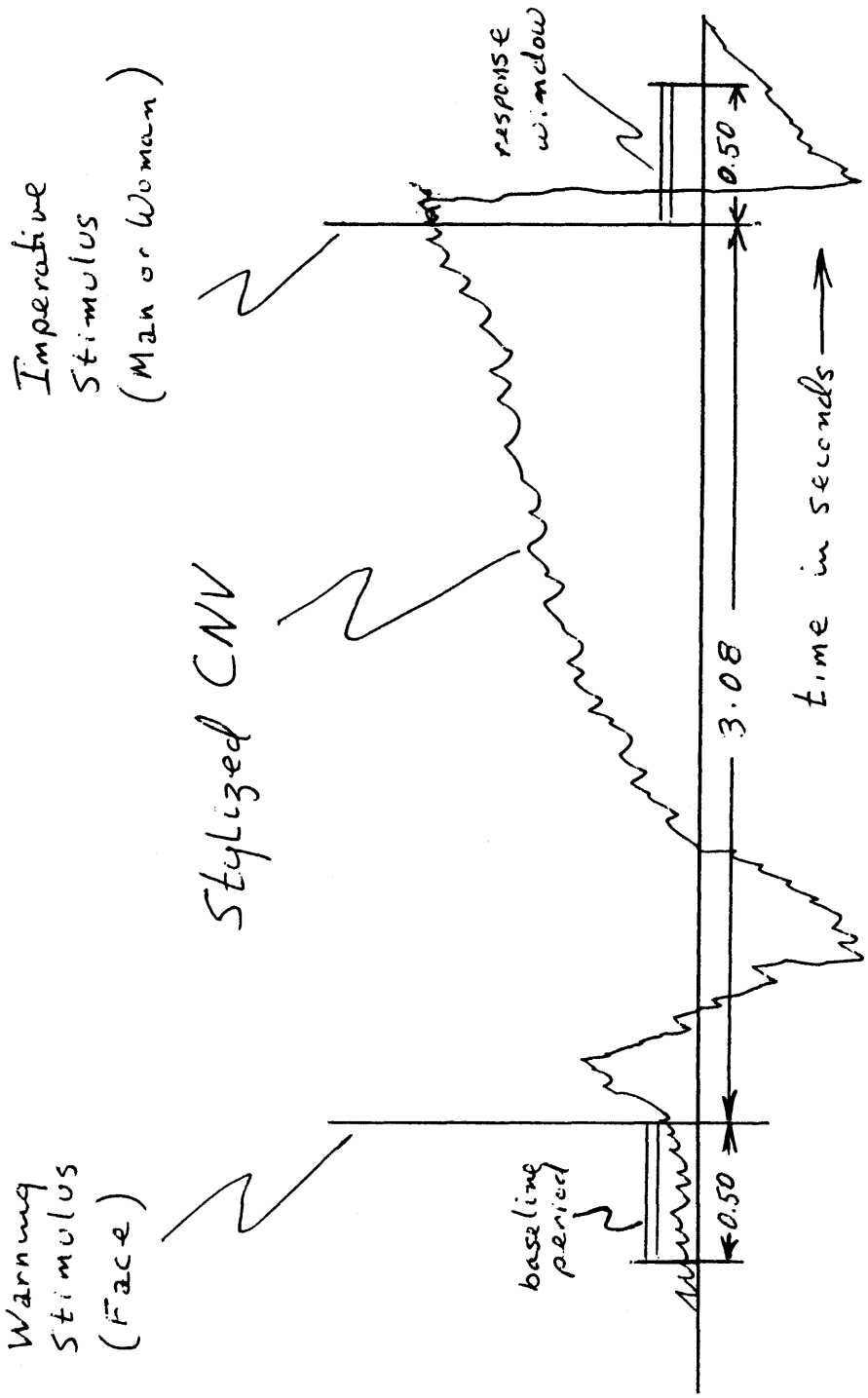


FIGURE 1

Timing diagram for each trial

EEG is recorded from three electrodes located along the midline in frontal, central, and parietal positions (F_z, C_z, and P_z according to the International 10/20 system). An additional electrode located just above one eye is used to detect blinks and eye movements. The electrodes are of the silver cup variety and are carefully chlorided and maintained. They are held off the skin by means of an epoxy coating applied around the lip. All four channels are referenced to yoked ear lobes and are amplified using a low frequency time constant of 10 seconds and a bandwidth of 35 Hz. (Above 35 Hz the amplifiers roll off very sharply to reject 50 Hz noise from the power lines.) EEG and eye data are displayed on an oscillograph and are digitized by the laboratory computer at the rate of one sample every 7.0 milliseconds (about 142 samples per second). A total of 512 samples are gathered from each channel during the half second baseline period and the 3.08 second interstimulus interval of each trial.

The computer program which controls the stimuli and samples the physiology also checks for contamination by artifact. Data for each trial are first inspected to ensure that they lie within the useful analog to digital converter range. If so, they are examined for eye movements or blinks. If excursions in the eye channel exceed a criterion value set separately for each subject, the trial is disregarded. The program next applies a fast Fourier transform to each EEG channel and examines the power spectra for evidence of muscle artifact. If any channel contains greater power above 15 Hz than below, it is judged to be contaminated, and the data from all electrodes for that trial are omitted from further analysis. The first four trials from each run of forty are thought of as 'practice' and are similarly discarded by the program. Trials on which the subject makes a premature button press are also excluded. Data from trials judged by these criteria to be free from artifact are reduced to 256 points per channel by digitally filtering above 35 Hz. They are re-transformed, and stored on a removable cartridge disc pack for subsequent off-line analysis.

SUBJECTS

Ten subjects (six males) were chosen from those volunteering. Their selection was governed in part by convenience in scheduling the use of the laboratory facilities. All subjects were previously acquainted with the nature and purpose of the experiment. Some were laboratory personnel. Others were family or friends of the experimenters. Most of the volunteers had previous experience as subjects in psychophysiological experiments. As a group they

displayed a high tolerance for the minor discomforts associated with the electrodes and the experimental procedures. Most were very cooperative in holding eye movements and blinks to a minimum, although this occasionally proved to be a difficult task.

One child aged eleven years was included in the group. The others spanned the range from 18 to 48 years. One subject reported a recent serious head injury with attendant EEG slowing; however, no abnormality was evident from the data of this experiment.

The subjects were welcomed as collaborators in the research. Their questions about the experiment were answered as fully as possible, and their occasional comments or suggestions were taken seriously.

PROCEDURE

Experimental sessions were held during the evening when the building is quiet. Normally three persons were present in addition to the subject. The author was present on all occasions. He was usually assisted by two advanced students. These women welcomed the subject in English or in Dutch, applied the electrodes, read the standardized instructions and generally tried to provide a hospitable setting. The entire experimental procedure consumed about two and one half hours per subject.

Upon arrival the subject was led to the experimental room to begin a fifteen minute period of eye adaptation seated in front of the back-lighted opaline projection screen. This period provided an occasion for music and light conversation. The purpose of the preliminary forced-guessing experiment with the low contrast smiles and frowns was explained to the subject and some example slides were shown.

The forced-guessing task was then given with 34 low contrast smiles and frowns presented in a pseudo-random order. If the subject scored significantly above 75% the brightness of the projected images was reduced, or if he scored below it was increased, and the test was given again (with the slides in inverted order). A satisfactory scoring rate (22 through 29 correct) was usually achieved after only a couple of iterations.

The subject was next taken to another room for electrode application. Seven electrodes were applied after a thorough cleaning of the sites with acetone. The three electrodes along the midline were affixed with collodion. The remaining electrodes were applied to the skin with adhesive collars. An electrode paste containing sodium chloride was injected into each electrode through a small hole, and each site was lightly punctated until its impedance to

the earth electrode was reduced below 10 Kilohms as measured at 15 Hz. The application of electrodes generally required at least half an hour. The time was passed as pleasantly as possible with music, coffee and conversation.

Electrodes in place, the subject returned to the experimental room for another period of eye adaptation. During this time he was read a set of standardized instructions (see Appendix) in either Dutch or English and shown examples of both the warning slides and the imperative stimuli. Finally he was given twenty to thirty practice trials, offered feedback about eye blinks, and encouraged to respond with the button presses very quickly.

The first two experimental runs of forty trials each were given next separated by only a brief rest. The second two experimental runs followed a longer interval. Some subjects left the room during this break and consequently required an additional period of eye adaptation. Feedback about reaction times and the number of trials lost to artifact was given at the end of every run.

ANALYSIS

All analyses were carried out on seven-variable representations of the CNV waveforms. The seven variables chosen were the dc component and both phases of the first three Fourier frequency terms. These variables were selected following the pilot study in which they appeared to give stronger results than a comparable number of variables formed by averaging over adjacent time points. Although these variables permit the general shape of the CNV waveform to be resolved from the background EEG efficiently, they would not allow a detailed analysis of its faster components such as the evoked potential which follows the warning stimulus. In fact no separate analysis of this portion of the curves was carried out for lack of a well-framed hypothesis.

An extensive analysis procedure established before any data were collected was applied separately to each of the ten subjects and each of the three electrode sites. This procedure aimed at classifying the CNV's from a subject's ESP trials using a reliable discriminant function derived from trials on which the warning stimulus provided some information. This was not always possible to do, as it sometimes happened that no suitable discriminant function could be fashioned. For this study a discriminant function was deemed reliable if it were derived from data showing a multivariate difference significant at the 0.05 level. Nineteen of the thirty electrodes yielded data with suitable differences. Every subject contributed at least one such electrode to the analysis, and some contributed all three.

The central electrode position provided the greatest number of reliable discriminant functions, eight, while the parietal site provided six and the frontal five.

It was thought on à priori grounds that the ESP trials should more resemble the low information ones than those for which full information was provided. Consequently it was desired to base the discriminant function upon these trials whenever that proved possible. Eight of the nineteen discriminant functions were obtained from the low information trials. When a reliable discriminant function could not be obtained from them alone (i.e. when they did not themselves show significant differences), an attempt was made to base one upon the combined low information and full information trials. Such combined data provided ten of the discriminant functions. The full information trials were viewed as being somewhat more distantly related to the ESP trials. A reliable discriminant function was sought from them only when one could not be obtained from data which included the low information condition. Only one discriminant function was in fact obtained from the full information trials alone.

The discriminant functions were applied to the CNV's recorded at the corresponding electrodes during the ESP trials. In this way a single number was attached to each such trial indicating its position along the dimension which best separates the trials on which the discriminant function was based. A t-test on these numbers then reflects the degree of separation found between the two kinds of ESP trials along this same dimension. In this way the analysis seeks to determine if the ESP trials show differences of the same kind as those evident on trials where information was provided.

RESULTS

The experimental procedure was successful in producing well-developed CNV's. In the case of the full information trials the averaged waveforms generally showed clear differences of the expected type when visually displayed. The low information trials were less clearly distinguishable for most subjects, and visual difference were rarely apparant for the ESP trials. The averaged waveforms for one subject are shown in figures 2,3, and 4. The upper (more negative) traces in the photos of figure 2 correspond to the opposite-sex situation in which a rapid response was made by the subject. Some differences are also discernable here in the positive components of the wave evoked by the warning stimulus. These may also be seen in the averages for the low information trials in figure 3. The latter effect was not consistently observed

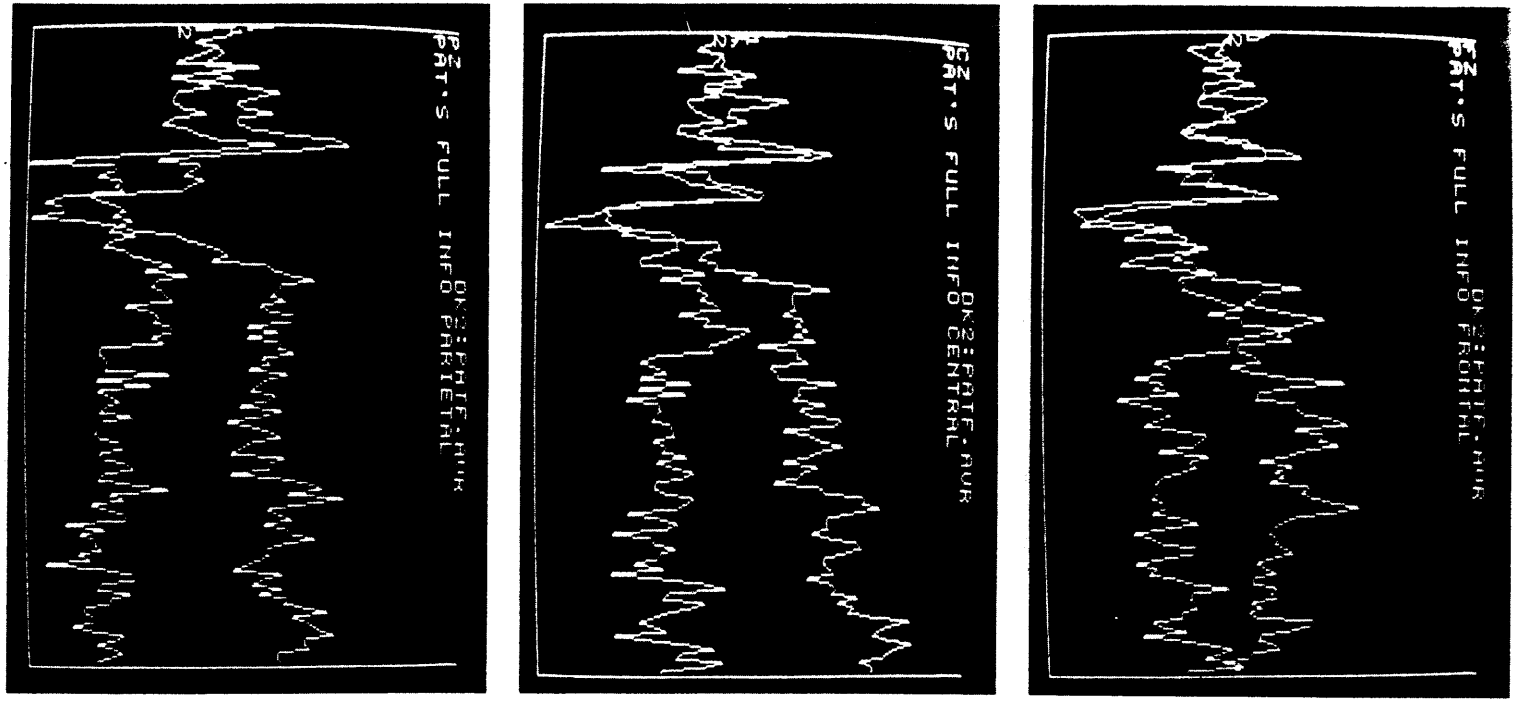


Figure 2

Averaged CNV waveforms for one
subject's full information trials

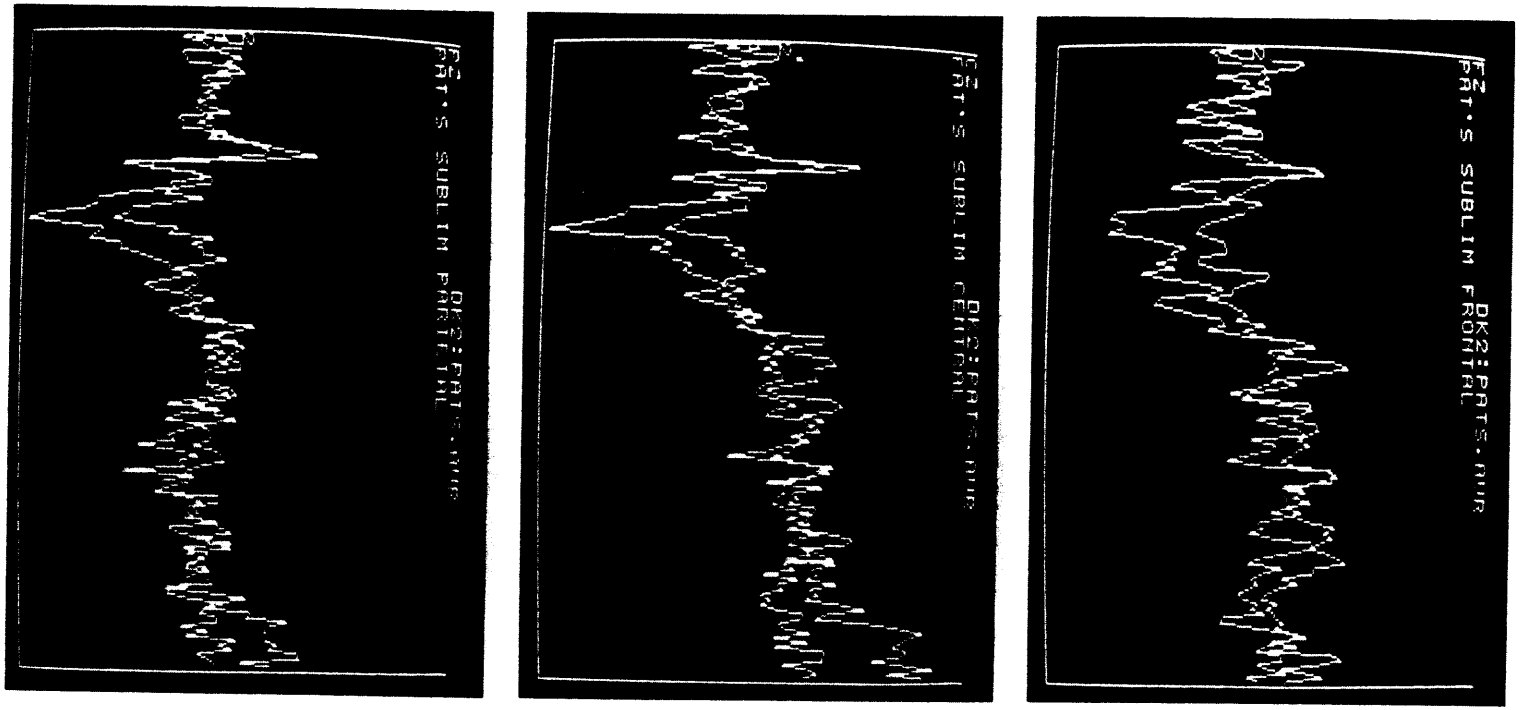


Figure 3

Low information trials - same subject

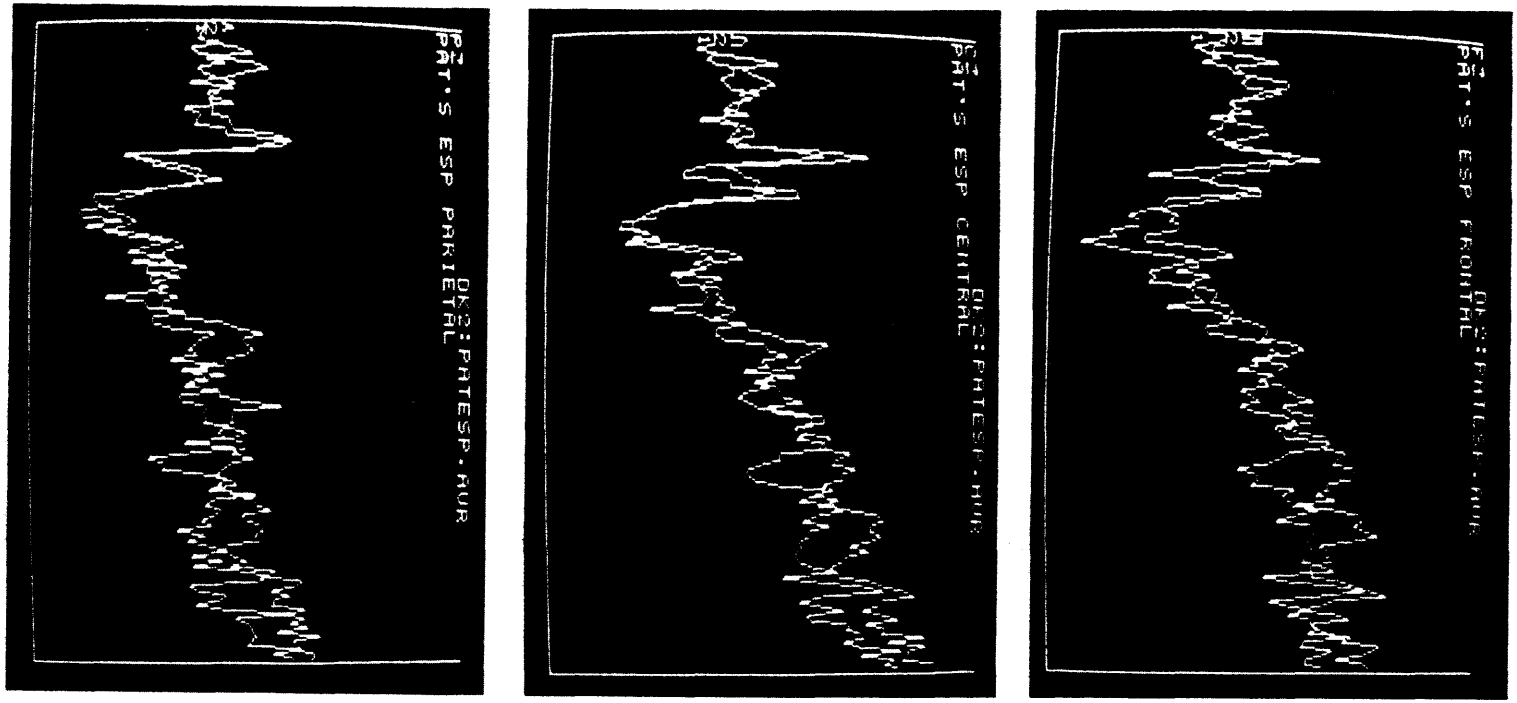


Figure 4

ESP trials - same subject

across subjects.

Nineteen classification tests were carried out and their results are displayed in table 1. The degrees of freedom shown for the subjects are derived from the number of ESP trials classified. This varied from subject to subject depending upon the number of trials identified as contaminated by artifact and omitted from further consideration by the data-gathering program. Nevertheless each entry might be conveniently regarded as approximately a z-score. The literal notation following each entry designates the source of the discriminant function used in the classification (F for full information, L for low information, and FL for their combination).

The entries in each column of table 1 are independent. It can be seen that the column means do not depart significantly from zero. Thus no single electrode site showed evidence for classifiable CNV differences in the ESP trials. A more inclusive multivariate analysis adequately accounting for the correlations along the rows of the table is not possible owing to the large number of vacant cells. Only one entry in the table has an à priori probability of less than 0.05. It is not unreasonable, on the chance hypothesis, to encounter such a value among nineteen entries.

A post-hoc examination was made for evidence of PK upon the random number generator. It was found that same-sex imperative stimuli were selected following the ESP warning slides more often than expected. This effect derived almost entirely from the first run for each subject where the rate was 62% (p less than .01). The

Table 1

Classification of ESP trials by discriminant functions

subject	df	F_z	C_z	P_z
BM	32	-1.338 (FL)	-0.243 (FL)	nc
MJ	31	nc	0.207 (FL)	nc
LS	21	nc	-0.930 (L)	-0.445 (L)
JK	30	0.600 (FL)	1.604 (FL)	0.890 (FL)
HB	34	nc	-0.667 (F)	nc
PH	41	0.326 (FL)	0.210 (L)	0.047 (FL)
MS	33	nc	"2.226 (L)	1.693 (L)
SH	23	0.734 (L)	0.448 (L)	0.013 (L)
TH	31	0.600 (FL)	nc	nc
WH	40	nc	nc	-0.662 (FL)
means		.1844	.3568	.2560

" $p < .05$

random generator has been thoroughly studied in other laboratory operations, and the effect may be a genuine one.

DISCUSSION

This study provides no evidence in favor of the psi-related hypothesis it was designed to test. Thirteen of the nineteen classifications of table 1 are in the predicted direction, but any real effects fall well below the sensitivity of the experiment. The procedures were sufficiently sensitive, however, to reveal the presence of information in the CNV's to the low contrast stimuli. An analysis to show this was undertaken in order to demonstrate the à priori reasonableness of the ESP hypothesis. Briefly, this analysis used discriminant functions derived from the full information trials to classify those with low information warning slides. In this way it was hoped to show that the CNV's continued to encode information in the absence of certainty, thus making them candidates to expose this situation in the ESP trials. In fact, this analysis succeeded (see table 2) even though very few electrodes provided significant differences among the full information trials alone. The reasons for this limitation were explored, as they are

TABLE 2

Classifications using discriminant functions from the full information trials. The data show the decreasing discriminability of the CNV's as less information is provided in the warning stimulus

electrode	subject	full	low	ESP
F	BM	4.609	1.322	0.191
P	PH	7.187	1.911	-0.047
C	BM	6.320	1.929	0.571
C	HB	4.828	0.749	-0.667
C	JK	4.960	2.282	0.957
C	PH	5.143	2.729	-0.629
C	mean	5.313	1.922 (p=.02)	0.058

factors which limit the sensitivity of the method generally. Two major sources of difficulty were identified in what was necessarily an entirely post-hoc proceeding.

Small but consistent eye movements sometimes obliterate the CNV differences. Following a clearly-seen frown, some subjects often lowered their gaze, inducing a negative potential in the EEG electrodes which resembles the negative CNV shift that appropriately follows smiles. It is thought that the tendency to consistent eye movements could be reduced by making the back-lighted screen dimmer and hence easier to look at. As this would increase the recognizability of the low contrast warning stimuli, new slides compensating for this change would also be required. A brighter and smaller eye fixation lamp might also reduce the prevalence of eye movements.

Several of the Fourier coefficients used in the present analysis did not significantly aid in discriminating the CNV's at any information level. Their presence only serves to increase the variance in all tests and to increase the degrees of freedom for the hypothesis in the multivariate tests. Eliminating them would strengthen the study in two ways. First, the increased significance levels would allow discriminant functions to be constructed for more electrodes. Second, the increased signal to noise ratio would permit better classifications to be made.

It is possible to fashion an analytic measure of the sensitivity of the total experiment. This measure inextricably mixes limitations which derive from experimental procedures poorly suited to the production of discriminable CNV's with those that are caused by less than optimum analysis procedures. Nevertheless a measure of the overall ability of the experiment to find an effect like that hypothesized (i.e. to reject the null hypothesis) is important, especially given the negative results obtained.

The hypothesis is that CNV's to the ESP trials will display differences of the same type as those seen when information is provided. Consequently it is desired to know how small this resemblance might be and still be detected. One way to do this is to assume a significant result and to work backwards from the answer. For the central electrode , for example, eight classifications were made so that the chance hypothesis could have been rejected for an average classification t-value exceeding 1.19. The ability of the discriminant functions used to reveal differences of the type hypothesized was tested by applying them to the training data. The resulting t-values were corrected for the differing numbers of trials for comparison with the ESP requirements. At the central elctrode these corrected values averaged 4.62. Now the

background EEG (which contributes the variance) is by hypothesis the same for both kinds of trials. Consequently it would be possible to detect a difference in CNV's like that seen in the informed trials but reduced in magnitude by the ratio 1.19/4.62, or about 26%.

It is clear that this sensitivity could be improved either by experimental procedures which produced larger and more consistent CNV differences or by a selection of analysis variables which better extract these differences from the noise. The parietal suffers from fewer attempted classifications and the differences there would have needed to excede about 32% of their size in the training trials to have been seen. The frontal electrode not only had fewer attempted classifications but also had generally weaker differences in the trials used to generate the discriminant functions. The sensitivity there to reject the null hypothesis was about 49% of the informed differences.

It is also possible to interpret the results actually obtained in these terms. The mean classification statistic was in the predicted direction at each electrode. These means fall consistently below the corresponding significance thresholds and are reasonably attributed to chance. They might nonetheless be taken as estimates of differences actually present in the CNV's for the psi-mediated trials. The frontal differences would then represent about 23% of those seen on the training trials, the central about 5%, and the parietal about 10%.

It should be remarked that a variation on this experiment was also developed and tested with three pilot subjects. It sought to combine the CNV analysis reported here with a forced-choice guessing task by requiring subjects to respond to a neutral imperative stimulus (extinguishing the eye-fixation lamp). The photograph of the man or woman was offered one second later as feedback about the correctness of the guess. This test was self-paced as the subject could initiate each new trial at his own discretion by pressing a button. It was conjectured that these differences would serve to increase the cognitive involvement of the subjects in the performance of the task. Poorly developed CNV's were obtained, and the procedure was judged by the author to be excessively tiresome. It was consequently abandoned in favor of devoting additional attention to the task reported here.

These results do not encourage the widespread adoption of this experimental technique. The difficulties and expense associated with recording and analyzing the data do not recommend the approach as a screening procedure. Investigations of this type would appear to be more appropriate as tools for elucidating the

processes of psi in highly selected subjects.

ABSTRACT

The CNV's of ten subjects were examined for differences depending upon the nature of the stimulus about to be presented. The differences sought were those found for the same subject under conditions which provided information about the stimulus to come. The criteria used were derived from applying multiple discriminant analysis to the Fourier coefficients of the CNV waveforms. Although the experiment was sufficiently sensitive to detect differences on the order of 30% of those seen in the informed condition, there was no evidence for psi-derived information.

ACKNOWLEDGEMENTS

Many of the laboratory facilities and analysis programs used in this experiment have been developed over the past several years by other researchers. The contributions made by Sybo Schouten and Ed Kelly have been particularly important to this study. Bea Zant and Lydia Ferrari supported the experimental effort from the critical phases of the pilot studies through the final acquisition of data. To them the study owes its standardized instructions and many competently applied electrodes as well as much of its general psychological tone. Special electronic apparatus of high quality was developed by Arie Vliegenthart and by Wim Harmsen. The author gratefully acknowledges his indebtedness to these people and to his other colleagues in the Parapsychology Laboratory.

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APPENDIX

INSTRUCTIONS TO SUBJECTS

This is an experiment in which we measure your reaction time to certain pictures. During the experiment we will also study a special brainwave that appears when a person is preparing to respond rapidly.

The pictures are projected onto the screen in front of you. They are given in pairs, and your task is to respond to the second picture of each pair. The first picture may help you to prepare your response by providing some information about the second one.

The first picture of each pair shows a stylized face. Sometimes the face is shown with a smiling mouth and sometimes with a frowning mouth. On some of the trials the face is shown clearly and on others it is more or less difficult to see. Here are some examples of the faces:

The second picture of each pair is a photograph of a man or a woman. You will be responding when you see a picture of a woman (man). Here are some examples of these pictures:

The faces should help you anticipate the sex of the person to be shown in the next picture. A frown is always followed by a man (woman), and a smile is always followed by a woman (man). In the experiment the faces are shown only briefly so that you may not always be certain which one you have seen. You should prepare your response as well as you can based upon whatever information you have.

Throughout the experiment a small red lamp will glow near the center of the screen. Fix your eyes upon it so that you will be prepared when the face is shown. Following the appearance of the face you will have a few seconds in which to prepare your response before the second picture of the pair is seen. When it appears you should press the button in your hand as quickly as you can if and only if it shows a woman (man). Please notice: you are to make no response if it shows a man (woman).

The brainwaves that we want to study cannot be measured in the presence of eye movements or blinks. Consequently it is important for you to fix your eyes steadily upon the small red lamp and to hold them there until after you have made your response. You may

find it helpful to rest your eyes by blinking them at the conclusion of each trial, as several seconds will pass before the next face is shown.

Please give your undivided attention to the task. With a little practice you will find it possible to anticipate the time at which the second picture is shown so that your responses can be registered very quickly. Some incorrect responses are expected, so do not be concerned if they occur.

Here are some practice trials to familiarize you with the procedure:

Do you have any questions?

Please sit quietly for a moment. We will ask you to begin the experiment soon.

ESP AND THE DEFENSE MECHANISM TEST (DMT)
A FURTHER VALIDATION

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University of Iceland

During the last 15 years a few experiments have been conducted in which ESP performance has been successfully predicted by ratings obtained on the Defense Mechanism Test (DMT) developed by Ulf Kragh and his associates at Lund University (Kragh & Smith, 1970). These experiments have primarily been conducted by Martin Johnson, who initiated the DMT technique in parapsychology.

The DMT is a projective test in which tachistoscopically-presented stimuli are utilized to measure what has sometimes been referred to as 'perceptual defence', or preconscious defense organization (PDO), as it is called by Kragh. A rather detailed description of the DMT can be found in Kragh and Smith (1970) and to a lesser degree in Johnson and Kanthamani (1967).

In the experiments conducted so far it has been found that subjects showing a high level of PDO in their DMT test protocol tend as a group to obtain ESP scores at or below mean chance expectation, whereas subjects showing a low level of PDO tend to obtain ESP scores above mean chance expectation. Thus there appears to exist a correlation between ratings and ESP scores. In experiments completed so far, correlations (Spearman Rank) have varied from .26 to .79. Most of these experiments have, however, been done with comparatively few subjects. In table 1 a survey is presented of these experiments including the number of subjects participating in them, obtained correlations, and the statistical significance of each experiment. This survey includes, to the best of my knowledge, all the ESP-DMT experiments in which the objective has been to find out whether a general correlation exists between the total DMT rating (usually expressed in a nine-point or five-point scale, although more often a ranking of protocols from 'best' to 'worst' has been utilized) and ESP scoring. In all these experiments the ESP test has been of the

Note: This publication fulfils the publication policy as stated on the page opposite page 83 of this issue.

clairvoyance type.

TABLE 1

Survey of experiments involving DMT and ESP

		No of Ss	r_s	P
Carpenter, 1965		10	.79	<.01
Johnson & Kanthamani, 1967	I	16	.67	<.005
	II	11	.59	<.05
Johnson, 1975		18	.42	<.05
Johnson & Lübke, 1977		49	.26	<.05

Apart from the fact that all these experiments are significant - at least at the 5% level - it is of interest to note in table 1 the gradually and steadily declining strength of the association between DMT and ESP. The first experiment (Carpenter 1965) yielded a correlation coefficient of .79 which is the highest so far obtained, whereas the correlation for the last experiment (Johnson & Lübke 1977) was the lowest, r = .26. I will not try to analyse possible causes for this, at least apparent gradual decline, but it certainly brings to mind the possibility of an experimenter effect.

One experiment done by Miller and York (1976) using a somewhat unauthorized version of the DMT failed to yield significant results but showed a tendency in the expected direction ($.05<p<.10$).

Considering the poor repeatability of experiments in parapsychology in general, the results reveal a rather remarkable degree of repeatability and tempted the author to conduct an experiment testing the relationship between ESP performance and perceptual defensiveness as measured by the DMT.

HYPOTHESES

The primary purpose of this experiment was to test the hypothesis that PDO ('perceptual defensiveness') as measured by the DMT is related to ESP performance. This relationship as expressed by Johnson (Johnson & Lübke 1977) can be stated more technically as follows:

Hypothesis A1: Subjects having protocols characterized by no or moderate signs of PDO classified as isolation should tend to manifest positive scoring (above mean chance expectancy), especially if there are also moderate signs of the PDO classified as projection.

Hypothesis A2: Subjects having protocols characterized by strong signs of isolation should on the whole score according to chance level except when signs of isolation classified as 'discontinuities' are present, or where these signs are in combination with (moderate) signs of denial and/or projection, then subjects should tend to manifest psi-missing (below MCE scoring).

PROCEDURE

Each subject had to participate in two sessions, one for DMT-testing and the filling in of two questionnaires (the results of which will not be discussed in this article), and another for the ESP part of the experiment. For most of the subjects these two sessions took place on two consecutive days.

Subjects

37 subjects took part in the experiment. All were male students and volunteers. Subjects were told that they had to participate in a perceptual test and in an ESP test. The only condition for participation was availability for two sessions. To prevent knowledge of the nature of the DMT stimuli from spreading among prospective subjects, they were recruited from five different departments of the university, all the subjects (6-8) from the same department participating in the DMT test at the same time. Students in the department of psychology were excluded. 14 students of psychology served as experimenters in the ESP part of the experiment and they also recruited the subjects. The experimenters were not completely naive about the nature of the DMT, although they had only very vague ideas about the rationale of the test. However, they had no idea about the method of evaluation and during their participation in the experiment they never came into proximity of the DMT protocols.

The DMT testing

Since the DMT is still only used by very few psychologists and

considerable training is required to score it, Johnson (MJ) was asked to carry out the DMT-testing. He came to Iceland for this purpose and brought the necessary DMT test equipment used in previous ESP/DMT group-studies. Six to eight subjects were tested at each group-session, all five groups being tested in two days. The DMT was administered according to the manual worked out by Kragh and the same holds for the exposure times used, illumination level, etc..

The DMT protocols consisted of drawings and symbols used by the subjects to indicate their responses and also of short, written comments. Immediately after all the DMT-testing had been completed, MJ started coding the DMT protocols, based on EH's verbal translation of the written comments for him. Rating and final evaluation were carried out after MJ had left Iceland. While MJ and EH worked on the DMT protocols the student-experimenters conducted the ESP part of the experiment. MJ was kept ignorant of the ESP scores until EH had received the DMT scores for all the subjects. The DMT scores were expressed on a five-point scale, 1 indicating a protocol with the highest level of PDO whereas rating 5 expressed a protocol with the lowest level of PDO. Measures were also taken to ensure that EH was kept ignorant about the subjects' ESP performance during the time the translation of the DMT protocols took place to avoid every conceivable 'cueing' of MJ.

The ESP task

The ESP task consisted of 80 trials, 40 trials of clairvoyance and 40 trials of precognition. For the clairvoyance test we used an electronic machine with four push-buttons (an 'ESP-tester' produced by Paratronics, Las Gatos, California). For each trial the subject indicated his choice of a correct target by pressing one of the buttons ($p=\frac{1}{4}$). Two subjects were tested together in this part of the experiment. Two attending experimenters tried to develop a light and competitive spirit among each pair of two subjects (in one case three subjects). To further increase the subjects' motivation, every subject who obtained 15 or more hits in the clairvoyance test (MCE=10) was offered a long-playing record of his own choice.

The 40 clairvoyance trials were divided into 4 runs of 10 trials each. As they made their trials the subjects could read whenever a hit was obtained. To further a competitive and playful spirit among subjects who were tested together, they had to carry out their runs alternately. Subject A started with his first run, then subject B did his first run, then subject A did his second run, then B did his second run, and so on until both had carried out their four runs.

Each pair of experimenters only tested three or four pairs of subjects, namely those they had brought to the DMT testing session. It seems that we succeeded in making the subjects well-motivated and interested in what they were doing.

The precognition test consisted of 40 trials and was conducted immediately after the clairvoyance test had been completed. This was a paper and pencil test. For each trial the subject had to make a cross in one of four boxes that a computer at the State University of Utrecht would randomly select for each subject a few days after all the subjects had completed the experiment. Sybo A Schouten was responsible for this part of the experiment, as well as for making sure that MJ could not obtain information a) regarding the computer-generated targets (the precognition part of the experiment) and b) regarding data on ESP scoring (the clairvoyance part) which had been sent to Schouten by EH. The trials (rows of boxes) were ordered into four columns of ten trials each. No feedback was given in the precognition test, no reward promised and no attempt was made to create a competitive spirit.

A week after the last day of ESP-testing, it took four days, Schouten of the Parapsychology Laboratory at Utrecht mailed 40 sets of computer-generated random numbers (obtained via the FORTRAN RANDU function), each containing 40 numbers and the number of the subject for which that set was intended, to be used as targets for the precognition test. On the same day EH mailed copies of all 37 ESP sheets to Schouten. This was done before MJ had finished evaluating the DMT protocols and before he mailed the DMT scores to EH.

RESULTS

When the total ESP scores (for clairvoyance as well as precognition) were related to the subjects' degree of 'perceptual defensiveness' as expressed in their DMT protocols, a one-way analysis of variance revealed a highly significant F value of 4.33 ($df:4/36$; $p<.005$). Since both the DMT scores as well as the ESP scores were approximately normally distributed, a Pearson correlation was calculated between these scores, yielding $r=.53$, which gives a significant $p=.0004$. For the DMT-ratings and the corresponding ESP scores,see table 2.

Thus our main hypothesis is confirmed. A highly significant relationship appears in our data between preconscious defensive organization in the DMT and ESP performance, in such a way that subjects with a high level of PDO tend to obtain ESP scores below

TABLE 2

Total ESP scores of individual Ss grouped by their DMT ratings

DMT	ESP	DMT	ESP	DMT	ESP
1	13	3	23	4	23
1	16	3	20	4	20
		3	17	4	18
2	23	3	21	4	25
2	13	3	20	4	26
2	13	3	22	4	24
2	19	3	21	4	17
2	17	3	19	4	18
2	19	3	21	4	22
2	21	3	23	4	21
2	27	3	21	4	21
2	19	3	23		
		3	23	5	24
				5	27

TABLE 3

Means and standard deviations of total ESP performance (clairvoyance and precognition combined) in relation to DMT-ratings

DMT-rating	Number of Ss	Mean ESP score	Standard deviation
1. high defensiveness	2	14.50	2.12
2.	9	19.00	4.47
3.	13	21.08	1.80
4.	11	21.36	2.98
5. low defensiveness	2	25.50	2.12
Total	37	20.54	3.54

mean chance expectation whereas subjects with a low level of PDO tend to perform above mean chance expectation.

A glance at table 3 reveals a considerably difference in the mean ESP performance of subjects with different levels of perceptual defensiveness. With decreasing defensiveness these means steadily increase from 14.50 to 19.00, 21.08, 21.36 to a mean ESP performance of 25.50 for those two subjects who showed least 'defensiveness'.

Unfortunately subjects with extreme levels of perceptual defensiveness were very rare. But it is interesting to note that in the extreme DMT groups, 1 and 5, we find the lowest (13) and the highest (27) ESP scores, though the same scores were also obtained by subjects in group 2.

When the clairvoyance and precognition scores are considered separately in relation to the DMT, it becomes apparent that the clairvoyance condition has contributed most to the high correlation of the total ESP scores with the DMT (see table 4).

TABLE 4

Means and standard deviations of clairvoyance and precognition performance in relation to DMT-ratings

DMT-rating	Number Ss	Mean score Clairv.	Mean score Precog.	Standard deviation Clairv.	Standard deviation Precog.
1. high defensiveness	2	3.50	11.00	3.54	1.41
2.	9	9.67	9.33	2.74	2.74
3.	13	9.92	11.15	1.71	2.23
4.	11	10.91	10.46	2.39	2.38
5. low defensiveness	2	15.00	10.50	1.41	0.71
Total	37	10.08	10.46	2.94	2.34

For the clairvoyance condition only, the correlation is r=.56 (p=.0002), but for the precognition condition the r=.10 (ns). No explicit hypothesis was stated on the possible effects of the two conditions.

DISCUSSION

It seems reasonable to assume that the clairvoyance situation was more appealing and motivating for the subjects than the precognition situation. The outcome of the study may imply that the DMT is a better predictor of ESP performance when the subjects really make an effort to perform. Another reason for the observed difference may be a kind of decline effect in the efficiency of the DMT as a predictor in such a way that it may successfully predict a strong initial effort but fails to predict the outcome of a longer ESP testing. The fact that different modes of psi were presumably operating may also be a cause for the observed difference in ESP-scoring. It should be noted that all previous experiments were conducted as tests of clairvoyance. A distance effect is also a possibility as the clairvoyance testing took place in Reykjavik but the precognition targets were generated in Utrecht. It may be possible to put all these assumptions to a critical test in subsequent experiments.

The impressively strong relationship between the DMT-ratings and ESP scores evokes a question about the objectivity of the evaluation of the DMT protocols. To what extent is the DMT an objective test and to what extent could MJ's ratings be influenced by his own ESP? Comparatively detailed instructions are given in the manual for the evaluation of DMT protocols, but such instructions can never be complete. Fortunately, a number of studies on the inter-rater reliability of the DMT have been performed. Kragh states: "The inter-rater reliabilities are low to satisfactory in comparison with ordinary aptitude tests, and as good or better than those of ratings based on projective methods. The reliability coefficients seem to correlate positively with the increased training of the rater, and with the use of the complete code version. The coefficients vary between .57 and .90 with a mean (for all raters) of .70". (Kragh 1970, p.185). For the evaluation of the DMT protocols of our subjects the complete version was used and, it must be added, MJ is an expert rater of the test having worked closely with its author. The objectivity of MJ's rating can therefore be expected to be relatively high. One should also notice that not only MJ has rated the DMT protocols in previous experiments. A successful prediction, based on DMT data was made by Kragh and Balint in Johnson's study of 1975. In a future experiment it is planned that the DMT protocols will be rated independently by two expert raters.

The high correlation obtained in this experiment seems almost 'too good to be true'. If we assume, based on this and previous

replication studies which have been carried out, that there truly is a steady significant relationship between the DMT and ESP scoring, it evokes some questions. What does the DMT really measure? Does it measure a stable or transient trait? Is our notion of the notorious unreliability of ESP scores over time correct? If the DMT measures a stable trait (which to some extent seems to be implied by Kragh since he recommends the test for predictions of long-term behavior) and the discussed relationship is not artifactual, it seems to follow that ESP scores are to some extent reliable. On the basis of experience most researchers have assumed that they are not, or else are stable to a meager degree. Systematic studies on this issue are surprisingly lacking.

Rather successful results from validation of the DMT for personnel selection, especially in military settings, where resistance to emotional stress is the crucial problem (Kragh & Smith 1970) makes it likely that the DMT does measure rather stable traits. The result of this experiment as well as previous DMT-ESP experiments may indicate either that ESP scores are not as unstable or inconsistent as many have been led to believe, or that these results may be caused by some effect not necessarily originating in the subjects but perhaps in the experimenters. In recent years the so-called observational theory has been used as one of the tools for discussing and understanding the 'experimenter effect' which has for a long time been recognized and debated in parapsychological circles.

So far a relatively high correlation has been demonstrated between DMT data and ESP scoring. However, the theoretical foundation on which this observed correlation is based is, at least for the time being, a rather weak and tentative one. Johnson has tried to give a coherent view of a conceivable process or mechanism behind psi-missing and psi-hitting, as well as of the rather dramatic switch of ESP scoring direction which has been reported to occur if emotionally loaded and pertinent target material is introduced for a certain subject (Johnson 1971). Somewhat in the same vein, Beloff has discussed similarities between subliminal perception and ESP, as well as the notion of the 'Bergsonian filter' applied in a parapsychological context, to try to shed some light on the functioning and rarity of ESP in everyday life relative to normal perception (Beloff 1972; 1974). Only through further experimental research can we hope to gain some more insight into these challenging questions.

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OBSERVER OR EXPERIMENTER EFFECT? A FAKE REPLICATION

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During the last few years the experimenter effect has become one of the most used notions in the parapsychological literature (see Kennedy and Taddonio 1976 for a comprehensive review). Very often however it is not quite clear what is meant by this effect. Sometimes the author is suggesting psychological effects of the experimenter's attitude towards the subjects or the effect of gentle handling of animals etc.. Sometimes references are made to the famous observations of Rosenthal (1966) who showed that manipulating experimenter's expectancy will result in different measurements. He assumed that these effects were mainly caused by misobservations and unintentional cues from the experimenter. By far the most interesting interpretation of the experimenters effect is the parapsychological one. In some way or another the experimenter is assumed to use his psi abilities to influence the results.

Walker (1974) proposed a model for psi phenomena in which the process of observation, bringing the outcome of an event into consciousness, is essential in the description of psi. Any psi experiment can be considered to be a process with different possible outcomes. (The experiment might yield different scoring rates, etc..) The conscious observation of its outcome destroys all the possibilities except the one which actually is observed. It is obvious that the experimenter who is often the main observer with regard to the results might play an important role. However, the real focus in his model is the observer who collapses all the possible states into the one which is observed. (We will follow for the moment this line of thinking and forget the difficulties which arise from the fact that the variables which are assumed to govern this measurement process are bound to be time and space independent; see Houtkooper or Hartwell 1977). A nice example of the observers effect is found in the experiment of Feather and Brier (1968) in which the data obtained in a GESP test was randomly

Note: This publication fulfils the publication policy as stated on the page opposite page 83 of this issue.

distributed over two different checkers. Both obtained opposite significant results. Note that the checker, in the Walker interpretation, is the 'collapser' of all possible outcomes. Another example can be found in Millar 1976.

In 1976 two studies on psi 'learning' in rats were done. In the first one by Sanford the rats showed an incline in scoring while in the replication study of Davis a decline was observed. Davis discussed these opposite findings in terms of an experimenter effect and proposed to repeat the experiment with new experimenters with manipulated expectancies. It is not quite clear from his discussion whether a Rosenthal type of experimenter effect or an observer psi-effect was assumed to be responsible for the results. It should be remarked that if the latter is the case those damned rats are not necessary at all.

In the present replication of the psi 'learning' in rats experiment therefore no rats were used while the random number generator generated trials. The data resulting from this fake replication were arbitrarily split into two parts and, without being observed by the experimenter, sent to Davis and Sanford respectively. It was hypothesized that the part of the data which were sent to Sanford would show an incline effect and the part of the data send to Davis would show a decline effect, assuming that both had kept the same expectancies as they had in 1976. The present experiment can be seen as a Pandora's Box experiment, a type proposed by Walker (1976).

PROCEDURE

In the spring of 1977 the program which simulated the replication was run twice. The unobserved data of the first run were sent to Davis while the data of the second run were sent to Sanford. Both were asked to have a close look at these raw data which were given in a table similar to those they had used to represent their own experimental data.

The data which were sent to D. were returned within a few weeks. However, some delay occurred in the response of S.. Finally his data came back via D. who even did some statistical evaluation himself on the latter data. To be more specific: the data send to D. were first observed by D. and secondly by the present author, while the data send to S. were first observed by S., secondly by D. and thirdly by the present author.

RESULTS

The data collapsed over runs, 'rats' and 'days' are given in table 1.

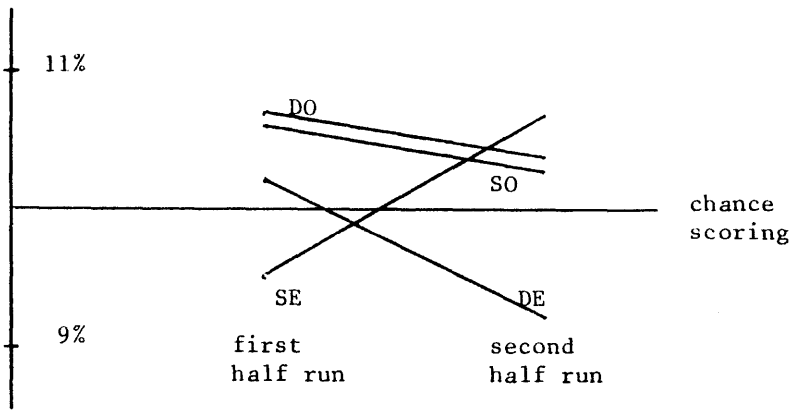
TABLE 1

Scoring on halves of run collapsed over runs and 'animals'

data	run	hits	misses	total	%hit	z
D.	1st half	1058	8942	10000	10.58	1.93
D.	2nd half	1025	8975	10000	10.25	0.83
S.	1st half	1056	8944	10000	10.56	1.87
S.	2nd half	1016	8984	10000	10.16	0.53
both	1st half	2114	17886	20000	10.57	2.68&&
	2nd half	2041	17959	20000	10.20	0.97
both halves		4155	35845	40000	10.39	2.58&&

&& $p<.01$ o.t.

Figure 1



The perceptual scoring is graphically represented in figure 1.(DO and SO) together with the scoring rates which were obtained in the real experiment of D. and S. in 1976 (DE and SE).

It can be seen that the data which were sent to D. do show a non significant decline as was the case in his own experiment. However, the data send to S. show the same decline, in strong contrast with the data of S's own experiment. Therefore the hypothesis based upon the observers expectancy effect is not confirmed. It should be noted that our RNG shows a significant deviation from chance whereas the real rats failed to obtain any over all scoring.

THE RANDOM NUMBER GENERATOR

The RNG is constructed by a hardware electronic noise generator which is sampled by the computer's A/D converter for every trial. The resultant value is converted to the interval (0,9) and compared with the target value. This target value is incremented after each trial to eliminate first order non randomness. (If the target value becomes 10 it is reset to 0; this procedure simulates a RNG with a hit probability of p=1/10). As a control the program was changed in such a way that instead of the total of 40000 trials, 4000000 trials were done. The number of hits in this control runs was 4000501. (200327 and 200174). This deviation is not significantly different from chance expectation and it must be concluded that the over all deviation obtained in the fake replication of the psi 'learning' experiment can not be attributed to a non randomness of the pseudo rat. (Another way to show this is to calculate the CRd between the real and the control runs: CRd=2.49).

DISCUSSION

The interpretation of the present results is rather difficult. Do they support the observational theories and the observational interpretation of the experimenter effect? What actually happened is that the RNG during the fake replication has behaved in an overall non-random way. The author was aware of the experiment on the moment that the simulation was started. So there might have been a direct PK influence from him. The similarity of the results independent of the actual observers does support this interpretation. It should be remarked that the author has very strong emotional feelings against experiments which show some positive and negative extra-chance but no over all scoring. (This was the case in the real experiment by D. and S.). On the other hand the results might have been contaminated by the fact that D. was a second observer of the data send to S.. If we try to fit the present findings to

the observational models we can not avoid discussing the effects of sequential observers and the associated divergence problem. This problem arises from the fact that in the observational models any observer of experimental results might influence them, including all future observers. Schmidt (1975) pointed out that this implied that only chance results would occur for large numbers of observers. Houtkooper (1977) verified the calculations of Schmidt for the average scoring values. He also showed that the variance in the expectation values increases with increasing number of observers. This results in the absurd situation that only two experimental outcomes are allowed when an infinite number of observers are to be expected: only pure hitting or pure missing.

The similar treatment of variance applied to Walker's formula yields reasonable figures as long as finite numbers of observers are used (see appendix). Hartwell proposes a modification of the observational theories by introducing a sequential dependency of the psi strength of the observer on the result of an experiment. However this is in deviation from the essential time independency of these models. Walker shows in a recent article (1977) that taking realistic values for the 'information' and number of observers involved, expectation values of a posteriori hit probabilities different from chance expectation can occur. (It can be expected that even with his realistic values the variance in these average scoring will be large; thus many, many experiments are required to get reproducable results). Walker's interpretation of these findings is really far reaching. Physical laws are thought to drift with the consensus of the percipients. Following this line of thinking reproducable (and published!) parapsychological results (not depending on a special subject) will easily be obtained once the scientific community has reached consensus on these phenomena. The problem is that consensus is probably never to be obtained without a reproducable experiment.

However it might be that a publication describing how to do a significant experiment with limited and well-specified observers (and hence unpublishable results) might serve as a tool to gradually change the opinion of the scientific community too.

In the present experiment we have to deal with three main observers (Davis, Sanford and the experimenter) and furthermore the readers of this journal.

A tentative evaluation of the results in terms of the observational theories can be given by assuming that Davis succeeded in producing about the same psi information in the data sent to him as he did in that obtained from his own experiment, while Sanford did not. Davis

again, while observing Sanfords data, reproduced this effect. The author's observation might be responsible for the over all scoring. The effect of the readers can only be guessed. Following Walker's figures (1000 readers with an average psi strength of 10% of the main observer) the effect is negligable. However there are too many 'assumes's and 'mights' in this reasoning.

Further research involving well defined observers is required.

ABSTRACT

In this article an experiment is reported which is a replication of two earlier studies of Sanford and Davis on psi 'learning' in rats. These latter experiments yielded contradictory results which might be explained in terms of an experimenter effect.

It was hypothesized that this experimenter effect should be identified with the observer effect as suggested by the recent theories of Walker and Schmidt. In these theories any effect is 'caused' by the observation of the result. Or in a not quite adequate term: by the PK or retro-PK of the observer. To test such a hypothesis the rats are not essential at all. Therefore they were in the present experiment not used while a computer was steering a RNG. A total of 40000 trials were run using this set up. The results of 20000 trials were (unobserved by the author) sent to Davis while the results of the other 20000 were sent to Sanford. It was expected that these results should be contradictory in the same way as was reported after the two real experiments. This was not the case. However an over all deviation of 155 hits were obtained ($z=+2.58$; $p<.01$). This unexpected result is discussed in terms of the observational theories.

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APPENDIX

An evaluation of the divergence problem in terms of variance in the experimental results.

In a recent article Walker (1977) shows that the observational theories are not really doomed to predict pure chance results when many observers (such as the readers of a journal) are allowed, if one takes realistic figures for the total number of observers.

Houtkooper (1977) showed that this expected tendency towards chance scoring is not the only problem which arises with many observers. The variance of the average scoring rate increases with increasing number of observers. He evaluated this for the formula of Schmidt. Here we will treat the variance using Walker's formula.

The effect of many observers on the target state selection is given by:

$$\sum_j \Omega_j = \sum_{ij} p'_i \log_2 \frac{p'_i}{p_i} \qquad (1)$$

where Ω_j is the psi information contributed by observer j to the system. The quantities p_i and p'_i are the a priori and psi-induced a posteriori probabilities for the target state i. It should be noted that psi-hitters and psi-missers, if contributing the same

amount of information, cancel each other. This can be described by allowing negative information in the formula.

We will consider the case of a coin flipping experiment observed by a subject and N-1 observers. The formula reduces in this case to:

$$\sum_j \Omega_j = p' \log_2(2p') + (1-p') \log_2 2(1-p') \qquad (2)$$

To calculate tbe expected values of p' and p'xp' we apply the approximation used by Walker to write p as a function of Ω.

$$p' = 0.5 + c_1 \frac{\Omega_t}{|\Omega_t|} \sqrt{|\Omega_t|} \qquad (3a)$$

$$p'^2 = 0.25 + \frac{c_1 \Omega_t}{\sqrt{|\Omega_t|}} + c_1^2 \Omega_t \qquad (3b)$$

where $c_1 = \sqrt{\frac{\ln(2)}{2}}$ and where Ω_t is the total information given by:

$$\Omega_t = \sum_j \Omega_j = \Omega_{subj} + \Omega_{obs} \qquad (4)$$

or the sum of the information due to the subject (Ω_{subj}) and the total information due to the N-1 observers (Ω_{obs}).

We assume furthermore that the psi abilities are uniformly distributed amongst the population:

$$f(\Omega_{obs}) = \frac{1}{2\sigma} \qquad \text{for the interval } (-\sigma, +\sigma) \qquad (5)$$

$$= 0 \qquad \text{outside this interval}$$

and σ is given by

$$\sigma = \sqrt{N-1} \times V \qquad (6)$$

where V corresponds to the absolute scoring rate of the observers.

Now we can calculate E(p') and E(p'^{2}) by integrating:

$$E(p')=0.5 + c_2 \int \frac{\Omega_t}{\sqrt{|\Omega_t|}}\, ds \qquad (7a)$$

$$E(p'^2)=E(p') - 0.25 + c_1c_2 \int |\Omega_t|\, ds \qquad (7b)$$

where the integration interval is from $-\sigma$ to $+\sigma$ and $c_2 = \sqrt{\frac{\ln 2}{2}} \cdot \frac{1}{2\sigma}$. This can be solved analytically:

$$E(p')=0.5 + 0.66c_2 \left((\sigma + \Omega_{subj})^{1.5} - (|\sigma - \Omega_{subj}|)^{1.5}\right) \qquad (8)$$

$$E(p'^2)=E(p') - 0.25 + c_1c_2 2\sigma\, \Omega_{subj} \qquad (8b1)$$

for $\sigma<\Omega_{subj}$ and

$$E(p'^2)=E(p') - 0.25 + c_1c_2 \frac{\sigma^2+\Omega^2_{subj}}{2} \qquad (8b2)$$

for $\sigma>\Omega_{subj}$.

We calculate this for Ω_{subj}= 0.0002886 and V= 0.000002868 which corresponds to a positive scoring rate of 51% for the subject and a mean absolute scoring rate of the observers of 50.1% (see table 2).

TABLE 2

N	E(p')	$E(p'^2)$	var(p')
1	.510000	.260101	.00000000
1000	.509959	.260059	.00000083
100000	.505617	.255703	.00005489
1000000	.503171	.253422	.00024095
1000000000	.500560	.258432	.00785779

Some remarks should be made about these results:
1) Only the variance due to the extra observers is taken into account and not the variance due to the instabilities in subject's performance. This is not realistic.
2) The treatment applies specifically to the case where many observers are directly observing a coin flipping experiment, for example, if the experiment is broadcast by a television station. Then and only then it is the probability distribution of the individual trials or the a priori $p_{hit}=.5$ which can be modified by the 'psi information'. If one considers the problem using a more realistic approach, that is: some direct observers and many more indirect observers of the over-all results then the treatment should be split into two parts:
a) Modification of the p=.5 hit probability by the direct observers.
b) Modification of the shifted normal z score distribution by indirect observers. Walker (1976) showed that by putting information into the total score distribution larger effects could be obtained than by putting the same amount of information into the individual trial score. This means that the divergence problem might be stronger than suggested by the figures of table 2.

EXPERIMENTER PSI-EFFECTS:I.RETROACTIVE EXPECTANCY EFFECTS

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INTRODUCTION

Recently, parapsychologists have paid much attention to the problem of experimenter effects, and in particular to the problem of the effects of experimenter expectancy on scoring patterns in psi experimentation (Parker, 1975 ; Taddonio, 1976; Thouless, 1976; White, 1976). The data and arguments on the matter of experimenter expectancy effects in parapsychology, and indeed on psychology generally, are far from clear-cut.

Rosenthal's (1966) data are probably the best-known data pertaining to experimenter expectancy effects, and parapsychological reviewers often cite these data in support of the view that, in psychology, the expectance of an experimenter can unintentionally bias the outcome(s) of experiments.However, Barber (1976; Barber & Silver, 1968) and others (e.g., Jensen, 1969; R.F.Q.Johnson, 1976) have presented powerful critiques of Rosenthal's work and it is unfortunate that parapsychologists do not appear to have examined their arguments closely. For example, Thouless (1976), discussing the 'Pygmalion in the Classroom' study of Rosenthal & Jacobson (1968), argues that the study succeeded in showing that children whose teachers were told that they were expected to show a spurt, intellectually, in the near future showed greater 'IQ gains' than other childeren, and moreover Thouless states that "the sole condition of a measurable difference in results (between 'expected gain and control children-C.L.S.) was the investigators' expectation that there would be such a difference" (Thouless, 1976, p. 261; my italics). The study shows nothing of the sort, of course. First, the 'gain' children only showed significantly greater IQ gain on performance, and not on verbal, IQ, an effect which is odd since verbal IQ is more susceptible to educational factors than performance IQ (the latter, indeed, being the basis of so-called 'culture-fair' IQ tests). Second,

Note: This publication fulfils the publication policy as stated on the page opposite page 83 of this issue.

Rosenthal & Jacobson used the same form of the IQ test chosen on both testing occasions, which would optimize specific practice gains and limits the generalisability from the date which Rosenthal & Jacobson presumably wish for. Third, the teachers themselves administered the IQ tests, which Jensen (1969) rightly condemns as a faux pas par excellence in this type of research. Also, the error variance of the data is at issue between Rosenthal and Jacobson and their critics (again, see Jensen, 1969). This study, if it shows anything at all, shows that methodologically adequate approaches are needed to the Pygmalion phenomenon, and not the experimental approach used by Rosenthal & Jacobson.

Barber & Silver (1968) and Barber (1976) note literally dozens of experiments which have failed completely to show any trace of an experimenter unintentional expectancy effect. More important, Barber has shown how many of Rosenthal's results are post-hoc in nature (see Barber, 1976, on the experiments by Rosenthal et al, 1963, 1964). Also, experimenters were allowed to cheat which they sometimes did (Rosenthal, 1966), or else they simply failed to follow procedures properly (Barber, 1976; R.F.Q. Johnson, 1976). These effects are not exactly what one generally considers when one talks of unintentional experimenter expectancy effects.

Barber (1976) notes the existence of several methodologically sound studies on experimenter unintentional expectancy effects (e.g., those by Adair & Epstein, 1968; Blake & Heslin, 1971; V.S. Johnson, 1970; Marwit, 1969; Page, 1972; Rosenthal & Fode, 1963) and it seems likely that the effect does exist. However, it does seem remarkable that parapsychologists, so acutely aware of the problems of post-hoc reporting (Morris, 1975; Wiklund, 1977; also see publication policy of this journal), should apparently have accepted so easily the doubtful data presented by Rosenthal (1966) on expectance effects. Perhaps this derives from a feeling of inferiority parapsychologists may have vis à vis experimental psychology; in any case, it is right to approach psychological data with as much critical analysis of data and arguments as is the case in parapsychology.

Turning to the literature on experimenter expectancy effects in parapsychology, the studies by Layton & Turnbull (1975), Parker (1975), Taddonio (1976) and Broughton (1977) are of interest. Layton & Turnbull 'conditioned' subjects by use of different introductory lectures, hoping to manipulate belief in psi and evaluation of psi separately. Without going in detail, the post-hoc results of the first study could not be replicated in a follow-up study. Parker (1975), in a pilot study, found that positively conditioned experimenters, led to expect high scoring, obtained insignificantly (CR_d=1.64, P=.0505, one-tailed) highler results than experimenters negatively conditioned.

Also of great importance here is the fact that the experimenter effects were largely idiosyncratic. Of the three 'positively conditioned' experimenters, one alone obtained the psi-hitting the other two obtaining a small negative deviation from MCE between them. The successful experimenter was a strong 'sheep' to begin with, but she was also an attractive young girl. Lastly, recording errors could have occured in Parker's study, as he acknowledges, and if they had they would have remained undetected.

Taddonio (1976) reported highly significant and replicable results when she used positively and negatively conditioned experimenters; the 'psi-hitting conditioned' experimenter obtained significantly better scores than the 'psi-missing conditioned experimenter'. Recording errors could not have occurred in this study (or are extremely unlikely to have occurred). Taddonio's study is strong evidence for the operation of an experimenter expectancy effect in psi testing.

Broughton (1977) reported some intriguing data on expectancy effects. He generated his expectancies after a psi test. Subjects, following a test, were given a prediction about the outcome of their test with the results; Broughton, working with the Schmidt (1975) model, was hoping to find a retroexpectancy effect. The strongest finding in his experiment, however, was an interaction between prediction (high or low score) and condition (subject sees correct results versus subject does not see correct results, but false ones), which was associated with $P<.005$. Within the terms of the Schmidt (1975) or Walker (1974) models, an experimenter psi effect must be present here. For those less committed to the observational models, the results resemble nothing so closely as the old subject differential effect (Carpenter, 1977; Rao, 1965) and indeed they produce a feeling of déjà vû when compared with the results of a not radically dissimilar experiment conducted by Schmeidler (1964).

Combining the thoughts underlying Taddonio's and Broughton's papers, the present author wondered if the manipulation of experimenter expectancy after data had been collected would be effective in influencing the data. If so, this would have strong implications for parapsychological experimentation. The study to be described was a first step in the investigation of retroactive experimenter expectancy effects, and the task was designed to make it maximally difficult for subject psi to operate. Details of the study will now be given.

METHOD

Subjects

Ten subjects were tested in the experiment. Nine of them were male and one female. They were all members of the University S.P.R.

Procedure

The essence of the procedure was that subjects made guesses in a precognitive ESP task which were later compared with ten target orders. After all data had been collected and scored half of the ten different target sequences were randomly designated as 'high expectancy' and the other half 'low expectancy'.

At a regular Sunday evening meeting of the University S.P.R., the experimenter asked the ten subjects to complete four runs of 100 guesses per run for a 'long term precognition experiment' in which targets would be randomized digits in the range 1 through 5 incl. No further details of the experiment were (or have been) given to subjects. Since the University S.P.R. is quite active in experimentation, the present study was soon forgotten by subjects following this first, data-collection phase (February 1977).

When all subjects had returned their response forms, 10 Xerox copies of each subject-run were made, and the subjects random-ordered using a computerized quasi-random ordering program. This step was not an essential randomization step in the study. Then, the experimenter obtained an entry point to the RAND Corporation tables (RAND, 1955) and read off, sequentially, 40,000 random digits and transcribed them on to the 400 Xerox sheets, in order (from Subject 1/Run 1/Target sequence 1 to Subject 10/Run 4/Target sequence 10). The targets were transcribed using a stencil to obscure responses from the experimenters sight. This procedure means that for every subject run, 10 target sequences exist. For the purposes of transcribing targets, the digits 6, 7, 8, 9 and 0 were converted into 1, 2, 3, 4 and 5 respectively. In the ensuing months until June, 1977, the experimenter scored the 400 sheets for direct hit score, and also -1 and +1 displacement scores.

It is clear that with over 10^5 transcriptions and checkings involved here some errors must have occurred. However, it will be seen later, when the critical randomization step in the experiment is discussed, that any errors made up to this point in time could not have produced artefactual evidence of psi so far as the experimental hypothesis was concerned.

When all 400 sheets had been scored the experimenter typed out a copy of the -1, direct hit, and +1 scores and took Xerox copies of these to a fellow member of the Department of Psychology, Mr. M.L., and lay these face-down in front of him. M.L. then prepared three separate entry points to a computerized pseudo-random ordering program. He prepared these by using a binary cut method as described in a previous paper (Sargent, in press (1), to generate a digit in the range -32,000 to +32,000 inclusive. The prepared entry points were then written by M.L., together with his signature, on the Xerox copies of the scores from the ESP test.

Each entry point was then used to activate the computerized pseudorandom ordering program, which random ordered the digits 1 through 10 with 40 repetitions (10 subjects x 4 runs). The first five digits from each ordering of 10 were taken as the 'high expectancy' target sequences for that subject-run whilst the last five were taken as the 'low expectancy' target sequences for that subject-run.

Thus the critical randomization step in the experiment is the preparation and use of the three entry points to the pseudorandom ordering program by M.L. Hence, errors in scoring and recording prior to this phase of the study could not have effected results artefactually in terms of relevance to the hypothesis under study.

The scores corresponding to the now designated high and low expectancy target sequences were tabulated and scored by the experimenter, re-checked by him, and finally checked a third time by a paid helper.

Experimental hypotheses, predictions, and planned data analyses.

From the studies of Taddonio (1976) and Broughton (1977) it was predicted that scoring would be significantly higher on high-expectancy targets than on low-expectancy targets. The subject was taken as the unit of analysis, and a binomial statistic was used to assess the prediction for the -1, direct hit, and +1 scores alone. An overall effect was assessed by the use of a suitable chi-square statistic ($\chi^2 = -2 \Sigma \log_e P$ with 6df; see Honorton {in press}).

One-tailed statistical tests were used to test the experimental predictions.

RESULTS

The experimental hypothesis was partially confirmed. Table 1 shows the full data.

TABLE 1

Results of Experiment

	scoring dimension		
	-1	direct hits	+1
Subjects scoring higher on high-expectancy targets	9	4	3
Subjects scoring higher on low-expectancy targets	1	6	6
No difference in scoring	0	0	1
Exact binomial P, one-tail	.01	NS	NS

The expectancy manipulation was successful on -1 displacement only (P=.01; corrected for multiple comparisons, P=.029). No effect of expectancy manipulation was found for direct hitting, or for +1 displacement, or overall (the chi value being insignificant).

It might be noted in passing, although this is of course completely post-hoc, that the one subject who scored higher on low-expectancy targets on -1 scoring was the single female of the group. This is only an incidental observation.

DISCUSSION

The complexity of the psi task involved was introduced in order to make the operation of subject psi unlikely in the present experiment. However, given the doubtful relation between complexity of targets and psi scoring (see, e.g., Stanford, 1977), it seems likely that this assumption - that a complex psi task makes the operation of psi less likely - is mistaken. However, in terms of observational theory the experimenter must have been the psi source in this experiment since he was the only person to obtain full feedback concerning results.

The expectancy manipulation achieved some degree of success. The fact that the manipulation was successful in the case of -1 displacement scoring only is of particular interest and will be commented on further in a later paper (Sargent, in press (4)). However, the fact that the manipulation of expectancy after the event can affect the form and significance of data is a finding which has quite profound implications for parapsychology. For one thing, double-blind designs of experiments must be regarded in a new light if conceptual replications of this study and the one reported by Broughton (1977) are carried out with success.

In this experiment, to put matters simply, I got (to some extent) 'what I wanted' in the way of results. However, speaking personally I have observed, in some research (Sargent, in press (2)), results Which went completely contrary to my expectations and desires in the first instance; and I have also reported findings from a study which were significant and internally coherent despite the fact that I cared very little about the outcome of the study (Sargent, in press (3)). It is plain from my experience, and other parapsychologists may agree, that we do not always get what we want, or what we expect, and how much we care about an experiment seems at best to bear a tenuous relationship to just how the study 'turns out'. Still, it is clear that formal studies of experimenter expectancy effects, personal attributes effects, attitudes and moods, etc., will have to be undertaken. It is hoped that the present study, the results of which are suggestive, may underline the need for such research.

