

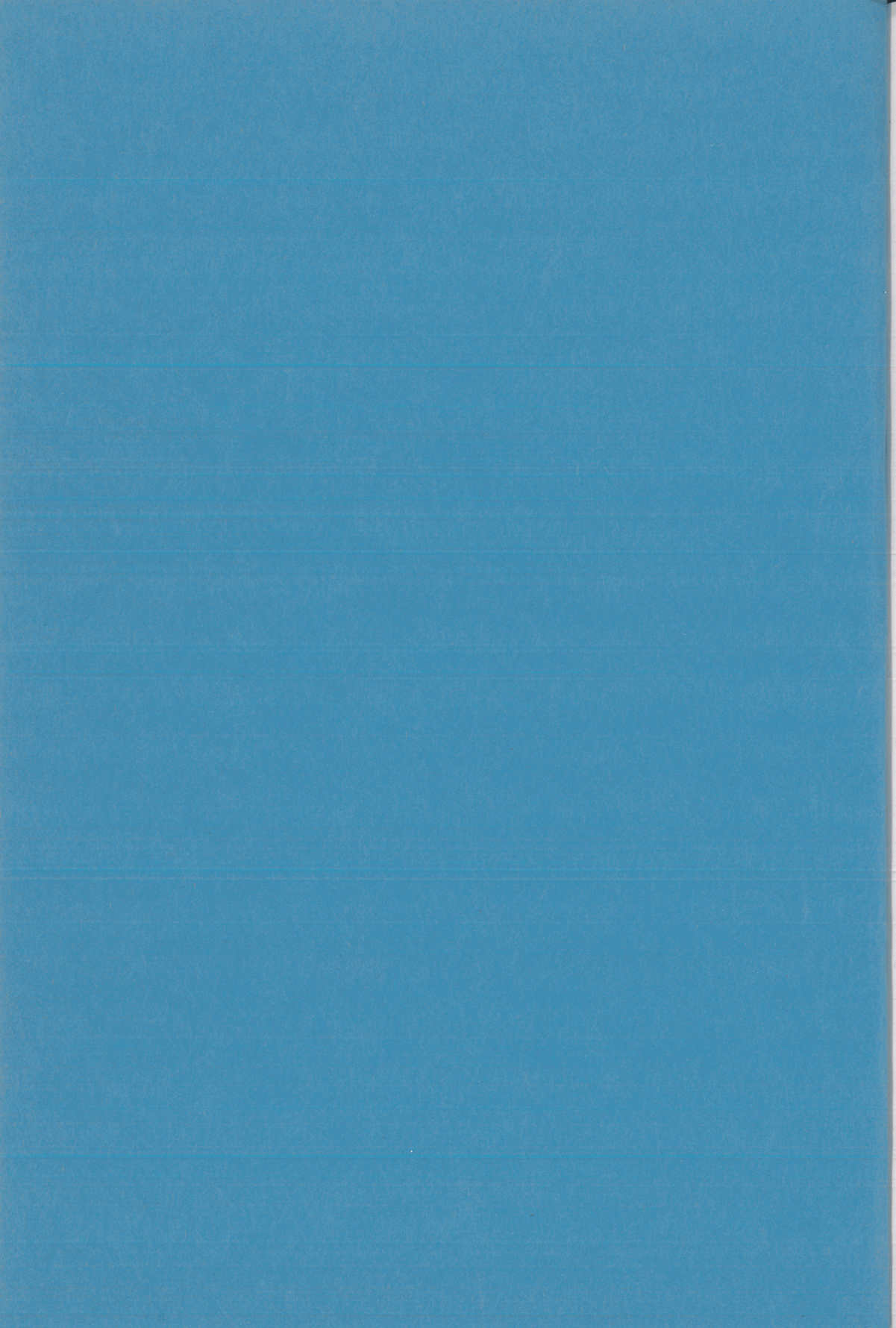
# RESEARCH LETTER

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SCORING IN A PRECOGNITION TEST AS A FUNCTION OF  
THE FREQUENCY OF READING ON PSYCHICAL PHENOMENA  
AND BELIEF IN ESP

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Extrasensory perception has been defined as a "response to an external event (perception) not presented to any known sense" (Pratt et al. 1940). From the beginning of Rhine's pioneering work at Duke University parapsychological research was primarily oriented towards proving experimentally the existence of its main object of study, namely extrasensory perception (ESP) and its different forms (clairvoyance, telepathy, precognition). During the last decades research in this field has been increasingly oriented towards searching for relationships between ESP and other, mostly psychological, variables (Stanford 1974). In experiments some such relationships have been found.

Schmeidler had observed that subjects who were convinced of the existence of ESP seemed to get higher scores on ESP card tests than those who rejected the idea of extrasensory communication. In extensive experiments with unselected subjects Schmeidler's informal observations were substantiated. It was found that "subjects who accepted (though sometimes with reservation) the possibility of paranormal success under the conditions of the experiment" scored above chance in ESP tests, whereas those "subjects who rejected this possibility" scored below chance (Schmeidler and McConnell 1958). Though replications have generally been hard to come by in parapsychology, Schmeidler's findings have found support in several replications (Musso 1965, Bhadra 1966, Ryzl 1968, Moss and Gengerelli 1968). Schmeidler referred to believers in ESP as "sheep" and to the sceptics as "goats". The "sheep-goat" variable has been considered one of the most, if not the most, consistent predictor of ESP yet discovered (Palmer, 1971). Not one but several related criteria have been used to define this variable, the most common being

the belief versus disbelief in the existence of ESP in an abstract sense.

It is an old finding that motivation is a crucial factor in ESP tests (Rhine 1973), which may either facilitate or inhibit the ESP scores according to belief or disbelief. Believers in ESP are likely to be better motivated towards test performance than disbelievers who will argue that only chance scoring is possible, which might have an inhibiting effect on their performance. Another indicator of high motivation for ESP tests might be the frequency of reading done on the subject of psychical phenomena. It seems plausible that the frequency of reading articles and books on psychical phenomena might reveal more distinctly a preoccupation with and actual interest in psychical phenomena than the common criteria of abstract belief in the existence of ESP. In a recent extensive survey on psychical experiences and attitudes towards the paranormal among the Icelandic population (Haraldsson 1975), a significant relationship was found between the frequency of reading on psychical matters and the reported frequency of psychical experiences.

In the present experiment we tested the hypothesis that ESP performance is related to the frequency of reading psychical material. Further we tested the old sheep-goat hypothesis.

#### METHOD

The experiment was carried out by eleven experimenters, all students of psychology but planned and supervised by the author. There were 449 subjects, some 25 classes in vocational schools, high schools and colleges. The ESP test consisted of each subject "guessing" which of five different letters (L,X,Y,O,Z) a computer would randomly select in each of 100 numbered cells that were printed on a sheet of paper that was given to each participant. It was stated that the computer would select the 100 random letters separately for each subject.

After the subject had filled in his 100 "guesses", he turned to a second page where he was asked to answer the following multiplechoice questions:

1. Do you think that the existence of telepathy and precognition is: a) unthinkable, b) possible, c) certain.
2. Do you read books or articles on psychical matters:  
a) often, b) seldom, c) never.

Two further questions were included on frequency of dream-recall and precognitive dreams which had also been a part of a previous experiment and are not relevant for this paper.



After all the subjects had completed their task they were numbered from 1 to 449 in alphabetical order for each class, classes of each experimenter numbered according to the age of the class, and finally experimenters were put in alphabetical order with all their subjects. Thus subject number 1 was that student who was first in alphabetical order of the youngest class of that experimenter who was first in the alphabetical order of the eleven experimenters.

When all subjects had been numbered we obtained from the university computer 449 sets of 100 random letters. For each subject that set of random letters that carried his number was filled into the 100 cells of his sheet.

It was counted as one hit each time a subject had written into a cell the same letter as the computer selected for that cell. The number of hits each subject made served as a measure of his ESP. All steps in the evaluation were carried out three times to avoid possible errors.

## RESULTS

By chance alone we can expect to get 20 hits on the average for each subject since five different letters were used and each subject made 100 trials. The mean number of hits for all the 449 subjects was 19.86 which deviates insignificantly from the theoretical mean.

When we divide the subjects according to the way they answered our questions, we find, however, significant differences in the mean performance of these groups. These differences can be seen in table 1 and table 2.

