RESEARCH IN PARAPSYCHOLOGY 1972

Abstracts and Papers from the
Fifteenth Annual Convention of the
Parapsychological Association, 1972

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PREFACE

The Parapsychological Association was founded in 1957 at Duke University as an international organization to support scientific and educational efforts in parapsychology. Parapsychology is the science which deals with the capacity of humans and other living beings to obtain information about their environment without known means (extrasensory perception or ESP) or to induce physical changes in their environment, again without known means (psychokinesis or PK). ESP information is sometimes about the thoughts or emotions of someone else (telepathy), at other times about objective physical events (clairvoyance) or future happenings which cannot be inferred logically (precognition). In addition to doing experiments, parapsychologists conduct field investigations of persons and situations which seem to involve ESP and PK (together called psi) phenomena. These studies include investigations of so-called mediums and psychics, reincarnation subjects, poltergeist cases, and so on.

The main activity of the Parapsychological Association has been its annual conventions where parapsychologists from all parts of the world meet to exchange research findings and ideas. Usually an outside speaker is invited to give a talk about the field from the perspective of his expertise and interests.

For the last eight years, the Parapsychological Association has published an account of the papers presented at its annual conventions in the Proceedings of the Parapsychological Association. The convention program is open to any paper which the Program Committee considers worthy, whether or not the author is a member of the Association. Because of this and because of the international scope of the Association, the conventions provide a yearly survey of parapsychology around the world and the Proceedings give an up-to-date overview of this work.

Eight issues of the Proceedings have been published

previous to the present volume. The last of these has a cumulative index, and the eight volumes constitute a uniform collection. The uniformity has been interrupted with the present volume. This is a step ahead for the Parapsychological Association and the former Proceedings. By placing the publication of the Proceedings (now titled Research in Parapsychology) with the Scarecrow Press, the Association is seeking not only increased circulation, particularly to libraries, but also a reduction of financial and administrative burdens both for the Parapsychological Association and for the Psychical Research Foundation which hitherto has assumed the responsibility for publication and distribution.

Most important, the collaboration between the Parapsychological Association and the Scarecrow Press promises a substantial increase in the readership of Research in Parapsychology which will help to meet the demand for information about the explorations into the potentials of man--and animal--which we call parapsychology.

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THE FIFTEENTH ANNUAL CONVENTION

The Fifteenth Annual Convention of the Parapsychological Association was held at the University of Edinburgh, Scotland, on September 2-5, 1972. A total of 175 people attended, of whom 58 were Association members. John Beloff was chairman of the Program Committee, which also included J. G. Pratt, K. M. Goldney, Alan Gauld, and Hans Bender. The Research Briefs Committee was composed of J. Blom, Martin Johnson, and J. Kappers. Halla Beloff served as chairman of the Arrangements Committee.

The British author Arthur Koestler was the Invited Dinner Speaker. John Beloff of the University of Edinburgh, as Parapsychological Association president for 1972, gave the Presidential Address. At the beginning of the convention, the University of Edinburgh held a reception for those attending the meetings, following which Fraser Nicol gave the first of two special presentations. The second special presentation was given by R. A. McConnell at the conclusion of the convention. On Sunday, September 3, there was an open meeting of members and associates of the Parapsychological Association.

In keeping with the practice followed in the former Proceedings of the Parapsychological Association, of which this issue is the first in a continuation series, Research in Parapsychology 1972 contains condensed versions of the special presentations, research briefs, and papers at the Fifteenth Annual Convention, arranged in groups corresponding to convention sessions. The Presidential and Invited Dinner Addresses are given in full at the end.

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SPECIAL PRESENTATIONS

A PSYCHIC TOUR OF HISTORIC EDINBURGH

Fraser Nicol (Lexington, Massachusetts)

Paranormal, or quasi-paranormal, phenomena in Edinburgh have a history going back more than 800 years. The purpose of this paper is to trace the history of what began as stories hardly distinguishable from myth, continued through more plausible but poorly authenticated cases until higher standards of evidence began to emerge nearly two centuries ago, and finally developed in the twentieth century into strict methods of research which are now in full swing at the University of Edinburgh.

The earliest known case was that of King David I who while hunting in the neighborhood of the city in 1124 was attacked by a stag. When the king grabbed at the animal's antlers he found himself instead grasping a silver crucifix. Out of this incident, we are told, the king was inspired to build Holyrood Abbey. The story, however, was not recorded until 300 years later, and modern historians regard it only as a legend.

In 1513 two supposed apparitions manifested within a few days of each other, at Linlithgow and at Edinburgh Market Cross. Both apparitions were said to have uttered warnings against King James IV's projected invasion of England. Long afterwards Sir Walter Scott, while accepting the surface facts as probably true, suggested that the "apparitions" were political stratagems invented and acted out by the king's opponents in the hope of dissuading him from his intended war. The king however led his army into England and was overwhelmingly defeated at Flodden, he himself losing his life. In the sixteenth and seventeenth centuries every standard of sane evidence was thrown to the

winds by many witchcraft trials leading to many burnings at the stake.

In the eighteenth century a very curious mediumistic experience befell Lady Primrose, who had for some time been separated from her dissolute husband. She visited a medium who invited her to look into a mirror. There she saw a vision of her husband being married to an unknown lady in an unidentifiable church. Suddenly Lady Primrose's brother (then abroad) appeared in the church, and stopped the bigamous marriage. Lady Primrose was reported to have written an account of her experience, and on her brother's return to Edinburgh, we are told, he confirmed his sister's experience in all essential particulars, stating that the actual incident occurred in Amsterdam.

Highland second-sight and numerous apparitional cases became much talked about, and attracted the interest of well-known people who lived in or visited Edinburgh, including Daniel Defoe, James Boswell and Samuel Johnson. Sir Walter Scott's severely objective writings on paranormal incidents helped to raise the standards of evidence in Scotland. Serious experimental research in mesmerism and travelling clairvoyance was introduced by William Gregory, professor of chemistry at the University. D. D. Home's "physical" phenomena were investigated by a group of doctors and scholars who were favorably impressed. And later, among famous people who took part in psychical research, were two natives of the city, Robert Louis Stevenson and Sir Arthur Conan Doyle.

After the Society for Psychical Research was founded in London in 1882, Edmund Gurney, its leading investigator, visited Edinburgh to cross-examine witnesses in a number of very interesting spontaneous cases. It may be mentioned that three future Presidents of the S. P. R. --Eleanor Balfour (Mrs. Sidgwick) and her brothers Arthur and Gerald--were born in or near Edinburgh. Quantitative research began in the city in the 1930's, largely due to the persuasive influence of Whately Carington who twice visited the city. Psychokinesis experiments with dice (the earliest outside the United States) began about that time; Carington's paranormal cognition experiments with drawings involved in part two groups of Edinburgh residents; and Dr. Mary Collins of the University Psychology Department recruited 26 percipients for the research.

The most impressive development in recent times was the series of strictly scientific researches carried out by Dr. John Beloff and his colleagues at the University. And finally this year, largely due to the efforts of John Cutten and the generous support of friendly benefactors, a "Student in Parapsychology" has been appointed at the Psychology Department of the University.

PARAPSYCHOLOGY AND THE OCCULT*

R. A. McConnell (University of Pittsburgh, Pa.)

The successful propagation of a scientific revolution requires an expanding chain reaction of private discovery and public acceptance. It is only by the formal acknowledgment of new discoveries throughout our schools and colleges that trained scientists and supporting money can be drawn into the growing intellectual enterprise. In the last five years parapsychology has become a respected subject for discussion in the university classroom. This shift is made possible by the disintegration of social values and beliefs that had no place for psi phenomena. Although this disintegration has provided an opportunity for parapsychology, it also poses a threat.

In addition to an unending succession of pseudo-scientific books on ESP, television programs have appeared nationwide, dealing in violence, death, and psychosis, and claiming to be based upon the findings of parapsychology. Syndicated astrology has crept upward to middle-class newspapers and family magazines. Advertisements in the daily press sell lessons for the development of psychic powers. We are living in a crescendo of popular superstition whose only relation to parapsychology is through the substrate of weak, sporadic natural phenomena to which both attempt to relate.

The distinction between the interest of the common

*Dr. McConnell's address has been accepted for publication in the Journal of the American Society for Psychical Research, Vol. 67, No. 3, July 1973.

man and that of the scientist in psychic phenomena--a distinction based upon discipline, technique, and goals--is lost upon the layman and is in danger of denial by scientists, who are repelled and frightened by the present populist mockery of reason as a guide to life. It is still far from certain that the classroom study of ESP will be able to fend off the destructive emotional demands of the populace so that parapsychology can continue to develop as a science. The danger can be apprehended from the following four examples of recent anti-scientific, occult activity.

The first example illustrates occultism among ignorant but influential public figures. The New York Times of October 9, 1971, reported a speech by the president of the Board of Education of New York City at a forum sponsored by the Public Education Association. The president was quoted freely as follows: "If astrology is correct," the reason some youngsters pose behavior problems in class is that their "birth signs" conflict with those of other children or the teacher. He went on to say: educators and counselors should use "every art and science, including astrology, to know their pupils better, predict behavior, and individualize instruction." On October 27 the New York Times reported that this statement had been defended by the executive director of the National Association of Adult Educators.

The second example illustrates the trend for educational institutions to toady to anti-intellectualism under the guise of serving the needs of the community. A student had informed me of a course in parapsychology being taught at our county community college. Upon request, the college sent me a catalogue sheet in which, under the heading "Metaphysics and E. S. P.," the course was described as including astral projection, the human aura, dreams, unidentified flying objects, and Eastern mysticism. The instructor was named but was identified only by the title "Exorcist." More recently, this instructor has been featured in a Pittsburgh magazine, where she was described as a professional practitioner of white witchcraft who rides a Cadillac instead of a broom. The teaching of such courses by nonprofessional personnel as money-making, "continuing education" courses is widespread among American institutions of higher learning.

A third example of anti-scientific occult activity is the commercialization of psi phenomena as part of a system

for "improving one's mind" by meditation. There are variations, but the package of ideas to be sold generally consists of the following elements: (1) a pseudo-theory explaining how mind-science and alpha waves can solve all personal problems, (2) training in self-hypnosis, (3) training in self-assurance by secular prayer, (4) training in the avoidance of uncomfortable thoughts, and (5) training, or purported training, in extrasensory perception. The most successful of the organizations peddling this package consists of five interrelated business corporations selling what is known as the Silva Mind Control Method. Its growth in recent years has been phenomenal, and its promoters have been able to persuade educators to include it in several high school and college curricula.

My fourth example illustrates the current trend toward premature application of parapsychology by sincere practitioners in religion and medicine. A nonprofit California organization called the Academy of Parapsychology and Medicine recently published a book called Varieties of Healing Experience, offered as the transcript of an interdisciplinary symposium held in San Francisco in October 1971, and consisting of lectures given by seven directors of the Academy. The scientifically most offensive of these is the lecture given by the chairman of the Department of Materials Science at a great American university, a man of established scientific standing. While in England, he became enamored of the "Delawarr radionics diagnostic instrument," a neatly built portable case having a panel sprinkled with knobs and dials connected by meaningless hidden electrical wires.

Using this machine, he claims, one can "heal the general spectrum of ailments in humans, from the common cold to serious illnesses of the various glands.... The method has also been used for prospecting for oil, water, or minerals ...; for finding lost articles or people; for designing industrial equipment or selecting staff; [and] in education, for lowering a student's resistance to a subject, school, or teacher." He goes on: "The device contains ... a cavity covered with a rubber membrane that serves to provide a yes/no response to a particular, mentally-held question.... The procedure for obtaining this response is to drag one's fingers repeatedly across the membrane without pressing so hard that the rubber bunches up.... The no response to a held thought is that nothing special happens as the membrane is rhythmically rubbed. The yes response to a held thought is that friction between the finger and the

membrane seems to increase and [a sticking sound] is obtained. . . ."

