
PARAPSYCHOLOGY AND EDUCATION

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When I first read the title of the topic of this conference my first thought was that I should put the entire emphasis upon the type of research and study program that we are offering at the Parapsychology Laboratory of the State University of Utrecht, the Netherlands. On second thoughts however, I realized that a number of issues which can be discussed under the heading of Parapsychology and Education is very extensive. I chose to consider the following:

1. Some facts about our laboratory;
2. Study program;
3. Research projects in progress;
4. Publications and system of publishing.
5. Parapsychology: Implications for science, and our responsibilities as educators.

The Parapsychology Laboratory of the State University of Utrecht was established in 1974, in connection with the inception of the, at least formally speaking, first regular chair and professorship in parapsychology in Europe. The laboratory has an independent status in relation to the Psychological Laboratory of the University. Our laboratory is nevertheless located within the same building, and we have the privilege of sharing the library, as well as having full access to the equipment and apparatus within the Psychological Laboratory. We have, of course, a budget of our own. Our laboratory has four full-time positions, one of which is intended for a visiting research worker.

Visiting research workers can stay for a maximum of one year, and a minimum period of two months. During the stay, the visiting research worker is free to take up any type of research work, assuming of course, that the type of work can be carried out within our laboratory. There is, however, a certain emphasis on experimental work, but a visiting research worker is also allowed to carry out a theoretical study, or use his or her time writing up an article or a thesis based on data which have

been collected previously elsewhere. It is also expected that the visiting research worker take part in some of the educational activities in order to enrich the program. During the first year and a half of our existence we have had four visiting research workers.

EDUCATIONAL PROGRAMS

For post-graduate students in psychology (the majority of whom are working on their Ph.D theses) a study program comprising ten problem-orientated seminars was presented. The main objective of these seminars was and is to introduce the students at this level to selected parapsychological problems. A strong emphasis was put on the methodological aspects of how to approach the research problems. The methodological problems were to a large extent approached from the point of view of the philosophy of science.

During the Fall semester of 1974, the topics of the seminars were mainly devoted to basic methodological issues, illustrated by problems from parapsychological research. During the Spring semester of 1975 stronger emphasis was placed on purely parapsychological matters, but of course involving a strong methodological connection. A list of the topics treated in the two series of seminars may be informative:

Fall-Semester of 1974

1. The Task of Methodology.
2. "Scientific Method" in Behavioral Science.
3. Laws, Functions, Contents, and Types.
4. The Place of the Experiment in Science.
5. The Place of Measurement in Science.
6. The Function of Statistics in Science.
7. The Place of Models in Science.
8. The Use of Theories in Science.
9. Explanations in Science.
10. Discussions of the above Topics, and Evaluation of the Course.

Spring-Semester of 1975

1. Introduction to Parapsychology.
2. Facts and Beliefs in Parapsychology.
3. Spontaneous Cases. A Challenge.
4. A Comparison of Parapsychological and Psychological Journals.
5. Psi and Subliminality.
6. Poltergeist-phenomena.
7. The Psychobiology of Psi.
8. The Survival Issue.
9. The Challenge of Precognition.
10. Models of Control and Control of Bias in Experimental Parapsychology, and Evaluation of the Course.

Prior to each seminar, the participants were given rather extensive reading assignments which they had to complete beforehand. The ideal maximum number of participants in any group should not exceed fifteen. Participants were given academic credit for attending the seminars.

For freshmen in psychology, a three week course was given. This course was very elementary. The participants received a general introduction to parapsychology and research methodology, including some statistics. They had to carry out a variety of small experiments under supervision, the results of which they had to evaluate.

There exist possibilities to do thesis work on a parapsychological topic, but since there is no sub-faculty of parapsychology, the thesis has to be defended within the sub-faculty of psychology (which in turn belongs to the faculty of the social sciences).

As a general policy we would not recommend as a first alternative, that a person write his thesis on an exclusively parapsychological topic; we would rather suggest that the person select a topic which is of interest and relevance to both psychology and parapsychology. This policy is based on considerations concerning the chances of obtaining a position after completing the Ph.D. work.

RESEARCH PROJECTS IN PROGRESS

The notion of a relationship between ESP and subliminal processes is an old one. The controversy about the existence and character of subliminal perception has ceased in recent years, and has made way for psychodynamic interpretations.

Professor Kragh's Defense Mechanism Test (DMT) has now been in use for more than a decade, both in studies of perception-personality and as a predictor of scoring behavior in an ESP task. The test can be described as a projective one. A tachistoscopic technique is utilized and "threatening" pictures like those of the Thematic Apperception Test are presented. The DMT was introduced into parapsychology by the present speaker in 1963, during a stay at the Duke Laboratory. The main discovery, based on results from a number of studies, seems to be that the DMT is a useful instrument for the prediction of a subject's scoring direction. By and large, subjects manifesting a low level of Perceptual Defensive Organization (PDO) in their protocols (thought of as being related to a low level of anxiety proneness) tend to score *above* M.C.E., whereas certain other types of PDO seem to be related to the psi-missing syndrome. Extensive studies are being carried out to try and find out more about these and a few other suggestive relationships.