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RECENT INVESTIGATIONS OF MICRODYNAMIC PSYCHOKINESIS, WITH SPECIAL EMPHASIS ON THE ROLES OF FEEDBACK, EFFORT, AND AWARENESS

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My purpose, in this paper, is to describe a number of recently completed experiments in the area of "microdynamic" psychokinesis (PK) which were conducted in our University of Houston and Mind Science Foundation laboratories. These experiments were originally performed for a variety of reasons, but they are grouped together here because of their relevance to a particular issue I wish to explore: the role of feedback and egocentric effort in PK. There exists ample evidence that persons can successfully perform PK tasks in the laboratory when they are fully aware of the tasks, are actively striving to succeed, and are receiving feedback concerning their success or failure. However, one might ask whether these three factors (awareness, egocentric effort, feedback) are really necessary for the occurrence of PK. Awareness and effort do not seem to be involved in spontaneous instances of ostensible PK occurring outside of the laboratory (e.g., Jung, 1963; Rhine, 1970; Stanford, 1974). There are also indications that laboratory PK can occur without awareness and effort (e.g., Schmidt 1975a, 1976b; Stanford and Fox, 1975; Stanford Zenhausen, Taylor and Dwyer, 1975; Terry and Schmidt, 1977). Whether PK can occur without feedback is a complex issue, the resolution of which depends upon one's definition of "feedback". This issue will be treated in detail later.

This paper contains two major sections. In the first section ("FINDINGS"), several PK experiments are described in which the amount and nature of feedback to subjects and experimenter vary from study to study; awareness of the PK task and task-related effort also vary. The conclusion which will be seen to emerge from these findings is

that respectible levels of PK can indeed obtain under conditions of limited or restricted feedback. In the second section ("INTERPRETATIONS"), I will pay special attention to how the findings discussed in the first section dovetail with those of other investigators in casting some light on the issue of the role of feedback, effort, and awareness in PK performance. Stated in other words, this paper describes an exploration of the limits (if any) of the conditions under which PK is possible.

FINDINGS

The experiments described in this section are discussed in a definite order; this order reflects the gradually decreasing role of immediate, trial-by-trial feedback to participants in the studies. All experiments involve PK effects upon random event generators (REGs), the random element of which is provided by the indeterministic physical processes of radio-active decay or thermal noise in semiconductors.[1]

Experiments involving immediate trial-by-trial feedback to both experimenter and subjects.

Mode 1/Mode 2 experiments. Evidence from a variety of sources (lesion and stimulation studies, split brain research, dichotic listening experiments, and lateral eye movement research) suggests that man's two cerebral hemispheres are specialized for different modes of information processing (Dimond and Beaumont, 1974). The "dominant" hemisphere (the left, in right-handed persons) appears to excel in linguistic, mathematical, logical, temporal, abstract, sequential, and analytical skills. The "minor" hemisphere (the right, in right-handed persons) performs more poorly on the above, but appears to excel in tasks involving music, facial recognition, imagery, spatial performance, simultaneous processing, and holistic judgments. In this paper I am using the term "left hemispheric" as a convenient shorthand to denote a particular cluster of cognitive or information-handling processes which are analytical, linear, and logical in character. Similarly, the term "right hemispheric" is used to refer to another set of

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[1] An exception to this is found in the first of the "Mode 1/Mode 2" experiments, in which an electro-mechanical REG was used.

processes--those which could be characterized as nonanalytical, holistic, nonlinear, alogical, and intuitive. Thus, the terms refer primarily to psychological processes, not brain loci. In order to avoid implications or conclusions about brain structure which I do not intend to make, I will use the more neutral terms "Mode 1 functioning" and "Mode 2 functioning" to designate the nonanalytical and analytical clusters, respectively. Mode 1 functioning closely resembles a mode of consciousness described by Deikman (1971) and termed the "receptive mode", while Mode 2 functioning is quite similar to what Deikman calls the "action mode".

Anecdotal observations suggest that analytical, interpretive, logical, linguistic Mode 2 functioning is antagonistic to good psi performance, while nonanalytical, noninterpretive, paralogical, nonlinguistic Mode 1 functioning is conducive to good psi performance. This hypothesis is suggested by the spontaneous reports of gifted psychics, sensitives, and laboratory subjects concerning how they seem to be functioning while performing well on psi tasks. The evidence in this area has recently been reviewed in a paper by Broughton (1975) and some experimental findings by Broughton (1976) and by Maher and Schmeidler (1976) are consistent with the hypothesis suggested here.

Some preliminary work has been conducted in our University of Houston laboratory (Andrew, 1975; Braud, Smith, Andrew and Willis, 1976), in which our strategy has been to attempt to evoke Mode 1 functioning in some of our subjects by involving them in some nonanalytical, noninterpretive tasks such as listening to music and nonlinguistic sounds, solving spatial problems, appreciating depth, and imaging in visual, kinesthetic, and other modalities. In other subjects, we attempted to evoke Mode 2 functioning by engaging them in analytical, verbal, mathematical, and logical tasks. While our subjects were presumably functioning in these different modes, we tested their PK performance. The assumption was that the two modes of functioning are incompatible and that encouraging Mode 1 activity would inhibit Mode 2 interference and hence facilitate PK. Encouraging Mode 2 activity should contribute even more interference and disrupt PK.

These experiments are presented here since all involve immediate, trial-by-trial, visual feedback to both experimenter and subjects. In the first experiment (Andrew, 1975), the participants attempted to influence an REG consisting of a high-speed electric motor and an electromechanical timer. While the timer timed a period of five seconds (plus or minus its tolerance factor), the motor rotated a commutator which resulted in eight lights rapidly flashing. At the end of the timed period, the contact disc rotated to a halt,

finally stopping in a position which resulted in one of the eight lights remaining on. The lights were present on the experimenter's console and were duplicated on a console in the subject's room. They were arrayed in two banks of four each: four on the left and four on the right. Randomization tests indicated the machine to be indeed random (due to inconsistencies in the motor speed and slight errors in the timer): each light had the same probability of being illuminated when the machine finally stopped turning. Two thousand randomization trials before, during, and after the experiment indicated that the probability of the generator coming to a rest on a left light equalled the probability of its stopping on a right light; also, no consistent patterns were generated.

Ten subjects (college student volunteers) participated in each of the two conditions: one group listened to and followed tape recorded instructions for "Mode 1" functioning, while another group listened to and followed "Mode 2" instructions. While simultaneously engaging in their respective "priming" exercises, the subjects attempted to influence, via PK, the final stopping position of the REG system. They were asked to try to make one of the lights on one side come on 25 trials, then try to make one of the lights on the other side come on for the next 25 trials. Half of the subjects did 25 left then 25 right trials, and half did 25 right then 25 left trials. If the generator stopped on the target side (right or left), a hit was recorded.Mean chance expectation was one-half, or 25 correct out of the total 50. The light coming on provided immediate feedback of success or failure on each trial for both experimenter and subject.

Subjects were classified as hitters if they scored above chance, or missers if they scored below chance. The results indicate significant PK-hitting for the "Mode 1" group (binomial $p = .020$), significant PK-missing for the "Mode 2" group (binomial $p = .011$), and a significant difference between groups (Mann-Whitney $U = 8.5$; $p < .002$, two-tailed).

In the second and third experiments (Braud, Smith, Andrew, and Willis, 1976), some minor procedural changes were made. Instead of an electromechanical REG, an electronic REG was used in which randomness was contributed by thermal noise in semiconductors.

Experiment 2 (conducted by Gary Smith) was conceptually similar to Experiment 1. Twenty subjects attempted to influence the electronic REG while processing Mode 1 material. This material was presented via a tape recording and consisted of 23 minutes of music, natural environmental sounds, and electronically synthesized sounds suggesting depth and imagery. Twenty additional subjects attempted to influence

the REG while processing Mode 2 material: counting the letters in words, solving math problems, solving reasoning and logic problems, and listening to linear prose. The REG illuminated a random pattern of lights once every five seconds. Four of the lights were red, four were blue. The subject's task was to attempt to influence the REG so that a particular color would be illuminated more frequently than the other color for a block of 40 trials. For the next block of 40 trials, the subject attempted to reverse the target pattern of the first block. The target color was reversed again for the third block of 40 trials, and once more for the fourth (and final) block of 40 trials. Half the subjects attempted to influence the REG in the order: red, blue, red, blue; and half used the order: blue, red, blue, red. The probability of a hit was $p = 0.5$. Experiment 3 (conducted by Sue Willis) was identical to Experiment 2, with the exception that the experimenter in this case was naive with respect to the experimental hypothesis.

Subjects were classified as hitters if they scored above chance, missers if they scored below chance, and neither if their score was at chance level. The results tended to support our hypothesis. In Experiment 2, there was significant PK hitting under the Mode 1 condition (13 hitters, 4 missers, 3 neither; binomial $p = .025$, one-tailed), chance performance under the Mode 2 condition (9 hitters, 10 missers, 1 neither), and a significant difference between the two conditions ($p < .05$, one-tailed, Mann-Whitney U Test). In Experiment 3, there was nonsignificant PK-hitting under the Mode 1 condition (12 hitters, 7 missers, 1 neither), chance performance under the Mode 2 condition (10 hitters, 8 missers, 2 neither), and a nonsignificant difference between the two conditions. If the results of these two experiments are combined with those of Experiment 1, there is significant PK hitting under the Mode 1 condition (33 hitters, 12 missers, 5 neither; binomial $p = .0014$, one-tailed) nonsignificant PK missing under the Mode 2 condition (20 hitters, 27 missers, 3 neither), and a significant difference between conditions ($p < .01$, one-tailed, by Mann-Whitney U Test). Thus, engaging in Mode 1 activity seems to facilitate PK performance, whereas engaging in Mode 2 activity seems to interfere with good PK performance and yield either chance performance or PK missing.

The factor I would like to stress concerning these three experiments is that each one involved immediate, trial-by-trial visual feedback and that this feedback was presented to both experimenter and subjects. Good PK performance occurred under these feedback conditions.

PK influence on another person's physiology. Two experiments were conducted to determine whether one person could psychokinetically influence another person's physiological activity while receiving

immediate analogue feedback concerning this activity (Braud,1977a), in each of the experiments, ten volunteers listened to tape recorded progressive relaxation exercises, autogenic phrases, and suggestions for mental stillness while being exposed concurrently to ganzfeld stimulation. During a certain period of the experiment, the experimenter attempted to influence the ongoing spontaneous galvanic skin reactions (GSR activity) of the volunteer in a distant room. A Schmidt binary random event generator, (the randomicity of which is provided by radioactive decay of an internal Strontium 90 source) selected twenty 30-second periods during some of which he attempted to increase and during some of which he attempted to decrease GSR amplitude. The polygraph tracing provided immediate, trial-by-trial, visual feedback to the experimenter concerning successes and failures. In the first experiment, the volunteers were unaware that attempted PK influences were to take place; in the second experiment, the volunteers were aware that PK influences would be attempted, but did not know exactly when or in which direction. In both experiments, mean GSR amplitude was significantly higher during "increase" than during "decrease" influence periods. In each experiment, approximately 58 percent of all GSR activity occurred during the "increase" influence periods. Thus, PK influences upon living target systems apparently occurred when immediate analogue feedback was provided for successful influences.[2]

Anpsi experiments. Additional experiments have been conducted which involve immediate, trial-by-trial feedback. The first four of these, (Braud, 1976) involved a new procedure for testing PK in animal subjects. A total of 34 aggressive tropical fish were given a 20-second mirror presentation (a positive reinforcer for these animals) as a feedback stimulus for hits on a Schmidt random event generator. The random element in this REG is contributed by radioactive decay of an internal Strontium 90 source. The performance of these animals was compared with that of 34 nonaggressive fish (for which the mirror was not reinforcing) and with the hit rate of the random generator during

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[2]These findings are interpreted as suggesting a PK influence by the experimenter upon the volunteer. Alternatively, the volunteers could have telepathically or clairvoyantly detected the "increase" vs. "decrease" target sequence and manifested this knowledge via appropriate autonomic responses; i.e., these could have been studies of receptive psi with the experimenter's target sequences and his cognitive and emotional activities serving as targets.

randomness tests conducted without the presence of fish in the apparatus. In Experiments 1 (pilot) and 2 (first confirmation), 100 trials were run on each of four days, with an intertrial interval of 20 seconds. In Experiment 3 and 4 (second and third confirmations), the fish were tested for only one day. In Experiments 1 and 2, the aggressive fish exhibited PK-hitting on Day 1 only. There was a decline effect over the four test days. The aggressive fish of Experiment 4 exhibited PK-hitting on their sole day of testing. The aggressive fish of Experiment 3 exhibited chance performance. The performance of the nonaggressive fish in each of the four experiments and the scores of the random generator on randomness trials never differed from chance expectation. Collapsing across experiments and combining all Day 1 scores (Day 1 procedures were identical for all fish) yielded significant PK-hitting for aggressive fish (CR = 2.81, $p = .005$), chance performance for the nonaggressive fish (CR = .20, $p < .05$), and a difference closely approaching significance between aggressive and nonaggressive fish ($CR_d = 1.84$, $p = .066$).

Thus, again, PK occurred when subjects and experimenter received trial-by-trial feedback. For the animal subjects, this feedback was immediate. The experimenter received his trial-by-trial feedback later, when he scored the punched paper tape record of the fish's performance at the end of each session. A possible retroactive PK effect in a situation similar to this has been documented by Schmidt (1976a). The issue of retroactive PK will be discussed in a later section of this paper.

In three additional experiments, (Braud and Kirk, 1977) the same general design was used, but "person-fish teams" (rather than fish alone) attempted to influence the REG to produce an increased incidence of mirror presentation to the fish. In Experiments 1 and 2, each of ten person-fish teams was tested for 100 trials, during which each hit resulted in a 20-second mirror presentation and hence an opportunity for the fish to display aggressively (a positive reinforcer for the Siamese fighting fish used in the study). In Experiment 3, each of ten person-fish teams was tested for 200 trials during which each hit resulted in a 10-second mirror presentation. Significant PK-hitting occurred in Experiment 1 and in the combined series but not in Experiments 2 and 3 considered separately. The scoring level (approximately 2 percent) for the teams in the present series approximated that for fish tested alone in the prior series. Randomness tests performed in the absence of experimental subjects indicated that the REG did indeed function randomly.

Experiments involving immediate trial-by-trial feedback to experimenter, but not to subjects.

Transcendental Meditation Study. A study was conducted (Braud and Hartgrove, 1976) in which the clairvoyance and PK performance of meditators was compared with that of nonmeditators. Only the PK portion of that study will be described here. Twenty volunteers participated in the study. Ten volunteers were meditators associated with the San Antonio Center of the International Meditation Society who had been meditating regularly for time periods which varied from 18 to 60 months (mean: 32 months). Ten non-meditating volunteers were solicited from among persons in attendance at introductory lectures on Transcendental Meditation (TM). These persons were not yet meditators but they were assumed to have personality and interest characteristics similar to those of the meditators.

During a 20-minute period, the subject was asked to attempt to influence a Schmidt binary random event generator (in which electronic noise in semiconductors provided the basic random element) while either meditating (if a meditator) or resting with eyes closed (if a non-meditator). The REG produced +1's and -1's at a rate of 50 per second for twenty 20-second run periods, separated by 40-second rest periods. Thus, 1000 numbers were generated for each run, yielding a total of 20,000 PK trials. Half of the subjects attempted to influence the REG so that more +1's were generated; half attempted to increase the frequency of -1's. Subjects were instructed that they could influence the REG psychokinetically by intending for the desired outcome to occur and by confidently imagining a successful outcome. An experimenter (J.H.) remained in the subject's room and recorded the REG scores at the end of each of the twenty runs. Since J.H. could see the scores (which were continuously displayed on readout tubes) throughout the run, she received immediate trial-by-trial visual feedback in this experiment. However, the subjects themselves (who had their eyes closed during all runs) never received such feedback. The only feedback received by subjects was knowledge of the number of successful runs(runs in which hit counts exceeded miss counts) and this gross, delayed feedback was given several minutes after the conclusion of the entire session.

Scores for meditators and nonmeditators did not differ significantly for the PK task. Combining the two groups yielded significant PK missing overall (CR = -2.11, $p = .03$, two-tailed). When assessed independently, neither the performance of the meditators nor that of the nonmeditators differed significantly from chance. Randomness tests indicated that, in the absence of an attempted PK influence, the REG produced equal numbers of +1's and -1's. In this experiment, a moderate amount of PK occurred under its particular limited feedback conditions.

Experiments involving immediate trial-by-trial feedback to neither experimenter nor subjects.

PK with and without feedback. In this experiment (Braud and Braud, 1977), ten volunteers attempted to influence a Schmidt REG (in which radioactive decay provided the random element) so that it would produce more hit than miss events. A within-subjects design was employed in which the volunteer attempted several runs of PK, some with and some without concurrent trial-by-trial sensory feedback. In this instance, immediate visual feedback was provided by the direction of apparent movement of illuminated lamps in a circular, "clock face" array; both experimenter and subject received feedback in this condition. For the nonfeedback condition, the visual display was disengaged and neither the subject nor the experimenter received trial-by-trial feedback. The experimenter received delayed, gross feedback at the end of each run as he recorded the scores from the REG's electromechanical counters. The subject received delayed, gross feedback at the very end of the experiment when he or she was informed of the number of successful runs (those in which hit scores were greater than miss scores).

During feedback runs, the subject was instructed to watch the "clock lights" display while intending for the lights to "move" always in a clockwise direction. It was suggested that the subject "wish" for the light to move appropriately, imagine it moving properly, attend to it strongly when it moved correctly, and ignore its incorrect moves. No mention of "effort" was ever made. During nonfeedback runs, the subject was instructed to attend to the darkened display while imagining a light moving from bulb to bulb always in a clockwise direction and at the same rate as in the feedback runs. Both experimenter and subject wore headphones through which they listened to white noise throughout the runs. Each subject completed twelve runs of 256 trials each--six with and six without immediate visual feedback. The trial rate was one per second and the inter-run interval was 30 seconds.

In this experiment, the mean number of hits for the feedback condition did not differ significantly from chance. The mean number of hits for the nonfeedback condition was significantly greater than chance ($t = 2.70$, $p < .05$, two-tailed). Hit scores for the two conditions did not differ significantly from each other. For our present purposes, the important finding of the study is that significant PK scoring can occur in the absence of concurrent sensory feedback of its results.

PK without feedback. Since significant PK hitting occurred under the nonfeedback condition of Experiment 1, a second experiment was designed

in which the absence of feedback was enforced even more strictly (Braud and Braud, 1977). Several procedural changes were made in Experiment 2: (a) all trials were run without feedback, (b) the subject reclined throughout the session in a comfortable reclining chair, (c) a "passive volition" or "effortless intention" attitude toward the PK task was encouraged, (d) the actual PK trials were preceded by tape-recorded exercises for progressive muscular relaxation, autogenic phrases, suggestions for mental stillness and quietude, and suggestions for success at the PK task, and (e) the experimenter attempted to remove himself and his influence from the experiment as much as possible.

The subject was asked to allow the REG to generated more "hits" than "misses". For this experiment "hits" were described as events which would have advanced the right (hits) counter and moved the lights in a clockwise direction (if these were not disengaged) and would have been indicated by holes in the hit column of the punched paper tape, a sample of which was shown to the subject. The PK task itself consisted of ten runs of 128 trials each, generated at a rate of one trial per second. There were 20-second rests between runs. During the experiment, neither experimenter nor subject received trial-by-trial feedback. The "clock lights" display and the electromechanical counters were disengaged and the REG outputs (hit and miss counts) were recorded remotely on punched paper tape by a device housed in a sound-attenuating enclosure in another room. The punched paper tapes were later scored by another person who counted the holes while unaware of which were hits and which were misses. At the end of the experiment, the scorer gave a list of total counts (of holes in two positions on the tapes) to the experimenter who decoded these numbers into total numbers of hits and misses. Thus, the experimenter received only very gross, delayed feedback. When the experiment had been concluded, the subjects were notified by telephone about the outcome of the experiment as a whole; not even their individual total scores were revealed to them.

A single-mean t test was used to compare the hit scores of these twenty subjects with mean chance expectation. The scores were significantly greater than chance expectation ($t = 1.89$, $p < .05$, one-tailed). Randomness tests accumulated throughout the course of the two experiments indicated no departure from randomness in the REG in the absence of PK attempts.

Marginally significant PK effects occurred under the "nonfeedback" conditions of these experiments. These conditions could more accurately be called "limited feedback" conditions. The importance of these findings lies in the suggestion that immediate, trial-by-trial feedback does not appear essential to the occurrence of experimental PK.

Schmidt (1976a) has already provided evidence, in his "time displaced" or "retroactive" PK experiments, that feedback need not be immediate for successful PK to occur; in fact, significant PK effects obtained even when feedback to the subject was delayed for as long as days or weeks (Helmut Schmidt, personal communication). What the present findings suggest is that feedback need not be of a trial-by-trial nature to be effective. In these experiments, the only feedback received by the subjects was information about the number of successful runs (in Experiment 1) and information about the outcome of the experiment as a whole (in Experiment 2). The only feedback received by the experimenter was information about individual run scores (in Experiment 1) and total run scores (in Experiment 2). While immediate, trial-by-trial feedback may augment PK performance, it does not appear necessary for the simple occurrence of PK. Gross, delayed feedback seems sufficient for PK manifestation.

Experiments with immediate feedback to neither experimenter nor subjects; subjects unaware.

Conformance behavior. I have already discussed two experiments in which one person attempted to influence the physiological activity of another person through PK. That aspect of the experiments involved immediate trial-by-trial feedback. I have delayed discussing another aspect of those experiments until now: an aspect characterized not only by the absence of immediate feedback but also by the absence of the subject's awareness that they were participating in a PK experiment.

The subjects for these studies were tested in what might be best described as a "relaxed atmosphere". They reclined in a comfortable chair in a pleasant, dimly illuminated room, listening to instructions designed to aid them in relaxing the muscles, quieting the autonomic nervous system, and distracting visual and auditory stimulation by means of a ganzfeld procedure. They were, in fact, asked to relax as completely as possible. While they were relaxed, an experimenter attempted to influence their resting GSR amplitudes in different directions on different occasions. The direction of the attempted influence was determined by the nature of 30-second epochs randomly selected, on line, by a Schmidt REG (based upon radioactive decay). This binary REG was activated automatically every 30 seconds. If a + 1 was generated, a "relaxing" selection (natural environmental sounds and relaxing music) was played through a network of tape recorders and switching devices. If a - 1 was generated, an "activating" selection (harsh sounds and activating music) was played. During the

GSR influence period, the experimenter listened to the generated random sequence of these sounds and attempted to increase the distant subject's GSR amplitude during activating selections and decrease the amplitude during relaxing selections. The subject's themselves could not hear the generated sounds and were, in fact, unaware of the existence and function of the REG-tape recorder system. The system ran for 32 minutes, making a "decision" every 30 seconds. During ten of these minutes (20 decisions), the experimenter listened to the output of the system through headphones and used the selections as cues for the direction of the attempted GSR influence. During the remaining 22 minutes (44 decisions), however, no one listened to the output of the system. The experimenter's headphones were disconnected and the sequence of relaxing vs. activating output sounds was recorded on a cassette tape (which no one ever listened to) and on punched paper tape.

The interesting finding which I'd like to report now concerns the behavior of the REG during those periods when no one was sensorily aware of its outputs. Significantly more relaxing than activating sounds were generated (61.59%, $t = 5.83$, two-tailed, $p < .002$) for the pilot study and 60.23%, $t = 3.41$, one-tailed, $p < .005$ for the confirmation study). Equivalent randomness tests conducted in the absence of subjects indicated that the REG itself was not biased: the number of relaxing selections generated did not differ significantly from chance under these conditions.

In both of these experiments, the REG-tape recorder system generated significantly more "relaxing" than "activating" sound segments. This occurred when the sound segments were inaudible to both volunteer and agent but not when they were audible to the agent. In Stanford's PMIR model (1974a,b) and in its later elaboration as a "conformance behavior" model (Stanford, 1977a, 1977b), it is assumed that individuals may scan their environment unconsciously, psychically, to detect circumstances that may satisfy their needs or predispositions. When such "incentives" are detected, the occurrence or timing of instrumental acts may be altered so that the probability of encountering the incentive is increased. Psychokinesis may also occur, unconsciously, in the service of needs. It may be the case that, in the present experiments, our volunteers psychically detected events, viz. recorded relaxing and activating sounds, which were potentially conducive to or disruptive of, respectively, the relaxed atmosphere which was encouraged in the experiment. Activating sounds (if heard) would disrupt, while relaxing sounds (if heard) would be congruent with or conform with the volunteer's disposition (encouraged by the experimental protocol) to be quiet, relaxed, and inactive. If the volunteer could detect these potentially congruent and incongruent

sounds (unconsciously, psychically) he or she might "influence" the REG (unconsciously, psychokinetically) so that a great number of congruent sounds were selected. It is conceivable that all of this could occur without any form of conventional sensory feedback. An everyday life analogue would be successfully avoiding a potential accident (perhaps even via an unconscious PK influence) about which one never learns sensorily. Alternatively, the necessary feedback, if any, may be psychically mediated. During the period in which the sounds were audible to the agent and used as cues by him, the agent's predisposition to have an approximately equal number of relaxing and activating influence opportunities may have predominated. A similar disposition would have existed during randomness tests. If some sort of ultimate or overriding motive or predisposition must exist and eventually be satisfied in order for these hypothesized events to occur, it may be something as simple as the disposition of both volunteer and experimenter to have the experiment succeed in the most direct manner possible.

An alternative interpretation of the REG effect is that the experimenter-agent unconsciously exerted a contemporaneous PK influence upon the REG during the appropriate periods of the experiment, without feedback, or exerted a time-displaced (retroactive) PK influence upon the REG allowed by the delayed, trial-by-trial feedback about the activity of the REG which he received later when he scored the individual "relaxing" and "activating" holes in the punched paper tape record of the REG's output. Schmidt (1976a) has recently discussed the possibility of such time-displayed PK effects and has provided some experimental evidence consistent with his retroactive PK hypothesis. It is also possible that the volunteers contributed a time-displaced PK influence upon the REG, since they too received delayed feedback about the activity of the REG. This feedback to volunteers consisted of overall scores only and was not of a trial-by-trial nature as it was for the agent-experimenter who scored the paper tapes.

Obviously, much further research will be needed to attempt to determine which of these various interpretations is most appropriate.

INTERPRETATIONS

I have just reviewed the results of seventeen psychokinesis experiments conducted in my own laboratories during recent years. Twelve of these experiments involved immediate, trial-by-trial sensory feedback to both subjects and experimenter. One experiment involved immediate feedback to the experimenter but not to the subjects. Two experiments involved immediate feedback to neither

subjects nor experimenter. In the remaining two experiments, not only was there an absence of immediate feedback for the participants, but the participants were unaware that a PK experiment was in progress. The results of these experiments are conveniently summarized in Table 1. Although the magnitudes of the PK effects are not always impressive, what is clear from an inspection of the Table is that there is no obvious reduction in PK associated with the progressive elimination or restriction of immediate, trial-by-trial feedback to the participants in the experiments. It is recognized that the various studies contributing to the Table differ from each other not only in terms of degree of feedback but in many other respects as well. However, the Table at least gives us an indication that immediate, trial-by-trial sensory feedback is not a necessary condition for the occurrence of experimental PK.

These are certainly not novel findings, but, rather additional demonstrations of findings already in the parapsychological literature. In his recent review of psychokinesis experimentation, for example, Stanford (1977b) described a large number of experiments yielded evidence for significant psychokinetic effects in the absence of immediate, trial-by-trial sensory feedback and in the absence of "egocentric effort" or "trying to make things happen". While space does not permit a review of all of those experiments here, I will mention a few of them to indicate the types of paradigms which have yielded data consistent with the hypotheses stressed in this paper.

Feedback

Let me mention first several experimental designs which have yielded evidence for PK without feedback. As mentioned in the Introduction, "feedback" is a difficult issue with which to deal since it can occur in so many different ways. However, it is possible to simplify the issue by considering certain well specified characteristics of feedback such as: (a) its delay, (b) its degree of specificity, and (c) which participants in the experiment receive the feedback.

Since it is quite likely that persons other than the "subjects" may influence the outcome of an experiment, it is important to consider the amount and nature of feedback received by all participants, especially the "experimenter". In fact, the "psi mediated experimenter effect" is looming ever larger as a major methodological issue in current parapsychological thinking (see, for example, the recent reviews by Kennedy and Taddonio, 1976 and White, 1976). In my review

TABLE 1

Summary of Seventeen Psychokinesis Experiments

I.	Feedback to both subjects and experimenter	
	A. Braud (1977), Experiment 2	$\underline{t} = 2.96$
	B. Braud (1977), Experiment 1	$\underline{t} = 3.07$
	C. Braud, Smith, Andrew and Willis (1975)	$\overline{CR} = 3.13$
	D. Braud and Kirk (1977)	CR = 2.81
II.	Feedback to experimenter but not to subject	
	A. Braud and Hartgrove (1976)	CR = 2.11
III.	Feedback to neither experimenter nor subjects	
	A. Braud and Braud (1977), Experiment 1	CR = 2.60
	B. Braud and Braud (1977), Experiment 2	CR = 1.74
IV.	Feedback to neither experimenter nor subjects;subjects unaware	
	A. Braud (1977), Confirmation	CR = 3.32
	B. Braud (1977), Pilot	CR = 4.51

of the seventeen experiments presented above, I have attempted to specify feedback conditions not only for the subjects but for the experimenter as well. I suggest that in their future publications investigators attempt to specify as fully as possible the feedback conditions for all participants in their studies. This may lead to a better understanding of the role of feedback.

Evidence consistent with the hypothesis that immediate, trial-by-trial sensory feedback to the subjects of an experiment is not a necessary condition for the occurrence of experimental psychokinesis is provided by the following experimental designs: (a) "retroactive" or "time-displaced" PK, (b) "hidden target" and "blind" PK, and (c) "restricted feedback" studies.

Schmidt (1976a) has recently provided evidence that PK effects (upon electronic REGs) can occur under conditions in which feedback to subjects concerning the behavior of the target REG is delayed for as long as days or weeks. In this paradigm, a binary REG is activated to produce a series of random numbers at time t_0. No one observes the generated output of the REG (which is tape-recorded or recorded in some other manner) until much later, at time t_1, when a subject listens to the tape recording and attempts to "enforce" a greater frequency of one of the binary random numbers via PK. The positive results obtained through the use of this method have been interpreted by Schmidt as evidencing a PK effect on the part of the subject which works "backward" in time to influence the operation of the REG at time t_0 (controls guarantee that the tape recording itself is not altered during playback). If this interpretation is correct, then only greatly delayed feedback is provided concerning the events influenced by PK. Note that this feedback, although delayed, is still of a discrete trial-by-trial nature.

In "hidden target" and "blind" PK, the subject attempts to influence some random process (e.g., dice, electronic REGs) via PK so that the outcome of that process matches some hidden, unknown target. Since the subject does not know the target, he or she cannot receive feedback for successful or unsuccessful attempts. Such experiments have been conducted by Cox (1974, 1976), Fisk and West (1958), Forwald (1963), Mitchell and Fisk (1953), and Thouless (1951), and their outcomes have been quite successful. Here again, if the target sequence was revealed to the subject at the end of the session, delayed feedback was provided.

In "restricted feedback" experiments, feedback on a trial-by-trial basis is simply not provided to the subject, although the latter knows which event is the target. The Braud and Braud (1977) study described above exemplifies the design. Studies of this type indicate

that successful PK can occur under these conditions.

There would seem to exist certain instances in which feedback may actually interfere with successful PK performance. Especially early in a session or on other occasions when PK phenomena are not yet in evidence, feedback might be discouraging to an individual attempting PK. William James was reported (Padfield, 1976) to have said that when he came to will a table to move in his room, he was prevented from doing so because the perceptual image of the table sitting full square on the floor in front of him continually interfered with his mental picture of the table moving across the room paranormally. The discouragement resulting from the conflict between the intention for an event to happen and the perception of the event not happening would be minimized by eliminating feedback and hence knowledge of the event not happening. Additionally, it would seem that feedback would increase egocentric involvement in a task and it will be argued, below, that egocentric involvement may itself disrupt PK performance.

What is needed now is a series of parametric studies in which delay of feedback and degree of specificity of feedback are independently and systematically varied to determine their influences on the magnitude of a PK effect, with all other factors constant. Studies of this nature would help delineate the precise role of feedback in psychokinesis.

Effort

Some of the findings listened in Table 1 also indicate that effortful striving to "make things happen" is not a necessary condition for successful experimental PK. In Experiment 2 of the Braud and Braud (1977) study, an attitude of "passive volition" or "effortless striving was encouraged with moderately successful results. The "Mode 1" exercises used in the Braud, Smith, Andrew and Willis (1976) study involved less "effort" than did the "Mode 2" exercises and the former were more conducive to PK hitting than were the latter. In the portion of the Braud (1977a) study in which the REG generated significantly greater "relaxing" than "activating" selections, the subjects were unaware that a PK experiment was in progress and therefore were not striving to succeed. These present findings are consistent with other indications already in the literature that "egocentric effort" does not seem to be a necessary condition for successful PK. In fact, excessive egocentric effort even appears to interfere with successful PK under certain conditions.

The possible deleterious effect of egocentric effort upon PK performance may be discussed in the contexts of: (a) "witness

inhibition", "ownership resistance", and "ritual" (b) effortless intention, (c) release of effort, (d) secondary objective PK, and (e) nonintentional PK.

Several writers have discussed the importance of externalizing responsibility for PK effects in facilitating their occurrence. Batcheldor (1966) and Brookes-Smith (1973) have suggested that the very observation of psychokinetic effects ("witness inhibition") and the feeling of personal responsibility for having produced them ("ownership resistance") both exert inhibitory effects on further instances of PK. Stanford (1974b, 1977b) has stressed the inhibitory influence of "egocentric effort" upon PK performance. Padfield (1976) had argued for the importance of "psychic support figures" (e.g. external agencies, discarnate entities, extraterrestrial intelligences, etc.) in the production of paranormal physical phenomena. I have suggested in my contribution to last year's conference (Braud, 1976), that excessive striving to succeed in psi tasks may interfere with performance. All of these suggestions indicate that if attention is focused too strongly upon the "performer" in a PK experiment, that person's PK performance suffers. Shifting attention away from the performer may facilitate PK performance. One such attention-shifting device is the "ritual" -- some action or set of actions performed with the belief that the action is itself quite efficacious in producing the desired paranormal outcome. The reliance upon "psychic support figures" is one of the more obvious rituals. Some less obvious rituals may include: (a) "automatisms" of various sort (such as the use of pendulums, ouija boards, automatic writing or speaking, aura reading, psychometry or object reading, "radionics" devices or "psychotronic generators"); (b) possibly "superstitious" physiological maneuvers such as the effort and increased metabolic activity induced by Ted Serios, Nina Kulagina, Felicia Parise, Uri Geller, and the subjects in Honorton and Barkdale's (1972) "muscle tensing" experiment; and (c) "psi conducive procedures" themselves and investigators' expectations that paranormal effects will be facilitated by certain ways of interacting with subjects, certain ways of "motivating" them and certain environmental conditions such as particular laboratory settings, weather conditions, etc. A research project of the highest priority would seem to be a careful determination of which of these factors influence psi directly and which influence psi only indirectly via their ritualistic aspects.

"Effortless intention" rather than "egocentric effortful striving" seems to be an attitude or set or task approach which yields good results in PK experiments. Just such an attitude was encouraged in Experiment 2 of the Braud and Braud (1977) study. There are already several indications in the experimental literature of the usefulness

of effortless intention. Thouless (1951), in describing his own PK performance, wrote that a real effort to succeed led directly to failure, while an effortless intention to succeed ("I want to succeed, but I don't really care whether I do or not") was effective. Camstra (1973) reported that the absence of concentration on their task was more effective than its presence in facilitating his subjects' PK performance. Charles Honorton typically encourages an effortless intention or "passive volition" strategy in his successful studies of PK on electronic REGs. Batcheldor (1966) has reported that ostensible macro-PK effects which were inhibited by an attitude of tense expectancy and intense observation did occur when this inhibiting factor was replaced by less effortful activities such as singing, joking, laughing, etc. Stanford (1974b, 1977b,) has argued that while motivation is important for PK success, it can have self-defeating consequences when it leads to a direct effort to "make things happen". He argues that since subjects do not really know how to do PK, effortful striving may be "wrong effort" that can waste energy by misdirection of attention. All of the thoughts contained in this paragraph are quite consistent with a conceptualization of PK as a goal-directed rather than a mechanism-directed process.

Anecdotal observations suggest that paranormal physical phenomena frequently occur during or immediately following a "release" of the effort which had until that moment been expended in trying to succeed at the psi task (e.g., Honorton 1974; Stanford, 1976; Targ and Puthoff, personal communication). A similar phenomenon appears to be the "lag effect" noted by Pratt and Woodruff (1946), Watkins, Watkins, and Wells (1973), Wells and Watkins (1975) and other workers in which PK effects of large magnitude occur some time after subjects abandon their PK attempts. A formal laboratory test of the "release of effort" hypothesis was successfully carried out by Stanford and Fox (1975).

In "secondary objective" studies, a subject is involved simultaneously with two nonconflicting objectives in a PK task: the subject focuses attention and effort on one objective (the primary objective) but does not think about the other objective (the secondary objective). Cox (1951, 1954) reported two experiments in which the PK results for the secondary objective were of sizable magnitude and were generally superior to those for the primary objective. Here again, an attitude of "letting things happen on their own" seemed to be more PK-conducive than one of "trying to make things happen".

I am using the term "nonintentional PK' to indicate a significant PK effect occurring in either an experiment or a condition of which the subject is unaware, as well as a PK effect occurring when the subject does not intend for one to occur. Poltergeist or "recurrent

spontaneous psychokinesis (RSPK)" reports (e.g., Roll, 1977) provide an extensive collection of cases of apparently nonintentional PK. Puthoff and Targ (1974) reported an incident in which a gifted PK subject (Ingo Swann) produced a PK effect in a well-shielded magnetometer while not trying to influence it but simply attending to it and even by merely discussing the apparatus. In experiments by Camstra (1973), Schmidt (1975a), Stanford, Zenhausern, Taylor and Dwyer (1975), and Terry and Schmidt (1977), significant PK effects occurred under conditions in which the subjects were unaware that they were producing these effects. The effect in the Braud (1977a) study described above in which the REG-recorder system generated significantly more "relaxing" that "activating" sounds was also an unconscious, nonintentional[3] PK effect. Since these effects were nonintentional[3], conscious effort to succeed was not involved.

CONCLUDING REMARKS

Recently, several investigators have stressed the importance of sensory feedback in successful psi functioning (e.g., Schmidt, 1975; Tart, 1975). However, there also exist indications that feedback is not a necessary condition for the occurrence of psi. Further, under certain conditions, feedback may even interfere with effective psi. Like feedback, conscious effort does not appear necessary for psi functioning and it, too, may actually inhibit psi. These conclusions have interesting practical implications for psi researchers. If psi typically functions unconsciously and effortlessly in everyday life, and if it can be inhibited by effort (including effort engendered by feedback and increased awareness) it is really wise to encourage feedback, effort, and awareness of psychic functioning in the laboratory? Would it not be better to design experiments which would allow psi to function as effortlessly and unconsciously as it does in everyday life?[4]. Charles Honorton (1977) and I (Braud, 1977b) have been devoting

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[3] "Intentional here is taken to mean conscious intent. It is almost certain that the subjects in these experiments "intended" at some level for the obtained effects to occur; however, they were not "aware" of this intention. It can be argued that even so-called "nonintentional" or "psi mediated instrumental response" (Stanford, 1974a, 1974b) effects are intentional in some sense, since they are goal- and need-related.

[4] Stanford's (1974) "PMIR" experiments represent an approach to this problem.

considerable effort to the exploration of psi-conducive conditions--conditions which seem to increase subjects' awareness of their psi functioning. We have been attempting to produce conscious psychic experiences in the laboratory. The findings just discussed indicate that there are limitations to this approach.

The reviewed findings also have implications for parascience itself. As we "scientifically" investigate the psi process, we become increasingly aware of its functioning and become increasingly tempted to exert control over it in order to study it further and in order to apply it. In view of the possibly deleterious effects upon psi of awareness and effort, could not this entire enterprise be self-defeating?

> I confess that at times I have been tempted to believe that the Creator has eternally intended this department of nature to remain baffling, to prompt our curiosities and hopes and suspicions all in equal measure, so that, although ghosts and clairvoyances, and raps and messages from spirits are always seeming to exist and can never be fully explained away, they also can never be susceptible of full corroboration.[5]

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A REMARK ON THE "DIVERGENCE PROBLEM"

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In my first article on a mathematical theory of psi[1], I was led to an unreasonably looking divergence paradox. Considering a random event which is witnessed by a large number of observers who on the average show no PK abilities, it appeared that these observers could nevertheless have an effect on the outcome of the random process. While this article was in print, Tom Etter pointed out to me that the paradox resulted only from an elementary error in my derivation rather than from a flaw in the mathematical psi model. What I had overlooked was that the selections of the observers for the experiment involves some chance processes which are also, according to the general formalism, subject to PK effects. Taking this systematically into account, the divergence paradox disappeared.

In order to clarify the matter, I wrote a second article[2] which studies the initially overlooked effect in detail. Unfortunately, however, the relevance of this article for the divergence paradox was so obvious to me that I did not spell out this significance explicitly. It might be appropriate to amend this omission in this journal because of a recent article by J. Houtkooper [3] in which the author amplifies the error in my first paper without being aware of the simple solution of the paradox provided by my second paper[2].

Let me use a simple example to recall how the divergence paradox arose out of an unjustified assumption, and to show that under proper treatment the paradox disappears. Consider an experiment where a die ($p = 1/6$) is thrown while a large number N of observers watch for the occurrence of a six. For this present discussion, we can disregard the presence of an overt PK subject, but we have to consider that the observers may act unwittingly as psi sources.

The divergence paradox arises if we assume that the observers have deterministically specified psi strengths $\theta_i = e^{x_i}$ $(i=1,...N)$ so that, as indication of overall psi-neutrality, the x_i values are symmetrically distributed around the origin (Reference 1, Appendix A)

Then for a large number N of observers with individually small PK effects, the quantity $S = x_1+...+x_N$ is normally distributed:

$$(1) \quad P(S) = \frac{1}{\sqrt{2\pi}\sigma} e^{-S^2/2\sigma^2} \quad \text{with } \sigma^2 = N\overline{x_i^2} \; .$$

Noting that the total psi strength θ for a particular observer group is

$$(2) \quad \theta = \theta_1 \cdot \theta_2 \cdots \theta_N$$

we obtain as expectation value for a six (averaged over the many possible observer groups)

$$(3) \quad p' = \left[\frac{\overline{p\theta}}{\overline{p\theta + q}}\right]_{\exp} = \int_{-\infty}^{\infty} P(S) \frac{p \cdot e^S}{p \cdot e^S + q} \, dS$$

If we have a sufficiently large number N of observers so that $\sigma^2 \gg 1$, the integral in (3) assumes the value $\frac{1}{2}$, i.e.,

$$(4) \quad p' = \tfrac{1}{2}.$$

Therefore, it appears that a sufficiently large number of observers, balanced between psi hitters and missers, would change the probability for the occurrence of a six from its chance value $p = 1/6$ to $p' = \frac{1}{2}$. This is the divergence paradox.

The divergence paradox disappears if we drop the unjustified assumption about the symmetric distribution of the x_i-values and take into account that the observers for a particular experiment have to be selected somehow. Following the discussion of Reference 2, let us assume that the N observers for the experiment are selected randomly (by "N throws of an M-side die") from a much larger pool of M "possible observers". M should be so large that the withdrawal of the N observers does not appreciably alter the original composition of the pool. This picture of random observer selection will be appropriate at least in cases where we have no previous knowledge about the observers' psi strengths. See Reference 2 for the discussion of other cases.

To study the effect of the N randomly selected observers numerically, we can use two approaches:

(a) We label a particular history of the experiment by the outcome of the die fall (six or non-six) and by the possible θ-values $(\theta_1, \ldots, \theta_M)$ which were assigned in the random observer selection to the 1^{st}, 2^{nd}, ..., N^{th} observer. (Note that the observer selection is an integral part of the experiment). Next, we calculate from the psi axiom, the new probabilities for all the possible histories and derive from this the total probability for a six to occur. Examples for similar calculations were given in Reference 1.

(b) Following Reference 2, we first calculate the effect of one randomly selected observer on the outcome of the die fall. Noting that according to the general formalism (Reference 1) N simultaneous observers have the same effect as N subsequently looking observers, we can easily generalize the effect of one observer to calculate the effect of N observers. For the effect of one randomly selected observer, Reference 2 gave (Equations 3 - 6) the following equivalence theorem: If we randomly select a psi source (an observer) from a group of M sources (the possible observers) with strengths $\theta_1,\ldots,\theta_M$, then the selected source applied to one random event acts like a source with the effective strengths

$$(5) \quad \hat{\theta} = M^{-1}.(\theta_1+\ldots\theta_M).$$

Consequently, the N observers act like one source of strength θ^N. Thus, in particular if $\theta = 1$ (this is our new condition for psi neutrality in the original pool of all possible observers) then the observers have no effect on the die fall, i.e., there is no divergence paradox. The approach (a) gives the same result. Equation (7) of Reference 2 shows how the statistical distribution of the θ-values in the randomly selected observers is different from the θ-distribution in the original pool, i.e., how PK influences the selection.

After the removal of this particular divergence paradox there remains a problem which one could term in a more general sense the "divergence problem". While the mathematical model of psi can well describe limited systems in which the psi sources are under control of the experimenter, we have not yet learned to cope mathematically and conceptually with the effects of uncontrolled sources of whose existence the experimenter may not even be aware. Reference 4 gives some examples and points out that we certainly cannot deal with these problems before we have added to the theory some law stating the conditions for psi sources to be created and destroyed in nature.

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TOWARDS A THEORY FOR THE POLTERGEIST

William G. Roll

INTRODUCTION

On February 7, 1546, eleven days before his death, Martin Luther wrote his wife from Eisleben, where he had travelled to mediate a dispute among his quarellsome friends, the Counts of Mansfeld; "If I come among them, should I live I'd like to become a Poltergeist so that their pride would be squeezed out of them through God's mercy" (Jonas and Coelius, 1970, p. 22). Luther was in poor health and indicated in several ways that his death might be near. Whether or not he was thinking about the future when he joked about returning to haunt his friends, his remark suggests that poltergeist pheneomena were known and discussed more than four hundred years ago and that it was believed they served to punish or frighten people. The implication that such disturbances were caused by spirits of the dead was probably not meant seriously since Luther and most of his European contemporaries accepted the Biblical account that the resurrection of the dead must wait for judgement day. The more common idea was that poltergeist disturbances were caused by demons. This belief is interwoven with the description of the first poltergeist case in our survey of 116 reports, "the devil of Mascon" which let loose in the parsonage of François Perrault in Mascon, France on September 14, 1612.(Thurston, 1954, pp. 39-50). This case, of which some incidents are mentioned in a previous paper in this Journal (Roll, 1977), came to the attention of Robert Boyle, the famous chemist and founder of the Royal Society. Boyle knew Perrault and caused his account to be published in English and to be taken seriously by many people. There is no indication, however, that Boyle or anyone else at the time looked on the poltergeist with the eyes of a scientist. For that we must move up to the 19th century, from the Royal Society to the Society for Psychical Research.

Important though they are, I shall not here review the early studies of the poltergeist (e.g. Barrett, 1911; Flammarion, 1924;

Cox, 1961; Owen, 1964; Roll, 1976; Zorab, 1964). Instead I shall let the poltergeist speak for itself through the witnesses to its antics.

The material consists of 116 cases from the 17th century to the present. My criteria for accepting a case are reviewed in the previous paper (Roll, 1977). A case has to include several physical incidents of which at least one has been witnessed by an apparently reliable observer and seems to be an example of RSPK. In 91% of the cases, there were witnesses from outside the family or group primarily involved and in 63% the witnesses included parapsychologists, police officers and others with professional training. Ninety-seven of the cases appear in the professional parapsychological literature, the first of these occurring in 1850. Using this year as a watershed, I divided the cases into four periodes: The 19 pre-1850 reports which go back to 1612; 25 cases, 1850-1899; 38 cases, 1900-1949; and 34 cases, 1950-1974.

The previous paper concerned the question whether RSPK can be investigated under controlled conditions of observation. With the reservation that the 116 reports may represent a biased sample in favour of cases which occurred under good conditions of observation, the answer was clearly affirmative. In only 6% of the cases with outside witnesses did the phenomena seem to recede when the visitors were present. It is true that in a large proportion, 45% of the cases, objects were not likely to move when under direct observation by the witnesses (Roll, 1977, pp. 50-55). This fortunately is no serious problem for RSPK research: In nearly all RSPK cases (92%), the effect is focused on certain objects or areas (Roll, 1977, p. 57); the investigator can therefore isolate these from ordinary physical causes. The fact that 40% of the cases included some kind of successful test or challenge was the result of the observations by those present that certain objects or areas were especially likely to be affected by the unknown energy or entity (Roll, 1977, pp. 58-65).

In the present report I explore the cases for light they may shed on the nature of RSPK. I first examine the reports for information about the central person and then about the occurrences which happen around him or her. I quote from the witnesses to the events to illustrate the features brought out in analysis. Finally, I bring together the main characteristics of the poltergeist in an explanatory sketch.

THE CENTRAL PERSON

Age and Sex.

In December, 1761 Mr. Henry Durbin, a respected citizen of Bristol,

England began an investigation of two girls aged 13 and 8 who "were afflicted in an extraordinary manner" by wounds and scratches which suddenly appeared on their bodies and by other strange disturbances. On January 5, 1762 Durbin was with the girls in a room which had a chest of drawers:

"On the chest of drawers stood a wine glass which I saw glitter in the sun, and was astonished to see it rise from the drawers without hands. It rose gradually about a foot perpendicularly from the drawers; then the glass seemed to stand, and thereupon inclined backwards, as if a hand had held it; it was then flung with violence about five feet and struck the nurse on the hip a hard blow... There was no person near the drawers when it rose; the children were standing near me, who saw it and ran to the other end of the room, fearing that it would be flung at them, as things generally were... This was about nine in the morning, in clear daylight, close by a sash-window" (Thurston, 1954, pp 20-21).

In the majority of the 116 RSPK cases, the disturbances seem to cluster around some person or occasionally, as in the Durbin case, around two or more people (see Table 1).

Among the 116 cases, 92 (79%) seemed to be associated with a person or occasionally two persons. For the 19 RSPK cases prior to 1849, in 14 (74%) there seemed to have been focal persons; the 25 cases, 1850-99, showed central persons in 19 (76%); in the 1900-49 collection of 38 cases, there was such a connection in 30 (79%); and in the recent 34 cases, 1950-74, 29 (85%) showed this relation.

The relatively low percentages in the earlier periods probably are due to a preoccupation with the phenomena rather than with the people who might be associated with them. However, the increase over the four periods of cases with focal persons is not statistically significant.

In some cases, the events followed the person out-of-doors, during visits to relatives, and so on. This was true for 39 (42%) of the 92 cases. The distribution over the four periods is 6 (32%), 7 (37%), 13 (43%), and 13 (45%). In two other cases no focal person was identified but the phenomena followed the family when they moved. A common factor runs through these cases which further points to the link with a person: There were generally no disturbances when the focal person was asleep.

The Durbin case involved two young girls. In the first two periods, there was a curious predominance of females over males (see Table 2). In the 92 cases with focal persons, 56 were females and 36 males, 61% versus 39%. (When more than one person was involved, I counted the person who seemed to be most closely associated with the phenomena; I did the same in calculating ages). The difference may be of borderline

TABLE 1

Cases with Focal Person(s)*

Years	Total Number	Cases with Focal Person(s)	Percentages
1612-1849	19	14	74
1850-1899	25	19	76
1900-1949	38	30	79
1950-1974	34	29	85
Totals	116	92	79

*Occasionally there were two or more focal persons

TABLE 2

Sex of Focal Person

Years	Total Number	Female		Male	
		Number	Percentage	Number	Percentage
1612-1849	14	10	71	4	29
1850-1899	19	16	84	3	16
1900-1949	30	16	53	14	47
1950-1974	29	14	48	15	52
Totals	92	56	61	36	39

significance (at the P=.05 level, chi square test) depending on the proportions of males and females in the general population in the times and places involved. This favouring of women, however, was entirely due to the two early periods. During 1612-1849 there were 10 females versus four males (71% and 29%); and during 1850-99, 16 versus three (84% and 16%). The proportions then evened out for 1900-1949 to 16 versus 14 (53% and 47%); and 1950-1974 to 14 versus 15 (48% and 52%). If we assume that there were about as many males as females in the populations in the periods and places in question, the difference for the second period is quite significant (X^2=8.9, 1 d.f. P<.005). I do not know the reason for this difference. Most poltergeist agents are young, and perhaps girls then led more stressful lives than boys or perhaps cases with girls were for some reason more likely to be reported. In any case the two recent periods suggest that sex is no longer associated with RSPK agency.

For 74 of the 92 persons who seemed to be associated with the incidents, their ages were given (see Table 3). This ranged from eight to 78 (actually a man said to be in his late 70's, similarly a woman in her 70's is entered as 75 years old). The average for the 43 females was 15 years and for the 31 males 17 years, but the medians were only 13 in both cases. This difference was the result of two people in their 70's in the fourth period. For the four time periods, the average ages for females were 12, 15, 12, and 19; and for males 15, 13, 14, and 21. The medians for females were 12, 13.5, 15, and 13; and for males 14, 12, 15, and 14. The mean age for both sexes was 13. If we take account of 18 others who were only known to be adults, the mean would probably increase to 14 years in both cases.

Health

Dr. Eliakim Phelps, a Presbyterian minister in Stratford, Connecticut, "had married, late in life, a widow with four children; two girls, of sixteen and six, and two boys, of eleven and three years of age. It is suspicious, or remarkable, that Dr. Phelps, himself, believed in clairvoyance and had treated diseases by means of mesmerism. He seems, too, to have brought the trouble upon himself. Early in March of that year (1850) he was in touch with a gentleman from New York who came to him, at Stratford, in order that they should hold seances and try to obtain replies, through rapping, to questions that they asked. These attempts were, apparently, successful. A few days after this, on 10th March, when they came home from church, this is what they found. The furniture was thrown about, all over the house, as if the whole building had been ransacked...(An) eyewitness, Mr. H.B. Taylor

reports: 'In my presence the elder boy was carried across the room by invisible hands and gently deposited on the floor. A supper table was raised and tipped over when the room was completely empty of people. In one instance the boy's clothes were cut to ribbons... On March 13th, in the presence of several persons, articles moved through the air, and a brass candlestick fell from the mantlepiece and continued to dash itself against the floor until broken. A shovel and tongs set moved out from the fireplace and proceeded to hop about in a dance in the middle of the floor'.... Another witness, Mr. Horace Day, gives this odd piece of evidence: 'On the occasion of my visit Mrs. Phelps hurriedly entered the room, reporting her son missing... Later, Mrs. Phelps led the group to the backyard, where the boy was found in the hay mound, in a seemingly comatose state, from which he recovered in about an hour'" (Sitwell, 1959, pp 46-48).

Several of the focal persons show behaviour which seem symptomatic of a mental or psychological disorder. It is not always clear whether the problem preceeded the RSPK incidents or was brought on by them but even in the latter case there may have been a predisposition. In 49 (53%) of the 92 cases with focal persons, these suffered from more or less debilitating ailments (see Table 4). In 22 (24%) of the accounts, the people were described as having seizures or dissociative states or as being prone to such states. Sixteen of the 22 were observed one or more times as having muscular contractions, comas, convulsions, fainting fits, trances, seizures or other dissociative episodes. Four others had been diagnosed as epileptics. Of these three were treated for the disease at the time of the RSPK outbreak and one had previously shown epileptic spikes in the EEG. A fifth, whose EEG was taken some months after the incidents, produced a short burst of such spikes. This RSPK agent and another were prone to dissociative episodes according to their psychological tests.

If we examine the distribution over the four periods, 1612-1849, 1850-1899, 1900-1949, and 1950-1974, the 22 RSPK agents are distributed as follows: 3 (21%), 5 (26%), 5 (17%), and 9 (31%). The slightly higher figure in the last period was probably due to increased interest in the behaviour of the RSPK agent and improved methods of observation, such as the EEG. It seems likely that the other figures would have been larger if similar diagnostic tools had been available then. In this context it is relevant that there were no diagnosed epileptics in the two first periods; the first appeared in the third period, with three in the fourth period, not counting the borderline case who was also in the fourth. Estimates of the prevalence of epilepsy in the general population is usually quoted as .5%, but in a study which included patients who had only one known attack, it is 74% (Robb, 1974).

TABLE 3

Age of Focal Person

Years	43 Females		31 Males	
	Average	Median	Average	Median
1612-1849	12	12	15	14
1850-1899	15	13.5	13	12
1900-1949	12	15	14	15
1950-1974	19*	13	21*	14
Totals	15	13	17	13

Note: Of the 13 women whose ages were not stated, nine apparently were above 13 years old. The same was true for three of the five males. If account is taken of these people, the medians for both sexes would probably increase to 14 years.

*The comparatively high averages are due to the fact that the figures include a man and a woman in their seventies.

TABLE 4

Health of Focal Person

Years		Suffering from epilepsy, muscular contractions, comas, convulsions, fainting, fits, trances, seizures		Other medical or psychological problems	
	Total Number	Number	Percentage	Number	Percentage
1612-1849	4	3	21	1	7
1850-1899	12	5	26	7	37
1900-1949	10	5	17	5	17
1950-1974	23	9	31	14	48*
Totals	49	22	24	27	29

*This increase is significant at $P < .05$

Assuming the more conservative figure of 1 percent for the incidence of epilepsy in the population, the probability of finding four or more epileptics amongst our 92 agents is $P < .02$ (using the Poisson approximation to the binomial). Three out of the 29 in the fourth period is significant at $P < .001$.

Rogo suggests that "there is another way of looking at Roll's data. It is true that many cases of epileptic poltergeist agents are coming to light, but this may be artifactual. We know that many people are afraid to report poltergeist disturbances for fear of ridicule. If a household contains a member afflicted by epilepsy, it is quite reasonable to believe that cases might be reported more often than the run-of-the-mill poltergeist (if there is any such thing) for several reasons. First, the family might report it thinking that the disturbances are related to the epilepsy. Secondly, a qualified doctor might witness the disturbances and realize that an investigation needs to be initiated. Trained professional people might be less reticent to call in a parapsychologist than the bewildered family itself". (Rogo, 1977, p.61). This objection may be valid for the Midlands case (McHard, 1973) since this was indeed reported by the girl's psychiatrist. The three other accounts were apparently not due to the medical problems but to the publicity from the RSPK disturbances (Barrett, 1911, pp. 397-402, Gilbert, 1910, Thacker, 1910) or because the family (Pratt and Palmer, 1976) or the police (Solfvin and Roll, 1976) sought the aid of parapsychologists in dealing with the phenomena. If the McHarg case is omitted, the probability figure for the overall relationship declines to $P < .07$ but remains significant at $P = .05$ for the 1950-74 period.

In addition to the epilepsy or dissociative episodes, nine of the 22 persons had other or related problems. Three were said to suffer from hysteria, three from nervous attacks or excitement, and three had behaviour problems.

Twenty-seven (29%) of the 92 central persons had other problems. One was dwarfed and lame, another terminally ill and a third had a kidney disease. The latter and three others were also said to suffer from hysteria. In two cases, however, this may have been brought on by the RSPK disturbances. One of the hysterics also had a consumptive tendency and another was a victim of nightmares and somnabulism. Two were said to be neurasthenic, one of them had rickets and was sickly and the other had tried suicide and showed somnabulism. Disturbed sleep was common to several, including one who sweated and trembled during sleep. One of the RSPK agents was a schizofrenic, another in a state of nervous collapse, and a third a violent alcoholic. Two children were hyperactive; one of these also had a low IQ and behaviour problems. Three others were mentally retarded, one of these being tubercular; and three children had behaviour problems.

In the fourth period, as a result of parapsychological investigations or because the family had sought professional help on their own, several of the RSPK agents received psychological or psychiatric

examinations. The 13 RSPK agents who participated in such studies were found to have repressed aggression or low tolerance of frustration. Four of them were also among the people with dissociative episodes and four were in the group with miscellaneous problems (the two hyperactive children, the schizophrenic, and the violent alcoholic). Repressed aggression was probably also part of the problem for many of the RSPK agents in the previous periods, particularly the hysterics and neurotics. Aggression and irritability are also common among epileptics. Five of the agents in the fourth period seemed normal except for the aggression shown on the psychological tests. There is an increase in the last period in the number of people suffering from miscellaneous problems: 1 (7%), 7 (37%), 5 (17%), and 14 (48%). This increase (significant at $P<.05$, chi square test) is probably the result of improved methods of observation and increased interest in the RSPK agent in recent times. Of the total of 49 persons with more or less severe medical or psychological problems, 23 were in the fourth period, representing 79% of the RSPK agents in this period. The best estimate of the proportion of RSPK agents with organic or functional problems may come from this period. If anything, 79% may be an underestimation. It should be kept in mind that the attention of observers usually centers on the RSPK incidents rather than on the agent and furthermore that the period of observation is generally confined to the duration of the RSPK occurrences. With more attention to the agent and more thorough medical and psychological studies, more psychological or medical problems may emerge.

Precipitating Events

In April 1904 Karin Nauckhoff and her husband moved into a home in Southern Sweden which had a reputation of being haunted. They shared the house with the manager of a factory. On May 9, he had some guests for the evening. Mr. Nauckhoff was away and Karin went to bed early. "... towards midnight, she heard the party break up. Two of them... left in search of their lodgings...and Karin was on the point of going to sleep when she heard heavy footsteps mounting the steps of the veranda. Immediately afterwards three loud knocks resounded. When she had recovered from the first shock, she dressed and went to open the door. Before her stood one of the two men who had just left. They had not been able to find their way in the dark and wanted a lantern. Karin gave him a lantern and went back to bed. She was falling asleep when three more knocks, just like those which had recently frightened her, made her start. She got up, went to the door and found nobody there. Back in bed, she heard the same three knocks resound repeatedly for about an hour...Karin had no thought

but that these knocks were a practical joke on the part of the guests of the evening before, or of some person, so she did not worry about them much. But the following night... the three knocks began again and were repeated, with intervals of silence, for about two hours... The next day Mr. N. came back to the villa. The row having begun again at night, he resolved to throw some light on the subject and catch the disturber of the night. A watch was established inside and outside, and the house was inspected from cellar to attic, but without result. Nothing was discovered and yet the blows did not cease. Mr. N. and his wife changed their room, they even installed themselves in the attic; the noise followed them everywhere, and it was not long before they perceived that it was connected with the presence of the lady " (Flammarion, 1924, pp. 362-3).

The knockings, which were investigated by Hjalmar Wijk and Paul Bjerre, lasted for about six months. In 38 (41%) of the 92 cases with focal persons, the RSPK events began at a time when there were changes or problems in the home or family which may have affected the agent. The Karin case and the Phelps case are among eight (21%) where the incidents errupted after a move to a reportedly haunted house or followed mediumistic or spiritistic cummunications, or where RSPK occurrences reportedly took place in the previous home of the focal person or had centered around a relative. The Karin case is the only one (3%) where a sudden, startling occurrence seemed to release an RSPK outbreak. In 15 (39%) the RSPK incidents started up after the agent or the family had moved, when a parent was away or the agent, usually a child, had to share its bed with somebody else; in 12 cases (32%) the focal person was ill or subject to unusual psychological stress; and in two cases (5%) the incidents followed shortly after the death of relatives or friends.

The cases where precipitating events were identified are distributed as follows over the four periods, 4 (29%), 7 (37%), 13 (43%), and 14 (48%). Again the slight increase over the four periods is probably the result of increased awareness by observers of the agent and the possibility that he or she and therefore the events, may be affected by external circumstances. Some of the precipitating events were clearly stressful. With respect to the moves or other changes in the family situation, we do not know how these affected the focal person, but changes of this type require adaptations to a new social and physical environment and may be stressful, especially for schildren.

In the Phels case, the eleven year old boy who seemed to be the center of the occurrences lived with an elderly stepfather. (Sitwell, 1954, pp 45-54). John Palmer has observed (Pratt and Palmer, 1976) that in two RSPK cases he studied, (Palmer, 1974; Pratt and Palmer,

1976) the children were not living with their natural parents at the time of the RSPK outbreaks. He raises the possibility that this may have created a stressful situation which led to the RSPK incidents.

If we set 18 as the age when a person can normally be expected to leave home, there are 61 girls and boys at his age or lower for whom we know where they lived (I have included seven whose exact ages are not given but who are referred to as boys, girls, or teenagers). Of these, 38 (62%) were living away from home and 23 (38%) were at home when the events began. Thirty-five were girls of whom 21 (60%) were away and 14 (40%) at home. Of the 26 boys, 17 (65%) were away from home and 9 (35%) at home. Among the 23 boys and girls who were living at home, in four cases there was only one parent in the home and in three, the child was a late-comer to elderly parents.

Of the 38 children who were living away from home, 18 (47%) were 13 years or less. Since we do not know how many children, in the populations and periods under consideration, did not live with their natural parents, a statistical assessment is not possible. It would appear, however, that the figures were considerably less than the 62% for poltergeist children below 19 years or the 29.5% for children below 14. It seems that many of the RSPK children may have had a more stressful childhood than others at their age.

Geomagnetic and Monthly Factors

In an exploratory study of the relation between the inception dates of published and unpublished RSPK cases and disturbances of the earth's magnetic field, we found (Roll and Gearhart, 1974) that there were generally more global geomagnetic disturbances on days following the inception of RSPK than on the day preceding inception.

Twenty-two of the published RSPK cases for which we have inception dates (for five, approximate dates) fall within the period 1906-69 for which we have been able to obtain tables showing daily geomagnetic perturbations. Of these. 13 were followed bv increased magnetic disturbances as compared to the day before and eight by less, an insignificant difference (for one the disturbances were the same). For 11 cases, the first RSPK day was above both the monthly and yearly average for magnetic disturbances and eight below, also an insignificant difference. (In three it was the same as the yearly or monthly average).

The month of inspection was recorded for 90 of the 116 cases. November had the highest number of cases with 14 while February and June had the lowest with four each. The differences were insignificant.

Termination

Exorcism: The father of Julio's girlfriend in Miami (Roll and Pratt, 1971) was a medium and volunteered to try to get rid of the entity he felt was causing the breakage in the novelty warehouse. At various places on the floor he placed fern leaves and objects intended as offerings or "plaything" for the poltergeist. The latter continued to prefer glasses, ashtrays, and other breakables. Finally the medium said that Julio must be cleansed of the spirit by taking a bath in a tub filled with milk. The day following this, everything was quiet in the warehouse and it seemed the ritual had been successful. It had to a measure: the milk had not been heated for Julio's bath and he stayed home with a cold the next day. When Julio returned, so did the poltergeist.

Many of the people afflicted by poltergeists have tried to make them go away by means of exorcism and related procedures. Among the 116 cases such activities were tried in 30 (25%). These attempts were distributed as follows over the four periods: 4 (21%), 4 (16%), 13 (34%), and 9 (23.5%). It is interesting that such attempts were about as common in the twentieth century as in earlier ages.

Of the 30 exorcism attempts, the RSPK phenomena ceased after 5 (17%) and continued after 25 (83%; in five of the latter the occurrences seemed to intensify during the rites and after four there was a temporary relief).

In one case it could not be determined whether the rites temporarily enhanced the phenomena or caused them to quiet down: Movements of objects and knocking sounds had been going on in Sauchie for eight days in November, 1960; on the nineth day it was quiet and on the tenth a service of intersession was conducted by four ministers (Owen, 1964, p 134).The knocks resumed during the service and continued for three quarters of an hour afterwards but then the phenomena died down -- though there were sporatic occurrences at least until January 23, 1961.

It is uncertain if the four cases which ceased after the rites did so as a result of these. For two of the four cases the duration of the RSPK eruption was not known. In two others they were two and three months. As I pointed out in the previous paper (Roll, 1977, p.55), the median duration of an RSPK outbreak was two months so these two cases has perhaps run their course when the rites were tried. In general exorcism seemed ineffective as a means to deal with RSPK.

Moves and Reduction of Stress: Once it had been noticed that the phenomena followed a servant or other employee, this person was sometimes dismissed, or if a member of the family, he or she was sent to another relative. Occasionally the family as a whole or some

member would move. The phenomena at times followed the family or the focal person to the new location, but sometimes they ceased. (In two of the cases the house was burnt or severely damaged by fires supposedly set by the poltergeist).

Of the 116 cases in 35 (30%), the incidents terminated after the focal person or someone he or she had frequent interaction with moved away. The distribution over the four periods is 1 (5%), 13 (52%), and 10 (26%), and 11 (32%). The figure in the first period is significantly lower than the others (P=.02,chi square test). I do not know the reason for this. Perhaps during the early period people were less mobile than later and tended to stick together even when a demon had attached itself to one of the members of the group.

To determine whether moves actually tended to shorten the course of the events we need to examine the durations of the cases. In 31 where duration is recorded the median is 1.13 months versus 2 months for all the cases. To evaluate the difference statistically, the averages have to be used: 52.4 versus 155.6 days. This difference is significant ($P<.05$,chi-square test, one-tailed) and suggests that the moves tended to shorten the duration of the incidents.

In 11 cases the occurrence seemed to terminate for other reasons, but usually this happened so late in the course of the events that the causal connection seems doubtful. For instance, one case ceased after a year with the inception of the girl's menses, in another after two years of psychoanalysis. Only three such cases ceased before two months. In one it was after payment of a debt incurred by a deceased person believed to haunt the area, in another by hypnotic suggestion, and in the third when the 10-year-old agent returned to school after having to stay home with a back injury.

Development of RSPK in Relation to Medical Symptoms

If there is a relation between the agent's health and the RSPK occurrences, we might expect the development of the latter to be related to the severity of the medical symptons. An examination of the four reports involving epileptics show the relevant information lacking for the Midlands case.In the Elwin March case, "The coughing fits accompanied by stretorous breathing began either in October or September (1909). At the time of the outbreak in the Marshall Street house Elwin was under Dr. Birney's care for this malady and was being treated for epilepsy. About a week previously the attacks had been so violent, accompanied by pain in the side, that Elwin became completely exhausted physically. Mr. Sawyers says that on two occasions Elwin fell to the floor from exhaustion" (Thacker, 1910, p.575). Apparently the RSPK phenomena began after the physical symptons had been relieved.

In the Pratt-Palmer case, "The report of a clinical EEG recorded on the granddaughter in 1971 referred to an 'abnormal' resting EEG consisting of 'potential epileptiform activity of a generalized nature' during photic stimulation. However, this abnormality was less evident in a recording made in January 1974" (Pratt,1975). If this was also true in October that year when the RSPK phenomena began, these came when the likelihood of epileptic symptoms had receded.

In the case Solfvin and I studied, the severity of RSPK occurrences from poundings, to explosions, to movements of objects, seemed to be connected with increasing stress in the life of the 21 year old RSPK agent, but his two grand mal seizures were followed by the only two periods when no RSPK incidents were reported (Solfvin and Roll, 1976, p.118).

In this and the Elwin March report, it seems as if the RSPK incidents were substitutes for the medical symptoms. This also appears to be the case in some of the accounts of agents who had not been diagnosed as epileptics but who suffered from seizures. In the Bell Witch case the sound phenomena ceased temporarily when the 12 year old Betsy Bell had her "spells" (Ingram, 1894, p.51). Similarly, Bjerre (1947, p.67) found an inverse relation between Karin's convulsions and the RSPK knockings. In the Wem case, the young servant girl who was the center of the disturbances, "while she had been a healthy child before the outbreak of the disturbances, she afterwards fell into ill-health and became subject to fits. It should be added that Miss Maddox in her signed account states that, during some of the disturbances at which she was present, Emma Davies cried out that an old woman was at her, and would not let her breathe, and called to her mother to relieve her (Podmore, 1896, p.67). Though this type of recurrent hallucinatory experience is sometimes associated with epileptic attacks, Emma Davies was not thought to be ill at the time of the RSPK outbreak.

During epileptic attacks, the patient often enters a dissociated state. Such states are sometimes reported for RSPK agents. Movements and sounds had centered around a young housewife in Russia in 1810: "On January 8th my wife fainted on seeing a light float from under her bed, small at first, but growing to the size of an india-rubber ball as big as a soup-plate. Next day the raps began in the daytime, at 3 p.m., when she had lain down to sleep, and from this time they followedher everywhere as she walked about the room. This development troubled us both greatly, and alarmed us as to her health; for though she was not particularly afraid of the phenomena she always felt a peculiar weakness when they began, with a strong tendency to sleep" (Leaf, 1897, p.324).