### Belief in ESP

Of our 449 subjects, whose mean age was probably around twenty, 20% reported being certain about the existence of telepathy and precognition, 74% were of the opinion that these phenomena could possibly exist, whereas only 6% thought they were unthinkable. These figures come very close to the results of our above mentioned recent survey of a random national sample of some 1100 Icelanders between thirty and seventy years of age, (Haraldsson, 1975b).

There were 90 Ss convinced about the existence of ESP. They obtained 1841 correct guesses, or 41 guesses above the theoretical mean ( $z=+1.08$ ) and thus obtained a mean of 20.46. Those 331 Ss who considered the existence of ESP a possibility showed a mean of 19.83. On the other hand, those 28 Ss who considered the existence

TABLE 1  
 Scores on a precognition test and belief in  
 the existence of extrasensory perception (ESP)

	Number of subj. of hits	Number of hits	Mean score	Dev. hits	z	P	Mean for males	Mean for females
Believe in existence of E.S.P.:								
Certain	90	1841	20.46	+41	+1.08	-	20.19	20.74
Possible	331	6565	19.83	-55	-0.76	-	19.61	19.99
Unthinkable	28	511	18.25	-49	-2.32	0.02	19.13	17.23
Total	449	8917	19.86	-63	-0.53	-	19.71	19.98



TABLE 2  
Scores on a precognition test and  
frequency of reading on psychical phenomena

Frequency of reading of psychical material:	Number of subj.	Number of hits	Mean score	Dev. hits	z	P	Mean for males	Mean for females
Often	105	2192	20.88	+92	+2.01	.04	20.44	21.07
Seldom	225	5077	19.91	-23	-.36	-	19.91	19.91
Never	89	1648	18.52	-132	-3.50	.0005	18.72	18.29
Total	449	8917	19.86	-63	-.53	-	19.71	19.98

of ESP unthinkable obtained for correct guesses a mean of 18.25 and 49 correct guesses below the theoretical mean. This deviation from the theoretical mean equals -2.32 standard scores. A Chi-square test (Armitage, 1971) that takes into account the ranks of the groups (a, b, c) reveals a significant relationship between the number of correct guesses and belief in the existence of ESP ( $\chi^2 = 5.59$ , 1 df,  $P = .02$ ). This is a confirmation of the old sheep-goat hypothesis. The sheep-goat effect is usually only found with a relatively large number of subjects. Expressed in percentages the deviations from the theoretical means are nearly always very small, the sheeps scoring slightly above chance and the goats slightly below, as in the present experiment.

#### Reading of psychical material

Of our 449 subjects 23% claimed they often read articles or books dealing with psychical phenomena, 57% would seldom read such material and 20% would never read it. The percentages of these young subjects are almost identical with those found in our national survey of the Icelandic population.

The main purpose of the experiment was to test the hypothesis if frequency of reading psychical material effects ESP performance.

As a glance at table 2 reveals there is considerable difference between the obtained means of correct guesses of those who read books or articles on psychical phenomena often, seldom or never. There were 105 who would read such material often. Their mean score was 20.88 with 92 correct guesses above the theoretical mean. ( $z = +2.01$ ,  $P = .04$ , one-tailed). Those who seldom read such material obtained a mean of 19.91 which is very close to the theoretical mean. 89 Ss claimed never to read about psychical phenomena. Their mean number of correct guesses was 18.52, and their result was 132 correct guesses below the mean chance expectation. ( $z = -3.50$ ,  $P = .0005$ , one-tailed).

By using the above mentioned Chi-square test we obtain for the three groups a  $\chi^2 = 16.62$ , (1 df,  $P = .00005$ ). This result shows a highly significant relationship between the number of correct guesses and the frequency of reading of psychical material and thus confirms our main hypothesis.

#### Dreamrecall and frequency of precognitive dreams

As in a previous experiment (Haraldsson, 1975) no relationship was found between these two variables as we assessed them and ESP.



## DISCUSSION

The results obtained confirm both the old finding that belief in ESP affects ESP performance and our hypothesis that there is a positive relationship between ESP test performance and the frequency of reading books or articles dealing with psychical phenomena. It must also be added that the old sheep-goat effect is only marginally significant (parapsychologists usually adhere to the 1% level of significance) in spite of the relatively large number of subjects, the second largest in any reported experiment of this kind. Though the sheep-goat effect has found several replications (Palmer 1971) and no experiment has been reported that shows a reversal of the effect (negative relationship between ESP and belief) this relationship is usually very slight so that a large number of subjects seems to be needed to demonstrate it. Though the believers in ESP as well as the disbelievers score in the predicted directions (believers above the mean chance expectation and disbelievers below it), only the disbelievers' scores are marginally significant.

Frequency of reading psychical material appears in this experiment to be a more effective predictor of ESP performance than the distinction according to belief in the existence of ESP. Here we find the believers' scores significant of the 4% level of significance and the disbelievers' highly significant.

It may be of some interest that for both of our hypotheses the bulk of the significance was contributed by the female subjects. The males' scores alone were not significantly affected by belief in ESP or reading of psychical material.

As would be expected the belief in ESP and reading of psychical material shows a slight but substantial correlation,  $r = .40$ .

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## PARAPSYCHOLOGY AND PSYCHOLOGY TEXTBOOKS

D. Scott Rogo

In 1972 I carried out an informal survey in an attempt to determine how parapsychology was covered and represented in undergraduate psychology textbooks.<sup>1</sup> The results of that survey were rather disappointing. Of the ten texts surveyed, only one book, the popularly used Hilgard-Atkinson Introduction to Psychology<sup>2</sup> even came close to covering the field in any systematic fashion. Yet even its coverage was woefully inadequate. The other texts usually relied upon questionable source material, focused only on the early Duke work, by-passed more impressive contemporary research, and generally but subtly disdained the importance of parapsychology to psychology or to education in psychology. These texts were all published between 1960-1972.

Parapsychology has undergone a rapid maturation since that time. The publication of parapsychology experiments in such periodicals as Nature, the Proceedings of the I.E.E.E. and other scientific journals has done much to advance parapsychology within the scientific establishment. But what of academic psychology? Although certain branches of psychology---notably the humanistic and transpersonal psychologies---have also matured over the last few years, one must still question how well parapsychology is understood or appreciated by mainline psychologists. Obviously, some hints might be ascertained by looking at what recently published psychology textbooks have to say about parapsychology.

The following brief survey reviews how parapsychology has been treated in five recently published undergraduate textbooks. In order not to bias the sample or selectively choose texts, the following criteria were used when selecting these five volumes: (1) The books had to be designed specifically as texts for undergraduate introductory psychology courses. (2) They had to be currently used in such courses at a selected college or university. (For expediency, the psychology department at California State University, Northridge (CSUN) was chosen. I learned, while preparing a survey on academic attitudes towards parapsychology, that this department had a rather neutral position.

The department stresses experimental psychology.)<sup>3</sup> (3) The books had to have been published after 1972. All told five books met these requirements. I should add that about a dozen undergraduate introductory courses are offered at CSUN and each instructor is granted full autonomy to choose texts for his classes. Therefore, the five texts used in these courses are most likely a reasonable cross-section of books used throughout the country.

The first text surveyed was 'Psychology' by Robert Silverman of New York University.<sup>4</sup> Only four paragraphs are devoted to parapsychology. The only experimental work cited by Silverman is the well known Duane and Behrendt ESP tests carried out between identical twins which was published in Science in 1965. However, the author suggests his reason for including this rather weak experiment by writing, "...the authors of the study carefully refrained from drawing any general conclusions." Why didn't Silverman choose a strong experiment whose designer did come to a firm conclusion about ESP in order to exemplify modern parapsychology research? This question is left suspiciously unanswered. The only other parapsychology work covered by the author is a brief description of how ESP card-guessing experiments are carried out. Silverman does mention that in one published card-guessing experiment a selected subject scored an average of 6.8 hits per ESP card run, but also points out that he scored as low as 4.0 hits at times. However, Silverman apparently does not realize, understand, or explain the significance of negative (psi-missing) scoring, nor does he realize that an average score of 4.0 instead of the expected 5.0 hits per run over a large number of trials is itself statistically significant. What is more revealing, though, is that Silverman does not even give a reference (by author and date) for this alleged experiment. The reader therefore has no way to check out the original report.

This type of slipshod coverage given parapsychology is typical of the way the field was treated in textbooks published during the 1960s. There is no coverage of tightly controlled contemporary research; only weak work is cited and referenced; and there is a general tendency to focus on types of ESP testing procedures (such as card-guessing) which are no longer dominant in parapsychology.