Attempts are also being made to collect and analyse DMT data from subjects who, on the one hand, have manifested very pronounced extra-chance scoring in a laboratory setting or who, on the other hand, have experienced or performed things in a real-life context which lead them to believe, and perhaps others too, that they possess paranormal abilities.

Another line of research within the subliminal field which has been pursued for some time is related to the so-called Pötzl-effect. Here attempts are made to Pötzl-influence a subject's scoring behavior by means of subliminally exposed "micro-traumatic" stimuli-stimulus concepts which in subsequent ESP experiments will be used as one category of targets in a multiple-choice test situation. It is hypothesized that this procedure of "subliminal induction" will, in one way or another, influence a subject's psi-vigilance. So far the findings have not substantiated this surmise in a clear-cut way.

Recently a new approach has been introduced in which subliminal stimulation is utilized as an independent variable. The experiment has both a cognitive part and an ESP part. Two experimenters take part in the experiment, and the procedure is based upon individual testing. The subjects are given a problem-solving task which is supposed to be rather absorbing. One presumes that the task is interrupted just before the subjects arrive at the correct solution to the problem. After an intermission, a subliminal stimulus is given. Fifty percent of the subjects, this number being randomly determined, are given a subliminal stimulus depicting the correct solution, whereas the remaining fifty percent receive a subliminal stimulus depicting an inadequate solution to the problem. After another intermission the ESP part of the experiment is administered by the other experimenter. The subject is instructed to use "his intuition" to pick out the envelope which he thinks contains a target depicting the correct solution to the problem-solving task. In this multiple-choice "pick-out" test p is $1/5$; $q = 4/5$.

Each subject carries out 50 trials. His degree of success is not evaluated during the test session. During the course of the experiment neither of the experimenters is aware as to which type of subliminal stimulus a certain subject has received. So far a pilot-study involving 54 subjects has been carried out. No clear-cut overall outcome was observable.

Does distance affect paranormal communication? For a long time this challenging question has been asked. One of the weaknesses of almost all the studies which so far have been carried out in order to test such a distance-in-space effect, is the fact that either the experimenter

or the subjects, or both, have been cognitively aware at least of the fact that a distance parameter has been involved in the experiment. Hence the expectancies of the experimenter, the subjects or their combined working may have exerted an influence upon the outcome of the experiment (here I am supposing that extrachance scoring occurred and that that scoring was attributable to the operation of psi).

For a couple of years now, I have been working on the outline of what is both a complicated and a rather sophisticated ESP-Distance Experiment, which under favorable conditions may yield important and informative results. The experiment will be carried out in close cooperation with Mr. Einar Sverre Pedersen (E.S.P.), well-known for his pioneering work on polar navigation, which made it possible for Scandinavian Airlines to be the first airline to introduce the Polar Route into their schedule.

A few of the main features of the study should now be briefly mentioned.

a) the subjects will not, by means of normal cognition or inference, be aware that they are taking part in an ESP experiment involving the distance parameter;

b) the person (E.S.P.) who will be responsible for the assignment of the target sequences for each of the subjects will not be aware of the identity of the subjects; nor will he be aware of their location;

c) the administrator of the study (myself) will not be able, except by means of ESP, to trace the location of the above mentioned Mr. Pedersen and the targets at the announced time of the experiment;

d) the target sequences, with a special coding for each of the subjects, will be sent by Mr. Pedersen, (also responsible for the assignment of the targets), to a computer center, where the information will be fed into a computer. The corresponding sequences of calls made by each of the subjects will also be sent to the computer center, where the information (with the identification code for each subject) will be stored in the computer.

e) by utilizing a certain design characteristic, incorporated in the outline of the experiment, some possibilities will exist to find out if precognition alone could account for a possible overall extra-chance scoring.

f) the computer will be programed to decipher the distance code and to evaluate the possible influence of the distance parameter.

It should be mentioned that the distance between the targets and the subjects (subjects will be located in at least three different countries) will range from a few miles to thousands of miles, and that such a change in distance can be brought about in a few hours. According to

present planning, this semi-computerized ESP-Distance Effect Study will commence at the beginning of 1976.

Does ESP function in everyday life without being noticed? How could such a hypothesis be tested? It is reasonable to suppose that the advantages of testing ESP in a laboratory setting are partially offset by factors such as lack of real-life challenge, personal significance for the subject, and decline of the subject's motivation, etc. Dr. Rex Stanford and myself have tried to tackle this problem, quite independently of each other, and to outline test-situations which retain the control advantages of the laboratory, while a subject is placed in a real-life high-motivational situation, and is totally unaware that he is taking part in an ESP test. A couple of years ago I carried out three experiments in which university students participated. They were given a written exam in psychology. The exam consisted of eight questions, each followed by a blank space large enough for a short answer. The test sheets were attached to the front and back of a large, sealed envelope. Inside, underneath the appropriate spaces, attached to each side of a piece of cardboard of the same size as the test sheets, were copies of the front and back sheets respectively. On the sheets within, unknown and invisible to the subjects, were correct answers to four of the eight questions. For each subject a random procedure selected which four of the eight questions would have a "correct target."