This metallurgist then gives several pages to his own high-level theoretical explanation of the operation of this machine in terms of atomic states, three kinds of mind, potential distributions in etheric space, spectral-distribution curves, spirit waves, the 12 meridians of Chinese acupuncture, frequency sets, E and H waves in resonant cavities, and "the metaphysical principle: as below, so above--as above, so below." The kindest thing that could be said for the Delawarr machine is that some exceptionally psychic person, using it as a psychological crutch, might come up with true answers, just as a water dowser might get information with his forked hazel stick. Ironically, the machine had been exposed in 1961 in a 20-page article in the Journal of the Society for Psychical Research.

How should we oppose, or seek to avert, the ill effects of the vulgarization of parapsychology? Our efforts must be creative rather than critical. Paramount among our continuing objectives must be the production of careful research, the maintenance of high standards in publication, and selectivity in admitting apprentices to our professional company. We must also encourage the teaching of parapsychology--not by witches, spirit mediums, and religious healers, but by professional science teachers, preferably psychologists, in our high schools and colleges. And finally, we should recognize the psychological need behind much of the popular interest in parapsychology, the need for an undeniable basis for relating man to mankind, so as to allow the choice of a value system conforming to nature.

In the U. S. A. no one with institutional power and intellectual sophistication has taken time to examine parapsychology for its sociological potentialities. Perhaps parapsychologists should look elsewhere for a fuller appreciation of their subject. The systematic dissemination of the serious experimental literature to other areas of the world could broaden our base of support among orthodox scientists and foster a climate of healthy competition. In those places where there is substantial intellectual oppression, questions concerning inter-human relations are more poignantly asked and only a little encouragement might lead to a creative flowering of experiment and theory beyond our present capabilities.

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RESEARCH BRIEFS

Session I*

A PARANORMAL DREAM DURING ANALYSIS

Emilio Servadio (Italian Society of Parapsychology, Rome)

Psychoanalysts, including myself, have often reported apparently ESP dreams occurring in their patients during analysis. The following case seems to me to be a nice example of ESP in a dream, and of the exploitation of an ESP stimulus in exactly the same way that the so-called dream-work can utilize material from the day before perceived by the subject through normal channels. On the night of June 22, 1972, an American patient of mine had the following dream: "I saw a strange machine having sixteen cylindrical bodies. Somebody told me, 'If you write the first fifteen lines of a poem, or of an article, the machine will write the sixteenth line.'"

In the days preceding the dream, some interesting things had occurred in the life of this patient, who had been an obsessional neurotic and was now almost recovered. He had had to make great efforts to keep under control his aggressive drives, which were constantly stimulated by the behavior of a woman with whom he had had a close relationship in the past. He had manifested his desire to get away from this woman and from the whole situation. Moreover, he had been busy translating an article on parapsychology in which the following sentence had particularly impressed him: "Modern science was built upon the ruins of prelogical, magical, superstitious beliefs."

The patient's associations to the manifest content of the dream were scanty. The main thought that came to his

*Chairman: J. Kappers, Amsterdam.

mind was this: "It is now fifteen years since I started consulting doctors and psychoanalysts. With you I have progressed a lot, but I do not consider myself completely cured. I do hope that my sixteenth year of psychological work will be the final one, the year of my recovery." As you remember, the "machine" of the dream could "complete" an intellectual work (a poem, or an article) if the first fifteen lines had been written. It seems clear, on the basis of the reported life events of this patient, that there were in him some clear representations of destruction, ruins, aggressive drives, etc., along with the ideas of reconstruction, progress, and recovery. The main unsolved point of the dream was the mysterious "machine with sixteen cylinders." The patient had no idea of anything resembling this machine, and could not produce any association to this puzzling dream image.

Two days afterwards, on June 24, the patient bought an issue of Time magazine at a newspaper stand in Rome. The issue was dated June 26, 1972. On the upper left part of page ten there was a photograph, released by the U. S. Defense Department, representing an "aerial view showing damage to petroleum storage tanks near Hanoi." This photograph clearly showed sixteen cylindrical bodies (the petroleum tanks), seen from above and in very bad condition. The photograph was accompanied by an article entitled "Effects of the Bombing." On the right side of the same page, an article on the situation in Northern Ireland had the title "Hints of Peace."

In cases of this sort the relationship between patient and analyst is often relevant to the production of the paranormal dream. I therefore tried very hard to see what could have been my indirect contribution to this paranormal occurrence. The only thing I can say is that during those days I was very preoccupied with the possible repercussions on my patient of the continuous nagging and aggressive behavior of his previous mate. Perhaps the patient, by producing the dream, had wanted to give some pleasure and reassurance not only to himself but also to me; first, by producing a paranormal phenomenon for my sake, and second by pointing out that there can be good perspectives even after years of destruction and fight, and that even in the midst of a war situation there are after all, as Time magazine had hopefully printed, "hints of peace."

POLTERGEIST AND APPARITIONAL HAUNTING PHENOMENA AFFECTING THE FAMILY AND ASSOCIATES OF AN ADOLESCENT GIRL WITH WELL-CONTROLLED EPILEPSY

James F. McHarg (University of Dundee, Scotland)

In November 1971 a fourteen-year-old girl was referred to her local child psychiatry clinic because of poltergeist and apparitional haunting phenomena which were also affecting her whole family and even certain friends. She was living with her father, mother, older sister, and younger sister in a semi-detached (duplex) house in a town in the English Midlands. In a clinical, consultative capacity I was able to spend two days in February and four days in May of 1972 interviewing the patient and others involved at the clinic, and also visiting the home.

It appeared that at the age of nine the girl had begun to suffer from generalized convulsions in which there was a simple visual aura of a colored ball appearing in the right half of the visual field. An abnormal EEG focus had been demonstrated (in the left cerebral hemisphere) and the seizures had been quickly brought under control by medication. Four years later, in 1970, at the age of thirteen, the girl began to have visual experiences of a more organized kind, also starting in the right half of the visual field. It was conjectured that this was related to a brief period of disturbing experimentation, by the family, with the ouija board. At first she saw an old man, who was taken to be her long-deceased paternal grandfather. Then, in 1971, she repeatedly saw a young girl who claimed to have been strangled in 1808 and who wished to be buried in consecrated ground. After the vicar blessed the house this apparition did not return, but it was replaced by others.

Involvement of the rest of the family and of friends began when they witnessed ostensible poltergeist phenomena such as doors and curtains opening and shutting, objects moving, and a cassette tape recorder being repeatedly switched off. A fifteen-year-old boy told me that, while upstairs on his own, he had seen a Wedgwood pot on a cabinet move from one corner to the opposite corner and back again and, before he ran out of the house in fright, he had had the door of the room slammed in his face. Others had seen a pair of scissors on a flat surface moving toward this boy and a cushion whisked away when he sat

down. The older sister had seen toffee-papers rise up a foot or two into the air and then slowly subside again. A teacher in her twenties, whom I interviewed at the vicarage, had allegedly been having plates thrown at her in her own home.

The purpose of these phenomena had been assumed to be to draw attention to apparitions which then began to be seen by others, both singly and collectively. These were not only of dead persons, but also of persons known to be alive. Dogs, bears, birds, and devilish "horny things" were also seen--a coldness was usually experienced in the part of the body nearest to the apparition. Shared apparitions sometimes appeared, to different observers, to be differently dressed. Individuals had even occasionally seen themselves, a phenomenon known as autoscopy. A later-reported phenomenon consisted of the whole family being impersonated, so that friends spoke of seeing them in places where they had not been.

There was a particularly frightening episode during the night of October 9, 1971. Warning of this had enabled the vicar and a colleague experienced with such phenomena to be with the family and immediate friends in order to pray with them. This episode seems to have been characterized mainly by a black magic type of content: the sound of drums and chanting and apparitions of human beings with horns or animal heads. An angel was also seen. None of the apparitions was seen by the vicar, but he observed ostensible demon-possession of others during trance-like states, and one boy spoke in a strange tongue.

It was impossible for me to draw a firm conclusion about this case. A long-sustained hoax, although not impossible, seemed improbable. There was nothing to suggest drug-taking. Heightened suggestibility of the group, with hysterical phenomena incorporating the hallucinatory experiences of the patient, was the obvious explanation. However, I found no actual evidence of this. I was impressed, moreover, by the care the family had taken to avoid publicity. Furthermore, the ostensible psychokinetic phenomena, if they have not been proved, have not been disproved. Finally, the comic quality of a few of the apparitions (e.g., skeletons wearing top hats) greatly puzzled the family itself. Black magic groups were understood to exist in the district and this raised the possibility that the family was telepathically "picking up" the activities of these

groups and even that the groups were deliberately paranormally producing the haunting phenomena. Meanwhile, the apparitions seem to be continuing.

A METHOD FOR MEASURING PK ABILITY WITH ENZYMES

Herman K. Kief* (Amsterdam)

There have been reports in the parapsychological literature of a number of methods by which investigators have tried to measure objectively the psychokinetic influence of an individual on enzymes or enzyme systems. These methods have been in my opinion too elaborate and too vulnerable to various kinds of artifact. For this reason, I decided to use a very common, reasonably standardized biochemical instrument which could render the results more reliable and perhaps more convincing as evidence for the PK hypothesis.

I used the so-called manometrical method of Warburg. Within a special piston-flask I put a standard quantity of the enzyme carbondioxydeanhydratase, together with substrate, activator, buffer, and so on. I chose this enzyme for two reasons: it can be obtained in a highly purified state and it gives a very well standardized reaction velocity. The chemical action of the enzyme in this apparatus consists of converting water and carbon dioxide into H_2CO_3. The trick of the Warburg technique is that the piston flask is directly connected with a manometer, which measures the pressure of the gas in the flask and thus the extent of the chemical reaction. The flask itself is suspended in water kept at a constant temperature of 36 degrees Centigrade by a thermostat. In the Warburg apparatus there are at least seven of these piston flask-manometer systems, often more.

If we record a curve of the carbon dioxide absorption by the enzyme over time (which is very easily and very exactly measured by reading the manometers every five or ten minutes), we get an S-shaped curve which starts out flat, then rises sharply, and finally flattens out again. In

*Presented by J. Kappers.

the first part of the curve the enzyme is adaptating and taking up molecules of its substrates. At the point where the curve begins to rise the reaction is started efficiently and the carbon dioxide is rapidly absorbed. At the point where the curve flattens out all the carbon dioxide in the closed system has been used and the reaction ends due to lack of substrate. The inclining portion of the curve is usually a straight line and makes a sharp angle with the time axis. This angle can be measured very exactly and is a good parameter of the reaction velocity.

I did a PK experiment using this apparatus, in which a number of flask-manometer systems were randomly divided into two equal groups. All the flasks got exactly the same quantities of enzyme, substrate, activator, buffer, etc. Then one group was "treated" by a subject, while the other group was not. The "treatment" consisted of making "magnetical passes" over the flasks for about two minutes. In this pilot study there was no significant difference between the reaction velocities of treated and untreated enzymes. I plan, however, to continue these experiments using gifted subjects.

PSYCHOKINETIC EFFECTS ON YEAST: AN EXPLORATORY EXPERIMENT

Erlendur Haraldsson† (American Society for Psychical Research, New York) and Thorsteinn Thorsteinsson (University of Iceland, Rejkjavik)

The procedure used in this experiment was as follows: a small amount of yeast (.25 gr.) was put into 300 ml. of a nutritive solution and shaken until homogeneous. This solution was poured into 20 test tubes, each containing 10 ml., which were randomly divided into two groups of 10 test tubes each: one experimental, one control. The experimental test tubes were closed and then were all simultaneously placed in front of a subject who was instructed to try for 10 minutes to increase the growth of the yeast in the

†Presented by Haraldsson. In future, dagger indicates speaker.

solution by psychokinesis. The subject was not allowed to touch the test tubes. Then all 20 tubes were stored in the same place for 24 hours, after which the growth of the yeast was measured in each of the tubes by a light-absorbance colorimeter. An assistant who did not know which of the tubes were experimental and which control made the measurements.

Seven subjects participated in a total of 12 sessions; five subjects had two sessions, two had only one. Each session was conducted as described above, so that there were a total of 240 test tubes used in the experiment, 120 of which were experimental and 120 control. For purposes of analysis, each experimental tube was paired with a control tube used in the same session, and the yeast growth in the two tubes was compared.