A lengthier overview of parapsychology is given in the Paul Mussen and R. Rosenzweig (both of the University of California, Berkeley) text, 'Psychology: An Introduction'.<sup>5</sup> Although these authors give the field lengthier coverage than Silverman does, their understanding of parapsychology is hardly any better. They begin by pointing out that psychologists are more impressed by controlled

tests for ESP than by anecdotal accounts of spontaneous ESP. This is a valid point, so naturally the reader expects that a fair and critical appraisal will be given to the vast dominions of experimental parapsychology. However, this is simply not the case! The authors merely define parapsychology and the basic terms used in the field. They do so rather poorly. I doubt if any well read psychologist would naively define telepathy as "one person reading another person's thoughts". Afterwards they go on to explain the principle behind ESP card-guessing experiments. However, instead of summarizing key experiments which have become classics in the field, the authors simply quote the oft-heard and just as often rebutted criticisms which have been directed at card-guessing procedures. They point out such possibilities as methodological flaws, recording errors, optional stopping, etc.. The authors, of course, do not realize that these criticisms were answered in full in 1940.<sup>6</sup> But they do admit that, "... later studies with more careful controls still reported results better than chance." However, the authors refrain from citing or referencing any pertinent literature.

The rest of the Mussen-Rosenzweig text carefully avoids discussing the implicatings of this admission. Instead they divert their discussion to focus on why psychologists generally do not believe in the existence of ESP. Their arguments are really straw men. They argue, for instance, that it is a little disheartening to be told that "believers" score better than "disbelievers" on ESP tests. Yet, they do not explain for their readers, nor do they themselves apparently understand, the nature, meaning or psychological significance of the "sheep-goat effect". One wonders if the authors are even aware what a sheep-goat test is. Secondly, they argue that no subject has ever consistently scored 10.0 hits per run in an ESP card test. One wonders, in consequence, what criteria for evidence these psychologists would apply to standard psychological experiments? The authors continue on by pointing out how better controlled ESP and PK tests usually result in declining results. This statement is, of course, ridiculous. The authors only support their statement by drawing upon Girden's famous 1962 paper criticizing PK results<sup>7</sup>, but completely ignore the several rejoinders to Girden's slipshod paper which were subsequently published in reaction to it.<sup>8</sup> So one begins to suspect that Mussen and Rosenzweig have selectively reported their data in order to make the evidence for ESP look poor.

The authors end by arguing that ESP "makes no sense". This is a valid observation; but is it a valid criticism?



The authors deliberately seem to try to make parapsychologists look silly by writing, "...some have even tried to explain ESP as a factor of physical phenomena like 'space-time warps". One can only wonder where Mussen and Rosenzweig came up with that idea. No parapsychologist would be so naive, and the authors cite no source or reference for their charge.

Lyle E. Bourne's and Bruce Ekstrand's 'Psychology: Its Principles and Meanings'<sup>9</sup> is a bit more sympathetic to parapsychology, although their coverage is slight and superficial. The authors (both of whom are listed as teaching at the University of Colorado) first define the basic terms used in the field and describe how a card-guessing experiment is carried out. They do admit that, "considerable laboratory evidence, totaling many hundreds of experiments, indicates the existence of these... phenomena, even though they are sporadic, unreliable, and weak". But they add, "Yet scientists in general do not accept the existence of ESP and PK. Why?"

First of all, I doubt very much if their statement that "scientists in general" do not accept the evidence for ESP and PK is accurate today. But nonetheless, Bourne and Ekstrand do seem willing to make a positive statement about the extant evidence favoring the existence of psi. This approach certainly places their text on a different level than the other two which I have so far reviewed. The authors continue by pointing out that resistance to parapsychology is due to a Kuhnian-type paradigm clash. (Bourne and Ekstrand are the only textbook authors, at least of those I have read over the years, who appreciate the pertinence of Kuhn's views to the debate between psychologists and parapsychologists.)

Bourne and Ekstrand end their mini-section by arguing that one must approach the study of psychical phenomena open-mindedly. They also seem to realize that there might be a significant relationship between ESP, PK, and altered states of consciousness. Unfortunately, they cite no source material on this topic for their readers. Unfortunately too, they erroneously list drug-induced states as being psi-conducive. The experimental evidence for a drug-ESP relationship is extremely equivocal, so these authors are in error in making such a claim. As might be expected, they do not give their readers any references to books or papers on parapsychology. So their brief coverage, while better than might be found in most textbooks, will still really not educate their readers. Perhaps, though, readers of this text will be encouraged to look into the ESP question more thoroughly on their own. One gets the impression that most

psychology texts are trying to dissuade students from becoming sidetracked on parapsychology.

One of the lengthiest overviews on parapsychology I have seen is given in the new (9th) edition of Philip Zimbardo's (Stanford University) and Floyd Ruch's (University of Southern California) 'Psychology and Life'.<sup>10</sup>

The authors begin by defining the terms parapsychology, ESP, clairvoyance, precognition, and psychokinesis. Although their definitions are generally sober, they do slip. They state that PK is synonymous with "levitation". This is, of course, a totally senseless and inexcusable misconception. In reading Psychology and Life, one gets the impression that the authors are about to take parapsychology more seriously than do most authors of introductory psychology texts. At the very beginning of their section on "The Field of Parapsychology" (pp. 269-271) they point out the involvement and importance of J.B. Rhine, Gardner Murphy, and William McDougall to the field. They even mention the Foundation for Research on the Nature of Man and the Journal of Parapsychology. But soon after these intimations, the authors abort their discussion of parapsychology proper to ask, "Why is the available evidence not convincing to the skeptics?"

Perhaps the most annoying feature of psychology textbooks is this very habit of covering parapsychology in negative terms. They usually refrain from asking 'What is the best available evidence supporting the existence of psi?' Instead they invariably ask, 'Why isn't ESP accepted by psychologists, scientists, or the skeptics?' Thus, these books immediately and unfairly place parapsychology on the defensive. I have never seen a psychology textbook which questioned why many psychologists are skeptical of the evidence supporting many of Freud's concepts, subliminal perception, or inherited I.Q.. Yet, this question is invariably raised when the topic of ESP is introduced. Practically all the books outlined in this brief review have approached the subject in this way. But to get back to the Zimbardo-Ruch text:

The authors continue on with their discussion of ESP by explaining how ESP card tests are conducted and admit that, "... the results indicate that some subjects do score significantly above chance expectation". This statement is not followed through, and merely leads the authors to a discussion on "Criteria for Acceptable Evidence" which they apply to the ESP question. They argue that the following criteria must be met before ESP can be accepted as a genuine phenomenon: (1) The controls under which an experiment is run must be so tight that ESP can be the only possible explanation for any results achieved. (At this point, the authors bow in homage to C.E.H, Hansel). (2) High scoring subjects must

continue to be successful when tested in other laboratories. (3) Replication must be achieved by independent workers using similar testing procedures. (4) Experiments must be designed in such a way that the evidence for ESP does not rest solely on statistical inference.

There is nothing wrong with these four points of criteria, although by promoting them the authors indicate that they have little understanding of parapsychology's complexities. One must also wonder if they would insist upon such stringent evidence before accepting findings in any branch of conventional psychology, where replications and consistency are extremely capricious. (If they did, I dare say that their book would have been a good deal shorter!) However, the authors really seem to imply that these standards of evidence have not been met in parapsychology. One could argue that this is not the case. Point (1) was answered long ago after a systematic survey of all published ESP experiments was made.<sup>11</sup> Point (2) was answered by the recently completed work with such high scoring subjects as Pavel Stepanek and Sean Harribance, both of whom have been tested quite successfully in different laboratories. Point (3) has been answered by the large number of replications of the sheep-goat effect. And Point (4) is adequately answered by the large number of successful free-response but quantifiable ESP tests using dream, ganzfeld and relaxation variables. One wonders if the authors of this text were unfamiliar with this body of research, or merely ignored it.

The authors conclude their discussion on parapsychology by associating it with beliefs in "astral energy" and "psychic vampires" and add a few gratuitous remarks about the "will be believe". As with many other texts, not one single experiment in parapsychology is described or referenced for the reader to illustrate faulty methodology! On the other hand, negative reactions to parapsychology (i.e. the writings of Hansel and Martin Gardner) are fully referenced. Thus, the student or general reader is directed only to hostile writings on parapsychology, and is not given the opportunity to read positive contributions on or about the field.