The object of the investigation was to discover how well the students performed on the "correct" target questions (the ones backed by answers), compared with the "nontarget" questions. Because "target answers" were provided for only half the number of questions, each subject was able to act as his own control. In experiment 1 and 2 the information related to the target answers was correct, whereas in study 3, *incorrect* target answers were used. The evaluation of the answers was made following a rather detailed manual. The ratings were made blindly and independently by two raters. The inter-rater reliability was at quite an acceptable level. In all three studies, the unseen answers seemed to influence the quality of the subjects' own answers in the expected direction in studies 1 and 3 to a statistically significant level.

The findings seem strong enough to warrant replication and more extensive application, and we are now busy developing new types of disguised ESP tests—tests which nevertheless are still accessible to a statistical evaluation.

Recently we designed and carried out a pilot-study in which a test designated as the test of the "Hidden Dutch Cities and Villages" was used. The same rationale was used as in the case of the written academic exam. A subject is given a test sheet attached to a sealed

envelope. On the sheet there is a square comprising 567 letters, arranged in 27 lines times 21 lines. The letters are to a certain extent arranged randomly. At the bottom of the sheet there is a list of 18 Dutch cities and villages. The names of the cities and villages are embedded within the square of letters. In this cross-word-like pattern, six of the geographical names are spaced horizontally, six vertically, and six diagonally. The test is presented as both a cognitive and a perceptual one, and each subject is given 10 minutes to try and find as many of the names as possible. Inside the envelope, unknown and invisible to the subject, is a copy of the sheet. For half the group of subjects, the geographical names on the target sheet are encircled and have locations identical to those on the test sheet; for the other half, the names on the target sheet are encircled but displaced in relation to those on the test sheet.

The first study, involving 54 subjects, did not substantiate the hypothesis regarding a general influence from the disguised targets, but the study will be extended and modified, and the subjects who produced extra-chance scoring will be re-tested. By and large we are working hard on the development of this area. A very interesting type of ESP test, disguised as a reaction-time test, is just in the process of being introduced.

Our present visiting research worker, Mrs. Christa Lübke (from Freiburg Institute, Germany), has outlined and will very shortly commence a rather extensive study on the effect of an experimenter's expectancy upon a subject's cognitive, perceptual-motor, as well as ESP performance. She intends to use this study for her Ph.D. thesis in psychology.

Recently, parapsychology has been the object of studies; 1) from a sociological point of view, 2) from the point of view of the history of science. I strongly believe in the usefulness of cooperation between parapsychology and philosophers of science. So far, surprisingly few philosophers of science have indicated an interest in parapsychology. Dr. Michael Scriven and the Swede Ingemar Nilsson seem to be exceptions here. It is my impression that philosophers of science are, on the whole, not very familiar with our field and its findings. Even such a distinguished philosopher of science as Mario Bunge seems to be very ignorant regarding modern research standards in parapsychology. In his well-known textbook entitled *Scientific Research* (1967) he claims that parapsychology is a good example of a pseudo-science, as according to him we do not use open hypotheses. I do not believe that we have really succeeded in propagating our findings in a clear and informative way. Here I see an educational responsibility and a challenge. I feel too,

that this would pay off well. We, on our part, would benefit very much from a feedback of constructive criticism of our field from philosophers of science. Mr. Nilsson, at the University of Gothenburgh, Sweden, is well acquainted with the history of parapsychology and the present picture of problems and results, with their claimed implications. He is a well-trained psychologist, as well as a historian of science, although his special field is the philosophy of science. Because of this background and his keen interest in parapsychology, we decided to invite him for a couple of months as a research associate. He chose to write a couple of articles on the topic "The Paradigm of the Rhinian School", articles which are just in the process of being printed.

The main reason to start anpsi studies lies in the potentials which are offered by this research for studying brain processes associated with psi activity. Before this stage is reached some conditions have to be fulfilled. Electro-physiological research requires a heavy investment in money and time, so before commencing with such experiments one should be fairly certain that during the experiments the subjects will show psi activity. Therefore our research is aimed, in the first place, at finding a set of experimental variables which will enhance psi vigilance, or finding a hereditary effect that will enable us to breed "gifted" subjects. Up till now our efforts have been concentrated on manipulating experimental variables. However, although some experiments show significant results, the total outcome of all these experiments is disappointing, as regards finding a consistent pattern in experimental conditions associated with psi activity. Nevertheless, Dr. Schouten is continuing his studies and hopes, by exploring new techniques, to be able to obtain more satisfactory results.

A second line of research carried out by Dr. Schouten concerns psychophysiological studies. These studies are based on either a "transmission" of emotions, or on arousal effects. A major problem encountered in these experiments is, that a subject reacts to an emotionally stimulated "sender" by showing a rise in his arousal level, which lasts much longer in time than the specific arousal effect induced in the sender. Besides this, some methodological problems, concerning the definition of what exactly constitutes an ESP response in this experimental situation, have still to be solved.

Mr. Henk Boerenkamp, one of our staff-members, is carrying out a rather extensive study on selected subjects or "paragnosts." There are parapsychological as well as psychological aspects which are considered in this study, among other things including a micro-analysis of the perceptual as well as social interaction between the sensitive and the sitter. The study is intended for a Ph.D thesis in psychology.