RSPK knockings began in 1967 in the home of a 12 year old boy living in England. The sounds were heard from the boy's room upstairs "whilst Brian was downstairs in full view of the assembled company, and, indeed, whilst he was actually outside the house". A neighbour reported: "Within a couple of minutes of being in the house I heard someone hurrying or tumbling down the staircase. The door at the bottom of the staircase was flung open and Brian half-staggered through, propping himself against the kitchen door. I had the impression that he seemed to be in a half-dazed condition, as if half-asleep. He was, however, fully dressed. The noises continued with the same intensity" (Lamb et al, 1973, p.9). According to Bjerre (1947, p.66) Karin also entered a slightly dissociated state whenever the sounds began.

THE OCCURRENCES

Attenuation Effects

In February, 1921, Mrs. Ernest Sauerbrey of Hopfgarten, Germany was gravely ill and in addition was plagued by inexplicable knocks and movements of objects. In the presence of Commissioner of Police Pheil from Weimar, some objects moved while no one was near. Pheil stated, "A police officer set a jug of water two metres away from Frau Sauerbrey. At the very moment that he turned away,the jug was already in motion. The same thing happened with a water basin" (von Schrenck Notzing, 1922 p. 204). The case resulted in a court trial and the Magistrate, Justizrat Thierback, observed that "generally speaking the movements were more marked in proportion to their nearness to Frau Sauerbrey" (von Schrenck Notzing, 1922, p. 204; I take "more marked" to mean more numerous).

The proximity effect, or put differently, the attenuation of the number of incidents with increased distance from the focal person, reinforces the impression that he or she is the agent for the phenomena. This effect can only be clearly demonstrated if the distances have been measured and if the attenuation of incidents is statistically significant. Such attenuations, first reported in 1968 (Artley and Roll, 1968; Roll, 1968), have now been found in nine (26%) of the 34 cases in the 1950-1974 period. (Bender, 1969; Eisler, 1975; Palmer, 1974; Roll, 1968, 1970; Roll and Stump, 1969; Roll, Burdick and Joines, 1973; Solfvin and Harary, 1976).

I know of only one case where an analysis was made for attenuation but none was found. In the Scherfede water case (Bender, 1974, pp 138-41) there was no decrease in the number of times water appeared in relation to the position of the 13-year-old girl who seemed to

be the agent. However, the distances measured were those between the girl and the place where the water appeared, its origin being unknown, while in the other studies the distances were those between the person and the starting place of the moving object.

Focussing and Unusual Trajectories

Showers of stones rained on a Lutheran parsonage in Gröben, Germany in 1718 but "consideration was shown to prevent the inmates and curious observers from being injured by these missiles. If a group of people were standing near the window assailed, then, the pastor tells us, the stones after breaking through the panes, fell perpendicularly to the ground close to the wall as if they were spent and some force checked them... on one occasion a large stone seemed to be falling perpendicularly and with great velocity right on the minister's head. One of the maids who was looking on shrieked in terror, but the stone turned aside in full career, though at the cost of a pane of glass through which it passed into the court outside" (Thurston, 1954, p. 36f). This example of a stone throwing poltergeist illustrates the focussing on objects of a particular type. Focussing may also involve individual objects and special areas. The effect, which was discussed in the previous paper, was found in 92% of the cases (Roll, 1977, p. 57f).

The Gröben poltergeist has another feature. The pastor, Jeremias Heinisch, writing mainly for the benefit of his ecclesiastical superiors, noted the unusual trajectories of the stones "welcher nach der Ordnung eines natürlichen Wurfts unmöglich bleibt" (which is impossible according to the laws of natural throwing. Thurston, 1954, p. 37).

Taking a look at the 105 cases with moving objects, "floating", "wavering", "zigzag", "sinuous", "hovering", or "fluttering" movemements are often reported. There are also changing speeds in flight, levitations of objects and objects coming around corners or rotating in the same spot. Forty-three of the 105 cases or 41% show one or more of these features (see Table 5). The proportions do not differ greatly in the four groups: 5 (31%), 14 (58%), 11 (30.5%), and 13 (45%).

Teleportation

Mr. Adam, whose law office in Rosenheim, Germany, had earlier been the scene of RSPK disturbances (Bender, 1969, pp 93-95), became interested in such occurrences and visited a family in Nicklheim where RSPK occurrences were taking place in 1968. "They had told him that objects which disappeared were later seen falling outside the house. He put bottles containing perfume and tablets on the kitchen table, asked the inhabitants of the

house to go outside, closed all the windows and doors, and then left himself. After a time, the perfume bottle appeared in the air outside the house, and a bit later on, the bottle of tablets appeared in the air at the height of the roof and fell to the ground in a zigzag manner" (Bender, 1969, p. 96).

In the Smithfield disturbances investigated by Roger Wells and Martha Simpson (Roll, 1975 a) 70 objects not belonging in the home were apparently teleported and sometimes seemed to pass through ceilings or walls. The incidents which happened close to the husband in the home, a man in his late 70s, were first reported to us by his stepdaughter after she and her husband visited the house. At that time, "a small hand mirror dropped from the ceiling to the floor without damaging the ceiling through which it seemed to pass, and without breaking when it struck the floor" (p.7). Another time, a retired member of the Washington, D.C. police force, witnessed the sudden appearance in a room of a seashell.

Cases with apparent instances of teleportation are scattered throughout the collection. Altogether there are 18 such cases or 17% of the total of 105 (see Table 5). Eleven of these also show unusual trajectories. The teleportation cases are distributed over the four groups as follows: 1 (6%), 2 (8%), 10 (27%) and 5 (17%).

Until the present survey, I have not taken teleportation seriously as a possible genuine RSPK characteristic. While the evidence is still weak compared to more conventional poltergeist effects, there are too many reports by serious witnesses for easy dismissal. I now tend to agree with Hans Bender that this phenomenon may be real and should be taken account of in attempts to explain the poltergeist.

Other Effects

In October, 1974 J.G. Pratt made an investigation of knocking sounds and movements of objects in the home of an elderly couple and their grandchildren, a boy and a girl, aged 9 and 10. Four times the noises were accompanied by an apparitional 'hand', observed on each occasion by more than one witness, that was seen striking the glass as the noises were heard. Three of these occasions were before my arrival, two of them coming a few minutes apart when three policemen and a builder who were standing in the front yard saw a hand striking the inside of the glass of the front door. The other time before my arrival was when the grandmother and the boy were cleaning up the dishes at the kitchen sink and they both saw a 'white-gloved hand' striking the window from the outside. At that moment the grandfather and the girl were driving away on the street in front of the house. The fourth time an apparitional hand was seen on Saturday evening

while I was present and had my tape recorder running" (Pratt, 1975).

As I pointed out in the previous paper (Roll, 1977, p.57), in 27 (23%) of the 116 cases there were reports of apparitions representing human figures, animals, demons, hands, fingers, or amorphous shapes. In 13 (11%) intelligible voices were heard, including whispering and singing; seven of these also had apparitions. There were other effects, such as woundings and fires, but the apparitions and voices were more abundant. In the hope that they may throw light on the RSPK process, let us take a look at them (see Table 6). The 27 cases with apparitions are distributed as follows over the four periods: 7 (37%), 7 (28%), 6 (16%), and 7 (21%); and the 13 cases with intelligible voices 6 (32%), 2 (8%), 2 (5%), and 3 (9%). There is a decrease in both over the four periods. The figures, however, are not independent since hearing voices and seeing visions may be related and since both were reported in seven of the accounts. In any case only the decrease in voices from the first period to the last three is significant (P=.05, chi square test).

The decrease may be associated with a reduced tendency in more recent times to personify RSPK phenomena. In the early cases, the observers may have been more likely to suppose that they were dealing with a personal entity which, if it could make knocks and throw things should also be able to speak and make itself visible. This is not to say that the experiences were always hallucinatory: Several were shared by the observers present, including visitors and investigators.

Though collective hallucinations may be produced by suggestion, no doubt heightened by the (other) RSPK disturbances, it may be worth exploring whether there could have been an objective basis for some of the experiences.

There was a total of 33 cases with apparitions and/or voices. In eight (24%) the RSPK agents were the only ones to see or hear anything; in 17 (51.5%) the apparitions or voices were experienced by relatives or other members of the household either by themselves or in addition to the RSPK agent; and in 8 (24%) cases people outside the immediate group witnessed the phenomena. The two former categories may consist predominately of subjective experiences or the effects of suggestion. The eight collective experiences, sometimes involving police officers and other professional witnesses, raise the possibility that there was an objective or physical basis to some of the experiences.

Next let us consider the information provided by the experiences. In addition to the visions and voices, intelligible information was occasionally provided by writing which would mysteriously appear on walls and other places; by Ouija Board spellings; and by mediumistictype communications received by the agent. The knocks and raps, which were common in many of these cases, provided information through the use

TABLE 5

Unusual Aspects of Motion

Years	Unusual Trajectories ("Floating","wavering", "zigzag","fluttering" movements; changing speeds; curving, and rotating movement)		Teleportation (Reported cases of the movement of objects through walls,ceilings, etc.)	
	Number	Percentage	Number	Percentage
1612-1849	5	31	1	6
1850-1899	14	58	2	8
1900-1949	11	30.5	10	27
1950-1974	13	45	5	17
Totals	43*	41	18*	17

*In 11 cases both unusual trajectories and teleportation were reported

TABLE 6

Apparitions and Unexplained Voices

	Apparitions		Voices	
Years	Number	Percentage	Number	Percentage
1612-1849	7	37	6	32
1850-1899	7	28	2	8
1900-1949	6	16	2	5
1950-1974	7	21	3*	9
Totals	27**	23	13**	11

*The decrease in cases with voices from the first period to the last is significant at $P<.05$

**In 11 cases both apparitions and voices were reported

of conventional codes, such as indicating the letters of the alphabet by the number of raps. These methods often supplemented the information produced by the apparitions or voices. In addition there was one case where the information only came by writing and another only by mediumistic communications (I am disregarding the few cases where professional mediums were called in to identify the entities; the material in any case was either indefinite or contradictory). There were 11 cases where the information came only by raps.

Altogether there were 47 cases (40.5%) of the 116 where meaningful information appeared by one of these means. In about half, 24, some kind of entity was identified and in 23 there was no personal entity or its origin or nature could not be determined. In the former group, three main types of entities were manifested. In eight of the 24, deceased persons appeared; in three deceased persons, living people, and demons were manifested at different times; ten cases only featured demons or imaginary characters; and three only living people.

Some of the 11 cases which apparently involved deceased communicated provided correct information about them, in others it was false or implausible or the information could not be verified.

Haunting RSPK

Three of the cases suggestive of discarnate agency and lacking a living agent, were haunting-type cases (Cornell and Gauld, 1959-60; Pierce, 1973; and Stevenson, 1972 pp 243-246.)

As a rule, hauntings are distinguised from poltergeists. Hauntings are thought to be "place oriented" that is, to be associated with a special location, usually a "haunted house" (haunting comes from the same root as home and literally means homing, presumably by a discarnate entity to his or her earthly habitat). Poltergeists, on the other hand, are "person oriented". As a rule things only happen when a particular individual is present. At the same time, there usually is a tendency for the disturbances to be concentrated on certain objects or on certain areas, so-called object or area focussing (Roll, 1977, pp 57 f.); so that extent RSPK is also "place oriented". Occasionally, the (primary) agent is not at home during an incident (Roll, 1970, 1975 b, p. 137). Perhaps the RSPK "charge" on a focussed object is then triggered by someone else.

Haunting phenomena are sometimes said to go on for years with different families in the haunted house or place reporting the incidents. Such cases are difficult to investigate because the physical incidents, if any, are generally few and far between. Typically haunting occurrences are hallucinatory, that is, only certain individuals hear or see anything while others who are present experience

nothing. This personal associations may be true also for the physical aspects of haunting: Sometimes these seem to subside when another family moves in, only to resume when that family has left and a third family has taken over. To an extent hauntings, too, may be person oriented.

There are other similarities, such as hallucinatory or apparitional experiences which are typical in hauntings and as we saw, not uncommon in poltergeist cases. Also, as we have observed, RSPK outbreaks sometimes take place in haunted houses. Such a case was reported by G. Solfvin and F. Matthews at the 1976 convention of the Parapsychological Association (Solfvin and Matthews, 1977).

Towards an Explanatory Model

The Swedish psychiatrist, Paul Bjerre (1947) who investigated the RSPK knockings around Karin suggested in 1905 that electric current in brain cells was converted to another energy form which was then radiated into the environment and produced the sounds (Bjerre, 1947, p. 79). Bjerre characterized Karin's fainting fits, convulsions, and anxiety attacks as "organic hysteria" (Bjerre, 1947, p. 75).

When Bjerre reprinted his study of the Karin case in 1947, he explained the underlying similarity between poltergeist cases by drawing the following analogy. "Both hysterical and especially epileptic attacks appear with similar symptomology in different individuals. One may draw an analogy between the rumblings and throwings of objects near a (poltergeist) medium with the violent energy eruptions in the other attacks. When the nerve energy extends beyond the skin... it still remains connected not only to the conscious and unconscious complex but also to certain pre-existing channels of energy. In the same way as the outpoor of energy from an epileptic short circuit follows certain pathways in the nervous system, so does the transformation of the externalized energy to new energy forms and their expression follow certain schemata provided by the organism" (Bjerre, 1947, p. 135, my translation).

Owen (1964) considered an epileptic theory of RSPK but was more impressed by the evidence for hysteria in the personalities of RSPK agents and he regarded poltergeist effects as additions to the repertoire of hysteric symptoms. These, which may include convulsions and dissociative episodes, seem to be ways of dealing with anxiety which in turn may result from unacceptable needs and aggressions. As we saw before, recent psychological studies of RSPK agents have shown evidence for low tolerance of frustration and for unconscious aggression. The question is whether the tensions to which the RSPK agent seems to be subject are released in CNS eruptions and then in

RSPK disturbances or whether the latter occur in the absence of CNS paroxysms.

The explorations of the possible relation between brain events and psychic occurrences was greatly stimulated by the advent of the electro-encephalograph (EEG). Hans Berger (1940) the discoverer of the EEG had a theory for telepathy rather similar to Bjerre's. He thought that the electrical energy produced by nerve cells may be transformed to psychical energy and that this is the vehicle for telepathic transmission. He pictured this energy as similar to but not identical with electromagnetic waves.

So far brain wave studies in parapsychology have focussed on the reception of information (ESP) or energy rather than on its transmission. In PK we seem to deal with transmission and in RSPK with sudden an recurrent transmission. If these are the results of CNS eruptions, poltergeist incidents may be regarded as a special class of epileptic symptoms.

It is interesting that epilepsy, as poltergeist, implies spirit intervention. Epilepsy comes from the Greek verb to seize or to be possessed (by a spirit or a god). The word long ago lost its occult connotation and is now defined as the "sudden and recurrent disturbances in mental function, state of consciousness, sensory activity or movements of the body, caused by paroxysmal malfunctions of the cerebral nerve cells" (Encyclopedia Britannica, 1974). It does not seem unreasonable to suppose that sudden and recurrent disturbances of cerebral nerve cells may be related to the sudden and recurrent disturbances known as RSPK.

In addition to the fact that RSPK incidents have been reported around epileptics and people with symptoms suggestive of epilepsy, several other factors make the CNS theory deserve the attention of RSPK researchers. Many epileptics show "irritability... with sudden and unpredictable variations of mood: they are suspicious, quarrelsome, egocentric, circumstantial, religious, and (show) a slowness and stickiness of thought that borders onto mental subnormality" (Pond, 1974, p. 580-581).

The theme of aggression recurs in both the personalities of RSPK agents and in the nature of the events themselves. The "stickiness" or perserveration of people with brain dysfunctions refers to the tendency for the patient to have the same thought or carry out the same activity repeatedly. This trait does not come out strongly in the personality tests of RSPK agents but it does seem to be expressed in the incidents themselves. The focusing effects, the repetitive incidents involving an object or area, are characteristic of RSPK. In some RSPK cases (Roll, 1968) the selection of objects, including objects on which there was focusing, made sense in terms of the agent's

personality. For instance, the theme of the bottles which lost their caps with explosive sounds seemed to recur in the agent's perception of rockets and bombers in the Rorschach inkblots.

Epilepsy can be brought on by a variety of developmental and congenital defects. Some of the 27 RSPK agents with miscellaneous complaints had problems which are sometimes associated with epilepsy: The disease may be found in children who are hyperactive, who have behaviour disturbances, and in mentally retarded people; it can be caused by birth injuries, tuberculosis and alcoholism.

Epilepsy is concentrated in the lower age brackets: "over half of known cases appear before age 15" (Goldenson, 1970, p. 407). Petit mal usually ceases before adulthood. The median age for RSPK outbreaks is 14, that is, about half of the cases in the survey erupted before the age of 15.

In patients with epilepsy "seizures may be brought on by particular odors, certain types of music, by reading, by a loud voice or other startling stimuli"(Robb and McNaughton, 1974, p. 272). Emotional and psychological disturbances (Robb and McNaughton, 1974) may also bring on an epileptic attack. Similarly, RSPK occurrences may be triggered by startling or stressful events in the life of the agent. Epileptic attacks can sometimes be controlled by adjustments in the social situation of the patient. The same seems to be true for RSPK.

A person who is prone to epileptic attacks and "who is subject to persistent conflicts, frustrations and tensions generally has more seizures than one who is rarely upset and leads a relatively satisfying, productive life. A recent theory holds that psychomotor seizures may serve the purpose of reducing tension, since some patients feel relieved after they have had an attack" (Goldenson, 1970, p. 404). Julio, the central person for the Miami disturbances, sometimes seemed less tense after an RSPK incident. I once asked him how he felt: "I feel happy; that thing (the breakage of an ashtray) makes me feel happy; I don't know why". Another time there was a long period of inactivity: "Now I am nervous because nothing happens" (Roll, 1976, p. 153).

In some of the cases the epileptic attacks, convulsions and so on, appeared to be inversely related to the RSPK events. It seemed as if a channel of energy had been diverted from the physical organism to the physical environment. Sometimes the agent entered a dissociated state during the RSPK disturbances, a familiar occurrence in epileptic attacks.

In the majority of RSPK cases, however, the agent's state of consciousness was not noticeably altered during the RSPK occurrences. This fact, the fact that RSPK occurrences often occur many times a

day, and the fact that the agents usually are young, point to petit mal seizures as the closest analogy to RSPK. Petit mal seizures mainly occur in childhood and are frequent, varying from one or two attacks a day to several dozen. One type of petit mal is the 'myoclonic" seizure in which arm muscles contract but the person remains conscious (Golderson, 1970, p. 408). Perhaps we shall come to regard poltergeist movements as a form of myoclonic seizure which occur outside the agent's physical organism.

In a few cases epileptic patients seek out the precipating situation which brings on an attack. Some children, with petit mal which can be triggered by photic stimulation, use finger-flickering to bring on the attacks, apparently because they enjoy these brief states of unconsciousness (Pond, 1974, pp. 576-592). In some RSPK cases (Bjerre, 1947; Fodor, 1958; Grant, 1965; Ingram, 1894; Price, 1933, pp. 228-273; Muriel Roll, 1969; and Thurston, 1956, pp. 32-36), the agents developed physical mediumship, that is, they apparently gained some measure of conscious control of the RSPK phenomena. It would be interesting to explore how this was achieved and if the methods used involved CNS stimulation.

Epileptics sometimes have auditory or visual hallucinations, and hallucinations are apparently caused by CNS disturbances (Siegel, 1977). Several of the RSPK cases were associated with the seeing of apparitions and hearing of voices. Of the 33 such cases, 12 (36%) involved the agents who reportedly had epilepsy , muscular contractions and so on. In other words more than half of the 22 agents with such complaints saw apparitions or heard voices (or relators or visitors had such experiences). This relationship is significant (P=.05, chi square test, P .02, see Table 7) and seems to support the relation between RSPK and CNS eruptions.

Some aspects of RSPK suggest familiar energy patterns. For instance, lighter household objects usually move greater distances than pieces of furniture. The strongest evidence that we are dealing with energy comes from the attenuation studies where the number of disturbed objects is seen to decrease with distance from the focal person. In five of these, the attenuation was studied more closely: In four of the cases (Roll, 1968, 1969, Roll, Burdick and Joines, 1973; and Roll and Stump, 1969) the decrease followed the exponential decay function better than the inverse function (the case where the inverse function gave the better fit, may have been complicated by dual RSPK agency, Roll, 1970). In other words, the pattern in which kinetic energy (i.e. movements of objects) appeared in the space surrounding the agent suggested that this energy resulted from the transformation of an unidentified energy.

The evidence that RSPK energy was concentrated near the agent seemed

TABLE 7

Relation between agents with dissociative episodes and hallucinatory experiences

		Visual or auditory hallucinations experienced by agents, relatives or visitors		
		YES	NO	TOTALS
Agent with dissociative episodes	YES	12	10	22
	NO	21	49	70
	TOTALS	33	59	92

$\chi^2 = 4.36$, 1 d.f., $P < .05$

to be inconsistent with another pattern: In all five studies it was found that distant objects tended to move greater distances than objects close to the agent. To throw light on this curiosity, we selected the Miami case for which we had the greatest number of reliable data and made a correlation analysis between the various aspects of motion (Roll, Burdick, Joines, 1973). We found that objects close to the agent tended to be short, clockwise, and to point away from him while distant objects moved further, were counter-clockwise, and pointed slightly inwards. Such a pattern could result if energy waves were radiated from two positions on the agent's body and if they were non-synchronous. The pattern of interaction between the waves might then become a beam which moved around the agent and varied from short and fat to long and skinny. Some of the peculiarities of RSPK motion such as the floating or fast-slow speeds and the rotating and circular motions could result if the objects were picked up and carried along by the beam, if they were dropped, picked up by another sweep and so on. An analysis of the Olive Hill disturbances (Roll, Burdick, Joines, 1974) supported the rotating beam theory. The focusing effect could also be explained in terms of wave theory (Joines 1975).

The theory that recurrent paroxymal events in the central nervous system are related to RSPK dovetails with this picture. In epilepsy the neural discharge begins in a particular location (the epileptic focus) and then rapidly spreads to other parts of the brain. In a computer study Braziers (1973, 153-177) was able to trace the timing and speed of discharges from the trigger zone to other parts of the brain. She found a regular progression along anatomical pathways lasting from a few to many milliseconds. The EEG transmissions resulting from different parts of the brain were out of phase, and might provide several loci of CNS discharges in addition to the place of origin. The theory that recurrent CNS disturbances are related to RSPK seems to complement the rotating beam theory.

The most direct way of testing the CNS theory for RSPK is to take EEG recordings of the agent during the RSPK incidents. This has been attempted in two studies (Pratt and Palmer, 1976; Solfvin and Roll, 1976). The transmitting unit, which was designed by Fritz Klein of the Department of Anesthesiology, Duke, is about the size of a package of cigarettes and is connected to scalp electrodes. It has an attena which transmits to a nearby FM receiver and tape recorder. The unit can be kept in a pocket and allows the person to move about normally. In neither case were there any incidents when the central persons were wearing the EEG electrodes and carrying the telemetry units.

EEG measurements can also be made during conventional PK tests. Palmer (1975) has suggested that attempts be made to create the

psychological conditions in the laboratory which seem to facilitate RSPK. During periods of high PK scoring, it could be determined if the EEG record showed CNS eruptions. Other facets of RSPK could be explored experimentally, such as the attenuation and focusing effects.

If CNS disturbances sometimes result in RSPK incidents, we should be attentive to the possibility that infants and animals with CNS problems might also be RSPK agents. The possibility that an infant was an RSPK agent has come up, I understand, in a recent study by Palmer. It should also be considered in at least one of the cases in the present survey (Pierce, 1973). However, the author of the report regarded the case as one without a living agent and I accepted this in my classification of the case.

Concluding Remarks

A small but significant proportion of RSPK agents have been diagnosed as suffering from the sudden and recurrent disturbances of the central nervous system known as epilepsy and many others show mental and behavioural characteristics suggestive of epilepsy or have problems which may bring on CNS disturbances. In view of the fact that it is only in recent years that we have paid much attention to the health of the RSPK agent and that the reports usually emphasize descriptions of the phenomena rather than the characteristics of the central person, it is highly suggestive, in my opinion, that as many as 22 of the 92 agents were reported on one or more occasions to have seizures, comas, etc. or to be prone to dissociative episodes while another 27 had other medical or psychological problems some of which may be associated with CNS dysfunction.

The CNS theory does not imply that all epileptics are likely to be RSPK agents. The CNS eruptions which result in familiar forms of epilepsy do not necessarily also result in RSPK. In the same way as CNS disturbances may bring on myoclonic seizures and no other epileptic symptoms, so CNS disturbances may result only in RSPK and not in any of the familiar epileptic symptoms. The theory also does not suppose that all poltergeist agents suffer from familiar forms of epilepsy.

Whether or not RSPK is associated with CNS eruptions, epilepsy has suggestive similarities to poltergeist attacks and may offer insights into the nature of "seizures" outside the physical organism. The etiology of this form of CNS dysfunction and the conditions which tend to enhance or inhibit seizures have parallels in the lives of many poltergeist agents and in the development of RSPK disturbances.

A theory for RSPK must be more than a simple extension of neurology. It requires a radical change in thinking. Usually advances in science are marked by new concepts: To understand the poltergeist, it may be better to give up some old ones. If we do not assume a sharp distinction between a mental or inner world and a physical or outer one, RSPK phenomena become less paradoxical. This approach does not result in a paucity of conceptual tools but in a great wealth: The laws and regularities which have been discovered by the physical sciences become hypotheses in the exploration of psychical processes and the patterns which have been found in psychical processes become hypotheses in the exploration of physical events.

RSPK occurrences are psychophysical. They simultaneously express patterns which we generally call physical and patterns which we call mental. A simple RSPK occurrence, such as a glass in the Miami warehouse which lifted itself over some objects I had placed in front of it (Roll and Pratt, pp. 446-47), suggests intelligent guidance. At the same time, the events conformed to the attenuation pattern and in other ways suggested an energetic process. Similarly, the focusing effects appear to be examples of perseveration at the same time as they can be understood in terms of wave theory (Joines, 1975). Fast-slow movements, the "floating", curved, and rotating trajectories make sense in terms of the rotating beam theory (Roll, Joines, Burdick, 1973, 1974) -- assuming a built-in guidance system.

Teleportations, if real, suggest in a particularly striking manner a psychological side to RSPK. Our thoughts are not limited by walls and closed space: we can easily and immediately call up ideas or images relating to distant objects. Mentation has another framework: it is a function, at least in part, of the associative cortex. Perhaps a neural discharge of sufficient strength which combines two images, such as mirror and bedroom, is reflected in the sudden appearance of an actual mirror in the room.

RSPK occurrences are also social or interpersonal. In large measure it appears to be the agent's relation to the family or group which results in the build-up of tension that leads to the RSPK outbreak. When the investigator arrives, he becomes part of the situation and may enhance or inhibit the events (Roll, 1977, pp. 50-55).

RSPK outbreaks are rare, but usually highly traumatic to the family or group they descend upon. The possibility that they are related to brain dysfunctions makes it possible to envisage a poltergeist therapy similar to treatments for epilepsy (Solfvin and Roll, 1976) The main interest of the poltergeist, however, may be what it suggests about ordinary people and the everyday world, in particular about the relation between what we call mental and physical processes. In the words of Hans Bender "Poltergeist phenomena are...a via regia or royal road to an understanding of man, of his position in nature and of nature herself" (Bender, 1974, 142).

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CORRECTION NOTE

A printer error occurred in the paper "Sex Differences and Dominance Effects in a Novel Clairvoyant Psi Task" by C.L. Sargent in this journal for November 1977 on page 51.

The first sentence of the second paragraph on this page (line 6) read "Considering a breakdown of this scoring, we find that the group shows significant psi-missing on cross-sex targets (t=2.24, df=49, p=.034)". This sentence should have read: "Considering a breakdown of this scoring, we find that the group shows significant psi-missing on cross-sex targets (t=2.24, df=49, p=.03); the psi-missing on male names (which is, of course, not independent) is also significant but not at such a low p-value (t=2.18, df=49, p=.034).

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PUBLICATION POLICY

Twice a year the Parapsychology Laboratory of the University of Utrecht publishes the European Journal of Parapsychology. The object of the European Journal of Parapsychology is to stimulate and enhance the interest within this field, especially in our corner of the world, by communicating research results and issues related to professional parapsychology. Although there will be an emphasis on experimental work, theoretical articles are also welcome. Contributions from all over the world will appear in the journal.

A hallmark of the European Journal of Parapsychology is the attempt to avoid selective reporting, that is, the tendency to bury 'negative' results and only to publish studies that 'turn out'. To avoid turning the journal into a graveyard for all 'unsuccessful' studies, we require that the acceptance or rejection of a manuscript should take place prior to the phase when the experimental data are collected. The quality of the design and methodology and the rationale of the study should be judged as per se more important than the level of significance of the outcome of the study. As a practical rule, we advice a contributor of an article to submit a design of his planned study before the study is actually carried out. The rationale of the study should be stated, as well as all the hypotheses related to it. Furthermore one should try to specify the number of subjects, the number of trials, etc., plus the type of statistical methods one plans to use for one's evaluation.

Priority will be given to the publication of studies which fulfil the above-stated publication policy.

The final manuscript with presentation of results must reach us two months in advance of the official publication date, that is May 1st and November 1st.

ARE WE READY FOR THE ESTABLISHMENT OF A EUROPEAN BRANCH OF THE PARAPSYCHOLOGICAL ASSOCIATION?

Martin Johnson

A few years ago the Southeastern Regional Parapsychological Association (SERPA) was established and in February of this year this organization held its Fifth Conference at West College, Cassoltown, GA.

In connection with the 21st PA Convention in St. Louis it was disclosed that two more regional branches of the P.A. are under way in the United States. At a symposium on 'Current Trends in European Parapsychology' the (present) author raised the question whether the time may be ripe to establish a European branch of the PA, an idea which I have previously touched upon (See EJP, V1, No 3, p. 2). Reportedly the council of the PA was quite in favor of this idea. However, before trying to establish such a branch one should carefully consider what the use and the objectives of such an organization should be.

Most Europeans taking an active part or interest in parapsychology might feel that the establishment of such a branch would be a good thing to balance the rather strong American dominance which we admittedly have within the PA. The question is whether that dominance really is an unwarranted one, if one considers the clear lead at least in output of professional papers which Americans do have. A glance in the PA membership list for January, 1978, reveals that there are approximately 70 full members among the Americans compared with about 30 European full members. Personally I was rather surprised to find that not only was the American dominance not greater but that there are so many full European members! The already comparatively strong European representation within the PA may in itself qualify for the establishment of a European Regional Branch (ERPA). However, most relevant in this connection is to ask the question: What should be the purpose of such an organization? Should it be to arrange regional conferences or conventions at which papers are presented (in the same way as so far has been the practice at the SERPA-meetings)?

I am far from sure that that is a realistic or beneficial approach in a European setting. Based on experience one could say that it is far from easy to obtain a sufficient number of high quality papers for the two main events that we presently have - the annual convention of the PA and the annual Conference of the SPR. In short, I don't think that a regional European branch of the PA should see as its task to arrange full fledged Conventions where papers are presented. That would imply a sort of competition with its mother-organization and with the SPR Conference. It would also imply a tremendous amount of organizational work, and there would also be considerable financial problems involved. The keyword should not be competition but cooperation. As I visualize the issue, the main functions of the ERPA should be:

1) To arrange regional colloquiums or review-meetings at which members and associates of the PA, actively engaged in research could discuss research projects under way, or research problems leading to the outline of an experiment. The meetings would offer an excellent opportunity for exchange of ideas and obtaining useful criticisms before the experiments actually have been carried out.

1.1) The meetings should also be helpful in integrating disparate research efforts. One of the functions of the meetings could be that it should serve as a clearing house regarding what kind of contrivances that are available in the field - for instance what kind of hardware and software that are in existence at a certain research center.

2) A second and at least as important function of a European branch should be to stimulate and guide students and persons interested in carrying out some piece of research but living in areas which are less integrated in the mainstream of parapsychological research. It should give them a chance to see and learn and to obtain many pieces of information of importance for their design and carrying out of a field study or laboratory work. In this way the organization could be of great educational importance and could exert a guiding or steering influence on enthusiastic but less sophisticated newcomers in the field who may need help and supervision to be able to carry out a study which ideally could lead to a paper of such quality that it could be considered for presentation at the regular PA Convention.

Perhaps the first appropriate opportunity to come together and discuss tentative plans for the establishment of a European regional branch would be at the SPR Conference in Edinburgh, to be held in

April of 1979. Meanwhile it would be good if interested readers pondered upon the idea and gave feedback to this journal about what they think we should do within a regional branch of the PA.

TESTING THE 'ADVANCED WAVE' HYPOTHESIS: AN ATTEMPTED REPLICATION

Dick J. Bierman

1. INTRODUCTION

In a previous study an exploratory experiment was reported in which precognition with immediate feedback of the targets was compared with precognition with delayed feedback of the overall run-score. (Bierman, 1977). On the basis of the 'Advanced Wave' hypothesis of Feinberg (1974) little or no precognition is to be expected in the delayed feedback condition. The experimental results however revealed a marginally significant scoring rate in this condition while in the immediate feedback condition no extra-chance scoring took place.

The previous study was submitted for publication after the phase of experimental data collection was completed contrary to the publication policy of the EJP. Therefore it was felt necessary to do at least one well defined replication.

2. METHOD

The events to be predicted are random numbers in the range (1 to 5), generated by an electronic RNG within 10 milliseconds after the subject has entered his guess at a computer terminal. The author, who had contributed most of the data (37 out of 42 runs) to the previous experiment, served as the sole subject in the present experiment. Each morning and each afternoon in the period from Jan. 16 to Feb. 21, 1978 a single run was performed as part of a daily routine of PK and PREC selftests. A run consists of 50 trials. The first and all other odd numbered trials are followed by feedback of the generated target number. On the second and all other even trials no feedback is given. The two feedback conditions are thus mixed within one run. After completion of the run the subject gets feedback of the overall runscore of the 25 even numbered trials. The expectation to be tested in this experiment was that psi would

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occur in the delayed feedback condition. Such a finding would reject the 'Advanced Wave' hypothesis as an explanation for precognition.

3. RESULTS

3.1 The effect of feedback condition

In total 50 runs were done, twenty-five in the morning and an equal number in the afternoon. The results are given in Table 1 together with those obtained in the previous study.

TABLE 1

	Condition				
	feedback			non feedback	
	trials	score	dev	score	dev
Present study					
morning	625	122	-3	129	+4
afternoon	625	121	-4	130	+5
Previous study	1050	204	-6	235	+25*
Totals	2300	447	-13	494	+34

* $p < .05$ (one tailed)

As in the previous study, the scoring rate in the non feedback condition is higher than in the feedback condition. However, none of the cells nor any of the differences between conditions (as evaluated by t-tests) is significant. There is no evidence in the planned analyses that psi occurred in this experiment.

Altogether the significance of the marginal extra-chance scoring in the pilot experiment reduces too as that finding now turns out to be the only statistically relevant one out of 9 tests.

3.2 Post-hoc analyses

Nonetheless, it was considered worthwhile to perform a number of post-hoc tests on the data for any suggestion of light they might shed on the 'Observational Theories'. (See discussion).

3.2.1 Salience effects

The fact that no psi was measured in the overall runscores might be due to mixed psi hitting and missing within the runs. Therefore the total scores over the 25 odd trials per run (trials with feedback) were divided into 5 subscores over the first 5, second 5 etc. trials. The same was done for the 25 even trials. (non feedback condition). A further test was done to check for a possible correlation between the subscores in both conditions. Such a correlation would strongly argue against the 'Observational Theories'. The subscores per condition, summed over the 50 runs, are given in Table 2. It can be seen that none of the figures differs significantly from the MCE of 50 hits. The correlation of the subscores in the two conditions was not significant neither.

TABLE 2

	subscores					
	1-5	6-10	11-15	16-20	20-25	total
feedback	49	50	48	43	53	243
non feedback	48	58	57	47	49	259

3.2.2 Correlation of PREC scores with preceding PK-test

As was mentioned, the reported PREC runs were part of a daily routine of self-tests. Preceding each PREC run, a short PK (RNG) test was done. The precognition scores are split dependent on the over all scoring direction of those PK tests. The results are given in Table 3. The number of precognition runs presented here is smaller than 25 per condition because PK tests were implemented a week after the start of the precognition series and due to PK tests with exact chance outcomes. Although above chance scoring in the PK-test is followed by the same in the PREC-test, while below chance scoring

in the PK-test is followed by below chance scoring in the PREC-test, the actual figures do not differ enough to make the 'predictive' value of the PK-test significant.

TABLE 3

	Preceding PK scoring direction						
condition	PK 'hitting'			PK 'missing'			
	runs	score	dev	runs	score	dev	t
feedback	16	84	+4	22	104	-6	.45(df=35)
non feedback	16	92	+12	22	106	-4	.93(df=35)
totals	32	176	+16	44	210	-10	1.01(df=72)

3.2.3 Run-score distributions

Run-score distributions were calculated for both conditions to explore the possibility that withholding feedback on the subjects performance in the non feedback condition would result in 'run-score targeting', that is the subjects goal becomes to obtain an above chance score irrespective of the magnitude. One could argue that this over all goal is obscured in the feedback condition by the specific goal to hit on a trial by trial basis.

It can be seen that runs with a score of 5 (chance expectation) are underpopulated in favor of run-scores of 4 and 7 in both conditions. The number of runs with a runscore 5 is 11 out of 100. This differs significantly from the expected number of 19.6. ($z=2.17$). Although this is not very impressive it is noteworthy because of its possible theoretical relevance. (See discussion).

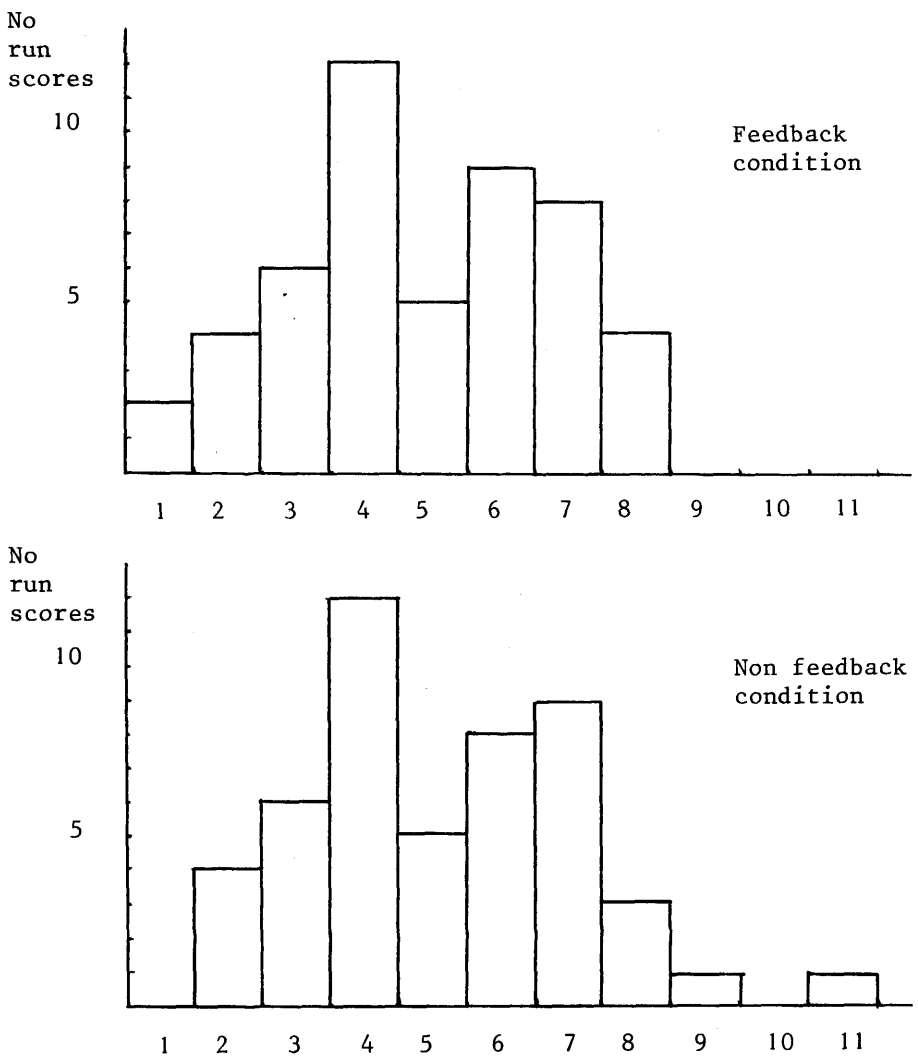

No run scores
10
5
Feedback condition
1 2 3 4 5 6 7 8 9 10 11
No run scores
10
5
Non feedback condition
1 2 3 4 5 6 7 8 9 10 11

FIGURE 1

DISCUSSION

The present study failed to replicate a marginal feedback effect found in the pilot study; therefore no conclusion can be drawn about the 'Advanced Wave' hypothesis.

The analysis for salience effects was done because any salience effect in the non-feedback condition would argue for a time dependency which is difficult to explain in terms of the 'Observational Theories'. This disconformation of the theoretical prediction would be even stronger if the salience effects in both feedback conditions turned out to correlate. However no effects were found.

Correlation with preceding PK tests on the other hand would argue for an identical basis for PK and PREC phenomena: the 'Observational theories' contain such an elegantly unitary model. The t value of 1.01(df=72) is not high enough to favor an 'Observational Theory' interpretation.

The run-score distributions in both feedback conditions show a similarity with regard to the underpopulation of exact chance runscores. Although this phenomenon is only marginal and might very well be one of those post hoc findings there are two reasons to pay somewhat more attention to it.

In the first place the same phenomenon turned out to be present in old data which were reanalysed to check for it. (Note that this inference is not so strong as it seems. It was shown in a recent study that such a finding could very well be due to retro-active 'influence' on the original data by the person now re-analysing it. (Weiner and Bierman, 1978).

In the second place it can easily be shown that within the framework of information based models (Walker, 1974; Mattuck, 1976) a decrease of chance runscores can be expected if the subject simply targets for a positive runscore instead of for hits on a trial by trial basis.

ABSTRACT

Fifty runs of precognition self-test were done. In half of the 2500 trials no feedback was given. In this condition the scoring rate was non significantly higher than in the condition where trial by trial feedback was given: this in the same direction as the condition effect found in the pilot study. As no significant results were obtained here no conclusion on the 'Advanced Wave' hypothesis could be drawn. A discussion of the measured run-score distributions is given to emphasis its possible theoretical relevance.

ACKNOWLEDGEMENTS

The present study was done during a stay as visiting research fellow at the FRNM in Durham. I want to thank the staff of the Parapsychological Institute for the stimulating environment which they create and which, I feel now, is a fundamental requirement to perform reasonable parapsychological research.

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AN EXPERIMENTAL STUDY ON THE INFLUENCE OF EXPERIMENTERS' EXPECTANCIES ON LIFE SPAN AGE DIFFERENCES IN PK-PERFORMANCE

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INTRODUCTION

As with the greater part of the experiments in experimental psychology, by far most experiments in parapsychology have been conducted with undergraduates, high school students, in brief, with young adults serving as subjects. The reason for this is quite clear: in most research centers a lot of students are available as potential volunteer subjects. This situation may lead to biased experimental findings as Rosenthal and Rosnow have pointed out (1). Concerning parapsychology, it is most investigators' opinion that a subject's age has very little or nothing to do with his psi ability. However, there has been very little systematic research on this topic in parapsychology. Gardner Murphy suggested that paranormal abilities are at their height when normal biological functions are also at their height (2).

Research on aging and human skills has pointed out, although in a gross simplification, that in tasks which make their chief demands upon organic capacities of man, the performance level related to age tends to follow a skewed bellshaped curve (3). The level of performance rises from practically zero to a peak in early adulthood, and then declines to some in-between level. Following Murphy's suggestion, for a similar paranormal task, for example a PK-task, one can expect a similar relationship between level of performance and age.

On the other hand, experiments with young children have been carried out, giving rise to the idea that an inversed relationship can be expected. For instance, Louwerens (4) and van Busschbach (5) obtained highly significant results in an ESP-test with nursery-school pupils. Also Johnson, Cronqvist, Danielson and Mondejar (6)

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and Spinelli (7) reported significant results in testing children. The present authors' experiences in the past lead to the idea that such an inversed relationship must be expected for the level of PK-performance in relation to age.

However, there are indications that one has to be very careful in interpreting certain effects found in parapsychology as being related to subject characteristics. It must be borne in mind that the experimenter himself may (unconsciously) play an important role in the creation of the results of his experiments. His beliefs, motivation, expectancy may be reflected in his experimental findings, which is known as the experimenter effect, extensively discussed by Kennedy and Taddonio (8).

The present paper describes an experiment undertaken to study the effect of the experimenters' expectancies with regard to differences in PK-performance through the life span.

A large number of subjects were tested for PK-performance by two groups of experimenters, with different expectancies with regard to scoring levels related to subjects age,

The first experimenter group expected that children would obtain the highest scores, adults the lowest scores, and aged subjects in-between scores. This is defined as the positive expectancy (PE).

An opposite expectancy, defined as negative expectancy (NE), was induced in a second group of experimenters. This group expected that adults would obtain the highest scores, children the lowest, and aged subjects in-between scores. In addition, the PE-experimenters expected that female subjects would score higher than male subjects, while the NE-experimenters expected the opposite.

It must be noted that defining the first mentioned expectancy as positive, and the second as negative, only expresses that the expectancies are opposite to each other.

It was hypothesised that scoring levels of the subjects are in accordance with the experimenter expectancy towards it.

METHODOLOGY

Experimenters and their expectancies

The NE-experimenter group was formed by six freelance researchers selected from members of the 'Braidkring', a group of Belgian intellectuals with a strong interest in paranormal and mystic phenomena. Selection was based on their enthusiasm to assist in quantitative parapsychological experimentation.

During one of the regular meetings of the Braidkring-members, SRU research associates gave a short exposé of their research activities, and it was stressed that experimental findings considerably gain in

value if they can be replicated by others. Next, the Braidkring members were asked to perform an experiment on the possible relationships between age and PK-ability, as a replication of an earlier SRU experiment. After selection of the free lance researchers, by means of discussions and interviews, the negative expectancy was induced. This was done by showing them relevant results from research on aging and human skills, and fictitious data from parapsychological experiments on this topic (in fact such experiments have never been carried out by the SRU). It was stressed that the same type of relationships seems to hold for both research fields.

The PE-experimenter group consisted of three SRU research associates. Their expectancy towards age differences in PK-performance was based upon the idea that paranormal abilities of the individual show up strongest when the normal communication and interaction with the outside world are lessened.

On the other hand, being familiar with recent results and discussions about experimenter effects, they also knew that any effect found in an experiment on age dependency of PK-ability could be a consequence of the underlying expectancy towards it. As the PE-experimenters themselves designed the present experiment, they knew of the hypothesis of experimenter expectancy induced group differences. They thus expected to find such differences in their results due to the different expectancies of the experimenters in the PE and NE groups.

Subjects

Subjects were divided into three age categories: children, adults, and the old age group, indicated resp. as groups A1, A2, and A3.

Group A1 was formed by children with a maximum age of 5 years, while no restrictions were made for their minimum age. The only requirement was that these children were able to distinguish different die-faces, as dice-throwing was used as a PK-task. Concerning this ability to distinguish, advice was given by the children's teachers (the children were obtained mainly from nursery schools).

Group A2 consisted of adults, with an lowerlimit age of 15 years, and an upperlimit of 35 years.

These subjects were obtained from different sources. Mainly they were undergraduate students from various disciplines, while others were members of various clubs.

The subjects of age group A3 all exceeded the age of 65 years (retirement age). They were required to enjoy a good mental health.

These subjects were obtained from homes and social clubs for the aged.

Each experimenter group tested 324 subjects, 108 subjects within each age group. No restrictions were made concerning the number of males versus females in each age group, or in the total number of subjects.

Each subject was tested just once, so in total 648 subjects were tested.

The age limits of the subject groups as described above were chosen arbitrarily, although it was felt that a reasonable division of man's lifetime was obtained.

Procedure

For each subject the PK-task consisted of twenty throws with a precision die (Mason & Co), with which the subject must try to hit a given target face. The die was shaken in a regular dice cup and then poured in the open mouth of a randomizer, which must prevent manipulated throwing. This randomizer is described in the appendix.

A run length of twenty throws was chosen as to retain the subject's interest in the test. Often long runs give rise to growing boredom from the subject's side, which may lead to a decline effect in the scores. It was hoped that this could be avoided by using such a short run length.

The target faces were determined in advance, in a counter-balanced way. For each group of 108 subjects three latin squares were used as target determiners. The squares were randomly generated from basic sets given by Fisher and Yates (9), by means of a standardized procedure given by West (10). Thus within each group of 108 subjects all die faces were targets an equal number of times, ruling out possible die bias effects.

Another source of possible bias effects may be found in the randomizer. For practical reasons three randomizers were used (limiting the total time duration of the experiment, as more experimenters could test subjects simultaneously). Although the randomizers were of the same type, it is possible that they show some unintended constructional differences. This may cause two types of bias in principle: an effect from differences in randomizing properties, and a psychological preference effect. Concerning the latter one, it is thinkable that a particular feature of a randomizer may cause a subject preference for that particular randomizer, which may be reflected in the scores. In order to distribute these possible bias effects equally over all subject groups, each of the randomizers was used within each of the subject

groups. Actually each randomizer was used in combination with one of the latin square sets. It must be noted also, that a randomizer was used always with the same die and cup.

Subjects were tested individually by one of the experimenters, in close cooperation with a familiar person for the particular subject(s). This person could be the children's teacher, chairman of an association, welfare worker etc. In most cases the experimenter recorded the results of the trials, sometimes this was done by the familiar person. In all cases both of them checked the records, immediately after throwing. Besides a decrease in recording error risk, it was felt that an approach like this would avoid the subjects being ill at ease because of the unfamiliar experimenter (which seems particular relevant for children and for the aged). The avoidance of such a disposition is desired because there are indications that it may be correlated with low scoring (11).

The subjects received the instructions to hit the target face as often as possible. To most of them it was said that the experimenter wanted to study certain laws of probability. Of course this could not be told to the children. To them it was presented as a kind of new game. Actually, the children considered it as a very exciting new game indeed. Most of them were very excited about seeing the die jumping and rolling in the randomizer.

Analysis of data

The data of this experiment have been analysed by means of a three way analysis of variance, with factors age (three levels), subject sex (two levels) and experimenter expectancy (two levels). As we were interested in the factors and their levels as treated here only, all factors are considered as fixed.

Of prime importance of course are the interactions of "Age x Expectancy" and "Sex x Expectancy", as these have to be significant at least to confirm the hypothesis.

However, significance of these interactions is not sufficient, the directions of the scoring differences have to be in accordance with the expected directions, too. To establish this, it is necessary, in case of significance, to analyse for simple interaction effects.

Two more analyses were planned in advance.
The first one is a two way analysis of variance with factors age (three levels) and randomizer (three levels, resp. randomizers R1, R2, and R3). It was thought that a possible psychological preference effect for one of the randomizers would not show up for each of the age groups. This could be detected by the interaction effect. The

main effect of randomizers could detect possible differences in randomizing properties. The last analysis is a straightforward frequency analysis of the thrown die faces (by means of a chi-square),in order to detect possible bias or preferences.

RESULTS

The distribution of the subjects and their mean PK-score over the various age-sex-expectancy categories is shown in Table 1. The summary of the three way analysis of variance is given in Table 2. It must be noted that the within cell variance can be computed directly. Since no estimation is required, one can use df= ∞ in the denominator of the F-ratios.

This summary shows a significance of the three factor interaction. In order to find out where this unexpected result comes from, an analysis for simple interaction effects has been carried out. Here, the sums of squares of the main analysis are split up into components for each of the age groups. The results are shown in Table 3.

The"S x E" interactions for the adult age group and for the old aged group are significant. When for these categories the sums of squares are split up further for each of the expectancies, significances are found for the "(A2,NE) x S" simple effect ($F_{1,\infty} = 5.99$, $0.01 < P < 0.03$) and for the "(A3,PE) x S" simple effect ($F_{1,\infty} = 7.29$,$0.005 < P < 0.01$).
The NE-experimenters obtained a significant scoring difference for the adult subjects, more specifically, female subjects obtained higher scores than males (mean PK-scores resp. 3.90 and 3.10). The PE-experimenters obtained a significant scoring difference for the aged subjects: here also a higher score for the female subjects than for the males (mean PK-scores resp. 3.74 and 2.73).

The results of the analysis for possible bias effects of randomizers, a two way analysis of variance with factors age and randomizer, are summarised in Table 4. The main effect of randomizer is nonsignificant, an indication that no effects of possible differences in randomizing properties occurred. However, there is a weak significant interaction effect of "Age x Randomizer". This may indicate a psychological preference effect for one (or more) of the randomizers, for one (or more) of the age groups.

More information on this effect can be obtained by computing the variances between sexes for each combination of age group, expectancy and randomizer. Table 5 shows the results.

Several of these combinations show a significant scoring

TABLE 1

Distribution of subjects over the various categories, and mean PK-scores

		Experimenter Expectancy	
Age group	Sex	PE	NE
A1	M	n= 56 m= 3.26	n= 67 m= 3.37
	F	n= 52 m= 3.09	n= 41 m= 3.14
A2	M	n= 63 m= 3.55	n= 46 m= 3.10
	F	n= 45 m= 3.22	n= 62 m= 3.90
A3	M	n= 26 m= 2.73	n= 49 m= 3.32
	F	n= 82 m= 3.74	n= 59 m= 3.08

n= number of subjects
m= mean PK-score
PE= Positive Expectancy
NE= Negative Expectancy
M= Male
F= Female
A1= children
A2= adults
A3= old aged

TABLE 2

Summary of three way analysis of variance

source of variation	SS	df	MS	F	
Age (A)	7.04	2	3.52	1.27	
Sex (S)	2.71	1	2.71	0.98	
Expectancy (E)	0.06	1	0.06	0.02	
A x S	8.86	2	4.43	1.60	
A x E	6.73	2	3.37	1.21	
S x E	0.00	1	0.00	0.00	
A x S x E	31.98	2	15.99	5.76	(a)
within cell		∿	2.77		

(a) $0.001 < P < 0.005$

TABLE 3

Analysis for simple interaction effects, sums of squares split up for each of the age groups

source of variation	SS	df	MS	F	
A1 x S	2.26	1	2.26	0.82	
A1 x E	0.05	1	0.05	0.02	
A1 x (SxE)	0.00	1	0.00	0.00	
A2 x S	3.36	1	3.36	1.21	
A2 x E	1.18	1	1.18	0.43	
A2 x (SxE)	16.21	1	16.21	5.85	(a)
A3 x S	5.93	1	5.93	2.14	
A3 x E	5.04	1	5.04	1.82	
A3 x (SxE)	15.89	1	15.89	5.73	(b)
within cell		∿	2.77		

(a) $0.01 < P < 0.02$ (b) $0.01 < P < 0.05$

TABLE 4

Summary of analysis of variance with factors age and randomizer

source of variation	SS	df	MS	F	
Age (A)	7.04	2	3.52	1.26	
Randomizer (R)	3.86	2	1.93	0.69	
A x R	32.28	4	8.07	2.90	(a)
within cell		∿	2.77		

(a) $P < 0.05$

TABLE 5

Analysis for scoring differences between sexes as a detector of psychological preferences for randomizers

source of variation	df	MS	F	
(A1,PE,R1)xS	1	1.47	0.17	
(A1,PE,R2)xS	1	0.06	0.02	
(A1,PE,R3)xS	1	0.69	0.25	
(A2,PE,R1)xS	1	0.35	0.13	
(A2,PE,R2)xS	1	0.90	0.32	
(A2,PE,R3)xS	1	0.87	0.31	
(A3,PE,R1)xS	1	5.34	1.93	
(A3,PE,R2)xS	1	19.59	7.07	(a)
(A3,PE,R3)xS	1	0.73	0.26	
(A1,NE,R1)xS	1	5.29	1.91	
(A1,NE,R2)xS	1	10.67	3.85	(b)
(A1,NE,R3)xS	1	12.62	4.56	(b)
(A2,NE,R1)xS	1	4.01	1.45	
(A2,NE,R2)xS	1	50.80	18.34	(c)
(A2,NE,R3)xS	1	1.68	0.61	
(A3,NE,R1)xS	1	16.39	5.92	(a)
(A3,NE,R2)xS	1	0.51	0.18	
(A3,NE,R3)xS	1	0.40	0.14	
within cell	∿	2.77		

(a) $P < 0.05$ (b) $P < 0.01$ (c) $P < 0.001$

TABLE 6

Frequency analysis of thrown die faces

die face	obs. freq.	deviation
1	2185	25
2	2128	-32
3	2095	-65
4	2166	6
5	2112	-48
6	2274	114
	$\chi^2_5 = 9.82$, $P > 0.10$	

difference between sexes. Sometimes R1, sometimes R2, and sometimes R3 seems to be favoured or disliked.

The frequency analysis of thrown die faces is shown in Table 6. No significant deviations from the expected frequencies emerged ($\chi^2 = 9.82$,df=5).

DISCUSSION

The hypothesis that the subject's scores are in accordance with the experimenter's expectancies with regard to them, is not confirmed in this experiment. Also no significant age or sex differences are found. However, an unexpected result emerged: adult females tested by NE-experimenters obtained higher scores than males, while aged females tested by PE-experimenters also scored higher than (aged) males).

These effects seemed to be caused by psychological preference effects for randomizers.

The failure to find significant age or expectancy effects could be a consequence of the rather complicated composition of the experimenter groups. In particular, the status of the PE-experimenter group is unclear. To follow a distinction in function, as given by Barber (12), the PE-experimenters can be seen as experimenters and as investigators at the same time. As investigators they did not expect a real age dependency, but an age effect as a consequence of the expectancy they had as experimenters. Although this expectancy was partly due to their role in the experimental design, it was also based on suggestive evidence from previous observations (spontaneous cases). Thus, in some ways the expectancy of the PE-experimenters had a stronger basis than that experimentally induced in the NE-experimenters. An ideal in designs such as used here would be to confine observations to groups with natural expectancies or motivations, but this is in practice hard to realise. Such a situation was taken advantage of in a study of Jacobs and Breederveld (14).

A puzzling finding of the experiment reported here, is that adult females tested by NE-experimenters obtained higher scores than adult males. The same scoring direction was found for the aged subjects tested by PE-experimenters.

This shows some weak correspondence with findings of psychological research on aging and ("normal") human skill. Here it was found that sex differences in motor performance become more apparent with increasing age (13). And, although in most physical and motor measures boys were superior to girls, on aiming tasks, which require motor coordination,girls were consistently better (15). As die throwing can be seen as a paranormal counterpart of a normal aiming

task, our unexpected finding suggest that the same type of age-sex interaction also holds for psi tasks. However, it must be borne in mind that a lot of research on aging and human skill has been done showing no sex differences. The same can be said about sex differences in psi tasks. So it seems too far fetched to speculate on the reasons why the adult and aged subjects tested by resp. the NE and PE-experimenter groups in particular showed the effect, or on the responsible features of the randomizers.

ABSTRACT

It is hypothesised that differences in PK scoring levels related to life span age differences are in accordance with the experimenters' expectancies towards them.

Each of two experimenter groups with inverse expectancies tested 324 subjects. These subjects were divided into three age categories: young children (maximum age 5 years old), adults (between 15 and 35 years old), and aged persons (over 65 years old).

The PK task of each subject consisted of twenty throws with a precision die, in which a target face must be hit as often as possible. To prevent manipulated throwing, dice throw randomizers were used.

An analysis of variance indicated that the hypothesis was not confirmed. However, an unexpected interaction effect between age, sex and expectancy turned out to be significant below the P=0.005 level. This effect seemed to be caused by psychological preferences, reflected in the PK scores, for one (or more) of the dice throw randomizers.

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We hope they excuse us for the policy we followed to induce their negative expectancy.

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APPENDIX

Description of die throw randomizer

The randomizer consists of a transparant perspex construction, with a height of 25 cms, and side surfaces of 16 cms.
The top side was left open (mouth of the randomizer).
Inside, four grids were placed, the distance between the grids being slightly greater than the body diagonal of the die. The grids consisted of thin, horizontal bars, the distance between the bars also slightly greater than the body diagonal of the die.

The bars of the second and fourth grids were perpendicular to those of the first and third grids. The bars of the parallel grids were shifted about a distance of approximately half the body diagonal of the die.
When a die is thrown into the open mouth of this randomizer, at least one, on the average more grid(s) are hit, in a jumping and rolling movement of the die.
The bottom of the randomizer consists of a wooden surface of 16x16 cms. This surface is covered with green felt.

In two opposite side surfaces a large rectangular hole was made, in order to be able to remove the die from the bottom.

One of the randomizers constructed in this way, was tested for randomization properties, prior to the experiment.
About 10,000 releases of a die from a specified position did not show a significant deviation from theoretical randomness, as measured by a multiple runs test.

ATTEMPT TO OBSERVE PSYCHOKINETIC INFLUENCES UPON A RANDOM EVENT GENERATOR BY PERSON-FISH TEAMS

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INTRODUCTION

Recently, a series of experiments was conducted in our laboratory exploring the feasibility of a new procedure for testing psychokinesis in animals. An experimental apparatus was arranged so that "aggressive" tropical fish (siamese fighting fish, cichlids, barbs) received immediate positively reinforcing feedback whenever "hits" were registered by an electronic random event generator. Feedback took the form of a twenty-second presentation of a mirror to individually-tested fish. Previous behavioral research (Thompson and Sturm, 1965; Hogan, 1967; Melvin and Anson, 1970) had already indicated that the stereotyped aggressive display elicited by mirror-stimulation was positively reinforcing to these animals. It was predicted that aggressive fish might psychokinetically influence the random generator to produce an increased incidence of "hits" when the latter allowed opportunities to display aggressively. The results of four preliminary experiments were consistent with this prediction (Braud, 1976). The generator's hit rate did not differ from chance expectation when nonaggressive fish (goldfish, "community" tropical fish) were tested in the apparatus, presumably because mirror feedback was not reinforcing to these animals. A series of randomness trials, conducted with no fish in the apparatus, indicated that the random generator was free of bias.

In the present paper, we report the results of three additional experiments conducted to determine whether PK scoring rate might be augmented by the addition of "human helpers" to the design. In these experiments, human subjects worked together with the fish by attempting to influence the random generator so that more hits (mirror presentations to the fish) would be produced. It was

hoped that this "person-fish team" arrangement might be a motivationally optimal one. In fact, striking results have been reported by other investigators (Rhine, 1971; Watkins and Watkins, 1971; Wells and Klein, 1972) when person-animal teams were involved in other psi-testing paradigms.

Our a priori hypotheses were as follows:

(1) For each of the three experiments, the number of "hits" generated by the random event generator would exceed mean chance expectation. (CRs, one-tailed); and

(2) When the results of all three experiments were combined, the number of generated hits would exceed mean chance expectation (CR, one-tailed).

METHOD

Subjects

Thirty persons from the San Antonio area (fifteen male, fifteen female) participated as the "human helpers" in the experiments. Each of the three experiments involved ten persons. All thirty persons had previously participated in psi experiments at the Foundation, and had been recruited initially by means of newspaper and radio advertisements, posted notices, and invitations extended at various lectures by Foundation staff members. Subjects were selected for these particular experiments simply on the basis of interest and availability, and not because they had evidenced any particularly striking receptive psi or PK performance in prior experiments.

The "aggressive" fish were seven male Siamese fighting fish (Betta splendens) obtained from a local supplier. The fish were housed in aerated and filtered aquaria maintained at 24° C, with pH = 7.4. The fish were physically and visually isolated from one another by means of opaque partitions placed in their home tanks. The fish were fed daily with Shrimp-el-etts pelleted food; feeding time was at least thirty minutes after the last fish had been tested on a given day. A nine hour light/ fifteen hour dark maintenance cycle was provided by flourescent overhead lights. The fish were housed in a large closet, the closed doors of which provided complete light attenuation and partial sound attenuation.

Apparatus

The fish were tested in an apparatus previously described by Braud and Weibel(1969). This apparatus consisted of two white, wooden enclosures (25 x 22 x 23 cm), each containing two 25-watt light bulbs. The enclosures were separated from each other by 12 cm,

which allowed a clear plastic tank to fit between them. This tank, filled with water to a depth of 13 cm, held the fish during an experimental run. One of the enclosure walls adjacent to the plastic tank consisted of a one-way mirror and a single sheet of light-diffusing onionskin paper. When the lights in the nonmirrored compartment were illuminated, with those behind the mirror extinguished, the mirror was highly reflective. Reversing the illumination prevented reflection. The apparatus was wired to a Schmidt binary random event generator through appropriate relay and timing circuitry so that each "hit" (on the random generator) resulted in a 20-second mirror presentation. Hits ($p = 1/2$) and misses ($p = 1/2$) were automatically recorded on separate electromechanical counters as well as on punched paper tape. The permanent paper tape record permitted a trial-by-trial analysis of the data. The mirror apparatus was housed in the closet with the fish; the random generator, timer, relays, counters, and paper tape recorder were housed outside of the closet. The paper tape recorder was enclosed in a sound-attenuating box so that the sounds of its operation would not disturb the fish or the human helpers.

A small red light-emitting diode (LED), with associated power supply, was attached to the mirror apparatus so that it would be visible to a "human helper" who sat before the apparatus in a comfortable chair. This light, which was not visible to the fish in the apparatus, served as a signal to the helper that a trial was imminent, i.e., that a "decision" was about to be made by the random generator outside the subjects' cubicle.

Procedure

When a subject arrived for his appointment, the experimenter took him to the testing cubicle and discussed briefly the rationale, procedure, and findings of the fish - PK studies which had already been completed (Braud, 1976). The subject then was told the aim of the present experiments--to determine whether the addition of a "human helper" would improve PK resuls above those obtained with fish alone. It was explained to the subject that he would be given a signal (by means of the red LED) five seconds before the random generator made a decision for that trial. While the light remained on (five seconds), the subject was to intend for a "hit" to occur, i.e., for the generator to produce a mirror period. The subject was told that the fish would probably be influencing the generator to maximize hits and that he should cooperate with or help the test fish achieve this goal. The human helper was told to use whichever strategy he preferred. Both fish and human helper received immediate feedback for each trial outcome (presence or absence of mirror).

Each subject was allowed to choose one of the seven fish as the other member of his "team". The human helper then placed the chosen fish into the test apparatus, the experimenter extinguished the overhead lights, closed the cubicle door, and began the session. At the beginning of each run, the fish and its helper were allowed a five-minute period to adapt to the test conditions. During this pretest adaptation period, the random generator was turned off and the mirror was nonreflective. After the adaptation period, a test run of 100 trials was initiated. The random generator was turned on and trials were run at a rate of one every 20 seconds. A "hit" was defined as the condition in which the oscillator of the random generator was stopped by the internal random source so that a count on the right (hit) counter was registered; a "miss" was an internal condition of the random generator associated with a count on the left (miss) counter. A hit resulted in a 20-second mirror presentation to the fish; a miss resulted in a 20-second nonmirror period. After starting the run, the experimenter left the room and did not return until a timer signalled the end of the 33.3-minute run. He then recorded the hits and misses from the counters, returned the fish to its home tank, and discussed with the subject the results of the test run.

The first two experiments consisted of one hundred 20-second trials and were conducted by W.B. The third experiment consisted of two hundred 10-second trials and was conducted by J.K.

RESULTS

The raw data for each of the three experiments are given in Table 1. Summary statistics for the experiments, separately and combined, are given in Table 2. An examination of these tables reveals that our first hypothesis was confirmed in the case of Experiment 1, but not in the case of Experiments 2 and 3. Our second hypothesis (that the combined series would yield extra-chance results) was confirmed.

Randomness Tests.

A total of 4,000 randomness test trials (equal to the total number of experimental trials conducted) were distributed nonsystematically throughout the time period of the experiments. During these trials, the random event generator was allowed to run as usual, but no fish was present in the test tank. Of these 4,000 trials, 2,004 were "hits", yielding a deviation of + 4, a CR of 0.13, and a non-significant p. There was a 50.10 percent hit rate. Thus, the event generator was indeed functioning randomly.

TABLE 1

Raw Data For The Experimental Series

Experiment 1				Experiment 2				Experiment 3			
Subject	Fish	Hits	Devn.	Subject	Fish	Hits	Devn.	Subject	Fish	Hits	Devn.
1	4	59	+ 9	1	6	54	+ 4	1	4	100	0
2	2	51	+ 1	2	5	51	+ 1	2	5	109	+ 9
3	4	52	+ 2	3	7	52	+ 2	3	2	108	+ 8
4	4	48	- 2	4	6	48	- 2	4	4	102	+ 2
5	3	57	+ 7	5	6	50	0	5	5	103	+ 3
6	7	54	+ 4	6	3	51	+ 1	6	2	110	+ 10
7	4	52	+ 2	7	5	50	0	7	4	92	- 8
8	1	51	+ 1	8	4	47	- 3	8	5	95	- 5
9	4	56	+ 6	9	3	44	- 6	9	2	111	+ 11
10	6	51	+ 1	10	4	57	+ 7	10	4	100	0
M.C.E. = 50				M.C.E. = 50				M.C.E. = 100			
CR = 1.96				CR = .25				CR = 1.34			

TABLE 2

Summary Statistics for the Experimental Series

Series	Trials	Hits	Misses	Deviation	CR	p*	% Hits
Experiment 1	1000	531	469	+ 31	1.96	.025	53.10
Experiment 2	1000	504	496	+ 4	.25	<.05	50.40
Experiment 3	2000	1030	970	+ 30	1.34	<.05	51.50
Combined	4000	2065	1935	+ 65	2.05	.02	51.62

*all ps are one-tailed

DISCUSSION

When aggressive fish were tested alone in the present PK paradigm (Braud, 1976), significant PK-hitting occurred, with a performance level of approximately two percent above chance expectation. It was expected that, in the present experiments, the addition of human helpers to the design would be accompanied by an increased scoring rate. While significant PK-hitting occurred in the present experiments, the performance level was again approximately two percent greater than chance expectation. Thus, the fish alone scored just as well as the person-fish "teams".

A scoring superiority of person-fish teams over fish tested alone would be expected on the basis of:

(1) previous empirical findings of excellent psi scoring by person-animal "teams" of various sorts (e.g., Rhine, 1971; Watkins and Watkins, 1971; Wells and Klein, 1972);

(2) the prediction, in Schmidt's (1975) mathematical psi model, that an "addition" effect should occur in which a strong psi effect can be induced by a combination of weak psi sources when the psi sources influence the same random event generator and when feedback is provided to each of the summating psi sources; and

(3) a prediction deriving from Stanford's (1977) suggestion that PK may be increased when the subject's "egocentric effort" in the psi task decreases. In the present experiments, the human helpers were told that positive reults had already occurred when fish were tested alone (and thus expected positive results in these experiments). It would appear likely that the subjects believed they were contributing to an effect which was already occurring and thus not feel solely responsible for the production of the effect. This setting would seem to approximate the one which Batcheldor (1966), Brookes-Smith (1973; Brookes-Smith and Hunt, 1970), and Stanford (1974, 1977) suggest as an optimal one for the manifestation of strong PK effects. The shared responsibility involved in a team effort would seem to be accompanied by reduced striving, which Braud (1977) suggested might be conducive to psi success.

The "person-animal team" effect would appear to involve a motivational component contributed by a human-animal "empathy" factor; such a factor may have occurred only in a very weak form (if at all) in the present experiments, due to the use of "cold and scaly" fish rather than "warm and furry" mammals as test animals.

The particular setting employed may not, after all, be the most appropriate one for a test of predictions (2) and (3) mentioned above.

The possibility continues to exist that the experimenter is the true locus of the PK influences in these experiments and that the

consistent two percent PK level may be attributable to the constancy of this particular psi-mediated experimenter effect.

SUMMARY

Prior experiments have yielded a small but consistent (two percent) PK effect in a design in which aggressive fish received mirror-presentation feedback for "hits" on an electronic random event generator. This paper reports three new experiments in which the same design was used, but "person-fish teams" (rather than fish alone) attempted to influence the random generator to produce an increased incidence of mirror presentations. In Experiments 1 and 2, each of ten person-fish teams was tested for 100 trials, during which each "hit" resulted in a 20-second mirror presentation and, hence, an opportunity for the fish to display aggressively (a positive reinforcer for these fish). In Experiment 3, each of ten person-fish teams was tested for 200 trials in which each hit resulted in a 10-second mirror presentation. Significant PK-hitting occurred in Experiment 1 and in the combined series, but not in Experiments 2 and 3 considered separately. The scoring level for the teams (in the present experiments) approximated that for fish tested alone (in prior experiments).

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This experiment was conceived and designed by W.B. The subjects in Experiments 1 and 2 were recruited and tested by W.B.; those in Experiment 3 were recruited and tested by J.K. Statistical analyses were done by W.B. and J.K. The paper was written by W.B.

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A NOTE ON POSSIBLE INTRAGROUP EFFECTS IN PRECOGNITIVE ESP

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Mihalasky (1970) reported that when mixed groups of subjects can be divided into dominant and non-dominant subgroups, the non-dominant groups tend to obtain lower ESP scores than the dominant groups. Division is based on sex, but may be based on other factors such as race or profession.