The last book surveyed was Elton B. McNeil's 'The Psychology of Being Human'<sup>12</sup> (Dr. MacNeil teaches at the University of Michigan). By coincidence, this volume offers the lengthiest coverage granted the field of any of the texts included in this report. Sad to say, the section is unsystematic and error-ridden.

Dr. MacNeil begins his four page summary by pointing out that

parapsychology is accepted more readily by the Soviet scientific establishment than by American psychologists. His reason for beginning with this editorial comment is not clear, and the author soon changes his focus by defining the basic terms employed in parapsychology. MacNeil's main point of discussion is ESP in general. He begins by pointing out that most psychologists do not believe in psi. However, his discussion on this topic is, in several respects, underhanded. He quotes the views of Soal and Bateson and Gertrude Schmeidler on the rarity of good ESP subjects and on the lack of impact parapsychology has made on psychology. But he does not mention that all three of these authorities fully support parapsychology and have made notable contributions to the field. He thereupon cites Crumbaugh's criticisms against the statistics used in ESP experiments and those included in another psychology textbook! Only rarely does he discuss any of the published experiments in the field. When he does, it looks as though he has especially sought out either weak work or work which was questionably analyzed. He points to one of Rhine's precognitive experiments which had only meagre results (it was analyzed by means of covariance of salience ratio) as though it were typical of the Duke work. Only in passing does MacNeil mention long-distance ESP tests, but focuses on the S.R.I. work with Uri Geller. Needless to say, he concentrates most intently on the criticisms of the work! Finally, he attempts to come to grips with the sheep-goat effect but gets totally confused. He writes that the effect only seems to prove that ESP tests are biasedly designed in favor of the "believers" and are therefore invalid. I really don't fathom what this criticism is supposed to mean. MacNeil does not explain how this bias was designed into the experiments and I think it is quite clear that he does not even really know what a sheep-goat test is or how it is run.

What can be said by way of conclusions? I am afraid that parapsychology is being treated in psychology texts today just as poorly as it was in the 1960s. This is truly disheartening. With all the constructive recent promotion and publicity the field has received, and in light of the recent developments made by way of methodology and technology, one might have hoped that authors of psychology text books would have become more aware of what parapsychology has achieved. Instead, we find that even recently published psychology textbooks still harp on out-dated criticisms, out-dated research, out-dated issues, and cite none of the more contemporary and impressive research being carried out in the field. Furthermore, they don't even attempt to guide their readers to responsibly written literature on the subject. What is worse,

many of these books cover the field so superficially and erroneously that they will probably misinform their readers rather than educate them. In consequence, one wonders how accurate these texts are when covering mainline psychological topics. Based on their writings on parapsychology, they must be very inaccurate indeed. Paradoxically, some of these books do admit that strong evidence for ESP does exist. But in every instance the authors refuse to discuss this evidence. Instead they divert attention to such questionable issues as why many psychologists reject parapsychology. In short, the coverage parapsychology is receiving in undergraduate psychology textbooks is abysmal.

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## PK INFLUENCE ON MALIGNANT CELL GROWTH

Frans Snel

## INTRODUCTION

Only a few experiments have been carried out during the last years with PK on a biological system. This is understandable, as the methodology of these kind of studies is essentially more complex than for studies using non-living systems as targets.

This complexity is mainly due to the unknown probability characteristics of the measured variables which calls for either high aim - low aim studies or perfectly matched control groups. Furthermore, living organisms do have sensory input mechanisms which increase the number of variables to be controlled experimentally. Finally, the variables measured often include a subjective component which causes extra error and may cause systematic error if the analyzer is aware of the experimental conditions (Davis, to be published).

Barry (1) reported a PK experiment using a fungus culture as target. This experiment suffered from the problem that the stove used for cultivation was subject to power failure, which might have had a bigger effect on the treated dishes than on the control dishes. Staying with reports of PK on lower organisms, Haraldsson (4) tried a PK experiment using yeast. Unfortunately, only an abstract of his work is available, while so far as it goes this work - in which careful matching of control and experimental condition took place - looks sound. It is not possible to say more about it.

In my opinion the best controlled experiments have been done by Grad (6) on PK on plant growth. The methodology is clear and his results strongly suggest PK. A similar experiment by Nash and Nash (10)

yielded only marginal results.

Some experiments have been reported using higher organisms as targets (mice). Miss Elguin (2) tried PK in experimental tumorigenesis. Though this work has been reported only in the form of an abstract, the report does not suggest a careful handling of the material: her preparations suffered from infections which have an influence on the tumour growth, and she used an unreliable method for measuring the tumours. Watkins and Watkins (3) used PK for the resuscitation of anaesthetized mice. Their very good results depend mainly on the person who timed the experimental group. It is an open question whether these results depend on the gifted subject, on the timer or on both. It might have been better to change the timers after every trial. This experiment was repeated by Wells and Klein (9). They too found significant results, but of a much lower level. This experiment looks very good, but it cannot be considered an experiment on healing since the mice were anaesthetized and not ill (The same for the experiment of Watkins and Watkins: it is a simple model and it worked quickly. It would be better to inflict a wound or use germs etc.). Grad (7) did experiments on woundhealing in mice. This too is a good controlled experiment with a clear methodology and good results.

The experiments with higher organisms show essentially the same methodological problems as the experiments with lower organisms and do not give much more information. So, our plan was to devise a practicable experimental design, easy to execute and with as few uncontrolled variables as possible.

#### METHOD

Basically a psychic healer attempted to inhibit the growth of mouse leukaemia cells in tissue culture, as compared to controls. One gifted subject was available and the experiment was designed around him. Three experiments were conducted using closely similar procedures.

Start up routine.

A standard ampoule of mouse leukaemia cells (Code TLX 9, originally C57 Black) was taken from storage in liquid nitrogen at  $-178^{\circ}\text{C}$ . and was then defrosted. The DMSO (dimethylsulfoxid), which might otherwise effect the growth of the cells was washed out using a standard procedure. A culture bottle was prepared with 20 ml of a sterile standard medium (RPMI 1640, foetal calf serum and antibiotics) and the cell preparation was added. The bottle was then placed in a stove. The

stove had double doors, the inner one of plexiglass, the temperature was maintained at 37 C., the concentration of carbon dioxide in the air was held at 5% by a constant flow system and the humidity was greater than 90%. After 3 days the cells have multiplied enough to make it possible to add the required number into the 20 bottles of the experiment.

### Experimental Cultures

These 20 bottles initially contained 20 ml of medium and starter culture was added so that the final concentration was 10,000 cells per ml. The concentration was checked electronically with a Coulter Counter. The bottles were then numbered from 1 to 20 and randomly divided into two sets; experimental and control. The two piles were then placed in the stove by a person otherwise unconnected with the experiment. Everything was left for 4 days (96 hours). Subsequently, the bottles were taken out and the concentration of cells was counted with the Coulter Counter. In the first experiment this value was calculated from the mean of three half ml samples and in the subsequent two experiments from two such samples.

### PROCEDURE

#### Experiment 1

We asked a gifted subject (natural healer, v.P.) to join us and we explained to him what we expected him to do (reduce mitosis). He considered working on cancer cells very difficult, because he experienced it as negative behaviour. Nevertheless, he came to the laboratory to have a look at the surroundings and at the stove. He was shown the bottles. The plexiglass door remained closed and he was not allowed nearer than two meters and was not allowed to touch any of the bottles. Subsequently photographs were taken of the bottles with a Polaroid SX 70 camera. v.P. was given the photograph with the target bottles and he took this home. He worked on it from home, distance 15 km., for four days twice a day. In this first experiment the DMSO was not washed out. Although we strived for optimal matching, the experimental and control bottles had to be placed in separate piles and thus separate compartments of the stove on request of the subject.

#### Experiment 2

The same subject (v.P.) cooperated in this experiment. He did not come to the laboratory again. Instead he only received a photograph.



### Experiment 3

On this occasion we used all the available people in the laboratory as subjects (analysts, researchers, students). Generally they did not think it possible to influence the mitosis in this way, but they cooperated.

### Experiment 4

This was a replica of experiment 3. The subject was the natural healer again. He was to try to repeat the results of experiment 2.

## RESULTS

### Experiment 1

All the bottles contained only dead cells, the targets as well as the controls. There was therefore nothing to count.