In addition to the more elaborate research projects just mentioned some preliminary work has been carried out, on a strictly methodological basis, in the two following areas:

- (1) Attempts to try and outline new types of "detectors" to illustrate that something is "out there" in the vicinity of a target, when a subject reports an OBE related to the target;
- (2) Development of methods by means of which alternative hypotheses articulated in "survival research" can be tested.

PUBLICATIONS

Closely related to the carrying out of research programs is the communication of the results. Here I take pleasure in drawing your attention to the fact that we have recently established a periodical identified as the *European Journal of Parapsychology*. The journal will appear twice a year. The main emphasis will be on experimental articles, but theoretical articles will also be welcome. The latter category should be concerned with issues such as definitions in parapsychology, or design of experiments, or with the development of useful methods for the evaluation of parapsychological results or observations. It should be stated that theoretical must not be viewed as antithetical to experimental, since theories are just as important for scientific understanding as experimental data are for theories. It should also be understood that experiments are no more and no less than observations made under rare and special circumstances, which we ourselves choose, or at best "only experience carefully planned in advance," as R.A. Fisher put it.

When it comes to the ideal of science to which we adhere, we have to admit that we are rather strongly influenced by Karl Popper's school of thought, although one should always recall that no single current approach to the philosophy of science is free from serious and fundamental objections. At any rate, we sympathize with the idea that it should in principle be possible to put a theory to a severe test of falsification before one adopts it as a useful tool in science. I personally share Sir Karl's view, that the more complicated a theory is, the less it says, for the harder it becomes to falsify and the easier it becomes for those who defend it to "immunize" the theory. Another hallmark of the *European Journal of Parapsychology* will be the avoidance of selective reporting, that is, the tendency to bury negative results and only to publish studies that "turn out." For one thing, we believe that there may be a chance to learn something important even from negative findings; for another, the policy of only accepting "supporting"

findings may very well exert a strong temptation to "doctor" one's data. As a practical rule, we suggest that the acceptance or the 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. We also believe that this editorial policy should save the journal from becoming a graveyard for all the studies which do not "turn out." We shall work hard to enlarge our editorial staff in such a way, that most European countries will have a representative on it.

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

The establishment of a *European Journal of Parapsychology* is hopefully one of the first steps in bringing about a closer cooperation between parapsychologists in Europe. Cooperation between Edinburgh, Freiburg, and Utrecht has been initiated, and I am convinced that a pooling of our know-how and resources will be helpful in improving the quality of the research programs, as well as the educational programs. We also propose to reinforce this cooperation by arranging regional meetings at least once a year. I believe there exist possibilities for establishing a European Parapsychological Association, although I am not quite certain how such an organization should be formally affiliated with the Parapsychological Association.

Besides educational responsibilities within the framework of the University, there are also extramural ones. A strong public interest in parapsychological matters is shown by individuals, organizations, and mass media.

I am afraid that representatives of the press, radio, and TV, are too often biased in favor of the sensational and appealing news, rather than the releasing of subject-matter information in its popularized form. At least during the development phase of our laboratory we have had to curtail our extramural activities rather severely, to be able to manage what we consider as the high priority tasks.

Finally, I would like to make a few statements in my capacity as educator in parapsychology. I must confess that I have some

difficulties in understanding the logic of some parapsychologists when they proclaim the standpoint, that findings within our field have wide-ranging consequences for science in general, and especially for our world picture. It is often implied that the research findings within our field constitute a death blow to materialism. I am puzzled by this claim, since I thought that few people were really so unsophisticated as to mistake our concepts for reality. I am, of course, just as puzzled by the attempt to unify science and religion based on parapsychological findings. Here I am thinking of Dr. Mitchell's bold enterprise. I deeply respect his learned involvement and his beliefs, but his position, viewed from an epistemological point of view, is not convincingly strong. He seems to believe in the so-called physio-teleological proof of the existence of God, a way of viewing Man and his Universe which has a long and venerable history, but its weakness is commonly recognized among theologians, philosophers, and philosophers of science.

I am sure that it is not of any great help to say that Dr. Mitchell enjoys the company of many respected and distinguished men of science such as Eddington, Einstein, Jeans and Lundmark, to mention a few names. It is hard to deny however, that the motivation behind parapsychological research has, from the very beginning, been religious rather than purely scientific. I would not like to go as far as to say that parapsychology will not perhaps provide us with knowledge relevant to the understanding of some religious phenomena, but I believe that we should not make extravagant and, as I see it, unwarranted claims about the wide-ranging consequences of our scattered, undigested, indeed rather "soft" facts, if we can speak at all about facts within our field. I firmly believe that wide-ranging interpretations based on such scanty data tend to give us, and with some justification, a bad reputation among our colleagues within the more established fields of science. These critical words do not imply that I am not aware of the fact that almost every theoretical system involves some aspects of a particular world picture.

This is the case, even in fields which are well established and enjoy the reputation of being "pure" sciences. Even there, you will find presuppositions and traditional ways of viewing. These presuppositions are not necessarily articulated, but taken for granted, and transmitted via the educational process.