Friedman, Schmeidler and Dean (1976) could confirm Mihalasky's results. When the present authors read Friedman's interesting paper, they just had carried out an investigation on possible influences of birth order on ESP ability (Jacobs & Breederveld 1976). Here, 300 subjects each guessed to 30 random numbers in the range from 1 - 6. At the time of guessing, the target sets already had been prepared, but had not yet been assigned to the subjects. This was done by Jacobs when all 300 subjects completed their guesses.

On reading the Friedman paper, Breederveld realized that a substantial part of the birth order subjects had been tested in the classroom and that most of the classes investigated had sex minority groups. Moreover, the conditions of the birth order experiment were very similar to those of the experiments of Friedman c.s.. In view of this, it seemed interesting to the present authors to see if they could could confirm the results of the Mihalasky and Friedman studies.

To this end, from the classes tested, those were selected that had sex minority groups (male or female) of 30% or less. In these classes 52 subjects belonging to the majority groups obtained 270 hits (MCE=52x5=260) and 19 subjects belonging to the minority groups obtained 98 hits (MCE=95). On comparing the mean scoring of the majority groups, 5.19, with the mean scoring of the minority groups, 5.15, it may be concluded that the difference is in the

direction predicted by Mihalasky and Friedman c.s., but only to such a slight extent that the hypothesis to be tested has not been confirmed by this investigation.

It is the opinion of the present authors that one has to be very careful in interpreting effects found by Mihalasky and Friedman et al. as being related to characteristics of the subjects investigated. There are indications that these effects can be better described as experimenter expectancy effects: when the investigators try to replicate their results, the effects have a tendency to die out. Other investigators almost always fail to reproduce those kinds of effects.

The decline in the experimenters influence may be due to growing boredom or lack of motivation. Recently, Kennedy and Taddonio expressed a similar point of view (Kennedy & Taddonio 1976).

Concerning the Friedman work it may be argued that the intragroup effect was not among Friedmans hypotheses, which implies that it could not be attributed to his expectancy with regard to it. But in our theoretical considerations Schmeidler was the crucial person, and she expected the effect. If not, she did not search for it, Schmeidler's post-hoc expectancy might have influenced the data in a similar way as Helmut Schmidt's subjects influenced pre-recorded targets.

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EXPERIMENTER PSI-EFFECTS: II. A POSSIBLE 'EXPERIMENTER MINDPRINT'

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In a variety of experiments, to be discussed below, the present experimenter has observed numerous significances on -1 displacement scoring, and none on +1 displacement. It will be suggested that this may be due to the operation of experimenter psi, and the results of a direct test of this hypothesis will be reported. However, it is in order first to present a resumé of the experiments in which the -1 effects have been noted.

First, two experiments were carried out to study the effects of sleep deprivation on clairvoyant psi (Sargent, 1978a). Each experiment lasted 36 hours and subjects completed one ESP test every 30 minutes throughout this period. The test involved a standard, 50-trial run clairvoyance task, targets being randomised digits in the range 1 through 5 inclusive. Subjects checked end-of-run scores after each run for direct hit scoring only. Post-hoc, it was noted that two of the three subjects in the first experiment had showed -1 psi-missing in the second half of the experiment ($P < .01$ in each case). The experimenter was the first observer of the -1 scores. In a replicative study with four subjects, two subjects showed the predicted second-half of experiment psi-missing on -1 displacement ($P < .03$ in each case, $< .01$ overall). Some secondary findings concerning psi-missing focussing at the end of the run and possible diurnal periodicities in scoring are not of direct concern to us at the present time.

One peculiar feature of the replication experiment was that a third subject showed strong ($P < .01$) -1 psi-missing in the first half of the experiment. At first, I was inclined to believe that this was simply artefactual upon the number of subjects and scoring schemes involved in the two experiments, but at the Conference at which these results were first presented, Charles Honorton commented

to me that he doubted that this was the true explanation.

In an unpublished study, sleep deprivation was employed to influence scoring in a precognitive task of some complexity (a revision of the Fisk-Mitchell (1953) clock-card test). Here, four experimental sessions of 12 hours each were employed, in order: control 1/sleep deprived 1/sleep-deprived 2/control 2. A single subject was tested. This was the experimenter, for practical reasons. Since the study is unpublished, full details will not be given here, but basically significantly higher scoring (P=.011) was observed under control conditions than under sleep-deprived conditions for -1 displacement scoring.

In a completely different type of experiment, strong -1 hitting was observed. The experiment used a single talented subject, and the clairvoyance task was the rating of the sex and age of persons shown on 12 concealed pictures (Sargent, 1977c). In informal pre-testing, the subject has shown some displacement tendencies, and so it was decided in advance of the experiment to score -1 and +1 displacements as well as direct hit score. The subject showed no extra-chance scoring on direct hitting or +1 displacement, but scored 7 hits on 11 trials on -1 displacement for age and sex (P=.25,exp.hits=2.75,obs.hits=7, exact binomial P=.007). The subject received end-of-run feedback, and the experimenter was the first observer of the scores.

Finally, significant -1 effects were noted in an experiment reported in this journal (Sargent, 1978b). In that experiment, 10 subjects each completed 400 trials in 4 runs for a precognition experiment in which the targets were randomised digits in the range 1 through 5. Each of these 40 runs, unknown to subjects, was scored against 10 separate target sequences and, later, a computer random-ordering program was used to separate the target sequences into expected-high and expected-low scoring targets. It was predicted that subjects would show higher scoring on predicted-high than on predicted-low targets, and this prediction was upheld for -1 scoring only (P=.01).

Two points might be noted. First, P-values noted have not been corrected for multiplicity of comparisons when this is appropriate. However, all the significant values cited remain significant at the .05 level of significance or better when such a correction is applied to data. Second, in all cases the experimenter was the first observer of the -1 scores, which may or may not be important (Hartwell, 1977).

A summary of the results reviewed is presented in Table 1.

TABLE 1

Experiments yielding significant -1 effects

Reference	Nature of experiment	Form of significance	Predicted?	P-value	Experimenter as subject
1978a	Sleep deprivation/ clairvoyance	Overall missing in second half of 36-hour experiment	No	<.01	Yes
			No	<.01	No
			Yes	.029	No
			Yes	.028	No
			No	<.01	No
1977c	Concealed pictures/ clairvoyance task	Overall hitting	No	.007	No
Unpublished	Sleep deprivation/ precognition	Differential between conditions	No	.011	Yes
1978b	Expectancy effects, precognition	Differential between conditions	Yes	.01	No

Overall, then, the present experimenter has observed eight sets of significant -1 effects, but no significant +1 effects (exact binomial P=.008, two-tailed). At this point we may accept that this may be just a statistical oddity. Indeed, this was my attitude to these -1 effects at first.

However, one aspect of the -1 effect is of interest. They have only been observed in the absence of direct-hit significance of even the grossest post-hoc variety. After Honorton had commented on the aberrant subject from the second sleep deprivation/ clairvoyance experiment, this plethora of -1 effects in the absence of direct-hit significance became more puzzling to me until I considered Braud's postulated 'spreading-thin effect' (Braud, 1978; Braud and Kirk, 1977; Wood, Kirk, and Braud, 1977).Braud basically suggests that for a given individual for a given time epoch there is only a limited amount which can be achieved by psi. In turn, this embryonic hypothesis suggested to me that the -1 effects, possibly, represented the operation of a quantity-limited experimenter psi effect. The -1 effects would appear to be more likely to be an experimenter psi effect than, say, be attributable to some psychological element in the subject-experimenter relationship for several reasons. First, several different types of task were involved in the experiments noted. Second, the experimenter was always the first observer of the -1 scores. Third, in the first sleep-deprivation/clairvoyance experiment, in which the experimenter acting as subject showed significant -1 psi-missing, his -1 scoring also showed low variance, which may be another of his 'mindprints' (Sargent, 1978c).

However, the experimenter psi hypothesis would clearly be only a wild speculation if no test of it were possible. Fortunately, the hypothesis that the -1 effects noted are a quantal experimenter psi effect is capable of yielding predictions which may be tested postdictively. Predictions which may be drawn from it are (1) significant -1 effects should not be noted when direct-hit significance is present in data, and (2) significant -1 effects should be noted when direct-hit significance is absent from data (or, at least, they should arise significantly more often than chance would predict).

The first of these predictions is not a sound one to test since, in essence, a negative is being predicted and proving negatives is not generally regarded as a respectable scientific enterprise. However, the experimenter conducted 22 analyses of -1 effects in several data-sets which had yielded direct-hit significance (including data reported in my 1977b, 1977d and 1978d papers). No significant -1 effects were noted, in the form of overall significance or significant differentials. To some extent, then, the first prediction is verified.

To test the second prediction-that -1 effects will be observed in the absence of direct-hit significances-what was required was a replicated null data-set in which trial-by-trial feedback had not been employed. The experimenter possessed just one such data-set, which has been reported in this journal (Sargent, 1977a). In the experiment in question, the effects of amphetamine and diazepam on precognitive psi were examined, with the experimenter as a solo subject for reasons of time expenditure and safety. As hoped, Control and Placebo sessions yielded quite null direct-hit scoring in this experiment. Amphetamine produced a predicted decline in scoring (P=.014) and diazepam a predicted, but insignificant, incline (P=.09). The hypothesis that -1 effects are quantal experimenter-psi effects predicts here that the Control/Placebo data should yield -1 significance. In order to test this prediction, the data from the first of two Control and Placebo sessions were post-hoc 'scrounged' to yield a result which, hopefully, could be replicated in the second Control and Placebo data-sets.

One significant result emerged from the first post-hoc analysis. In the first half of the Placebo session, significant -1 psi-hitting was observed (expected hits=274.4, obs. hits=304, deviation=+29.6; t=2.44, df=27, P=.022, two-tailed). This was the only significant result to emerge. Hence, data from the second Control and Placebo sessions were analysed to see if this effect could be replicated. In the first half of the Placebo II data, strong psi-hitting on -1 displacement was observed (observed hits=314, deviation= +39.6; t=2.83, df= 27, P=.0043, one-tailed). This was, again, the only significant result to be noted. No significant +1 effects of any sort were observed.

So, in total the experimenter has now observed 10 significant -1 effects, 4 of which were predicted, and no significant +1 effects at all (exact binomial P=.002, two-tailed). Evidence has been offered to suggest that this represents an experimenter psi effect. The discovery of a covert -1 effect in the Placebo data as reported above suggests quite strongly that experimenter psi is at work. Before considering the implications of this finding, it might be advisable to reassure those who might consider that the experimenter is simply a very bad checker; in the experiments noted, eight checkers (five right- and three left- handers) have been involved in checking data.

First, the specific implications for my own work of the discovery of this possible experimenter 'mindprint' (Eisenbud, 1963) might be noted. First, the conclusions drawn from the sleep-deprivation work (Sargent, 1978a) must be revised and independent replication must be a priority. I hope that this will happen, since a secondary feature

of the data (diurnal periodicities in scoring) are of considerable interest, somewhat underresearched, and yet there is growing evidence (Harley, 1978; Schmitt and Stanford, 1978) that periodicities in scoring on psi tests are of importance.

It will also be of interest to see if the present experimenter obtains any more significant -1 effects now that he is aware of the likelihood of experimenter psi being operative in such results. If -1 effects are this experimenter's 'mindprint', he is now effectively a subject who knows the experimental hypothesis.

The more general implications of the finding reported here are of considerable importance. It is becoming increasingly clear that successful experimenters are successful subjects in experiments themselves, and also that idiosyncratic features in their own psi test performance seem to have some relation to the results they obtain from subjects; evidence on the latter point is weak, however, and this paper details perhaps the clearest example of this phenomenon. Much work needs to be done on experimenter psi effects. How much of the variance of experimental results is attributable to experimenter psi? Is it really nearly 100%, as Millar (personal communication) suggests, or much less if a need-related experimental manipulation is employed, as Stanford suggests? Most likely, subject variables and task variables influence the operation of experimenter psi considerably.

Suggestions for research into the problem of experimenter psi could form a considerable and lengthy paper, and it is not my intention to make detailed suggestions for such research here. One program of research which might be mentioned, however, in view of the success of the internal attention states research in parapsychology (Honorton, 1977, 1978) is the study of the effects of varying the states of subjects, experimenters and investigators (Barber, 1976) separately as well as varying feedback contingencies to participants separately. The personal opinion of the experimenter is that the psychology of subject-experimenter interactions is a primary target for research rather than experiments which manipulate feedback contingencies to experimenters, since the latter have yielded results which are, frankly, contradictory (e.g., Bierman, 1978; Broughton, 1977). However, irrespective of how one chooses to tackle the problem, it is clear that experimenter psi effects are a major subject for research. To end on a provocative note: many persons have speculated about 'psychic support figures' (Padfield, 1976), but as yet the possibility that experimenters use subjects as psychic support figures has not been clearly spelled out. Combining this thought with the possible links between psi phenomena and magic (partly covered by Reichbart, 1978) suggests some provoking and unconventional research possibilities.

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ON THE EXPERIMENT OF BRUGMANS, HEYMANS, AND WEINBERG

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Beginning in May 1920 an ESP experiment was carried out by Brugmans, Heymans and Weinberg of the University of Groningen, the Netherlands, with a gifted subject, a student named van Dam. This experiment, of which some early results were reported by Brugmans at the first and second international conference on parapsychology in Copenhagen and Warsaw,raised much interest both because of the exceptional performance of the subject and because of the many interesting features of the study. Of special historical interest is the fact that psychophysiological methods were applied to study the relationship between the 'state' of the subject and his ESP performance, which makes this experiment probably the first in parapsychology in which psychophysiological variables were studied.

The reports of the experiment that have previously been available in French and English (for example, Brugmans (1922, 1924), Carington (1946), Hansel(1966), Murphy(1927,1961), Pope(1952), Soal and Bateman (1954)) are terse and fragmentary. The results which Brugmans (1922) reported were based only on the data of the first seven sessions comprising some 187 trials held between May 1920 and September 1920. In fact, the experiment lasted till June 1922 and ultimately consisted of 24 sessions and 589 trials. Apparently the experimenters never felt it worthwhile to report the results of the additional sessions, thinking that by the end of the experiment van Dam had lost his ability. As we will show, even at the end he was doing quite well by contemporary standards.

The experiment was in several respects unusually sophisticated for its time, and for a number of years afterward was widely regarded as one of the more substantial bodies of evidence supporting the existence of psi phenomena. In recent decades, however, its reputation seems to have slipped considerably. In part this no doubt reflects the normal erosion through neglect that comes with passage of time. The main factor, however, is certainly a series of seemingly damaging critical attacks on the experimental procedures.

The most serious such attacks involve two categories of possible problems: First, it has been suggested that the target orders were so structured as to render statistical analysis of the direct hit results invalid (for example, Rhine, 1977, p. 28; to our knowledge, no supporting evidence for this claim has ever been published). Second, the suggestion has been made by Soal and Bateman (1954)--and eagerly accepted by Hansel (1966)--that van Dam's entire performance might be the result of sensory leakage.

We have been fortunate enough to gain access to the original targets and response series of the entire experiment. Also available, in Dutch, is a series of reports in the "Meededeelingen der Studievereeniging voor Psychical Research" (Proceedings of the Dutch Society for Psychical Research). Taking advantage of this opportunity, our aims in this report are several:

1) to provide a relatively complete English-language account of the experiment and its main results as regards targets, responses and scoring;

2) to redo the main analyses using modern methods, and to extend these analyses to the previously unreported data;

3) to address specifically the critical questions raised above concerning target orders and sensory leakage; and

4) to report a variety of novel analyses of the complete data, including both new analyses directed to old questions and analyses for previously unreported effects.

In a following publication we hope to be able to present more data on the results of some variables studied in these experiments, especially those concerning the psychophysiological measurement of the 'passieve staat' of the subject and those concerning the employment of drugs like alcohol and bromium.

Description of the experiment

In the summer of 1919 the experimenters, Prof. G. Heymans, Dr. H.J.F.W. Brugmans, and A.A. Weinberg, came in touch with a student in mathematics and physics, A.S. van Dam, 23 years old, who felt that he was telepathically gifted and who offered himself as a subject for investigations. Heymans was at that time director of the first Dutch Laboratory for Experimental Psychology, Brugmans one of his staffmembers, later to become Heyman's successor, and Weinberg the conservator of the laboratory.

The first pilot studies with van Dam were carried out by Weinberg and van Loon. Van Loon was probably another staffmember who shortly afterwards left for the Indies and therefore could not participate in the main experiment. The results of these pilot studies which involved in addition to van Dam two other subjects, were reported

in the Journal of the Society for Psychical Research (January & February 1921). In these experiments various sensory modalities were employed as target objects. It was tried to transfer telepathically colours, tastes, feelings and moods, because the experimenters felt that "very probably - not all the contents of consciousness could be transferred with equal ease, considering the fact that the various processes of consciousness make a very different impression on the human mind". Furthermore they were interested whether one kind of mental process especially would be transmitted more easily than others, i.e. impressions of an emotional character.

From the results of these experiments the authors concluded that "our experiments undeniably demonstrate that extra-sensorial transmission of the contents of consciousness is possible" (J.SPR February 1921, p.44). Another conclusion was, that emotional processes of consciousness are more easily transmitted than others, and that the impressions received by the best developed organs of sense were very likely transmitted most easily. The latter conclusion has undoubtedly played an important role in the decision to employ the transfer of motoric impressions in the main experiment with van Dam.

Unfortunately, because of the design these pilot studies do not lend themselves for statistical analyses. Another objection is that all sessions were held with agent, subject and observer being in the same room. Apparently Heymans and Brugmans were therefore not so convinced of the validity of the results as were Weinberg and van Loon. For the main experiment they took extensive precautions to avoid sensory leakages, like blindfolding the subject, putting agent and subject in different rooms,etc. Secondly Heymans et.al. stated in his report to the Dutch SPR (1921) that pilot studies with van Dam were carried out, involving the transfer of taste, colour and forms, but that the results although being positive could not be considered as striking. It should be noted, however, that Heymans and Brugmans had clearly a different opinion about what constituted a 'striking result' than we have nowadays. They seem to have applied a rather subjective criterion which required a much higher scoring rate than would presently be required by criteria based on statistical significance.

van Dam showed a strong initial preference for what was termed in those days "Rubini" experiments. In such an experiment (directly modeled after stage performances by an individual of that name) an object is hidden and the subject, moving around freely, tries to locate it while someone knowing the place of the object remains in the vicinity acting as agent. The experimenters recognized

clearly that all such procedures, including those involving no direct contact between agent and subject, were defective because of various possibilities of sensory leakage. They therefore set out to design a procedure which would be free of these defects, while retaining maximal resemblance to the "Rubini-type" situation, which van Dam enjoyed. (Rubini-type trials were still employed during later experimental sessions, but only as a kind of warm-up for the formal testing to be described; the results of these trials were apparently extremely successful, but they were strictly segregated from the results of the formal trials). An interesting point arose when the experimenters discovered that it was very difficult for van Dam to visualize. Instead he relied heavily on motoric imagery. Thus for instance forms were represented internally as movements. Hence the experimenters decided to design their experiment in such a way that van Dam would be able to apply this ability in a rather direct and congenial way. The central idea of the experiment was that agent-experimenters would try to transfer "telepathically" motoric impressions to van Dam. It is important to note that the experiment was thus not designed as a guessing task. Rather, the object was to transfer impressions of movements.

In order to be able to evaluate the results of the attempted transfer of these impressions, a sort of checkerboard was constructed with 48 squares. This board measured 40x30 cm., each square being 5x5 cm. The squares were indicated by giving the eight columns the letters A to H (from left to right) and the six rows the numbers 1 to 6 (from bottom to top). The letters and numbers were printed on the board. Each square of the bottom row contained the letter of the corresponding column, and on each square of the other five rows the number of the row was printed. According to some photographs printed in the "Meededeelingen" the experimenters had a clear view of the letters and numbers from their position in the other room (see also Murphy, 1961). We do not know whether it was possible for van Dam to distinguish the boundaries between the squares, when moving his hand over the board.

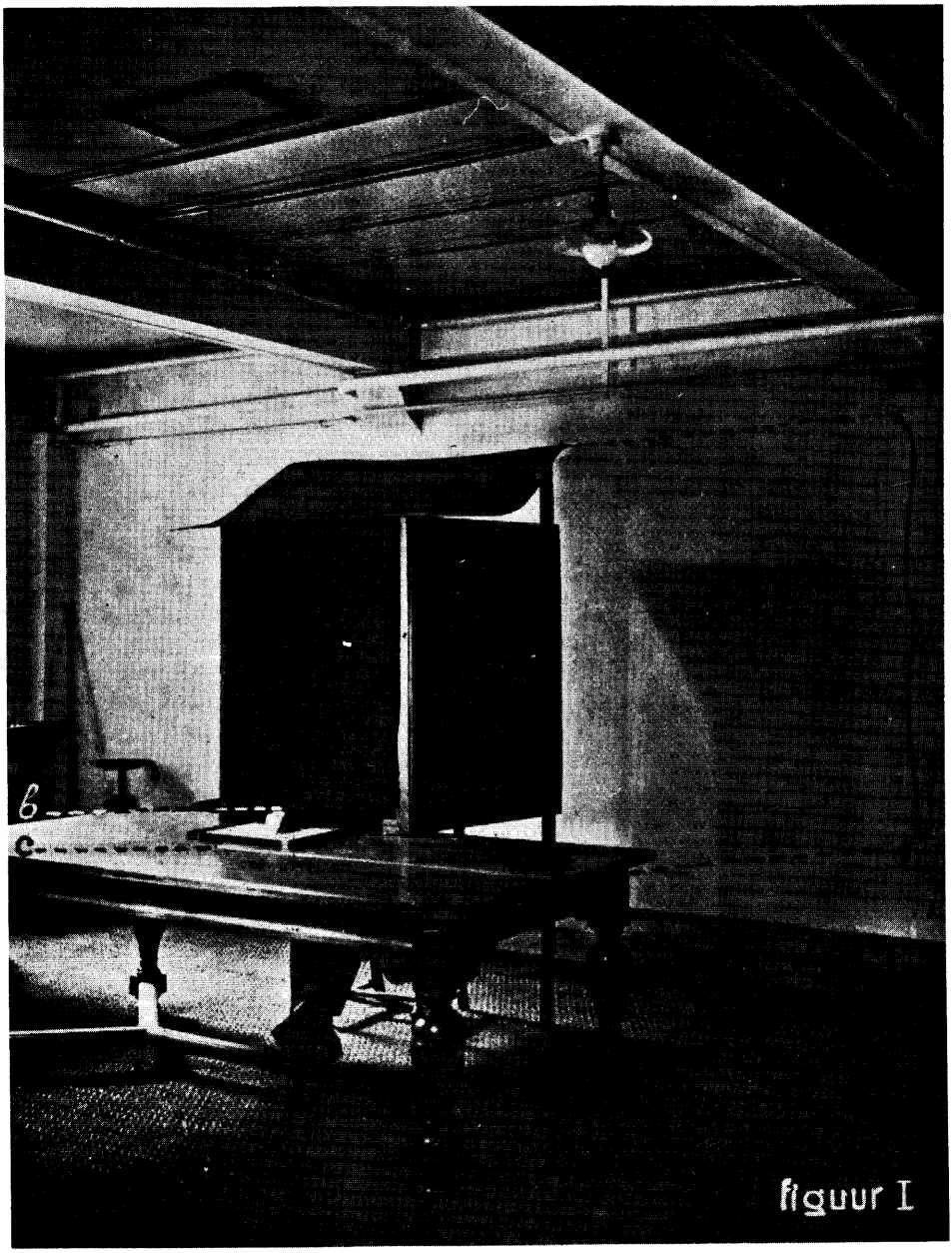
This checkerboard was chosen because it would enable the experimenters to fix a certain point as the target area for the motoric movement, and because they would be able this way to fix the probability of a direct hit. The task of the agent was to 'steer' the hand of the subject to the target area (in a Rubini experiment the location of the hidden object), by transferring the impression of the movement of the hand from the present position to the target area. If the subject felt that he had reached the target area, he indicated this by tapping his finger twice on that

area. It was stressed, however, that the subject should only give the agreed-upon signal when he felt that he had reached the target area. In other words, the subject was in fact required not to guess, but to wait till he received the impression that his finger had reached the target square.

So actually the experiment was more complicated in its psi aspects than it looks superficially. In fact each trial consisted of a varying number of "subtrials" because every time the subject shifted the position of his hand, the agent started a new subtrial by trying to transfer the movement from the new position of the hand to the target area. Secondly, after successfully transferring these movements the agent also had to transfer the impression that the target area had finally been reached and that the subject should stop moving his hand. From the report in the "Meededeelingen" it can be concluded that van Dam clearly experienced the latter as a separate task. He had to receive a distinct impression that his task for that trial was finished.

Hence a "hit" can be thought of as terminating an unknown number of subtrials, in which momentary motoric impressions were to some extent successfully transferred, and in which at a given moment the impression was also successfully transferred that the task was completed and that the subject should give the signal. A trial was rated as a miss, if the subject ended the trial by giving the signal when pointing to a square different from the chosen target square. But even in these trials an unknown amount of successful transfer of motoric movements might have occurred. In fact, the authors state in their report that occasionally it happened that in a given trial the hand was repeatedly 'steered' to the target square, but that the agent failed each time in transferring the impression that the target area had been reached. We discuss this so extensively here because we feel that it is important to realize that while the statistical analysis presented is ultimately based only on whether the target area was correctly identified or not, this aspect-the feeling of van Dam that he had reached the target square-constituted only part of the experiment.

The main psi aspect, the transfer of the motoric impressions, can not be analysed because we lack detailed information both about van Dam's hand movements and about the specific impressions which agents tried to convey. Because of the fact that the main part of the experiment lies in the 'steering' of the hand, however, it might be expected that the behavior of the subject as far as the 'selection' of the response squares is concerned might have been rather different had he been involved in a pure guessing task. Some statistical evidence consistent with this expectation


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is presented below.

The experimenters took a number of special precautions to exclude possibilities of sensory leakage. Two rooms were used for the experiment. In one of the rooms in a sort of half-basement of the Psychological Institute a cabinet was erected in which van Dam was seated. Three sides of this cabinet were closed, the sides left and right of the subject and the top. The side in front of the subject was covered with a heavy curtain in such a way that he could pass his hand under the curtain and move it on the surface of a table in front of the cabinet on which the checkerboard was put. This cabinet was applied to ensure that he could not see the ceiling of the room or the checkerboard on which his hand moved. As a further precaution, and also to help him more easily reach a relaxed state, van Dam remained blindfolded during the session.

In the ceiling of this room, which judging from the photographs was rather high, an opening was made of 32x52 cm. The separating floor between the two rooms, the upper one located on the ground floor of the building, was, according to Carington (1938) who inspected the rooms personally, of 'a most uncompromising solidity'. The floor of the upper room consisted of planks of about 2,5 cm. thickness, laid on about 25 cm. thick joists which were spaced at about 35 cm. The ceiling of the basement room consisted of lath and plaster fixed to the joists. The opening mentioned above was sealed off at both sides with glass windows. The distance between the glass windows must have been approximately equal to the thickness of the joists, that is about 25 cm. Although according to the photographs in the "Meededeelingen" the floor of the upper room was normally not covered with rugs, a thick mat was used on which the agent stand during the trials.

The sound isolation between the two rooms was of such quality that according to the report in the "Meededeelingen" the experimenters could not make themselves understandable from one room to the other even when shouting at the top of their voices. Hence when the experimenters stayed in the upper room, the onset of a trial was indicated to van Dam by striking the floor with a hammer. During the sessions the upper room where the experimenters stayed was kept dark by closing the curtains and shutting off all lights.

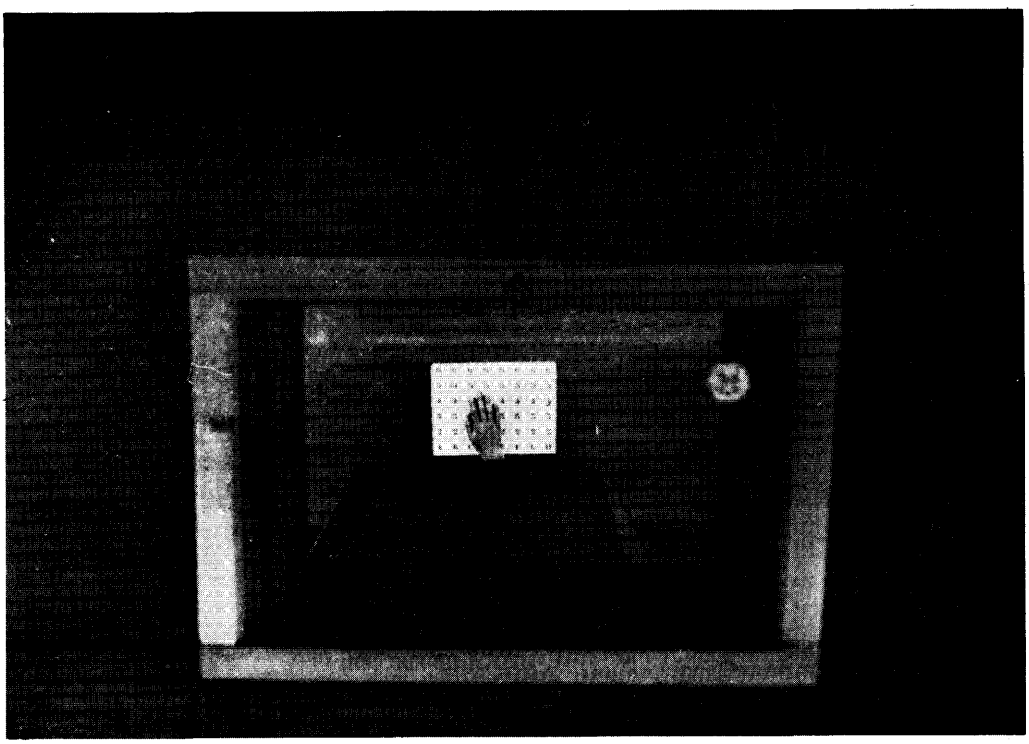
In each trial one of the experimenters acted as the agent, while the others functioned as observers. They changed roles within a session in a systematic way. According to the report, the agent selected the target square and the observers were not explicitly informed about the target until after completion of the trial.

The agent selected the target square by shuffling separately two stacks of cards, one containing the letters A to H, the others the numbers 1 to 6. Each stack contained one copy of each card.* After shuffling, the agent mentally noted the target and placed the two stacks upside down on the table. The bottom cards facing the surface of the table represented the letter and number of the target square. After completion of the trial one of the observers picked up the stacks and recorded the letter and number. It is not stated clearly when this recording took place, but it can be assumed in view of the whole procedure that it happened after the response of the subject was recorded by the observers. On the other hand, it seems possible that the observers might get some idea of the location of the target area during the trial, based on the behavior of the agent as he attempted to influence the movement of the hand of the subject. The reports do not discuss this possibility, but in the "Meededeelingen" it is stated explicitly that the observers only learned the identity of the target after the trial, when it was recorded.

In the event the subject tapped in between two squares, both possible response squares were recorded whether the ambiguity was in letter, in number, or both. The experimenters invariably rated trials with an ambiguous response as a miss, including those trials in which one of the recorded response squares was identical with the target square. On all but 17 trials the response time was also recorded. Although this is not specifically stated in the report, we can assume that at least in the distance condition, where all experimenters stayed in the upper room, the subject was not informed after each trial about the details of that trial. It is not likely that one of the experimenters went down the staircase and back again only to tell van Dam how well he succeeded or what the target was. It is more likely that van Dam was informed about the results after the session was completed. At least as far as the targets are concerned this assumption is backed up by the statistical analyses, presented below.

It is also not clearly stated which hand was used by the subject when making the movements. In the "Meededeelingen" three pictures are shown of the cabinet with a person sitting in it. In all three pictures a right arm appears from under the curtain. Assuming that these pictures were taken sometime during the sessions, the subject can be supposed to have used his right hand.

* In some previous reports (for example Hansel (1966)), the procedure is incorrectly described as one in which slips of paper bearing the letters and numbers were drawn from bags.



In total 589 trials were run, in 24 sessions of unequal length. (For two trials near the end of the experiment no response was recorded; these trials were excluded from the analyses reported below). The experimenters allocated trials in varyingly systematic ways to conditions whose effects on the performance they wished to study. These include in particular experimenters, distance, and certain physiological factors. The three principal experimenters were Brugmans, Heymans, and Weinberg, although a fourth, currently unidentified experimenter contributed eight trials near the end of the experiment. Distance was studied at two levels, a distant condition (233 trials) in which all experimenters remained in the upper room watching van Dam through the porthole in the ceiling, and a near condition in which the agent took a position about 1 meter away from the checkerboard in front of the subject. It is not clearly stated where the observers stayed in these trials, but we assume that they stayed in the upper room. The experimenters were aware that in the near condition the possibility of some sensory cueing could not be excluded. Although the agent tried to control himself as much as possible, they state that: "in the near condition tactile and visual impressions were completely excluded, but auditory cues, e.g. because of movements and respiration, could of course not be considered to be entirely excluded" (Meededeelingen, p.6). Physiological aspects of van Dam's performance were studied in two main ways; by including some sessions in which he was given either alcohol or bromide in advance, and by attempting in some sessions to quantify his trial-by-trial physiological state using measures of galvanic skin response, pulse, and respiration. As stated before we hope to discuss these aspects in a subsequent publication on this experiment.

RESULTS

Analysis of the Target Sequence

As mentioned above, questions have been raised about the adequacy of the target sequences obtained. In this and succeeding sections we give detailed information sufficient to answer these questions. For the analysis of the target and response series the whole sequence of trials is taken as one unit. We assume that possible non-random effects in the target series are most likely due to inadequate shuffling of the decks of cards. Since the decks must have been specially constructed for this experiment, they probably were left undisturbed between the sessions.

Zero-order target effects

The first question is whether the possible targets were selected with equal frequency apart from expected statistical fluctuations. The overall frequencies of the target letters, numbers, and individual squares are presented in Table 1.

TABLE 1

Frequency distributions for target letters, numbers, and individual squares

		Number						Letter totals
		1	2	3	4	5	6	
Letter	A	15	20	21	13	25	9	103
	B	18	16	20	19	13	16	102
	C	12	13	16	12	17	4	74
	D	6	11	9	7	5	5	43
	E	9	13	17	6	18	10	73
	F	11	13	13	10	13	11	71
	G	12	22	10	12	31	11	98
	H	2	6	4	3	4	4	23
Number totals		85	114	110	82	126	70	587

It is evident from these data that the shuffling procedure was not adequate to ensure equal sampling probabilities for letters, numbers, or squares.

For letters, $\chi^2 = 78.6$, df = 7, $p << .001$. For numbers $\chi^2 = 24.5$, df = 5, $p < .001$. There is systematic over-representation of the letters A, B and G and the numbers 2, 3, and 5; and systematic avoidance of the letters D and H and the numbers 1, 4, and 6. Not surprisingly, the frequency distribution for individual squares is correspondingly non-uniform, with $\chi^2 = 145$, df = 47, $p << .001$.

An important question is whether the processes of selection for numbers and letters were statistically as well as procedurally independent. This is easily tested by computing the chisquare for cell frequencies, given the marginal frequencies. The resulting value is 32.1 with 35 degrees of freedom, which is entirely non-significant. This result strongly supports the experimenters' statement that the target squares were chosen by independent selection of their letter and number attributes.

The pattern of non-uniformity in zero-order frequencies for letters and numbers proved to be similar for near and distant conditions (Table 2) as well as for ambiguous and non-ambiguous trials (letters, $\chi^2 = 8.8$, df = 7; numbers, $\chi^2 = 4.4$, df = 5).

TABLE 2

Target letters and numbers in near and distant conditions

Letters:	A	B	C	D	E	F	G	H
near	62	61	41	29	40	46	62	13
distant	41	41	33	14	33	25	36	10

$\chi^2 = 3.7$ df = 7

Numbers:	1	2	3	4	5	6
near	52	70	70	51	68	43
distant	33	44	40	31	58	27

$\chi^2 = 2.9$ df = 5

It was also fairly stable across the duration of the experiment: Dividing the experiment into halves and comparing target frequencies for letters and numbers in the two halves, we find no difference for numbers and only a marginally significant difference for letters (see Table 3).

TABLE 3

Frequency distribution of target letters
First vs. second half of experiment

letter	A	B	C	D	E	F	G	H
first half	50	38	36	21	34	46	52	16
second half	53	64	38	22	39	25	46	7

$\chi^2 = 17.2$, df = 7 , $p \sim .02$

The main changes contributing to this result are the increased frequency of B and the decreased frequency of F in the later part of the experiment. Note too that the overall pattern is if anything more non-uniform in the second half.

So far it appears that the non-uniformities of the target frequencies may be uniquely associated with physical properties of the decks of target cards. However, there is also apparently a contribution from the experimenters, possibly related to their shuffling techniques. The frequencies of target numbers do not differ across the three main experimenters, although there is a trend in this direction ($\chi^2 = 17.8$, df = 10, $.05 < p < .10$); however, for letters there is a strongly significant effect (see Table 4).

TABLE 4

Experimenters vs. target frequencies for letters*

letter	A	B	C	D	E	F	G	H	Totals
Brugmans	26	71	34	8	25	27	29	8	228
Heymans	36	19	26	18	21	19	30	6	175
Weinberg	38	11	13	16	26	25	38	9	176
Totals	100	101	73	42	72	71	97	23	579

* unidentified fourth experimenter omitted

$\chi^2 = 69.5$, df= 14, $p << .001$

The most conspicuous contributors to this result occur in the column corresponding to 'B' targets, which are strongly over-selected by Brugmans, and less strongly under-selected by Heymans and Weinberg. Nonetheless there is still some visually obvious evidence of uniformity, particularly in the low frequency of 'H' targets. Furthermore, a statistical measure of agreement in the ranked frequency distributions across experimenters is still almost significant (Kendall's W = .61 corrected for ties, $.05<p<.10$; Siegel, 1956). So although there is evidence of disparity, particularly in the frequency with which the different experimenters selected letter attributes of target squares, those differences are not too large, and leave considerable evidence of some more basic selection biases originating presumably in the physical properties of the target cards. Fortunately, as we will show in the sequel, the scoring rates are virtually identical across experimenters.

First-order target effects

First-order sequential dependencies were analyzed using a test which is asymptotically independent of the observed zero-order biases (David and Akers, 1974). For letters the analysis yields $\chi^2 = 67.1$ with 49 df, $p \sim .05$, 1-tailed, suggesting weak dependencies among successive letter targets. No such effect was found in the target number sequence ($\chi^2 = 19.5$, df = 25).

A sequential analysis on the complete targets is not possible because of the too low frequencies of most of the cells. Instead a "distance" analysis was carried out between the successive targets. Distance is defined here as the square root of the sum of the squared differences in number of letters and numbers between two squares. For instance, between A1 and C5 the distance is $SQRT(2^2+4^2)$. In total there are 31 possible different distances between squares. Such an analysis can detect different kinds of sequential dependencies which are not related to individual targets-- for instance, a tendency to select the next target relatively far away from the preceding one. The expected distribution is obtained by enumeration of all the possible distances between all the targets. (The actual target set was applied, because it had already been shown that the targets were not selected with uniform probability). Comparing the expected distribution of distances with the distribution based on the actual target sequence (see figure 1) no difference was observed ($\chi^2 = 23.5$, df = 30). Applying this same analysis to the observed distribution of successive distances for target letters and numbers separately also leads to non-significant results (letters: $\chi^2 = 5.0$, df = 7; numbers: $\chi^2 = 5.5$, df = 5). This implies that the marginally

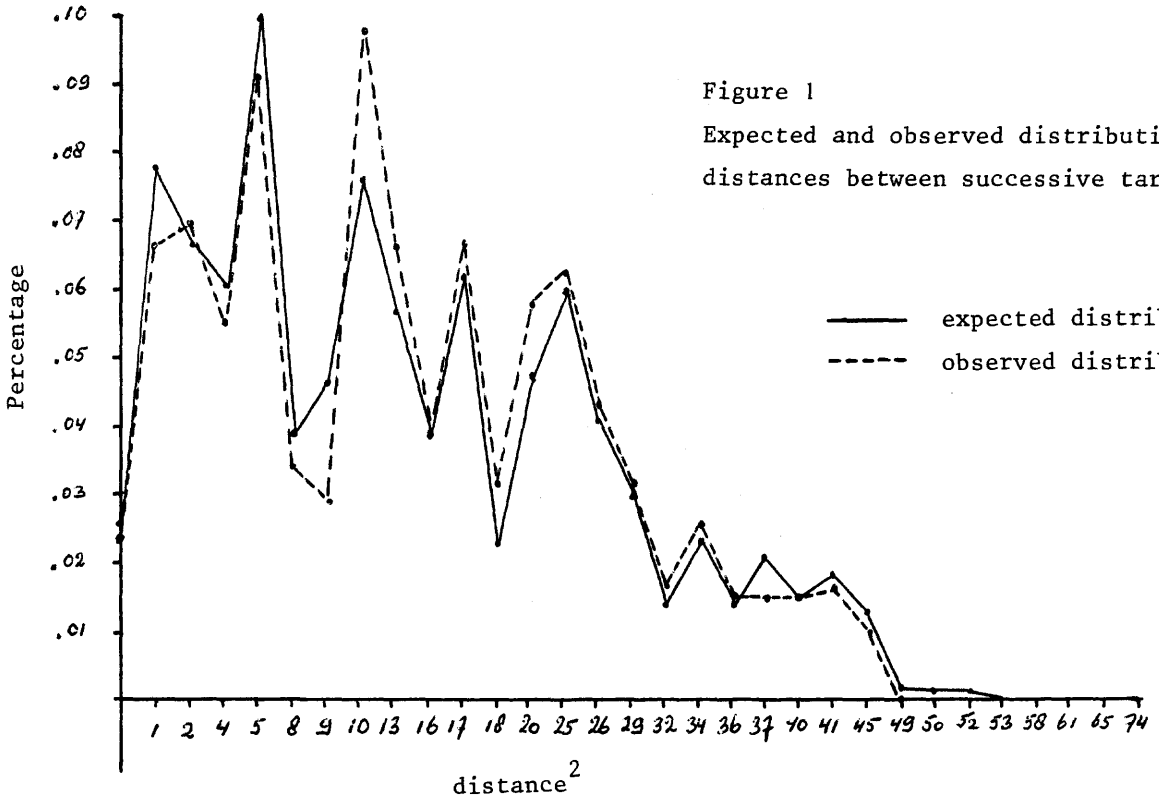


Figure 1

Expected and observed distribution of distances between successive targets

significant sequential dependency in the target letter series cannot be based on a general tendency affecting all letters. On the other hand, a study of the differences between the observed and expected frequencies of the sequential target letter combinations did not reveal any systematic pattern, nor could we find a specific set of combinations which can be held responsible for this marginal significant effect.

Some additional analyses have been carried out to check obvious candidates for specific kinds of non-random sequential effects. For example, dividing the checkerboard into left and right halves, the sequential target combinations were transformed into left-right combinations to see whether there exists a systematic tendency to change the targets from one half of the board to the other. No such effect was found (see Table 5).

TABLE 5

Sequential targets: left-right

	trial N+1:	left	right	
trial N	left	172	149	$\chi^2 = 0.3$ df = 1
	right	149	116	

The same analysis was applied dividing the board into upper and lower halves (see Table 6).

TABLE 6

Sequential targets: up-down

	trial N+1:	up	down	
trial N	up	129	147	$\chi^2 = .03$ df = 1
	down	148	162	

Note: all 1 df chi-squares corrected for continuity

An equally non-significant result was observed when dividing the board into a central part and a side part (see Table 7).

TABLE 7

Sequential targets: central-side

	centre	side	
centre	184	139	$\chi^2 = 0.8$ df = 1
side	139	124	

Finally, in an effort to avoid overlooking any previously unsuspected but strong forms of sequential dependency in the targets, we traced out manually some of the successive transitions between targets, separately for numbers, letters, and squares. This was done for approximately the first 100 trials in each case, carrying us well past the period of van Dam's most dramatic scoring (sessions 2 and 3). No new candidates were discovered in this way.

From all these analyses we conclude that there is little evidence of sequential dependencies among successive targets, except for the very marginal and diffuse effect involving individual target letters. Moreover, no systematic pattern could be found which might explain even this very weak dependency; hence it is difficult to see a priori how the subject could have taken advantage of this effect even had he known about it. Since the targets were generated successively, and since the distance analysis did not yield any significant deviations for numbers, letters, or squares, higher-order effects appear even more unlikely.

Analysis of the Response Sequence

In 131 cases the observers could not discriminate between two adjacent squares when the subject tapped his finger, and wrote down both squares as response. Murphy (1961) appropriately raises the question whether the manner in which response ambiguities are resolved has any impact on the main statistical results. In order to bracket the possibilities we decided to create and analyze in parallel two versions of the data, in each of which the ambiguous responses are uniformly resolved in one of the two possible extreme ways. Thus one dataset was constructed by always choosing for ambiguous responses the more distant response from the target square (the MDR set); while for the other set (the LDR set) the less distant response was uniformly chosen. (For non-ambiguous responses, of course, the datasets are identical). Unless

otherwise stated, all analyses reported below were carried out on the MDR set. As it happens, none of these statistical results differ in more than minor respects between the two sets. Most analyses have been carried out independently by the two authors. Afterwards it turned out that the MDR and LDR set of responses used by us differed slightly due to the fact, that in a few ambiguous trials both responses had an equal distance to the target square, in which case each of us randomly assigned one of these responses to each set. This also created some minor discrepancies between different tables presented below.

Zero-order response effects

The overall frequencies of the response letters, numbers, and squares are presented in Table 8.

TABLE 8

Frequency distributions for response letters, numbers, and individual squares

		Number						Letter totals
		1	2	3	4	5	6	
Letter	A	5	10	12	11	15	3	56
	B	9	10	12	20	9	5	65
	C	9	17	27	14	10	5	82
	D	18	12	21	24	15	3	93
	E	7	7	21	43	14	8	100
	F	8	11	16	24	15	11	85
	G	7	13	20	16	16	4	76
	H	3	3	3	7	11	3	30
Number totals		66	83	132	159	105	42	587

As was the case for the targets, there are strong zero-order response effects at all levels: For letters, χ^2 = 48.6 with 7 df, p<<.001. For numbers, χ^2 = 95.2 with 5 df, p<<.001. For squares, χ^2 = 227 with 47 df, p<<.001. Furthermore--and in contrast to what we saw for targets--the response attributes are not chosen independently; van Dam's choices of letters and numbers are significantly associated (χ^2 = 57.9, df = 35, p<.01). From inspection of Table 8 it is apparent that he had a strong general tendency to terminate the trial on more centrally located squares.

These response patterns are rather stable throughout the experiment. They are consistent across such factors as near vs. distant conditions (letters, χ^2 = 15.3, df = 7, p ~ .05; numbers, χ^2 = 6.8, df = 5), clear vs. ambiguous responses (letters, χ^2 = 10.8, df = 7; numbers, χ^2 = 9.3, df = 5), and experimenters (letters, χ^2 = 14.6, df = 14; numbers, χ^2 = 8.2, df = 10). (For near vs. distant, the letters are chosen with more nearly uniform frequency in the distant condition).

When the frequencies of response letters and numbers are compared for first and second half of the experiment, the number frequencies are again consistent, but there is again significant difference in the frequency distribution of the letters (see Table 9). Since distant trials were more frequent in the first half of the experiment, this effect is correlated with the one just mentioned, and of the same form--i.e., letters are chosen with more uniform frequency in the first half.

TABLE 9

Frequency distribution of response letters
First to second half of experiment

	A	B	C	D	E	F	G	H
first	30	31	37	38	42	50	46	18
second	26	33	45	54	60	33	31	12

χ^2 = 14.6 df = 7 p = .05

We shall say more about this table subsequently in discussing the question whether van Dam's response patterns show any evidence of reflecting the biases of the target sequence.

First-order response effects

No first order dependencies between the successive responses were observed for the response letter combinations ($\chi^2 = 42.7$, df = 49), but a marginally significant first-order sequential dependency was observed for the numbers ($\chi^2 = 41.8$, df = 25, $p < .02$). Note that this is opposite from what we found when analyzing the target series. The sequential dependency for the response numbers is not due to the familiar response bias to avoid repetitions. In fact, both in the number and letter response series the number of observed repetitions slightly exceeds the expected frequencies.

The distance analysis of the response series showed only non-significant differences between the observed distribution of distances of the successive response squares and the expected distribution ($\chi^2 = 22.1$, df = 30). The same applies to the distribution of the distances between the successive response letters ($\chi^2 = 3.8$, df = 7) and to the distribution of the distances of the successive response numbers ($\chi^2 = 9.1$, df = 5).

Analyzing for specific non-random patterns in the response series, no effect was observed when comparing successive responses as regards left and right half of the board ($\chi^2 = 1.31$, df = 1) or upper or lower half of the board ($\chi^2 = 1.96$, df = 1). A slight effect was found when comparing successive responses marked as central or side squares (see Table 10), its form being a weak tendency to choose the next response in the same area (central or side) of the board as the preceding one.

TABLE 10

Sequential responses: central-side

	trial N+1:	centre	side	
trial N	centre	325	104	$\chi^2 = 4.2$ df = 1
	side	105	52	$p \sim .05$

We emphasize that these sequential dependency effects in the response series are unusually slight compared to the sequential dependencies which are typically found when subjects participate in a guessing task.

This confirms what we stated in the description of the experiment, that the experiment was not designed as a guessing task and was probably not experienced as such by the subject. It should also be noted that these effects are lacking in the target series.

Relationships between target and response biases

On the assumption that complete trial-by-trial feedback was provided, van Dam obviously had opportunity to acquire information about properties of the target sequence, whether consciously or unconsciously. Even if only hit/miss feedback or no feedback at all was provided, however, it is still important to inquire whether biases in the response data are in significant respects congruent with those of the target data.

We have addressed this question in a number of ways. First, we generated scatterplots and spearman rank-order correlations comparing zero-order frequencies in target and response data for letters, numbers and squares, and comparing first-order frequencies for letters and for numbers.

As indicated above, there is strong evidence of non-uniformity in overall selection frequencies for numbers, letters, and squares for both targets and responses (Tables 2 and 8). However, despite the conspicuously low frequency in both series of the letter H and the number 6, the overall relationship between target and response biases is for both attributes non-significant (for the letters it is, in fact, an inverse relationship; $r_s = -.275$, NS). Although for squares there is a marginally significant positive correlation ($r_s = .308$, $t = 2.19$, $df = 46$, $p<.05$) it is clear from the plots that this weak relation is dependent on the low frequency in both series of the compounds of H and 6.

The weak evidence of sequential dependency in the target and response series occurred only for letters (but not number) and numbers (but not letters) respectively. Moreover, the relationships between overall first-order frequencies of targets and responses for both letters and numbers are non-significant, although there is a trend for the numbers ($r_s = .305$, $t = 1.89$, $df = 34$, $.05<p<.10$). Because dependencies in the target letter sequence had proved (marginally) significant, however, we carried the analysis for letters a stage further even though there was no overall evidence of sequential dependency for the response letters. Specifically, we compared the signs of the deviations from expectation in target and response series to see whether any indication could be found that the subject's response pattern might have followed the marginally significant tendency for first order dependency between the target letters. In 25 cases the deviations are in the same direction, but in 38 cases in an opposite direction (one case was

dropped because one of the deviations was zero). This difference is not significant and even in an opposite direction from what would be expected if the subject had made use of the possible knowledge about the slight dependencies among successive target letters. Hence we can conclude that this marginally significant effect in the target series had no influence on the subject's response behavior, and that it can be neglected when analyzing the ESP data.

Since there was significant evidence of change (from first half to second half of the experiment) in van Dam's frequency of responding with the various letters (Table 9), we also checked to see whether these changes tended to be congruent with the significant changes that occurred across the two halves of the experiment in the target letter frequencies (Table 3). As can be seen from Table 11, the direction of the changes are the same for 7 of the 8 letters. However, this is nowhere near significant either by a sign test or by the more powerful Wilcoxon matched-pair signed-ranks test (Siegel, 1956). Thus there is no indication that van Dam was adapting his response pattern to match the shifting frequencies of the target letters. Moreover 3 of the 4 largest response shifts occur in directions congruent with his own overall biases, and contrary to those of the target series.

TABLE 11

Shifts in frequency for letters
between first and second half of experiment

	A	B	C	D	E	F	G	H
targets	3	26	2	1	5	-21	-6	-9
responses	-4	1	8	16	18	-17	-15	-6

Finally, still assuming that van Dam was informed after each trial about the target for that trial, we investigated the relationship between target square and the response on the next trial to see whether we could find an indication that his responses were influenced by this possible knowledge of the target for the previous trial. No such relationship could be detected when applying a distance analysis between target and response on the next trial ($\chi^2 = 28.8$ df = 30).

To summarize, there is little evidence of overall congruence either in composition or structure of the sequences of targets and responses, and no evidence that van Dam's changing responses reflect increasing knowledge of properties of the target sequence. Although there are massive distortions in the overall frequencies of occurrence of the targets, there is little indication that van Dam's responses systematically reflect even these very gross distortions, and the inequalities of target and response frequencies are in any event easily handled in the analysis of direct hits through use of Stevens' method (Burdick and Kelly, 1977). In the statistically more critical area of sequential patterns, there is only slight evidence of dependency in each series separately, and less evidence of congruent biases between them. We therefore conclude that a valid statistical analysis of the experiment is entirely possible. In the sequel, this conclusion will receive further confirmation from several properties of van Dam's scoring performance. These include in particular the extremely high scoring rate; the fact that this scoring is widely distributed over the possible targets; and the fact that the scoring tends generally to be worse in places where the evidence of possibly relevant biases is, if anything, stronger--for example in the second half of the experiment, and in the numbers.

Analysis of Direct Hits

Using the MDR response data (more distant responses, see page (263), van Dam scored 118 exact hits in 587 trials, approximately ten times the number expected by chance. This scoring is also widely distributed over targets; every number and letter target is hit substantially in excess of chance expectation, the letters in particular being hit at rates uniformly above 2.5 times expectation. Confusion matrices are presented in Tables 12 and 13. Evaluation of direct hits by Stevens' procedure leads to a z-score of 29.7 (MCE = 12.8, variance = 12.4). For the LDR data (less distance responses, see page (263) the same analysis leads to z = 32.1 (N_{hit} = 127, MCE = 12.9, variance = 12.5).
We note in passing that the exact (Stevens) values for mean and variance differ only slightly from the theoretical binomial values (MCE = 12.23, variance = 11.97), or for that matter from the Poisson (MCE = variance = 12.23). Thus the choice of analysis model has very little impact on the enormous overall significance of the direct-hit results. In particular, the close correspondence between the binomial and Stevens results shows the minimal effect and uncorrelated character of the substantial distortions of the raw frequency distributions for targets and responses. The distance

TABLE 12

Confusion matrix for hits on letters

		Target							
		A	B	C	D	E	F	G	H
Response	A	31	3	2	2	6	5	5	2
	B	10	32	5	0	4	6	5	2
	C	16	12	26	4	5	7	8	4
	D	11	15	13	22	11	7	11	2
	E	14	17	8	5	27	16	10	3
	F	8	8	10	4	14	26	15	0
	G	11	10	6	6	2	2	37	2
	H	2	4	3	0	4	2	7	8

TABLE 13

Confusion matrix for hits on numbers

		Target					
		1	2	3	4	5	6
Response	1	22	7	9	6	12	9
	2	8	33	13	5	18	6
	3	19	24	29	19	25	15
	4	20	26	31	39	27	16
	5	9	11	24	9	40	12
	6	7	12	4	4	4	11

analysis between targets and responses (see also page 260) shows not surprisingly that the observed distribution differs very strongly from the expected distribution, obtained by enumeration of all possible distances between all targets and all responses (χ^2 = 909 df = 30 $p<.01$). As can be expected, the main difference is caused by the excess of zero distances or hits.

The 233 trials in the distant condition by themselves are also highly significant, yielding 45 hits for z = 18.3 (MCE = 4.9, variance = 4.8). Furthermore, comparison of the distant versus near condition shows that there is no significant difference in scoring rate between these conditions, taking the individual squares as targets (Table 14).

TABLE 14

Hits and misses in distant and near conditions

	near	distant	
miss	281	188	χ^2 = .08 df = 1
hit	73	45	

Analogous results appear when analyzing the scoring in near vs. distant conditions for letters and numbers separately (letters: χ^2 = 0.07, df = 1; numbers: χ^2 = 0.2, df = 1). The trials in near and distant conditions also proved to be equally distributed over the three experimenters (χ^2 = 0.4, df = 2); that is, each experimenter acted as agent in both conditions in relatively the same proportion of trials.

Comparing the scoring rates over the three experimenters, Heymans appears to be the most successful agent, and Brugmans the least successful. However, these differences among experimenters are not statistically significant (see Table 15; the unidentified fourth agent, who participated in 8 trials and obtained 1 hit, was omitted from this analysis).

TABLE 15

Scoring across experimenters

	B	H	W	
miss	189	132	141	
				$\chi^2 = 3.4$ df = 2
hit	39	43	35	

Similar results are obtained in analyzing scoring over experimenters separately for letters and numbers, although in the scoring on letters there is a marginally significant difference ($p = .05$) mainly due to a slightly higher scoring rate when Weinberg acted as agent.

Not only is the rate of direct hits very high, but it is much higher than would be expected from chance association of hits on the letter and number attributes (see Table 16). This strongly confirms the conclusions tentatively reached by Foster (1952), based on data from the first seven sessions alone.

TABLE 16

Joint scoring on letters and numbers

	miss number	hit number	
miss letter	322	56	
			$\chi^2 = 109.9$, df = 1, $p << .001$
hit letter	91	118	

From Table 16 we can also derive estimates of van Dam's scoring on the attributes separately, eliminating the direct hits. A simple and conservative way to do this is to analyze number hits only for trials in which letters are missed, and letter hits for trials in which numbers are missed. For letters, this reveals strong positive scoring with N = 413, MCE = 51.6, variance = 45.1, $Z \sim 5.7$. For numbers, however, there is a slight tendency toward missing (N = 378, MCE = 63.0, variance = 52.5, $Z < -1$).

As can be observed from Table 8, the subject showed much stronger zero order response effects on the numbers than on the letters. We also found stronger sequential dependencies in response numbers than in the response letter sequence. This suggests that the guessing aspect may have played a more important role in the up-down (number

attribute) direction than in the left-right direction. In other words, the left-right movements may have been dominant for the subject as compared to the movements in the up-down direction. Assuming that the excess of hits in those trials where no complete hit was achieved was due to the transfer of motoric impressions, this could explain the excess of hits on the letters. That is, it may have been easier for the experimenters to 'steer' the hand in the left-right direction, while in the up-down direction the subject was forced to rely more on guessing.

A further analysis of the scoring on the individual letters and numbers was carried out in which the expected scoring is based on the observed frequencies of targets and responses. That is, given the amount of hits on letters (209) we calculate, based on the relative frequencies of letters in target and response series, how this number of hits can be expected to be distributed over the individual letters. Comparing these expected values with the observed distribution of hits over the individual letters, we can test whether or not the scoring was significantly higher on some individual letters.

TABLE 17

Scoring on letters and numbers, adjusted for target and response frequencies

	A	B	C	D	E	F	G	H
letter O	31	32	26	22	27	26	37	8
E	27.4	31.5	28.8	18.8	35.4	28.0	35.9	3.3

$\chi^2 = 10.2$ df = 7 ns

	1	2	3	4	5	6
number O	22	33	29	39	39	11
E	16.5	27.5	43.1	38.9	38.3	8.7

$\chi^2 = 8.2$ df = 5 ns

Although the overall results of this analysis are entirely insignificant, there is a slight suggestion for both attributes that higher scoring rates occur on symbols which have lower expected values (due to their

low frequencies of occurrence as targets and/or responses). This trend is somewhat more pronounced for the numbers. Together, these results appear consistent with the suggestion that the subject did not treat the experiment as a guessing task, but that the guessing aspect may have played a more prominent role in the numbers or up-down direction. Also the scoring rate proved not be different for the left or right half of the board or for the upper or lower half.

Analysis of Misses for Distance Effects

An important question in an experiment of this type is whether missing responses are still related in systematic though erroneous ways to the possible targets. Foster (1952) reports an analysis (based on data of Brugmans (1922), from the first seven sessions) directed at possible distance effects on letter and number attributes for responses which were not direct hits. The analysis reported seemed to suggest that missing responses might tend to occur in the physical neighborhood of the target. However, this analysis makes the untenable assumption that the targets were selected with equal probability.

We have approached the analysis of distance effects in two different ways. First, we performed complete consistent-missing analyses on the confusion-matrices for numbers and letters separately (see Tables 12 and 13), using the methods outlined in Burdick and Kelly (1977). A preliminary test for displacement effects showed that no such effects occurred for either attribute for +1 or -1 displacements. (Incidentally, the absence of -1 effects--choosing the target from the previous trial as the current response--is unusual, and suggests again that van Dam is not choosing responses in ways characteristic of subjects involved in guessing tasks with trial-by-trial feedback).

For numbers, the consistent-missing analysis is completely non-significant ($\chi^2_{CM} = 19.7$, df = 19). For letters it is also non-significant ($\chi^2_{CM} = 47.7$, df= 41), but there is a slight suggestion of a possible distance effect in that 10 of the 14 cells which are nearest-neighbors of the cells on the main diagonal have observed values exceeding their expectations, some substantially so.

Unfortunately, because of the small number of trials in relation to the size (48x48) of the relevant confusion matrix, the direct consistent-missing analysis cannot be extended to the individual target squares. It remains possible that statistically non-significant individual tendencies toward distance effects involving the separate attributes might also tend to occur jointly in the whole-target data.

To examine this possibility we carried out a distance analysis of the type described earlier, looking this time at the observed vs. expected distribution of distances between targets and responses, for misses only. The results are again entirely non-significant (χ^2 = 32.2, df = 30). Examination of the detailed distributions (Figure 2) shows that the only visually interesting discrepancy occurs at the shortest distance, probably due to the non-significant excess hitting on nearest-neighbors for letters, mentioned above.

Thus the two analyses agree in showing that the scoring has been basically an all-or-nothing affair, with misses distributed across the board according to what would be expected from chance association of targets and responses.

Analysis of Scoring Trends

We have analyzed for two classes of non-random temporal patterns in the distribution of scoring, namely decline effects and clustering of hits.

Figure 3 presents the distribution of the scoring rate over the sessions. It clearly shows a strong decline in scoring rate across the duration of the experiment. The experimenters felt that this decline was strong enough to justify the statement that van Dam had lost his telepathic ability. As has been stated in the description of the experiment, they evidently applied rather subjective criteria instead of statistical ones in forming this opinion. As can be seen from Table 18 the decline effect from first to second half of the sessions is strongly significant, but even in the last part of the experiment the scoring is still significant. We use a chisquare analysis in preference to the CR_d because we are interested specifically in the question whether the observed scoring rates are different in the two halves of the experiment.

TABLE 18

Scoring in first and second half of the series

	first 12 sessions	second 12 sessions	
miss	278	191	
			χ^2 = 16.0 df = 1 p<.001
hit	94	24	

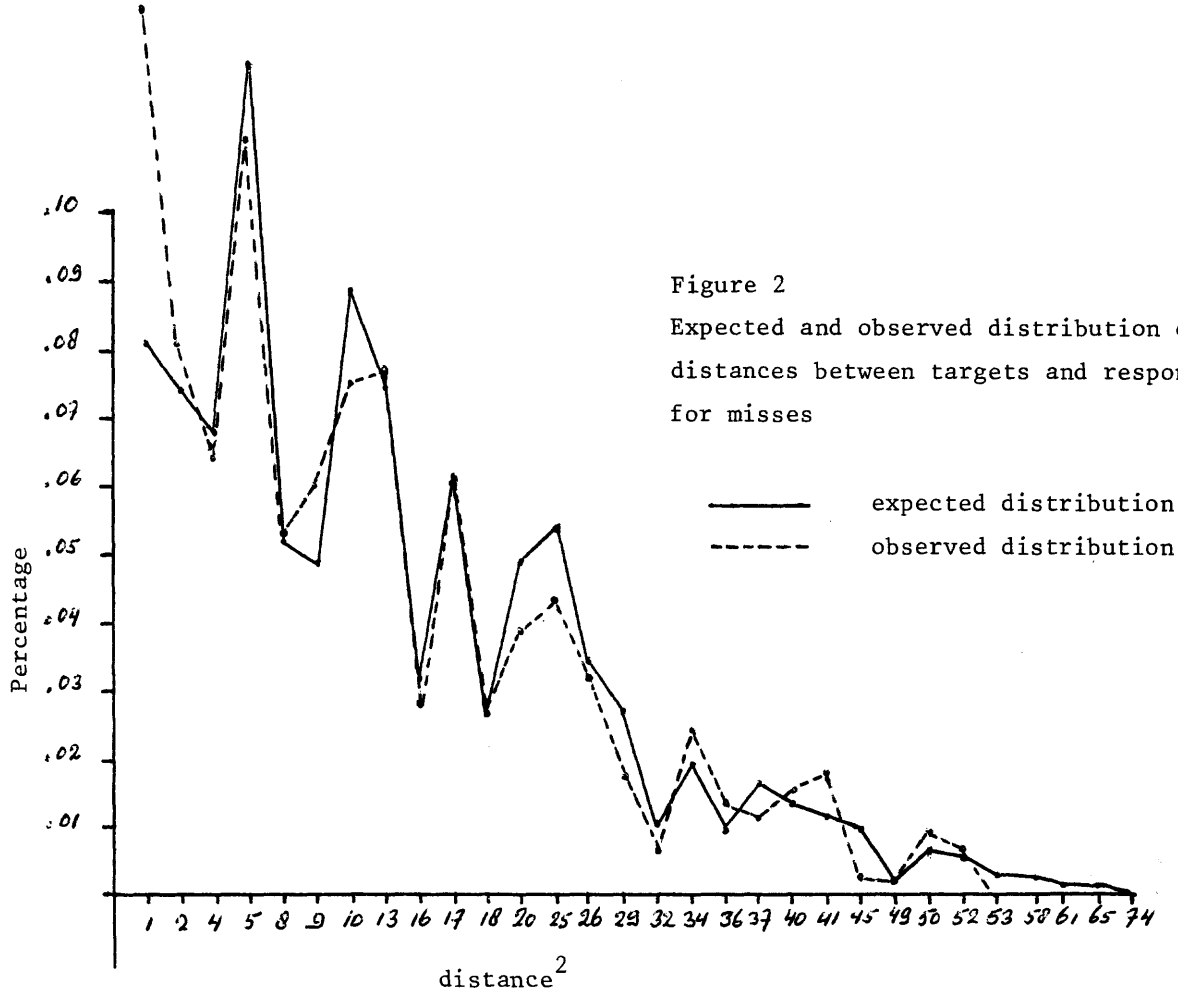


Figure 2
Expected and observed distribution of distances between targets and responses for misses

expected distribution
observed distribution

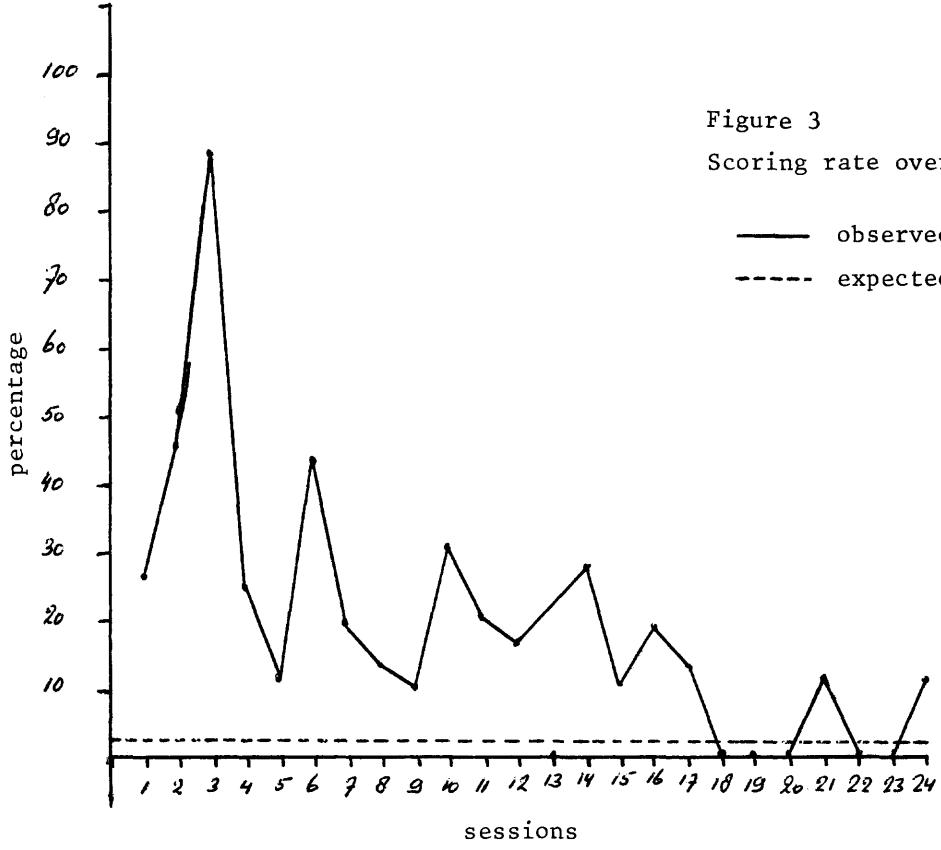


Figure 3

Scoring rate over the sessions

——— observed

----- expected

The last 5 sessions, held in May and June 1922, still yielded 7 hits in 83 trials ($p \sim .002$, Poisson). Nevertheless, if our assumption is correct that trial-by-trial feedback was provided, the strong overall decline seems to stand as a counter example to Tart's (1976) model of psi-stabilization. (The experimenters mention that even during the first seven sessions van Dam experienced increasing difficulties in getting into a suitable frame of mind for the experiments, but they attribute this difficulty to his increasing preoccupation with preparation for examinations toward the end of that period. No explanation is offered for the continuing decline in subsequent sessions, however).

Although the procedure is somewhat artificial because of the widely varying numbers of trials per session, we have also analyzed for within-session declines by dividing each session in half and pooling trials over sessions. Again using the chisquare analysis there is no evidence of differential scoring between session halves. First halves included 51 hits, second halves 66, $\chi^2 = 2.09$ with 1 df.

The experimenters recorded their opinion that van Dam scored most strongly when he had reached what they called a "passive" state. (Heymans, 1924). This opinion is based primarily upon physiological observations, but it is also consistent with the results of analyses for clustering of hits, independent of their overall rate of occurrence. Taking the entire series as a unit, the Wald-Wolfowitz z-score for strings (Burdick and Kelly,1977) is -5.16, $p < .001$. (Although the procedure is again somewhat artificial, this time because it ignores run boundaries, this circumstance appears unlikely to affect the result or its interpretation; only one hit-string crosses a run boundary, a string of length 2 connecting sessions 4 and 5). Similar results are obtained for letters ($z = -2.85$, $p < .003$) and for numbers ($z = -5.68$, $p < .001$), although these are of course not independent of the main result for squares. Two sessions contributed especially strongly to the stringing effect; session 2, in which 11 hits were scored in 24 trials including a string of 8 hits; and session 3 in which there were 16 hits in 18 trials, including a string of 12 (furthermore, the two misses were on adjacent squares). Even in the less successful later sessions, however, the hits still tend to cluster even though the groups are smaller and more scattered. Thus, whatever conditions were conducive to success in the task tended, once established, to persist for periods of time sufficient to span a number of trials. Furthermore, this effect is certainly underestimated due to the form of the presently

available statistical tests, which in effect dichotomize the data by considering only one degree of success at a time. (To illustrate, in the example given above for session 3, van Dam received no partial credit for his two near-misses. The exact-hit stringing analysis thus counts five strings in that session, where a more sensitive procedure might choose a number much closer to one).

Analyses of Response-Time Data

The overall distribution of response times follows a characteristically chi square-like pattern, with considerable right-hand skew caused by small numbers of very slow responses. Fur purposes of analysis we have therefore either applied a normalizing (log) transform (for ANOVA), or blocked the overall distribution into a small number of bins (for table analysis).

Both forms of analysis indicate a significant association between response time and distance from the target, which results from an excess of exact hits at short response times (see Figure 4). The simple 1-way ANOVA for misses vs. hits on squares has $_1F_{368} = 13.5$, $p < .001$, and the same pattern appears for numbers and letters separately, though with lesser strength. Furthermore, response time is influenced by other factors as well: for example, there is a massive difference between near and distant conditions ($_1F_{568} = 36.8$, $p << .001$), the trials in the near condition being completed faster; and there is a marginally significant difference in the distribution of response times within the two halves of the sessions ($\chi^2 = 20.1$, df = 10, $p < .05$), responses tend to be slower in the second half. In both cases no difference exists as regards scoring rate, and so the explanation appears likely to involve normal psychological factors. The slower responses on distant trials, for example, may result from van Dam's feeling less confident and more hesitant under these conditions. Likewise the slowing of responses over the course of a session may reflect some sort of decline in motivation.