The results indicated a psychokinetic effect: in the 120 pairs of test tubes there was more growth in 58 of the experimental tubes than in the corresponding control tubes; in 33 there was less growth; and in 29 pairs the growth was equal. Each session was evaluated by the Wilcoxon Signed-Ranks Test. A transformation into z-scores gave for the combined 12 sessions a z of 2.39 ($P < .02$, two-tailed). Three of the subjects were engaged in healing, two as mental healers and one as a physician. The bulk of the positive scoring was done by these subjects ($z = 3.80$; $P = .00014$, two-tailed), whereas the "non-healers" gave chance results ($z = -.08$).

A PILOT STUDY ON THE RELATIONS BETWEEN PK SCORES AND PERSONALITY VARIABLES

John Mischo and R. Weis (Institut für Grenzgebiete der Psychologie, Freiburg)*

After extensive psychodiagnostic studies of two poltergeist agents, J.M. came to the conclusion that a neurotic personality structure, in addition to a state of intense frustration, seems favorable for the emergence of PK. To

*Presented by Christa Lübke.

test this observation we investigated whether subjects participating in a PK experiment perform better in a state of frustration than in a normal, unfrustrated state. Further, we investigated whether there are relationships between PK performance and personality variables measured by two German questionnaires. PK performance was conducted without frustration (in the control series) or in a presumed state of frustration (in the experimental series). Extent and direction of frustration were rated by two experimenters according to the observable behavior of the subjects. Two further variables were studied: attitude toward PK and the psychological meaning of the color targets used.

Fifty students at Freiburg University volunteered for the experiments. This sample was fairly representative of all Freiburg students as far as sex and field of study were concerned. The PK testing was done with the "psi recorder," an electronic device that produces random sequences of targets. First of all, the subjects rated each of the five color targets according to the impressions the colors made on them. Then the first 250 PK trials in the control series were performed: the subjects tried to influence the random number generator in favor of one of the five color targets. Each color served as target an equal number of trials. In the following recreation pause, the PK attitude questionnaire was filled out. Then the subjects were given a very difficult, but simple-looking problem task. They were told that a solution was expected within 15 minutes. With four exceptions, the subjects had not found the solution after 40 minutes, and the task was stopped. The subjects were now in a state of more or less strong frustration as rated by the experimenters. Immediately after finishing the frustration-inducing task, the experimental PK series (500 trials) was conducted. After this, the experimenters tried to reduce the experimentally-evoked state of frustration. Four to 14 days later, the second 250 PK trials in the control series were performed, and the two personality questionnaires were given. These were the Freiburg Personality Inventory, which assesses a variety of traits, and the ENNR, which measures extraversion, neuroticism, and rigidity.

The PK attitude questionnaire, rating of the target colors, and rating of the subjects' behavior by the experimenters showed no significant relationship to PK scores. Significant correlations were found, however, between PK scores in the experimental series and five personality variables: calmness and sociability were positively related to

PK scores; and depressivity, neuroticism, and inhibition were negatively related. All correlations between personality variables and PK scores in the control series were insignificant. A factor analysis was performed on the personality variables and PK scores. Factor loadings on the first factor showed a similar clustering of traits relating to PK scores in the experimental series. We interpreted these findings as follows: subjects with a neurotic personality structure have, in a state of frustration, a higher probability of achieving negative PK (PK missing). This can perhaps be regarded as support for J. M.'s original hypothesis.

We feel that our study not only contributed to the understanding of PK, but in addition provided a methodological model. We took as our starting point the results of investigations on recurrent spontaneous psychokinesis (poltergeist cases) and used these to generate hypotheses. Through the multivariate methods used in the subsequent experimental study, we succeeded in clarifying some of the complex conditions related to the emergence of PK. The univariate experiments most often conducted in PK research could not have achieved this clarification.

TARGET PREFERENCE AND POSITION EFFECTS IN A PK INVESTIGATION WITH TEN SUBJECTS

Björn J. Steilberg (Study Centre for Experimental Parapsychology [SCEPP], Amsterdam)

A PK test was conducted with ten subjects (five men and five women), using a randomizing chute with a stationary release point, down which four high-precision dice were simultaneously thrown. In each session four people took part: the subject, the experimenter, the assistant experimenter, and a witness. The subject was allowed to determine his first target for a given series by throwing a die; after that a prearranged numerical order was followed, so that each of the six die-faces served as target an equal number of times. The dice were first shaken in a dice cup by the experimenter and then dropped into a receptacle placed at the top of the inclined chute. From the receptacle they fell down the chute over many interposed baffles, so that

they bounced around freely, and came to rest on the bottom of a dice box. Both the assistant experimenter and the witness kept a complete record of every die-face that turned up. While each of the record-takers was in a position to see the dice himself, his records were not visible to the other record-taker.

A run consisted of 24 single die readings. At the end of each run the assistant experimenter and witness both counted the number of hits appearing in the column for that run on the record-sheet, then went on to the next run on the page. Four runs were thrown for each die-face in succession. The results for each die-face were recorded on separate sheets, so that for each session a six-page record resulted, consisting of 24 runs. One session constituted a series.

The subjects were tested under two conditions. In the first condition they were to influence, by conscious effort, the fall of the dice so as to make the specified face turn up. In the second condition they were to influence the dice by visualizing the appropriate face. It was determined in advance that each subject would do three series (72 runs) under each condition. A total of 1440 runs (34, 560 single die readings) were completed by the subjects. When total number of hits was evaluated the experiment did not give evidence of PK. However, there was a significant incline effect between the first and third series ($P < .01$, two-tailed) and the first condition was significantly more negative than the second condition ($P < .01$, two-tailed). Furthermore, there was a strong preference for die-faces 2, 4, and 6, in that scores on this target-group were higher than on the target-group 1, 3, and 5 (CRd = 3.1; $P < .001$, two-tailed). When the two experimental conditions were evaluated separately, the first condition was found to have a significant overall negative deviation ($P < .02$, two-tailed), and an incline effect from the first to third series which had a suggestive P-value of .03. In the second condition the preference for the even target-faces was especially pronounced (CRd = 5.1).

Earlier studies of position effects in PK research have shown that more pronounced position effects were found when the preferred target-faces were used. In this experiment a comparison of the position effects within the run was made when the 1, 3, 5 faces (nonpreferred) were the targets and when the 2, 4, 6 faces (preferred) were used.

When scores on the four segments (six die readings per segment) of the run were compared for each target-group, significant incline effects were found between the first and second segment and between the third and fourth segment in the preferred target-group (CRd = 4.7 and 3.8), and significant decline effects were found between these segment-pairs in the nonpreferred target-group (CRd = 3.8 and 5.7). These incline and decline effects were found in all subjects, which may suggest that the psychological attitudes of the subjects toward the different die-faces affect their PK performance in a consistent way.

PK CONDITIONING

Bert Camstra (Study Centre for Experimental Parapsychology)

R. F. Hefferline some years ago did a study showing that it was possible to condition a voluntarily uncontrollable thumb twitch. This led me to try to condition the presumably voluntarily uncontrollable PK faculty. A pilot study and two main experiments have been done. The general procedure was that the subjects were rewarded every time a certain number was generated by a random number generator. During the session the subject had to listen to pop music, disturbed by heavy white noise. The random number generator was placed in front of the subject. Each time the number 99 was generated from the range 1 to 99, which occurred by chance approximately once in 20 seconds, the subject was reinforced by removing the noise for 10 seconds. Conditioning was presumed to take place if the number of generated numbers before the number 99 appeared dropped from 99, which was mean chance expectation, to a lower level. That is, successful conditioning was shown by an increase in frequency of the generation of the number 99.

The pilot investigation explored the influence of amount of reinforcement, arousal level, and informational set, and yielded a number of hypotheses that were subsequently confirmed in the first main experiment, in which each subject did 60 trials. The first 20 were considered a pretest and the last 20 a posttest. Conditioning was measured as the difference between success rates on the pre- and posttest periods. As I had predicted, subjects

who were asked to concentrate on their task were not conditionable whereas subjects who were not asked to concentrate were, in that they produced significantly increased generation rates of the target number. Subjects who were told that it was a telepathy task (which it was not) were significantly better conditioned than subjects who were told the truth (that it was a PK experiment). Subjects who were instructed to concentrate on their presumed telepathy task were actually best conditioned on the PK task.

On the basis of these promising results a second experiment was done in which a number of modifications were applied. First, the reinforcement level was enhanced by introducing a disturbed video program as well as the music. As a reinforcement, the disturbance on the TV screen as well as in the music was removed. Second, ten-trial pre- and posttests were given in which the subjects were not reinforced (in the first experiment these periods had actually been part of the conditioning session). Success, as before, was measured as the difference between pre- and posttest generation rates. Between the pre- and posttests the subjects did 60 conditioning trials with reinforcement. Finally, a control group was introduced in which the procedure was identical except that there was no subject present.

The results of this second experiment were disappointing. Virtually no significant influences of the factors studied were obtained, nor was there any overall conditioning effect. The control group also gave chance results. However, in the experimental group similar trends to those found in the previous, significant experiment were seen. I hypothesized that the lack of significance was due to the fact that the second experiment yielded four times as much error variance as the first. When I discarded the ten-trial non-reinforced pre- and posttests and used instead the first and last 20 trials of the conditioning period, as had been done in the first experiment, the error variance was reduced to one and one-half times that in the first experiment. Thus the fact that in the second experiment pre- and posttest periods consisted of only half as many trials as in the first, seems to have contributed a major part of the error variance.

Moreover, the introduction of the video program did not bring the improvement that was expected. The reason for this is not clear. In the first experiment the random number generator was directly in front of the subject, while

in the second experiment its numbers were projected on a corner of the TV screen, while the generator itself was in an adjoining room. The difference in distance or in psychological situation may have been influential.

TESTING THE PSI ASSOCIATION HYPOTHESIS

Dean Lucas and W. G. Roll† (Psychical Research Foundation, Durham, N. C.)

Many parapsychological writers have suggested that it is possible for a percipient to respond to the mental images associated with a target object. This notion is consistent with an extension of the psychological laws of association to a more general parapsychological setting, an idea previously suggested by W. G. R. In this experiment we asked whether a percipient could respond correctly to targets which were physically identical and distinguishable only by their associated mental images. Our tentative answer was yes.

The targets consisted of four decks containing 10 cards each. One deck had pictures of people on the cards, five males and five females. The second deck had descriptions of the same pictures written lightly in pencil, one on each card. The third deck consisted of cards onto which five male and five female images had been previously hallucinated by a hypnotized person not otherwise connected with the study. The fourth deck contained 10 blank cards with no pictures, writings, or images. All cards were concealed in black envelopes.

The percipient, Miss DeVita Hartis, was handed each card in turn and asked to guess its sex. She was seated behind a screen, having only tactile access to the envelopes. Twelve runs were performed, four a day for three days. A run consisted of making one guess at each card in each deck. During each run the four decks and the cards within each deck were presented in random order. Between runs the order of the cards was re-randomized. All decks except the one containing blank cards were analyzed for hits in the usual manner. Significant positive scoring was found for the deck associated with mental images ($P<.03$),

and for the three non-blank decks combined ($P < .01$). There were no significant differences between the three decks. This result is consistent with the theory that a percipient can respond to targets on the basis of their associated "images" alone.

We also analyzed guessing patterns on all four decks and compared patterns within each deck for the two halves of the experiment. This was done in two ways: by an analysis of distribution of strings of like calls on the same card, and by a statistical method we call symbol frequency. A low string distribution indicates a tendency to call the same symbol many times in succession on many of the cards in a given deck. A high symbol frequency reflects a trend toward calling some symbol unusually often, although not necessarily in succession, on many of the cards in a given deck. These are both measures of something similar to the focusing effect found in research with Pavel Stepanek, the difference being that we did not compare scores on individual cards but averaged these scores and made comparisons between decks.

Contrary to expectation, the overall string distribution on the four decks was significantly high ($P < .02$), suggesting a tendency not to call the same symbol in succession on a given card. The "image" deck had significantly high symbol frequency for the first half of the experiment ($P < .02$), and was significantly different in symbol frequency from both the deck with pictures ($P < .01$) and the blank deck ($P < .02$). There were no significant differences between the decks for the second half of the experiment. We were not able to draw clear-cut conclusions from these data. It appeared, however, that in the first half of the experiment guessing patterns were altered by the presence or absence of "images" associated with the target material.