### Experiment 2

A person otherwise unconnected with the experiment measured the cell concentrations. Targets and controls were counted alternately: the operator was blind to which was which. The results are presented in table 1. The given numbers are the calculated number of cells per ml. For the targets, we found in the 10 bottles a mean number of 6946.3 cells per ml and for the controls 5000.0 cells per ml. This is a difference of 1946.3 cells/ml or 38.93% more cells in the targets than in the controls. A Mann Whitney U test (non-parametric) showed this to be a significant difference ( $U=0$ ,  $p<.002$ , two-tailed).

### Experiment 3

In this experiment using laboratory personnel as healers we found a target mean number of 3616.3 cells/ml, and for the controls a mean number of 2836.0 cells/ml (see table 2). The mean difference is 780.3 cells/ml or 27.51% more cells in the target bottles. The U test was again significant ( $U=16$ ,  $.002<p<.02$ , two-tailed).

### Experiment 4

We found a target mean number of 2825.8 cells/ml and for the controls 3467.6 cells/ml (see table 3). The mean difference is 641.8 cells/ml or 18.5% less cells in the target than in the control bottles. The U test was significant ( $U=11$ ,  $.002<p<.02$ , two-tailed).

TABLE 1

	Number of cells per ml										mean/ml
Target	6644	6977	7489	6404	6266	6904	6189	6494	8490	7606	6946.3
Control	5397	5857	2136	4764	4905	5078	5352	5132	5345	6034	5000.0

TABLE 2

	Number of cells per ml										mean/ml
Target	3863	3489	4376	4209	3828	2741	2942	3707	3924	3084	3616.3
Control	3784	2896	2535	2334	2122	2770	3298	3111	3325	2185	2836.0

TABLE 3

	Number of cells per ml										mean/ml
Target	3244	3244	2798	2526	2615	2658	3200	2478	2807	2688	2825.8
Control	3888	3207	3052	3147	3147	4025	3556	3264	3871	3683	3467.6

## DISCUSSION

In this experiment we tried to combine the best of two worlds: the anthropological approach of using a specially gifted healer and the rigorous control of laboratory procedures.

The results indicated that there were strong statistically significant differences between cultures remotely treated by a healer and control samples. However, from the three viable experiments, in two of them (2 and 3) the results were contrary to the target direction, which to many parapsychologists might be suggestive of psi-missing.

In these experiments however, there is also a clear possibility of a methodological artefact. Because the healer claimed he could concentrate only on a single area, it was not possible to distribute the control and experimental bottles randomly mixed on the floor of the stove. Thus it is conceivable that the results are due not to the effect of the healer but to unavoidable minor differences between the conditions of the two piles of the stove. It should be remarked that the scoring direction becomes consistent with a prediction that bottles in the upper compartments do grow faster than in the lower compartments. Investigations of temperature and humidity revealed no discernable differences.

To check out this possibility 3 additional dummy experiments were run, similar to the above without participation of a healer. The results of these 3 experiments are presented in table 4. In table 4, experiment II, there are 4 very low counts which are probably due to diluting errors. Therefore, these numbers are not reliable, but the table shows faster growth in the predicted direction as do the other experiments (Targets for experiments 2 and 3 were in the upper compartments and the controls in the lower ones. Targets for experiment 4 were in the middle compartment and the controls in the upper one). The differences between target and control groups in the dummy experiments were 7.96%, (11.9%), and 8.35% respectively. This shows clearly a place dependency, but the numbers are within the range of normal growth spreading; that is a difference of maximal 10% within a group when not influenced.

To check out this still further we ran 3 additional experiments with the bottles on the same level in different compartments in the stove. The results of these 3 experiments are presented in table 5.

We found that the normal spreading varies between 2% and 8% (2.5%, 2.49% and 7.21%). So, spreading in growth within groups and variation

TABLE 4  
 Dummy experiments on the same places

Experiment I		Experiment II		Experiment III	
Target	Control	Target	Control	Target	Control
5996	5022	6925	2655	5355	5871
5749	5430	6231	3767	5802	5770
6713	5601	2901	5615	5535	6483
5626	6115	2910	4671	5470	6056
mean/ml					
6021.0	5542.0	4741.7	4177.0	5540.5	6045.0

TABLE 5  
 Bottles on same level in different compartments of stove

first experiment										
1056	1084	1122	1115	1084	1106	1065	1078	1140	1018	mean/ml: 1086.8
second experiment										
4517	4635	4905	4882	4882	----	5028	4990	----	5190	mean/ml: 4878.6
third experiment										
4360	4418	5100	4294	5028	5072	4702	4157	4748	5389	mean/ml: 4726.8

in numbers between levels account for differences up to a maximum of 10%. Thus any differences in these dummy experiments were much smaller than in the main study. This may suggest that the experimental results involving the healer were not entirely artifactual. Further work with a more sophisticated design will allow the question to be conclusively settled.

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## CORRELATION OF ESP SUCCESS WITH BIOLOGICAL RHYTHMS

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A virtually untapped area of research in parapsychology is the relationship between ESP success and biological rhythms. We know that there is a circadian rhythm to body temperature, blood pressure, respiration, blood sugar, hemoglobin levels, and amino acids, among other things. Because of this rhythm, drugs affect us differently at different times of day. Also, according to the biological time of day, people perform differently on psychological and physiological tests. Luce even goes so far as to suggest that the failure to replicate an experiment of another scientist may be due to the biological times of the circadian rhythm of either the subject or the experimenter (Luce, 1971). In the past decade, a tremendous amount of research has been done to investigate the various kinds of rhythms, many of which have a known biological base and many which do not as yet. Parapsychologists on the other hand have not generally begun research in this area in spite of the fact that we know that ESP is affected by biological and psychological factors (Honorton, 1974; Morris, 1977). We have paid attention to factors such as the comfort of a subject or whether his mental attitude is cheerful, but we continue to give tests, on the whole, in ignorance of or in defiance of the biological/psychological rhythms that have been established. The one exception to this trend is a study done by Schmitt and Stanford (1978) where they showed that subjects in the preovulatory cycle scored significantly better than women in the postovulatory cycle in a ganzfeld ESP test. Two years ago I began a series of exploratory experiments to see whether ESP success can be correlated with certain biological rhythms. In the first experiment, I sought to find a correlation between ESP success and a circadian rhythm, taking into account both time of day and body temperature. In the second series of experiments, I investigated a purported biological rhythm, the popular Fleiss-Swoboda Biorhythm, and sought to inquire about a correlation between ESP success and any of the important times in the biorhythm chart.

## TEST 1: CIRCADIAN RHYTHM

I have already mentioned several biological phenomena that

seem to follow a circadian or daily rhythm. As an example of how such circadian rhythms may affect ESP ability, we can examine a study done at the National Institutes of Health in Bethesda (Luce, 1971) where experimenters established that there is a circadian rhythm in sensory acuity. It is directly related to steroid levels, so that sensory acuity reaches its maximum at around 3:00 a.m. and then begins to decline as steroid levels increase. Then during the day steroid levels decline again and sensory acuity reaches a high point around 5:00 to 7:00 p.m. If we hypothesize that ESP is an extra-sensory kind of perception and works better when normal perception is inhibited or blocked (Broughton, 1976; Maher and Schmeidler, 1977), other things being equal, we might expect ESP success during those times of the day when steroid levels are high, say, at mid-morning.

There is also a circadian rhythm to body temperature, and the cycle seems to be a very basic one. It is quite difficult to change in a normal adult and does not seem to be affected by food or muscular activity. This rhythm is important because it is correlated with many other rhythms. At high temperature, other functions, such as speed of pulse, pulse pressure, pulse-wave velocity and circulating blood volume are changing. Because of the importance of the temperature, I decided to use it in an exploratory test of ESP success and circadian rhythms. Naturally, other factors such as the cycle of sleep and the amount of sleep will affect the circadian rhythm, but the temperature is relatively impervious to these factors in the normal adult.

#### METHOD

One unselected subject was asked to take her temperature with a basal thermometer five times a day for ten consecutive days. The temperatures were taken at 7:30 a.m., 1:15 p.m., 5:30 p.m., 11:00 p.m., and 3:00 a.m. in the same room and in the same location. Immediately following this, the subject took an ESP test consisting of 200 trials, thus totaling 1,000 trials per day and 10,000 trials for the series. Targets were generated by a random event generator. The tests were all scored at the completion of the experiment.