In light of the above, we reanalyzed the relationship between hitting and response time as a two-way ANOVA, extracting the variance associated with the near/distant factor. There is no interaction, and the main effects for both factors persist at their original levels of significance or better. Moreover neither effect appears artifactually dependent on a hidden relationship between response time and series position (which is correlated with both miss/hit and near/distant).

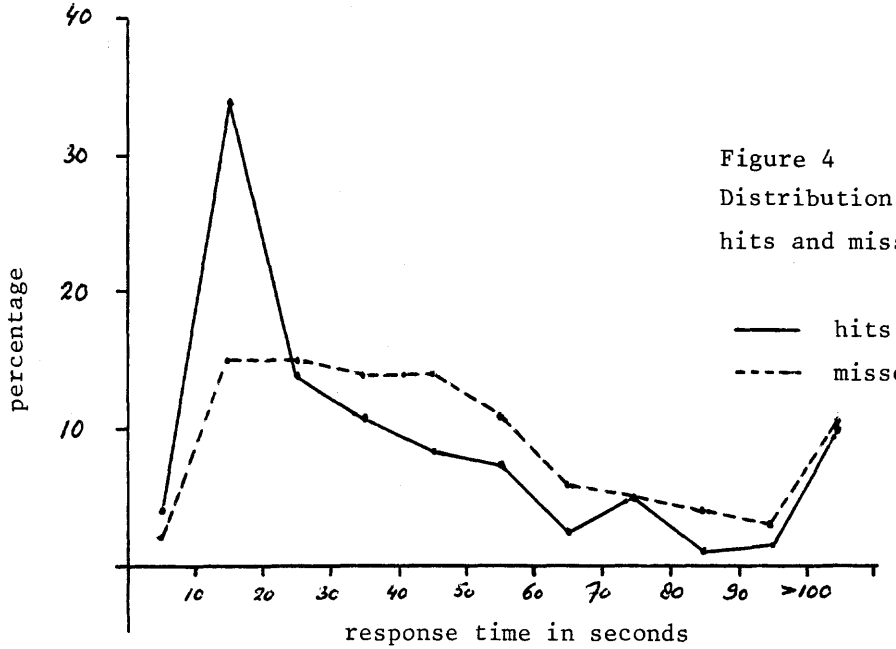


Figure 4
Distribution response times for hits and misses

Thus we have clear evidence that correct responses tend on the average to be faster. (At the same time, however, we should emphasize that a number of very slow responses were also hits, including the slowest response of the entire experiment--9 minutes, 9 seconds. Indeed, the score rate is above chance at every level of response time in Figure 4). There may be numerous factors influencing response time, and the interpretation of this result does not appear straightforward. Unfortunately, the available reports contain no helpful information on this question.

A further puzzle is raised by an apparent effect of experimenters on response time ($_2F_{559} = 5.83$, $p < .003$), contributed mainly by slow responses occurring when Brugmans acted as agent. We unfortunately cannot tell from the reports whether van Dam typically knew in advance the identy of the agent, so that this result might or might not represent some sort of psi effect.

The Ambiguous Responses

In 131 trials an ambiguous response was recorded. As stated before, all analyses presented have been based on the MDR set of responses, using those responses in the ambiguous trials which were more distant from the target square.

As mentioned above, neither the target letter distributions nor the target number distributions differ significantly for ambiguous and non-ambiguous trials (letters: $\chi^2 = 8.8$, df = 7; numbers: $\chi^2 = 4.4$, df = 5).

The distribution of response letters and numbers in the LDR set of responses do differ for ambiguous and non-ambiguous trials (letters: $\chi^2 = 14.3$, df = 7, $p = .05$; numbers: $\chi^2 = 21.6$, df= 5, $p < .01$), but these differences disappear when the distributions for letters and numbers of the MDR set of responses are analyzed (letters: $\chi^2 = 10.8$, df = 7; numbers: $\chi^2 = 9.3$, df = 5). We assume that the deviant distributions in the LDR set of responses are caused by the way these responses were formed. We know that the targets were not equally distributed over the board, and in particular that the numbers tended to be concentrated in the centre. By taking systematically those responses which are closest to the target we probably introduce a slightly deviant frequency distribution for those responses, in effect exaggerating van Dam's already pronounced tendency to choose responses in the interior of the checkerboard. This is directly supported by examining the LDR and MDR response frequency distributions. For both letters and numbers the LDR set shows higher frequencies in the interior, and in particular the significant non-uniformity of the number response

distribution in the LDR set of responses is mainly caused by strong negative deviations for the extreme numbers, that is the numbers 1 and 6. Thus the MDR dataset appears in general to represent a more "natural" way of resolving the response ambiguities for purposes of statistical analysis, although as we have indicated none of the main results is affected by this choice.

We have not been able to determine unequivocally the sources of response ambiguity, which could lie either in the subject, the observers, or both. Before discussing these possibilities, let us outline the main facts which a correct account must explain.

First, the scoring rates for trials with ambiguous responses are markedly lower than for trials with clear responses. Even using the LDR data (since the MDR set has by definition no exact hits on ambiguous trials), the effect is strongly present in the scoring on exact squares (Table 19).

TABLE 19

Scoring on squares in ambiguous and non-ambiguous trials

LDR set of responses

	non-ambiguous	ambiguous	
miss	338	122	$\chi^2 = 20.6$, df = 1, $p < .001$
hit	118	9	

In this table the hit rate for non-ambiguous responses is almost 4 times that for ambiguous responses, even though the latter is itself better than 3 times MCE, $p < .003$(Poisson). For the numbers the same effect is marginally present ($\chi^2 = 4.4$, df = 1, $p < .05$), and for the letters it is again very strong ($\chi^2 = 18.9$, df = 1, $p < .001$).

Second, the response times are markedly longer for the ambiguous trials, by both a chisquare analysis ($\chi^2 = 25.3$, df = 10, $p < .005$) and the ANOVA using log response time ($F_{1,568} = 9.76$, $p < .002$). There is also a significant association between experimenter and clear vs. ambiguous response due mainly to a large excess of ambiguous responses occurring with Brugmans as agent (Table 20), and as mentioned earlier the response times for Brugmans are also longer; however, the appropriate 2-way ANOVA's show that these effects are independently significant and additive.

TABLE 20

Response ambiguity vs. experimenters (MDR data)

	B	H	W	
clear	163	139	146	$\chi^2 = 8.06$, df = 2, $p<.02$
ambiguous	65	36	30	

Finally, we looked at the form of the response ambiguities, to determine whether they occurred predominantly in one or the other direction. The difference as regards direction is non-significant, considering that there are seven boundaries for letters and five for numbers. (Table 21).

TABLE 21

Form of response ambiguity

letter differs, number equal	number differs, letter equal	letter and number differ
54	42	35

What seems to us the most plausible interpretation of those observations requires as an essential premise that van Dam could distinguish (presumably by tactile means) the boundaries between squares. The reports do not explicitly state that this was the case, but the photographs suggest that the lines were etched or dug into the surface of the board. We then suppose that van Dam himself was the main source of ambiguity, deliberately tapping between two squares when he felt uncertain (and hence was more likely both to respond slowly and to end by missing the target). The higher rate of ambiguous responses with Brugmans acting as agent would be easy to explain if van Dam knew the agent for each trial in advance and simply felt less confident with Brugmans. Even if he did not consciously know the agent, the result might still be a psi effect indicating that he knew unconsciously, or that Brugmans was for unknown reasons a less effective agent. The view that van Dam was the source also appears relatively consistent with our knowledge of the physical layout of the situation and with the data on the form of ambiguous responses: From the photographs of the checkerboard taken down through the ceiling it appears very unlikely that the

observers would have had any difficulty in correctly reading an unambiguous response by van Dam.

Plausible though it is, this is only one of possible "stories" about the data, and the available material is not sufficient to allow us to choose decisively among them. In particular it remains quite possible that ambiguity was contributed from the observer's side, either solely or in addition to ambiguity introduced knowingly or unknowingly by van Dam. For example, the excess ambiguity when Brugmans was agent might indicate that Heymans and Weinberg (who in this case functioned as the observers) tended to be more indecisive recorders and more inclined to rate a given response as ambiguous (of course we would then have to explain why they chose systematically thus to treat responses which were slower and more likely to be misses, but perhaps these factors were also related to some aspect of the trajectory of van Dam's hand that made it more difficult to read his final response).

Another possibility is that the observers knew what the target was, and tended to stay on the safe side by rating ambiguous trials which were potential hits as non-ambiguous misses. Alternatively, they might have had a tendency to rate some ambiguous trials as complete hits when they had a choice between the target square and an adjacent square. Both explanations suppose, however, that the observers knew the target before the response was recorded. They might conceivably have learned this from the behavior of the agent, but in that case we should expect a strong difference between the near and distant condition as regards the amount of ambiguous trials. In the distant condition the agent stayed in the upper room, could not be seen or heard by the subject and hence he was rather free to show his expressions. It is explicitly stated, however, that to avoid sensory cues the agent tried to control himself as much as possible in the near condition. However, the difference as regards number of ambiguous responses between near and distant condition is marginally significant in favor of the latter, the distant condition having relatively more ambiguous trials (see Table 22). If the observers had tried to guess the

TABLE 22

Ambiguous responses in near and distant conditions

	near	distant	
non-ambiguous	285	170	
			$\chi^2 = 4.45$ $p<.05$
ambiguous	68	63	

target from the agent's behavior in order to avoid ambiguity when the response was close to the target area, one should expect an opposite effect, because it must have been much more difficult for them to guess the target in the near condition. The results of Table 22 make much more sense in terms of our original hypothesis, that van Dam voluntarily generated ambiguous responses when he felt particularly uncertain.

If we assume the worst, that is, that the observers knew the target and tended to rate ambiguous responses bordering the target square as complete hits, we can calculate an estimate of the resulting excess of complete hits based on the ratio of ambiguous versus non-ambiguous responses for the misses. This ratio is 122:338 (see Table 19). There are 118 non-ambiguous responses which are complete hits. We now assume that part of these hits should have been recorded as ambiguous responses. Hence the number of 118 is reduced by 122/338 x 118 - 9 = 34, we subtract 9 because 9 ambiguous hits have been recorded. So based on these assumptions we should have expected approximately 84 direct hits and 43 ambiguous hits. Even in that case the result of the experiment would be highly significant when considering complete hits only, while for the distance distributions between target and responses the difference would be minimal.

DISCUSSION

The experiment and its statistical results have been presented in considerable detail. To complete the agenda set forth in our introduction we will now draw upon the results outlined above in reassessing the question of possible fatal methodological difficulties in the experiment. The statistical difficulty, that the properties of the target order might be inadequate to support valid statistical analysis of the data, has been decisively overthrown. There can be no reasonable doubt that results of extreme statistical significance were obtained. The procedural difficulty, however, that these results might be explained by sensory leakage, is more serious and requires further discussion.

The hypothesis advanced by Soal and Bateman (1954) and eagerly accepted by Hansel (1966), is that the observers may have become excited as van Dam's hand approached the target square, and hence quite unintentionally (perhaps through some sort of bodily movement shifting the distribution of weight on the floorboards) conveyed an auditory "stop" signal.

Whately Carington (1946) had considered and rejected this same argument. He had paid a visit to Groningen in 1937 and carefully

examined the floor between the two rooms, which he found to be "... of such solid construction that no ordinary movement, change of breathing, or the like, could possibly have acted as a "stop" signal unless we assume a quite extraordinary degree of hyper-sensitivity on the part of the subject..." Soal was willing to make that assumption, and rationalized it by pointing to the case of the stage-telepathist Fred Marion as an example of the required sensitivity. Although his discussion acknowledges that the experimenters were good psychologists and likely to be on guard against auditory cues, Soal clearly intimates that they would not have been prepared by experience to cope with the capacities of a Fred Marion.

Evidently Soal was not familiar with the background of the experiment, which in this instance is essential. Not only were the experimenters prepared in a general way to deal with problems of sensory leakage, but also they were prepared specifically for the problems posed by a Fred Marion, or in their case a Rubini. They were entirely aware of the possibility that sensory cues not noticeable to "normal" people might produce hits, and state explicitly their view that all of the Rubini-type demonstrations were defective for that reason. Against this background, their procedures were specifically framed so as to be "Rubini-proof". The elimination of visual cues can be unambiguously judged as completely satisfactory, based upon the reports and the photographs. The difficulty in judging the more critical auditory case arises because the reports do not and cannot convey in an analogously simple way the adequacy of the physical barrier. But certainly the experimenters' testimony that they took all possible prior precautions to eliminate auditory cues, in full awareness of the Rubini problem, must be taken seriously.

It also seems likely that in practice the experimenters would quickly have detected any general contingency of the hypothesized sort between van Dam's success and their behavior upstairs. Scrapes or thumps on the floor audible downstairs to van Dam would almost certainly have been audible to the experimenters as well.

Furthermore, such cues even had they existed would probably not have been adequately specific to identify the target. Recall that the agent's general task was constantly to steer van Dam's hand toward the target. Rather than passively awaiting the final outcome, the agent was involved throughout the course of a trial and hence if generating any auditory cues at all, probably generating many cues of similar form. Also the observers might have generated cues unrelated to the target, particularly at the beginnings of trials.

The auditory-cues hypothesis also leads to several other expectations that seem inconsistent with the data, even if we allow that target-specific cues could have occurred as van Dam groped over the target square. First, one would presumably expect to find a statistical excess of responses in near-neighborhoods of the target squares, but this effect did not occur. Second, the scoring might reasonably be expected to differ across experimenters because of subtle differences in the subtle cues they produced in playing the role of agent, but it does not. Similarly, scoring might be expected to differ between near and distant conditions because of differences in quality and quantity of available cues, but it does not. Third, one might expect that longer trials would be more likely to result in hits, since on the average van Dam's hand would cross the target area more often and thus provide more opportunities for him to receive auditory cues; exactly the reverse occurred. This argument assumes in fact a constant generation of very weak auditory cues. In the case of occasionally clear auditory cues, an opposite effect could be expected. The subject could then respond quickly with a good chance of being correct, while in those trials where no cues were provided he would tend to take more time for his response and would be more likely to miss the target. But considering the construction of the floor between the two rooms, it is difficult to imagine that such strong auditory cues could have been perceived by the subject but would have passed unnoticed by agent and observers. Finally, one could reasonably expect van Dam to improve or at least maintain his ability, but instead it declined rather swiftly.

Therefore, although absolute certainty cannot be claimed, the weight of the historical and experimental evidence seems to us to favor strongly the experimenters' own belief that they had succeeded in eliminating sensory alternatives to the psi hypothesis. Carington (1938) stated that he had carefully discussed the possibility of 'stop signals' with Prof. Brugmans who however remained convinced that nothing of the kind could have occurred. Both the principal investigators, Heymans and Brugmans, became convinced of the reality of telepathy by this experiment, amongst others because they felt they had excluded all possible sensory cues. There was certainly room for improvement in the randomization and recording procedures, and the reports could easily have eliminated the need for several inconclusive discussions merely by giving more details about procedural matters such as the physical structure of the checkerboard, the sources of response ambiguity, and the information van Dam received before and after trials. Nevertheless the experiment remains in our view professionally done and

fundamentally sound, in many respects still very good and certainly exceptionally good for its time.

It also contains a rich variety of interesting and statistically strong effects, a number of which are reported here for the first time. The results that strike us as most relevant to contemporary needs in parapsychology, however, are those such as the stringing results and most particularly the GSR results-- suggestive of a psi-conducive psychophysiological state. Certainly one of the most conspicuous facts about psi phenomena is their tendency to congregate around certain individuals. Correspondingly, a high-priority task for experimenters is to press as far as possible analysis of the factors at work in such individuals, that may account for their exceptional abilities in this kind of research. The experiment of Brugmans, Heymans, and Weinberg is certainly a major landmark.

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THE LIMITS OF PARAPSYCHOLOGY

John Beloff

In the November Issue 1977, of the Journal, the Editor, Martin Johnson, posed the question: who is a parapsychologist? And he rightly points out that, to answer this question, we must first decide what is meant by parapsychology. "Does it", he asks rather plaintively, "include psychotronics, Kirlian photography, studies of 'bioplasma', transpersonal psychology, Backster plant communication, healing, survival research, astrology etc.?" Certainly, not the least of the problems which confront parapsychology is that of defining its own terms of reference and it is this problem that I want, in this paper, to discuss.

The broadest definition of parapsychology and the one to which, in the past, I myself have adhered, is that which defines parapsychology as: the scientific study of the paranormal. A more restricted definition would be: the scientific study of psi phenomena, where 'psi phenomena' are taken as a subclass of the paranormal which depends critically on the exercise of the subject's mental powers. Thus, the Society for Psychical Research, in the official statement of its aims which is reprinted inside the cover of every issue of its journal, speaks of examining "without prejudice or prepossession and in a scientific spirit those faculties of man, real or supposed, which appear inexplicable on any generally recognized hypothesis" (my underlining). And, in a like vein, the Journal of Parapsychology, in the glossary which accompanies each issue, speaks of parapsychology as: "the branch of science which deals with psi communication i.e. behavioural or personal exchanges with the environment which are extrasensorimotor". In these two examples there is no mistaking the reference to mind, indeed the very words, parapsychology or psychical research proclaims this involvement.

Until quite recently I considered it perfectly safe to use the broad definition because I did not believe that there were any phenomena that were paranormal, in the sense of being "inexplicable on any generally recognized hypothesis" that were not, at the same time, manifestations of mind. I knew, of course, that there existed a great many borderline or occult sciences which purported to study such

phenomena but I did not believe that they were scientific and so I thought they could be ignored. Today I am no longer so sure. Several things have since come to my attention which suggest that there may be some radical anomalies in nature that are not due to the intervention of mind and cannot be subsumed under the categories of ESP or PK.

Consider, for example, a borderline phenomenon like that of acupuncture. It so happened that a certain Ph.D. student at the University of Edingburgh whom I knew well, Duncan Stewart, successfully validated the analgesic properties of acupuncture in a way that wholly eliminated the effects of suggestion (Stewart, Thomson & Oswald 1977). Now there is, of course, no necessity to think of acupuncture as paranormal and, indeed, Stewart made no such claim. Certain theories are now current which suppose that it may work by releasing certain pain-inhibiting substances ('endorphins') that are now known to be secreted by the brain. At all events, there is no reason to doubt that, in due course, acupuncture will be integrated into accepted neurophysiological knowledge. Nevertheless, there can be no question that, until very recently, acupuncture would have been dismissed out of hand by every medical authority in the West as a nonsensical relic of a prescientific era and any analgesic properties that might have been claimed for it would have been put down to the familiar and all-pervasive power of suggestion. The very fact that it belonged to a belief-system wholly alien to Western Science ensured that it would meet with a resistance such as every occult and paranormal claim always evokes.

Far more devastating, however, in upsetting my complacency was the discovery that astrology, that archetypal pseudo-science which I had always treated with contempt, was not wholly lacking in empirical support. It was, finally, the visit to this department of Michel Gauquelin which clinched the matter for me in this respect. For the fact is that, as a result of the Gauquelins' massive labours, which have since been independently corroborated by sceptical, not to say hostile, committees both in Europe and the United States (M & F Gauquelin 1977) it is now no longer possible to deny that certain hypotheses which ostensibly make sense only within the astrological tradition have stood up to attempts at disconfirmation and have done so, moreover, with a reliability and robustness beyond anything that one could point to within parapsychology itself. Very briefly, what the Gauquelins have succeeded in demonstrating is that special groups, selected on the basis of certain objective criteria (e.g. international sportsmen as cited in the appropriate reference works), tend to be born more often than chance would allow when a particular planet occupies a critical sector of its diurnal

cycle (in the case of sportsmen, the planet Mars).

Now it is only fair to add at this point that Gauquelin, himself, does not regard his findings as a confirmation of traditional astrological beliefs even if his hypotheses derived from this source. He hopes, rather, that a scientifically acceptable explanation of them will in due course be forthcoming. Meanwhile he has himself offered certain conjectures regarding the possibility of a physical influence of the relevant planet on the mother or on the foetus itself which, according to the latest evidence of medical science, can, to some extent, control the labour which leads to its delivery via certain hormonal secretions. Since 1966, Gauquelin has been able to demonstrate quite clearly a planetary effect of an hereditary kind such that there is a significant tendency for an individual to be born when a given planet is in a critical sector if one or, better still, both its parents were born when this planet was in such a sector. Hence, if we suppose that the planet can influence the hormonal mechanism of the foetus and if, further, we suppose that individuals will vary in their sensitivity to this influence and that this sensitivity is somehow linked to certain innate personality traits and that, finally, the personality of the offspring tends to resemble the personality of one or both of its parents, then we have a natural explanation of sorts for something like Gauquelin's Mars effect (M & F Gauquelin 1977a). But, of course, there is here, as Gauquelin himself would not deny, a staggering chain of assumptions and conjectures which have to be granted before the explanation can even begin to appear plausible. Meanwhile, such astrological type findings fit more neatly the Jungian principle of synchronicity than they do either the natural sciences or parapsychology.

The third encounter to shake my preconceptions was my exposure to the UFO evidence. This came about as a result of reading Scott Rogo's 'The Haunted Universe' (Rogo 1977), a book which I would not ordinarily have bothered to read but which, for personal reasons, I decided both to read and to review. The author, who is a prolific American writer of popular books on parapsychological topics as well as being a researcher known to readers of this journal, deals here with two particular themes: religious miracles and UFO visitations. The thesis he sets out to defend is that both classes of phenomena can best be understood as special manifestations of PK emanating from a purely human source (whether it be that of an individual or of a group). If this thesis is correct then, so far as ufology is concerned, we may look forward to a time when it has become absorbed by parapsychology although, in the process, parapsychology would have to be stretched to accommodate a range

of phenomena of a magnitude exceeding anything we have so far had to reckon with even in the literature on poltergeists.

Previously I was content to treat the UFO evidence in much the same dismissive way as the orthodox scientist treats the parapsychological evidence. The arguments against accepting the E.T.I, (Extra-Terrestrial Intelligence) hypothesis (Hynek 1972), i.e. that UFOs are space vehicles of a technologically more advanced civilization existing somewhere in the Galaxy, have always struck me as so compelling that no amount of ordinary human testimony was likely to make me change my mind on that score. Furthermore, the accounts received about such objects, with their sudden appearances and disappearances and their crazy kinematics, suggested something much more akin to apparitions than to any machine or vehicle known to man. Consequently, Rogo's parapsychological interpretations, and in particular his evidence of a link between UFOs and poltergeist phenomena (often the same subject is involved with both) were sufficiently appealing to make me want to reconsider my whole attitude to the UFO question. Of particular importance, in this connection, was the case of Mrs. Stella Lansing which he presents and I want therefore to dwell on this case in some detail.

It was in 1971 that Mrs. Lansing came to the attention of Dr. Berthold Schwarz a psychiatrist who also happened to be interested in ufology and is also well known in parapsychological circles. She had a previous history of mental illness which was somehow connected with her UFO sightings, a point that, no doubt, did nothing to increase her credibility. However, Dr. Schwarz persevered and, in February 1971, he accompanied Mrs. Lansing on one of her UFO field trips. The upshot is best given in his own words:

"I went with her at 4 a.m. to one of her familiar UFO sites, overlooking a hill top that was cleared for high-tension wires.[&]

[&]I am not sure how we are to construe the phrase "cleared for high-tension wires". Does it mean (as I took it to mean) that the site had been scheduled for the erection of pylons or that the high-tension wires were already in place? In this connection Martin Johnson makes the following interesting and pertinent observation: "a large number of UFO observations have been made above and around high tension wires, sightings which are characterized by being yellow-orange and by their gliding movements. It has been suggested, and also to some extent simulated under laboratory conditions, that these glowing phenomena are either plasma phenomena or discs of gas which, due to the high-tension wires, have been brought to glow. . . . ." (personal communication).

(continued at bottom next page)

At that time the dark sky was suddenly lit up and we saw a round pulsating, bright yellow-orange, noiselessly gliding light, which expanded and contracted, went out and relighted. Mrs. Lansing intermittently photographed this over several minutes while I tape recorded the event. Stars and the moon were also seen and photographed as controls." (Rogo 1977 p.114).

On another occasion Dr.Schwarz was joined by Dr.I.Grattan-Guinness, a British mathematician and editor of 'Annals of Science' (a learned journal devoted to the history of science), who witnessed a similar incident in the presence of Mrs.Lansing outside the house of Dr. Schwarz (Grattan-Guinness 1975). Dr. Grattan-Guinness draws attention to this ability which Mrs.Lansing seems to possess to impress images onto photographic film "after the manner of the psychic photographers" although these images do not necessarily correspond to the objects seen in the sky and may take the form of "clocklike formations". By good fortune I was able to meet Dr.Grattan-Guinness when he came to Edinburgh last August to attend the International Congress for the History of Science and he was able to confirm the favourable impression he had formed of this case which he had expressed in writing. It was this ability to produce images which brought her to the attention of Dr.Jule Eisenbud who carried out some tests with her. Although he obtained no spectacular results he satisfied himself that there were some unexplained effects on film. In a private communication which I received from him he writes: "My opinion of Stella is of a woman obsessed with UFOs and of being paranormally responsible for much or all of what she sees and obtains on film . . . " Thus we have here a case which brings together in a single subject UFO phenomena and psychic photography. Such a case is not easily accommodated within the familiar conceptual framework of parapsychology as this has developed since the advent of Rhine.

I come next to what at present may be considered the most serious challenge to parapsychology as this has so far been understood

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If this is indeed the explanation of the Stella Lansing sightings certain questions will have to be answered. For example, were there also high-tension wires in the neighbourhood of Dr Schwarz' house where the second sighting mentioned here, as witnessed by Dr Grattan-Guinness took place? In general we can say that if the Martin Johnson hypothesis is correct we shall find such phenomena associated with certain special locales and certain atmospheric conditions; if the Scott Rogo hypothesis is correct we shall find them rather associated with particular psychic individuals.)

and practised in the West, namely the movement of East European origin best known, perhaps, as 'psychotronics'. The term itself was introduced by Z. Rejdak, a Czech, who tells us that he borrowed it from a certain French radio engineer. It is a portmanteau coinage combining the words 'psyche' and 'electronics' and it may be considered as roughly synonymous with the term 'psychoenergetics' (the preferred expression in the Soviet Union) or with the terms 'paraphysics' or 'parascience' (terms widely current now in the West). Basically, it conceives of psi phenomena as manifestations of a special kind of energy associated with living organisms. As Rejdak himself puts it:"we suggest calling psychotronic, phenomena which concern energy produced in the process of thought or in acts of will" (Rejdak 1977, my translation). Particular prominence is given to the concept of 'bioplasmic' energy or the 'bioplasmic'body, concepts first introduced, it appears, by the Russian investigator V.M. Inyushin (Moss 1974), and since used extensively by adepts of Kirlian electrophotography as a short of rationale for this technique. In their best-selling book 'Psychic Discoveries Behind the Iron Curtain', which provides the most comprehensive coverage of the psychotronic movement in English, Ostrander and Schroeder state the case for this concept with a disarming naïveté when they write: "Thanks to the Kirlian discovery, bioplasmic energy can be seen by anyone in photographs and electron microscopes. It can be scientifically studied as it swirls in sparkling flares of colour" (Ostrander & Schroeder 1970). (To most of us it will be news that we can see energy of any kind, though it requires energy in order to see at all, but it would be unfair to be too literal, the authors are journalists, not scientists).

The International Association for Psychotronic Research held their third international congress last June in Tokyo, and some 130 papers were read on this occasion by participants from all over the world. A useful summary of the proceedings has been compiled by Benson Herbert (head of the 'Paraphysical Laboratory', Downton, Wiltshire, England) and appears in the 'Parapsychology Review' for Sept/Oct 1977. From this it is easy to see that most of these papers would never have been accepted for a Parapsychological Association Convention largely because they deal with phenomena and concepts which are not regarded as having any validity by a body such as the Parapsychological Association, however much popularity they may enjoy in Russia and Eastern Europe. Perhaps the most bizarre product of the psychotronic movement is the invention of 'psychotronic generators' by the Czech, Robert Pavlita, who took the idea of psychic energy so literally that he decided to try and store it and harness it to drive a mechanical device ! Such a claim

would have come as no surprise to Anton Mesmer but it naturally aroused the deepest misgivings among Western parapsychologists especially as Pavlita steadfastly refused to let anyone examine his generators !

Hardly less bizarre were the claims made on behalf of 'pyramid power'. In 1959, a Czech radio-engineer from Prague, Karl Drbal, took out a patent at the Czech patent office for a device he called 'The Cheops Pyramid Razor-Blade Sharpener'. Basing himself on the reports of a French archaeologist who observed that the remains of animals and of food found in the Egyptian pyramids were preternaturally well preserved, he reasoned that the preservative agent could be nothing other than the form of the pyramid itself. Accordingly, a small plastic pyramid should suffice to extend the working-life of an ordinary steel razor-blade which had been kept suitably positioned inside it as well as preserving organic matter from immediate decay. Two articles have appeared recently in the Canadian parapsychological journal 'New Horizons' which test these claims, the one dealing mainly with razor blades (Simmons 1973) the other with plants of the species phylodendron (Flynn 1977). In neither case were the claims upheld and no difference was found of an objective kind between the specimen kept in the pyramid and the control specimen kept outside. However, pyramidology has a long history and it is unlikely that the superstitions to which it has given rise will be scotched by any objective evidence, especially now that psychotronics has provided a fertile field in which they can flourish.

Another phenomenon which figures rather prominently in the writings of the psychotronic school is the so-called 'Backster Effect' to which Martin Johnson alluded. According to Cleve Backster plants have feelings and are specially sensitive to any destruction of life that may be going on in their vicinity. He claimed that this could be verified by attaching electrodes to their leaves and observing the fluctuations in electrical resistance recorded by the polygraph. Despite repeated failures to confirm these claims (which he first made in 1968) when conditions were tightened up to exclude artefacts due to humidity etc., the world-wide interest which they aroused has still not abated and, not surprisingly they again came up for discussion in Tokyo in a theoretical paper given by a scientist from the Argentine.

The idea that plants might represent psi-sources in their own right, as distinct from merely being targets for a human PK effect, is one that has attracted many serious parapsychologists who would spurn anything as crudely anthropomorphic as Backster's 'primary perception in plants'. For example at the 1977 Parapsychological

Association Convention in Washington, the philosopher, Dr.Hoyt Edge, from Florida, described an experiment which he had carried out to test whether a plant could influence the output from a Random Number Generator when this was coupled with a lamp which provided the plant with its only source of light. However, the one session which yielded a significant departure from chance did so in a negative direction, a result hard to reconcile with the idea of the plant being the responsible agent, at least without much special pleading (Edge 1977). This experiment recalls a series of experiments which created a stir some years ago which were carried out at the FRNM under the ill-fated W.J. Levy. He used live chicken eggs to see whether they would be able to influence a Random Number Generator coupled with a lamp which served as their incubator. Some attempts to follow up this line of investigation were later carried out in Britain by John Randall but without, I gather, much success and since then it seems to have suffered from the eclipse which overtook its originator. John Randall, a biologist and a parapsychologist of the strict school, was sympathetic to the vitalist interpretation of psi (Randall 1975) and hence was open to the possibility that all living things without exception might possess the potentialities for generating psi effects.

Let us now take stock of where we stand. First, there can be no doubt whatever that, to the public at large, we are by no means the only discipline entitled to pronounce on matters of a paranormal nature. One has only to visit one's local bookstore to realize that what we call parapsychology as no more than a fraction of the total literature dealing with the paranormal which is classified usually under the heading of 'The Occult'. Nor is it only in a commercial context that we find ourselves juxtaposed with some strange bedfellows. The popular weekly science journal, 'New Scientist' devoted a special section last July to 'Parascience' under which umbrella it discussed three particular topics: (1) The Bermuda Triangle; (2) UFOs; and (3) Spoon-bending (sic). During the past year two new journals have been launched in the United States, both dealing with paranormal claims but the one from a sympathetic standpoint, the other from a sceptical or antagonistic position. The former calls itself the 'Journal of Occult Studies', the latter the 'Zetetic'or 'Skeptical Inquirer' and is the mouthpiece of the self-styled Committee for the Scientific Investigation of Claims of the Paranormal (C.S.I.C.P.). Both journals agree, however, in making no distinction between parapsychology and the various other fringe or para-sciences.

Certainly, when it comes to Ufology, the sheer scale of the evidence makes it very difficult to ignore. Since 1947 thousands

of sightings have been documented, one investigator, Dr.David Saunders of the University of Colorado, has a file of some 50,000 computerized records and these represent no more than a fraction of all the alleged sightings. Thus the Official Condon Report, which did not, of course, acknowledge the reality of UFOs, admits that probably no more than ten percent of those who believe they have spotted a UFO bother to report it to the authorities. A Gallup Poll carried out in the United States in 1973 showed that some 11% of all respondents claimed to have seen at least one UFO in the course of their lives, a figure which, if extrapolated to the general population of the United States would amount to 15 million persons ! Of course, a substantial proportion of all sightings have no doubt a straightforward naturalistic explanation as do the sightings of the Loch Ness Monster. (One scientist who believes in the reality of the Loch Ness Monster as a flesh-and-blood creature is Dr.Roy Mackal of the University of Chicago. He reckons that over 10,000 sightings of the monster have been reported over the years of which 3,000 have been recorded in written or printed form. Of the latter he accepts only 251 as having a prima facie validity after discarding all reports that seemed to him to suggest "waves, birds, logs and other such known objects", see R.P. Mackal 'The Monsters of Loch Ness' (London: Futura Paperback 1976) p.84.)
But there is a hard core of UFO cases which are quite clearly incompatible with any other normal explanation except that of sheer mendacity. And since some of these involve more than one informant this implies a fairly extensively conspiracy to deceive (let us not forget that President Carter himself once reported a UFO sighting !). The trouble is that if we do adhere to the conspiracy theory we have got to ask ourselves how much of the parapsychological evidence, at any rate of the spontaneous kind, we may have to discard for the same reasons.

The situation in which parapsychology now finds itself may be seen as the result of two developments of recent history: the so-called 'occult revival' which hit the American campuses during the 1960s and the reemergence of parapsychology in the countries of Eastern Europe dominated by a Marxist-Materialist ideology. These together with the continuing growth of the UFO movement have created a climate of opinion in which the public image of parapsychology has become confused with its many competitors.
In the light of this situation there are, I will suggest, two ways by which parapsychology could seek to preserve its integrity and its autonomy. First it could apply purely evidential criteria in deciding which phenomena it would recognize. In this way, some of the more outlandish claims, such, for example, as the Backster Effect

could be excluded simply because the evidence was too weak to pass muster, not because of any a priori objection to the use of plants as psi-sources. If we do adopt this policy then we are back again with our broad definition of parapsychology as the scientific study of the paranormal and, provided only that the evidence can stand up to scrutiny, it would not matter whether the paranormal was represented by plants, eggs or even pyramids.

Indeed, until we know a great deal more about the nature of psi there may well be no alternative way of defining our science than in terms of this purely negative concept of paranormality. Exactly the same situation, we discover, currently prevails in ufology. Alan Hynek's definition of a UFO (Hynek 1972 p.10) could, mutatis mutandis be applied to any psi phenomenon:

"We can define a UFO simply as the reported perception of an object or light seen in the sky or upon land, the appearance, trajectory, and general dynamic and luminescent behaviour of which do not suggest a logical, conventional explanation and which is not only mystifying to the original percipients but remains unidentified after close scrutiny of all available evidence by persons who are technically capable of making a common sense identification, if one is possible".

But we should never abandon the hope that, as parapsychology matures, it will become possible to define its scope in more positive terms, just as ufology, if it is ever to progress, will have to decide as between the E.T.I, hypothesis and the hypothesis of psychic creation. At present there are, as I see it, two issues which parapsychology must settle before this stage is reached. The first is the question of whether psi phenomena are energetic and physical or whether they are purely informational. The psychotronic approach, as we have seen, assumes that they are physical and, like consciousness itself, the manifestation of a special kind of energy. There is, however, another school of thought which has been gaining ground recently and which has received detailed treatment in recent issues of this journal. This is represented by the so-called 'observational theories of psi' and it is specially associated with those who have come into parapsychology from the physical sciences, like Helmut Schmidt, E. Harris Walker, Brian Millar or Richard Mattuck. On this view, psi phenomena are essentially the outcome of biassing the probabilities inherent in the target-system so that events occur which, while extremely improbable, do not actually violate any known laws of physics apart from the fact that the entropy of the target-system is

changed in a direction contrary to that indicated by the second law of thermodynamics. The source of this bias is none other than the observer himself and it is brought into play at the very moment when the observer witnesses the end result of the process as displayed in the appropriate feedback channel. This implies, among other things, that psi operates retroactively in time, an implication that many find highly paradoxical if not absurd. We need far more empirical evidence before we can tell whether the observational theories, as currently understood, will be vindicated but my guess is that an informational interpretation of psi is more likely to prove correct than an energetic one.

The other critical issue that will need resolving is whether a vitalist or a mentalist view of psi is justified. My own surmise here is that the pioneers of the Society for Psychical Research and the founders of the Duke Laboratory had the right idea in supposing that psi was a function of mind rather than of life as such. Not necessarily the human mind of course, it is a faculty we may well share with other species, especially perhaps with our domestic animals with whom we interact, but this is still a far cry from looking for a psi source in brine-shrimp, cockroaches, to say nothing of eggs, plants or tissue-cultures ! Yet, precisely because I favour the mentalist position, I am anxious that work should proceed that is inspired by the vitalist position, for only the failure of the latter can secure the adoption of the former. Unfortunately, the vindication of vitalism is never straightforward because no matter how carefully we automate our experiments, at some point a human observer must be brought in to verify the results and this immediately opens the way to a counter-interpretation in terms of a psi experimenter effect along the lines of the observational theories. Yet the problem is not, in the last resort, insoluble. If even one definite psi effect could be discovered using plants or simple organisms as the psi source such an effect should be highly stable since biological specimen, unlike temperamental human subjects, ought not to vary very much either from one occasion to another or from one specimen to another.

Finally, let me assure the reader that, in offering these comments inspired by the Editor's original question: who is a parapsychologist ?, I am emphatically not making a plea to relax our hard-won standards of evidentiality. My purpose was to see whether in the present state of knowledge a line could be drawn between the legitimate province of parapsychology and the much wider field of the paranormal. In particular, I wished to alert some of my fellow parapsychologists to some of these more fantastic

claims we have been hearing recently from this more extended domain which I, in company with so many other parapsychologists reared in the scientific tradition, have hitherto preferred to ignore. Not, let me add, with a view to accepting necessarily any of these claims but at least with a view to maintaining a watching-brief on such evidence as may appear rather than treating it, as too many straight scientists still treat the parapsychological evidence, as beneath our notice.

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THE OBSERVATIONAL THEORIES: A PRIMER

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INTRODUCTION

Psi effects are distinctive because they are anomalous in terms of known physics; nonetheless until recently testable theories in parapsychology were largely restricted to those dealing with the psychology of the phenomena. However some recent attempts have been made to attack the central mysteries with the conceptual tools of theoretical physics. A number of the theories which have aroused interest share certain common features and they have been collectively dubbed "observational theory" (Schouten, 1977). The psychological theories deal primarily with such questions as whether personality differences or the induction of altered states of consciousness have an effect on the results of psi tests. The observational theories, primarily, attempt to answer such queries as: "How (if at all) does psi depend on space and time?"; How are ESP and PK related?"; "Can large-scale psi effects be produced by adding together many little psi efforts?".

The observational theories have been expressed in technical mathematical form. An unfortunate consequence is that the theories have remained largely inaccessible. The primary function of this paper is to render the ideas of the observational theories more accessible to the experimental workers, who alone can put them to empirical test.

While the observational theories share common features, the differences between them are quite as striking as the similarities, thus it will be necessary, even in this introductory sketch, to examine them to some extent separately.

Of the several I discuss primarily the principal two, Evan Harris Walker's theory (1974, 1975, 1976, 1977a) and Helmut Schmidt's mathematical model (1975a, 1975b). Mentioned in less detail is Donald and Martin's thermodynamic theory (1976): some of the ideas of

Lucadou and Kornwachs (1977a, 1977b) are also briefly described. While not discussed here, Hasted's (1977) use of a modified version of the "many worlds" interpretation of quantum mechanics and Mattuck's (1976) theory are also of interest. Stanford's (1978) "conformance behaviour" model shares virtually all the features of the observational theories. However, it differs in one crucial respect (Stanford, personal communication), namely in his model observation of the result by the subject is not necessary for psi to occur, in contrast to the observational theories (see later).

The observational theories range all the way from purely phenomenological description up to fully-fledged theory. In this paper the theories will be examined under three headings(a) the theoretical background (b) the model which prescribes how to apply (c) the mathematical formalism. It is at the level of the model that the main similarities lie; so I shall deal with this first in a general way.

The Model

The model can be subsumed under three headings:

(1) Feedback
(2) Time-displacement PK
(3) Pure Chance

I shall examine each of these separately in the context of PK and then discuss the more general unitary model encompassing ESP.

Psi effects are claimed to differ from more ordinary physical phenomena in their disregard for factors expected to be crucial, such as space and time: thus success rate in a telepathy experiment is nearly, or perhaps as good when subject and agent are at opposite points of the globe, as when in adjacent rooms. That psi behaved particularly oddly with respect to temporal ordering was suggested some decades ago by the success of precognition experiments, in which the targets to be guessed were not determined until after the guesses had been made. It appeared that the future must here influence the past, contrary to the usual past to present flow. This raised two particular problems (a) cut adrift from safe anchorage just how could parapsychologists proceed to deal with the phenomena: could some alternative ordering principle be found to substitute for our lost causality? The second difficulty was (b) selectivity: it had been clear for some time that a problem existed, for how could the subject guess through an intact pack of cards in a clairvoyance test with the same success rate as if each card were separately exposed: just how could he distinguish

between the target card and an adjacent one? With the realization that he could just as well guess the card that would be there tomorrow, or next week, the problem multiplied. No wonder that faced with these formidable difficulties many eminent parapsychologists of the period threw in the towel and declared psi "non-physical".

For some decades the search for an ordering principle for psi effects languished and an alternative approach was tacitly adopted by the majority of parapsychologists. On this view the subject could potentially access any event in space and time by ESP and he did in fact scan this extended environment for events of importance to him (e.g. Stanford, 1974). It was hoped that owing to the unknown features of ESP-scanning the information-processing demands of such a system would be much less than naive analysis would suggest. However, even if this postulate is accepted such a view poses large problems, for with such a powerful scanning mechanism at S's disposal it seems somewhat absurd that we should expect to learn anything about psi: how could we distinguish whether an experimental manipulation had an effect per se or whether the subject merely scanned to find out the experimental hypothesis and then produced data to confirm (or refute) it?

Unless the subject entered into a "gentleman's agreement" only to use his psi where he was meant to, then our elaborate control conditions would be in vain. It seems that such an unlimited psi must for ever be condemned to being a mere laboratory curiosity since we could never find out anything more about it than its existence.

(1) Feedback

The possibilities for an alternative ordering principle are quite strict (a) it must allow for aberrant temporal ordering (b) there must be some means of "focussing" psi and (c) it must limit the ambit of psi so that it cannot just "strike out of the blue". In 1972 Harris Walker outlined the first observational theory, in which feedback served as the pivotal point for a proposed alternative ordering scheme. The model,which at first we limit to PK,is quite simple, if a bit startling at first sight: the idea is that only otherwise random events (Section 3) about which the subject later receives normal sensory feedback (observes) can be subject to PK influence by him. In order, then, to succeed in a PK experiment (or an ESP one, as will be shown later) the subject must at some time find out how he has scored; however (leaving the psychology aside) it can be just as effective if he is told immediately or in say 20 years time. On this basis, the ambit of PK is well curtailed, for it can only affect events which the subject later observes.

(2) Time-displacement PK

Since the process which generates the targets must precede the feedback it is tempting to characterize psi as involving something like time-reversed causation. The subject's later sensory contact with the targets acts as the physical connection necessary for him to affect them at the earlier time when they were generated. Thus, in a PK experiment, what the subject is wishing for while the RNG is working is quite irrelevant to the outcome: what he wants when he is actually seeing the results is all that matters. On the view of the observational theories it does not matter if the subject knows the targets to aim for at the time the RNG is working; thus all PK is like blind PK. Eventually the subject does see the targets (or alternatively the hits) but only after the data are scored. On this viewpoint the subject actually performs his PK after the experiment has ended on the traditional view. The observational approach stands in marked contrast to the two step mechanism of the "scanning" theory of blind PK in which the subject must find out about the target by ESP before he can alter it by PK. The similar scoring rates generally observed in practice under the two conditions (Stanford, 1977, 338-340) supports the observational theory rather than the two step process, which might be expected to be less efficient than direct PK.

The theory goes further than allowing abberant temporal ordering: it requires that all types of psi involve such a quirk, even those we had formerly thought of as cozily contemporaneous. Taking this kind of theoretical idea seriously, another pioneer of the observational theories, Helmut Schmidt, set up two kinds of PK experiments. In the first kind the subject tries to affect some psychologically appealing display, for example he may try to make the pitch of a tone (which is determined by the outcomes of an RNG) rise just by thinking about it. In the second the feedback display is (usually unknown to the subject) coupled not to a currently active RNG but to a recording of the outputs of such a device, made at some previous time. The recording is made in some automatic way so that the subject is the first living being to "observe" the tones. Under the latter, apparently ludicrous "time-displacement PK" conditions scoring has been obtained at comparable rate to the contemporaneous version (e.g. Schmidt, 1976). This kind of experiment again adds to a unitary view of what seem initially to be quite different phenomena.

The problem of selectivity with which we started is no difficulty on the observational view since the subject is connected via the feedback to only one particular event at a time, on which he may exert his PK. Although we have characterised psi as involving something like time-reversed causation we should be wary of asserting that this

type of effect is entirely analogous to normal causation and should not be surprised if in some respects they are quite disparate.

(3) Pure Chance

The remaining basis of the observational theories to be discussed takes the form of an additional constraint on what can be effected by PK and may provide some insight into the nature of the PK process. Modern physics takes the position that the future state of some systems cannot be predicted even in principle given all the relevant data about the current state of the system. This is true of quantum systems, where the final uncertainty arises from the theoretical limits in the precision with which the current state can be measured (Heisenberg Uncertainty) and also for macroscopic systems driven by such quantum systems e.g. a Geiger tube monitoring a radio-active sample. On the other hand, in many of the systems we measure and ascribe to chance, for example the number of cars driving past a given point in 10 minutes, the term chance covers apparently only our disregard of complex but perfectly causal processes. Walker has shown, however, that in the classical dice-throwing PK test of the placement type, the process (which we would normally consider as being perfectly causal, although we should be unhappy at having to work out the moment-by-moment position of the dice) is in fact dependent on the Heisenberg uncertainty in the initial position and momentum of the dice, which grows enormously with the number of bounces. In other words even in such a macroscopic system, fundamental quantum uncertainty enters to some degree and to the extent it does the outcome is inherently unpredictable. Walker points out that statistical PK-testing situations use just such test procedures. Walker, indeed, takes the view that all true (as opposed to apparent) chance events in the world are ultimately traceable to quantum effects. However that may be, the observational theories all take the position that only pure-chance events can be affected by PK: thus, one can affect radio-active emission but not the outcome of an ideal computer-generated pseudo-random sequence, or its "hardwired" counterpart, a completely deterministic physical system. (Of course, in practice, no computer-generated pseudo-random sequence is ideal since its starting point may be decided by processes including chance events, similarly any physical process has some imperfections; thus in the real world the effect of PK on such systems will not be entirely zero but should certainly be considerably smaller than for the corresponding pure-chance process). This is in marked contrast to the traditional view of PK as a force:

a force is governed by no such contraint and can, of course, affect a purely deterministic system, such as a computer, just as well as it can a pure-chance process. Furthermore if it is true that there is a high proportion of pure-chance involved in many macroscopic uncertainties we have the suggestion, at least, that in a statistical sense buses (by virtue of their inherent uncertainty) should be affected by PK as well as electrons. As a spectacular example we may imagine that the bus driver was struck by a single cosmic ray (a pure-chance event) and developed cancer: his driving is erratic because of consequent poor health. We may, of course, conceive of many other much more probable pure-chance contributions to the uncertainty in bus timing.

The Unitary Model

So far we have only considered PK. Here, as we have seen, the observational theories give, for example, the picture that the subject hears the tones which were generated by a pure-chance process some time before. At the moment of feedback he causes the RNG at the time of generation to become biased in such a way that he hears more hits. It is one of the great strengths of the observational theories that by the addition of only one plausible postulate, namely that the brain contains at least one pure-chance process which determines, in part, the subject's guessing behaviour, that the cognitive forms of psi can be readily (and quantitatively) assimilated to the same model. Suppose that a subject performs a card-guessing test: after each trial a computer tells him whether or not he was right. If the computer generates the targets in a deterministic way then these can not be affected by PK; so that to secure correspondence between target and guess and hence to be informed at the time of feedback that he had a hit, the subject must direct his PK onto his own "internal RNG" which determines his choice at the time he made his guesses. To put it another way, ESP is simply a special case of time-displacement PK in which the subject's earlier guessing behaviour plays the part of the RNG.

The observational theory view of ESP is thus very different from that implied by the scanning model: large parts of the brain actively devoted to scanning all existence are no longer necessary: instead we require only a hypothetical PK effector system, the function of which is to modify pure-chance probabilities, in addition to the ordinary sensory and motivational cerebral machinery.

Theoretical background and Formalism

Here I shall treat the theoretical backgrounds quite sketchily, since from the point of view of the experimental parapsychologist, once derived the theories make little or no further reference to the ground from which they sprang.

Walker's Theory

Walker's theory has undergone considerable modifications in the course of its development. His most current views are expressed in an as yet unpublished paper (Walker, unpublished) in which he attempts to eliminate some of the ambiguities of his earlier work: the present summary is based largely upon this exposition.

There are a number of suggestive analogies between quantum effects and psi phenomena. One might for example consider the tunnel effect, in which a submicroscopic particle can disappear from a closed container and re-appear outside, to be a prototype for teleportation. Walker takes the view that psi phenomena are basically large-scale quantum effects. While quantum phenomena are generally noticeable only in the microscopic world, under some special circumstances, particularly at extremely low temperatures, notable macroscopic events occur. For example the superfluid helium II (at about 2 degrees above absolute zero) has the disconcerting habit of running spontaneously up the walls of its container and escaping. A number of similar phenomena, such as the abrupt loss of electrical resistance shown by some metals at very low temperatures (super-conductivity) are well known.

Quantum theory is currently in a process of ferment: while yielding effective algorithms to compute experimental results, at many points the underpinning of the structure is being questioned. There can be scarcely a university without some physicist with an unconventional interpretation of the formalism. Walker's approach to psi phenomena is via a problem (the measurement problem) which has long troubled philosophically-inclined physicists. The formalism of quantum theory yields results in terms of the state vector (Ψ). Of course, in real life we do not see state vectors but instead experience events. In these terms the state vector represents all the possible states that may occur together with their associated probabilities; but it does not tell us which of these will actually happen. According to quantum theory the selection comes about by pure chance depending on the probabilities for each possible state. Stated crudely, the measurement problem is how the state vector becomes an event when it is measured. Numerous solutions have been proposed: the most widely accepted of these is that a thermodynamically

irreversible transition occurs when the microscopic system interacts with the measuring instrument; thus on the conventional view the state vector is collapsed to an event long before it reaches the macroscopic world. Walker, however, criticises this idea as being arbitrary, grafted onto quantum mechanics, rather than being derived from it. He goes on to suggest that the state vector is not in fact collapsed at the microscopic level but can continue into the macroscopic world. Walker boldly suggests the kind of process (a measurement process) which can then collapse the state vector. This is not the place to discuss the idea in detail but in general terms collapse takes place only when two states resulting from the same quantum event are compared. Walker designates this a loop interaction and defines the measurement process in mathematical terms. According to this view any apparatus whatever which performs a loop interaction can reduce the state vector: one such device he asserts, which is constantly performing loop interactions, is the human brain.

Walker has previously proposed a substantial theory of synaptic transmission (Walker, 1977b), which differs from the various ideas previously proposed in that it involves quantum mechanical tunnelling at the synaptic cleft. This theory is ingenious, plausible and testable. If the experimental investigation which this theory appears to deserve is undertaken we should shortly know for certain if it is correct. He extends this to an even more speculative scheme involving the quantum mechanical coupling of large regions of the brain by the mechanism of hopping conduction of electrons via "stepping stones" of soluble RNA molecules and considers that this constitutes the physical basis of conscious awareness. The purpose of this extension is to provide an explicit mechanism (which will not be detailed here) for the proposal of Eccles (1960) that a small "mind" influence on a single synaptic knob might be sufficient to produce macroscopic results on brain activity. Walker points out that the brain activity affected may be that associated with observing different outcomes of the event: furthermore, because this activity e.g. seeing the dice in a given position, is part of the loop interaction collapsing the state vector, this can "pull into line" the as yet undecided external world, in just the same way as the external system can pull into line the observer's brain state (perception). According to Walker's treatment the observer's brain and the external world are coupled via his senses so that the two constitute one quantum mechanical system: if the set-up is described by "hidden variables" as Walker did earlier, the observer and the external system share certain of them.

While Walker would, I think, be willing to concede the extremely speculative nature of the postulated neurophysiological mechanisms

he would claim to have demonstrated one conceivable way an observer could control an observed pure-chance process in the external world, given that he can control by PK a single electron within his own brain. Even on conceding this, however, I am by no means convinced that Walker says anything about how that single electron is affected, beyond simply postulating that it is.

In summary, on the basis of Walker's theory someone observing the outcome of an external quantum process is not a mere passive onlooker but actively assists in the collapse of the state vector. When the observer has no control over his internal mechanisms which are coupled to the quantum process or he has no particular desired outcome, things proceed according to the classical prescription: if, however, one of the possible states of the system involves pleasant stimulation e.g. a bank note being dispensed and he has some control over his internal systems, then he will tend to select the pleasurable potential state as the one to be actualized. Similarly if the possible states of the system include an unpleasant one the subject will make sure it does not come about.

Walker's theory comprises a large number of ideas, some likely, while others seem quite implausible. At least three sub-theories are involved (1) the physical theory (2) the neurophysiological theory and (3) his phenomenological treatment (below), most important from the parapsychologist's point of view. Each of these topics requires extensive theoretical and empirical testing: it is to be hoped that they will receive such examination. The general area of the measurement problem certainly seems a good place to look. If we take the PK experiments of Schmidt at face value as showing PK influence upon quantum processes then it seems we are virtually forced to take some such line. It is worthy of note that Walker has derived all 3 basic items of the general observational model from theories (1) and (2) and has not merely arbitrarily adopted this picture.

Walker's phenomenology, itself only rather loosely grounded on the above theories, is based on information in the sense of information theory. He advances the postulate that the subject can, within the context of the observational model, affect a given random system by adding a constant amount of information to it per unit of (effective) time. In other words, given the same amount of PK "effort" regardless of whether, for example, the subject works on a single target for 10 minutes or makes 100 trials in this time the result will have approximately the same probability of arising by chance. Furthermore the level of significance should be the same regardless of whether the target system is falling dice or a Schmidt

machine etc. Thus for Walker the significance of a result is independent of the RNG and even of the a priori probability of a hit: it does, however, depend critically on the length of time the subject spends; if, for example, he takes four times the (effective) time per trial the result should have twice the Z-score (and above-chance scoring rate). The problem lies in the specification of what part of the time the subject spends in the experiment is actually effective time: is it how long he spends thinking about the experiment, or the time his brain spends processing the feedback signal or the period of elation consequent to seeing a hit? The theory does not specify the answers. We can, of course, artificially cut across these distinctions by making all trials psychologically identical and, unknown to the subject, playing some trials back several times.
We should, however, be very wary of using the time factor in an ad hoc way. From the postulate of constant information rate alone most of the testable consequences of the theory flow. To calculate the relative size of PK effect a subject may produce on different systems it is necessary only to evaluate the states with equal quantum mechanical probabilities. In particular Walker is interested in PK effects on analogue systems such as thermistors, magnetometers etc., rather than the behaviour of discrete sources of randomness.

A further postulate is required for more than one subject working on the same PK targets: Walker proposes that the total amount of information expended is equal to the sum of that for each individual working alone. This corresponds to the above-chance scoring rate being the square root of the sums of squares of the individual contributions.

Schmidt's Model

While Schmidt's thinking clearly springs from the same general quantum-mechanical world-view, in particular that there is something odd about pure-chance happenings,he specifically forgoes any reference to microscopic mechanisms in favour of a concise and intendedly relatively unambiguous mathematical formalism which is free of those flights of fancy, which at once adorn and disfigure the work of Walker. In contrast to Walker, Schmidt simply assumes the three basic ideas of the observational model ready made and makes no attempt to relate them to the rest of physics. Schmidt argues forceably that there is little point in speculating about microscopic systems so long as we are unable to observe psi in such systems: there is no way we can test our deductions. He points out, however, that in the meantime we can occupy ourselves

usefully in trying to find out how psi-like effects can happen consistent with the world we know: he therefore proposes a formalism describing a world with psi-like features. This is put forward in a thoroughly provisional way, in full knowledge that it is only a basis for discussion and may bear merely a superficial resemblance to real-life psi. His hope, however, is that the formalism is sufficiently general that it will point towards critical areas in the psi process and raise new questions which can be answered empirically.

Schmidt's model is geared specifically to discrete sources of randomness and psi sources (idealized PK subjects). His primary idea is that the PK effect which can be exerted on a single trial is independent of how that trial is produced: regardless of whether it is determined by radio-active emission or by sampling a noise diode 1000 times or whether indeed it is the result of a whole experiment the same scoring rate is to be expected, providing these trials can not be sensorially distinguished. He does, however, (unlike Walker) consider that the scoring rate depends on the a priori probability of a hit. Schmidt starts with the self-evident assertion that PK increases the probability of a hit from p to p^1 (and the miss probability changes from q to q^1). He proposes that the size of the effect is defined by the following simple equation:

$$\frac{p^1}{q^1} = \frac{p}{q}\,\Theta$$

where theta is a constant representing the strength of the psi-source, greater than one for psi-hitting. This means that the most efficient p value should be around 0.5, the Z-score decreasing as p deviates from this: thus only a small PK effect should be evident using target events with, say p=1/100 if Schmidt's model is correct in this respect.

It should be noted that in a real experiment a hit does not necessarily signify, say, a head but might for example be five heads in a row or a variance increase or scoring very close to MCE etc. The hit definition is dependent on the experimental set-up and upon the subject.

It turns out that on solving the above equation for p we find:

$$p^1 = \frac{p\Theta}{p\Theta+q}$$

There are only two possible futures on each trial, a hit or a miss: a glance at the above equation shows that there are two terms, one $p\Theta$ and the other just q. We may consider these terms as "weightings" proportional to the PK-changed probabilities of the two possible futures, the desired and indifferent state respectively. We can make intuitive sense of this by supposing that the desired outcome p "fires" the psi source and receives the augmented weighting p_Θ : the miss signal has no effect at all on the psi source and its weighting remains equal to q. The psi enhanced probability of a hit p^1 is now given by the hit weighting $p\Theta$ divided by the sum of the weightings for the two possible states $(p\Theta+q)$. This general scheme works out also for more complex cases and thus provides an elegant method of computing results by breaking down a complicated example into a simple combination of the states for all possible futures.

Like Walker, Schmidt suggests a combination rule for psi-sources working on the same targets. Maintaining the weighting scheme described above, he proposes that the future branch in which a number of psi-sources "see" a hit should be enhanced by all of the thetas: for two sources Θ_1, Θ_2 the hit weighting then becomes $p\Theta_1\Theta_2$. This corresponds nearly to addition of the above-chance scoring rates of the respective psi-sources.

Schmidt's strategy is to build as few pre-suppositions as possible into his formulation, which is used primarily as a basis for experiment. This can be expected to modify both equation and model and once the formulation has reached a state of tolerable agreement with empirical findings then an interpretation should follow naturally from it.

Donald and Martin's Thermodynamic Theory

Donald and Martin tackle the problem from yet a different direction. As their point of departure they seize upon the causality-reversal features of psi effects. How, they ask, can we build reversed causation into our picture of the world? For organisms probably the most important aspect of time is that defined by the Second Law of Thermodynamics: as time increases the entropy (disorder) of the universe as a whole increases. Thermodynamics can now be largely derived from a consideration of the behaviour of the constituents (statistical mechanics) and within this scheme the favoured (though not the only) explanation of the origin of the Second Law is that there exist boundary conditions on the past of the universe but not on the future i.e. the past was a highly unusual, highly ordered state.

Donald and Martin have little difficulty in extending thermodynamics to the case in which there are boundary conditions on both past and

future. An important feature of the way they do this is that they do not need to know all the relevant laws of Nature but they make inferences directly from observed transition probabilities only. Ultimately they derive two different kind of entropy, one the normal kind, future-directed entropy, and for our purposes past-directed entropy, that involved in psi effects. Having derived this symmetry they have to explain why so little of the past-directed entropy is in fact observed: many possible explanations exist, for example in an expanding and contracting universe we may be near the beginning of the expansion where little "pre-effect" is yet in evidence. Organisms typically make use of the order of the past to affect the future e.g. I eat bacon and eggs so that I can write this paper. The postulate is made by analogy, that organisms have developed the ability to make use of the order of the future (which they concentrate from the infinitesimally small amount in the environment) and use it to affect the past.

If this seems an incredible scheme it is because it tells us nothing about mechanism: this is both the strength and weakness of the thermodynamic approach. While the future-directed entropy increase seems reasonable enough because I know something of the ways in which food is metabolized etc. it should be emphasized that such knowledge is by no means necessary and that in fact for some decades thermodynamics yielded answers in physics which were not then understandable in microscopic terms: it may be hoped that the situation will be similar with this formalism. While the importance of the theory is much greater than the limited use I shall make of it here, I wish to make only one point. Although starting from a completely different viewpoint and apparently never having heard of Walker's work, the equation these theorists derive is identical (though couched in terms of entropy) to that of Walker (the subsidiary assumptions required to apply it to experiments are different, however). While it would be foolish to interpret this as supporting the mechanism proposed by Walker, it does, I submit, increase our confidence that on a phenomenological level some equation of this general form should apply, rather than the formalism of Schmidt which has despite its elegance, considerable internal difficulties (this will be discussed in another paper).

Some consequences

The theories make many predictions for experiments, or perhaps more precisely they raise many interesting suggestions for experimental investigation. In particular, the validity of the general observational model requires much attention. The most contentious feature is undoubtedly the pivotal role of feedback.

Is feedback actually the one necessary physical condition which connects the subject to an event somewhere in the world out there? Alternatively can PK simply strike anywhere: can I now interfere with D.D. Home's levitation just by thinking about it? While nature may have arranged it like that, this would be unfortunate for parapsychology, since it would seem virtually impossible ever to control a phenomenon which can strike without warning, any place, any time. Whether or not feedback should prove to be the key, the observational theories should at least stimulate experimenters into asking "What kind of link (if any) is necessary between subject and system for PK to occur?".

If it is true that if a subject never learns of his success then he can never succeed, it follows that one should not be able to precognise anything happening after one's death. If a system has more than two possible outcomes e.g. a ball bouncing down a dividing 3-output tube and a PK subject is informed on each trial only whether the outcome is output A or not, he should not be able to affect the B/C ratio by PK, in marked contrast to a force-type model (Schmidt, 1975b).

At this point it is useful to define what is meant by feedback in the context of the theories: does it need to be trial-by-trial or will a total score do or could even telling the subject "you did well" prove enough? Schmidt's model gives a clear answer on this point - feedback must be trial-by-trial: although the total score could be affected it would in Schmidt's model constitute effectively a single trial with $p=0.05$ being the definition of a hit. Under this model a subject could have little effect on the total score. Under Walker's model, however, the subject may choose not to spend his information bit-by-bit but may save it up till the end, when he can expend it on the total score (run-score targeting). Because the same amount of information is used in both cases the significance of the results should be about the same. In the latter case, however, the subject can not affect the distribution of hits within the experiment, so no position effects should be evident. The prediction of Walker's theory for being told only "you did well" (if the result is significant) is not so clear but it is likely that a subject could achieve little under these conditions. With a strong psi-source, then, it seems the question of the importance of feedback could easily be answered. In general, however, psi is not unequivocally associated with the subjects, and other persons such as the experimenter, who do get feedback, may be the actual source of psi; thus the question becomes greatly complicated and at the least it will be necessary to manipulate the feedback available to the experimenter as well as to subjects

in order to have any chance of reaching a conclusion. Some preliminary experimental work has been done by Braud (1978) among others. The results suggest that PK can still occur under conditions where neither subject nor experimenter get trial-by-trial feedback, but only total scores: the prediction of Schmidt's theory that trial-by-trial feedback is necessary has therefore been falsified. As Braud (1978, p153) comments "What is needed now is a series of parametric studies... (to)...help delineate the precise role of feedback in psychokinesis".

A major consequence of the proposed pivotal role of feedback which does not seem to have been generally realised is that if this is true, virtually all our investigations up till now have been looking at the wrong place! As we have seen, the triggering of the subject's psi source occurs at feedback, which has traditionally been seen as extraneous to the experiment proper. Consequently it has been a somewhat haphazard operation, occurring perhaps long after the experimental sessions and often quite outside the control of the experimenter. On the basis of the observational theories it would not, thus, seem surprising that our efforts to control the phenomena have so far proven largely ineffectual. Typically we have looked at or manipulated the subject's psychology and physiology when he is guessing the targets or willing the RNG. On the observational model, nothing at all of interest is happening in the PK subject at that time and all we would expect of the ESP subject is that he makes of himself an efficient RNG. What would we find if we ~~we~~ were to look at the subject when he is actually performing psi at the moment of feedback? What if we manipulate his mood or alter his expectation at that time? Some preliminary work has already been done along these lines (Broughton, 1977) with suggestive results.

The theories predict that scoring rate is independent of the complexity of the pure-chance RNG. In practice Schmidt has compared simple and complex RNGs and found no significant difference in scoring between them (Schmidt, 1974).

According to the observational theories PK can work only on pure-chance systems. In terms of hardware, tumbling cubes can be influenced much more than a smoothly rolling sphere: similarly a pure-chance RNG should be affected more than a computer-generated pseudo-random sequence, of which only the entry point may be determined by pure chance. Stanford (1978, p206) cites supportive experimental data for mechanical systems.

The space-time independence of the models led Schmidt (1976) to try time-displacement PK in practice. The technique of recording RNG outcomes on cassette-tapes and then sending them to subjects by post (thus giving them a "psi-channel" straight to his laboratory) promises to be a valuable one.

Walker makes a number of retrospective calculations for particular experiments. In each case they seem rather unsatisfactory. For example, he calculates the distribution of ESP ability in the population and compares this with a sample of about 1000, culled from the literature, finding at least suggestive agreement. We know well enough that to be published in the early literature results had to be significant and we may reasonably consider that then, as now, there were more insignificant experiments than significant ones, thus the results are not likely to be typical of the general population. Secondly, we know that the role of the experimenter is as critical as the subjects: one experimenter may obtain significant scoring from most groups that he tests, while the majority simply confirm the binomial theorem. Since Walker takes no notice of this in his calculation I cannot take his result very seriously.

The best of Walker's results concerns the dice placement PK tests of Forwald: he finds excellent agreement for different types of dice between the calculated and actual size of results. This is, at first sight, most impressive but when we realize, as Walker himself tells us, that "The data obtained are unique in that physical conditions of the cubes were shown to have important effects on the magnitude of the phenomena" we may find ourselves wondering whether this dependence was merely the result of Forwald's psychological expectation. This opinion is rather reinforced when we find out that the determination of physical dependence was Forwald's intention in these experiments and that the data fitted his own theory rather satisfactorily too.

Walker also examines the experiment of Schmeidler with Ingo Swann in which he tried to influence the recorded temperature of an isolated thermistor. He finds adequate scope within his theory to allow the size of effect found: this is rather a pity since the present author has demonstrated that these results were more likely due to use of inappropriate statistical evaluation than to PK (Millar, 1976).

However, it should be pointed out that the retrospective analysis of experiments is a very blunt tool indeed in parapsychology. In work which has not been specifically designed to test a given question it is most unlikely that it will be possible to interpret the result unambiguously. The only safe way is to make the prediction before running the experiment and to his great credit Walker has attempted to do just that: he makes the forecast, for example, that it will be easier to produce a spurious spectral line by PK than to deflect an existing one.

One experiment of Schmidt's (1973) is worthy of particular note. His model does not predict any decrease in scoring rate as the

feedback rate increases, whereas Walker's does. Using as feedback rates 30 and 300 per second Schmidt found a significant decrease in scoring rate at the higher speed: however, the Z-scores (and corresponding efficiencies per unit time) were not significantly different. Certainly, this seems to support Walker's theory rather than his own, though it could be argued that Schmidt's model as it stands does not take into account the known limitations of the sensory channels at faster speeds.

Not only does Schmidt propose a clear scheme for the relationship between PK and ESP he gives us detailed instructions for finding out if, in fact, the different kinds of psi are related in the way suggested. All we need to do is to use the correct physical set-up (without changing the subject's psychological set) and we should be able to produce precognition from PK and vice-versa. Indeed, we should be able to "synthesize" any kind of psi we like, given a psi-source of any apparent type, if Schmidt's model is actually correct in this respect.

An experiment of this type (Schmidt and Pantas, 1972) converted precognition into PK. Subjects predicted which of four lights was to come on next by pressing the appropriate button. Unknown to them, however, when the RNG produced, for example a 2, lamp number 2 was not activated but instead the bulb 2 to the right (1 2 3 4 1 2 3...) of the button pressed was lit. Hence, under this condition a hit could be obtained only by causing the RNG to produce 4 s, even though to the subjects it appeared the same as a precognition test. Similar scoring rates were obtained under the two conditions.

One of the most exciting consequences of Schmidt's model is that it predicts a linear increase in scoring rate if PK targets are presented (unknown to the subject) more than once, for example if the same targets are presented four times the scoring rate should be about 8 % above MCE if the corresponding figure for singly presented data is 2 %. Walker's theory also predicts an increase but not so much: in this example the repeated targets should have a 4% rate. Schmidt (1976) reported an experiment with significantly enhanced scoring under these conditions, though others (Millar and Broughton, 1978) have been unable to secure scoring under either condition.

As will be pointed out later this kind of experiment is important not only for practical reasons but is also crucial for theoretical understanding.

The Experimental Control Dilemma

The space-time independent formalism, which is one of the strengths of these theories is also nearly a fatal weakness, for if we take

them seriously we must in our experiments take into account not only the nominal subjects, experimenter, experimental assistants etc. but also anypne who sees the journal report at any time (or perhaps even anyone who may see the report). It is clear that to make the observational theories workable some factor(s) in the real world must in some way limit the situation to that we actually see, namely that only the few people involved in running the experiment or checking the data have a measurable effect upon it.

Schmidt (1975b) demonstrated one facet of the problem in an explicit example in which a large number of later observers with no psi ability on average nevertheless could be shown to "wash out" the scores a psychic would have attained without their attentions. Houtkooper (1977) extended this analysis and showed that the problem is that the variance of the scoring rate increases with an increasing number of later observers. Walker (1977), however, calculated the actual magnitude of the "washing out" effect and found the number of later observers would have to be in excess of a million to wipe out the effect of a psychic. Thus he concluded "there is no observer divergence problem but rather an observer divergence effect which probably can be fruitfully studied". Walker, however, completely failed to realize that the point at issue here is not the particular example but whether parapsychologists can conceivably exert the kind of control that is necessary for parapsychology to become a science. Schmidt's example is limited to only small effects from later observers but consider the ludicrous situation if a really strong psychic got out of bed on the wrong side: he could wreak havoc on all the work reported in the journals he read that day!

Schmidt (1978, 1977) attempted to show that the problem did not arise if the possibility were taken into account that subjects selected themselves into experiments by use of psi: this solution, though, seminal, did not prove durable (Millar and Hartwell, 1978). It seems that within the current formalism there could be no way to eliminate interfering effects: there could be no such thing as an isolated system. Mathematically, the problem primarily arises, as Houtkooper showed, because the variance "takes off". The reason for this behaviour is that the psi contributions from different observers augment one another in both Schmidt' and Walker's theory.