A WRITTEN ACADEMIC EXAM AS A DISGUISED TEST OF CLAIRVOYANCE

Martin Johnson (University of Utrecht)

It is often assumed that one of the limitations of testing ESP in a laboratory setting is that the situation is

so very different from one involving spontaneous phenomena. In an attempt to overcome this limitation, I have designed a new method of testing ESP in a compelling real-life context under conditions characterized by high motivation and by the fact that the subjects did not know that they were taking part in a psi experiment. Three studies were done using this method between May 1971 and May 1972. In all, 107 subjects took part. The ESP test was embedded in a written academic exam given at the end of a course in clinical interviewing at Lund University in Sweden. The subjects were not aware of the ESP component; their attention was directed at the exam questions themselves, and they were highly motivated to answer them correctly.

The ESP targets were typed answers to the exam questions, located under the sheets on which the questions were written, spatially corresponding to the areas on the sheets where the subjects were to write their answers. In the first two studies, these answers were correct; in Study III they were incorrect. In all three studies the exam was composed of eight questions; questions 1 to 4 were placed on one sheet, and questions 5 to 8 on the other. These two sheets were glued onto the front and back covers of an envelope, shielded with aluminum foil, into which the targets were inserted. On each test-set, four of the eight questions had target-answers inserted into the envelope. The assignment of the four ESP targets was made according to a strict random procedure. A special instruction was given that misleadingly explained the peculiar appearance of the test-set.

I evaluated the answers to the exam questions blindly in all three studies, but in Studies I and III an independent co-rater also took part. Rating scales were used (based on detailed manuals worked out in advance) to evaluate the "correctness" or "quality" of the answers. These ratings were shown to be reliable. For each test-set (or subject) the total sum of ratings on target-related questions was compared with the total sum of ratings on non-target-related questions. In Studies I and II, in which the target-answers were correct, it was hypothesized that the total for each subject should be higher on the target-related than on the non-target-related questions. In Study III, in which the target-answers contained misleading information, the reverse hypothesis was made. These hypotheses were substantiated in all three studies. In Study I the outcome was significant at the .02 level; in Study II it was marginally

significant ($P < .045$); in Study III it was extremely significant ($P < .005$). One-tailed tests were used.

This method offers several new and interesting features. In addition to the advantages mentioned earlier, it avoids the problems inherent in a forced-choice technique, and thus introduces spontaneity into the situation. Furthermore, this free response method and rating technique should not be affected by already existing response preference patterns in the subjects or by the development of such patterns during the testing procedure. There are good reasons for thinking that such a response bias (psychologically based non-random behavior) could exert a cancelling-out effect on an operating "weak" psi process.

Research Briefs Session II*

THE DETECTION OF FRAUD IN A CASE OF ALLEGED DERMO-OPTIC PERCEPTION

Rex G. Stanford†, J. G. Pratt, and Ian Stevenson
(University of Virginia, Charlottesville)

We investigated claims of "finger vision" on the part of a 12-year-old girl (our pseudonym: Alice Wondereyes) who had received considerable publicity because of her alleged ability. We wish to point out to potential investigators the kinds of evidence for fraud one can easily look for and can often detect in cases of this kind. The claim made by and for Alice is that she can "see" printed material--pictures, type, etc. --through her fingertips. The typical character of her performance is to have someone "hold shut" her eyelids with one or more fingers while she seems to read material held in front of her at approximately the same time as she runs her fingertips over it. She has claimed similarly to read material turned away from her. She also sometimes reads very short words or a few letters of a word when her arms are placed inside an illuminated box through round holes with rubber sleeves attached to the holes extending the length of her arms, but not secured tightly around the arms.

*Chairman: Martin Johnson, University of Utrecht.

In the course of three one-day investigations, we gradually were forced to the conclusion that the performance was invariably accomplished through Alice's eyes rather than through her fingers. The evidence we obtained to this effect consists not only of Alice's failure to perform under really "tight" conditions and her successful performance under "weak" conditions, but, much more importantly, the specific character of her success under whatever conditions it occurred. The most important evidence can be summarized as follows: (1) Alice's degree and rapidity of success in a given condition seemed to be directly proportional to the probable ease of obtaining visual cues. (2) In all the performances we observed, the nature of the information Alice obtained, where she obtained it on the printed page, and the specific conditions of obtaining it made ordinary visual perception the likely explanation of her success. (3) Specific observations of her performance showed that when there were obvious potential peekholes provided by lapses in the finger-hold over her eyes or in some other shielding device, Alice either moved the reading material to a position where the advantage of such holes could be had or simply read only what would obviously already be on a line of sight through such holes.

(4) Alice generally reported information on a page when she did not have her fingers over it, and, more importantly, showed a tendency to read the lines while she was running her finger underneath them. (5) Alice, on pages consisting of numerous lines of type, often skipped a line in her reading and, even though she sometimes made efforts to do so, was often unable to get back to the lines she had skipped. (6) Often, when she was making a change in the object she was reading on the page, she would suddenly and very quickly move her finger entirely across the page (without touching) and point to, or put her hand on, a new object (e.g., another small picture) and then identify it. She acted as though she had previously caught a glimpse of it on the page even though her hand (and thus her purportedly dermo-optic perceptive fingers) had never touched it.

(7) Alice was repeatedly observed to try to cover up potential evidence of when and how she might have used visual perception. In general, she waited until "slippages" of conditions had passed and then reported what she had seen. (8) Often we observed Alice taking actions which strongly suggested she was motivated to increase the ease

of visual perception in the experiment. (9) Alice showed an obvious decline of morale and lack of willingness to try to perform (or willingness to "try" for only a very brief period) when conditions would certainly have ruled out visual perception. (10) The performance was reminiscent of visual perception rather than ESP. She reported essentially complete and accurate information at all times and showed none of the partial, gradually developing, or distorted images characteristic of ESP.

AN INVESTIGATION OF RANDOMNESS OF CALLS AND CARDS IN ESP EXPERIMENTS

John Mischo and Hans-Joachim Grünzig† (Institut für Grenzgebiete der Psychologie)

The evaluation of quantitative ESP experiments is based on the assumption of binomial distribution of hits (number of correspondences between calls and cards). This assumption can mathematically be proven to be correct only if the sequence of calls and/or the sequence of cards is a stochastic, random sequence of the symbols used. Until now, however, there has been no satisfactory summarizing presentation of methods for checking the randomness of sequences of calls and cards in parapsychological research.

Therefore, after thoroughly investigating the possibilities mathematical statistics offer for checking the randomness of sequences of numbers, we have decided on three methods. (A) An analysis of total frequencies to check on whether each of the symbols has the same empirical probability of occurring within the whole sequence; i. e., whether each of the symbols has occurred equally often, taking sampling error into consideration. (B) An analysis of fractional frequencies to check on whether each of the symbols has the same empirical probability of occurring at any place within the run. (C) An analysis of sequences to check on whether after any symbol S_1 each of the symbols has the same empirical probability of occurring (doubles), whether after any combination of symbols S_1S_2 each of the symbols has the same empirical probability of occurring (triples), and so on. The randomness of higher-order

combinations of symbols ($S_1S_2S_3$, $S_1S_2S_3S_4$, etc.) can only be checked if you have a huge number of trials because they occur very rarely and so their expected frequencies would be low. For the analysis of sequences on triples, for example, you need at least an estimated 12, 500 trials.

The most powerful of these methods is the analysis of sequences; the least powerful is the analysis of total frequencies. By means of these three methods we checked the calls and cards produced in ESP experiments conducted at the Institut für Grenzgebiete der Psychologie in Freiburg. The data were: 12, 750 trials using hand-shuffled "closed" ESP card decks done by 13 high-scoring subjects, 12, 750 trials using the same type of deck done by 13 randomly-scoring subjects, 11, 250 trials (13 subjects) using hand-shuffled "open" ESP card decks, 5, 700 trials (6 subjects) using Random Number Generator I, and 28, 800 trials (6 subjects) using Random Number Generator II.

The investigation of the calls showed that by all three methods highly significant non-random patterns occurred. Subjects were not able to call sequences of random numbers. When symbols were written on an ESP record sheet patterns emerged which varied from subject to subject. This held both for total frequencies and for sequential dependencies. When the plane display panel of Random Number Generator II was used quite different call patterns were observed. Among the five positions on the plane surface, subjects chose the middle positions more often than the outer ones. When sequential dependencies were analyzed, we found that subjects tended to a spatial symmetry on the middle positions and to an avoidance of repeating the outer right position.

Other findings emerged from the analysis of calls. All subjects displayed a tendency to show call patterns at the beginning of a run which disappeared later on in the run. Subjects with random scores consistently avoided making the same call twice. Subjects with extrachance results, on the other hand, were much more flexible in their call patterns. It seems as if in the randomly-scoring subjects the process of extrasensory perception is in competition with quite rigid, individual call habits which work as a suppressor of ESP.

The investigation of cards was performed on the same data using the same three methods. Target sequences of hand-shuffled "closed" ESP card decks turned out to have highly significant deviations from randomness using both the

analysis of fractional frequencies and the analysis of sequences. Only when high-scoring subjects were used did these decks show random fractional frequencies. Thus it might be assumed that cards used with "good" subjects were more thoroughly shuffled than other cards, and that the experimenters tried to change their shuffling habits from run to run. But even these efforts could not prevent the occurrence of highly significant sequential dependencies. Target sequences of hand-shuffled "open" card decks were acceptably random in terms of fractional frequencies, but were significantly non-random in terms of sequential dependencies. Thus it appears that hand-shuffling is insufficient to produce random sequences. In light of this, previous successful experiments using this technique should be re-examined and their results perhaps revised.

Random Number Generator I produced total frequencies with no significant deviations from the expected frequencies. The analysis of fractional frequencies, however, revealed severe fluctuations of frequencies within the run, with in some cases extremely high non-chance probabilities. Furthermore, the analysis of sequences showed significant triple-combinations. Consequently, this machine must be regarded as only restrictedly usable for ESP experiments. Only Random Number Generator II survived all our tests in a satisfactory way and turned out to be reliable for obtaining random sequences.

MEDITATION PRIOR TO THE ESP TASK: AN EEG STUDY WITH AN OUTSTANDING ESP SUBJECT

Rex G. Stanford† and John Palmer (University of Virginia)

In this study we worked with Malcolm Bessent, an English sensitive who has been a successful subject in a number of published studies. During one week Mr. Bessent gave psychic readings of 20 persons. The target persons were read in an order determined by a random permutation of their names. The target person was not present during the reading, and Mr. Bessent did not know the person's name or sex.

Mr. Bessent read the persons during a series of

seven sessions in each of which he typically read three persons; only in the first session did he read two persons. Prior to each session he meditated with eyes shut for about 15-20 minutes while monopolar right occipital EEG recording was made, referenced to the contralateral earlobe. This recording continued throughout each session. The raw output of the EEG amplifier was processed through an electronic filter which triggered an oscillograph (and passed the EEG signal) only when frequencies within the alpha band and above a certain amplitude occurred. With this filtered record, alpha frequency and alpha percentage could readily be calculated. To date the EEG record has been scored only for alpha frequency (in Hz). This measure could be derived for the presession meditation period but not for the readings, since Mr. Bessent wished to keep his eyes open during the readings and was allowed to do so.

The ESP score for each reading was obtained as follows. After the tapes of each reading were transcribed, each target person received a booklet consisting of a copy of his own reading and a copy of each of three other readings randomly assigned to him. He was blind as to which reading was his own. The position of the target person's reading in his booklet was counterbalanced over subjects. The target person rated each of the four readings on a 21-point scale as to how well and uniquely it seemed to apply to him. The ESP score for each reading was derived by computing the difference between the rating given his own reading and the mean of his ratings for the other three (control) readings.