As an educator in parapsychology I would also like to create suspicion among my students for all types of scientisms. Scientisms are no more and no less than pernicious exaggerations of the function and status of science, or of a scientific paradigm, at a certain time in a certain cultural setting. The risk of relying too strongly on a scientism is

highlighted by the much quoted case of the French Academy towards the end of the eighteenth century, when it declared that the idea of meteorites sometimes falling from the sky was a superstition, unworthy of their enlightened times. We are no less dependent on theory and presuppositions for distinguishing between facts and superstition.

And then let me just touch upon the concept of repeatability! May I remind you of the highly stimulating dialogue on this topic by Dr. J.B. Rhine and Dr. John Beloff! I certainly agree with Beloff that we, in the present situation, are lacking a highly repeatable experiment, and that our situation would be somewhat better off if we did happen to have such a thing. (Note that Dr. Beloff does not inform us about what kind of requirements he has in mind as to the level of reproducibility he considers as acceptable, as opposed to non-acceptable.) As I see it, Beloff highlights the fact that experimental parapsychology is still a rather "immature" science. (According to Imre Lakatos "mature" science consists of research programs, whereas "immature" science consists of a mere patched-up pattern of trial and error.) As an educator however, I would like to make a rather pedagogical point related to the doctrine of repeatability being a *sine qua non* in experimental research.

May I remind you of the phlogiston theory! With this theory *accurate predictions could be made*; different scientists could *replicate* each others results. In other words, *a high degree of repeatability thus existed*, and nevertheless it turned out that the theory was false, since it was based on erroneous propositions as to the nature of matter! What I here want to say is, that not even the joint criteria of *repeatability*, and having a *theory* by which they could make rather adequate *predictions*, was enough to constitute a reliable line of demarcation between what was "scientific" and what was not.

At any rate, I would like to suggest that we, as parapsychologists, strive to keep an open mind on every issue, no matter how baffling or incredible it may appear at first glance. As scientists, we have to take certain risks, for instance in choosing certain statements and data as the starting points for our work, and temporarily at least, we have to ignore and live with conflicting findings, until such a time that our research projects have reached a certain level of development. At the same time as I am asking for a certain persistence and endurance in our work, I see it, however, as a duty to protest against the regrettable habit of using parapsychology as a kind of trash can for all mysterious and occult ideas, no matter how poorly validated they are. We should not be afraid to confess that we are badly lacking a consensus as to what is paranormal and what is not. Based on previous experience, I suspect

that the readers of our articles will be easily led to believe, that there really does exist a consensus on the "paranormal" character of all the peculiar and disparate effects reported in our publications; effects which may not have more in common than the fact that they are hard to explain. What I would here like to stress is that intellectual honesty does not consist in trying, by any means, to prove one's position; rather it involves trying to specify the conditions under which one is willing to give up one's position.

I have been pleading for a mixture of open-mindedness, self-criticism, endurance, and flexibility, in our behavior as research workers and educators. Further, we should avoid thinking that everyone who disagrees with us and rejects our claims, is an ignorant, unsophisticated being. I adhere to Popper's view that there is a certain need for dogmatism in science: "the dogmatic scientist has an important role to play. If we give in to criticism (and wild ideas) too easily, we should never find out where the real power of our theories lies".

In this closing section, I have to make a confession which may sound paradoxical. One of the reasons why I am interested in parapsychology is the cryptic character of the data. My position, simply stated, is that sometimes data however "odd" they may look, may provide a point of departure for very fruitful research and for significant theoretical advances. At the same time, one should remain very much aware that some "odd" out of place data may turn out to be an artifact, depending on errors of observation and interpretation. In other words: data may seem to be cryptic because there is really nothing to be explained except the artifact. I suggest that we, as parapsychologists and educators, should be courageous enough to admit that we often may be dealing with mere artifacts.

For quite a time it has been the popular view that the natural sciences are the paradigm of knowledge. This view has been systematized in various types of positivist philosophy. I suspect that my way of appreciating the paradigm of knowledge shows me to be a child of my time. There are moments, I confess, when I tend to think that paranormal phenomena cannot be properly understood until we have been able to create new ideals of science.

I am far from certain how one might be able to initiate such a process. At any rate, it may be of some educational value to dwell upon the following statement by Paul Feyerabend: "the sciences after all, are our own creation, including the severe standards they seem to impose upon us. It is good to be constantly reminded of the fact that science as we know it today is not inescapable, and that we may construct a world in which it plays no role whatsoever".

DISCUSSION

RAO: Dr. Johnson, I don't know if I understood correctly the point you are making. If I am wrong, please correct me. You seem to think that we should not exaggerate the implications and significance of parapsychological findings and that they are not as terribly important as several of us seem to think. If this is what you said, I'm afraid I have to disagree with you and I will tell you why. Now, if a person is making exaggerated claims in terms of reaching beyond the results he has, I agree with you, and I think we have to be humble enough to limit the impact of our findings in terms of their range to very specific and narrow limits that judgment would allow. But I would not be involved in research in this field, where the possibility of finding the phenomena is so minimal and the percentage of its manifestations so minute, unless I'm convinced that this, when found, is going to have enormous implications. I feel it does. For one thing, parapsychological findings raise serious doubts of the adequacy of the dominant paradigms in current science. This to my mind is more important than anything else I know in psychology. I recall reading Dr. Beloff's book *The Existence of Mind*, in which he considers parapsychology to be "the ultimate battle ground on which the mind-body controversy must be fought out." If what I am saying makes any sense, the implications of parapsychological findings are extraordinarily important to anyone whose business it is to deal with ideas, and I think man's business is, to a significant degree, to deal with ideas after he learned to fill his stomach. So I must very respectfully disagree with you, Dr. Johnson. Parapsychological findings, if true, are tremendously significant to our understanding of man.