The problem reduces to one of modifying the formalism so that contributions do not augment. The simplest (if most empirically implausible) way of doing this is to assert that only the first observation has any effect. Hartwell (1977) proposed that the system becomes gradually less and less susceptible to PK influence

with each successive observation. While reasonable, this solution seemed rather arbitrary and Millar and Hartwell (1978) suggested instead that the problem be tackled at source and the combination theorems for psi-sources should be changed to a non-augmenting form. On doing this it appeared that the imperfections (noise) inherent to all feedback channels are adequate to limit the ambit of psi, even though it may be of its own nature completely space-time independent. It is interesting to speculate that the virtually undegraded feedback of the results generally received by those actively engaged in an experiment may be the fortuitous cause of their large contribution to that result. If so we can show how to use the limitations imposed by the feedback deliberately to create a virtually isolated system. While this idea looks promising it is still in the course of development and has not yet been put to experimental test.

Another possible solution to the problem has been put forward by Lucadou and Kornwachs (1977a,b) within the context of their system theory approach. Their fundamental idea is that in addition to quantum uncertainty there is another kind of uncertainty present in a complex system (such as a human being) simply due to its complexity: this latter is involved in psi events. If I understand their position correctly a consequence is that the perception of patterns on the data by the observers and their memory of these patterns may be crucial factors in PK. Consider a PK experiment in which a subject tries to enhance the generation rate of heads: as he sees the result of each trial he may effect it by PK: all of its uncertainty is removed by the observation. As long as he remembers an individual trial a later observer can have no further PK effect upon it. Soon, however he ceases to remember the trials individually and retains only a more general idea of his scoring rate. The system regains some of its uncertainty: this means that it becomes again subject to PK influence. However, so long as the first observer remembers his overall scoring rate the second observer cannot affect that but can introduce any other pattern into the data, such as a decline effect, just so long as the first observer is not aware of the decline: a third observer could introduce a variance effect and so on. These workers suggest that the result of Schmidt's (1976) psi enhancement experiment are to be interpreted in this way: the overall scoring is due to the subjects but the excess of hits in the repeated trials as compared to the singly presented ones is attributed to Schmidt. This approach is an ingenious one, though surprisingly the idea does not seem to have been formalised. Needless to say it is wide open to experimental test.

Since all of these ideas for solution of the experimental control

dilemma involve limitations on the combination of contributions from different observers an examination of the combination of psi-sources in general and the psi-enhancement paradigm in particular becomes of great theoretical importance.

Observational Theory and the Cybernetic View

The traditional view of psi has been basically a cybernetic one, in which ESP acts in a similar way to the senses and PK like the motor systems. For example, if a subject is asked to make a die come up 6 by PK we would imagine him monitoring its fall by ESP, catching it in a suitable position and giving it the right size of mental poke to make it come to rest with the 6 uppermost. While plausible with dice, if a Schmidt machine is used instead we must imagine the subject keeping track of an oscillator which works perhaps three orders of magnitude faster that the brain. Stanford (1978) has devoted many pages to discussion of the weaknesses of the traditional view and Beloff (1978), not without reason, has stigmatised the cybernetic model as "grossly extravagent". We have seen, on the other hand, that on the observational theories the whole thing is worked by the feedback: there is no feverish working of masses of neurons, just a firing of the hypothetical PK effector system by feedback events. In a real sense this system is "goal-oriented". The jump from the cybernetic view to that of the observational theories is, however, a wide one and one which is difficult to make. Theoretical discussion has been clouded because of failure to make the distinction (e.g. Isaacs, 1977).

In some sense PK may be regarded as equivalent to an entire normal feedback loop. Consider a mouse in a learning experiment: first a lamp glows and then a few seconds later he receives an electric shock when he goes for food. After many repetitions the animal learns that when the light is on he will be shocked if he attempts to eat; he therefore ceases trying for food when the light is on. His anticipation of the probable future event causes him not to eat. In a comparable ESP experiment it is not the anticipation but the actual future (feedback) events which affect the mouse in such a way that he does not feed at dangerous times. The controlling information comes "directly from the future" rather than from the organism's past. It is important to realize that from the point of view of Walker and Schmidt PK does not involve any necessary flow of energy between subject and object but "merely" the introduction of a correlation between them.

Experimenter Effect

A survey of the recent parapsychological literature reveals that the really convincing evidence for the occurrence of psi is largely

confined to two categories (a) special subjects with unselected experimenters and (b) special experimenters with unselected subjects. The latter is considerably greater in quantity, though it is contributed by only perhaps half a dozen special experimenters: foremost among them are Helmut Schmidt and Charles Honorton. Category (a) poses no problem, but group (b) at first sight seems anomalous; however the importance of the experimenter in eliciting results has long been noted in the literature. This has generally been attributed to some experimenters having a special psychological knack for coaxing psi from subjects, though the experimental evidence supporting this assertion is at best vestigial. It is found, moreover, that similar differences occur between the results obtained by investigators even when subjects and investigator never meet (e.g. Fisk and West, 1953). This finding seems somewhat difficult to reconcile with a purely psychological explanation.

Furthermore we have somewhat more direct evidence that experimenter-psi is involved in some instances. In an experiment Honorton reported (1976) a colleague analysed his pre- and post-experiment randomness tests and found them to be suspiciously random (for details consult the paper); they were so "good" in fact that such a happening would be expected by chance alone only once in 10,000 such experiments. This dwarfs by two orders of magnitude the significance obtained in the experiment itself.

The influence of those other than the nominal subjects on the results of psi tests has recently been the topic of extensive reviews (Kennedy and Taddonio, 1976 and White, 1976a,b).

It seems to me that the most plausible way of rationalizing these findings is to suppose that there is a certain amount of psi in the general population. Good subjects, however, depending on their occupation, may fall on either side of the fence and thus call themselves either good subjects or successful experimenters. If we are tempted to ask where our current D.D. Homes and Rudi Schneiders are, my feeling is we need look no further.

This is clearly a somewhat radical view; however we are forced to confront the issue that there is something very singular about the success of these experimenters. The work at Edinburgh by my colleagues and myself over several years has shown no resemblance to Schmidt's findings, even when we employed not only 13 different experimenters but a total of 256 subjects (Broughton, Millar, Beloff and Wilson, 1977). In the European tradition (see publication policy of this journal) the Edinburgh group is one of those few laboratories which publishes null findings and in the low frequency of reports by other workers of convincingly positive findings with unselected subjects using Schmidt-type set-ups, we see confirmation of our own null results.

In a recent review paper Honorton (1978) has examined the micro-

PK work on which the above conclusion is based. His own evaluation is diametrically opposed to mine. He points out that 35 out of 54 such experiments (a spanking 54%) have yielded significant results and concludes that this constitutes "a substantial level of replicability". While this is not the place to discuss the larger question of replicability it is nonetheless useful to point out the flaw in Honorton's argument and to illustrate the relevance of this kind of data to the experimenter effect. Quite simply he uses the wrong measure: of course Schmidt gets replicable results - merely running one's eye down his 21 studies in Honorton's list will reveal that only 5 did not turn out (a success rate of 76%). The question we really want to ask, though, is what percentage of otherwise unselected experimental psychologists would get significant results on attempting to replicate one of Schmidt's experiments. No such large-scale study has been performed; however if the hypothesis that the results of these experiments is largely the result of experimenter psi is a correct one we might expect to see a difference in success rate between those experimenters who have themselves produced significant results as subjects and those who have not. Furthermore we may reasonably consider that this latter (non-psychic) group is more typical of the general population of psychological researchers: hence we may make an extremely rough estimate of success-rate in the larger population from that of the non-psychic group of experimenters. For the psychic group, Schmidt (1973, 1974), Honorton (1976), Stanford (Stanford and Stevenson, 1972), Braud (1978) and Pantas (Schmidt and Pantas, 1972) no fewer than 33 experiments from 41 are significant (80%). For the non-psychic group, on the other hand only 2 studies from 13 (15%) turned out. As Broughton (in press) has aptly pointed out "... (While)... parapsychology ... (does not have) ... a repeatable experiment ... it certainly seems to have repeatable experimenters". The selection criterion used above can certainly be criticised as arbitrary but it is decidedly no less contentious than Honorton's tacit argument that the set of experimenters on his list is representative of the general population of experimental psychologists. In addition to the above, one of the two non-psychic experimenters on Honorton's list has informed me of 10 other insignificant experiments he has conducted: this brings the grand total for this group to 2 significant studies in 23 (9%). Far from providing evidence of substantial replicability the success rate of about 1 in 11 such experiments seriously raises the question of whether it is at all worth-while for non-psychic experimenters to conduct experiments with unselected subjects.

The general idea of experimenter psi fits rather well with the

observational theories: since the experimenter observes the scores he might be expected (if he has psi) to use his subjects' guessing as an RNG on which to exert his own PK influence. If this is true we should expect to have reports of subjects able to "transfer their powers" to others in this way. In fact the literature is replete with such anecdotes (cf. Bill Delmore's card "tricks", Kelly and Kanthamani, 1972), though (perhaps because it seemed so bizarre) this ability never seems to have been the object of experimental investigation.

More important than providing a plausible "little tale", however, the observational theories provide means to test the assertion and, at best, to partition any psi effect into subject and experimenter components, by appropriate manipulation of the feedback. Thus, the psi-based experimenter-effect theory can be compared favourably with the psychological explanation in terms of potential falsifiability.

It is important to note that this view of the experimenter effect is not shared by all observational theorists. In particular, Schmidt, while having suggested that some other work might have this explanation, is still firmly convinced that the psychological effect is the important factor in his own work. Whatever view we take it is clear that only experiment can resolve the issue.

Empirical Evidence

What empirical evidence have we about the truth or falsehood of the observational theories? Throughout this paper much mention has been made of the beautiful experimental work done by Schmidt. This, taken at face value, presents very strong evidence for his theoretical position. As we have seen, however, the specific evidence is largely confined to his work since he is one of the few able to "elicit" psi. If we take the view outlined above, namely that he is "a luminous man who sees gleams of light wherever he looks" then I fear we have to dismiss his evidence as telling us anything about the psi process. On the contrary a close examination of his work suggests to this author that it may tell us much more about Schmidt's hopes and desires than it does about psi. Any objective evaluation of the truth of the observational theories must therefore await further work.

The Limitations of the Theories

(1) The theories are not yet worked out in detail. They must be considered as merely a skeleton framework to which the structure has yet to be added. In particular, unless we can find the limitations of psi within this new framework we are in the position

of having found "the universal solvent" without the benefit of anything to put it in (the experimental control dilemma).

(2) The relationship of the kind of PK discussed here to that reported in say a poltergeist outbreak is obscure. While plausible conjectures have been presented elsewhere (Mattuck, 1976; Millar, 1975) little work has yet gone into this problem.

(3) The observational theories yield many predictions which can be tested given a source of psi. Alas, so far at least, they do not help us to find such sources. Until more than a very few parapsychologists can elicit psi with some regularity it is to be expected that progress will be slow indeed.

(4) A more general objection frequently raised against these theories is that while they describe psi phenomena, they do not in any real sense explain them. While this is quite true it is equally true for Newtonian mechanics, for example. A very important function of science is simply to describe. Whether the theories are useful (true) or not will be decided by the value they have for predicting results which were not known at the time the theory was constructed.

CONCLUSION

It may be thought by some that the tentative conclusions drawn in this paper are somewhat gloomy ones. While there can be no doubt that in some ways the observational theories are bad ones, I am myself by no means pessimistic. I have tried to give a balanced view of both benefits and drawbacks but it seems to me that the observational theories are the most exciting thing that has happened in parapsychology since Duke days. They promise, at last, to yield a picture of psi which is not only comprehensible but quantitative. Even should we find, as seems not unlikely, that much we have been measuring for the last 50 years is not at all what we thought, then at least the theories may give us ways of starting over.

I shall let Walker have almost the last word: he says: "I have equations that actually work and fit the data. That is the ultimate recourse". Whatever we may think of his own claims, can it be doubted that this attitude is a healthy one for parapsychology?

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Twice a year the Parapsychology Laboratory of the University of Utrecht publishes the European Journal of Parapsychology. The object of the European Journal of Parapsychology is to stimulate and enhance the interest within this field, especially in our corner of the world, by communicating research results and issues related to professional parapsychology. Although there will be an emphasis on experimental work, theoretical articles are also welcome. Contributions from all over the world will appear in the journal.

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HAAKON FORWALD

1897 - 1978

Mr. Haakon Forwald, of Ludvika Sweden, passed away on September 16, 1978, at the age of 81. He was recognized as one of the pioneers in laboratory studies of PK. During the 50's and 60's, prior to the introduction by Dr. Helmut Schmidt of electronic random generators into PK, Mr. Forwald vied with W.E. Cox as the most creative and ambitious worker in that field of research. There are clear-cut indications that Forwald independently of Cox, perhaps even before Cox, developed the so-called placement technique in PK-testing. One consideration favouring his employment of this method was that its analogue nature seems to lend itself more naturally to the investigation of physical questions, than did the essentially digital dice-face method then usual. While much of Forwald's most enterprising work involved the placement technique he also sometimes used the ordinary die-face approach.

In the mid-fifties Forwald carried out several original investigations in which he tried to calculate the energy involved in PK-interaction. In this type of study he made use of the so-called 'scaling displacement method'. He simplified the measurements by releasing dice onto a surface which had in advance been divided into squares of standardized size (1954). He also studied the influence of different surface textures of the cubes (1955); the density of the cubes, their thickness, coating, etc. (1957, 1959). Based on the outcome of such studies he tried to conceptualize the nature of the PK-force. Later on he tried to relate some of his findings to gravity. On that topic his main contribution was published in the monograph: 'Mind, Matter and Gravitation' (in the Parapsychology Foundation's Parapsychological Monograph series). In this monograph Forwald states that his experimental findings had 'led to the recognition of mathematical structures which are the same in PK-forces as those observed in physics'. He claimed that the findings suggest that PK forces are of a gravitational nature, originating from a mind over matter influence which acts on atomic nuclei in the target material used in the experiment. Forwald conjectured

that 'the mind is a non-energetic quality, but that it can interact with matter'. Be that as it may.

It has often been stated that one of the shortcomings of Forwald's work was that he almost always functioned as his own subject as well as experimenter. However, one should notice that he was able to produce the same kind of results under conditions when the outcome could not be the effect of motivated perception. Here I think of some of the results which were obtained during his stay at the Parapsychology Laboratory of Duke University and in studies conducted at Dr. Robert A. McConnell's laboratory in Pittsburgh in which photographic recordings were utilized. In his experiments with Dr. McConnell he confirmed a highly interesting, previously reported internal effect, implying an outstanding success on the first trials of sets. At the very least Forwald demonstrated repeatably that he himself must be considered an outstanding PK-subject.

Forwald's contribution was considered important enough that he was awarded the then existing William McDougall Award for Distinguished Work in Parapsychology (1959). He received the McDougall Award after he in collaboration with J.G. Pratt had published the article 'Confirmation of the PK Placement Effect', which appeared in the March issue of the Journal of Parapsychology, 1958. More recently it should be mentioned that Forwald's experimental findings have played a far from unimportant part in Evan Harris Walker's theory of psi phenomena, one of the observation theories. Walker states: 'The unique aspect of this phenomenon is the importance of the physical conditions of the material system being affected, specifically the composition and geometry of the cubes. This fact together with the extensive experiments by Forwald allowed a detailed test of the theory... . The ability of the theory to account for the details of Forwald's experimental results provides compelling evidence for the present theory'(Walker in 'Psychic Exploration', Putnam's Sons, New York, p.565).

Forwald was an original research-worker who, in contrast to a number of American parapsychologists, was neither especially keen about nor skilled in the marketing of his ideas. He seemed to me to be a very modest person and could even be characterized as rather withdrawn and almost shy: he rarely attended a parapsychological meeting. Surprisingly enough I met him and made his acquaintance for the first and only time in 1976 at the Utrecht P.A. Convention.

Haakon Forwald was born in Mandal, Norway on 21 August 1897, he was raised in Oslo where he also graduated from college.

From 1918 to 1923 he served with the Flying Unit of the Royal Norwegian Navy in which he attained the rank of lieutenant. Later on he took up studies in civil engineering. In 1925 he received his batchelor's degree at the Technical University of Danzig. He continued his studies at the Swiss Federal Institute of Technology in Zürich and finally at the Royal Institute of Technology in Stockholm. He served for eight years as an engineer in Switzerland (at Brown-Bovery Co. and Oerlikon). His major contribution to engineering was made at ASEA, Ludvika, Sweden, where he served for 30 years. Here he distinguished himself as a very versatile engineer, working much of the time with electrical circuit breakers and pneumatic devices. His inventiveness is well demonstrated by the fact that he obtained hundreds of patents in Scandinavia, the U.S.A. and Europe.

In 1932 Haakon Forwald married Miss Birgit Steffenberg from Stockholm. They had two daughters, one son and seven grandchildren. Mrs. Forwald still lives in Ludvika, Sweden.

Mr. Forwald had two very absorbing hobbies: parapsychology and classical music.

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Martin Johnson

AN EXPERIMENT WITH THE HEAD OF JUT

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Parapsychology in recent years has been provided with several theories of psi which yield testable hypotheses. Unfortunately these hypotheses can only be tested in the presence of a reasonably stable and identifiable source of psi. Some of these, The Observational Theories, have compounded the problem by discarding the traditional means of identifying the source of psi effects in typical parapsychological experiments (See Millar (1978) for a complete discussion). Nonetheless, if a situation can be created in which psi effects are demonstrated and in which specially adapted statistical procedures provide a reasonable chance that the source of the effects can be identified, then the data thus provided may go some way to supporting, refuting, or refining those hypotheses.

Of first priority for experimental parapsychologists is the goal of achieving reasonably stable psi performance either from groups or individuals. Naturally this has been a goal throughout the history of the field but the continuing debate concerning the repeatability of experimental results in parapsychology (Honorton, 1978; Millar, 1979; Sargent, 1979; see also forthcoming special issue of EJP) and the recently highlighted problem of psi-based experimenter effect (Kennedy and Taddonio, 1976; White, 1976a,b; Broughton, 1977; O'Brien, 1978) demonstrate clearly that the degree of stability in psi results needed for testing the more promising hypotheses has not yet been achieved.

This paper fulfils the publication policy of this journal. An abbreviated report of this work was presented at the Third Annual Society for Psychical Research Convention in Edinburgh, 2-4 April 1979.

On the positive side, however, there are indications that progress toward this goal is being made, for example in the area of GESP through the ganzfeld/relaxation work associated with Honorton and his colleagues. Another area of promise is that of PK studies with electronic and atomic random event generators where most of the work has been associated with Schmidt and his colleagues. Unfortunately in this latter area there has been a notable inability to duplicate such success in European labs, starting with the pioneering work in this area by Beloff and Evans (1961) and continuing to the present (Bierman and Wout, 1977; Broughton and Millar, 1977; Houtkooper, 1977; Millar and Mackenzie, 1977; Broughton, Millar, Beloff and Wilson, 1978; Jacobs and Breederveld, 1979).

The advent of computer controlled REGs has made the repeatability problem with regard to the "micro-PK" findings particularly awkward. It is awkward because while on the one hand these devices provide a highly flexible methodological tool ideally suited to studying the observational or indeed any other hypotheses they have on the other hand resulted in a proliferation of studies which fail to find any PK effects at all. Very recently, however, attempts have been made to use such computer set-ups to recapture some of the qualities in the test situation which the early researchers considered of paramount importance for the elicitation of psi: interest, excitement, and motivation. Such studies (Weiner, 1978; Beloff, Broughton and Wilson, 1978) have embedded PK tests in engaging and highly motivating games which the subject "plays" on a computer. In a sense these experiments are the modern high-technology descendents of the studies by Steen (1957), Ratte (1960), and Ratte and Greene (1960). The experiment reported here is another in this line.

There are many reasons for hoping that game playing situations, particularly of the type which can be developed on a computer, will be successful in eliciting PK from subjects. It has always been thought that the less self-consciously experimental a psi-testing situation can be made the better are its chances of producing psi. In the early days of ESP testing Rhine and his colleagues stressed the importance of employing, within the limits if adequate experimental control, game-like test situations that would provide both motivation and enjoyment for the subjects (Rhine et al., 1940). Similarly Rhine (1947) noted that some of the most dramatic ESP "successes" came as a result of a playful challenge to the subject.

Well designed computer games can abolish boredom and bring

about real interest and motivation to do well for the subject. Indeed, without much difficulty all the factors traditionally thought to be good for psi can be put into a computer game and all the factors thought to be bad for psi can, for the most part, be kept out.

Early work with PK games was carried out by Steen (1957), who incorporated a PK test into a simulated baseball game played with dice, and by Ratte and Greene (1960), who used a basket ball style game with dice. Unfortunately the test conditions for these studies were far from ideal, particularly with regard to the recording of the complex scoring arrangement, and Stanford (1977) has pointed out that the statistics were not entirely appropriate for the conclusions drawn. Nonetheless this work does provide some interesting suggestions that a game situation could facilitate the production of PK. Even more intriguing suggestions arise from the work of Ratte (1960) to the effect that an element of playful competition is the most important ingredient.

Clearly the early work can be considered no more than suggestive but combined with the general body of literature concerning the effects of motivation, interest, etc. on PK performance it forms a reasonable argument for the trying of computerised PK games as a means of eliciting more or less stable PK performance under ideal conditions of control.

The experiment described in this report had as its main goal the use of a game-like device for the eliciting of stable PK performance in groups of unselected subjects. Two main types of subject group were used, those who played singly and presumably not very competitively and those who played in small groups of friends where it was expected that a certain amount of playful rivalry would occur. In case evidence of PK was found the experiment also included a comparison of real-time versus pre-recorded targets and an interacting comparison of targets which were displayed very perfunctorally and targets which held the subject's attention. Finally, an attempt was made to see if suggestive results regarding the correlation of a subject's self perception of luckiness with his PK performance (Ratte, 1960) could be confirmed.

METHOD

Description of the PK game

The game used in this study mimics the "test of strength"

game found at carnivals and amusement parks where one is meant to hit a lever with a heavy mallet sending a weight up a wire to ring a bell at the top. In Dutch this game is called "Kop van Jut" or Head of Jut. The game in this study could be considered a test of psychic strength although in practice very little was mentioned about the PK aspect of the situation.

The device itself consisted of an aluminum frame on which was mounted a column of 32 small red lamps (light emitting diodes). Alongside the column of lights was a slot for a strip of paper which bore labels for different points on the scale. Lamp number 25 was labeled "Average" (in Dutch, Matig) and this represented the MCE for a run. Other labels were 17 "Terrible" (Vreselijk), 21 "Weak" (Zwak), 29 "Not Bad" (Niet Slecht), and above the 32nd light "Outstanding" (Uitstekend). On top of the device, over the column, was a bell which rang with a single "ding" whenever the score exceeded 32. Additionally, at the bottom of the column of lights was a green light marked "Ready" (Klaar) and alongside was a digital display of the number of runs remaining. Centrally mounted in the base was the button by which the subject initiated a run. The whole device stood approximately 80 cm high and was placed on a small table.

Controllong the "Kop van Jut" was a purpose-built device which interpreted signals from the computer program that ran the experiment. The general appearance of a run to the subject was that when he pressed the button the column of lights would begin lighting from the bottom upwards, very rapidly at first but with decreasing speed, until it came to a stop at some point, hesitate for a moment, and then fall back. (Essentially the lights were meant to simulate the parabolic trajectory of a weight.)

Additional Equipment

The experiment was under control of a computer program running on a PDP 11/40 under the RSX-11 operating system. The program obtained random targets, displayed them via Kop van Jut, and stored the results in the manner described below.

Random binary numbers for the targets were generated by a noise diode based random event generator coupled to the computer. Two types of control tests of the generation of random numbers were incorporated into the experiment and are described below.

Experimental Conditions

Each button-press by the subject initiated a run of 50 binary

trials. A session consisted of 24 such runs.

Several main manipulations were contrasted in the experiment though not in factorial design since some of the conditions were largely exploratory. Nonetheless statistical rigor was preserved using the "Edinburgh Split" method of data analysis (described more fully in the "Data Analysis" section).

As part of the experiment each subject completed a questionnaire on how lucky he sees himself to be and what are his attitudes to games of chance (Ratte and Greene, 1960). This questionnaire was used primarily because it had been used in the small amount of pevious research in games and the question to be resolved was whether a subject's feelings of luckiness in any way related to his PK performance on Kop van Jut.

The main between-subjects manipulation involved two different settings in which the game was played. Subjects in the "Individual" condition played the game individually, sitting alone in the room. Subjects in the "Group" condition played the game in the company of several friends (all of whom arrived together) each taking his turn at doing one session. The latter situation was expected to foster a general feeling of playful competitiveness among the participants which was likely to be absent in the former condition.

There were two within-subjects manipulations. One concerned two different means of target generation, contemporaneous and pre-recorded, and the other took advantage of the way the targets were displayed to the subject. Using an ABBA allocation half of the runs were done in real-time, i.e. the target for lamp n was not obtained from the REG until lamp n-1 had been lit, and the other half of the runs were pre-recorded, i.e. the targets were all generated and stored during a two-second pause which separated the runs.

The second within-subjects manipulation was a comparison of PK performance in the first half of each run with the performance in the second half. The reason for this was that, owing to the manner of displaying the hits, the first 25 trials, which cause the lamps in the lower range of the column to come on, were displayed very rapidly and were not generally the focus of the subject's attention. It was as the lights were slowing down and the column approaching its zenith that the subject's attention was riveted to the display as he hoped for "just a few more". It was therefore possible that differences might occur between the halves of each run and furthermore this effect could interact with the contemporaneous/pre-recorded manipulation in a way which could provide useful information for theorists

of the observational school.

Computer Program

The computer program which controlled the experiment ran in the following manner: Initially the program requests certain information such as the subject's name, number of runs, etc. It then checks that the appropriate file is available and sets the "Runs Left" display to 24.

A run cycle begins with a two-second pause after which the green "Ready" lamp is lit. When the button is pressed the computer begins to generate targets. It checks the REG: if the result is a hit the program stores it and then checks to see if it is time to light a red lamp, doing so when it is time. If the REG produces a miss the computer stores it and checks the REG again, until a hit is obtained at which time it checks the time and when ready displays it. Thus only hits are displayed to the subject via successive red lamps. As soon as one lamp is lit the computer resumes checking the REG for the hit which will light the next lamp until the total of 50 trials is reached. The timing of the interval between lamps is controlled by the hit's position in the string of 50 trials. When all 50 trials are complete there is a pause of two seconds at the zenith after which the computer extinguishes the lamps in a timed manner to simulate the column falling back.

A within-run control test of the REG is built into the program in that each time an experimental target is obtained from the REG the computer also gets a control target which is not displayed but only stored. Thus each target which governs the display to the subject and constitutes part of his experimental task is matched, one for one, with a control obtained at virtually the same time.

There are two types of runs between which the computer switches in ABBA fashion through the total of 24, the real-time condition (A) and the pre-recorded condition (B). The real-time condition was described above. The only difference which exists for the pre-recorded condition is that during the two-second pause which preceeds the run all 50 targets and 50 controls are generated rapidly and stored in computer memory. During the run, instead of getting the targets from the REG, the program obtains them from the memory locations. Thus in the pre-recorded condition all targets and controls exist before the button is pressed by the subject.

With the start of each run the "Runs Left" counter is decremented

by one. At the close of the experiment all data are stored and the experimenter notified. No data are shown to the experimenter at this time.

Procedure

When a subject arrived at the suite of rooms for testing he was greeted by the experimenter and shown to one room. He was given the instruction sheet which explained the experiment and asked to complete the Luck questionnaire. On bringing the completed questionnaire to the experimenter the subject was then shown to the room with the Kop van Jut apparatus. The procedure for operating the device was explained and the experimenter returned to the control room to start the program. As the program started the experimenter would look in on the subject to see that he had started his first run with no trouble and to wish him "Good Luck!". At the conclusion of the Kop van Jut game the subject was thanked and any further questions were answered.

Slight differences in the procedure were required when groups arrived for the group condition. When three or four subjects arrived they were shown into the Kop van Jut room and given the instruction sheet and questionnaire there. Whoever finished the questionnaire first started his session on the Kop van Jut while others watched. (Generally subjects completed the questionnaire in about the same time). In all cases the subject completed the questionnaire before playing the game.

Single subjects spent about 15 minutes in the experiment with the time about equally divided between the questionnaire and the game.

The instruction sheet (in Dutch) given to the subjects briefly explained that this was an investigation to see if those who were generally lucky in games of chance would also do well in our game of chance, the Kop van Jut. It also said that the results would enable the investigator to determine if "luck" involves an element of psychokinesis.

At irregular times interspersed with the running of the subjects a second type of control test of the REG was carried out. This consisted of running an entire Kop van Jut session without anyone operating the game. (Removing a wire in the control room had the effect of holding the button down). One of these 'autotests' was done for each subject although due to scheduling restrictions these were not matched in any sense.

Subjects

Subjects, most of whom were students in the psychology department, were recruited through notices posted in the department. Subjects were paid a small fee, generally in "subject hours" to fulfil course requirements although in a few cases non-students received cash.

Subjects were free to come alone (individual condition) or with friends (group condition). Between 20 and 25 subjects were wanted for each condition but as optional stopping was not a problem due to the automated and blind collection of data it was decided that a convenient stopping point for the groups would determine the number of individuals used as well (so that the number of individuals in each condition would be the same). In the event 24 group subjects were run and the first 24 (of a total of 32) individual subjects were used.

Analyses

A main feature of the analysis was the use of what has become known as the "Edinburgh Split", a method of combining pilot and confirmation aspects of data analysis in a single experiment, recently used in computerised parapsychology experiments at Edinburgh (Broughton and Millar, 1977; Broughton, Millar, Beloff and Wilson, 1978).[1] Essentially this is a computer conducted split of the data according to certain criteria allowing one part of the data to be called the "Pilot" and the other part to be called the "Confirmation". The pilot part can be analysed with all the freedom the experimenter wishes. Based upon his findings in the pilot part the experimenter then formulates specific hypotheses regarding the effects which he believes to be in the data and then rigorously tests for these in the confirmatory data part. Effects thus confirmed would be free from the all too common SSA problem (Statistical Scrounging Artifact) and would suggest a robustness not often found in parapsychological research. On the other hand, a failure to confirm would have to be accepted as such and in no way could it be excused by appeals to differences between the "experiments".

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[1] It should be noted that similar single-experiment pilot/confirmation splits have been used before, notably in Bhadra (1966).

Obviously such a method can only be employed in situations where data are collected without the experimenter knowing the results. Also, once a decision has been made to use this method it is not permitted under any pretext to combine pilot and confirmation data.

It was not necessary in this case that the particular tests be planned in advance. The main comparisons that were of interest were (1) general subject performance in each condition - individual vs. group playing, (2) performance of all subjects on the real-time trials vs. the pre-recorded trials, (3) performance of all subjects in the first 25 vs. the second 25 trials of each run. Performance in this case refers to either scoring or variance effects since either could be an indicator of PK operation in this situation. For example, significantly high variance in subject performance on a PK task was recently noted in a report by Terry and Schmidt (1978).

The splitting of data was carried out in the following manner: A computer program went through the stored raw data and, according to command, read the data for each condition in ABBA fashion producing certain summary statistics for either the A or B part. Thus half of the runs under each condition for each subject constituted one "side" of the split.

Questionnaires were scored in the manner of Ratte and Greene (1960) with some slight adjustments to allow for the fact that some of the games mentioned in the original are not popular in Holland. Questionnaire scores were, of course, not split but were tested for possible correlations with each half of the split data.

RESULTS

Questionnaire results

The questionnaires were scored first and the self-ratings of those in the individual condition were compared with those in the group condition. Not surprisingly it was found that, although the means of the two groups were not significantly different (Mean Ind.=7.75; Mean Grp.=11.04; $t(46)=-1.33$; n.s.) the variances of the conditions were significantly different, $F(23,23)=3.846$, $p(2t)=.002$, with the individual condition producing the higher variance. This indicates that those who filled out the questionnaire alone did so with greater abandon than those who filled it out in the company of friends. Individual subject scores ranged from -20 to +25 whereas group subject scores ranged between 0 and 20.

Pilot PK data

The data were split with the "A" part being deemed the pilot and produced for examination first.

To examine for overall scoring effects, in other words to ask the question, "Did the Kop van Jut game provide the conditions for enhanced PK performance from subjects in general?", scoring was examined by single-mean t-tests. Tests were performed for 1. total score, 2. first half run subtotal, 3. second half run subtotal, 4. real-time subtotal, 5. pre-recorded subtotal for individual, group and combined conditions. No significant departures from chance were found. The data are presented in table 1.

TABLE 1

Summary of single-mean t-tests for pilot data

Data	MCE	Mean	t	df
Individual condition				
Total scores	300	302.375	1.070	23
First half subtotal	150	150.750	0.544	23
Second half subtotal	150	151.625	0.908	23
Real-time subtotal	150	152.333	1.288	23
Pre-rec. subtotal	150	150.042	0.029	23
Group condition				
Total scores	300	299.833	-0.073	23
First half subtotal	150	152.500	1.648	23
Second half subtotal	150	147.333	-1.475	23
Real-time subtotal	150	148.375	-1.053	23
Pre-rec. subtotal	150	151.458	0.762	23
Combined conditions				
Total scores	300	301.104	0.695	47
First half subtotal	150	151.625	1.590	47
Second half subtotal	150	149.479	-0.402	47
Real-time subtotal	150	150.354	0.292	47
Pre-rec. subtotal	150	150.750	0.629	47

Comparisons were made between the first half run / second half run subtotals and the real-time / pre-recorded subtotals for the individual, group and combined conditions. One of the six comparisons was significant. This was in the group condition where the first half run scores were significantly higher than the second half run scores, $t(23)=2.128$, $p(2t)=.044$. Additionally, an examination of first half run / second half run differences within the real-time and pre-recorded subdivisions was made but no significant differences were found. The results of these tests are presented in table 2.

TABLE 2

Correlated t-tests for pilot data

Comparison	t(dep)	df	
Individual condition			
First - Second half subtotals	-0.381	23	
Real-time - Pre-recorded subtotals	0.948	23	
First - Second half for real-time	0.208	23	
First - Second half for pre-rec.	-0.595	23	
Group condition			
First - Second half subtotals	2.128	23	$p < .05$
Real-time - Pre-recorded subtotals	-1.179	23	
First - Second half for real-time	1.990	23	
First - Second half for pre-rec.	0.976	23	
Combined conditions			
First - Second half subtotals	1.254	47	
Real-time - Pre-recorded subtotals	-0.219	47	
First - Second half for real-time	1.576	47	
First - Second half for pre-rec.	0.213	47	

To assess possible differences between the PK performance of the individual subjects and the group subjects t-test comparisons were made for the two groups with respect to total scores, real-time subtotals, and pre-recorded subtotals. No significant differences were found. These results are presented in table 3.

TABLE 3

Independent t-tests on pilot data

Comparison	t(ind)	df
Individual vs. Group conditions		
Total scores	0.797	46
Real-time subtotal	1.664	46
Pre-recorded subtotal	-0.590	46

When the questionnaire scores were correlated with the PK data some interesting results emerged. For the individual, group and combined conditions the questionnaire scores were correlated with total scores as well as the first half / second half and real-time / pre-recorded subdivisions. For all divisions and subdivisions except one the correlations were positive. The correlation of the questionnaire with the total scores for the combined conditions yielded a suggestive result of $r(46)=.265$, $p(2t) <.07$. Examination of the two conditions showed that the greater part of this effect was to be found in the scores of the group condition where the questionnaire results were known to be more homogeneous. In the group condition the correlation with the total scores was significant with $r(22)=.506$, $p <.02$. Within this group the second half subtotal correlated positively and significantly with $r(22)=.406$, $p(2t) <.05$, and the best correlation of all was found in the pre-recorded subtotal, $r(22)=.518$, $p(2t)=.01$. These results are presented in table 4. The correlations with the matched control data are included by way of comparison.

Variance effects were assessed for the totals and subtotals using Chi-square tests (Rhine et. al., 1940). No overall variance effects were noted nor were any systematic relationships between the questionnaire scores and subject variances observed.

Examining individual scoring to see if there were any potential special subjects yielded nothing that was not well within chance expectation. Only one of the 48 subjects had a total which departed significantly from chance, in that case with CR=2.0.

Three individuals produced significantly high variances for the total results and one of these seemed worth examining in detail. The subject concerned (P.H.) was one of the few non-students in the study who was in fact a reporter for a Dutch newspaper and very skeptical

TABLE 4

Correlations of questionnaire with pilot data

Questionnaire correlated with	r	df		(matched con. r)
Individual condition				
Total scores	.209	22		(.033)
First half subtotal	.152	22		(.191)
Second half subtotal	.142	22		(-.227)
Real-time subtotal	.312	22		(-.065)
Pre-recorded subtotal	-.070	22		(.108)
Group condition				
Total scores	.506	22	p<.02	(-.169)
First half subtotal	.280	22		(-.070)
Second half subtotal	.406	22	p<.05	(-.155)
Real-time subtotal	.108	22		(-.228)
Pre-recorded subtotal	.518	22	p=.01	(.013)
Combined conditions				
Total scores	.265	46	p<.07	(.186)
First half subtotal	.205	46		(.095)
Second half subtotal	.163	46		(-.230)
Real-time subtotal	.186	46		(-.142)
Pre-recorded subtotal	.163	46		(.062)

of the existence of paranormal phenomena. His total variance yielded $\chi^2(11)=28.8$, $p=.002$, with the real-time subtotal yielding $\chi^2(5)=15.2$, $p=.01$, and the pre-recorded subtotal yielding $\chi^2(5)=13.6$, $p=.02$. Rather puzzling was the fact that the total variance for the matched control trials was also significantly high, $\chi^2(11)=20.16$, $p=.04$. The interesting feature of this subject's data was that all the high variance seemed confined to the first halves of the runs. The data for the first half of each real-time trial yielded $\chi^2(5)=12.4$, $p=.04$, pre-recorded runs $\chi^2(5)=14.0$, $p=.02$, and total first half run data $\chi^2(11)=26.4$, $p=.006$. Also the matched control data here were significant, $\chi^2(11)=21.27$, $p=.03$. This was in contrast to the second half run data where all variances were close to the theoretical expectancy. The variances and associated Chi-square

values for this subject are presented in table 5. Matched control data are in parentheses.

TABLE 5

Variances for subject P.H. (pilot data)

	First half				Second half				Total			
	Var.	Runs	χ^2	p	Var.	Runs	χ^2	p	Var.	Runs	χ^2	p
RT	12.92	6	12.40	.04	7.58	6	7.28	.20	31.67	6	15.20	.01
(c	10.58	6	10.16	.10	11.25	6	10.80	.08	21.17	6	10.16	.10)
PR	14.58	6	14.00	.02	4.25	6	4.08	.53	28.33	6	13.60	.02
(c	11.58	6	11.12	.07	3.25	6	3.12	.68	20.83	6	10.00	.08)
T	13.75	12	26.40	.006	5.92	12	11.37	.43	30.00	12	28.80	.002
(c	11.08	12	21.27	.03	7.25	12	13.92	.24	21.00	12	20.16	.04)

Theoretical variances for halves: 6.25; for total: 12.5.
Df = Runs -1

Hypotheses for confirmation

The analyses of the pilot data provided little to suggest any general hypotheses regarding subject performance or group differences in the Kop van Jut game. An attempt would be made to confirm the marginally significant differences between the first half run scores and the second half run scores.

The interesting findings of the Pilot centered on the relationship between the questionnaire scores and subject performance and there were encouraging indications that a real effect was involved. Accordingly it was hypothesized that in the confirmation data the total scores of the combined conditions would correlate positively and significantly with the questionnaire scores. Alpha was to be set at .05 with one-tailed tests. A secondary hypothesis was that at least the total scores of the group condition, where questionnaire scores were more homogeneous, would demonstrate a positive and significant correlation.

While it was disappointing that no hypotheses concerning the

general effect of the game could be put forward it was thought that a confirmation of the hypothesis related to the luck questionnaire could open the way to refining this and related predictors of success in game-like tests of PK.

As a minor effect, one for which a formal hypothesis could not be formulated, it was worth investigating whether the peculiar pattern of variance scores for subject P.H. was replicated in the confirmation half of his data. Explanations for the pattern would be premature but a strong replication of the effect would provide grist for the theoreticians' mills.

Confirmation PK data

The "B" part of the data served as the confirmation half and these were examined to see if the hypotheses were confirmed or the minor effects replicated.

Concerning the main hypothesis, the overall scoring for the combined conditions correlated positively but not significantly, falling well short of significance with $r(46)=.151$. The group condition total scores, with which the secondary hypothesis was concerned, also correlated positively but not significantly with $r(22)=.193$. In the confirmation data the group condition pre-recorded subtotal, which produced the highest positive correlation in the pilot, produced a slightly negative correlation in the confirmation. The confirmation data displayed a marginally significant positive correlation with the real-time subtotal but since this was not a finding in the pilot and in the face of the failure to confirm the main effects little weight can be placed on this result. It must be concluded that the hypotheses were not confirmed. Table 6 repeats the correlations of the pilot data and presents the corresponding correlations for the confirmation data.

The difference between the first half run and the second half run scores in the pilot was not replicated in the confirmation data. The scores were in the same direction, i.e. with the first half higher, but not significantly so with $t(23)=1.666$.

Finally the pattern of variance scores for subject P.H. found in the pilot was not replicated to any degree in his confirmation data. Table 7 presents the variances and Chi-square values of the confirmation data and this table should be compared with table 5.

TABLE 6

Correlations of questionnaire with scores
Comparison of pilot and confirmation data results

Questionnaire correlated with	df	r(pilot)		r(confir.)	
Individual condition					
Total scores	22	.209		.174	
First half subtotal	22	.152		.218	
Second half subtotal	22	.142		-.040	
Real-time subtotal	22	.312		.234	
Pre-recorded subtotal	22	-.070		-.009	
Group condition					
Total scores	22	.506	p<.02	.193	
First half subtotal	22	.280		.218	
Second half subtotal	22	.406	p<.05	.052	
Real-time subtotal	22	.108		.346	p<.05
Pre-recorded subtotal	22	.518	p=.01	-.058	
Combined conditions					
Total scores	46	.265	p<.07	.151	
First half subtotal	46	.205		.203	
Second half subtotal	46	.163		-.026	
Real-time subtotal	46	.186		.266	p<.04
Pre-recorded subtotal	46	.163		-.058	

The p-values for the confirmation data are one-tailed

TABLE 7

Variances for subject P.H. (confirmation data)

	First half				Second half				Total			
	Var.	Runs	χ^2	p	Var.	Runs	χ^2	p	Var.	Runs	χ^2	p
RT	10.92	6	10.48	.09	3.92	6	3.76	.58	16.67	6	8.00	.16
(c	1.92	6	1.84	.87	5.92	6	5.68	.34	9.50	6	4.56	.47)
PR	6.25	6	6.00	.31	5.25	6	5.04	.37	4.00	6	1.92	.86
(c	1.92	6	1.84	.87	10.25	6	9.84	.08	13.00	6	6.24	.28)
T	8.58	12	16.47	.12	4.58	12	8.79	.64	10.33	12	9.92	.54
(c	1.92	12	3.69	.98	8.08	12	15.51	.16	11.25	12	10.80	.48)

Theoretical variance for halves: 6.25; for total: 12.5.
Df = Runs -1

DISCUSSION

The Kop of Jut game described in this report succeeded in one aspect in that most subjects enjoyed playing it. This was apparent especially in the group condition where subjects particularly seemed to enjoy themselves and several times groups asked if they could return for future experiments. Unfortunately the success in producing an enjoyable task was not matched by success in getting subjects to produce PK.

Depending on one's particular reading of the evidence for success in PK tasks any number of reasons can be put forward to explain away this experiment's failure to find any paranormal effects. Such exercises are, of course, futile since none of these excuses have anything approaching the force of a prediction and all would have been conveniently forgotten had the experiment succeeded.

In the absence of any clear rules for producing PK effects investigators must rely on their reading of the hopeful signs in the evidence of others to guide their own work. In this project several such hopeful indications were combined in a project designed to elicit stable PK performance. The choice of a game-like device finds support not only in this investigator's reading of the evidence but also in Stanford's (1977) comprehensive review of PK testing where

he sees such techniques as hopeful because they embody aspects of motivation and intention which he regards as essential for PK. That the present experiment has failed is unfortunate but it will be less so if future work enables us to specify why.

What can be gained from this experiment at present lies mainly in the methodological example it sets for future work in this area. With the increasing use of mini- and micro-computers game-like tests of PK will be common and the automatic and blind collection of large amounts of data routine. It is essential for parapsychologists to insure that they do not bequeath to the field further unverifiable findings. Considering the ease of implementation for such techniques in typical computerized experiments pilot / confirmation splits similar to that used here should become the norm where the investigator wishes to guard against spurious effects.

Split analysis approaches will not eliminate the problem of psi-based experimenter effect but by varying, as they do, the parameters for only one of the possible sources of psi in an experiment they make experimenter effects easier to detect. Variations of the split analysis technique, for example using two different persons to do separate analyses, may even provide important insight into the experimenter effect problem.

SUMMARY

An attempt was made to use an enjoyable game-like device to elicit stable PK performance from unselected subjects. Two groups of subjects were used, those who played individually and those who played amidst friends with the latter condition expected to foster playful competition. Subjects rated their feelings of luckiness and these scores were correlated with performance.

Although conducted as a single experiment a split analysis technique enabled the data to be examined in pilot and confirmation design. Several statistical significances were noted in the pilot however none stood up to the confirmation analysis. Accordingly the experiment was judged as having failed to demonstrate PK effects.

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AN EXTENSION TO THE CNV STUDY AND AN EVALUATION

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The author has recently reported an experiment in which an electrical brain wave called the contingent negative variation (CNV) was examined for evidence of paranormally derived information about a future event (Hartwell, 1978). No significant results were obtained in that study which derived its subjects mainly from laboratory personnel and from the family and friends of the experimenters.

In the present experiment a very similar procedure was used with some subjects recruited according to the degree of perceptual defensiveness they showed on a previous Defense Mechanism Test. This test has been used as a predictor of performance on an ESP test in several previous investigations (for a summary see Johnson, 1977). In addition to these subjects, two others were selected because of their interest and their previous histories of spontaneous psychic experiences. One of these had been a subject in the earlier CNV experiment, providing results that supported the hypothesis. These latter two subjects were aquainted with the nature and purpose of the experiment. The subjects recruited from those taking the Defense Mechanism Test were not told about the parapsychological hypothesis being tested. They were told only that they have been selected because of their performance on the earlier subliminal test and that the warning stimuli used here provided varying levels of information, some of them being very difficult to see.

METHOD AND PROCEDURE

The experimental procedure employed was like that reported for the earlier study. Stylized faces served as warning stimuli and pictures of men and women as imperative stimuli in a CNV paradigm. The pictures were projected transparancies shown tachistoscopically. Subjects were asked to respond with a rapid button press to pictures of persons opposite their own sex. The warning faces sometimes

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provided information about the imperative stimulus to follow. A smile was followed by a person opposite the subject's sex, and a frown by someone of his own sex. Some of the smiles and frowns were easy to see (high contrast slides) and some were difficult (low contrast). Some of the faces were of low contrast and showed no mouths. These could be followed by either type of imperative stimulus as determined by a Schmidt-type random number generator. The trials using these warnings constituted the parapsychological portion of the test.

The experimental apparatus was also similar to that employed in the earlier study although several improvements were made in the stimulus delivery system. A random access projector was employed which permitted the order of the trials to be determined anew for each run. A shuffling procedure was devised which constrained each sequence to be balanced in its first order dependencies. That is, in each sequence of 36, each type of trial was followed exactly once by each of the six kinds (three levels of warning ingromation X two sexes for imperative stimuli.) Within this constraint the sequences were random. A reduced level of illumination was used for both the slide projector and the back light so that the screen was easier for the subjects to look at. A smaller and brighter eye fixation lamp was employed in the hope of reducing systematic eye movements.

The analysis procedures were also the same as those used in the previous experiment except that the CNV waveforms were represented with fewer Fourier coefficients. The multivariate tests for significant differences on the informed conditions were carried out with only three variables (the dc term and one phase from each of the lowest two frequencies). When significant differences were found, a discriminant function was formed and used to classify the ESP trials. Five Fourier coefficients were used in these discriminant functions so as to include a generally small contribution from the influence of the evoked response on both phases of the third Fourier harmonic.

The main hypothesis did not differ from that of the previous study. Classifications for the ESP trials were carried out for all electrodes where reliable discriminant functions could be fashioned from the informed trials. The resulting t-values were tested for a mean of zero across subjects, separate tests being done for each electrode position (F_z, C_z, and P_z). Two additional hypotheses were derived from post-hoc analyses of the previous study. First, it was expected that the person who had scored well before would do so again. Second it was predicted that the random generator would more often select same-sex than opposite-sex stimuli, especially on the first run for each subject.

RESULTS

Only one of the hypotheses was properly tested. None were upheld. Useful data were obtained from only five subjects whereas ten had been planned. The others were lost to a variety of annoyances summarized below.

A test of the main hypothesis based upon those numbers in hand does not show significant deviations from chance. Table 1 presents those numbers in the format employed in the earlier CNV study. The frontal and central electrodes are included in data from three subjects, while information about the pariental site is available for only two people.

TABLE 1

Subject	F_z	C_z	P_z
MS	nc	1.477^{F}	nc
AN	-1.470^{F}	0.627^{F+L}	-1.487^{F+L}
PI	-0.137^{F+L}	nc	nc
EL	nc	-0.532^{L}	-1.296^{L}
DE	-0.985^{L}	nc	nc
mean	-0.864	+0.524	-1.391

TABLE 1. Classification of ESP trials by discriminant functions. Upper litterals denote the source of the discriminant functions (F for full information trials and L for low information trials). No classification (nc) was done when significant differences were not seen in the informed conditions.

It is difficult to answer meaningful questions with a sample of this size. Six of the eight entries in the table are opposite in sign from that expected. None excede one and one half (theoretical) standard deviations from the expected mean of zero. An amalgamation of these data with those from the first study would also apparently lead to results reasonably attributable to chance. Certainly they do not strengthen the previous results.

The person who scored most strongly in the first experiment contributed the entry in the table corresponding to subject MS

(a centrally placed electrode). This is again the result most in accord with the hypothesis, but it does not represent a significant deviation from chance even with a one-tailed test.

Neither was a significant effect seen in the random generator's selection of imperative stimuli on the ESP trials. The overall totals were even (102-120), and the data from each subject's first run were 33 same sex and 27 opposite sex pictures.

DISCUSSION

This experiment and the very similar one which preceded it were developed to help assess the merits of one way of conceptualizing the operation of psi. The main hypothesis derives from the notion that psi is common in human endeavor and often serves an adaptive function. It is, however, thought to be a small effect, lying below the threshold of conscious awareness, and ordinarily quite hidden by a jumble of other and larger forces. There are many variations of this view, but perhaps the most fully developed has been published by Stanford (1974a, 1974b, 1977).

Little elaboration of the basic concepts is necessary to recommend some lines of experimental investigation. Stanford's own studies of unconscious psi tasks (Stanford and Thompson, 1974 and 1976) can be construed as evidence in favor of this general position. Although subjects in these experiments were without conscious knowledge that a certain random process would effect their futures or other people's futures, they were more often than not beneficially treated by the electronic dice. The scoring rates are low; the effect is a small one. But it is not significantly smaller than that seen in a host of other forced choice laboratory experiments in psi which involve or even emphasize conscious envolvement with the task.

Thus one is invited to conclude that conscious engagement is not a fundamental requisity for the occurrence of the phenomena. Indeed, depending upon the state of consciousness, it may be more of an impediment than a help. The central conclusion of the depth psychologies is that certain kinds of information must surmount formidable barriers in order to gain conscious apprehension. This recommends experiments with persons in those altered states of consciousness reputed to improve the accessibility of other forms of information. The studies with ganzfeld, hypnosis, meditation and dreams summarized by Honorton (1977) and Van de Castle (1977) serve to bolster this argument. The research indicates that subjects perform better when they inhibit competing sensory information structures and attune themselves to a small effect that may be

always or often present but is ordinarily inaccessible.

At some level it is surmized, a correlation exists between events in the subject and those in the laboratory random device. The size of this correlation and the structures in which it should appear are not specified by this line of reasoning. It is implicit, however that to be advantageous the information must be present in biologically significant but unconscious forms.

The CNV studies were undertaken in order to examine a particular brain wave for evidence of this effect. This brain wave was chosen because it seemed, a priori, a likely candidate for revealing information of this kind. The CNV may be understood as reflecting the person's readiness to respond. This waveform is known to be influenced by psychological factors (Tecce, 1972). It was thought that the CNV might also reveal unconscious information in a setting which strongly encouraged an appropriate preparation to respond.

In fact the experiments did not resolve a significant difference of the type hypothesized. It may be, of course, that a small psi effect is present in the data but is undetected by the procedures employed. It may also be that the CNV poorly reflects unconscious processes and mostly represents the outcome of conscious deliberations. In that case there is the possibility that a more sensitive indicator will ultimately be discovered.

For now, however, the main effect of the experiments must be to detract from the appeal of the conceptualization of psi as abundant but unconscious. Certainly the experiments serve to lower the chance that a large neural effect will be found lying below conscious awareness but capable of influencing normal human behavior. This idea must now be judged weak in the same way that most other ideas about the operation of psi have been found wanting. It has so far failed to suggest an experimental arrangement in which the phenomena can be clearly seen and reliably studied.

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SKIN POTENTIAL ACTIVITY AND GUESSING PERFORMANCE OF MORE AND LESS DEFENSIVE SUBJECTS IN A GESP TASK

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A GESP test was given to twenty seven university students who had been previously given a Defense Mechanism Test. We broke the subject pool into two groups depending upon the degree of preconscious defensive organization revealed by this test. Extreme scores on this measure of perceptual defensiveness have correlated with success on some ESP tasks (Johnson & Lubke, 1977). In this instance the group of previously tested subjects was too small to allow selection by extreme scores. The group was simply partitioned at the median and the less defensive half postulated as the better guessers. No prediction was ventured about overall guessing success.

A measure of autonomic nervous activity (changes in the skin's electrical potential) was taken on each trial to see if this unconscious physiological function might reflect the ESP targets independently of guessing behavior. It seemed possible to us that those lower in perceptual defensiveness might especially manifest any tendency to reveal paranormal information unconsciously. We therefore formulated a third hypothesis to this effect.

In choosing to monitor changes in skin potential we were influenced primarily by the simplicity and passive nature of this measurement. We then verified that significant differences were seen in the agent's skin potential activity when he actually looked at the target material. Although this measure has not been employed in previous parapsychological experiments its behavior is normally correlated with other functions enervated by the peripheral sympathetic nervous system. A review of the relevant studies is edifying mostly because it exposes the considerable amount of exploration that has been done by parapsychologists in this area.

Numerous previous experiments have been concerned with the skin's resistance to the passage of an electrical current impressed upon it

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by an external device. Changes in this resistance are relatively easy to detect and have long been studied under the name of galvanic skin responses (or GSR). In very early experiments that have recently received new attention, Brugmans (1921) recorded GSR with apparatus especially constructed for the purpose. It is not clear, however, what conclusions may be drawn from this pioneering effort (Schouten and Kelly, 1978). Carrington (1935) also tried to measure GSR in his work with the trance personalities of Mrs. Leonard and Eileen Garrett. He was ultimately unable to complete the study as planned because of recording difficulties. (Like the Brugmans work, this investigation anticipated many features of later parapsychological studies. The statistical method employed was an analysis of variance in which R.A. Fisher gave assistance).

The more recent history of scientific work in this area is carefully cited by Schouten (1976) in his recent report of the successful use of GSR in a telepathy experiment. The interested reader is referred to his discussion with the remark that the various studies are both disparate in their conclusions and difficult to compare.

A similar situation exists with regard to measurements of peripheral bloodflow using a finger plethysmograph. This function reflects not only the action of the sympathetic nervous system in constricting the vasculature but also the influence of the heart which is itself subject to additional neural mechanisms. Because of this, the many experiments which utilized this physiologic index are of less immediate consequence to the present study. They are also reviewed by Schouten (1976), however, who obtained marginally significant results with the plethysmograph in his own study.

METHODS AND PROCEDURES

The experimental regimen was composed of trials on which the agent (MJ) viewed either a pleasant (serene) picture or a group of unpleasant ones. For twenty seconds the agent attempted to communicate a feeling or impression to the subject who was located a few rooms away. During this time we recorded and analyzed the subject's skin potential activity. At the conclusion of each trial the subject registered a guess about the kind of stimulus by pressing one of two buttons. A total of thirty trials were given to each subject separated by fifteen second intervals.

Upon arrival a subject chose from a set of seven color slides the one he found most pleasing and appealing from an aesthetic point of view. He then rated two qualities of this slide along scales of semantic differential ("unpleasant" to "pleasant", and "makes me

feel relaxed" to "makes me feel tense"). The chosen picture was used as the pleasant stimulus upon which the agent concentrated during the experiment.

A Polaroid photograph was taken of the subject at this time. It was later placed in a binder along with a group of very unpleasant (somewhat frightening) pictures. There was general concensus that these pictures which included some war attrocities were indeed unpleasant. They were observed together by the agent when he attempted to transmit an unpleasant feeling to the subject. To dramatize the effect of the unpleasantness in relation to the subject, the agent used a needle by which he stabbed the image of the subject's hand appearing on the polaroid photograph - the hand on which the electrodes were attached during the experiment.

Skin potential electrodes were applied to the hypothenar eminance (raised part opposite the thumb) of the left hand and to the left forearm. The electrodes were of the silver-silver chloride variety, encased in a plastic surround. The skin was thoroughly cleansed with acetone before application. The electrodes were filled with a conductive paste and secured to the skin with adhesive collars. The impedances of both the active and the reference electrodes were checked with respect to a centrally located earth electrode to insure that the electrical connection was satisfactory.

The subject was lead to the experimental room and seated in a comfortable chair. He was instructed in the procedures and told to relax. The room was dimly lit and white noise of moderate volume was provided. After a five minute period for acclimation the study commenced.

The entire sequence of stimulus selection, trial timing, and data collection was under the programmed control of the laboratory computer. The target was chosen randomly for each trial by a Schmidt-type random generator. It was communicated to the agent by a pair of small lights. A single tone from a loudspeaker marked the onset of each trial and a pair of tones the end, whereupon the subject pressed one of two buttons to register his guess.

The skin potential activity was amplified using a voltage gain of 1000 and a low frequency time constant of 1.5 seconds. It was displayed on an oscillograph and digitized by the laboratory computer at the rate of twenty samples per second. The measure of skin potential activity for each trial was taken as the RMS value of these samples. A t-statistic was calculated over each subject's trials to characterize the difference associated with the agent's viewing pleasant or unpleasant stimuli.

RESULTS

None of the propositions this study was designed to test were sustained. The specific hypothesis tested and the actual results obtained were as follows:

1. The subjects in the less defensive group will guess more accurately than those in the more defensive group as evaluated by a t-test on the number of correct guesses.

Actually the more defensive subjects guessed somewhat more accurately than the less defensive ones, although not significantly so ($t = 0.93$).

2. The skin potential activity during trials with unpleasant stimuli will differ from those with pleasant stimuli. The difference will be assessed by testing the summary skin potential t-statistics for a mean of zero.

No consistent differences in the magnitude of skin potential activity were in fact observed ($CR = 0.29$).

3. The subjects in the less defensive group will manifest greater skin potential activity during the unpleasant stimuli than those in the more defensive group. This will be evaluated by a t-test on the average of each subject's activity during the unpleasant trials.

The less defensive subjects did show somewhat higher skin potential activity than the more defensive ones during trials with unpleasant stimuli. This result is in the predicted direction but is small enough to be reasonable attributed to chance ($t = 0,65$).

In addition to these formal hypotheses it was agreed to thoroughly study the data for the presence of PK upon the selection of stimuli by the random generator and to examine the accuracy of guessing for dependence upon reaction time and types of stimuli. On seventeen of the twenty seven runs an excess of pleasant stimuli was chosen by the random device. The result is suggestive but not significant. Guessing accuracy was assessed for dependence upon type of stimulus and response time by a four-cell chi-square test. For this test each subject's responses were partitioned at his median reaction time. Subjects were insignificantly more accurate in their guessing when they responded slowly to pleasant stimuli and quickly to unpleasant ones. No evidence was found for a correlation between guessing accuracy and skin potential activity.

DISCUSSION

In retrospect we are able to criticize several aspects of the experimental procedure employed here. The study was carried out during normal working hours in a downtown building and noise from the building and street was often found to be intrusive. We would favor a quieter experimental setting in any future attempts of this sort.

We would also allow more time and social contact prior to the testing period. In this experiment an assistant greeted the subject, applied the electrodes and gave the instructions. The schedule was tight. Another time we would slow the procedure considerably and assign some of these duties to the agent. However, these words of criticism would probably never have been written if the study had happened to "turn out".

We continue to support the concept of using pre-selected subjects in parapsychology experiments. Another time we would definitely restrict ourselves to extreme scorers on the Defense Mechanism Test.

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A PRELIMINARY INVESTIGATION OF CLAIRVOYANCE DURING HYPNOTIC AGE REGRESSION

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INTRODUCTION

Hypnosis has long been associated with augmented psi. Even the earliest reports of hypnosis (or "mesmerism", as it was then called) contain references to ostensible psi phenomena. These phenomena include: "transposition of the senses" (in which one sense seems to provide information of a type usually restricted to another sense), "community of sensation" (in which the hypnotized subject seems to respond to sensory cues unavailable to himself, but available only to the hypnotist), "mental suggestion" (in which the hypnotist induces images, reactions, or even hypnosis itself "at a distance"), and "clairvoyance" (typically, but not always, in the form of "travelling clairvoyance" in which distant events are described). Usually, however, the reported instances of psi accompanying hypnosis are not sufficiently evidential according to today's stricter standards.

Fortunately, the earlier ambiguous hypnosis-psi anecdotes have given way to better documented, better controlled, and less ambiguous laboratory studies of the influence of hypnosis on the psi process. In a recent review of the area, Honorton (1977) described 42 series in which psi was measured following hypnotic induction. Of these 42 series, 22 (52 percent) yielded evidence of significant psi effects rather then the 2.1 (5 percent) expected by chance. Of the studies allowing a hypnosis vs. control comparison, none indicated significantly superior psi in the control condition, while 12 out of 22 studies (54 percent) indicated significantly superior psi in the hypnotic condition. As a result of his exhaustive review, Honorton concluded: "I believe the conclusion is now inescapable that hypnotic induction procedures enhance psi receptivity". Honorton also concluded that the statistical efficiency (PQ%) of studies employing a free response

method of assessing psi was an order of magnitude greater than that of studies employing a restricted response (guessing) strategy.

A relatively unexplored advantage of hypnosis is that the increased suggestibility of the hypnotized subject might facilitate induction of psychological and physiological conditions which are favorable to psi. An example of the use of this strategy is found in a study by Grela (1945). Grela attempted to manipulate attitude in his hypnotized subjects by giving them suggestions favoring self-confidence in the ESP tests which followed, and comparing those scores with those obtained following suggestions unfavorable to ESP and inspiring doubt of ESP ability. The predicted outcome occurred, with the highest psi scoring in the session following positive suggestions and the lowest psi scoring in the session following negative suggestions. Casler (1962) also found a superiority in psi scoring for subjects given positive hypnotic suggestions for high ESP performance over subjects who were hypnotized, but not given explicit high-scoring suggestions.

Another example of the strategy of suggesting psi-favorable conditions to hypnotized subjects is a study by Honorton and Stump (1969) in which it was suggested that the subjects dream about the contents of concealed target pictures. Significant clairvoyant psi scores were associated with this dream technique. The hypnotic dream procedure has been reported with positive results, by Rechtschaffen (1970), Keeling (1971) and Honorton (1972).

Hypnotic suggestions have been used to induce two conditions which earlier research has shown to be favorable to psi: (a) a positive attitude and belief in ESP and (b) dreaming. The present study involves a third condition which has recently been shown to favor high ESP scoring, namely, "early age". Spinelli (1976) compared the GESP performance of subjects in six age groups ranging from 3.3 to 35.0 years of age. Highest scoring occurred in the youngest age group, lowest scoring in the oldest, and graded intermediate scores in the intervening groups. If replicated by further work, this finding of psi-superiority in the younger age groups would be most interesting in light of the observation that children would seem more likely than adults to possess certain characteristics favorable to good psi performance: less disbelief in the possibility of psi, more "right hemispheric" or "Mode 1" thinking and less "left hemispheric" or "Mode 2" thinking, a greater degree of "present-centeredness", and increased imagery and imagination. Thus, it seemed of interest to conduct a preliminary experiment in which it was suggested to hypnotized subjects that they regress to earlier ages; clairvoyance tests were then conducted at each of these suggested ages. If a type of psychological age-regression did indeed occur, it would be

interesting to note whether the predicted increase in psi scoring also occurred. Three a priori hypotheses were formulated: (a) significant evidence for clairvoyance would be obtained for the experiment as a whole, (b) clairvoyance scores for the hypnosis conditions would be significantly higher than those for the nonhypnosis condition, and (c) clairvoyance scores for the suggested younger ages would be significantly higher than those for the suggested older ages.

METHOD

Subjects: The subjects were ten female volunteers, ranging in age from 21 through 28 years. The volunteers were selected from among persons who expressed an interest in psi experimentation and who were interested in the possible usefulness of hypnotic suggestions for purposes of general relaxation and weight control. None of the volunteers reported any previous dramatic psychic experiences.

Hypnosis procedure: Except for two subjects who had been hypnotized at an earlier date by R.M., this was their first experience with hypnosis. Each subject received an initial explanation regarding hypnosis, age regression, and the nature of the experimental task.

Hypnosis was induced by using a relaxation method and was deemed successful (and the induction procedure terminated) when the subject evidenced eye catalepsy and arm levitation in response to suggestions for same. R.M. functioned as hypnotist, while W.B. witnessed the sessions and recorded the subject's responses.

Each subject attempted six clairvoyance tasks. Five of these tasks were carried out while hypnotized and one was carried out prior to hypnosis as a control measure. The non-hypnotic task was carried out at the subject's present age. The hypnotic age regression tasks were carried out at suggested ages of 21, 14, 7, and 4 years of age. The fifth hypnosis task was carried out at the subject's present age. (The one exception to this was in the case of the 21-year-old subject; she was regressed from her present age to 20, 13, 6, and 3 years). Prior to the experiment, all subjects had been quizzed regarding traumatic events at the above mentioned ages. No subject reported traumatic events.

After the non-hypnotic trial, each subject was hypnotized. When induction was completed, the following instructions were read:

> "I now want you to return to the age of ____. You will pick a very pleasant time and event. A time that makes you happy. After you have picked the event, I want you to tell me about it."

The description of the event allowed the individual to participate more actively in the age regression experience. The subject then had a target in an opaque envelope placed on her lap and was read the following instructions, modified from those of Honorton and Stump (1969):

> "When I count to three, you are going to have a dream. You will have a very interesting dream. It will be a very vivid and realistic dream. You will dream about a target in the envelope. It will be as if you are walking right into the picture in the envelope. You will be walking right into the picture; you will participate in whatever is depicted, and you will observe the picture from that standpoint. You will see everything very clearly because you will be part of it. You will dream for about five minutes and then awaken by yourself. If not, I will awaken you. When you awaken, you will remember the dream very clearly, and you will be able to describe it in great detail. When you finish reporting your dream, I will say relax 1, relax 2, relax 3, relax 4 and you will be just as relaxed as you are now. When I count to 3 you will have a dream about the target in the envelope. You will be very successful in this task."

The same procedures were used at each age regression. W.B. recorded, verbatim, the experiences mentioned by the subject at each suggested age. He also recorded the subject's "dream" impressions of the target contents. After all clairvoyance tasks had been completed, the subjects were debriefed completely as to all aspects of the experiment.

Target selection and preparation: For each volunteer, six target packets were prepared by an assistant, Robert Wood, who had no other involvement with the experiments. Each target was a 35 mm slide selected by means of a table of random numbers from a pool of 1024 such slides. The target was sealed inside of an opaque No. 000 "Jiffy Bag" envelope which was stapled shut. The six envelopes for a given volunteer were kept in the possession of W.B. until the volunteer apppeared at the lab for her experimental session. After a particular age had been suggested by R.M., W.B. placed one of the envelopes in the volunteer's lap, where it remained until the next age was suggested. It was then exchanged for the next target packet. After they had served as targets, the packets remained unopened until the very end of the session, at which time scoring was accomplished.

Psi measurement: The mentation verbalized by the volunteer during the five-minute impression period was copied verbatim by W.B. While the volunteer was being prepared for the test associated with the next suggested age, W.B. (still blind with respect to the identity of the target slide) coded the mentation protocol which had just been generated for the presence or absence of content in ten categories, according to the system developed by Honorton (1975). The accuracy of the volunteer's impressions was assessed statistically by comparing the binary content code of the mentation protocol with that of the pictorial target itself.

RESULTS

The binary target system scores for the clairvoyance test for the nonhypnosis condition and for the various hypnosis conditions are given in Table 1. Mean scores for all hypnosis tests (collapsing across the five hypnosis tests) and for the experiment as a whole (collapsing across the five hypnosis tests and the nonhypnosis test) are given in Table 2.

TABLE 1

Binary Target System Clairvoyance Scores for the Various Hypnosis and Non-hypnosis Conditions

Subject Number	Non-hypnosis Current Age	Hypnosis Age 21	Hypnosis Age 14	Hypnosis Age 7	Hypnosis Age 4	Hypnosis Current Age
1	5	6	5	4	4	9
2	8	6	8	8	5	4
3	6	6	8	5	6	5
4	5	4	5	4	6	8
5	6	6	4	3	6	6
6	6	4	5	5	5	6
7	6	5	6	8	8	3
8	2	4	4	5	5	6
9	7	8	2	4	5	5
10	5	1	8	7	6	7
$\bar{x}$	5.6	5.0	5.5	5.3	5.6	5.9

TABLE 2

Summary Statistics for Non-hypnosis, Combined Hypnosis, and Overall Clairvoyance Scores

Subject Number	Hypnosis Scores	Combined Hypnosis	Combined Hypnosis and Non-Hypnosis
1	5.0	5.6	5.5
2	8.0	6.2	6.5
3	6.0	6.0	6.0
4	5.0	5.4	5.3
5	6.0	5.0	5.2
6	6.0	5.0	5.2
7	6.0	6.0	6.0
8	2.0	4.8	4.3
9	7.0	4.8	5.2
10	5.0	5.8	5.7
$\bar{x}$	5.60	5.46	5.49
t	1.20	2.73	2.56
df	9	9	9
p	n.s	.015	.012

t non-hypnosis vs. combined hypnosis = 0.31, df = 9, p = n.s.

To test the hypothesis (a) (that significant clairvoyance would be exhibited in the experiment as a whole), a single mean *t* test was used, comparing the overall mean binary target scores of the ten subjects with mean chance expectation (5.0). This test yielded a significant *t* of 2.56 with 9 degrees of freedom (p = .015, one-tailed). Thus, hypothesis (a) was confirmed.

To test hypothesis (b) (that clairvoyance scores for the hypnosis condition would be significantly higher than those for the nonhypnosis condition), a matched *t* test was used, comparing the mean hypnosis score with the nonhypnosis score for the ten subjects. This test yielded a nonsignificant *t* of 0.31 (df = 9, p = n.s.). However, single mean *t* tests indicate extrachance clairvoyance scoring for the hypnosis condition (*t* = 2.73, df = 9, p = .012, one-tailed), but chance scoring for the nonhypnosis condition (*t* = 1.20, df = 9, p = n.s.). Thus, significant evidence for psi is present following

hypnosis, but not in the nonhypnosis condition, although the direct test of a difference between these two conditions is not statistically significant.

To test hypothesis (c) (that clairvoyance scores for the suggested younger ages would be significantly higher than those for the suggested older ages), an analysis of variance test was used, comparing the binary target scores for the five within-subject hypnosis conditions. This test failed to indicate a significant main effect of suggested age ($F = 0.33$, $df = 4$, $p = n.s.$). Thus, hypothesis (c) was not confirmed: suggested age had no effect upon clairvoyance score.

DISCUSSION

The significant binary target scores for the overall experiment provide another demonstration of clairvoyance and another indication of the usefulness of the binary target system in measuring this phenomenon. The finding that these binary target scores were significantly above mean chance expectancy for the hypnosis condition but not for the nonhypnosis condition requires comment. The significantly high scoring in the hypnosis condition replicates previous findings that hypnosis is a psi-favorable state. The reason for hypnosis' psi-conduciveness is not entirely clear, but several hypotheses may be put forth. Honorton (1977) has suggested that a mechanism underlying the psi-conduciveness of hypnosis may be the reduced level of psi-irrelevant "noise" due to the relative sensory isolation that hypnosis provides. Braud (1974, 1975) stressed the important role of the muscular relaxation and mental quietude associated with hypnosis. It may be the case that hypnosis is associated with a psi-conducive shift in cognitive information-processing away from a more analytical "left hemispheric" mode (Mode 2) toward a more nonanalytical, "right hemispheric" mode (Mode 1). In discussing psi-conducive conditions in general, Braud (1977) suggested several facilitating factors which may be operative in hypnosis in particular. Hypnosis is a well specified ritual in which a volunteer subject can place his trust and which can also remove some of his feeling of responsibility for good or poor ESP performance. "Ownership resistance" (Batcheldor, 1966; Brookes-Smith, 1973) and "egocentric striving" (Stanford, 1977) may therefore be lessened, freeing psi from the inhibition usually produced by these factors. Hypnosis may also facilitate certain psi-favorable psychological changes, such as a shift from "sensory reality" to "clairvoyant reality" (LeShan, 1974) or what Ehrenwald (1971) has called the "existential shift". Finally, the hypnosis-nonhypnosis differential may be a manifestation of an experimenter effect reflecting the investigator's expectations.