Mr. Bessent's overall ESP performance was slightly negative and nonsignificant (Wilcoxon matched-pairs signed-ranks test). The EEG-ESP analysis was based upon mean alpha frequency during the meditation period prior to each of the seven sessions. Thus there were seven groups of ESP scores, one group from each session, each of which was associated with a given mean alpha frequency for the meditation period preceding that session. The sessions were divided at the median into two groups composed of high and low mean-alpha-frequency measures; the low-frequency group (9.13 to 9.44 Hz) consisted of nine target persons, and the high-frequency group (9.45 to 9.65 Hz) consisted of 11 target persons. When these groups were compared for mean ESP score, no significant difference was evident (Mann-Whitney U test).

It was, however, apparent that almost all the large

differences between target rating and average paired control rating (regardless of direction) were contributed by the high-frequency group. Sessions in the high-frequency group thus contributed almost all the marked deviation from chance. A post hoc analysis showed that this effect was significant ($F = 7.78$, $df = 10$ and 8; $P < .005$). This P-value may, however, overestimate the significance of the finding since it is based upon a post hoc analysis.

The high-variance effect related to high alpha frequency in the meditation period preceding the session, if a reliable effect for Mr. Bessent, may be related to the findings of Rogers and Carpenter that variance effects are associated with mood. Mr. Bessent's mood may have affected both alpha frequency during meditation and the deviation of ESP scores. Although no formal measures of Mr. Bessent's mood were made, we believed we noticed definite changes in it which seemed to be correlated with alpha frequency during a given session.

PSYCHIC PHENOMENA RELATED TO LAW

Edward Haaxma (Utrecht)

I should like to discuss an area of study which lies somewhat outside the focus of this convention in that it does not have the purpose of explaining the origin and nature of psychic phenomena in a natural scientific way; instead, this area considers psychic phenomena in their juridical relevancy. I feel that a study of this topic would be a useful component of a multidisciplinary approach to psychic phenomena. Such an approach may hopefully lead to major advances in a field which has, in the past, suffered from inadequate theories and inconclusive research.

My aims in undertaking this research are, first, to explain the attitude of the clerical and governmental authorities toward psychic phenomena with reference to the most important texts of continental European law, in particular Dutch law, from the medieval period until the present day. Second, I should like to demonstrate a possible relation between law and psychic phenomena starting from the origin of law, with the aid of the findings of juridical philosophy and sociology.

In elucidation of these aims, I would like to point out the following. As regards the first aim, a superficial reading of a number of compendiums of law shows that many sections relate to people with extrasensory perception, acting as unregistered medical practitioners. We may presume that the lawmaking authorities, seeing themselves as guardians of the public interest, made these sections, often with penal character, in view of religious and ethical considerations. They felt that the manifestation of parapsychological phenomena, whether or not through the intermediary agency of people with extrasensory perception, might trouble the populace and thus form a threat to law. The documents bearing on witch-trials support this contention.

As regards the second aim, we have at our disposal much information about the origin and nature of law. It appears that in ancient times there was a strong connection between law and religion, which was personified, for example, in the role of the priest-judge. In my opinion, a connection between law and psychic phenomena also exists in the area of religious practices. Psychic phenomena frequently take place during religious ceremonies of primitive cultures and of non-Western religious groups. On such occasions, the conduct of the participants may give the authorities cause to take action to maintain law and order.

ESP SCORING AS PREDICTED FROM FOUR DEFINITIONS OF THE SHEEP-GOAT VARIABLE

John Palmer (University of Virginia)

Fifty-nine students from two college psychology classes were given a standard precognition test followed by a 22-item scale developed by me to measure various aspects of belief in and attitude toward ESP. The psychology classes were taught by Dr. Gertrude Schmeidler at the City College of the City University of New York. In each class, ESP tests were administered during two class sessions, but only the results for the first session completed by each subject were included in the analyses reported below. In each session, subjects completed 100 trials using the standard ESP symbols. Separate target orders for each subject were obtained from a random number table after the test

had been administered. Four scores were derived from the attitude scale, each reflecting one of the four definitions of the sheep-goat variable I had found in the process of doing a recent review of the literature. The questions representing each definition or criterion are listed below, preceded by their number in the attitude questionnaire. Each question was scored on a five-point scale.

Criterion 1 (belief in ESP in the test situation): (6) Do you believe that ESP can be demonstrated by card tests (e.g., by guessing the suits of playing cards)? (20) Do you believe that ESP can be demonstrated by the average subject under the conditions of the experiment you are participating in? Criterion 2 (belief in ESP in the abstract): (1) Do you believe it is possible to "read" another person's thoughts by means of ESP? (2) Do you believe that it is possible to acquire knowledge directly about an objective event by means of ESP? (3) Do you believe that it is possible to predict the future by means of ESP?

Criterion 3 (belief that one has had ESP experiences in the past): (15) Have you ever had personal experiences which you thought at the time might be attributable to ESP (e.g., thinking of a person you haven't heard from for a long time and immediately thereafter receiving a phone call from him or her)? (16) How confident are you now that at least one of your experiences was a genuine case of ESP? (If you answered "never" to the preceding question, omit this question.) Criterion 4 (belief in one's own success in the ESP test): (21) Do you believe you will demonstrate or have demonstrated ESP in this experiment? (22) What specific kind of score do you expect on the ESP test you have just taken or are about to take?

Subjects' scores for each criterion were computed by adding their scores on the items defining that criterion. In keeping with the analyses compiled in my literature review, sheep and goats were divided at as close to the median of the combined groups as possible for criteria 2, 3, and 4. Ideally, only subjects at the extreme negative end of the scale on items 6 and 20 should be considered goats by criterion 1. However, only one subject met the standard. It was therefore decided prior to analysis of the data to include the bottom one-third of the distribution in the goat category; this gives an even more extreme ratio of sheep to goats than that reported by Schmeidler and McConnell using a similar criterion.

A significant sheep-goat difference in the predicted direction (sheep scoring higher) was obtained only for criterion 1 ($t = 2.05$; $P < .025$, one-tailed). Goats scored significantly below chance ($t = 2.72$; $P < .01$, one-tailed). Items 6 and 20 contributed about equally to the effect, as did the two psychology classes. The most interesting aspect of the data, however, is the reversal of the sheep-goat difference for the other criteria. In an article based on my literature review, I suggested that criteria 3 and 4 seem to be less successful in separating sheep and goats in the predicted direction than criteria 1 and 2, and this trend is borne out in the present data. However, the reversal with criterion 2 is puzzling. It cannot be explained by the different cutoff points used for criteria 1 and 2, because when the sample was divided into thirds, midrange subjects scored like sheep on both criteria.

I am presenting these results mainly to illustrate the point that how one defines the sheep-goat variable can dramatically affect the results obtained. The data also lend support to Schmeidler's original notion that belief in ESP in the experimental context is the best way to define the sheep-goat variable. If so, this refinement should be noted in attempts to provide a theoretical explanation of the sheep-goat effect.

EXPERIMENTER EFFECTS IN ESP RESEARCH

Charles Honorton†, Margaret Ramsey, and Carol Cabibbo
(Maimonides Medical Center, Brooklyn)

It has long been believed that the experimenter-subject interaction is a crucial determinant of success or failure in parapsychological studies. The present study involves experimental manipulation of experimenter-subject interactions as related to performance in an automated precognition task. Thirty-six volunteer subjects participated for one session each in an ESP task consisting of four 50-trial runs on a binary Schmidt random generator with automatic registration of hits and trials. Subjects were randomly assigned to one of two experimental groups. Subjects in the positively-toned group spent 15 minutes informally interacting with the experimenter (either M. R. or C. C.) prior to the

ESP task. The experimenter structured the interaction in a way that could be characterized as "casual," "friendly," and "supportive." Subjects in the negatively-toned group were immediately oriented toward the ESP task. The experimenter structured the interaction in a way that could be characterized as "formal," "unfriendly," and "abrupt." The two experimenters ran an equal number of subjects in each of the two experimental groups.

At the end of the session, subjects responded to a 14-item questionnaire designed to assess their reaction to the session. Each of the 14 items was arranged in the form of a seven-point scale (e.g., "experimenter friendly vs. unfriendly" or "experimenter businesslike vs. casual"). Since mean questionnaire scores for the two groups were significantly different ($t = 2.93$, $df = 34$; $P < .005$), we concluded that the manipulation was successful.

The 18 subjects in the positively-toned group obtained a deviation of +67 (CR = 2.23; $P < .02$, one-tailed). The 18 subjects in the negatively-toned group obtained a deviation of -79 (CR = -2.63; $P < .005$, one-tailed). The difference in mean number of hits per subject between groups was 8.11. This difference was significant ($t = 3.62$, $df = 34$; $P < .001$, one-tailed). Spearman-Brown reliabilities (first 100 trials correlated with second 100 trials) were also significant, indicating that scoring levels were consistent across the session. For the positively-toned group the correlation was 0.639 ($df = 16$; $P < .05$); for the negatively-toned group the correlation was 0.830 ($df = 16$; $P < .01$).

The results support the hypothesis that friendly, supportive, informal experimenter-subject interactions are conducive of psi-hitting and that unfriendly, formal interactions are conducive of psi-missing. Utilization of a completely automated ESP testing device, with automatic registration of hits and trials, rules out the often-raised suspicion of critics that such experimenter effects are attributable to nonparapsychological factors.

A NEW PROCEDURE FOR DIRECTLY ENTERING AND PROCESSING RESULTS OF A DOUBLE-EFFECT TEST WITH A COMPUTER

Yvonne Duplessis (Paris)

For several years my colleagues and I have been engaged in research using a "double-effect" test, originated by René Warcollier. This telepathic test utilizes a dual-aspect target situation consisting of five colors and five die-faces; the results can be analyzed in terms not only of global hits but also of partial hits on color or die-face alone. We have tested single subjects and groups of subjects with this procedure, and have found it useful in training subjects. Each response the subject makes is written on a five-by-five matrix with the die-faces as one axis and the colors as the other axis. The subject selects one of the 25 squares of the matrix as his call; 25 calls (25 matrices) constitute a run.

In February 1972, an engineer and data-processing consultant working with our team realized that this five-by-five matrix format could be imposed on a standard IBM punched card. Subjects could make their calls by punching their responses directly onto the cards, which could then be analyzed by computer, thus eliminating much clerical work and increasing coding accuracy. Five matrices (five trials) are written on each IBM card, so that a standard run of 25 trials occupies five cards. The subject uses a small Port-a-Punch machine to punch the appropriate hole in each matrix. The numbers one to five (representing die-faces) are the vertical axis of the matrix, which is placed such that these numbers correspond with the same numbers printed on a standard IBM card. The colors are blue, green, yellow, red, and black, listed in that order (prism order) on the horizontal axis. Each matrix is separated from the others by several columns on the card, so that it can be easily punched and read.

With this standardized recording format, variations in target materials are possible. The numbers one to five, instead of representing die-faces, can be codified ESP symbols, so that colored ESP cards can be used as targets. Another advantage of using punched cards is that during the training period the results can be immediately checked by superimposing the sender's (target) and receiver's (call) cards.

Using this procedure for testing subjects, we have found that it has several additional features which make it greatly superior to the earlier method. (1) Punching produces fewer auditory cues than writing, a great advantage in a telepathic paradigm. (2) The act of punching becomes an automatism, reducing the conscious effort required of the subjects. This is especially true with regard to the colors, since the prism order in which they are listed is learned in childhood. In fact, color hits (as well as global hits) have increased since we began using this procedure. (3) The automatic nature of the response implies also that it is faster to punch than to write the calls. In one case the receiving response time of a subject has been reduced from 20 to 10 seconds per call. Thus some subjects receive ESP impressions by what we might call "automatic punching"; for those who visualize the targets it helps them to concentrate on their visualization, and thus increases their efficiency. (4) As the numbers are written all along the cards, it is easier to "see" by ESP their intersection with the colors columns. This method is suitable for use in clairvoyant and precognitive situations, as well as in the telepathic design we have followed. Because it facilitates fast decoding of information, it could also be applied to long-distance ESP research.

PSYCHOLOGICAL STUDIES OF LALSINGH HARRIBANCE IN RELATION TO THE RESULTS OF TWO FREE VERBAL RESPONSE EXPERIMENTS

Jacqueline Damgaard, W. G. Roll, Robert L. Morris†, Judith Klein, and Muriel Roll (Psychical Research Foundation)

We obtained psychological data on the psychic Lalsingh Harribance using standard instruments in order to evaluate hypotheses concerning the ways in which selected sensitives compare with "normal" subjects, to formulate hypotheses regarding the ways in which Mr. Harribance's personality might interact with the kinds of items he produces in free verbal response experiments, and to formulate hypotheses regarding changes in Mr. Harribance's personality and concomitant changes in his success or failure at various psi tasks.