JOHNSON: Thank you, Dr. Rao, for your viewpoints. I think that it's partly a misunderstanding because I had to abbreviate what I had written. I think that very often, just as in psychology, we have very scanty data and we should be very careful when it comes to the interpretation of the data. Personally I wouldn't be in the field if I didn't think that it's important and that the findings are important. But sometimes I think that we have too much popular writing and too wide-ranging interpretations. Well, I have nothing against it, even when scientists themselves do it, if they are using "if," or "maybe," or saying "this is a kind of science-fiction which you are allowed to carry out," if you declare these are not facts but possibilities. That's my attitude.

ROGO: I'm very happy to hear about the *European Journal of*

Parapsychology, and probably like many others here, I have many questions about the editorial policy of the journal. First of all, is it going to be devoted exclusively to experimental work, or will it also publish papers on theoretical and historical aspects? Will American parapsychologists be allowed access to publishing in this journal? Do I take it that the journal will be in English and when will it start to appear, and will it be a PA affiliated journal?

JOHNSON: Well, I'll try to answer your questions. There will be, I believe, a certain emphasis on experimental work, but it's not exclusively on experimental work. If you have a theoretical paper, you can publish it. Everything is relative in this world and the criteria can always be discussed. I'm very pleased to announce that Dr. John Beloff will be on the editorial board, as a British representative; Dr. Bender will be a representative of Germany. You can write a theoretical paper and I think you could also use case studies if you are interested in that and the journal is open; American contributors are welcome, of course. I don't know yet if it's possible to affiliate this journal in some way with the Parapsychological Association. These are matters which we have to discuss later. Now, the first issue will appear in October, and there will be two issues a year, published in English.

RHINE: Dr. Johnson, about the journal, in which I'm very interested, as I am in your whole policy and program—just a little question bothers me. Are you going to be open to publication of all the chance reports that come from around the world?

JOHNSON: I beg your pardon, sir?

RHINE: Are you going to accept for publication all the reports of chance results (I call them failure reports)? Are you going to be open for that, because if you are, I want to know where we can advise people to send such reports. You see, we had the idea for the *Journal of Parapsychology* many years ago that in this field if you don't get any evidence of psi you have nothing to experiment with; you have nothing but your failure to report. We settled on a simple program of agreeing to publish a short abstract, not ignoring the fact of the failure. The editors furnish a copy of the complete paper on request.

JOHNSON: Well, as I put it, by using the editorial policy of basing the acceptance or rejection of the manuscript prior to the phase when the experimental data are collected, we thought that we could avoid turning the journal into a graveyard of all the studies which didn't turn out. That means that one should, beforehand, if possible, if feasible, decide whether one should accept or reject a manuscript, try to

acquaint the authors with this policy and just see how it works out. We may have to surrender. That's quite possible.

PALMER: I am, like Mr. Rogo and Dr. Rhine, very happy to hear about this new journal and wish it a great deal of success. Also, I very strongly endorse your policy of trying to accept or reject experimental papers prior to knowledge of results. I don't know how it will work out; I certainly hope it will work out, and perhaps it will be a precedent for other journals. Just a brief comment about this problem of publishing only positive results. It seems to me selecting only positive experiments for reporting is in principle the very same thing as selecting only the positive runs in an experiment, selecting those out and reporting them—we certainly never do that for runs and I don't see why we should do it for experiments either. But I do have one question I'd like to ask you about policy, and that is, whether the editorial decisions will be based solely on methodological adequacy, or on the likelihood of the experiment coming out, being successful. I know this kind of criterion has been used by certain review committees for government agencies. I was wondering whether that kind of a criterion will be applied at all, or whether decisions will simply be based on methodological adequacy?

JOHNSON: Well, Dr. Palmer, I'm afraid I can't give you a very specific answer. I believe one could say that there are different rationales for a hypothesis put forward in a paper, and if it's a very unlikely one, I suspect that the editors will be prejudiced against it. I'm using the word "prejudice," but at the same time, I hesitate a bit because sometimes, but very rarely, you may miss something very important when it comes to unorthodox hypotheses.

NOVILLO: We are talking about the mind, its dynamics and effects. The proper subject of parapsychology is the mind and thus, we must ask ourselves: "what is the mind?" To plan a program of investigations, to research correctly, to lecture properly, we must understand what the mind means to us.

JOHNSON: My answer is rather short. Because I don't know very much about what constitutes the mind, I can't speak much about it. That's my answer.

TART: A comment on your editorial selection policy. It sounds a lot like the procedure used to review grant applications, looking at the methodology alone. It usually works out well. There are some apochryphal stories around, however, about investigators who first do the research, and when it's done, then they apply for the grant for it,

and spend the money on their next research project. This gives them a great deal of freedom. Unfortunately, the story goes that every once in a while a grant committee will turn them down, saying this research can't possibly work out, even though these people have already done it successfully. You might consider a two-phase policy, where you'd review studies without results, and then give the reviewers a second chance with the results.