Also deserving of comment is the finding that suggested age had no obvious influence upon clairvoyance scoring. It may have been the case that our age-regression suggestions were not really effective. The content of the imagery reported by our volunteers at each of the suggested ages indicated that they were indeed regressed. However, behavioral changes appropriate to the suggested ages (e.g., voice changes, changes in mannerisms) were present in only a few cases. Perhaps if more time had been devoted to the age regression process and if more complete and more inclusive suggestions had been given regarding age-appropriate functioning, age-regression might have been more complete and a better test of the suggested age-psi relationship might have been possible. Alternatively, Spinelli's (1976) finding of a relationship between age and psi performance may have been artifactual and our prediction of better psi performance at younger ages ill-founded.

We intend to continue this type of research and we urge other investigators to explore the possibility of facilitating psi through the use of hypnotically suggested psi-conducive conditions of various types.

SUMMARY

A preliminary study was conducted to explore the effect upon psi of hypnotically suggested age regression. Ten female volunteer subjects were hypnotized and regressed to the ages of 21, 14, 7, and 4 years. Free-response clairvoyance tests were administered at each of these hypnotically suggested ages, as well as at the volunteer's current age (which ranged from 21 through 28 years). One current age test was conducted while the volunteer was hypnotized and one was conducted without hypnosis. The accuracy of clairvoyant impressions of the target (a 35 mm slide randomly selected from a pool of 1024 slides) was assessed through use of the binary coding method recently developed at the Maimonides Laboratory.

Significant psi-hitting occurred in the experiment as a whole and in the combined hypnosis conditions. Significant scoring did not occur in the non-hypnosis condition. Hypnotically suggested age had no effect upon clairvoyance scoring; thus, the predicted psi superiority of younger over older suggested age was not found.

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THE "LLOYD EFFECT"

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INTRODUCTION

This paper describes an experiment to investigate the findings of Lloyd (1973), who reported detecting a cortical average evoked potential due to rhythmic telepathic stimulation. The experimental work reported here has previously been mentioned in a research brief (Millar, 1975).

When a subject is stimulated by an event such as a brief tone or a flash of light his brain reacts by producing a specific response (evoked potential) which can be picked up by electrodes on the scalp. The snag is that the brain is an enormous chatter-box and the tiny evoked potential (EP) is buried in the on-going activity. One way of teasing out the signal from the noise is to stimulate the subject many times and compute the average curve for about a second following the stimulus presentation. In this way the background activity tends to cancel while the evoked potential, being virtually the same each time, is undiminished. The ideal consequence is that the average curve is pure EP, with no admixture of background. In practice considerable noise may remain uncancelled. Evoked potentials have been studied for many years and are routinely employed for both diagnostic and experimental purposes. For a review see Regan (1972).

A pilot attempt had been mentioned, in passing, by Silverman and Buchsbaum (1970, p.155) to detect an EP to telepathic stimulation, but none was found. However, an anonymous physician, under the pseudonym of Lloyd (1973) reported a positive experiment carried on in 1970, which sparked off the work reported here.

Lloyd used a standard set-up for eliciting average auditory evoked responses. However, instead of presenting tones to the subject an agent tried to transmit an image telepathically whenever a light flashed. Stimuli were presented regularly at one second intervals and 60 sweeps of 500 milliseconds were averaged. EEG was recorded from vertex (C_z)with respect to mastoid, though which

mastoid is not stated. The agent tried to transmit the image "a cup of coffee", while the percipient did not know the nature of the image.

The data of only a single subject are reported. Five average curves are reproduced. It seems that the scales are incorrectly given: 50 microvolts and 1 millisecond should be 5 and 10 respectively. The first three curves are standard auditory EPs of about 13 μV peak to peak. The feature pointed out is an initial negativity of latency about 120 mS followed by positivity at about 200 mS. The remaining two show the results of the telepathic condition: the largest feature of the first is a negativity of 130 mS latency: while there is a positive peak at 225 mS it does not exceed the noise level of the rest of the curve: the peak to peak voltage is 6.2 μV. The last curve reveals a negativity at 85 mS and positivity at 165 mS, though again the latter is no larger than the noise of the remainder. Peak to peak this curve measures 5.0 μV. Lloyd contends that telepathic stimulation here gives rise to an evoked potential of the same form as auditory stimulation but of about half the amplitude.

Communication with Whitton (personal communication), who has worked with Lloyd, elicited the information that all of the agents and percipients then employed claimed some psi ability. No further intelligence was gained regarding Lloyd's methodology. However, Whitton (1975) later added one interesting item, namely that in other subjects the psychic EP did not have the form of an acoustic one: on the contrary it was different for different percipients but constant for each one.

The greatest defect of this experiment was that the evaluation of the result depended entirely on the non-blind judgement of the experimenter: how likely was it that EP-like waveforms would be produced by chance alone out of the uncancelled background noise? This is true with redoubled force where the waveform to look for is different for each percipient. To answer this kind of question control runs without an agent sending are needed.

THE EXPERIMENT

Despite its dubious features Lloyd's experiment seemed worth following up. The major problem in studying psi phenomena for most (but not all) parapsychologists (Millar, 1979) lies in finding any psi effect to study. Consequently the lure of psi on tap was strong. The experiment, carried out in 1975, was conceived as fundamentally a replication of Lloyd's procedure, with the addition of the necessary control condition. In accordance with the statistical considerations outlined in a companion paper (Millar, this journal)

the procedure was so arranged that non-parametric statistics would provide powerful tests of the psi hypothesis.

The rationale underlying the procedure to be described should be mentioned at the outset. The generation of an image on seeing a small bulb glow, as used by Lloyd, is unlikely to produce an appreciable EP in an agent, never mind a percipient. It was decided that the bets could be nicely hedged by replacing the indicator light with a strobe lamp but still using the flash as a cue for imaging. The light would itself generate a good EP in the agent's brain. The intensity of the flash is such that it can still readily be seen through closed eyelids: this was an advantage in that the activities of the experimenter, who remained in sight of the agent while tending the apparatus during the experiment, would not be visible and hence could not be distracting.

A second desideratum was that experimental and control conditions should be electrically identical, thus ruling out electrical pick-up as a possible artifactual cause of any significant difference. Consequently whether the agent saw the strobe lamp flash was controlled by an opaque cover, rather than by switching off the lamp. One complication was introduced by the noise made by the apparatus beside the agent, which ticked at every flash; thus the agent might inadvertently transmit each tick in the control as well as each flash in the experimental condition. The relays were therefore enclosed in a sound attenuating box and the strobe lamp cover was a thick muffling blanket. In addition the agent had masking noise presented to him via earphones. This helped, too, to mask extraneous distracting noises and in general to produce a ganzfeld-like environment.

APPARATUS

Two rooms were used, separated by two intervening rooms. The bounding walls were 9.0 m apart. The doors of both rooms were closed during the experiment and the blinds were pulled down. The percipient's room contained a large comfortable reclining arm-chair and the EEG amplifier. This was a DEVICES AC7C with input impedance 5+5 MΩ (plus M2 pen recorder with DC5H driver). A screened cable carried the amplified EEG signal to the agent's room, which contained most of the apparatus. The EEG signal was fed into one channel of a RACAL-THERMIONIC T3000 Mk II tape recorder. A controlling timer unit was set up such that pauses of 15 seconds were alternated with periods of 75 seconds, during which a strobe lamp flashed once every two seconds. A pulse marking each strobe was fed to the other used channel of the tape recorder. The timer unit relays were enclosed in a sound-attenuating box. The strobe

lamp was a STROBOSUN type 1203B, with flash duration of 5-10 μS. The lamp stood on a thick opaque cover which could be pulled around it. In addition, a noise-generator supplied stethoscopic earphones. A LINC-8 computer was used to process the EEG. The programs are described later.

SUBJECTS

Subjects were obtained by personal enquiry and by the subsequent spread of information. Each subject was asked to bring with him someone he knew. Most subjects were university students and in many cases at least one of the pair was known to the experimenter. Accounting for the usable data were 9 male percipients (6 male plus 3 female agents) and 11 female percipients (7 male plus 4 female agents).

PROCEDURE

Prior to the scheduled arrival of each pair of subjects the apparatus was readied and the gain of the amplifier was set. After arrival in the agent's room and a general chat the subjects were told about the experiment and its background in a positive way, concordant with E's own opinion at the time. They were then asked to decide who was going to play the parts of agent and percipient. The percipient sat in front of the flashing lamp a minute or so to familiarize him with what the agent would be experiencing. The pair were then asked to choose some easily visualizable image which the agent would send on every light flash. The images used ranged from simple geometric shapes (circle, triangle) and a few auditory ("treacle" - a dog's name) to "a goose taking off".

Whitton (personal communication) had stressed the role of the agent in the experiment, describing in detail the procedure he should follow. This information was given to the agent (appendix) and he was encouraged to practice in front of the lamp until he felt confident he had mastered the procedure. The percipient was advised to relax with eyes closed and maintain an intense curiosity about what was happening to the agent (appendix).

The electrodes were then applied. After thorough cleaning of the sites with acetone, three freshly chlorided silver cup electrodes backed with epoxy were attached to the percipient's scalp with collodion. Electrode gel was injected into the cups and each site was punctated until the DC resistance between electrode pairs was less than 10 Kilohms, preferably below 5. The electrodes were sited at approximately C_z and O_2 according to the International 10/20 system, but avoiding local scalp features. The third (earth) electrode

was fixed mid-way between them.

Agent, percipient and experimenter then made their way to the percipient's room and the EEG set-up was checked by taking a pen-record. The agent thus became familiar with the percipient's situation. After a last discussion the percipient made himself comfortable on the reclining chair and he was left alone to relax in subdued light with eyes closed for the half hour of the experiment.

On returning to the agent's room he was seated in front of the strobe lamp with eyes closed, in subdued light and with noise piped into his ears via stethoscopic headphones. The tape recorder was adjusted and recording was started. The controlling timer was switched on and in the 15 second delay before the lamp started to flash a packet containing a list of eight randomly ordered pairs of target order was opened. A pile of these had been prepared some months before the experiment by a colleague (R.B.), who generated them using a computerized RNG (Millar and Broughton, 1976). If the first target was EXPERIMENTAL the cover was removed from the strobe lamp, allowing the light to reach the agent, while CONTROL indicated that the lamp was to be covered up. The same procedure was followed for each of the 16 periods.

On completion of data collection the percipient was fetched and his electrodes were removed. The subjects were thanked for their help and asked to drop in in a few weeks if they wanted to find out how things went, though few did so.

In summary the technical data were as follows. Right occipital (O_2) EEG was taken relative to vertex (C_z). The amplifier time constant was set at the rather low value of 0.03 S, for conformity with Lloyd: the high frequency filter was set to 25 Hz, the -3 dB point, dropping -6 dB per octave thereafter. Eight randomized pairs of experimental and control periods of 75 seconds were presented: in each experimental period the agent was stimulated with a strobe every two seconds. This timing ensured that slightly more than the 32 flashes required for averaging were produced per period.

The data from one percipient were discarded for technical reasons: another pair was used to make up the total number to the pre-specified 20. Another percipient believed the experiment was over and prematurely disconnected himself from the amplifier: this session was re-started using a fresh target order.

EEG AVERAGING

The averaging program, of obscure origin, written in LINC assembly code, was implemented and modified by A.H., a collegue from the

Computer Science department. Subsequent tailoring of the program to the particular needs of this experiment was done by the writer.

Data were played back from tape at double speed, thus minimizing transcription time. On input of a strobe pulse the EEG signal was regularly sampled during the following 509 mS (EEG time). 512 points were sampled on every sweep. The program added these values point for point for the first 32 strobes of each period and stored the resultant curve on DEC-tape. This process was automatically repeated for each of the 16 periods.

A second assembly code program, AVEDIT, written by A.H. and subsequently extended by the writer allowed the inspection and manipulation of the curves. The computer did not 'know' which curves were experimental and which control and therefore it was unlikely that any programming error could produce spurious results. At this juncture eight keyboard-specified curves were added together point for point and the composite curve was stored on DEC-tape: these were the eight experimental periods as determined by the target list. The eight control periods were dealt with similarly.

For every subject, then, the data were reduced to eight pairs (E and C) of 500 mS curves, each curve based on 32 sweeps, plus one pair of composite curves, each curve based on all 256 sweeps.

STATISTICAL HYPOTHESES

The claim made by Lloyd was that an EP would appear in the experimental periods. That the form would simulate an auditory EP seemed even then somewhat implausible. How can an EP be distinguished from a non-EP? One way is to see if the peaks and dips on the curve are bigger. The specific measure chosen was the variance. Not only is this familiar but it is also directly proportional to a simple physical quantity, i.e. power or energy per time unit. The objective measure used was not the variance calculated from all 512 points but only from the interval 50 to 450 mS (points 51 to 453): sensory EP's (Regan, 1972), like Lloyd's two psychic examples, appear within these limits. The mean around which the variance was calculated, however, was derived from all points.

For each subject the variance was calculated for each of the 16 period curves and the two composite curves, using a FORTRAN program written by the experimenter.

There are two ways in which such an experiment might succeed overall, depending on the distribution of ability in the population:
a) a few individuals might exhibit large effects (the psychic star hypothesis)
b) most subjects might contribute a little (the democratic psi hypothesis).

Different analyses are necessary to test these two possibilities. The psychic star hypothesis was to be checked by calculation of the binomial probability associated with the observed number of subjects obtaining individually significant results. The test of success for each subject was to involve a Wilcoxon T comparing his matched pairs of experimental and control periods. The original employment of randomized pairs rather than random assignment allowed the partial elimination of the effects that passing time as such might be expected to have on the EEG and hence allowed a more powerful test. Any subject pairs showing individual significance were to be re-tested.

The democratic psi hypothesis was to be put to trial on the basis of the pairs of composite curves, each representing all 256 sweeps. The hypothesis to be tested was that there was a difference between E and C in terms of the variance measure, over all 20 subjects. It was planned to test this using Wilcoxon T. First, however, the data were to be transformed so that differences between subjects in brute power would not bias the result. A logarithmic transformation seems appropriate to establish the desired ordered metric scale (Siegel, 1956, p.76): this normalizes the experimental data for each subject with respect to his control.

Although, in practice, the variance measure regularly produced significant T-values of zero for a single subject when a sensory (photic) stimulus was substituted for an extrasensory one, it was thought possible that there might be subtle psi-determined differences which would not be revealed by the variance measure (the subtle psi hypothesis). The periodicity of the curve, for example, might be affected without changing the variance. Since pattern discrimination was not readily feasible on the machine available, recourse was had to blind assignment of each of the pairs as EC or CE. The significance of the result would be determined from the number of pairs correctly assigned from the total 160, by the normal approximation to the binomial. A confidence rating was also to be secured but was for interest only.

A colleague (R.B.) was given practice in discriminating weak sensory EPs from controls. He then blindly assigned the pairs of curves as displayed on the computer 'scope by AVEDIT: he also ranked his confidence in each assignment on a three point scale.

In summary three statistical hypotheses were to be tested:
1) Psychic star hypothesis on variance measure - binomial test of number of significant outcomes of 20 individual T-tests.
2) Democratic psi hypothesis on variance measure - T-test of transformed power measures from composite curves for each subject.
3) Subtle psi hypothesis - normal approximation to binomial of number of correctly assigned pairs.

RESULTS

1) None of the 20 individual T-tests yielded a significant result (table 1). This is not significantly different from the MCE of 1. There is thus no evidence of any star subjects.
2) The overall test on the 20 matched pairs gave a T of 71, in the opposite direction to hypothesized, where $T(2p \leq 0.05) = 52$. There is thus no evidence for a small effect consistent over subjects.
3) The number of curve-pairs correctly assigned, broken down by confidence rating, is reproduced as table 2. It will be seen that there is no evidence of a subtle psi effect.

TABLE 1

Objective measures for each subject

Subject	E	C	T	
			hypothesized direction	opposite direction
1	0.368	0.342		14
2	1.04	0.943	8	
3	0.564	0.763		15
4	1.13	1.51		16
5	0.543	0.644		16
6	0.641	1.15		16
7	0.338	0.544		17
8	0.442	0.510	13	
9	0.848	0.410	15	
10	0.295	0.280	10	
11	0.396	0.509	14	
12	0.434	0.510	--- 18 ---	
13	0.373	0.420	--- 18 ---	
14	0.211	0.153		12
15	0.233	0.300	7	
16	0.290	0.255	15	
17	0.202	0.190		16
18	1.09	0.577		17
19	0.339	0.440		17
20	0.362	0.457		9

$T(2p \leq 0.05) = 4$

TABLE 2

Blind judging results

Confidence rating	right	wrong	total
min 1	55	60	115
2	18	20	38
max 3	4	3	7
Total	77	83	160

There is no evidence of any psi effect whatever in the three planned analyses.

In addition to the individual T-values (8 in the hypothesized direction and 12 opposite), given in table 1, two other columns appear. These are derived from the variance measures of the composite curves, each based on all 256 sweeps. The data reproduced represent not the variances but the standard deviations directly in microvolts. In the control condition these range from about 0.2 to 1.5 μV. This gives some idea of the sensitivity of the experiment. It will be noted that in eight cases E is larger than C, as expected on the psi hypothesis and the reverse in the remaining 12. This experiment involved a total of 5120 sweeps in the experimental condition as against Lloyd's 120. Assuming approximately $\sqrt{N}$ enhancement this work should be capable of detecting a signal less than a sixth the size of that just discernable in Lloyd's.

POST HOC ANALYSES

Post-hoc analyses have no value beyond yielding hints for future work. In this field, however, even (negative) hints may be worth passing on.

It might be that any EP produced is not constant, as assumed. If it gradually changes with time so that what was a peak becomes a trough and vice versa, there might be no effect observable in the composite curve (the drifting EP hypothesis). If this is so and the period of variation is greater than say three minutes, then differences should be found between each of the E/C pairs, though the differences will be different in form for each pair. To check

this the number of pairs in which the variance measure was greater in E than C was evaluated for each subject: the number in the hypothesized direction ranged from 2 to 6 out of the 8 pairs. Over all 20 subjects the number correct was 79 as compared to MCE of 80. Thus no support was given to the post-hoc speculation of a drifting EP.

Another possibility is that EPs appeared initially but rapidly declined. To check this with respect to the variance measure the difference E-C was calculated as an index of EP presence in each of the eight matched pairs for each subject. These difference scores were subjected to a Friedman two-way analysis of variance by ranks (pair x subject) to determine whether there was any trend of the index of EP presence over the eight pairs. The sum of ranks over all subjects for each pair is displayed in table 3. This distribution

TABLE 3

Post hoc analysis for declines

Pair	Variancedifference sum of ranks	Judged pairs correct
1	94	8
2	98	10
3	74	5
4	92	12
5	109	12
6	81	12
7	76	7
8	96	11

MCE=90

yields $\chi^2=8.45$, with 7 df. This is not significant, nor is a trend discernable by eye. The number of pairs correctly assigned on the basis of blind judging is similarly broken down by curve pair in table 3. Again no evidence of decline is apparent. There is no indication of declines at all in these data.

Another possibility was that the subjects paid no attention to the agent but reacted directly to the lamp flashing, then so far as the EP is concerned both E and C would be identical. This seemed a

very tenuous possibility. One way partially to check it is to compute the (±) reference (Schimmel, 1967). This is calculated in the same way as the averaging process described above, except that every second sweep is subtracted rather than added. The signal thus tends to cancel out, leaving an estimate of the noise only. Consequently if any time-locked signal is present the variance measure in the average should be larger than that in the (±) reference from the same data. One drawback of this procedure is that any time-locked signal at all will be revealed and not merely an EP. While great care had been taken to minimise any electrical pick-up, it could not be assumed without further empirical test, that this had been reduced below the level of detectability. As a result only a negative outcome could be relied upon: a positive result would entail setting up an experiment to check for artifact.

Because of the amount of labour involved it was decided to select three subjects at random and only if there was a suggestion of a real difference, to proceed to the remainder. The random selection will not be described here since it is not critical. A variant of the averaging program was used to calculate the (±) references for these data. For each selected subject a Wilcoxon $\underline{T}$ was applied to determine if there was any real difference between the variance measure in the 16 averages and the 16 (±) references. None of the three $\underline{T}$s was significant: two were in the hypothesized direction and one opposite. Of the 48 pairs (three sets of 16) compared, 23 were in the hypothesized direction. Thus there was no evidence of the presence of a time-locked signal of any kind.

DISCUSSION

Absolutely no evidence of telepathically evoked potentials was obtained. The experiment was capable of detecting a signal a fraction of that claimed by Lloyd. Moreover the psychological conditions were excellent: many of the subjects were well-known to E: a number were interested enough to take home pieces of EEG.

There were, of course, some minor imperfections which should be mentioned. Because the laboratory was sited over the main supply for the building, 50 Hz could occasionally be seen among the EEG but with careful earthing to a single point it was not troublesome. After the experiment each agent was asked if he had heard the clicking of the lamp: two had. On experiment it was found possible for the agent to distinguish the clicks when they were marked by the flash but not otherwise. Nonetheless, the possibility of some subliminal stimulation of agents by clicks cannot be dismissed out of hand. Any such stimulation would reduce the sensitivity

of the experiment somewhat. If Lloyd is to be believed, however, any such passive reaction to stimulation would not in any case give rise to a telepathic signal.

Where does this leave the Lloyd effect? There are two possibilities, either his result depended upon some special percipient/agent/experimenter or it was artifactual. I have myself little doubt that his results are the product of a lamentably inadequate methodology coupled with a rose-tinted oscilloscope. Pseudo-EPs of the type claimed to show psychic transmission are not uncommon. Subjects who produce long trains of high amplitude alpha waves (errors which covary so far as averaging is concerned) are particularly good at producing results of this sort. The selected best from the 320 curves of this experiment reveals a magnificent Lloyd-like 'EP'. The initial negativity at 129 mS and the following positivity at 170 mS stand out well above the rest of the curve. Peak to peak the amplitude of 12.2 μV lies in the upper range of my collection of sensory EPs to fairly intense photic stimulation and makes Lloyd's best of 6.2 μV look rather puny by contrast.

Whitton (personal communication), while admitting the artifactual nature of the Lloyd effect, has suggested that it is an artifact, itself of psi origin. He refers to a paper (Whitton, 1974) in which he purports to show that the power spectrum from the EEG of three specially talented psychics, when producing apparently paranormal effects, assumed the form of a ramp: the energy was concentrated in the low frequencies. If I understand his argument correctly Whitton suggests that the agent's efforts produce the same kind of ramp spectrum in the percipient. Averaging of these slow regular waves leads to considerable uncancelled activity which is taken for an EP. This is not the place to discuss Whitton's claims of a ramp spectrum beyond pointing out that the work of other parapsychologists has not confirmed it. One would surely like to see some evidence, easy enough to obtain one might think, that the spectral distribution of a percipient's EEG can be affected by a remote agent, irrespective of what the agent's brain is doing at the time.

In any case, a change of rationale affects the results not at all. If the 'EPs' were produced by a psi-based artifact in Lloyd's case, then the same should have shown up in the work reported here. There seems every reason to attribute the Lloyd effect to a very well-known non-psi phenomenon, namely wishful thinking.

SUMMARY

Lloyd's (1973) claim of a telepathically evoked cortical potential was put to the test in an experiment with 20 subject pairs. Experimental periods (when an agent transmitted) did not differ from

control periods on either an amplitude measure or blind judging. Evidence is presented suggesting that the "Lloyd effect" was due simply to the hazards of non-blind visual inspection.

ACKNOWLEDGEMENT

During the performance of the experimental work the author was Perrott Warrick Student in Psychical Research (Trinity College, Cambridge). It was carried out in the Department of Psychology, University of Edinburgh, under the aegis of Dr. John Beloff.

I owe a special debt of gratitude to Anne Heinrichsons, who took on the LINC-8 (and won!). Nor was her brain output restricted to programming but above and beyond the call of duty she personally generated my very first evoked potentials.

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APPENDIX

INSTRUCTIONS TO AGENT

Because the information sent really acts as a timing signal it is extremely important that you visualize your image the very moment the light flashes and that you should do exactly the same thing every time. The image should be in mind as clearly as possible just for an instant and should be vivid and there should be an intense desire to have it transmitted. Then it is dismissed from mind and let go completely as you try to make your mind completely blank just for an instant. Thereafter, just relax and think about anything unrelated to the experiment or what you have just transmitted, until the next flash.

Remember Image, Blank, Relax.

INSTRUCTIONS TO PERCIPIENT

I want you to remain passive throughout. You will sit with eyes closed, as relaxed as you can get without actually falling asleep and you should avoid excessive movements, especially of the head and eyes and there should be as little muscular tension as possible, especially in the jaw and scalp. On the other hand don't sit all uptight trying to avoid all movements, just don't thrash about that's all. You should maintain an intense curiosity about what's happening to the sender. Is he sending the image? Try to feel what he is feeling. In imagination try to be him so that it is you who are sitting in front of the lamp.

DELAYED PK WITH MATTHEW MANNING: PRELIMINARY INDICATIONS AND FAILURE TO CONFIRM

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During the spring of 1977 a number of exploratory psi experiments were conducted with Mr. Matthew Manning at the University of California, Davis. This report describes the series of experiments in this group that the first author (J.P.) was primarily responsible for.

While living in Davis, Mr. Manning developed a unique rapport with a female graduate student, hereafter referred to as "Miss X", who was interested in parapsychological research. Mr. Manning believed that he could influence Miss X in subtle ways; e.g., make her scratch her ear by concentrating on her doing this. Miss X reported unique sensations or "energy" in Mr. Mannings presence, and she reported similar sensations and electrical equipment behaving erratically in her presence several weeks after he had left.

This relationship encouraged us at the time to pursue a series of experiments to determine whether Mr. Manning could transfer his psychic abilities to Miss X, who had demonstrated no such abilities previously. Specifically, we assessed Miss X's performance on a forced-choice ESP task.

PILOT EXPERIMENTS

All experiments to be reported in this article were conducted on a machine called ADEPT (Redington and Tart, 1976). A random event generator (REG) selects one of ten numbers which is presented to a sender on a circular display. The percipient, located in a different room, makes his response on a similar display and receives immediate feedback of the correct target after each trial. Because

the automated recording feature of ADEPT was not functioning at the time of the experiment, targets, responses, and hits had to be recorded by hand.

The initial pilot testing consisted of simply determining whether two volunteer subjects, not including Miss X, could achieve significant positive scores on ADEPT with Mr. Manning attempting to send them energy. J.P. served as agent. The results were disappointing, but Mr. Manning did not feel the rapport with these two subjects that he felt with Miss X.

Pilot testing with Miss X began with a control run, which she requested. Mr. Manning sat in J.P.'s office while Miss X completed one run (20 trials) on ADEPT. J.P. and C.T.T. were in the sender's chamber (a floor plan of our laboratory is presented in Figure 1), J.P. serving as sender and C.T.T. as recorder of the data. Miss X obtained seven hits, the highest number of hits ever obtained on a single ADEPT run to our knowledge (exact P = .004). This run was immediately followed by three experimental runs, which were conducted similarly to the control run except that Mr. Manning now lay on the bed in the receiver's room attempting to send energy to Miss X. Her scores on these runs were 4, 3, and 2, respectively.

The next "pilot" experiment was originally intended to be part of a formal series consisting of five sessions and four runs per session. Each run within a session was to be conducted in one of four conditions identifying a different mental state on Mr. Manning's part:

(1) Switched On/High Aim. Mr. Manning sends energy to Miss X intending her to obtain a large number of hits.

(2) Switched On/Low Aim. Same as (1), except Mr. Manning intends Miss X to obtain no hits.

(3) Mind Clear. Mr. Manning tries to clear his mind during the run. This is the state he said he was in during the run where Miss X had obtained seven hits.

(4) Reading (Control). Mr. Manning reads a magazine during the run.

The order of runs within a session was determined in a pseudo-random fashion. Prior to the experiment, Mr. Manning devised a code translating each condition to one of the four playing-card suits. Two copies of the code were given to J.P. One copy was placed in a sealed opaque envelope that was not opened until the entire experiment was over. The second copy was made available to Mr. Manning before each session to refresh his memory.[2] No one else was shown the code until the experiment was over. Before each

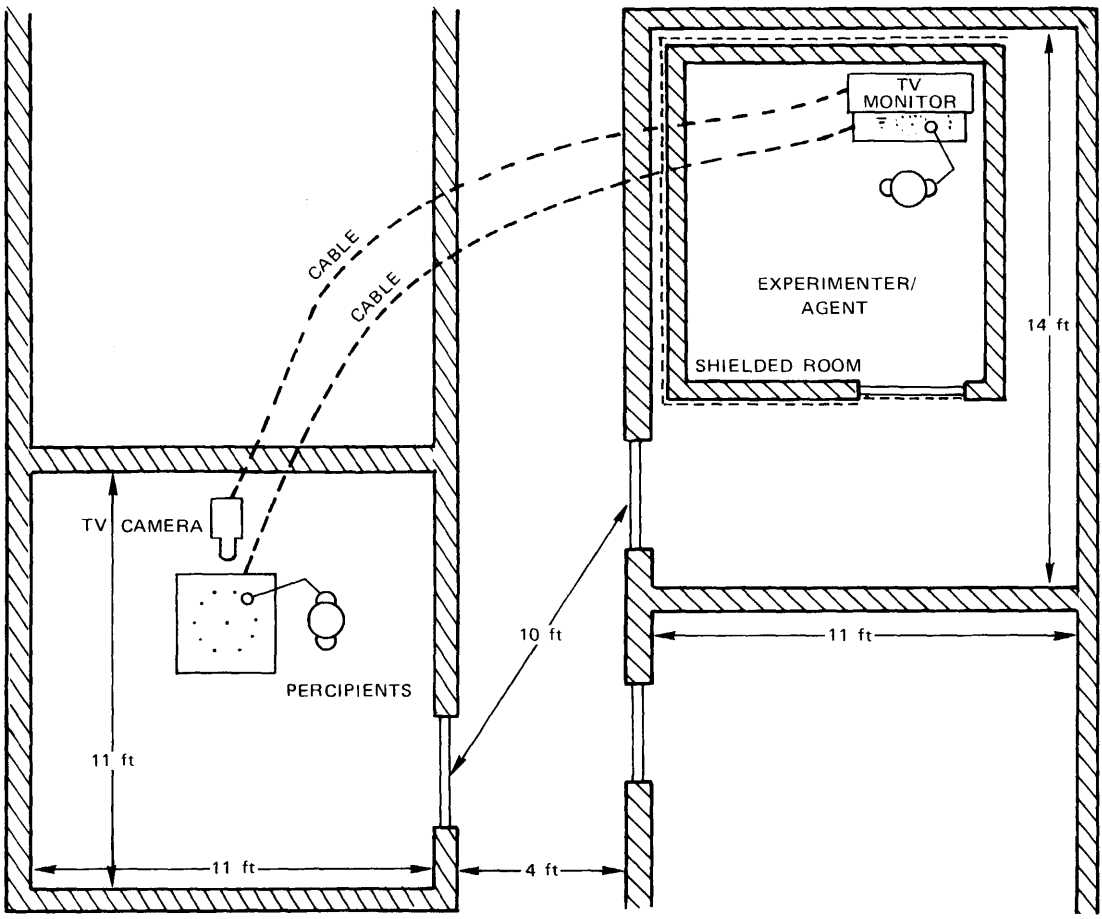

TV
MONITOR
CABLE
CABLE
EXPERIMENTER/
AGENT
SHIELDED ROOM
14 ft
TV CAMERA
PERCIPIENTS
10 ft
11 ft
11 ft
11 ft
4 ft

FIGURE 1

session, D.R. had Mr. Manning pick playing cards from a standard shuffled deck until one card of each suit had been selected. The order in which the suits were picked determined in a straight forward manner the order of conditions within the session.

The design called for Mr. Manning to remain in J.P.'s office during the runs. Pilot testing had suggested that good results could be obtained with Mr. Manning in a different room than Miss X, and this procedure had the added advantage of preventing Miss X from obtaining sensory cues that might provide information about the order of conditions. J.P. served as sender and D.R. as recorder.

The results of the first session were disappointing: only seven hits over four runs, one less than expected by chance.

At this point, Miss X insisted that the experiment would not work unless Mr. Manning was in the room with her. J.P. thus decided to abandon the remaining planned sessions. Another pilot session was then conducted, duplicating the procedure of the previous experiment except that Mr. Manning now lay on the bed in the percipient's room.

The results of this second session were somewhat more promising, at least to the extent that Miss X obtained five hits on the high-aim run. None of the results were statistically significant, however.

EXPERIMENT ONE

METHOD

A formal experiment was undertaken consisting of six sessions. The procedure was essentially identical to the last pilot session, with Mr. Manning and Miss X in the same room. Miss X was seated with her back facing Mr. Manning, but there is no way to conclusively rule out the possibility of subliminal awareness of sensory cues that might have conveyed information about the order of conditions. Although the experimenters could see, by means of the video apparatus in the sender's chamber, the area surrounding ADEPT's receiver console in the receiver's room, they could not monitor the room totally. At this stage of the investigation, it seemed more important to promote Miss X's confidence to see if significant results could be obtained. If we were successful, more elaborate procedures to eliminate sensory cues could be introduced in replication attempts. It should be stressed that we know of no way either Mr. Manning or Miss X could have obtained information about the target sequence by sensory means.

Each session consisted of four runs, conducted in succession without intervening breaks. Miss X preferred to respond quickly, so intertrial intervals were made as short as possible, generally less than 10 seconds.

J.P. and D.R. were in the sender's chamber, with D.R. serving as the official sender. Each made independent records of targets and responses. The two records agreed perfectly except for two trials, both of which had been correctly designated as misses.

The six sessions were conducted in three blocks of two sessions each. The blocks occurred on the afternoon and evening of May 30 and the afternoon of May 31. Two pre-designated "pilot" sessions, not included in the formal analyses, were conducted on the afternoon of May 31.

RESULTS

The results, illustrated in Table 1, reveal generally chance scoring, the only exception being session five. In this session, Miss X obtained five hits in each of the two conditions where psi-hitting was to be expected, the high-aim and mind-clearing conditions. However, analysis of variance revealed no significant effects (See Table 2).

Perusal of the target sequences from the experiment suggested a possible singlet bias which proved to be significant upon analysis of the sessions pooled (χ^2 (9) = 17.02, $p<.05$); the most notable feature of this bias was an excess of eights. This eight bias was primarily attributable to two runs which contained seven and eight eights, respectively. Each of these runs was the fourth run of the first session of its block.

TABLE 1

Number of hits on ADEPT per 20-trial run
as a function of mental state (Exp. I)

Session	High-Aim	Low-Aim	Mind Clear	Control	Total
1	1	1	0	1	3
2	4	2	0	0	6
3	1	2	0	2	5
4	1	3	2	0	6
5	5	1	5	2	13
6	0	4	0	1	5
Total	12	13	7	6	

TABLE 2

Summary of Analysis of Variance

(Exp. I)

Source	SS	df	MS	F
C (Conditions)	6.17	3	2.06	0.84
S (Sessions)	14.83	5	2.97	1.21
C x S	36.83	15	2.46	

EXPERIMENT TWO

METHOD

Because of the possibility that Mr. Manning might have exerted a PK effect on ADEPT's REG in Experiment One, an experiment was undertaken to test his PK ability on ADEPT more directly. Mr. Manning's task was to try to influence ADEPT to produce either an excess of eights or an excess of twos (the least frequently occurring target in Experiment One.) The experiment again consisted of six sessions, three blocks of two sessions each. During the first and third blocks, Mr. Manning aimed for eights in the first session and twos in the second. This order was reversed in the second block.

Mr. Manning responded on each trial by pressing the number on the receiver's console that he was trying to cause ADEPT to generate, thus scoring a hit whenever he was successful. As in Experiment One, both D.R. and J.P. made independent records of the target and response sequences from inside the sender's chamber, with D.R. serving as sender. The two records matched perfectly. Miss X was not involved with the experiment.

The runs in each session succeeded each other without a break. Targets were generated as rapidly as possible, consistent with accurate recording.

The first two blocks were conducted on the afternoon of June 1 (separated by about 45 minutes), and the third block occurred on the morning of June 4.

RESULTS

The results did not reveal any significant trends, although there were more eights than twos in the eight-aim condition and the reverse in the two-aim condition.

However, there again was one run in which a single target number was generated eight times. Again, it was the fourth run of the first session of a block--this time, the first block. Curiously enough, now that Mr. Manning was aiming for eights, the high biased target was the nine.

DISCUSSION

Additional analyses of the data suggested a possible explanation of the singlet biases in the two formal experiments. In Experiment One, both biased runs occurred two runs after the "low-aim" run. The low-aim condition is of particular interest because it was the only one of the four conditions in which Mr. Manning attempted to influence ADEPT rather than Miss X directly.[3] Of the 24 runs in Experiment One, there was only one other run that came two runs after a low-aim run but did not appear biased. This run occurred in the second session of a block, whereas both biased runs occurred in the first session of a block.

The above target biases could have conceivably resulted from a delayed PK effect, i.e., a time-lag between attempted PK and its manifestation. Such effects have been reported in other PK experiments (cf., Stanford, 1977) as well as in a metal bending experiment in which Mr. Manning was the subject (Whitten, 1974). Unfortunately, we had not recorded the times at which individual runs began and ended in Experiment One. However, by repeating the experimental procedure informally, J.P. estimated that the interval between the end of one run and the beginning of the run two ahead was approximately 2½ minutes.

We then turned to Experiment Two, where we may assume that Mr. Manning was attempting to influence ADEPT throughout.[4] The biased run in this case was the fourth run of a block, while PK was being attempted as early as the first run. Thus two runs intervened between the initial PK attempt and the manifestation of target bias in Experiment Two, as compared to just one intervening run in Experiment One. This apparent inconsistency was resolved when we noted that there was a delay of approximately 30 seconds between runs in Experiment One that did not occur in Experiment Two. Given this fact, our estimation procedure indicated that the delay between the end of the first run and the beginning of the fourth run in

Experiment Two was approximately 2½ minutes.

To sum up, in each of the three cases where a significant singlet bias (defined as at least seven of one number in a 20-trial run) was found in a target sequence, it followed an attempt by Mr. Manning to influence ADEPT by approximately 2½ minutes. The effect only occurred in the first session of a block. Given these constraints, the effect was noted in three of five possible occasions in the two experiments.

EXPERIMENT THREE

METHOD

On the morning of June 6, an experiment was conducted to verify the possible delayed PK effect discussed above. The design was similar to that of Experiment One, which seemed to have been a little more successful than Experiment Two in eliciting the effect.

Mr. Manning was asked to duplicate his state of mind in the low-aim condition of Experiment One during a single run on ADEPT. Following this run, there was a 1¼ minute break followed by two or three additional runs. The second of these additional runs began exactly 2½ minutes after the completion of the low-aim run.

Mr. Manning was told that during the additional runs he should simply watch the receiver's console of ADEPT but not attempt to influence it. This procedure was introduced because there is evidence suggesting that positive results are most likely to occur if the subject receives immediate feedback of the target sequences (Schmidt, 1976). We did not inform Mr. Manning beforehand of the experimental hypothesis.

There was no reason to believe that Miss X's presence had anything to do with the effect we were attempting to verify, so we did not feel it necessary to burden her with additional experimental duties. (For example, she was not present for Experiment Two where the effect occurred). Thus, a graduate student in the Department of Psychology served as a receiver. Another graduate student served as the timer, and he was stationed in the sender's chamber.[5] As before, D.R. and J.P. were in the sender's chamber making independent records of the target and response sequences, with D.R. serving as sender.

J.P. notified the timer at the end of the low-aim run to start his stopwatch, at which time J.P. immediately left the sender's room and knocked on the door of the receiver's room. This was a signal to Mr. Manning that the run was over and that he should return to a normal or "switched off" mental state. J.P. then returned

immediately to the sender's room. The timer signalled at the end of 1¼ and 2½ minutes to notify J.P. and D.R. to begin the additional runs.

It was predicted that during the second additional run one of the ten targets would appear at least six times. A secondary prediction was that this target would be "eight". The third additional run was to be omitted if the previous run confirmed the main hypothesis.[6]

RESULTS

None of the target sequences showed any evidence of bias at the singlet level. Thus our hypothesis was not supported.

EXPERIMENT FOUR

METHOD

Following a half-hour coffee break, a second attempt was made to confirm the hypothesis. The procedure was identical to that of Experiment Three, except that Mr. Manning was asked on the first run to simply try to influence ADEPT by means of any mental state he chose, not necessarily that of the low-aim condition of Experiment One. Also, Dr. Aaron Goldman, a visiting Professor in the Department of Mathematics, served as timer. A resident Professor in the Department of Mathematics, Dr. Howard Weiner, also was present in the sender's chamber as an observer.

RESULTS

The hypothesis again was not supported, as there was no evidence of singlet bias on the crucial run. However, six fives appeared in the target sequence of the first run, the run during which Mr. Manning was attempting to exert an influence.

DISCUSSION

The failure to confirm the possible delayed PK effect in Experiments Three and Four means that the pattern of post-hoc effects observed in the previous two experiments can only be considered suggesti The strong biases in the target sequences are not evidential by themselves, as they could be attributable to conventional machine artifacts. Any evidentiality they possess derives from their correlation with the mental states of Mr. Manning.

Such biases did not occur in other ADEPT runs conducted before and during Mr. Manning's visit. A string of 1000 control targets produced by D.R. before Mr. Manning's arrival revealed no significant singlet or doublet biases. It yielded only one sequence of four identical numbers in a row, and no longer sequences. Of possibly greater relevance are the results of 38 ESP "training" runs on ADEPT conducted during Matthew's visit a few days before the experiments in question. Mr. Manning served as percipient on 30 of these runs and as sender on the other eight; in none of these runs was he, to our knowledge, attempting to exert a PK influence on ADEPT. A single digit never appeared more than three times in any of these runs. Long strings of single digits have not been noted on ADEPT in experiments conducted soon after Mr. Manning left, but formal analyses of these data have yet to be completed.

If any point stands out in this series of experiments, it is the "perversity" of the few significant effects that were obtained. The clearest example of this perversity is the fact that the only run on which Miss X achieved a significant score was a designed control run during which Mr. Manning was not trying to exert psychic influence. In Experiment One, Mr. Manning was unsuccessful in influencing Miss X's rate of scoring, but the target sequences (which would have needed to be random had we wanted to unequivocably interpret any success that Miss X did achieve) were biased. In Experiment Two, when Mr. Manning was aiming for eights, the bias appeared on nines. Only in Experiment Four did Mr. Manning achieve the effect he was aiming for, but in this case we were predicting a displacement! Displaced PK was also a factor in another experiment with a different device undertaken with Mr. Manning during his visit (Tart and Palmer, submitted for publication).

Needless to say, all this was rather frustrating to the experimenters, not to mention Mr. Manning himself. It is reminiscent of the function of PK in poltergeist disturbances, where uncontrolled PK produces frustrations for those in the immediate environment, albeit of a much greater magnitude than we had to put up with in the lab. Mr. Manning's experience with ADEPT in general was a frustrating one for him, and this frustration could have produced both a reservoir of psychic tension requiring an outlet and a latent or unconscious motivation to channel that tension in a manner that would exasperate those other persons most closely associated with the source of the frustration. The plausibility of this line of thinking is reinforced by the fact that Mr. Manning was the source of large-scale, apparent poltergeist disturbances as a child (Manning, 1974), and by evidence suggesting that poltergeist activity is a vehicle for expressing repressed or suppressed hostility toward others or oneself (Roll, 1977).

Of course, none of these speculations are supported by the kind of solid evidence we would like. If our hunches about the perversity of Mr. Manning's psi are correct, the experimenter is involved in a game of trying to outwit this perversity, a game in which he is at a distinct disadvantage. A more promising approach might be to create experimental situations that are challenging enough to Mr. Manning to engage his PK and not frustrating enough to send it off course. This was our objective in the present project; we hope future experimenters will be more successful in realizing it.

ABSTRACT

A series of forced-choice ESP and PK experiments was conducted with Mr. Matthew Manning. The project was initially designed to have Mr. Manning psychically influence another subject's ESP scoring on a ten-choice random event generator (ADEPT). Planned analyses were non-significant, but a tendency was observed for ADEPT to repeatedly generate a particular target number, beginning approximately 2½ minutes after Mr. Manning attempted to influence it. (Influencing ADEPT was one indirect method he had used to try to effect the other subject's ESP scoring). Two confirmatory series failed to replicate this finding.

NOTES

1 These effects suggest caution to anyone who may wish to conduct an experiment of this type in the future.

2 Mr. Manning insisted that none else besides himself know the code, lest that person be a possible source for an effect on Miss X.

3 Mr. Manning's instructions were simply to cause Miss X to get no hits. He chose to accomplish this objective by causing ADEPT to pick a number other than the one she would pick.

4 Although the object of Mr. Manning's focus was the same in Experiment Two as in the low-aim condition of Experiment One, we cannot say this with equal confidence regarding his state of mind. Before Experiment Three, described below, J.P. asked Mr. Manning in which of the four conditions of Experiment One was his state of mind most similar to that he entered in Experiment Two. We hoped he would say the low-aim condition; instead, he

said the high-aim condition.

5 Our thanks to Mr. Henry Bennett for serving as the receiver and Mr. Fernando Fleitas for serving as the timer.

6 This procedure was introduced to cover the possibility that the effect occurred only on the last run of a block. The rationale behind this decision is complicated and will not be pursued further here.

ACKNOWLEDGEMENT

We wish to express our appreciation to Mr. Manning for generously devoting his time and energy to these experiments.

At the time these experiments were undertaken, Dr. Palmer was Assistent Research Psychologist in the Department of Psychology at the University of California, Davis.

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ANALYSIS OF SPONTANEOUS CASES AS REPORTED IN 'PHANTASMS OF THE LIVING'

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University of Utrecht

Compared to the amount of experimental work carried out, analyses of spontaneous cases appear to be somewhat unpopular in parapsychology: relatively few of such analyses have been published. Most outstanding amongst them are the studies by the British S.P.R., Louisa Rhine and Ian Stevenson. Although these studies contain highly interesting material their impact on experimental research seems to be little. One might wonder what the reason for this is.

It might be that experimentalists are less interested in spontaneous cases, because contrary to the nature of their work the conditions under which these phenomena take place are rarely well controlled. It is noteworthy that amongst those who have ever published a study on this subject hardly one can be found who also has a longstanding record of having been active in pure experimental work. Another reason is, undoubtedly, that it is still uncertain to what extent findings from analyses of spontaneous cases are explainable by non-ESP factors like for instance reporting-effects. A third reason could be that experimentalists consider the outcome of analyses of spontaneous cases as unsuitable for application to experimental work. In general such analyses yield a broad picture of how experiences suggesting ESP can influence the daily life of people. However, an experimenter needs hypotheses on which to base his work that can be operationalized in an experimental design. Or he is interested in findings of relationships between variables, which can be tested in his lab or may be applied to support or refute a theoretical model. It is true that studies on spontaneous cases hardly ever provide such clear-cut hypotheses or relationships, at least not in a form which lend themselves readily to incorporation into an experimental study.

Whatever the reason for the relative neglect of this area, I am of the opinion that spontaneous cases deserve more interest from experimentalists. Firstly, the results of analyzing such cases might be of importance both for designing models and for the construction of testable hypotheses. Secondly one can argue that spontaneous cases provide the very legitimation for research in parapsychology. After all if it were not for the fact that people claim to have experiences which they consider paranormal, there would have been no reason to start such research.

Spontaneous cases are suggestive of a possible unknown process of communication called ESP. It is this concept that parapsychologists consider as the central issue of their research. On the other hand, numerous critics reject ESP as so utterly inlikely (see for instance Hansel, 1966) that they feel that all research in this area is a priori meaningless, so long as it is not proven that such a phenomenon exists. Thus, parapsychologists have often tried to deliver such proof, but were each time met by firm rebuttals from the critics who were simply not convinced. Hansel's position nicely illustrates why these efforts were virtually bound to fail. His working hypothesis is that ESP is impossible and thus concludes that any other possible alternative explanation for the observed findings, including conspiracies and fraud if everything else should fail, must be accepted simply because it is more likely. From his point of view this is a very strong and convenient position. Stretched to the limits it forces the proponents of the existence of ESP to prove in every instance that any alternative hypothesis the critic is able to invent cannot explain the results. As we all know to deliver such proof is impossible, equally impossible and for the same reason as it would be for Hansel to prove that ESP does not exist.

It is arguable that much of the fundamental criticism against parapsychology can be traced back to this very point, the claim that the existence of ESP should be proven first before we are to be allowed to do research in our field. But parapsychologists cannot prove that ESP is the only possible way to explain the observations, mainly because ESP is a 'negative' concept (Schouten, 1973): it is at present without content. We are not able to say what ESP is, all we can say is that in a given experiment the most likely alternative explanations for the observed result can be ruled out and we therefore conclude that an assumed process called ESP has been responsible for the observed effects. Hence critics can attack on two fronts. First they can refuse to accept ESP unless we are able to tell them what it is and how it works. In this category we find critics

asking for well defined theories, repeatable experiments (which in our case requires knowledge about the process), etc. Secondly so long as we cannot meet this requirement they can demand that we prove that all other possible interpretations are not valid. Exponents of this view include not only critics like Hansel (1966), but also Spencer Brown (1953, 1957), Bridgeman (1956), and various others assuming faulty experimentation, wrong application of statistics, recording errors, etc. They all do not so much dispute the observations, but merely express their belief that some alternative interpretation of the data is more acceptable to them.

However, regardless of whether ESP exists or not, all human experiences and activities are a legitimate subject for scientific inquiry, including such human experiences as are found in spontaneous cases. Undeniably we are interested in the problem of whether ESP exists and how it might function: we are allowed to do so as long as we accept that reality is our primary arbiter. The important issue in the first place is not to prove whether ESP exists, but whether the concept of ESP is a more suitable working hypothesis for studying spontaneous cases and experimental results in parapsychology than other possible concepts or hypotheses.

I feel that we ask too much if we insist that others accept our claim that ESP exists before being able to tell them anything about what it is and how it works, or before we can demonstrate the phenomenon in a repeatable way (which in our case clearly requires that we know to a large extent how it works). On the other hand, the critic is equally asking too much when he states that we should first prove the existence of ESP before being willing to take research in this field seriously: just as hardly anyone would demand from a psychologist that he first prove the existence of the subconscious before he is allowed to apply this concept to explain certain types of behavior. But the scientific community can rightly ask us to demonstrate based on the material available that possible non-ESP based concepts and hypotheses are less suited to explain the phenomena observed than the concept of ESP.

If we are able to do so, and if our experimental work is of such quality that it stands comparison with the quality of research in other branches of social science, then we are in a strong position to defend ourselves against the occasional attacks challenging research in this field. I would also like to point out that historically progress in science has often been served by introducing concepts markedly different from the

accepted ones. Unless it clearly contradicts observed facts, there is hardly ever reason to reject a new concept a priori. It is rather its usefulness to the scientific enterprise which eventually decides whether such a concept is better jettisoned.

The aim of the present study is twofold. If spontaneous cases are to provide the basis for experimental research focussed on the concept of ESP, then we should present arguments based on the data demonstrating why this concept is to be preferred over possible non-ESP based concepts or hypotheses. Hence I will analyse the material to study whether such arguments, or counter-arguments, can be found. As long as it does not turn out that likely non-ESP explanations and other sources of errors are responsible for the observed findings I will treat the data applying the ESP concept. Consequently I will try to find relationships from which hypotheses can be derived which lend themselves to application in experimental research.

From the discussion above it follows that I consider spontaneous cases as, in the first place, a collection of human experiences with a common property, namely that the persons who reported these cases felt that those experiences had a paranormal character. The question arises as to why those persons who must have had numerous 'normal' experiences in life single out one or a few of these and attribute a paranormal character to it. It might be that they are justified in their belief that some unknown paranormal process has been responsible for these experiences, but other explanations are also possible. In the first place the material itself should provide us with clues as regards the hypotheses to investigate. Because the contributors themselves considered these experiences as clearly different from normal ones, it seems worthwhile to take seriously the possibility of an ESP contribution to these data. Therefore, when in the following I use terms like for instance 'ESP factors' I do this for convenience sake and not because I want to claim that it is proven that these cases are paranormal, or that an ESP process exists. If, however, it can be shown that based on the data available it is unlikely that non-ESP factors are responsible for the patterns observed in the data, then we can legitemately ask what these patterns suggest about the ESP process.

If these cases are not just random experiences we can expect to find some common properties other than the attribution of a paranormal character. However, as stated before we should take into account that variables like the effect of time on memory, unreliable testimonies, reporting effects, etc.,might have

influenced the reports and might yield better arguments for explaining the cases than applying the ESP concept. Hence when possible I will examine these effects to see whether the observed relationships can be explained by such factors. A second approach to investigate the effect of such variables will be to single out the better witnessed cases to see whether the observed findings still hold for these cases. If so we might conclude that such findings are relatively independent from the degree of corroboration of the cases, and are therefore not likely to be caused by such non-ESP factors as mentioned above. Besides discussing the findings based on an assumed ESP process I will mention some possible alternative hypotheses of a non-ESP character.

The Analysis

Since I do not possess a collection of spontaneous cases of my own I have taken the collection published in 'Phantasms of the Living' (Gurney, Myers and Podmore, 1886). One advantage of this collection is that the investigators have tried as far as possible to collect testimonies from witnesses to check for the accuracy of the content of the reports. Hence in a fair proportion of cases witnesses other than the percipient (the person who experienced the alleged paranormal impression) or target person (the person to which the experience was related) provided statements corroborating that the experience and the event (what actually happened to the target person) took place as was described in the report. For these cases the possibility of unreliable reporting is at least to a certain extent eliminated. On the other hand a disadvantage of this collection is that only telepathy cases (cases in which the time between experience and event was less than 24 hours) have been included.

'Phantasms of the Living' contains 712 numbered cases, of which 150 cases were not included in this study because 1: some cases are not spontaneous cases but descriptions of various experiments (for instance the cases 1 to 16); 2: some cases contain so few data that in nearly all scoring categories 'unknown' had to be scored (for instance the cases 253, 677); 3: some cases being reciprocal (for instance 641 to 646); 4: in some cases an event was lacking (for instance 254, 255). The data of the remaining cases were scored in 32 categories. The details of the scoring has been presented elsewhere (Schouten, 1979).

The categories applied are:

1: Number of the case as given in the 'Phantasms'
2: Sex of percipient
3: Age of percipient
4: Family situation of percipient
5: Sex of target person
6: Age of target person
7: Family situation of target person
8: Relationship between percipient and target person
9: Type of experience
10: Action taken by percipient
11: Conviction of percipient
12: Identification of target person
13: Identification of event
14: Number of details given in the experience
15: Number of correct identified details
16: Time of day of experience
17: Month of experience
18: Percipient being alone or not when having the experience
19: Number of witnesses to the experience
20: Experience indoors or outdoors
21: Seriousness of event
22: Target person focussing
23: Reason for percipient to think about target person
24: Number of witnesses of event
25: Time between experience and event
26: Time between event and reporting
27: Who sent in the report
28: Length of report
29: Probability of event
30: Feelings of percipient
31: Particulars of percipient
32: Distance between target person and event

In most analyses chi-square tests have been applied, because the data do not lend themselves to the application of more sophisticated techniques like multivariate analysis of variance. This implies that we have to interpret the data taking into account various possible interactions between different variables. Unless stated otherwise all p-values given are two-tailed. Slight differences in totals will be noticed for the numbers reported in the various tables below. These are not counting errors but due to the fact that in most cases one or more categories could not be scored for lack of data.

In each analysis only those cases have been included in which all variables have been scored.

THE ANALYSIS

The percipient

The distribution for male and female percipients for all cases is 246 male percipients (43.8%) against 316 female percipients (56.2%). Assuming that males and females were equally distributed in the population, this difference is significant ($z=2.95$, $p=.003$).

This result suggests that females are better percipients than males. However, there are a number of possible other explanations for this effect. It might be, that the assumption of an equal distribution of males and females in the British society in those days does not hold. It could also be, that the effect is related to a specific type of relationship in which women dominate. For instance, cases in which mothers get an impression of something happening to their child. To look elsewhere, it may be that it is simply a reporting effect. Perhaps females are more inclined than males to report a spontaneous case. Considering the importance of the question: "Is the dominance of female percipients a genuine effect related to ESP?", it seems worthwhile to investigate whether one of the alternative explanations should be accepted.

As regards the assumption that the effect is caused by a different number of males and females in the population, it should be noted that for population statistics a difference between females and males of this size (56% versus 44%) is exceptionally high and hence unlikely to be the sole contributor to the effect. If all persons involved in the cases, either as percipient or target person, are considered the males and not the females are found to have a higher frequency (males 598, females 509, $z=2.68$, $p=007$). This implies also, as will be shown later, that more males than females have been involved as target persons. Moreover, if cases between spouses are considered, the only instance in which per definition the number of males and females must be equal, the same trend in favor of female percipients is observed (males 17, females 32, $z=2.14$, $p=.03$). These data make it seem unlikely that a difference in population distribution for males and females is responsible for the effect.

In table 1 the data are presented for male and female percipients over groups who differ in type of relationship.

TABLE 1

Male versus female percipients in different relationships

spontaneous case between	male P	female P	z	p
spouses	17	32	2.14	.03
siblings	41	47	.64	.52
parent and children	18	43	3.20	.001
children and parent	44	39	.55	.58
other family members	32	41	1.05	.29

As can be seen, the same trend is observed in all groups except for those cases, in which children had an impression related to one of their parents. A t-test for related samples shows, that the difference between the means for male and female percipients is significant ($t=2.51$, $df=5$, $p=.05$). The correlation between both groups is extremely high ($r=.92$), and the variance in the groups does not differ ($t=.74$, $df=4$, $p=.50$). This implies, that the effect of a relatively high proportion of female percipients is rather stable over all groups. Hence the possibility, that the effect would be caused by one group of participants with a certain relationship, can be safely excluded.

As mentioned before, another possible explanation might be that the effect is due to an excess of female contributors of cases. This might result in the effect under investigation, since in the majority of the cases (62%) it was the percipient who sent in the report. If women were more inclined to take up the pen, then we could expect a higher number of female percipients. In order to analyse this, I considered the difference in frequency between male and female contributors, who were neither percipient nor target person themselves. It seems reasonable to assume, that if there existed a general tendency at the time for women to be more active than males as regards reporting cases, then this tendency

would also show for those cases in which the contributor was not directly involved as percipient or target person.

The result shows that there is a difference, but it are rather the males who are the more active contributors (males 119, females 67, z= 3.81, p=.0001). However, amongst the males are some persons (mainly reverends and authors), who because of their profession had probably a better chance of becoming acquainted with spontaneous cases in which others were involved. After subtracting their number the difference becomes 92 males versus 67 females (z=1.98, p=.05), still marginally significant in favor of the male contributors. Thus the excess of male contributors can not be considered to be caused by the fact that they might have had better access to spontaneous cases. Although males contributed more cases than females, when the contributor was not self-involved in the case, the difference between male and female percipients in those cases is found to be the same as for cases sent in by either percipient or target person (see table 2).

TABLE 2

Relationship between male and female percipients for different groups of contributors

Contribution by	Male P	Female P
percipient or target person	163	209
others	83	106

Chi-square = .01, df=1, p= n.s.

These data also imply that we can exclude too the possibility that female percipients might be more inclined to report their own cases than males would be. In that case we might have expected to find a relatively higher proportion of female percipients in the first row of the table. These results indicate that it is unlikely that a reporting effect should be held responsible for

the observed excess of female percipients.

Another possibility, although perhaps a bit far-fetched, is that male reporters are more inclined to report a case when a female acts as percipient. To check for this effect, the contributors who are neither percipient nor target person were split up into males and females and the relationship for number of reports on male and female percipients were studied. The results are presented in table 3.

TABLE 3

Male and female contributors reporting on male and female percipients

Contribution by	male P	female P
male	54	65
female	28	40

Chi-square= .16, df= 1, p= n.s.

The result is clearly not significant and hence there is no reason to suspect an influence from such a reporting effect.

The above presented analyses so far indicate that since the most likely counter-hypotheses are eliminated the dominance of female percipients might be in some way related to the ESP process. Before exploring this line further, some other analyses are presented who might be useful for interpreting this finding.

The target person

Over all cases it is found that 352 target persons are male (64.6%) and 193 are female (35.4%). This difference is strongly significant ($z=6.81$, $p << .01$). When we compare the distribution of male and female target persons over the different types of relationships (see table 4) the effect appears to be rather consistent with one exception. In those cases where children received an impression in which one of their parents was involved,

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

TABLE 4

Male versus female target person in different relationships

Spontaneous case between	male T	female T	z	p
spouses	32	17	2.14	.03
siblings	65	23	4.48	<< .01
parent children	52	9	5.51	<< .01
children parent	39	44	.55	.58
other family members	37	36	.12	.90
friends and acquaintances	119	60	4.41	<< .01

As table 5 shows, the effect cannot be attributed to a reporting effect. There is no difference between the proportion of male and female target persons over the different groups of contributors.

TABLE 5

Male and female target persons for different groups of contributors

Contribution of case by	P or T	family and other
male T	228	124
female T	130	62

Chi-square= .35 df=1 p= n.s.

Because of the size of the difference between the proportion of male and female target persons, we may also safely exclude the possibility that this difference would have been caused by a difference in population figures for males and females.

Since in the majority of the cases the event happening to the target person has been a rather unpleasant one (death or serious illness), the next assumption to be investigated is, that males became target person more often because of their life style which might have made them more prone to meet a serious accident or death. Hence I compared the seriousness of the event for both male and female target persons (table 6).

TABLE 6

Male and female target persons for different types of event

	death	serious illness	slight injury	trivial
male T	236	46	32	31
female T	137	19	15	19

Chi-square= 1.76 df=3 P=.62

Again no significant difference is observed. Therefore it can be concluded, that there must be a reason other than that mentioned above for the fact that males have a higher probability of becoming involved as a target person.

The relationship between percipient and target person

This section is devoted to the problem whether certain types of relationship facilitate the occurrence of a spontaneous case. It is well known that relatively more cases are reported when the participants have a close relationship, but it is still an open question whether this effect is related to ESP or to some other variables. It can be expected, that the better the participants know each other, the greater the likelihood that cases will be detected. Also there might be a tendency to report a case sooner when close friends or relatives are involved, than when the participants are relatively strangers.

Considering all cases it is found that the number of cases between close family members dominate heavily (see table 7). The constitute over 50% of all cases.

TABLE 7

Number of cases for different groups of relationships

close family	281	51.6%
other family	73	13.4%
friends and acquaintances	179	32.8%
strangers	12	2.2%

These figures show a trend similar to that observed in other studies on spontaneous cases, but do not prove as such that a close relationship facilitates ESP transfer. Since the likelihood of detecting a case must be much higher for those persons who know each other well I first excluded all cases where an effect of this factor might be expected. I assume that for all kinds of relationship, except for strangers, the probability of learning of the death of the target person must have been very high and about equal. This implies that I take only those cases for the analysis in which the event consisted of the death of the target person and in which the participants were no strangers. In this way we are able to study whether the same trend is observed in data in which the effect of differences in likelihood of detecting a similarity between an experience and the event is nearly equal for all groups. In table 8 the results are presented.

TABLE 8

Number of cases for different groups of relationships when all cases involve the death of the target person

close family	173	47.4%
other family	65	17.8%
friends and acquaintances	127	34.8%

As can be seen from the percentages presented, the number of cases between close family members still dominate to nearly the same extent.

It should be noted however, that when comparing these figures we can not assume that the average number of people involved in each relationship has been equal. On the other hand, that does not necessarily invalidate the conclusion that the number of cases is higher for near relatives, since it is more likely that in general the number of people involved in each relationship would show an opposite effect: that is, an increase in numbers when going down in the table. It might be expected that there are more distant relatives than that there are members of the own family, and also one can expect to have more friends and acquaintances than near relatives. Hence I consider it unlikely that differences in population size for the different groups could account for the higher number of cases observed between near relatives.

However, here again we must consider the possibility of a reporting effect. The death of relatives will probably impress the people involved more than the death of a member of one of the other groups and perhaps will be therefore sooner reported. When we compare only those cases where the contributor of the case was someone else than the percipient, no difference is found between family members and outsiders as regards the relative amount of cases reported for the different types of relationships. Because of the small numbers I have combined all family members, whether close or distant, in one group (see table 9).

TABLE 9

Cases reported by family members or outsiders involving death in relation to different types of relationships

	Reported by family member	Reported by outsider
close family	45	51
other family	13	7
friends and acquaintances	25	22

Chi-square= 2.31 df=2 p=.32

At least we can conclude, that family members do not show a tendency to report a case relatively more often when family members are involved than outsiders do, and we might take this as an indication that the finding of more cases between family members is not due to a reporting effect of this kind.

Another possible reporting effect might be that percipients differ as regards their willingness to report a case. Perhaps they would report a case sooner when relatives are involved than when the target person is an outsider. In that case we can expect to find relative more percipients when close family ties linked the percipient to the target person than for the other groups, if we compare cases reported by percipients with those, reported by other family members or outsiders. The data are presented in table 10.

TABLE 10

Number of cases reported by percipients and others involving death for different types of relationships

	Reported by P	Reported by others
close family	77	96
other family	45	20
friends and acquaintances	80	45

Chi-square= 17.1 df=2 p= .0002

Here the differences show to be significant, but the result is opposite to what was expected. It appears that percipients report a case more often when other family members or friends and acquaintances are involved. Perhaps this can be explained by assuming, that percipients are more reluctant to report a case in which the death of a near relative is involved, because they might feel that it is not proper to treat the death of a beloved one as another case for a scientific inquiry. Anyhow, whatever

the explanation might be, the result does certainly not support the hypothesis that more cases are reported between near relatives because of a tendency on part of the percipient to be more inclined to report a case when a near relative is involved. Hence we can conclude that it is unlikely that a reporting effect can explain the higher frequency observed of cases in which near relatives are involved. This conclusion is based on an analysis of only death cases, but I see no reason, apart from the fact that more trivial cases will be sooner detected between near family members, why this conclusion should not apply to all types of cases.

Since we know that most cases reported are between members of the same family, it seems worthwhile to study those cases in more detail. Since I assume that for this group the likelihood for detection of a similarity between an experience and an event is nearly equal for the different types of events, I included all cases reported in the analysis.

As before, within the family different types of relationships can be distinguished. The relation between the parents, between both parents and children, between children and parents, etc. The data for all these groups are presented in table 11.

TABLE 11

Number of cases between near relatives

Main category	P	T	n
spouses	female	male	32
	male	female	17
parent/child	father	son	15
	father	daughter	3
	mother	son	37
	mother	daughter	6
child/parent	son	father	22
	son	mother	22
	daughter	father	17
	daughter	mother	22
siblings	brother	brother	34
	brother	sister	7
	sister	brother	31
	sister	sister	16

Since I do not know the average size of the family in those days, and because the numbers are relatively small for all categories, it does not make sense to study the differences in totals for the main categories. Looking at the distribution of the figures within the categories it is obvious, that the already observed effect of a relatively higher number of female percipients and male target persons show also in these data. Not surprising, since cases between near relatives constitute over 50% of the total number of cases in the collection. We know that on average nearly 65% of the target persons are male, but in the cases where one of the parents acted as percipient the distribution is much more skewed in favor of the sons. Against 9 cases in which a daughter figured as target person we find 52 cases where the son was the subject of the experience. This is in strong contrast to the next category where one of the parents is the target person and one of the children the percipient. Here we find the only instance where females (mothers) have a higher frequency of being target persons than males (fathers). In the last category, cases between siblings, the difference between male and female target persons again favors the males. Here brothers dominate heavily with 65 cases against sisters with 23 cases.

The differences are not so striking when we look at the distribution of percipients in the various categories, but still occasionally strongly different from the average distribution of male and female percipients. Mothers are more as twice as often percipient compared to fathers when the event concerns one of the children. However, in the case of siblings the pattern again contradicts the general trend observed. It are the sons and not the daughters who are more often the percipient. In cases between children, the distribution of male and female percipients is nearly equal to the distribution found in all cases.

Before attempting to interpret all these findings it seems useful to sum up the main conclusions following from the analyses presented so far.

There are more female than male percipients.

There are more male than female target persons.

Compared to other types of relationships, more cases occur when near relatives are involved.

No indications were found that these results are caused by non-ESP factors like for instance reporting effects.

The main question is what these findings tell us about the ESP process. One may conclude that in general females are better percipients than males, that is, more capable of receiving an ESP

impression, and that it is easier to get an ESP experience when a male is the target person than when the event concerns a female. However, such a conclusion would imply that there must be some special reason, perhaps a biological one, which makes women better percipients and males better target persons. Especially the latter implication seems to me a difficult one to accept. We know that occasionally gifted subjects show up, so I can imagine that some people have a better ESP ability than others. But it is difficult to see why an accident happening to a male would be more easily transmitted by ESP than the same accident happening to a female. There are some studies suggestive of a possible influence of the target person on the ESP ability of the percipient, but I wonder whether these cases are not better explained by assuming, that the relationship between agent and percipient has been the crucial factor. At least we know that a number of studies, including the present one, strongly suggest that the relationship between agent and percipient is an important factor in facilitating ESP transfer.

Instead of assuming that some unknown factors create a difference between males and females as regards their ability to function as either percipient or target person, I feel that another explanation warrants consideration. We have seen that the relationship between percipient and target person is an important factor, but also, that within the small circle of the immediate family the probability of becoming either percipient or target person can strongly differ depending on the role of the person in the family. This suggests, that next to the relationship, or as a consequence of it, another factor is involved in determining the characteristics of a spontaneous case. We see from table 11 that mothers and fathers get much more ESP impressions about their sons than about their daughters. Also that siblings have relatively more often an experience related to their mother than to their father. Amongst siblings it are again the sons who are more important as target persons, and for marital partners the husband is about twice as often target person than the wife.

These data suggest, that within each relationship the position of the persons involved in terms of importance to the other or of emotional dependency exerts an influence on the likelihood of becoming either percipient or target person. Although I realize that it is rather speculative, I would like to propose as a hypothesis that within a relationship characterized by mutually emotional ties the probability of becoming the percipient is higher for the partner who is more emotionally

dependent, while the probability of becoming the target person is higher for the partner who is the object of this emotionally dependency. At least to a certain extent this hypothesis is supported by the data of this study.

The Victorian age can be considered as a mainly male-dominated society. In general males were probably more important in relationships, in terms of financial security, social status, and in the case of sons, in terms of future prospects. The same applies to the family. In general the husband was responsible for the financial situation and his social status reflected the social status of the whole family. Sons were more important than daughters, who were probably in most cases considered by the parents as only fit for marriage. For the future of the family and of the whole family, the success or failure of the sons in society must have been highly important. I assume that these differences in social positions between the members of the family must have had its reflection on the nature of the emotional relationship between them. The only exception observed, namely that children more often have an experience in which the mother is the target person, does not have to contradict this hypothesis. It is not unreasonable to assume, that children rate the importance of both parents differently from the way society in general does. Especially when being young, the mother is more important to the children than the father, and as such children are more depending on her.

A similar pattern can be observed when considering the percipients. Mothers had more than twice as often an ESP experience involving one of the children, compared to the fathers. Because mothers were mainly restricted to family life while the fathers spend most of their time outside the family, it is understandable that the children became more emotionally related to the mother than to the father. On the other hand, sons and daughters were nearly equally distributed as regards being percipient, whether the target person is one of the parents or one of the other children. This also ties in with the hypothesis mentioned above, because as regards the degree of emotional dependency from their parents or from their other brothers and sisters there is no reason to assume a difference between sons and daughters.

Hence I believe that the likelihood of the occurrence of a spontaneous case is to a large extent influenced by the relationship between the partners, and that the probability of becoming either percipient or target person is influenced by the nature of the relationship. In each relationship there is

usually a difference in the strength with which one partner is emotionally dependent on the other. The more dependent partner will have a higher probability of becoming the percipient, while the more 'dominant' or more important partner is more likely to figure as the target person.

The nature of the experience

As has been observed in various studies on spontaneous cases, one can distinguish a limited number of forms in which the ESP 'message' becomes conscious to the percipient. Table 12 gives a breakdown of the frequencies over the different types of experiences as found in this study.