In April 1969, Dr. John Altrocchi gave Mr. Harribance the Minnesota Multiphasic Personality Inventory (MMPI), the Thematic Apperception Test (TAT), and the Rorschach Test. The MMPI profile described a person comparatively free of psychiatric symptomatology, a person with considerable ego integration, and a person with a number of strengths including responsibility, sincerity, alertness, and loyalty to reality, plus the ability to see things differently from other people, clarity of thinking when needed, and general warmth and responsiveness.

Similarly, the TAT stories showed integration, drama, imagination, and a high degree of emotion and emotional control suggesting a creative person with a strong sense of drama. On the Rorschach Mr. Harribance showed a somewhat poorer degree of personality integration, more disruption in thinking, and less positive, healthy, or creative functioning. In general, however, the tests described a person of considerable ego strength, creativity, and emotional stability. In the summer of 1971 the MMPI, TAT, and Rorschach were repeated by Dr. Elaine Crovitz. This was a period of considerable stress for Mr. Harribance for the following reasons: it had not yet been determined whether he would be able to return to the P. R. F., his visa had expired and he had to go back to Trinidad without knowing whether he would be allowed to return to the U. S., he was in the process of adjusting to a recent marriage, he was somewhat fatigued from his experimentation, and he was disappointed at the lack of progress being made on a book which was to describe his life and research performance.

While the second MMPI profile indicated that Mr. Harribance still appeared energetic, enthusiastic, warm, and expansive to others, there was now evidence in the TAT and Rorschach of extreme underlying anxiety and depressive affect suggestive of a crisis period. Some stubbornness and evasiveness appeared. He was less willing to admit to feelings of anxiety and discomfort and was exerting considerable energy to prevent negative feelings from interfering with ongoing activities and relationships. There was a painful struggle going on between his Christian philosophy of life and more worldly concerns. Feelings of having lost his psychic power and fear of his aggressive impulses were intense. Under deep emotional impact his reality-testing suffered and he tended to misperceive and misjudge situations. In short, he was in considerable pain and in need

of emotional support and rest to restabilize his security.

Mr. Harribance did two free verbal response experiments at times corresponding approximately to the two psychological data periods. In each experiment he "read" 10 males and 10 females from a separate room, generating tape-recorded protocols which were made into questionnaires and analyzed by the Roll and Burdick unweighted and weighted methods. Experiment 1 was done when Mr. Harribance was in a nonstressed state. The overall results, based on target persons' blind ratings, were statistically significant; results for females alone were independently significant. When the data were analyzed by category of items, the most significant categories for the males were Physical Description and Family, while for the females Physical Description alone was significant. A previous analysis had also identified Love Life as a significant category for females.

Experiment 2, done during the more stressful period of the second psychological testing, indicated no ESP in the overall male or female data. The item category analysis also failed to replicate the four significant categories found in Experiment 1. However, the three categories which presented the strongest results (That Day for males and Interests and Problems for females) scored in the negative direction. This suggested that one possible effect of stress for Mr. Harribance may be a tendency for psi-missing in this type of experiment. It was also of interest that, until the stressful period, Mr. Harribance had participated with strong success in card-guessing experiments. Because of this success a series of experiments was planned with J. G. Pratt and W. G. R. as coexperimenters. The tests were conducted during the stressful period, and showed no evidence of ESP.

Research Briefs Session III*

ESP, CREATIVITY, AND HEART RATE VARIABILITY: A NEW METHODOLOGY FOR PROCESS-ORIENTED TOKEN OBJECT STUDIES

Alan D. Price (Wesleyan University, Middletown, Connecticut)

In a complex, exploratory study 24 student volunteers and five women regarded as "psychics" gave free responses

to four objects, while heart rate was recorded with a cardiotachometer. ESP success was studied in relation to heart rate variability, which was measured by computing the average range in heart rate per five-second interval during a session. Typed transcripts of all responses were sent for annotation both to the cooperating owners of the objects and to a reference group of 15 female individuals. The reference individuals each selected an object that they might have contributed for a token object study, and then annotated the transcripts as though the statements were intended for themselves. To increase motivation, they were told that previous research suggested that subjects often get impressions about objects or persons connected with an experiment, but not intended as targets.

The annotations of the reference individuals were intended for use in assessing the likelihood that a statement would be checked by one of the owners, if there were no ESP operating during the experiment. Also, it was planned to make use of a modification of Stevenson's technique for comparing the checking rates on objectively verifiable and subjective statements to detect response biases in the annotations of the owners.

As the first sets of transcripts began to be returned by the 15 reference individuals, more than a year and a half after the subjects had given their responses, it became apparent that the motivating instructions for these individuals had worked perhaps too well, in that they were checking very unusual statements much more frequently than had the owners. At this point it was tentatively concluded that the reference individuals may have become inadvertent precognitive targets for displaced ESP, and thus could not serve as a valid reference group. Consequently, it was decided to regard both owners and reference individuals as target persons, and to use another method for assessing the unusualness of the protocol statements. Accordingly, four judges were trained to rate the unusualness of an item (i.e., the probability that a member of a specified population of women would check that particular statement). In addition, three other judges categorized the protocol statements as either objectively verifiable or subjective. A test of checking bias was developed, and was given to owners, reference individuals, and judges for annotation, as though it were no different from all the other protocols.

[*p. 44] Chairman: Martin Johnson, University of Utrecht.

The ESP score used in this study was unusual and is not one that can be compared to some theoretical standard to test for the probable operation of ESP, nor used to compare groups; however, the data of this study suggest that it may be of value in studying the processes involved in the ESP system. The mean unusualness rating for the four judges for each item was computed, and the annotations of target persons were weighted 0, 1, or 2. Then an item ESP score was computed by multiplying the judges' mean unusualness rating by the largest weight provided by the annotations of the entire group of unbiased target persons. In view of the large number of target persons checking statements and the fact that a statement had only to be checked by one person to get a high score, only very unusual statements were used in determining a subject's mean ESP score.

The conventional Pratt-Birge analysis yielded no evidence of ESP. However, for the psychics the Kendall rank correlation between the new ESP score and mean heart rate variability was a perfect -1.00 ($P = .0083$). The curve fitted to the data points was extraordinarily smooth and was the mirror image of an S-shaped function. The ESP scores of the student volunteers were not related to mean heart rate variability, at least not in any simple manner. However, when the data for both groups were combined and plotted graphically, the resulting distribution had the appearance of two superimposed criss-crossing functions. This suggested that it might be possible to identify a moderator variable which was complicating the relationship between ESP and heart rate variability.

The student volunteers had also taken a test of creative thinking which yielded two scores--secondary creativity and primary creativity. Neither measure of creativity correlated significantly with mean ESP score. However, it still seemed plausible that the relationship between ESP and heart rate variability might be different for groups that were high and low in either secondary or primary creativity. Therefore, student volunteers and psychics were combined and then divided into two groups according to whether their secondary creativity scores were above or below the median. The same procedure was followed for primary creativity scores. The resulting correlations tentatively suggested that secondary creativity may be the moderator variable hypothesized. For high secondary creativity subjects the correlation was -.53, and for low secondary creativity subjects the correlation

was +.39; neither was significant. However, the difference between the two correlations was marginally significant ($P < .05$). The difference in correlations for the primary creativity split was not significant. These findings are, of course, post hoc, but they provide a new lead for clarifying the relationship between ESP and creativity.

AN IMPROVED DELPHI METHOD

John Mihalasky (Newark College of Engineering, New Jersey)

A number of years ago the Rand Corporation developed a method of technological forecasting called the Delphi Method. The essence of this method is to gather together a group of experts and have each of them give a quantitative answer to the question at hand. These answers are then pooled and the median value is chosen as the prognosticated answer. It was noticed that this procedure was similar to some that have been used in tests of precognition and clairvoyance. With the aid of a graduate assistant, research was initiated to determine whether a prescreening of "experts" would improve the efficiency of ESP results. The researcher was also interested in prescreening on the specific type of question to be answered; that is, giving a precognition test to prescreen for precognitive questions, and a clairvoyance test to prescreen for clairvoyant questions.

The hypotheses tested were: (1) On precognition-oriented questions the "experts" who scored above chance on a prescreening precognition test would have a median value closer to the truth and have a narrower distribution of guesses than subjects who performed at or below chance on the prescreening test. (2) The same effect would occur on clairvoyance-oriented questions using a prescreening clairvoyance test.

The "experts" used for these tests were college students, which duplicates the approach used to develop the original Delphi Method. Each participant in the present study was prescreened using a computer-scored, individualized precognition and clairvoyance test in which he was asked to guess 100 digits of random numbers. Those who

scored above 10 (mean chance expectation) became the group of experts. The rest formed the non-expert group. Both groups were then asked 10 precognition and 10 clairvoyance questions. The clairvoyance questions were innocuous items picked from the Almanac, while the precognition questions were based on some event that was to take place in the future. A clairvoyance example is: "What was the divorce rate in the State of Maine in 1908?" A precognition example is: "What is the opening day attendance at the Circus in Madison Square Garden in 1972?" These question types again duplicate the approach used by the Rand Corporation.

To date 97 people have been tested in three sessions (15, 42, and 40 people respectively). Only the clairvoyance questions have been analyzed. The analysis consisted of taking each of the 10 clairvoyance questions separately and assigning for each question a plus to the group whose median guess was closer to the true value, and a minus to the group whose median guess was further away from the true value.

For the analysis of distribution of guesses (size of variance), the group with the smaller spread received the plus sign. In both the median analysis and the variance analysis, it was expected that each of the two groups of clairvoyants would receive five pluses and five minuses by chance alone. When the median and variance data were analyzed by chi square, there was no significant indication that the experts did better than the non-experts. However, when overall scoring direction was examined in the three session-groups of subjects the experts received more pluses than the non-experts in two of the groups for the median analysis, and in all three groups for the variance analysis. It would seem, then, that the hypothesis would be worth further study, both in terms of running additional groups of subjects and in refining the technique for selecting experts and non-experts.

PRECOGNITION IN EVERYDAY LIFE: A PHYSICAL MODEL

Russell Targ (Parapsychology Research Group, Inc., Palo Alto, California)

In the field of parapsychology, data pertaining to three general classes of perception are studied: telepathy, which is mind-to-mind communication; clairvoyance, which is the perception of an event hidden from the ordinary senses; and precognition, which is the perception of a future event that could not be known through rational inference. Of the three, I believe that contemporary physics will find the least trouble in assimilating precognition. In order to understand this, one should reflect on the idea that "causality" is a fact observed in our lives or in the laboratory, and not a law of the universe. The present concern of physics with the possibility of information from the future leaking into the present is shown in a recent article by Benjamin Gal-Or in Science (April 7, 1972). His paper, "The crisis about the origin of irreversibility and time anisotropy," begins with the following paragraph:

> An old crisis in science is receiving renewed attention.... The crisis manifests itself most clearly when attempts are made to provide answers to such fundamental questions as: Is the origin of [time] irreversibility ... local or cosmological? Is it in the laws or in the boundary conditions? What might be the physical interrelationships underlying the expansion of the universe, information theory, and the thermodynamic, electromagnetic, biological, and statistical arrows of time? What is the basic nature of the somewhat mysterious time coordinate system in which the very physical laws are embedded?

I agree with Gal-Or in his conclusion that irreversibility appears to be more "fact-like" than "law-like." This implies that although information usually propagates from the present to the future, we should not be shaken to our foundation if experiments are devised that show that sometimes information is found to be transmitted in the other direction. Indeed, it is the symmetry in the solutions for electromagnetic wave propagation that suggests the model to be presented here to provide a working description of precognition. In constructing this model, I make use of the

advanced potential solution, which is well known in electromagnetic theory.