#### RESULTS

No correlation was found between either the time of day or body temperature and ESP success because the subject failed to display ESP in any of the time-based series. (see Table 1.)

## CIRCADIAN RHYTHMS

TABLE 1

7:30 a.m.		1:15 p.m.		5:30 p.m.		11:00 p.m.		3:00 a.m.	
Temp.	Hits	Temp.	Hits	Temp.	Hits	Temp.	Hits	Temp.	Hits
98.2	47	98.6	51	99.4	43	98.1	52	97.5	41
98.3	42	99.1	36	98.3	59	98.8	50	98.0	61
97.9	55	98.2	54	99.5	44	98.9	52	97.8	53
98.3	56	98.4	49	98.6	49	98.7	49	97.6	42
98.0	47	98.6	43	99.5	37	98.8	52	97.8	48
97.8	59	99.5	50	99.3	50	98.3	53	97.3	53
97.8	47	98.7	56	99.3	55	97.9	50	97.9	53
98.1	58	99.0	55	99.0	48	98.6	63	97.9	49
98.2	42	98.8	51	99.2	46	98.6	46	97.5	66
98.3	65	99.2	60	99.3	44	98.6	44	98.0	47
T 518		T 505		T 475		T 511		T 513	

The only series which deviated at all from chance was the 5:30 p.m. series, which had a CR of  $-1.29$ . Thus the hypothesis that ESP success should be more noted during periods of low sensory acuity was not supported.

## TEST 2: THE FLEISS-SWOBODA BIORHYTHM

The biorhythm theory has become popular particularly in Europe, the United States, and Japan over the past decade, and it has purportedly been used in clinics in Switzerland and the United States by doctors to determine the best time for operations as well as in factories to reduce the accident rate (Tatai, 1977). The theory states that every person goes through three

rhythms starting at birth, physical (23 days), emotional or sensitivity (28 days), and intellectual (33 days). The rhythms are thought to be biologically based by the proponents of the theory, but no direct support for this thesis has been forthcoming. At best, the proponents of the theory can point to correlations found in medical records which point to the existence of the cycles.

The physical and emotional cycles were first noted by two Europeans. Dr. Hermann Swoboda, professor of psychology at the University of Vienna, wondered whether men's feelings and actions were influenced by rhythmic fluctuations, and between 1897 and 1902 he studied dream reports and noted the recurrence of ideas on a 23-day and a 28-day rhythm. At about the same time in Berlin, Wilhelm Fleiss, a physician, noted both a 23-day and a 28-day cycle in diagnosing many of his patients. The 33-day intellectual cycle is based primarily on the work of Dr. M.J. Bennett and Dr. Rexford B. Percy who did research at the University of Pennsylvania between 1928 and 1932. It was accidentally discovered by checking the emotions of workers in railroad shops over periods of many months (Cohen, 1976).

The biorhythm theory states that the first half of each cycle is the discharge period, while the second half is a recharge period. Critical days are ones where the rhythms cross over from discharge to recharge or vice versa. According to the theory, it is particularly the critical days which must be taken into account in medicine and industry. It is only those days when one's confrontation with unusual situations or even normal situations in the environment may be hampered. For instance, the critical day in the physical rhythm may bring on headaches, fever, strokes, a worsening of a disease and outbreaks of accidents. The critical period in the emotional or sensitivity cycle may produce slips of the tongue, irresponsible utterances, quarrels, disputes, cerebral paralysis or a lowering of reaction. The intellectual critical day may bring a failure of memory or mistakes in memory or a lowering of intellectual ability and attentiveness. It is these critical days that proponents of the theory argue which supply the greatest statistical correlations with events in the environment. For instance, proponents are apt to point to a critical physical or emotional day and failure of an athlete to perform in an expected good fashion. Although some interesting correlations of this kind have been made in terms of such things as sports, traffic accidents, crimes and suicides (Tatai, 1977), critics of the theory say that it is easy to point to as many good performances on these critical days as bad ones. Some studies (Fix, 1976; Cronin, 1977) have failed to find correlations with sports performance. When

you add to these findings the fact that with 80 critical days a year, 22% of the days are critical ones, so any individual event that seems a correlation has a high chance of being artifactual. In addition, it is inadvisable to take a person's assessment of himself vis-a-vis the biorhythm charts seriously. In one test described later, one group of subjects was given correct biorhythms and the other false ones. Each subject in both groups was convinced that the cycles fit their moods, etc. perfectly, irrespective of whether he received accurate or false charts. At NL Industries, pipe-fitters were given wrong information about their biorhythms and accidents were reduced anyway (Hirsch, 1976). So that warning about accidents may be the causative factor in the reduction of industrial accidents and may not have anything to do with biorhythms. Clearly biorhythms have not been proved either to have a biological base or to be an undisputed correlate of such things as accidents or sports performance. However, some of the correlations are interesting, so masking my natural scepticism for such unproven claims, I undertook a pilot study of the correlation between ESP success and the various charts, paying particular attention to the critical days.

#### METHOD FOR PILOT

Biorhythm charts were made for four subjects for a 33-day period so that each important time in the chart could be used. The subjects were kept ignorant of their charts, but on each day that they were either in a high, low, or critical time in their chart, they were told only that it was an important time and asked to take an ESP test of 400 trials, 40 runs of 10 trials using the Paratronics ESP-1, a device that randomly selects one of four targets. After 10 trials, the number of hits is flashed on a counter. Each subject, then, completed a total of 3,600 trials (400 times each on nine important days), with a total of 14,400 trials for all four subjects. Each condition (high physical, low physical, etc.) had 1,600 trials ( $MCE = 400$ ) when totaled for all four subjects.

#### RESULTS

A correlation between ESP success and important days on the biorhythm chart was found to exist for low physical ( $CR = 2.66$ ,  $p. = .0078, 2t$ ), and critical sensitivity ( $CR = 2.77$ ,  $p. = .0056, 2t$ ). A marginally significant correlation was found for high sensitivity ( $CR = 2.21$ ,  $p. = .027$ ). (see Table 2).

## BIORHYTHMS PILOT

TABLE 2

	Hits	Deviation	CR
High Physical	391	-9	- .52
Critical Physical	398	-2	- .12
Low Physical	446	46	2.66
High Sensitivity	438	38	2.20
Critical Sensitivity	448	48	2.77
Low Sensitivity	425	25	1.45
High Intellectual	422	22	1.27
Critical Intellectual	398	-2	- .12
Low Intellectual	407	7	.40

These impressive results must be put in perspective, however, in that one of the four subjects scored at an incredible level, achieving 1036 hits with a MCE of only 900. This yields a CR of 5.23, resulting in astronomical odds against chance. One must question, then, whether the significant scores for the entire group may simply be an artifact of one subject's scoring patterns. In fact, an analysis partially bears this out in that if one removes this subject's scores from the group, the group fails to achieve significance in either the low physical or critical sensitivity. The CR for the high sensitivity remains at a marginally significant level of 2.1 ( $p = 0.35$ ).

The pilot study yielded interesting results in that a correlation was found between biorhythms and ESP success. However, that result may not be taken at face value as they seem skewed as a result of one subject's scoring. These findings were positive enough, however, for me to attempt a conceptual replication.



## CONFIRMATION 1

In reviewing the literature for biorhythms and the way to compute them, it struck me that there seemed to be some freedom in designating the critical time in the cycle. It also seemed to me that if the rhythms were biologically based, it is more probably the case that there is a progression of the biological processes toward critical days and that the days around the critical day are more critical the closer they are to the one day designated as critical. It seemed to me possible, then, to divide each cycle into thirds, a high, a critical and a low third. I asked two subjects to complete an ESP test of 200 trials each day for 33 consecutive days. Once again, the subjects were kept ignorant of their biorhythm cycles. Targets were generated by a random event generator (REG) and checked at the conclusion of the 33 days.

## RESULTS

The only correlation found with ESP success was the critical sensitivity, which yielded a CR of  $-2.6$ ,  $p = .009$ ,  $2t$  (see Table 3).

This compares with one of the successful correlations of the pilot study, which suggests that the original correlation may not be due merely to artifact. In addition, the description of the condition associated with this critical day--slips of the tongue and cerebral paralysis--indicates that this may be a time when one's rational thought processes are not overpowering. Thus, this time in the chart may be analogous to meditation processes which quiet normal mental processes and which thus enhance the subject's ability to display ESP (Honorton, 1974; Braud and Hartgrove, 1976).