TABLE 12

Distribution of types of experiences

intuitions	9.6%
dreams	33.5%
visual waking imagery	41.2%
auditory waking imagery	10.5%
visual and auditory waking imagery	2.3%
others	3.0%

An analysis into differences between male and female percipients yielded a non-significant result (chi-square= 4.68, df= 5). Hence it follows that there is no relationship between type of experience and sex of the percipient. Also no difference was found between type of experience and sex of the target person.

Next I studied whether the time of the day (night, morning, afternoon, evening) had an influence on the type of experience. Apart from dreams, who naturally are mainly restricted to the nights, no such relationship was found (chi-square= 17.1, df= 12). There is found, however, a striking difference between time of the day and total number of experiences. The data are presented in table 13.

TABLE 13

Time of day of the experience. Dreams excluded

	N	hours	%
night	104	8	24.0
morning	43	4	19.9
afternoon	70	6	21.6
evening	112	6	34.5

note: percentages corrected for differences in hours

It is evident that in a significantly higher proportion of instances the percipient's impression took place either in the evening or in the night. This might be interpreted as additional evidence for the hypothesis that less sensorial input facilitates the occurrence of an ESP experience. That the frequency of impressions taking place in the evening exceeds those taking place in the night is mainly a spurious effect, a consequence of not including dreams in this analysis.

If less sensorial input facilitates the occurrence of spontaneous cases in the dark hours of the day, it might also be possible that in the dark seasons more spontaneous cases occur. In autumn and winter the days will be in general darker and less clear than in spring and summer time. Considering the total number of cases only a trend in this direction is noticeable. For autumn and winter 217 cases are recorded against 187 cases for spring and summer time. However, this difference is not significant ($z=1.50$). When we compare the distribution of the various types of experiences for the different seasons (again with dreams excluded) no difference shows (chi-square=13.3, df=15). The same holds for the relationship between time of the day the impression was received and the season (chi-square= 6.5, df=9).

All these results imply that the season, nor the time of the day has an influence on the form the experience take. Contrary to my expectations an equally non-significant result was observed when comparing the frequencies of the different types of experiences for the two possibilities, the percipient being indoors or outdoors. (chi-square= 4.8, df=3, only for intuitions,

visual, auditory, and combined waking experiences).

Male and female percipients are found to have a relatively equal number of experiences for each period of the time of day (chi-square = 4.1, df=3). But there is a marked difference between sex of the percipient and being indoors and outdoors when having an ESP impression (chi-square= 17.6, df=1, p=00003). Not surprisingly it are the males who have relatively more often an ESP experience when being outdoors.

In a last effort to find some indication for why the ESP impression takes form in the different types observed, I studied the relationship between the social situation (alone versus with others) of the percipient and type of experience. Again no effect is detected (chi-square= 1.31, df=2, for intuitions, dreams, and waking experiences combined).

On one hand the results of the analyses presented above concerning the way the impression comes into the mind of the percipient are rather meagre. It does not give us, as was hoped for, any insight into why the percipient experiences the ESP 'message' in a specific way. On the other hand we might draw a negative conclusion. The analyses indicate that all sorts of situational factors, like time of day, season, being indoors or outdoors, and social situation, have no influence on the type of experience. Hence we are forced to consider some other possible assumptions. It might be that the type of experience is influenced by some property of the ESP process. One likely candidate in this regard is the content of the ESP 'message'. It might well be that the form of the experience is related to the seriousness or unpleasantness of the content, as if to protect the individual for too strong a shock. If there is some merit in this assumption, we can expect a preponderance of a less informative type of experience in the case of more serious events. In order to investigate this I studied the relationship between type of experience and seriousness of event. All waking experiences are combined in one group, and death events are compared to non-serious events (light injuries, trivial and positive). Serious illness has been excluded, since in general the event can not be located exactly in time. Moreover, it makes the analyses more easily comparable to for instance Stevenson's study (Stevenson, 1970), who also excluded cases of serious illness. The results are presented in table 14.

TABLE 14

Type of experience and seriousness of event

	death	non-serious
intuition	19	19
dream	114	51
waking experience	233	37

Chi-square= 35.0 df= 2 p= <<.01

It is clear that the assumption stated above does not hold. On the contrary, the most informative type of experience involves relatively the highest number of death cases. We will return to this problem later, when considering the amount of details associated with the experiences. For the time being we can conclude that the content of the 'message' does influence the way the message becomes conscious. Although we may accept a relationship between type of event and type of experience, undoubtedly psychological factors will also play a role. The present study does not lend itself for investigating such variables for lack of data. This will require a more experimental approach in which percipients, or subjects in laboratory experiments applying free response techniques, are studied as regards differences in psychological variables and relative frequencies of experiencing imagery or intuitions.

That the nature of the content is related to the type of experience shows also in another aspect, the identification of the target person versus the identification of the event. In table 15 the data are presented for correct identification of target person and event related to the 3 main groups of experiences.

TABLE 15

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

	target person identified	event identified
intuition	33	26
dream	157	156
waking experience	245	97

Chi-square = 32.3, df= 2, $p << .01$

Table 15 shows that the identification of the target person most often took place via a waking experience (not surprisingly considering the title of the study 'Phantasms of the Living'), while events are more often identified via dreams.

From this section we can draw two main conclusions. First that the probability of receiving a spontaneous ESP impression, regardless of the form it takes (except for dreams), is higher for the evenings and the nights. As stated before I consider this as supporting evidence for the facilitating effect of reduced sensory input. Secondly that situational factors do not influence the type of experience, but that the content of the ESP message does. We will return to this problem further on.

The event

A breakdown of the situation of the target person at the time of the experience, the event, is presented in table 16.

TABLE 16

Situation of target person at time of experience

death	66.7%
serious illness	12.5%
slight injuries	8.7%
serious material	.5%
slight material	.2%
trivial	10.0%
positive	1.4%

There are some remarkable features in this table. Firstly the strong preponderance of events related to persons. Material damage hardly plays a role. Although we are dealing principally with telepathy cases (cases in which the time between experience and event is less than 24 hours), I do not see a reason why even slight injuries should be so more often the topic of the spontaneous ESP experience than for instance serious material damage to the person. This seems to indicate again, that the target person, and his or her role in the relationship, is determinative for giving rise to an ESP experience. Secondly it shows that the proportion of death cases is unusually high in this study. Whether this reflects an interest of the authors of the Phantasms, Gurney et. al., or whether it is caused by the probably much higher mortality rate in the last century, is difficult to say. But, as we have seen, there is undoubtedly a connection between the excess of waking experiences in this study and the observed preponderance of death cases.

The high frequency of rather negative events is a common feature of all studies on spontaneous cases. I first consider possible non-ESP variables which might contribute to this phenomenon. It is clear from table 16 that positive events are rather rare, so there seems not much sense in comparing negative versus positive events. But we can compare serious events versus non-serious events.

The first assumption studied is that serious events are better memorized than more trivial events, and that therefore such cases are longer remembered and thus more frequently reported. The second assumption concerns the possibility, that people consider ESP experiences in which the event is not so

dramatic or rather trivial as less suited for reporting. I suppose most people, especially when they are not involved themselves, are sooner inclined to brush away an experience related to a trivial event as mere coincidental, than when it concerns the death of a person. The analyses of the material seem to confirm both assumptions.

First I studied the relationship between seriousness of the event and time elapsed before reporting the case. To avoid empty or nearly empty cells, I formed two categories for time elapsed before reporting, one for cases reported within a year, the other for the older cases.

TABLE 17

Relationship between seriousness event and time elapsed before reporting

	death	non-serious
reported within 1 year	23	20
older than 1 year	334	83

Chi-square= 14.4 df=1 p= << 01

Table 17 shows that relatively more cases with non-serious events are reported when the time elapsed before reporting is less than a year. Considering the importance of this question I also checked this relationship for all time intervals larger than 1 year split up (thus <1 yr, 1-2 yr, 2-5 yr, etc.), and found that the relationship remains significant at the same level. Hence I feel that we can conclude that cases in which the event is non-serious are less frequently reported when the time between experience and reporting increases, most likely due to a tendency to forget this type of case sooner.

When we compare the person who contributed the case with the seriousness of the case reported, we find again a significant relationship. Since in the case of death the target person can

not be the contributor, I excluded this category entirely. The results are presented in table 18.

TABLE 18

Who contributed the case in relationship to seriousness of event

	death	non-serious
contributed by percipient	207	85
other family	84	3
others	84	3

Chi-square= 45.7 df=2 p= << .01

This analysis shows clearly, that trivial cases are mainly sent in by the percipients because only six cases with non-serious events were contributed by someone who was not the percipient. I think we can safely conclude that this proves that outsiders consider a case involving a rather trivial event as less worthwhile to send in than a case in which death is involved. That percipients seem to be more inclined to report cases with non-serious events might be a consequence of the fact that it is in the first place the percipients who are most often baffled by the experience. At least they know that the experience was a real one, something one can not always expect an outsider to believe.

From the analyses presented in this section we can conclude, that cases involving negative events are much more often reported than cases involving positive events, material damage or trivial events. Considering the very low percentages for positive cases and cases involving material damage it seems very unlikely that some non ESP variable, like a reporting effect, can be held responsible. It seems more likely that mainly those events having something to do with the person itself are transmitted, probably because these events are in general more important to the relationship between percipient and target person than more material events. Since here the majority of the events seem

rather negative, I like to propose as a hypothesis, that events which pose a greater threat to the relationship will have a higher likelihood of becoming the subject of an ESP experience with the other partner. This hypothesis would explain why positive events, or serious material damage, are so underrepresented. Both types of events, however impressive to the person who is involved in them, are in general not threatening for the relationship.

That more serious events are observed than non-serious events is possibly an effect of both a tendency to forget trivial cases sooner, and to consider trivial cases experienced by someone else as less suitable for reporting. Unfortunately I can not estimate the strength of these effects, since we do not know how many cases, especially those involving trivial events, are never reported. Hence we are not able to conclude a possible relationship between seriousness of event and likelihood of occurrence of a spontaneous case. I suppose we can only find an answer to this problem by carrying out a systematic investigation into recent spontaneous cases, applying interview techniques on samples of the population.

The content of the experience

In 81.7% of the cases the percipient correctly identified the target person. In the remaining cases this was not, either because the percipient thought the event to be connected with someone else, or because no identification could be given. The identification of the target person was found to be significantly more often correct than the identification of the event. Here we find that the event is correctly identified in only 51.9% of the cases.

Thus it appears, that the identity of the target person has a higher likelihood of being perceived than the nature of the event. We have seen that the overwhelming majority of the events are of a rather negative character. Hence it looks like as if the most important thing to be transmitted is that something, mostly of an unpleasant nature, happened to the target person. It is the well-being of the target person which seems more important to the percipient than to know the specific reason, the event, which has been responsible for the state in which the target person ended up. I consider this additional evidence for the hypothesis, that it is the target person as part of the relationship, and the possible threat to the relationship, which matters.

There shows to be no difference between male and female

percipients as regards the relative frequencies with which the target person was identified (chi-square= 0, df=1), nor as regards the identification of the event (chi-square= 0.8, df=1). This is an interesting result, which in my opinion supports the assumption discussed before, that some other factor not related to the ESP process itself should be responsible for the preponderance of female percipients.

A difference was observed between male and female percipients as regards conviction and the tendency to take action as a consequence of having the experience. Males show significantly more conviction than females (chi-square= 4.48, df=1, p=.03), and take significantly more often action (chi-square= 3.85, df=1, p=.05). Both trends, the differences are not very strong, are to a certain extent related since taking action has been one of the criteria for assuming and scoring conviction. Furthermore it shows that no relationship exists between identifying or not identifying the target person and taking action (chi-square= 0.9, df=1). As we have seen before, there is also no difference between males and females in the proportion of correctly identified target persons. Hence the difference in taking action between males and females can not be explained by assuming that male percipients more often identified the target person. The most likely explanation of this finding seems to me, that the observed difference is not related to the ESP process, but that psychological differences between males and females caused by the social situation existing in those days are responsible for the effect. Males were by training and social circumstances probably more inclined to take initiative than females.

Stevenson (1970) found that agent focussing was positively correlated with action taken by the percipient in response to his experience. In the present study no such effect could be observed (see table 19).

TABLE 19

Relationship between target person focussing and taking action by percipient

	target person focussing	target person no focussing or unknown
action by P	19	61
no action by P	83	354

Chi-square = .69 df=1

However, it should be noted that in the present study the proportion of cases in which the percipient took some action (about 1:5.5) differs strongly from the data Stevenson presents (roughly 1:1). The reason for this difference lies mainly in the fact, that there exists a rather strong relationship between type of experience and taking action. Intuition cases, which were the subject of Stevenson's study, trigger relatively more often action than for instance dreams. (see table 20). Since intuition cases constitute less than 10% of all cases in the present study, it is not surprising that we find a much lower proportion of cases in which the percipient took some action.

TABLE 20

Relationship between type of experience and taking action

	percipient takes action	percipient takes no action
intuitions	16	34
dreams	22	161
waking experiences	41	256

Chi-square = 13.0 df=2 p=.002

A similar relationship turns out to exist between conviction and type of experience (chi-square = 43.5, df= 2, P=<< .01), although the difference between intuitions and waking experiences is rather small. This result confirms partly L. Rhine's finding (Rhine, 1962) that intuition cases are relatively more often accompanied by conviction than dream or waking imagery experiences.

As one of the most interesting aspects in studying spontaneous cases I consider the question how detailed the information associated with the experience is, and how many of these details fit the actual situation of the target person. I feel that this question has an important bearing on the broader problem of how to conceptualize ESP.

Unfortunately it is to be expected that several non-ESP factors will have an influence on the number of details reported. This showed already when I tried to score the total number of details given in the experience and next the number of these details which proved to be correct. In nearly all cases both numbers are equal, which implies that only those details find their place in the report which afterwards had turned out to be correct. I assume that this striking phenomenon is caused by a reporting effect, a tendency to avoid mentioning details which the contributor of the case, or the percipient considers as irrelevant because they are incorrect. Hence when in what follows details are analyzed the results are limited to only correctly perceived details.

But even the number of correct details associated with a case might be incorrect for different reasons. First we have to take into account a reliability problem. Scoring the sex of the percipient or agent is a clear-cut matter, but scoring the number of details even when applying the rules laid down in Schouten (1979) leaves sometimes room for differences depending on the judgment of the scorer.

Secondly, since in most cases the percipient knew the target person and often had some knowledge about his or her situation, the verbalized representation of the experience might have become contaminated by such knowledge. Furthermore, there is a common tendency often to present a more coherent and 'logical' account about past events than is warranted by the real facts. Last but not least, the experience is mostly related, or at least reported, at a time when the details concerning the situation of the agent are already known. Often the report is compiled after the narrator of the case has repeated the same story many times before on various occasions, which must have had its effect on

the content.

Despite all these factors which will increase the unreliability of the data I still consider it worthwhile to carry out the intended analyses. One reason for this is that I assume that in general the above mentioned sources of error will tend to increase the number of correct details reported. Both the fact, that the percipient in most cases knew the target person and learned of the event after the experience will rather lead to an increase of presumably correct details than to a decrease. As to the tendency to make the story appear more coherent, I assume that because of this it is more likely that incorrect details, 'spoiling' the overall picture, will be omitted.

One of the most surprising features of the data turns out to be that the average number of details given is as low as about half a detail per case. In 381 cases no details were present. In 103 cases one detail, and in 78 cases two or more details were correctly perceived. This implies that in only 32.2% of the cases one or more details were present. When we compare this percentage to the percentages of identification of the target person (81.7%) and of the event (51.9%), it is clear that details play a minor role in the experience.

The main problem investigated in this section will be, whether answers can be found to the questions why the observed number of details is so low and what the function of details might be in the ESP experience. However, before attacking these questions, I investigated first the influence of two different possible reporting effects.

The first one concerns the relationship between elapsed time and number of details. The results are presented in table 21.

TABLE 21

Relationship between number of details and time elapsed before reporting

	number of details		
	0	1	2 or more
reported within 1 year	29	6	10
older than 1 year	327	88	66

Chi-square = 2.7 df=2

The same analysis but carried out for all time intervals longer than 1 year split up in the applied scoring intervals yielded a similar non-significant result (chi-square = 11.9, df=8). Hence we can not assume that the low number of details is caused by a tendency to forget details in the course of time. It is also of interest to note that the length of the report is not related with the elapsed time before reporting either (chi-square= 4.77, df=6).

The second possible reporting effect concerns the length of the report (expressed in number of centimeters of printed text). People differ in writing habits and it might be, that the number of details reported depends too on how extensive the contributor presents the case. The data are presented in table 22.

TABLE 22

Relationship between number of details and length of the report

		number of details		
		0	1	2 or more
length	0 - 5	144	35	20
	6 -10	160	42	31
	11 -15	50	17	11
over	15	26	9	16

Chi-square = 16.9 df=6 p=.01

The significant result shows that this reporting effect exerts a certain influence. As might have been expected, there is a positive correlation between length of the report and number of details reported. Unfortunately we are unable to say, whether this relationship is caused by different writing habits from contributors, or by the fact that cases involving more details will lead to somewhat more extensive accounts.

An obvious question to put first is whether the type of experience has an influence on the number of details. Not surprisingly this proves to be the case (see table 23).

TABLE 23

Relationship between number of details and type of experience

	number of details		
	0	1	2 or more
intuitions	38	15	2
dreams	90	46	55
waking experiences	241	36	21

Chi-square= 75.3 df=4 p <<.01

As we have shown before (see table 15) there exists also a nearly equally strong relationship between identification of the event and type of experience, events being most often perceived in dreams. From both results follow that events and details are also most likely related. As will be shown later this indeed turns out to be the case. From table 23 we can conclude that dream experiences yield the highest proportion of details.

Secondly I investigated whether seriousness of event is associated with number of details. This is also found to be the case to a very significant degree (see table 24)

TABLE 24

Relationship between number of details and seriousness of event

	number of details		
	0	1	2 or more
death	300	45	30
not serious	42	34	37

Chi-square= 79.0 df=2 p <<.01

It is apparent that details are much more associated with non-serious or trivial events than with death.

Next I studied a possible sex effect by comparing the number of details for male and female percipients. The result turned out to be not significant (chi-square= 2.8, df=2). Thus females do not perceive more details than males, again showing that except for the fact that females are more often percipient there is no difference between males and females as regards the properties of the ESP experience.

To gain more insight in the possible function and meaning of details as part of the experience I compared the proportion of details for cases, in which the target person was correctly identified versus the proportion for cases in which this was not the case. The same analysis was carried out for cases where the event was or was not correctly identified.

TABLE 25

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

	number of details		
	0	1	2 or more
target person			
identified	305	81	59
not identified	75	17	8
	Chi-square= 2.4 df=2		
event			
identified	155	65	70
not identified	223	38	8
	Chi-square= 67.9 df=2 $p << .01$		

Correct identification of the target person shows no correlation at all with the number of details, but there is found a very strong correlation between number of details and identification of the event. The same holds for the various interactions between these two variables (for instance the same analysis between events

and number of details for only those cases in which the target person was not correctly identified, etc.).

In summary we can conclude, that there is a cluster of variables associated with number of details. Although the overall amount of details perceived is rather low, if details are perceived they are found to be strongly related to type of experience and seriousness of event. If the event is not known it is also highly likely that details are lacking. On the other hand, the number of details is not related to the identification of the target person.

My interpretation of these data is as follows, but I realize that other interpretations might be worth considering too.

I assume that the relationship between type of experience and number of details is a consequence of the way the impression reaches consciousness. Events and details are mainly associated with dreams, by nature more suited for incorporating details, while the identification of the target person, which requires only one item of information, most often came via a waking experience, usually in the form of 'seeing' the person.

We know also that correct identification of the target person happens more often than identification of the event.

Furthermore that the identification of the target person is not related to number of details. Considering this with the fact that the average number of details per case is low I assume, that the'knowledge' who the target person is comes mainly in an all-or-none fashion. That this knowledge is still now and then associated with details might easily be a consequence of the fact that nearly all percipients knew the target person relatively well and could easily see correct details based on this knowledge. Hence I assume that the percipient perceives by ESP that something has happened to the target person, and that this 'information' generally becomes conscious via a waking experience, that is a 'projection' of the face or body of the person in the outside world. It can be expected that in this hallucination details will be included based on the knowledge and expectations as regards the appearance and situation of the target person, and that some of these details will turn out to be correct.

On the other hand, the identification of the event is often by its very nature associated with knowing some details. Here lies an important difference with the identification of the target person. If the percipient knows that X is the target person, this knowledge is sufficient in itself. But if the percipient feels that X had an accident, there is no way to tell

what happened to X and how serious the consequences are if no details are known. One might even say that the less serious the event, the more specific as regards content the experience and event has to be to become recognizable at all. The finding that relatively more details are associated with non-serious events also points in this direction.

For instance, 'falling from the staircase' would be rated as an event 'slight accicent', associated with two details (type of accident: falling, place of accident: staircase), while X is dead would be rated as only the event correct and zero details. It might be assumed, that the function of details lies mainly in conveying the meaning of the content of the 'message'. In the case of death the knowledge of the fact that the person died is of paramount importance compared to knowing details about how the person died. But in the case of less serious events it is the details which more or less decide the meaning of the 'message', and hence they are more important there. Hence I propose that the above mentioned hypothesis, that the 'knowledge' of the target person comes in an all-or-none fashion, also in principle holds for perceiving the event.

Finally I studied the relationship between conviction and number of details. A significant result was observed (chi-square= 11,9, df=2, p=.003) but contrary to expectation it turned out that relatively more conviction cases are found when the number of details is zero. I consider this finding (which is associated to a certain extent with the result observed in table 20) as supportive for L.E. Rhine's (1978) conclusion that conviction has to be considered as a separate element. At least it appears to contradict the assumption that conviction would be based on a 'cognitive' conclusion attached to the experience, since in that case one would expect a positive correlation between conviction and number of details.

Distance

I included distance in the analysis because it might be expected that some relationships could occur between distance and a number of non-ESP variables. Knowing this might be of value when studying the effect of distance on ESP performance in other analyses of spontaneous cases or when applying a more experimental approach.

In the first place distance is found to be related to seriousness of event (chi-square= 16.1, df=1, p= << .01, distance split into two categories: within the country and percipient and

target person in different countries). As can be expected non-serious cases happen nearly always at close distances, undoubtedly due to the fact that frequent contact is needed to enable detection of such cases.

Furthermore distance and type of relationship are related. Cases involving large distances happen relatively more often between close family members (chi-square= 26.5, df=2, p= <<.01). I assume that this effect is also a consequence of the likely more frequent contact between close family members.

Distance has no influence on the number of details nor on the type of experience. That no relationship is found between distance and number of details, despite the fact that details are more often associated with non-serious events, is probably due to the low proportion of non-serious cases in this study. There is also no relationship between sex of the percipient and distance.

Hence we can conclude that studies seeking a possible relationship between distance and ESP are only meaningful when it can be assumed that the likelihood for detecting a similarity between an experience and event regardless of distance is equal for all cases.

The analyses for only the best witnessed cases

In order to study to what extent the results might have been influenced by the fact that in the majority of the cases no independent testimonies could be obtained to support both the correctness of the experience and event I have repeated all analyses reported in the previous section but this time for only best witnessed cases. Best witnessed cases are those cases where persons other than percipient or target person supplied statements printed in the 'Phantasms' corroborating the occurrence of the experience and the occurrence of the event as described in the report.

In these analyses I have followed the tables as presented above. In order to save space and since in nearly all cases the data for the best witnessed cases (160) do not differ from the data for all cases I only present the results and not the tables in full, except when a difference between best witnessed cases and all cases is observed.

The data for best witnessed cases for table 1 show the same pattern including the reversal in proportion of male and female percipients in the case of children percipients having an experience related to one of their parents. The data for tables 2 and 3 are non significant for best witnessed cases

(chi-square= .12, df=1; chi-square= 0, df=1) as was the case for these data for all cases. Table 4 for best witnessed cases shows the same trend as the data for all cases including a higher frequency for female target persons in the children-parent relationship. The data for table 5 for best witnessed cases also yield a non significant difference (chi-square=.03, df=1). The same applies to the data for table 6, but here I had to combine the categories death-serious illness and slight injuries-trivial to avoid expected frequences less than 5 (chi-square=.06, df=1). A slight difference was observed in the distribution for best witnessed cases for table 7 (best witnessed cases: 64,28,60,8; all cases: 281,73,179,12), although the general trend appears to be the same. This difference does not effect the conclusions drawn in this section since the difference in distributions for table 8 for best witnessed cases is non significant (chi-square=4.2, df=2).

For table 9 I had to combine the categories other family and friend/acquaintances to avoid too low expected frequencies. As in table 9 for all cases no significant difference was observed for best witnessed cases (chi-square=0, df=1). The data for best witnessed cases for table 10 yield a non significant difference (chi-square= 4.8, df=2) contrary to the results in table 10 for all cases. It is easily to see that this difference does not invalidate the conclusions drawn in this section. Table 11 is the only table which could not be checked for only best witnessed cases because of the very low frequencies obtained. The distribution of cases over type of experience for best witnessed cases did not differ from the distribution as presented in table 12 (chi-square= 3.64, df=5). A similar result was found for the distribution of best witnessed cases over time of day as given in table 13 for all cases (chi-square= 1.1, df=3). A significant difference into the same direction was observed for the data for best witnessed cases for table 14 (chi-square= 13.4, df=2, p=.001).

Although a similar trend is observed the data for table 15 for best witnessed cases yielded a non significant result contrary to the same data for all cases.

TABLE 15 (BW)

for only best witnessed cases

	target person identified	event identified
intuitions	10	11
dream	55	53
waking experience	60	32

Chi-square= 4.9, df=2

The distribution of best witnessed cases over the different types of events does not differ from the distribution as presented in table 16 for all cases (chi-square= 5.8, df=4).

Table 17 for best witnessed cases shows the same trend as observed in this table for all cases, but probably due to the low frequencies the effect does not reach a significant level. The same holds for table 18 for best witnessed cases only.

TABLE 17 (BW)

for only best witnessed cases

	death	non-serious
reported within 1 year	14	5
older than 1 year	111	14

Chi square= 2.1, df=1

TABLE 18 (BW)

for only best witnessed cases

	death	non-serious
contributed by		
P	84	15
other family	26	1
others	16	3

If we combine the last two categories in table 18 the resulting chi-square =.7, df=1, p=n.s.

In table 19 for best witnessed cases the result is non significant (chi-square=.06, df=1) as it was for all cases. Table 20 for best witnessed cases yields a significant difference in the same direction as for all cases (chi-square= 7.6, df=2, p=.02). Table 21 yielded, as for all cases, a non significant result when only best cases are considered (chi-square=.67, df=2). Table 22 for best witnessed cases yielded a significant difference following the trend observed in this table for all cases (chi-square= 19.7, df=6, p=.003). The same applies to table 23 for best witnessed cases (chi-square= 17.8, df=4, p=.001), and to table 24 (chi-square= 14.5, df=1, p=.0001; categories number of details 1 and higher combined). Finally table 25 for best witnessed cases yielded similar results as the data for all cases. The first section related to the identification of the target person yielded a non significant difference (chi-square= 5.51, df=2), while the second section related to the identification of the event yielded a significant relationship with number of details in the same direction as the data for all cases (chi-square= 23.3, df=2, $p << .01$).

We can conclude that in all analyses the data for the best witnessed cases show the same trend as found in the data for all cases and not one instance was observed where the data for the best witnessed cases contradicted the data for all cases. Hence we can consider the best witnessed cases as a representative sample from all cases.

In four instances a difference was observed to the effect that a significant effect for the data for all cases appeared to be

reduced to a non significant effect when only best witnessed cases were considered (tables 10, 15, 17 and 18). Undoubtedly this is partly due to the much lower frequencies obtained when taking only best witnessed cases, the total number of cases being then 160 against 562 when all cases are included. However, assuming that the significant effects observed in these tables were caused by an effect of possible incorrect reporting due to lack of confirmation by independent witnesses I will consider the question how this assumption would effect the conclusions and interpretations presented in the previous section.

The non significant result for table 10 for best witnessed cases does not contradict the conclusion that the data do not support the hypothesis that more cases between near relatives are reported because percipients would be more inclined to report a case when a near relative is involved. Such contradiction would only have been the case when a significant effect for best witnessed cases was observed indicating a trend in the opposite direction, that is percipients reporting more often such cases.

The significant result of table 15 for all cases implied a relationship between waking experiences and identification of the target person. Together with the results presented in tables 23 and 25 this led to the conclusion that type of experience, identification of target person and event, and number of details are related to each other. Since the results of the tables 23 and 25 for best witnessed cases are similar to those found for all cases it follows that we still can assume a relationship between number of details and type of experience and between number of details and identification of event. Identification of the target person is, according to table 25 for both sets of data, not related to number of details. Hence the non significant result of table 15 for best witnessed cases does not invalidate the interpretations presented following table 25 of the previous section. Therefore the only change when accepting that the significant result of table 15 for all cases has to be considered as an artefact would be to drop the conclusion that identification of the target person is associated with waking experiences.

The tables 17 and 18 are related to seriousness of event. If we accept, based on the data for best witnessed cases for both tables, a non significant difference then we have to conclude that there is no tendency to forget trivial cases sooner than cases involving the death of the target person, and that trivial cases are not more often sent in by the percipient than by others. These conclusions would in fact even more support the

interpretations given in that section, namely that serious events to the target person do have a higher likelihood of resulting in an ESP experience because the significant results of the tables 17 and 18 for all cases indicate that non-ESP factors also play a role in creating a higher proportion of reported cases involving serious events.

Hence in my opinion the few differences observed between the analyses for all cases and for only best witnessed cases do not lead to major changes in the conclusions and interpretations presented before. In one instance the difference has no effect (table 10), in two instances the difference lends more support to the interpretation given (tables 17 and 18), and only in one instance we would have to change a conclusion drawn in the previous section. Thus the main conclusion of analysing best witnessed cases is that the results found for all cases are apparently not influenced by unreliable reporting due to lack of independent testimonies.

Alternative hypotheses

As stated in the introduction the results obtained in the analyses have been first discussed applying the ESP hypothesis. These interpretations are naturally rather subjective, and probably better interpretations with regard to the implications of these findings for the ESP process can be formulated. However, besides applying the ESP hypothesis it is worthwhile to consider also the applicability of various hypotheses of a non-ESP character. Since it is unlikely that any alternative hypothesis could in itself explain all of the findings observed, a thorough discussion of this subject would require the formulation of different models combining aspects of various non-ESP hypotheses. Such an enterprise falls outside the scope of this paper. Hence it will limit myself to mentioning some of the hypotheses which are likely to form the elements of such a model and to discuss briefly which aspects of spontaneous cases are relevant to each of them.

The 'Being Worried' hypothesis

This hypothesis assumes that people often worry about the well-being of those people to whom they feel related and that they are inclined to attach a special meaning to their worries when it happens that these turn out to be true. This hypothesis would explain why spontaneous cases happen more often between near relatives since it may be assumed that people worry more

often about them than about other persons. It would also explain why 'negative' events like death or accidents constitute the overwhelming majority of the events involved, since negative events are inherent to worries. That only in some instances worries turning out to be correct are given a special meaning could be explained by assuming that it is especially those instances where there is no rational or real reason to be worried, which nevertheless turned out to be correct, which strike them as odd. Alternatively, perhaps people are inclined to attach a special meaning to them when they give rise to impressive experiences such as hallucinations and to disregard them when they are conscious in the natural way.

The 'Chance' hypothesis

This hypothesis assumes that spontaneous experiences represent only instances where there is a chance coincidence in time between an experience and an apparent fulfilment. These events could be rated as something 'special' only because the coincidence in time appears so striking. It is not easy to explain any results of this study based on this hypothesis only since chance is blind by nature and hence unlikely to yield consistent patterns in the data. This hypothesis requires at least a number of additional assumptions to explain why people single out certain chance coincidences and attach a special 'paranormal' meaning to them in such a way that it results in findings such as observed in this study.

The 'Fraud' hypothesis

Particularly with regard to spontaneous cases this hypothesis is difficult to apply because, contrary to the experimental situation, spontaneous cases are not isolated in place and time or restricted to a few persons. It is highly unlikely that the many people involved would independently construct their stories in such a way that such strong relationships are found as observed in this study. The same argument applies when one assumes that the investigators modified the reports received to support their own beliefs.

The 'Social Climate' hypothesis

This hypothesis assumes that in a given culture people expect 'paranormal' cases to take place and therefore are inclined to interpret certain occurrences in their life as 'paranormal'. This hypothesis could be applied in conjunction

with the 'chance' hypothesis. On this basis we might expect to find a number of similarities between the various cases sent in because the hypothesis supposes that there exists a generally accepted picture of what paranormal experiences are and under what circumstances they take place. Analogies can be found in the picture of witches (females, flying on brooms, bearing a special mark on their body) or vampires (males, drinking blood by sucking it out a living creature with their teeth, not being able to stand daylight). If we restrict ourselves to this study it is impossible to assess the merits of this hypothesis. It is clearly circular to assume that people believed at those times that 'paranormal experiences' took place the way they are reported and consequently they are reported that way. A crucial test of this hypothesis would be to carry out a cross-cultural study and to compare collections of spontaneous cases from different countries.

It is certainly possible to construct a model based on a combination of elements of the hypotheses mentioned above and perhaps including other non-ESP hypotheses as well which could explain 'ad hoc' the findings reported in this paper. But providing such a model is not sufficient in itself, just as it is not satisfactory to follow the 'Hansel approach' and to provide alternative hypotheses for each individual spontaneous case. The problem is not restricted to constructing an explanation which fits the data, although that is a necessarily step. Any such model should not only characterize the data as they are observed but must also generate predictions which can be verified either on the material itself, on new material or in experimental research. Only when a model fulfils this condition might it prove fruitful and contribute to progress in our understanding of spontaneous paranormal cases.

ABSTRACT

In this study spontaneous cases have been analyzed on the basis that these cases are a collection of human experiences with the common property that the persons having the experience felt that those experiences had a paranormal character. The aim of the analysis was not to prove that these cases suggest the existence of ESP, but to find relationships between different variables, to study as far as the material allowed whether non-ESP factors could explain the results observed and if not what hypotheses could be formulated when assuming that ESP has been responsible for these cases. For the analysis 562 cases

were taken from 'Phantasms of the Living' by Gurney, Myers and Podmore, published in 1886.

The data confirmed earlier findings concerning a dominance of female percipients, a dominance of serious events and a relatively higher proportion of case between near relatives. No indications were found that these findings are due to various sorts of reporting effects. In addition strong relationships were observed between different variables. When all analyses were carried out again for only best witnessed cases similar results were found. The data are interpreted applying the ESP concept and seem among other things to indicate that the relationship between people and the strength of the emotional dependency on the partner in the relationship are important factors in the occurrence of spontaneous cases and in the roles (percipient or target person) both partners will play in such a case.

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PHYSIOLOGICAL DETECTORS OF PSI

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INTRODUCTION

This paper is a critical evaluation of the literature on physiological detectors of psi, with emphasis on EEG, particularly evoked potential measures. The literature is discussed in light of the assumptions (often tacit) which underlie this type of research.

Psychophysiological studies in parapsychology fall into two distinct classes. In the first category correlations are sought between physiological variables and performance on some psi task. Differences between subjects as well as within-subject changes can be measured. Between subjects designs can answer questions of the kind - What psychophysiological characteristics do good ESP subjects have? Within subjects analyses, for their part, can allow the characterization of states particularly conducive to psi in particular individuals. This category will not be further examined here.

In the second category the approach is to use some physiological measure itself as an indicator of psi. A correspondence is sought between the randomly determined stimulation of an agent or inanimate system and the behaviour of the physiological index. Typically, though not always, this has taken the form of coincidence in time, for example, the skin resistance of a percipient might drop at the time a distant agent suffers an electric shock.

The second class of study has an elaborate rationale underlying it. It has been argued that at some point in ESP, information about the target must, in some unknown fashion, make its way into the nervous system of the percipient. Behavioural responses aberrantly related to the target, such as psi missing or symbolic transformation of drawings are generally interpreted as mis-processing of this initially received information, thus indirectly revealing the

existence of an extensive processing network between reception and behaviour.

If this is so even with successful subjects, it has been proposed, the problem in getting an untalented subject to score in a psi task might lie largely in this processing rather than in the primary ability to receive psi information, which is proposed to be latent in most or perhaps all of us. Put boldly, we all have the machinery to receive psi messages: it is merely that most of us have no conscious access to it.

On this basis, the nearer to the point of reception in the organism the signal can be tapped the less it will be subject to mis-processing. Results therefore should be less dependent on the constellation of psychological factors apparently so crucial to the usual behavioural methods of response registration. It has generally been assumed, by analogy to some studies in subliminal perception, that physiological measures are able to tap an earlier stage in the processing than behavioural read-out.

If all of the preceding is valid some very important empirical consequences follow. The primary one is that psi may be more readily detected using appropriate physiological measures than by the usual techniques requiring a conscious response.

How reasonable are the assumptions made? They can be criticised at almost every point. One should certainly be aware that the data from psychophysiological measures of subliminal perception, upon which one critical supposition rests, are themselves the topic of heated debate (Dixon, 1971) and should not be regarded as securely established. It would certainly be unwise lightly to equate the unconscious processing of the parapsychologist with the physiological changes measured in psychophysiology.

A more important assumption, however, has until recently rarely been challenged: it seemed almost tautologous that ESP-derived information must be processed within the organism. It was unthinkable that scoring above chance in an ESP test should not be as dependent on complex preceding processing as, say, scoring above chance in a subliminal perception task. Yet theorists have recently struck a blow at the very root of this view. Stanford (1978) has expressed grave doubts about ESP experiences being a degraded form of a received signal. In this the observational theorists Schmidt and Walker (Millar, 1978) are in agreement.

The model these workers share considers all psi to be modelled after the same pattern as Schmidt's time displacement PK work. Here the subject tries to affect some appealing display by PK: unknown to him, however, the display is driven not by a currently active RNG but by outcomes which were recorded at some previous time

without being observed by anyone. On these theories it is at the observation of the feedback that things of interest happen in the subject's physiology, triggering his psi source, in Schmidt's nomenclature. In the case of ESP the RNG which is affected by this triggering is none other than the percipient's own behaviour at the earlier time when he had the experience.

On the model of the observational theories, then, the percipient is doing nothing extraordinary when having a psi experience, though he may behave in a less deterministic way, thus making himself more susceptible to PK influence. If this view is true the effect of psi should be strongest at the level of the behaviour itself, being less apparent on any of the ,presumably many,random elements of the percipient' physiology which underly his behaviour. The situation is directly analogous to the majority vote (MV) technique in PK research. In this case to determine the outcome of a single trial an RNG is triggered not once but a number of times: the majority vote of these individual bits, which are not themselves observed by the subject, determines the result of each trial. If the feedback the subject receives is the majority vote, then he will score on this level, with the effect being randomly distributed over the bits comprising it (Kennedy, 1978).

On an empirical level, too, Schouten (this journal) has provided some support for Stanford's condemnation of the notion that anything less than a full-blown quasi-perceptual experience is degraded psi. Schouten has shown, within the limits of the case material he analysed, that spontaneous cases rarely give many details in addition to those already known by the percipient. Far from the typical one-detail cases being attenuated psi his data suggest that the vivid quasi-perceptual cases are the interlopers. These seem to be largely fantasies built out of normally acquired information with the addition of a single ESP-derived detail.

The "self-evident" assumption of degradation of psi information by processing within the organism has therefore had some of its supports gently nudged from under it: we must consequently enquire whether it can be kept aloft by the empirical evidence.

OVERVIEW OF NON-EEG AND EARLY EEG WORK

Excellent reviews of much of the relevant material are given by Morris (1977, p.709-710) and Beloff (1974). GSR, plethysmography and EEG have been examined as possible physiological psi detectors in a number of experiments.

Woodruff and Dale (1952) conducted perhaps the first adequate parapsychological experiment using GSR. It was unsuccessful as a detector. At the time of writing I do not have a copy of the original

paper: hence this work will not be treated further here. GSR has continued to be rather unsuccessful. Only one (Schouten, 1976) of the four adequately performed and reported studies in the classical mould found evidence of information transfer by this means, compared to the three (Tart, 1963; Sanjar, 1969; Beloff, Cowles and Bate, 1970) which did not. An experiment by Johnson and Hartwell (this journal), using skin potential instead of resistance also yielded no evidence of psi. However, Braud (1977) has reported success using GSR, though this was conceptualized as a PK experiment using the physiology merely as target system for Braud's own PK.

The early plethysmographic work (Figar, 1959; Esser, Etter and Chamberlain, 1967) did not allow statistical assessment. Development of the plethysmographic method is primarily due to Dean: unhappily most of his work appears as brief abstracts which it is not possible to evaluate adequately and as Beloff (1974) has pointed out they are "slight affairs" with perhaps only a single subject. I am aware of only two adequately documented reports (Dean, 1966; Dean and Nash, 1967) and these involved two and one subjects respectively. Tart (1963) found some evidence of information transfer using plethysmography; but Nash and Nash (1962), Sanjar (1969) and Schouten (1976) did not.

The brain itself, as the information-processing centre par excellance, seems by far the best bet for generating appropriate physiological measures. This did not escape the notice of parapsychologists and as early as 1958 (ASPR research committee, 1959) tentative attempts were made to induce EEG changes in a (drowsy or sleeping) subject by the photic stimulation of a remote agent. The equipment was primitive and no evidence of psi could reasonably be expected, nor was it obtained. Tart (1963), however, reported some evidence of increased EEG complexity as measured by an electronic period analyser in subjects at times when a painful two second electric current was passed through either Tart or a resistor, as compared to control periods. One point of particular interest in this study was that in contrast to the physiological measures his subjects gave no evidence of comparable differences at a behavioural level (pressing a key). This experiment was near the state of the art, given the instrumentation then available. There is one caveat, however. It seems that the analyses originally planned (Tart,1975) were between experimental and control periods, rather than between each of these and control periods arbitrarily chosen 10 seconds after the start of each two second trial. It is therefore not clear whether the three significances found in the 13 analyses were the results of psi or of data scrounging.

Duane and Behrendt (1965) reported the elicitation of alpha rhythm in one identical twin by eye closure of the other. Two out of 15 pairs of twins were reported to demonstrate this effect. However, this report is so sketchy and the work so methodologically inadequate that it constitutes little more than a clinical impression.

Recently there has been much progress in EEG technology and in particular the wide-spread availability of general-purpose digital computers has ushered in a great leap forward in the sophistication of data acquisition and analysis.

STATISTICAL CONSIDERATIONS

The capability for powerful analyses at the press of a button, however, gives rise to new problems. In general, the more powerful the method, the more numerous and elaborate are the underlying assumptions. It therefore becomes more important to be aware of the conditions under which a given statistic is likely to be invalid. The most usual methods of analysis of EEG data involve analysis of variance (ANOVA) (Kirk, 1968) and its multivariate extension (MANOVA) (Morrison, 1967). The techniques of ANOVA are quite familiar in parapsychology where they have been widely used in the analysis of forced-choice guessing data. They are well-suited to this kind of material: because the target generation is of a well-defined nature under experimental control it is known a priori that the assumptions of ANOVA are met (at least in the absence of psi).

In cases where the statistics of the dependent variable are not known (such as EEG measures) a number of tests of the assumptions have been developed. However, as Cochran (cited in Kirk, 1968, p. 60) has pointed out, with any particular set of data it is in principle impossible to ensure that all the assumptions are exactly satisfied. ANOVA techniques in this kind of situation are therefore approximate at best. Luckily it has been found in practice that most of the assumptions can be grossly violated without unduly affecting the result (Kirk, 1968, p.60-63) and ANOVA techniques are in common use by psychophysiologists. The same is broadly true of MANOVA, though there exists rather less empirical evidence on the effect of violation of the assumptions. It is, however, of considerable importance to note that while extremely elastic, these statistical tests are not indefinitely extensible. There exist many circumstances in which grossly misleading results are obtained by a blind application of these methods. It is the responsibility of the user to ensure he is not misled.

In parapsychological work researchers have particular reason to avoid this kind of error, which is not shared by workers in closely related non-parapsychological areas. Despite the claims of some

parapsychologists (Honorton, 1978), most researchers consider that work on psi has, as yet, an extremely limited record, with respect to experimental repeatability (Pratt, 1978, p.135-137; Millar, 1979). In parapsychology the odd and unusual forms the very subject matter for enquiry: the feats of mediums of a century ago are still discussed with interest. In most sciences results which do not replicate are quickly forgotten, rather than being singled out for special attention. Under these circumstances parapsychologists are obliged to take more pains than other scientists in ensuring that a result apparently showing psi is not in fact a mere artefact of inappropriate statistical evaluation. Whether a finding is more likely to be due to psi or to violation of some statistical assumption is a question without a universal answer. It is clear, however, which possibility is the more conservative.

These comments are not intended to discourage the use of sophisticated statistical analysis in parapsychology. On the contrary this writer is in substantial agreement with Burdick and Kelly (1977, p.82) who comment how unfortunate it is that parapsychologists have so far barely begun to make effective use of the greatly expanded resources of contemporary research methodology. Like any other tool, however, efficient use demands knowledge of its limitations.

Complementary to the analytical procedures discussed above, another set exists (favoured by this writer under many circumstances), for which minimal assumptions are made about the data. Siegel (1956) provides a useful compendium of the more commonly used non-parametric tests. Both sets of procedures have strengths and weaknesses. The particular value non-parametric tests have for parapsychological work lies just in their robustness. Non-parametrics are generally slightly less powerful than their parametric equivalents under conditions where the parametric assumptions are met,but are in some cases even more powerful when they are not. On the other hand the greatest strength of parametric techniques is probably the diversity of the problems which can be readily tackled by these methods. In contrast to the off-the-shelf nature of informative parametrics, equivalent non-parametrics are sometimes hard to come by, if available at all. The considerations mentioned above are nicely illustrated by the material now to be discussed.

EVOKED POTENTIALS IN PARAPSYCHOLOGY

Three different kinds of EEG evoked potential (EP) have been used in psi experiments, the contingent negative variation (CNV), the frequency domain EP and the time-domain average evoked response.

Of the frequency-domain work most of the effects reported have involved alpha-blocking rather than a true EP (driving) but these are included here for convenience.

Levin and Kennedy (1975) pioneered the assessment of the CNV as a psi indicator. The CNV is a slow negative shift of the potential of the cerebral cortex which is seen during the preparation for a motor act. Typically a warning stimulus is given to the subject and an imperative stimulus, on which he is to act, follows a second or two later. The CNV appears in the interval between the two stimuli, anticipating the actual occurrence of the imperative stimulus. This electrical change has sometimes been considered as a physiological concomitant of expectancy. The novel twist introduced by Levin and Kennedy was to make the motor act dependent on the outcome of a binary RNG. Consequently it was possible under these circumstances to determine whether the CNV waveform was a function of expectancy alone or whether it might depend also in part on the future behaviour of the subject, itself determined by the RNG. While initial results seemed promising (Levin and Kennedy, 1975) more thorough analysis did not confirm this and in the sequel (Kennedy, 1977), as yet unpublished, no significant results were found.

One important consequence of this work was the stimulation given to Hartwell (1978) who carried out the most sophisticated psychophysiological study so far attempted in parapsychology, building on this basis. Working at the University of Utrecht his design improved on the earlier work both in terms of psychological presentation and technical as well as analytical expertise.

In light of the earlier work Hartwell did not expect to find obvious differences between CNV waveforms depending on the future RNG outcome, at least when this was determined by the multivariate methods employed formerly. The initial employment of such techniques was rather like trying to locate the odd man out in a pile of unspecified objects: what was needed for efficiency was some idea of where and how to look. For this Hartwell appealed to subliminal perception. If the subject has access to a slight amount of information about the identity of the forthcoming imperative stimulus, this should give rise to the same kind of CNV differences between outcome conditions, regardless of whether the information is obtained by sensory or extra-sensory means. If this is true it should be possible to make predictions for the outcomes of ESP trials on the basis of trials for which information is presented subliminally. By similar reasoning subliminal trials ought to be classifiable on the basis of the differences in CNV observed when the subject is fully informed whether an imperative stimulus will be presented. This provides a partial check on the validity of the

assumption. In practice Hartwell incorporated the three different levels of sensory information into the warning stimulus, which consisted of a projected slide with full, low (or non-existent) contrast. These were smiling or frowning faces, a smile cueing the subject that a motor response would be required.

The whole experiment was carried out under computer control: the machine controlled presentation of the appropriate slides, consultation of the RNG and the acquisition of the EEG data. From the sensory CNV waveforms a discriminant function was calculated which discriminated significantly between the two contingency conditions. This was then applied to each ESP trial (48 maximum for each subject), giving rise to a single number for each trial, representing the strength of the bet that the waveform was of type x. A t-test between the two contingency conditions determined whether the prediction had any validity. For the ten subjects examined it did not, even though the subliminal trials did prove to be classifiable on the basis of the full information trials by these methods. The same story was repeated with a further ten subjects (Hartwell, this journal).

Targ and Puthoff (1974) reported an experiment in which production of alpha waves was determined by the behaviour of a remote flashing lamp (alpha blocking). A single subject (H.H.) was used, selected from six on the basis of success in pre-trials. The remote lamp (usually observed by an agent) either did not flash at all or at 6 or 16 fps for periods of ten seconds. Data were collected in sets of 36 trials, 12 of each of the three frequencies randomly inter-mixed. Seven sets were collected in toto. One second before each period an alerting tone was presented to both percipient and agent. EEG corresponding to the last four seconds of the period was selected for analysis so that the effects of the warning tone on the EEG had largely died away. Each four second epoch was subjected to Fourier analysis to give a power spectrum. Measures of peak power and average alpha power were calculated from them. To determine whether a real difference existed between the flashing and control conditions t-tests were applied as an approximation to the randomization test. For the 16 fps periods both peak and average alpha power were less than in the control periods to a marginally significant degree ($p<0.03$ and 0.04 respectively). The 6 fps periods did not differ significantly from the controls.

This report suffers greatly from the space limitations imposed by publication in Nature and perhaps because of this is open to a number of criticisms. Insufficient details are given about how each trial was initiated: was this automated or did the inter-trial interval depend on the experimenters in some way, thus leaving open the possibility of subject cueing by timing differences? It is not

quite clear that the pre-trials were excluded from the set of experimental data proper, though this may perhaps be assumed. Nor is the procedure involved in defining the alpha band for the average power measure described apart from the comment that it is derived from average spectra. An unsatisfactory feature is the use of no less than four different agent conditions in the seven sets of data collected. This may well give interesting hints about the phenomena but adds to the impression that this was very much a pilot study, with all the methodological imperfections this necessarily implies. Under such circumstances one tends to wonder what other on-going modifications to the experiment were being carried out, which have remained unreported?

Seven seems a very odd number of sets to take: was this pre-specified or controlled by external factors, or could the experiment have been discontinued unwittingly on a decline becoming apparent? As to the analysis, t is an acceptable approximation to the randomization test only when the kurtosis of the combined samples is small (Siegel, 1956, p.154-155): the report does not specify whether it was. A Mann-Whitney U-test might have been preferable since not only is a test of kurtosis unnecessary, but it can be more powerful than the t-test (Siegel, 1956, p.155-156). The authors comment that the t-tests were applied to all the data (i.e. seven sets) but the degrees of freedom they give correspond to only five sets (df=118 instead of 166). It is not clear what they have done here. It should perhaps be noted too that from the four analyses they have produced two significances. If one simply multiplies the probabilities by 2 to correct for multiplicity of analyses, the values become non-significant at the lowest conventional level.

An important feature of the experiment was that in addition to the EEG measure guesses were made about the targets. While Targ and Puthoff comment that these were at chance level the data are not reported. Finally it should be noted that they have been unable to replicate their results in further work.

Kelly and Lenz (1976) reported a pilot study based on the Targ/Puthoff design. A single subject, Kelly himself, was used because of his monochromatic EEG spectrum. Each of the five sessions they report was a self-contained mini-experiment with slightly modified set-up. Each session consisted of 60 trials of eight seconds duration. On each trial a remote lamp, without an agent observing it, either flashed at 16 fps or was not activated. The trials were randomly ordered with random inter-trial delays and the whole experiment was conducted under computer control. For analysis the eight seconds of each trial was split into four partly overlapping segments: 0-1; 0-2; 0-4; 4-8 seconds. These were converted to power

spectra and it would seem, though this is not clearly stated, the spectra were then blocked to 64 variable representation. The data were analysed by MANOVA to determine if an overall difference existed between strobe and no-strobe trials.

A series of results were reported:

First session: There was no overall MANOVA significance but a trend was observed to alpha blocking with strobe-on for the first four seconds.

Second session: A warning bell was introduced eight seconds prior to each trial. It is not clearly stated that there was no overall MANOVA significance, though this perhaps may be implied. For the last four seconds, however, there was a trend to alpha-enhancement with strobe-on.

Third session: On this occasion Kelly became drowsy. The overall MANOVA result is not reported, only some univariate tests which show alpha-enhancement with strobe-on for the first four seconds.

Fourth session: The overall MANOVA result is not reported. Univariates reveal alpha blocking with strobe-on for the first second.

Fifth session: This was almost identical to the fourth session. Likewise the overall MANOVA is not reported. Univariates show alpha blocking and sub-multiple driving at 8-9 Hz for the first four seconds with strobe-on.

I must confess here that my impression on first reading this paper was that these workers kept on applying tests until they found something "significant". On talking to Kelly, however, he assured me that their procedure was perfectly proper, proceeding to univariate tests only to further elucidate an overall significance. It is a great pity, then, that little trace of the multivariate statistics is to be found in their paper. I have not even been able to work out from their sketchy description what exactly was the multivariate design employed. Consequently it is impossible to evaluate the work. The set-up they describe is perhaps the most advanced available to parapsychology. It would be a great pity if their heroic efforts to construct such a system were to distract their attention from running subjects through it and reporting the results in a satisfactory way.

One criticism which has been leveled against these workers, however, is quite unfounded. Stokes (1977) argues that the results are invalid because Kelly, as subject, knew his own hypothesis. This is equivalent to saying that the results from a card-guessing test are invalid if the subject knows his task is to score above chance.

The first attempt, of which I am aware, to detect a time-domain average evoked potential to telepathic stimulation was reported by

Silverman and Buchsbaum (1970, p.155). An agent observed a strobe lamp which flashed every few seconds. The percipient's EEG was sampled for a period following each flash and the sweeps were averaged. Results were non-significant. It is not clear whether they tested one or four subjects: the experiment is mentioned only in passing and details are too sparse for assessment.

A similar kind of procedure was employed by Lloyd (1973): in this case the agent tried to transmit a mental image whenever a timing light flashed,once every second. 60 sweeps of 500 mS EEG were averaged. Lloyd presented two curves from a single subject, selected from an unreported number of attempts. He considered that these represented a psychic evoked potential of similar form to an auditory EP. Whitton (1975), however, later added that the telepathic EP had different forms with other subjects.

The question of whether the bumpy lines obtained were telepathically evoked potentials or merely residual background noise seems not to have been considered. Nor is any information given about the possibility of sensory leakage: could the percipients have been picking up subliminal sensory cues? The report as it stands is totally inadequate for anything other than establishing scientific priority. It is described as a pilot study and mention is made of publishing further details at a later date. This has not been done: the only available evidence for the Lloyd effect thus lies in two rather dubious curves from a single subject representing a miniscule two minutes of testing.

This reviewer (Millar,1975) carried out an experiment which was modelled on Lloyd's, with the addition of control periods in which the agent was not stimulated. 20 subject pairs were employed. For each pair the experiment consisted of a total of 256 sweeps experimental and another 256 control. Each set of 256 was divided into eight 75 second trials of 32 flashes at the rate of one every two seconds. The trials were ordered as eight randomized pairs (EC or CE) randomly selected for each subject pair.

There was no evidence of a psi-evoked potential in either individuals or the group as a whole, either using an objective amplitude measure or with blind judging. Curves similar to Lloyd's were, however, sometimes observed in both conditions due to the normal processes of EEG averaging. A companion piece (this journal) gives further details on this and Lloyd's experiment.

Instead of trying to detect an evoked potential Hearne (1977) tested whether the EP produced in response to unpatterned illumination would be modified by an agent. At the same time as the percipient's strobe lamp flashed the agent briefly saw a photograph of him. In addition to determining if there was any difference between experimental and control trials on which there

was no agent Hearne wanted to see if the degree of emotional closeness of agent and percipient had any influence on the result. Consequently he used two groups of pairs, four emotionally close, four not-close. Eight sets of 100 flashes were presented to the percipient at two second intervals. For each pair an individual order of four experimental and four control periods was determined on the spot by the agent's experimenter randomly selecting balls from a bag. EEG was taken from an occipital site 3 cm above the inion, referenced to left mastoid. An on-line computer performed the averaging of each of the eight sets of 100 sweeps of the 500 mS following the flash.

Hearne measured the latency and amplitude of each of the four main peaks of the visual EP. Amplitude was measured relative to the first point. Eight analyses of variance were performed, one for each measure. Of these, one, the amplitude of the earliest (N65) peak, showed a difference between experimental and control periods ($p=0.025$); furthermore an interaction between experimental condition and emotional closeness was apparent ($p=0.001$). Further investigation caused Hearne to conclude that this was due to a difference between experimental and control periods for the not-close group only ($p=0.01$). On average, in this group, the experimental condition had a peak smaller by $3\frac{1}{4}$ units than the control, though it is not clear if this represents microvolts.

The conduct of the experiment is exemplary: the analysis, however, is not so happy. The method whereby Hearne evaluated the amplitudes and latencies is not disclosed, though we may almost certainly assume it was performed blind. The kind of analysis of variance is described no further than to say it was performed by computer. However, it is, I believe, a type SPF-p.qru in the nomenclature of Kirk (1968, p.308): A is close/not-close; B is experimental/control; C is first/second half of experiment and D is order within each half. Of the 15 tests to which such analysis gives rise Hearne reports only the three of interest to him. The follow-up analysis for the not-close group only, seems to be a 3-way factorial with a single observation per cell.

It is at once noticeable that the error term in the main analysis for both significant effects (here B x subjects within groups) is smaller by a factor of about 700 than the between block error term. This is due to the high correlation between repeated measures. Under these circumstances the ANOVA assumptions are put under considerable strain and the question may be raised whether the effects detected are rather due to the error term being, for some reason, artefactually low.

One avenue of approach is to determine whether the effects reported continue to show up when the data are analysed using methods less

susceptible to violation of the assumptions underlying them. Fortunately Hearne gives the raw data, so it is possible to do this.

The main finding of an experimental/control (E/C) difference for the not-close group was first examined. To do this the E and C scores matched in terms of subject, half and order within half were compared: Wilcoxon T was applied to these 16 pairs as an approximation to the matched randomization test. The T obtained, 37.5 is not significant, thus lending no support to Hearne's contention of a real difference between experimental conditions.

The interaction significant in Hearne's analysis was then examined. The meaning of this is that the E/C difference should be different for the close as compared to the not-close group. To investigate this a single difference score (E-C) was calculated for each of the eight subjects and the four difference scores for the close group were compared with the not-close group by the randomization test (Siegel, 1956, p.152). All four scores in the close group are more positive than those in the not-close group. This corresponds to a probability of 2/70=0.0286. Hearne's data therefore support his inference that the close and not-close groups reacted differentially in response to an agent. It should be borne in mind, however, that this represents the selected best of eight different measures.

Nonetheless, the data do suggest that further work with Hearne's procedure is called for on a larger scale than the four subjects per block used here. The ultimate test of the reality of an effect is not the p-value but whether it can be reproduced.

In this summary of the use of evoked potentials in parapsychology, seven studies using three different kinds of techniques have been examined (excluding the passing reference of Silverman and Buchsbaum). The two using CNV (Levin and Kennedy, 1975; Hartwell, 1978) yielded null results, despite the excellent methodology of the latter experiment. Of the two frequency-domain EP studies (Targ and Puthoff, 1974; Kelly and Lenz, 1976) both are inadequately reported and both employed only a single subject. In the time domain Lloyd's (1973) work is a two minute affair with virtually non-existent methodology. Millar (this journal), using adequate procedures, found no evidence of psi-evoked potentials. The work of Hearne (1977) is excellently performed but the analysis and results remain sub judice: also four subjects per block is rather small for such an experiment.

Two observations may be made on this work. Firstly, the studies using a reasonably sized subject pool are almost universally unsuccessful in comparison to the single subject kind. Perhaps this represents a reporting effect, one never hears of the many small-scale pieces of work which did not show an extra-chance effect. In the

second place this kind of work tends to be singularly poorly reported. While quite understandable, such a state of affairs is particularly unfortunate in a highly technical area such as this.

PHYSIOLOGICAL PSI DETECTORS IN LIGHT OF THE LITERATURE

Having surveyed the literature the general questions raised in the introduction can be examined further. It was argued there that the traditional theoretical basis for the use of physiological detectors in parapsychology is quite dubious: if these ideas are to survive it must be on the basis of the empirical evidence. Do they? The question, it appears, has barely begun to be tackled experimentally: therefore the best that can be done is to form an impression on the basis of the available material.

The prime empirical consequence of the string of assumptions is that more subjects should give evidence of psi when using a physiological detector, than with behavioural read-out. Ideally, one could set up an experiment in which subjects randomly selected from a population are allocated to physiological or behavioural groups. The testing conditions should be equated in some way: pragmatically it might be best to compare the optimum conditions one could devise for each group and test both for the same period. The number of subjects attaining a given criterion could then be compared. No such experiment has yet been performed.

Perhaps one can make a rough comparison on the basis of experiments. Is there a higher proportion of successful experiments using physiological detectors than with more conventional procedures? For this purpose the experiments considered are those which are methodologically adequate and written up in reasonable detail at this time. Of the 13 eligible experiments listed in table 1 eight (62%) are significant on one or more measures: four of these significances are due to single subject experiments (for this purpose Braud is counted as his own single subject). From the residuum of 13, three (23%) are significant. These figures can be compared with the experimental studies listed under parapsychology in Psychological Abstracts for the period 1960 to 1970 (Billig, 1972), of which 88% reached significance or Honorton's (1976) survey of the card guessing work 1934-1939, which boasted 70%. By these criteria the success rate of physiological detector experiments seems rather below the norm. While, realistically, these allegedly normative figures probably reflect Journal publication policy much more than the actual success rates in psi experiments there is certainly no evidence that physiological work is more successful than other methods.

Another possibility exists for testing the traditional model:

TABLE 1

Summary of adequate experiments with physiological detectors

Experiment	Measures	Results	Number subjects	E Psi conducive
Nash and Nash (1962)	pleth	n.s.	10 pairs	
Tart (1963)	GSR	n.s.		
	pleth	sign.	11	yes
	EEG	sign.		
Dean (1966)	pleth	sign.	2	
Dean and Nash (1967)	pleth	sign.	1	
Sanjar (1969)	GSR	n.s.	20 pairs	
	pleth	n.s.		
Beloff et al (1970)	GSR	n.s.	20	no
Schouten (1976)	GSR	sign.	33	no
	pleth	n.s.		
Braud (1978)	GSR	sign.	1	yes
Targ and Puthoff (1974)	EEG p.spectrum	sign.	1	yes
Kelly and Lenz (1976)	EEG p.spectrum	sign.	1	
Hearne (1977)	EP modification	sign.	8	
Hartwell (1978)	CNV	n.s.	10	no
Millar (1975) and this journal	EP generation	n.s.	20 pairs	no

do subjects who score when using a physiological measure also score when using behavioural methods? This question has received some experimental attention. It does not seem to have been realised, however, that this is but one of the twin queries to which answers are necessary. The other is: "Do subjects who score when using a behavioural measure also score when using physiological methods?" It is all too easy to conceive of plausible reasons why a subject might not show overt evidence of psi yet reveal it via his

physiology. It might equally be that whose psi affects their observable behaviour show no physiological reactions. Only if the proportion of subjects characterized as 'physiological but not behavioural' exceeds that of 'behavioural but not physiological' is this in line with the traditional model.

Of the two studies in which both behavioural and physiological indices have been measured Tart's (1963) is, as mentioned above, somewhat suspect in terms of "post-hocery". The subject in the other relevant study (Targ and Puthoff, 1974) was Hella Hammid, who has proven to be extremely successful at remote viewing (Targ and Puthoff, 1976). If she is typical of subjects successful in EEG work it seems that as far as psi detection is concerned we would be well advised to put our amplifiers into retirement and concentrate on a bit of practice instead. The experimental data therefore do not provide much support on this point for the traditional model, though further work looking at both of the twin queries seems called for.

Is psi, as measured by a physiological index, less dependent on psychological factors? It would be possible to set up an experiment in which psychological conditions were manipulated similarly for groups using respectively physiological and behavioural responses; but this has not been done. Nor does what data there are bearing on the matter give support to the traditional model. In Hearne's (1977) study the major difference apparent was that between two psychologically different groups of pairs (emotionally close versus not-close). Also with Kelly and Lenz (1976) the nature of the EEG effects observed seemed rather dependent on Kelly's psychological state at the time. Furthermore, for what it is worth, Targ and Puthoff (1974) attributed Ms. Hammid's failure, on one occasion to react to the flashing lamp, to anxiety caused by the knowledge there was no agent. It is an open question whether these observations represent a lesser degree of dependence on psychological factors than would be obtained in non-physiology experiments: certainly, however, it does not seem that psychological involvement has been largely eliminated by the use of physiological measures.

One thing well-known about psi is that its appearance is very dependent on the experimenter. It has often been maintained, though without much evidence, that this is due to psychological interaction with subjects. If this is so, then it is of interest to enquire whether the pattern of psi-conducive experimenter/positive experiment and vice versa is less pronounced here than elsewhere. Because the number of data available are insufficient upon which one could reasonably expect to base any firm conclusion the experimenters in table 1 have simply been informally classified (last column)

into psi-conducive and psi-nulls, as well as unknown or other, on the basis of their other work. Of the three psi-conducive experimenters all reported significant experiments, while of the four psi-nulls only one, Schouten, did not run true to form. The apparent difference is, of course, not significant by Fisher's exact test, but, at any rate, it does not suggest that experiments using physiological detectors are markedly less dependent on the experimenter for their outcome than are other studies.

Overall there is very little in the literature supporting the traditional model and much seems to cast doubt on its truth.

It is, of course, possible to retain these ideas: one could argue, for example, that insufficient work has been done to justify premature rejection. This is manifestly true: one would certainly like to see much more experimental investigation of any validity the model may have. Unfortunately, in virtually all of the available material it has simply been assumed and no further checks have been made. It is possible, too, that it is basically true but simply over-optimistic in the estimate of the occurrence of psi in the general population: for example if one psi source in 50 could be found by physiological methods, as against 500 by normal means, such a difference would be undetectable on the basis of the extant experimental work.

ALTERNATIVES TO THE TRADITIONAL MODEL

It cannot be denied, however, that empirically the traditional view currently has only the most tenuous support. Under such circumstances it would be negligent not to enquire if alternative models exist and whether they are more compatible with the experimental evidence.

There is only one serious contender known to this writer, namely the view of the observational theories. On this basis, as was pointed out in the introduction, there is nothing extra-ordinary happening in the subject's physiology at the time he gives evidence of ESP: he is then acting merely as a rather complex random generator. This generator is acted upon by the triggering of the subject's (or possibly someone else's) psi source at the later time he receives feedback.

If there is nothing special happening in the subject until he gets feedback how can there be any evidence of physiological events which can be used to detect psi? There seem to be two principal possibilities: the subject may use his 'backward PK' in such a way that he becomes aware at the earlier time of the ESP information and then reacts physiologically to it: alternatively the physiological measure itself may be the part of his behaviour affected. Which happens depends on his psychological set at the

time of feedback and upon what kind of feedback he receives. The former possibility, physiological reaction to information received, is not of much interest. The latter, however, PK of his own physiology feedback, entails a considerable number of empirically testable consequences.

According to the observational theories any random process is potentially as good as any other similar random process as a system on which to demonstrate PK. Also Stanford's (1977) review of the PK literature (especially pages 368-370) suggests that just about any system with sufficient unpredictability on a macroscopic scale can be affected by PK. It therefore seems safe to consider any system with sufficient slop as a potential PK target.

Some experimenters will view with dismay the idea that their hard-won, elaborately processed physiological measures constitute just 'noise' on which PK may be exerted. How can this idea be compared with the classical one which regards physiological detectors as tapping part of the necessary cerebral processing machinery?

In priciple the answer seems clear enough: PK on physiological measures should behave like its siblings on other target systems, whereas real physiological indicia would be expected to behave more like the correlates of sensory events. If the physiological results are really due to PK in disguise then a scoring rate of about 50 to 60% on a $p=\frac{1}{2}$ target, strong dependence on the experimenter etc., could be expected. In practice, as noted above, the physiological material does not seem to differ noticeably from other psi experiments. Within the limits of the available material, then, the PK view seems to fit the facts somewhat better than the traditional one.

One rather basic question which is raised by the observational theories and which has yet to be put to definitive test is whether biological, rather than physical systems, are more susceptible to psi influence, as the traditional model posits. Alternatively will any system, animate or inanimate, do just as well as a PK target, providing it has the same degree of associated uncertainty, as the observational theories predict. This question is fundamental to the psychophysiological parapsychologist. Why bother laboriously producing a technically difficult measure if any old wiggly line from a worn-out amplifier will do just as well?

While much more incisive experiment is called for before any confident decision can be made, what small indications there are seem to favour the observational theories. Furthermore the possibility does generate a number of intriguing empirical consequences. One could, for example, intermix trials in which the 'physiological' measure the subject sees is either real EEG or

pseudo-EEG generated by an RNG/computer. On the PK view the subject should have the same effect on both.

Although physiological measures are rather appealing PK targets only a single experiment has in fact been performed using this kind of set-up. Braud's (1978) study in which he apparently affected the skin resistance of sensorially isolated subjects has already been mentioned. This work, in which a successful experimenter revealed that he is also a strong psi source highlights a general problem which appears in the formulation of the observational theories as well as in reality (Kennedy and Taddonio, 1976). The nominal subjects in most physiological detector experiments never see their results. If the observational theories are right then how can 'subjects' be the source of any psi effects observed? Is, then, the physiological work, even more than most parapsychology, a recipe for measuring not subject but experimenter psi? As Braud's experiment shows, a subject's physiology may be as susceptible to the experimenter's (backward) PK as to the subject's. Further work will have to come to grips with the experimenter contribution before a firm conclusion can be drawn from any experiment (Millar, 1979).

THE FUTURE

While the observational theories imply that most of the physiology work till now has been looking at the wrong place they also suggest where to look for real physiological events. When a successful subject receives feedback his psi source is triggered. Can that be taken literally, rather than as a mere mathematical abstraction? It should be possible to find out by using the kind of evoked potential techniques discussed above and their modifications, such as Wiener filtering (Nagawa et al, 1973).

The feedback is under experimental control and the psi-source triggering is presumably time-locked to that presentation. It may therefore be possible to find differences in the evoked potential to feedback between scorers and non-scorers, under psychologically matched conditions. If any such difference were to be found in this experiment or in its many obvious modifications it could prove a first step to pinpointing the physiological pathways involved in psi.

While dangling this rich prize, the observational theories seem to taunt: "First catch your psychic!"

SUMMARY

The literature on the physiological detection of psi is critically

reviewed, particularly in relation to the assumptions underlying it. It is concluded that these traditional assumptions are not supported by the empirical evidence. An alternative model, in terms of the observational theories, is offered and suggestions are made for experimental testing. In particular, it is proposed that cortical potentials evoked from high-scoring subjects by their sensory feedback, should be examined.

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PA's 1979 CONVENTION

The Twenty-Second Annual Convention of the Parapsychological Association will be held August 15 - August 18, 1979, at St. Mary's College, Moraga, California. The Convention will be sponsored by John F. Kennedy University.

Dr. John Palmer, President of the PA, is also functioning as Chairman of the PA Program Committee.

The deadline for submitting a paper has already expired (May 1, 1979). The deadline for proposing a symposium also expired on May 1, 1979.

The deadline for a research brief is June 15, 1979. Research briefs are short papers, up to one thousand words, detailing recently completed research or research in progress. A full and complete description of all aspects of the study (with the exception of work in which data have not fully been gathered or analyzed) should be supplied as a supplement to the brief when this information, for reason of space, cannot be included in the brief itself. Even in the case of incomplete studies, the proposed research brief must include the form of data analysis planned. Three copies of each proposed research brief should be submitted, typed double-spaced on one side of the sheet only on $8\frac{1}{2}$x11 inch paper, in a camera-ready form, to the Chairman of the PA Program Committee (Dr. John Palmer, John F. Kennedy University, 12 Altarinda Road, Orinda, CA 94563, U.S.A.).

Any Member or Associate of the PA may propose a roundtable to the Chairman of the PA Program Committee, provided that the proposal includes copies of letters or statements from the planned participants indicating their intention to attend the Convention. The deadline for such a proposal is June 15, 1979.

Roundtables should be relatively informal, and a maximum of spontaneous interaction between the participants and between the participants and the audience should be encouraged. However, formal presentations has to be restricted to five minutes. It is imperative, that each participant prepare a brief abstract describing his or her presentation or points to be raised for discussion.