The reason that I believe physics will find precognition the easiest of the psychic phenomena to describe lies in the fact that in physics everything that is not forbidden occurs, and physics does not forbid the transmission of information from the future to the present. The hypothesis proposed here is that significant events create a perturbation in the space-time in which they occur, and this disturbance propagates forward and, to a small degree, backward in time. Since precognitive phenomena are very rare, this disturbance must die out quite rapidly in the backward direction. The wave traveling in the forward direction is associated with ordinary causality.

Consider a coordinate system with a horizontal axis going from -t to +t, perpendicular to a vertical axis going from -x to +x. Such a system is often used to describe the temporal character of events, where x is the spatial coordinate and t is the time axis. If the origin (point of intersection) is placed at the here-and-now, future events will be at +t and past events at -t. When this system is perturbed at the origin, a large effect will, of course, be created right in the neighborhood of the perturbation. If these axes are superimposed on a pond of water, a rock thrown into the pond at the origin will give rise to ripples which will be discernible at some small distance +t to -t, and +x to -x.

Now if the pond is filled with a material that is more viscous than water, the ripples may be very small indeed. However, it is unreasonable within the framework of this model to imagine a medium so viscous that a point perturbation at the origin would have zero effect at some small distance +t or -t. I am postulating that physical events do not occur instantaneously. Moreover, if one is sufficiently close to an event, even though on the -t side, there is no physical contradiction to its being perceived. The accuracy of the precognitive perception predicted from this model will increase with the subjective "magnitude" of the event for the perceiver, and will decrease with increasing temporal distance from the event.

An example of such an event is the ringing of an alarm clock to awaken one. It is a common occurrence for one to awaken and look with surprise at one's alarm

clock, only to hear it click and then go off. Tart has shown that even sleepers with extremely good time sense can reliably awaken themselves with an accuracy no better than five or 10 minutes away from a desired event. I believe therefore that in this sort of incident, in which the time-lag is only a few seconds, one is awakened by the incipient occurrence of this large, timely, and unpleasant event.

APPLICATION OF THE QUANTUM THEORY OF CONSCIOUSNESS TO THE PROBLEM OF PSI PHENOMENA

Evan Harris Walker (Ballistic Research Laboratories, Aberdeen Proving Ground, Maryland)

I have developed the implications of my Quantum Theory of Consciousness to provide a foundation theory for psi phenomena. This theory is basically an extension of the Copenhagen interpretation of quantum mechanics incorporating "hidden variables." These hidden variables must have the properties of being not physically measurable (though accessible as "associable" quantities), nonlocal (i.e., independent of spatial and temporal coordinates), dimensionally infinite, and so over-constrained in the determination of the collapse of the state vector for a given physical event that separate observers of that event will agree on the resultant state of the system. Thus behind processes of the world that are physically measurable there exist processes that determine the development of events in the physical world. It has been possible to show that these processes provide a basis for the understanding of the existence of consciousness substantiated by certain neurophysiological evidence.

In this theory three data rate processes are shown to exist: the subconscious at about 10^{12} information bits per second, the conscious experience at 10^{8} bits per second, and a third, closely linked to the psychological concept of attention set or the philosophical notion of will, having a data rate (required to maintain the conscious state) of 10^{4} bits per second. This last process provides a data channel to other conscious observers of any event that is not limited by the conditions of the space-time manifold, thus providing the "information channel" on which telepathy, clairvoyance,

and precognition can be understood. The subject (observer) as a physical object in an ESP or PK event is part of a divergent quantum mechanical system having a state vector incorporating various potential system "histories" as component states, among them states in which high scoring occurs. The consciousness of the observer interconnected through the "will" channel by means of the hidden variables to the potential states of the system is the agency that determines the collapse of the state vector and thus brings into being through a willed selection the state involving call-record agreement.

Using the concept of "cues" from mathematical learning theory, in which the constraints on the hidden variables embedded in the conscious experience account for the dilution of significant cues by spurious cues, the distribution of ESP ability in the population can be understood. The use of the consciousness and will information bit rates allows the calculation of the genuine hit probability (Gridgeman's relation) caused by the genuine cue bias in the otherwise stochastic call process. The result is in agreement with existing data (based on 1002 subjects in various group experiments reported in the parapsychological literature).

Psychokinesis is a process in which the hidden variables of the observer determine the collapse of the state vector for a quantum mechanical system with macroscopically diverse potential states. Specifically, it is shown that in the placement experiments of Forwald the magnitude and physical dependence of his results can be deduced from an equation specifying the growth in the indeterminacy of the final position of dropped cubes resulting from the Heisenberg uncertainty relations. The derivation yields the PK displacement in terms of the distance traveled by the cube, the number of bounces, the angular rotation (displacement) per bounce, the quantum mechanical uncertainty in the orientation of the cube after the first bounce, and the impact parameter. The equations relating these variables have been used to calculate 13 data points which agree with Forwald's actual experimental data at an average level of 15 percent (ranging from 3 percent to 50 percent); moreover, the systematic trends evident in the experimental data are reflected in the theoretical data by 11 of the 13 data points. No adjustable coefficient is necessary except as required to account for serial declines.

The theory is consistent with the tenets of modern physics and accounts for the following characteristics of paranormal phenomena as studied in the laboratory situation: spatial independence (as in telepathy); temporal independence (as in precognition); sensor organ independence (as in clairvoyance); ESP and PK ability with complex, unknown, or "math problem" targets; the magnitude of the effect for both ESP and PK; the sporadic nature of the phenomena; the population distribution of ESP ability; the failure of positive results by Forwald in ball moving or placement experiments as predicted from a "psychic force"-type hypothesis; the improvement in PK results for large numbers of dice per trial; and the association of ESP and PK with "mental" processes of sentient beings.

SYMPOSIUM: POTENTIAL USE OF SPACE VEHICLES IN PARAPSYCHOLOGICAL RESEARCH*

WHAT CAN BE DONE IN SPACE EXPERIMENTS THAT CANNOT BE DONE IN EARTH EXPERIMENTS?

Karlis Osis (American Society for Psychical Research)

We at the A. S. P. R. have performed intensive research on the ESP channel and found the earth to be a rather messy place in which to work on this problem. Attenuation of ESP over distance appears to be slight. Much larger distances than are available on earth would allow for a more definitive and more precise test of relationships between ESP and distance. If ESP goes through the earth, it would be important to test the screening power of the earth. Even better would be the combination of the earth and the moon both interposed between the stimuli and the subjects. Would ESP work when the spaceship carrying the stimulus material was in orbit behind the moon and inaccessible to radio communication? In terrestrial experiments we cannot separate the screening of earth from distance effects. Earth's magnetism and also its gravity field might influence the ESP channel. Such influences, if detected, would give important clues to the nature of the energy conveying ESP. On the other hand, these unknown influences might seriously confound other variables in terrestrial experiments.

We surveyed the literature concerning the question: what are the minimal "addressing" cues necessary to enable subjects to reach distant ESP targets such as a space vehicle? We found that acquaintance with a person in the vicinity of the targets is important. The acquaintance can

*Chairman: Joseph H. Rush, Boulder, Colorado.

be "second-hand" or even via a token object. This is indicated by analyses of spontaneous ESP cases and M. Marsh's experiment with response drawings. More needs to be known about the ESP orientation system in order to arrange effective space research. In my opinion the incorporation of ESP experiments into space flights is a must for solving the problem of the ESP channel, which is one of the greatest mysteries of man's existence.

SOME PSI TESTS RELATED TO SPACE FLIGHT

Helmut Schmidt (Institute for Parapsychology, FRNM, Durham, N. C.)

The existence of man-made satellites provides in principle new possibilities for psi experiments which might lead to a better understanding of the space-time properties of psi. I will discuss two such experiments and show that some aspects of these experiments might be studied now, before satellites for psi research are practically available. Assume for our discussion that we have, just above our earth lab, a stationary spacecraft with a binary random number generator on board. Assume furthermore that we have a two-way radio link between the spacecraft and the earth lab. For our purpose an unmanned craft would be most suitable, in order to have the most clearly specified test conditions possible.

Consider first a precognition test in which a subject tries to predict a target sequence which is generated somewhat later at the spaceship and then transmitted to earth. The experimenter first sends at time A a radio signal which reaches the spaceship at time C. This signal triggers the generation of a short sequence of binary random numbers at the spaceship. At time B, before the target generation begins, the subject on earth predicts the sequence to be generated. At time D, when the generation is completed, the number sequence is transmitted to earth, arrives there at time E, and is immediately displayed to the subject and experimenter.

If this experiment yielded a significant correlation between the predicted number sequence and the sequence

actually transmitted to earth the result could be interpreted in three different ways: (1) the subject might have precognized the numbers as they were produced far away on the spacecraft. (2) The subject might have used long-distance PK to force the generator to conform to the previously-predicted generation sequence. (3) Bypassing the long distance, the subject might simply have precognized the target sequence as it would appear on earth at time E. If one assumes either processes (1) or (2) the scoring rate might depend on the spacecraft's distance. If (3) is true, however, one would expect the scoring rate to be independent of distance.

In order to have a "pure" long-distance experiment one would wish to eliminate the possibility of process (3). After the subject has made his guess one could transmit the prediction to the spacecraft. There a computer could evaluate the results and send only the final score back to earth, such that the detailed sequence of generated numbers never appears on earth. Even then, however, a high score might still be ascribed to short-distance precognition. We merely have to assume the action of "conditional" precognition acting in such a way that the subject senses on each trial: "If I make this or that guess, then this will increase or decrease the final score." Thus here no clear distinction between long-distance and short-distance psi is possible.

A pure long-distance psi experiment can be set up, however, in a PK arrangement. The spacecraft carries a continuously-operating binary random number generator, and the produced "heads" and "tails" are continuously transmitted to earth and displayed to the subject. The subject sits in front of the display device and tries to enforce an increased generation rate of, say, heads. Let us consider first the case where the spacecraft is not far from earth, such that the travelling time of the radio waves (and also, probably, any time delay in the PK effect) is negligible. Then one would expect an immediate response on the display device to the subject's PK efforts, and one could study scoring rate as a function of distance. With the help of a radio transmitter such a PK experiment could be done easily on earth over distances of hundreds of miles. Nash and McConnell have reported PK tests which were similar in principle to this design, but involved more modest distances. An extension of this work seems to me particularly important.

If the spacecraft is far from earth such that the radio

signal takes, say, one minute to arrive, then interesting new questions arise concerning the causal and temporal aspects of psi. Assume for example that a subject sits down in front of the feedback display and tries to increase the generation of heads. If he is a good PK subject, we might ask which of the following three possibilities we should now expect: (A) the display begins to show a change in generation rate two minutes after the subject starts his effort. This would be consistent with a hypothesis that it takes the PK signal, like the radio signal, one minute to reach the generator, and that a further minute of travelling time is required for the radio signal to bring the message back to earth. (B) The change in generation rate begins after only one minute. This would correspond to a hypothesis that PK acts instantaneously on the distant generator. (C) The change in generation rate occurs as soon as the subject begins his PK effort. This would support a hypothesis that PK is goal-oriented in the sense that it operates rather independently of the physical setup, and that it is essentially unlimited by conventional space-time boundaries. In this case the generator would have to change its generation rate one minute before the subject begins his conscious PK effort. This might not agree with our conventional concept of causality, but neither does precognition.

This experiment is particularly interesting in that it could give rather basic information on the spatial, temporal, and causal structure of psi. With some modifications, it could be done on earth. Instead of the time delay provided by the travelling time of the radio signal we could easily introduce some other artificial time delay between number generation and display. Then we could study whether PK automatically compensates for the time delay, such that the PK effort leads to an immediate change in the display. Experiments using this design are now in progress. In summary, a psi space experiment may be particularly suitable for studying, theoretically and experimentally, some of the basic features of psi. This does not imply that we should start with space experiments right now. On the contrary, some aspects of the space tests could be explored much more easily on earth. I think these and many other preliminary tests on earth will have to be made before we can even plan meaningful space psi tests.