## CONFIRMATION 2

Before the results of the first confirmation study were calculated, I carried out another test similar to the pilot study when I asked the subjects to take an ESP test only on important days in their charts. An additional twist was given to this experiment in that I gave all 18 subjects a biorhythm chart, which they thought was correct. In the case of 10 subjects, this assumption was correct. However, to 8 subjects I gave false biorhythm charts. It is interesting to note that in every case, the subjects reported that the charts reflected their moods, reaction times, etc., irrespective of whether the charts were correct or not. This fact should bring clearly to our attention the difficulty and perhaps danger of using these kinds of charts due to self-fulfilling prophecy and faulty self-perception.

## RESULTS

Due to some problems with subjects not completing the tests when they were supposed to, statistics could be gathered for critical days only. The results (see table 4) partially confirm my previous results that there is a correlation between ESP success and the critical sensitivity day.

## BIORHYTHMS CONFIRMATION 1

TABLE 3

	No. Days	Trials	Hits	MCE	D	CR
High Physical	23	4600	1186	1150	36	1.23
Critical Physical	20	4000	965	1000	-35	-1.28
Low Physical	23	4600	1159	1150	9	.31
High Sensitivity	21	4200	1043	1050	-7	-.25
Critical Sensitivity	21	4200	977	1050	-73	-2.60
Low Sensitivity	25	5000	1237	1250	-13	-.42
High Intellectual	21	4200	1025	1050	-21	-.75
Critical Intellectual	20	4000	994	1000	-6	-.22
Low Intellectual	24	4800	1237	1200	37	1.23

## BIORHYTHMS CONFIRMATION 2

TABLE 4

	CORRECT CHARTS					FALSE CHARTS				
	Trials	Hits	D	CR	p	Trials	Hits	D	CR	p
Critical Physical	3250	563	-87	-3.82	.0001	3500	679	-21	-.88	ns
Critical Sensitivity	2750	488	-62	-2.96	.003	3500	637	-63	-2.66	.008
Critical Intellectual	2000	378	-22	-1.23	ns	2750	526	26	1.24	ns

Totaling the results on the correct biorhythm charts, the subjects achieved a CR of -2.96, yielding a  $p = .003, 2t$ . The subjects however achieved an even more significant score on the critical physical day, achieving a CR of -3.82, which yields a  $p = .00013, 2t$ . The success of correlating the critical sensitivity day with ESP success was complicated by the fact that the only significant score on the false biorhythm charts was the false critical sensitivity in which subjects achieved a CR of -2.66, yielding a  $p = .0078, 2t$ . One may wonder if both these scores were due to the belief that high scores would be produced at this time, but the subjects in no way were given the expectation by the experimenter that this should be an unusually successful day, nor were the subjects particularly knowledgeable in parapsychology so that they should be able to guess (based on any literature) that this may be a propitious time for ESP. A further reason that the results were not due to subject expectation was that the sub-experimenter who handled all of the communication with the subjects did not have this expectation and was ignorant of the results of the previous studies. A clear cut case of the experimenter effect can be excluded since I had not calculated the results of the first confirmation study. Therefore, once again there is a question about the strong correlation found on three studies between ESP success and the critical sensitivity period. That a positive result should be found so consistently among different experiments points to the validity and reliability of the correlation, but in each case, some mitigating factor arises and must bring at least some question to the cumulative results.

### CONFIRMATION 3

#### METHOD

As a result of the conflicting but promising data collected in the three studies described above, I undertook another confirmation study which was more extensive than any of the previous ones. Ten subjects who were students in an Introduction to Parapsychology class I was teaching volunteered to take a daily ESP test of 250 trials over a period of 35 days. Once again they were ignorant of their biorhythms and had no expectations except that they were told that some positive correlations between ESP success and categories in the biorhythm charts were achieved in previous tests. Each subject was given answer sheets and instructed to make 250 guesses each day at whatever time they wished. They were encouraged to establish a routine time for their ESP test, and the

answer sheets were collected each week for that week. Before the experiment began, random targets were generated for each of the subjects (my appreciation to Joe Marie Haight and James Davis of FRNM for these targets) and the lists were kept locked in a filing cabinet in my office for the duration of the experiment. At the conclusion of the experiment, the answers were compared to the tables and hand-checked twice for accuracy.

Combining procedures from previous studies, I analyzed the data in two ways: 1) by using only the specific days designated as high, critical, and low days--I will call these "peak" days, and 2) by dividing the charts into thirds and using all of the days in the analysis--I will call these categories "total" days. My hypothesis, assuming that the previous data were not artifactual, was that there would be a correlation between ESP success and the critical sensitivity period, using both the specific "peak" critical days and the "total" critical third of the chart.

## RESULTS

Significant results were found using both kinds of analyses, but my hypothesis was not confirmed. (see Table 5.) Significant scores were not found in the critical sensitivity period; rather, the most significant results were found in the high intellectual period, with the analysis of the "peak" high intellectual yielding a  $CR = -2.79$  ( $p = .00526, 2t$ ). The "total" high intellectual category yielded a  $CR = .2.41$  ( $p = .016$ ). Marginally significant results were also found in the "peak" critical intellectual days and in the "total" high physical days.

## DISCUSSION

The results of the last study did not confirm my hypothesis, nor did they yield any discernible pattern when compared with previous studies. We are left with conflicting data. In each of the studies correlations were found between a significant level of ESP and one or another category of the biorhythm charts, with a correlation with the category of critical sensitivity found in several of them; but this result is not consistent. What may be the explanation?

Given the growing concern with and the evidence for the experimenter effect, it is fair to ask what my expectations were? Were they fulfilled? I would say not. When I began the

## BIORHYTHMS CONFIRMATION 3

TABLE 5

ANALYSIS OF "PEAK" DAYS						
	<u>Trials</u>	<u>Hits</u>	<u>MCE</u>	<u>D</u>	<u>CR</u>	<u>p, 2t</u>
High Physical	4250	829	850	- 21	- .81	NS
Critical Physical	6750	1371	1350	21	.64	NS
Low Physical	2500	480	500	- 20	- 1.00	NS
High Sensitivity	3250	665	650	15	.66	NS
Critical Sensitivity	6500	1283	1300	- 17	- .53	NS
Low Sensitivity	3000	586	600	- 14	- .64	NS
High Intellectual	2000	350	400	- 50	- 2.79	.0053
Critical Intellectual	5250	992	1050	- 58	- 2.00	.045
Low Intellectual	2500	491	500	- 20	- .45	NS
ANALYSIS OF "TOTAL" DAYS						
High Physical	30,000	5862	6000	- 138	- 1.99	.046
Critical Physical	30,000	5920	6000	- 80	- 1.15	NS
Low Physical	20,750	4155	4150	5	.09	NS
High Sensitivity	25,250	4968	5050	- 82	- 1.29	NS
Critical Sensitivity	31,000	6151	6200	- 49	- .70	NS
Low Sensitivity	24,500	4818	4900	- 82	- 1.31	NS
High Intellectual	25,250	4897	5050	- 153	- 2.91	.016
Critical Intellectual	29,000	5821	5800	21	.31	NS
Low Intellectual	26,500	5219	5300	- 81	- 1.24	NS

series of experiments, I was fairly skeptical of the veracity of the biorhythm theory, and I was surprised to find any correlation at all. It was not until the last confirmation study that I began to waiver in my skepticism and believe that a correlation would be found. But it was a correlation between ESP success and the critical sensitivity category that I expected, and that expectation was not fulfilled.

The net result of this series of experiments is that no consistent correlation has been found between ESP success and any one category of the biorhythm chart, using either the "peak" days, as the proponents of the theory recommend, or using the "total" number of days in the chart. In this study the search for a consistent biological correlate was not completely successful, but parapsychologists must continue to explore for biological correlates. From the results of these experiments, it seems that we may be more successful in our search in areas other than circadian rhythms or biorhythms.

#### ABSTRACT

An attempt is made to find a correlation between success on an ESP test and biological rhythms. In one experiment no correlation is found between a circadian temperature rhythm and ESP success. An additional series of experiments were performed to investigate correlations between ESP success and any period described by the Fleiss-Swoboda biorhythm theory. A pilot study and three confirmation studies all found correlations between ESP success and certain important times on the biorhythm chart, especially (in three of the tests) for the critical sensitivity period. However, mitigating circumstances point toward this correlation possibly being due to artifact. When all four studies are examined, no consistent correlation between any particular important time in the biorhythm chart and ESP success is found. The fact that so many correlations were found in the different studies points to the importance of further research in the area of ESP and biological rhythms, but the lack of consistent results in this series of experiments indicates that such research may be more profitably done in other rhythms besides the Fleiss-Swoboda biorhythm.