SCHMEIDLER: I want to congratulate you on that brilliant editorial policy. I particularly like it because it gives the editors a change to suggest the changes which it's just not practical to ask for after research is completed but which so often an investigator will gladly put in when the change is suggested in advance. I was particularly interested in the possibility of using the standards of dissertation committees, of having the author state in advance the hypothesis that he wants to test, which would make a nice clear distinction between the post hoc conclusions which sometimes are the strongest ones, and the initial hypothesis. If you could have somebody looking over the investigator's shoulder, making sure that the subjects are run after the proposal is put in, I think that you might have very constructive input into parapsychology. And I hope it would spread into other fields too.

STANFORD: I simply want to congratulate you on the journal and also what sounds like a very fine broad-range constructive program at the Utrecht center. Now, I want to comment specifically on the editorial policy which sounds to me to be a good one, and just simply to add some reinforcement behind that. I think one of the things that the willingness to regularly publish negative results can accomplish, is something very useful for future research. Aside from the fact of not attempting to replicate an experiment that has already failed in replication, it can also aid the ability of a person who is familiar with the literature, to estimate the possible strength of the effect that he may wish to study in an experiment. He can get an idea of the statistical parameters of the population that he's studying and therefore can design his experiment, including sample size, more intelligently and stand a better chance of replicating someone's results or extending those results, etc. If you don't have the total picture of results, this is very difficult.

John Palmer has beautifully exemplified this in one of his sheep/goat reviews, in which he showed that much of the work that isn't statistically significant on the sheep/goat effect, is, nonetheless, in line with the idea that there are population differences between sheep and goats. But if we didn't have the non-significant experiments available, we wouldn't

know about the strength of the effect, and couldn't apply that in future research on that topic.

Now the second point is that the publication of negative results, so-called, allows not only the elimination of certain sorts of artifactual findings from the supposed findings of the field, but it also does something else. I think it may encourage cross-experimenter replication, which is good—and it's particularly important, I think, because we all know that there is an experimenter effect in the psi area; possibly one that is psi-mediated as well as one that is socially-mediated.

RAO: As an investigator in the field, I would warmly welcome the opportunity to publish papers incorporating experiments that did not yield significant results for at least one reason, viz., I would have a few more to add to the list of published papers. But, thinking of the field as a whole and in the context of making certain policy recommendations for the consideration of those who are in charge of journals in our field, I would like to make a few suggestions, especially concerning the reporting of negative results.

The implication of Dr. Palmer's comment that nonpublishing of an experiment is equivalent to selection of data in a given experiment is valid, it seems to me, only if an experimenter who, let us say, did three experiments to test a variable, chose to report only the results of a single experiment which alone yielded significant results. I do not think this implication holds good in a number of other cases when the investigator chooses not to publish his data. I will explain why this is so.

The phrase "negative results" is an ambiguous one and means several things depending on the context of its use. Firstly, it may mean that the results obtained are insignificant in the sense they do not give evidence of psi. Secondly, it may also mean that the results do not confirm the hypothesis that is being tested, even though there is evidence of psi manifesting in the experiment, or that the results indicate a significant relationship opposite to the one expected. We find our journals publishing a number of reports which provide overall evidence of psi even though they fail to support a stated hypothesis or give significant results in the direction opposite to the one expected. Therefore, there is no issue on the second point.

It is reasonable to assume that a number of experiments which contain no significant data are not published because either the experimenter himself chooses not to publish them or the editors of the journals do not accept them for publication. This may happen in a variety of contexts. The experimenter may be attempting (1) to test a new variable or idea which has not been reported to have been examined or tested by others, (2) to try out a novel design or method,

(3) to confirm the finding of his pilot study, or (4) to replicate (a) his own finding which he reported earlier, (b) a finding reported by another investigator or (c) a study that has become for some good reason controversial in the field.

With regard to (1) and (2), I believe, the investigators as well as the editors of the journals should have freedom to publish or not to publish. The decision should be based on the importance of the idea tested or the significance of the method described. With regard to (3) it is a good thing that we no longer publish in our journals pilot studies which have not been confirmed, unless the pilot study itself is considered to be important for some special reason. When this happens, the failure to confirm a pilot study should be reported. With regard to unsuccessful replications, all that would be necessary is to publish the abstracts of the studies. It would be desirable if the investigator writes up a complete report of the experiment and supplies a copy on request to those interested. Concerning item (4c), which involves attempted replication of some important study which is the center of a continuing controversy, e.g., a reported significant relationship becomes questionable because some new circumstances have arisen, like the unreliability of the experimenter, I believe, there is a good case for reporting insignificant results. Thus I see a number of occasions which do not call for the publication of a full report of an unsuccessful experiment, an experiment which gave no evidence of the occurrence of psi.

The argument that all insignificant psi results should be reported comes from two quarters. First, there are those who are not yet convinced of the reality of psi and wish to evaluate the overall case for psi. They argue that they cannot be expected to come to a reasonable conclusion if the results of all the ESP experiments that have been so far conducted are not available. What are reported may be only the selected best and a large number of experiments which gave only chance results may not have been reported. Therefore, they demand that all experimental results should be published. This is an understandable position which is partly based on misunderstanding of the scientific method and partly due to the ignorance of the work done in our field. I shall not attempt to go any further into this because I do not think there is anyone here who is still in that state of mind.

The second group comprises those who are convinced that psi exists and that it can be measured in terms of test results. They plead for the reporting of all controlled experiments for two reasons. Firstly, if the journals publish only reports of experiments yielding significant results, this may encourage the investigator to select only parts of his

data and report a paper that meets the requirements of significance, ignoring the rest. Secondly, non-publication of experiments yielding insignificant results may result in a situation where the true strength of the relationship between variables may not be known in a field like parapsychology, where one cannot be very confident about replication. I believe the first argument is unjustified. It is in a sense as ridiculous as the accusation, which is sometimes made, that encouragement given to those obtaining significant psi results puts pressure on those who are less successful to fake their results. Selection of data is professionally unethical and it must be made known to be so to any one entering the field. It would be naive to think that publication of reports of insignificant results would be a sufficient condition to ensure against unethical selection of data.

The second reason has some face validity. It would be helpful to have information about all attempts to test a particular variable. But this is better served by the publication of an abstract than a full length paper. The abstract would be valuable for the reasons Dr. Stanford has already mentioned—for the assessment of the strategies of research and for arriving at a dependable judgement on the strength of the assumed relationship between variables.

I am afraid, however, there is still a confusion in the minds of some of us as to what an ESP experiment which gives essentially chance results really means. You will agree with me that psi research is somewhat different from other kinds of research that go on in psychology. This difference is in the very nature of the phenomena we study. When we attempt to study a relationship between two variables, such as personality disposition and psi-hitting, we make the assumption that in a given test psi would manifest so that we can examine whether the relationship holds. Unfortunately, our assumption sometimes is not fulfilled, because our results do not give any evidence of psi. I do not see how in such an experimental situation, where there is no evidence of psi, one can reasonably talk about a relationship between psi and another variable. It is like a psychologist attempting to relate creativity to a particular personality characteristic by correlating the scores on a personality test with a test of "creativity" which is not known to be valid. Evidence within an ESP experiment that psi is present in the data ensures the validity of the psi test without which no meaningful conclusions about psi can be drawn. A psi experiment which gives only chance results, is often without any value for an inference because the validity of the test employed is conditioned on the results obtained. Unlike in psychology, where a test once validated can be used on comparable samples without further validation, a psi test needs

validation within each experimental study. The validation in each case is the evidence that psi has manifested in the experimental situation. Therefore, for those who accept the reality of psi, a study that yields only chance results is often without much value. This makes the case for publishing chance results less defensible.

JOHNSON: I, at least, partly agree, and already financial reasons may make it turn out that way—that papers not giving significant results will be abbreviated a bit. But of course, then we have the difficulty with the fact that the procedure must be described adequately enough, I believe, to make it justified.

RHINE: First of all, on your last point, Dr. Johnson, you sound very reasonable. I think it's the way it will go. There is another point to add to this: When the editors of the *Journal of Parapsychology* publish an abstract of a paper they offer the reader a copy of the full paper on request. This policy enables the editors to avoid publishing pages and pages of empty results which probably no one will read when they see in the abstract that the data were due to chance.

In 1957 Pratt and I proposed in our textbook the pilot-confirmation technique, a program in which a person may do as many preliminary tests as he likes with no responsibility to anyone. When he discovers a successful procedure and gets evidence, then, and only then, does he see to it that all his controls, etc., are tight enough that he can draw a conclusion when results are secured. He will then see if he can follow up that pilot experiment with a confirmation experiment under these conditions. By this approach we can save both the experimenters and the editors a lot of useless negative reports; we are all spared the cost, the time, and the discouragement of pre-pilot test efforts that did not yield anything. (To be sure, there are strict rules about this pilot-confirmation; for one point, it is understood that the conclusion, if any, must rest only on the confirmation, not on the pilot.)

There are times of course when we do need to know about chance results. This is too obvious to need any discussion. For the most part today the non-significant results (or P. value) ideally belong to a subordinate section of an experimental design, in which, as planned the psi function was not *expected* to operate. Or again chance results may be anticipated where the internal conflict of scoring trends tends to cancel the deviations if they are in opposite directions from mean chance expectation. But chance results in simple pre-pilot exploration or training exercises can best go to the wastebasket.

TART: I'll throw a bit of quantitative data in here. About ten years ago, Burke Smith and I did a survey of members of the Parapsycholog-

ical Association. Among other things, we asked them approximately how many unpublished experiments they had done versus how many published experiments. This sample does not include beginners, as Dr. Rhine pointed out, but people who are presumably at least occasionally successful enough to go on as parapsychologists. The ratio was about three unpublished experiments for each published experiment. Now most of the experimental articles appear either in the *Journal of Parapsychology* or the *A.S.P.R. Journal*. There are eight issues a year between them. With a three to one ratio we could estimate that you already have 24 journal issues a year filled with what are presumably negative results. So I think you're going to have to use a more feasible editorial policy, such as publishing only abstracts of the studies that didn't come out but making the full text available by Xerox to experimenters particularly interested in that specific problem area.