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THE USE OF THE SENSORY DEPRIVATION TANK TO FACILITATE  
PSI-MEDIATED IMAGERY: AN EXPLORATORY STUDY

D. Scott Rogo

INTRODUCTION

Over the past few years, parapsychologists have experimented with various procedures which apparently help induce psi-mediated imagery, among these being the use of Ganzfeld stimulation (Honorton and Harper, 1974); progressive muscular relaxation (Braud and Braud, 1973); and the experimental manipulation of normal dreaming (Ullman and Krippner, 1970). No experimenters, however, have so far explored the potential use of the sensory-isolation tank (also sometimes incorrectly given the generic name of "Lilly" tanks) as a psi-conducive state inductor. This seems odd since there is some anecdotal evidence that the state of awareness produced by immersion in the tank leads to the production of psi-mediated imagery (Lilly, 1972).

METHODOLOGY

A specially built tank, 8' x 4' x 4', was used for this series of pilot explorations. Water was placed inside to a depth of approximately 1 ½' and was treated with Epsom salt for buoyancy. The subject entered the tank via a door at its top after dressing in a bathing suit or, if preferred, naked. (All subjects preferred the latter). The water was kept at approximately body temperature. The procedure allowed the body of the subject to float in a sealed, sound-attenuated and controlled environment where he was also cut off from normal sources of visual and olfactory stimulation. To facilitate imagery, a visual ganzfeld was also installed in the tank by attaching a red light to its ceiling, into which the subject could stare through halved ping-pong balls. No concomitant auditory ganzfeld was employed since combined visual and auditory ganzfeld stimulation is known to produce mental imagery (Bertini, Lewis and Witkin, 1964) which might interfere artifactually with

our goal of testing the imagery producing properties of the tank-environment itself. The ganzfeld was only employed in some of the tests; in the others the tank was completely dark. An intercom was constructed inside the tank so that the subject could report any mental imagery he might experience to an experimenter (in all cases D.S.R.) stationed next to the tank. Once inside, the subject was merely instructed to image freely, report all thoughts and images that come to mind, and to give himself the mental suggestion that the "telepathic message" we would be sending him during the course of the session would come to consciousness at the appropriate time.

For the actual test, a 35 minute session was employed. The first five minutes served as an orientation period during which the subject was allowed to adjust himself to the tank. After this period, all his mental images and mentation were recorded by the experimenter for a period of 30 minutes. An agent (usually a friend of the subject's or some one brought by the experimenter) was stationed in a building adjoining the experimental area. He was instructed to randomly select (by a card-shuffle procedure) one of twenty large manila envelopes, each containing four smaller envelopes in which thematically unrelated viewmaster reels (one per envelope) had been placed. By a further random process, the agent selected one of the four reels and one of six potential five minute "sending times". He then viewed the reel continuously at the appropriate time. Afterwards he was instructed to place the reel back in its envelop, shuffle all four envelopes, and place the target pool back in the large manila envelop. This was, in turn, handed to the experimenter at the end of the test. At no time was the agent allowed to see or interact with the subject until after the judging had been completed.

For the judging, the subject reviewed his mentation with the experimenter, and then ranked all the four possible targets against his imagery on a 1-4 basis. A rank of 1 was designated a direct hit.

## RESULTS

Since this experiment was meant only as a pilot study, only three subjects (all male) were used. They contributed a total of 4 sessions. These subjects were chosen on the basis of their familiarity with the nature and use of sensory deprivation tanks. The study was not geared specifically to seeing if a statistically significant scoring rate could be achieved, but was implemented to test whether the tank would help facilitate the production

of mental imagery and to see what methodological problems would arise from its use in ESP testing. The results of the four sessions showed little promise. There were no direct hits (and only one second choice) when one would be expected by chance. Neither were there any qualitative correspondences between the targets and the mentation reports.

## DISCUSSION

The primary objective of this pilot exploration was to study the methodological problems inherent in the use of sensory deprivation tanks in parapsychological tests. In this regard, the experiment was most enlightening. Even with the adjunct use of a visual ganzfeld, it does not appear that these tanks are especially useful for the production of imagery, at least in the way we used it. Two of our subjects reported barely any imagery at all, while the third only reported imagery after a long "incubation period" and a longer one than most subjects in the Ganzfeld apparently need. It is also interesting to note that the one subject who was able to come up with some substantial, though delayed, imagery already had some experience with the Ganzfeld during an earlier experiment (Rogo, 1976).

The reason for this failure may stem from three independent factors.

First, the deprivation tank is supposed to produce a state of total sensory isolation. This, however, is not strictly true. The unique sensation of "floating" in the tank, and the somatic stimulation produced by the water itself caused all three of our subjects to become focused on this pleasurable tactile stimulus. This focusing may have, in turn, inhibited mental imagery. Our subjects may therefore not have produced rich imagery either because (1) they failed to observe their imagery because of their preoccupation with somatic stimulation, or (2) this stimulation caused them to inhibit the production of mental imagery so that it would not disturb their concentration of their body awareness.

Secondly, there is also some evidence that, of all consciousness-altering induction procedures, the deprivation tank is most anxiety producing. Since there appears to be a negative correlation between psi and anxiety (Palmer, 1977), the psychological state induced by the tank may have been counter-productive to the relaxed, inwardly-focused state which appears to be psi-conducive (Honorton, 1974).

Thirdly, there also appears to be some relationship between the duration of exposure to a psi-induction procedure and how successful

it is for the production of psi-mediated imagery (Honorton, 1974). Anecdotal evidence indicates that sensory deprivation tanks only help induce psi experiences in subjects who have been subjected to its influence for extraordinarily long periods (Lilly, 1972). Our trials may have failed because we had not taken this variable into account.

Researchers who wish to experiment further with the sensory-deprivation tank will have to take these factors into consideration should they wish to implement its use in more formal ESP testing.

#### ABSTRACT

A pilot experiment to test the use of the sensory-isolation tank in ESP testing was implemented using three subjects who contributed four exploratory sessions. Each subject was immersed in the tank for a period of 35 minutes, during which he tried to image about a viewmaster slide being viewed by an agent. A visual ganzfeld was sometimes employed to help facilitate imagery. Although the pilot study was designed primarily to ascertain what methodological problems would arise from the use of the sensory-isolation tank, the experimenter noted no effects interpretable as ESP during any of the sessions. Of the various problems inherent in the use of isolation tanks for ESP testing, three primary variables may inhibit the production of psi: (1) The isolation tank environment may not be particularly conducive to the production of imagery, which is often a mediating vehicle for ESP, since it produces interfering tactile stimulation. (2) The tank environment itself may be anxiety-producing, which may negatively influence ESP scoring. (3) Some evidence exists that psi may only occur when a subject is subjected to the tank-environment for extremely long durations.

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Letters to the editor:

ON THE POSSIBILITY OF EFFECTS OF PSI UPON BIRTH-ORDERS

Whenever I see papers in psi journals involving the use of birth orders, one of which appeared in the February 1979 Research Letter ("Psi-Missing of Only Children: A Demonstration of an Experimenter Effect" by Jacobs and Breederveld), it occurs to me that some other researcher with the time and interest may wish to replicate an experiment of my own concerning the question of whether, in the conception of an offspring, its sex can be controlled through an unconscious application of psi by either parent. This was first reported in the SPR Journal (1957, Vol. 39, Pp. 65-78) under the title "The Influence of 'Applied Psi' upon the Sex of Offspring", by this writer, even though its significance was only suggestive. Mainly genealogies were consulted; but two exceptions (which also contained the most significant portion of the data) involved only Dutch families, one of which was a special census published by Janse\*.

There are two reliable ways by which this type of study may be done: one is as described in my original paper, and the other is suggested by J.L. Randall in "Psi Phenomena and Biological Theory" (SPR Journal, 1971, Vol. 46, pp. 163-164).

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\* Janse, L. Bijdrage tot het Onderzoek der Geslachtsverhouding bij de Geboorten. Middelburg, Zeeland, 1853.