THE POTENTIAL SIGNIFICANCE OF PSI EXPERIMENTATION INVOLVING SPACECRAFT

Joseph H. Rush (Boulder, Colorado)

Every enlargement of the scope of scientific investigation brings new information, both in the resolution of outstanding questions and in wholly unforeseen discoveries. The exploration of space, still in its infancy, already has resulted in such a broadening of scientific insight in many areas of physical science. It offers a new dimension for experimentation--a dimension that we may confidently expect will be beneficial to parapsychology as well as other fields. The most fundamental question concerning psi phenomena is how they relate to the orderly, causal world of physical science. Is ESP or PK performance subject to underlying physical laws comparable to those that apply to light or gravitation? Or is it, as many believe with good reason, independent of such physical constraints? Two parameters that affect physical phenomena according to simple laws are distance and intervening mass. The meager experimental evidence bearing on these factors in psi performance suggests a decline of success with increasing distance or mass shielding, or both. However, no definitive experiments on these physical aspects have been done. Probably they cannot be done on earth, because of the difficulties Karlis Osis has noted in this symposium.

Experiments in psi communication between earth and a spacecraft would afford opportunities to separate the factors of distance and mass shielding, and to experiment on a vastly greater scale with both. Early this year, for example, an unmanned spacecraft left earth for a flight beyond the solar system; it will take about two years just to reach the orbit of Jupiter. Radio communication is expected at least to that distance, about 500 million miles. If the equipment for a psi experiment were on board, an experiment could proceed for at least two years, with many participants and varying but accurately determinable distances and earth-shielding. It is this type of experiment that we can expect to clarify the question of physical constraints on psi. Like most relativity effects, physical correlates of psi may be very difficult to detect here because earth simply is too small.

Such gains are reasonably predictable. Yet the

greatest rewards for the exploration of new scientific territory usually are those no one suspected. Kepler, with the new dimension afforded by Tycho Brahe's exquisite planetary sightings, tried to discover the true circles in which the planets moved. Instead, he found that they move in ellipses, and revolutionized astronomy. Michelson and Morley, trying to measure the speed of the earth through a hypothetical "ether," found instead a practically null result that confounded Newtonian physics and laid the basis for relativity theory. And Columbus, for that matter, blundered onto America when he was only looking for a better trade route to India. When psi experiments go into space, we may expect our share of such unanticipated discoveries.

Competition for experimental accommodations on spacecraft is intense. The difficulties in arranging for psi experiments in space are obvious and formidable; but two significant factors will assist in developing acceptance of such work by space agencies, apart from the improving professional status of parapsychology. First, communication problems will be intensified as the range and duration of space missions increase. A radio signal takes about forty minutes between earth and Jupiter, and six hours between earth and Pluto! As radio communication becomes steadily more cumbersome and frustrating, interest in ESP as a possible alternative will increase. Second, public interest in space exploration has waned since the first moon landings; but public interest in parapsychological matters is tremendous. Some responsible involvement of psi experiments in the space work will tend to revive public interest and support.

If parapsychology has significant implications for the space programs, the converse also is true. Even a relatively simple space experiment requires a persistent, disciplined team effort by scientists from many specialties over several years. Parapsychologists will find it worthwhile, I believe, to undertake such a concerted effort along the following lines: to clarify experimental designs for space experiments; to develop appropriate experimental "hardware" for use on spacecraft; to carry out extensive preliminary tests of these designs and experiments on earth; and to gain acceptance of such experiments on space missions. Parapsychology and space exploration alike are intimately involved with the most profound relations of life and mind to the physical cosmos. It is peculiarly appropriate that they should pursue these investigations in close association.

5

PSYCHOKINESIS ON MECHANICAL SYSTEMS*

PK MEASUREMENT WITH BALLS IN A 32-CHANNEL MACHINE

W. E. Cox (Institute for Parapsychology, FRNM)

I designed a PK machine to investigate a variety of factors including task complexity, method of scoring, and position effects. In addition, it provided greatly increased testing efficiency in that it used a large number of targets per release. Approximately 1, 200 small steel balls were housed in a glass-covered case, 12 by 18 inches, which could be tilted. In the starting position, all the balls were at rest in one end of the case. When the subject threw a switch, a motor tilted the case and a mechanism released the balls at moderate speed. This made them roll randomly into 16 pairs of narrow chutes, each chute alternately colored red or green, at the other end of the case. Between each pair of red and green chutes was a strip of paper calibrated from 1 to 60. This enabled the experimenter to count the balls in each chute. All balls were returned to the other end of the case when the experimenter tilted the surface backward between releases. After each release, the experimenter noted which color in every pair of chutes had more balls. This constituted a majority-vote type of analysis in that each of the 16 pairs was considered as one trial. Balanced chute-pairs were discarded from the analysis. Chance expectation was eight such hits per 16 trials.

There were four pilot and eight confirmatory series, each consisting of 16 releases (256 trials). The last four confirmatory series were considered to be "re-confirmatory,"

*Chairman: J. Blom, University of Amsterdam.

and were analyzed separately. The experimenter was an assistant, Francine Matas. Most of the subjects were unscreened volunteers. There were two levels of task complexity. In Series III, VII, and XI, the assigned target color was green for eight releases and red for eight releases, in random order. This was the "straight PK" condition. In the nine remaining series the targets were more complex: a strip of paper was laid across the top of the machine showing a random order of the colored letters R and G, eight of each. They indicated the color which was the target for each individual chute-pair. Since it was virtually impossible for anyone to watch the balls and the target order at the same time, success in these nine series would imply a clairvoyant type of PK. The order was changed before every release.

When the series were combined, they showed significant overall PK. The four pilot series (I to IV) had 53.81 percent hits (CR = 2.36; $P < .02$). The first four confirmatory series (V to VIII) had 53.61 percent hits (CR = 2.23; $P = .02$). The four re-confirmatory series (IX to XII) had 54.23 percent hits (CR = 2.94; $P < .003$). The most interesting observation from the pilot series is that the proportion of hits when the target was a single color (Series III) was only 52.50 percent, less than any other pilot series. The other three averaged over 54 percent, indicating that subjects did better with a more complex task. In the confirmatory data, however, the straight PK series (Series VII) showed the second highest scoring rate, and in the re-confirmatory series the straight PK series (Series XI) gave the highest scoring rate of the four.

In addition to the majority vote analysis, a second scoring method was used in Series IV, VIII, and XII, in which the experimenter was her own subject. In these series all balls were counted, each ball constituting a trial. This analysis yielded much lower scoring rates (50.17 percent for Series IV, 50.25 percent for Series VIII, and 50.67 percent for Series XII) than the majority-vote method (54.81 percent, 51.75 percent, and 57.08 percent, respectively). This supported the conclusion I had reached on the basis of previous experiments that a majority-vote analysis is the most efficient. The relevant majority-vote CR's were 1.5, 0.7, and 2.2 for the three series, while by actual ball count CR's of only 0.04, 0.6, and 0.4 were obtained, respectively.

Two additional analyses were made based on a vertical and a horizontal division of the balls in the chutes. The vertical division analysis consisted of considering separately the trials where there were 25 or fewer balls in both chutes of the pair. It had been found in preliminary work that a greater deviation from chance expectation was generally produced by chute-pairs with relatively low numbers of balls in each chute. This was also true in the combined pilot series of the present experiment: the trials with fewer balls gave 60.16 percent majority-vote hits (CR = 3.93), whereas the trials with more balls gave only 49.74 percent hits, which was below chance expectation. The two sets of confirmatory series gave trends in this direction, but the magnitude was not as great (54.62 and 56.33 percent for fewer-ball trials in the confirmatory and re-confirmatory series respectively, while only 52.95 and 51.58 percent for more-ball trials in the two series-groups).

In the horizontal division analysis, the 16 chute-pairs were divided into four groups of four chutes each on the basis of their distance from the center of the target surface. I hypothesized that the four chutes in the middle, being logically the center of the subject's focus, should show the most PK. The chutes adjoining them should be next highest in scoring, the chutes adjoining them lower still, and the outermost chutes lowest of all. To maximize the sensitivity of this analysis, I used only the data from the chute-pairs with 25 or fewer balls. Again, the pilot data supported the hypothesis. The four central chutes had 67.24 percent hits (CR = 2.62), and the more distant chutes showed successively lower scoring rates. The two sets of confirmatory series showed somewhat different patterns: for Series V to VIII, scoring rate again increased from the outer chutes to the chutes adjoining them, and increased again to the chutes adjoining these latter chutes, but the chutes in the middle reversed the trend. They were the only chutes that showed below-chance scoring. Series IX to XII showed all four chute-groups to be above chance, with a scoring rate of 56.00 percent (not significant) in the central chutes. The experimenter was necessarily aware of my intention to do a vertical-division analysis, but knew nothing about the horizontal-division analysis until the completion of the experiment. The horizontal effects were thus independent of conscious experimenter bias.

PK CHANGES IN RECORDINGS OF TEMPERATURE

Gertrude Schmeidler (City College, City University of New York)

Ingo Swann, an artist with marked psychic gifts, asked me to study his apparent ability to change the temperature of a distant object. Subsequent research, described below, showed that he could indeed make recordings of temperature conform to the experimenter's directives of "Hotter" and "Colder." The paper is presented in the hope that the method of measurement which it describes may provide a technique for exploring the source of PK energy and showing how PK operates. The apparatus for measuring temperature change was a four-thermistor bridge, used in conjunction with a Beckman Type R Dynograph. Each thermistor was sensitive to extremely small temperature changes. Pretests showed that the thermistors recorded independently, permitting four simultaneous temperature readings at different locations. My colleague, Larry Lewis, generously monitored the Dynograph and in general took care of apparatus.

The basic procedure consisted of designating one of the thermistors as the target and instructing Ingo (as Mr. Swann prefers to be called) to make this target hotter or colder in a predetermined counterbalanced sequence. The three other thermistors were used to show temperature changes on Ingo's skin and elsewhere in the room. An initial exploratory day gave promising results. It was followed by five formal sessions with Ingo and one session each with two student volunteers. Each formal session was divided into two halves, separated by a rest period of indeterminate length. Each half consisted of 16 periods of 45 seconds' duration: eight rest periods alternating with eight experimental periods in which the subject was instructed to make the target hotter (or colder). The order of instructions for hotter or colder was predetermined in a counterbalanced design: ABBABAAB for the first half, then BAABABBA for the second half. Thus if the first half began with Rest, Hotter, Rest, Colder, Rest, Colder, Rest, Hotter, the second half would begin with Rest, Colder and continue as a mirror image of the first.

Dynograph recordings of temperature were independently scored by myself and one of two blind judges:

Geraldine Schwalb and Betsy Brown. Vertical lines on the Dynograph paper designated the beginning of the instruction and each five-second interval thereafter. An arbitrary baseline was selected for each thermistor readout. Difference from the baseline was recorded at each five-second interval. This yielded for each "Hotter" or "Colder" period one initial reading, eight middle readings, and one terminal reading. The data were evaluated by Abelson and Tukey's method for time series, which essentially consists of analysis of variance with a correction factor.

Evaluation of Ingo's ten half-sessions showed that seven gave significant differences in accordance with instructions (for one, $P < .05$, for one, $P < .01$, for five, $P \leq .001$) and three gave nonsignificant differences. In four of the half-sessions with significant differences the target was sealed into a thermos, at distances of five or 25 feet from Ingo. One of the student volunteers showed no significant difference in either half-session. The other, who had had considerable experience with meditation, showed a significant difference in accordance with instructions in his first half-session ($P < .001$) and a significant difference counter to instructions in his second half-session ($P < .001$).

The most striking response was unscorable because it occurred during a rest period. For Ingo's second session, he and I were in a room insulated by wire mesh, without radiator or window. Each of us sat about five feet from the uninsulated thermistor which had been the target in the first half. Next to it, on the table, stood a thermos into which a thermistor had been sealed. I told Ingo that for the second half he was to try to change the insulated thermistor in the thermos. He was pleased at the new challenge, and as we sat there chatting during the intra-session break, he wondered where within the thermos the thermistor was placed; in his own words, he "probed for it." Lewis, who had been monitoring the Dynograph, had been glancing intermittently at the readout for the thermos, and apparently each time had seen that--as he anticipated--it showed a flat line indicating no temperature change. But during the rest period it showed such marked perturbations that Lewis hurried to the experimental room to find why we had opened the thermos bottle--and found that the thermos was untouched, except by Ingo's psychic probe.

When the target was not insulated by a thermos, a counterhypothesis to PK is that changes in the subject's skin

temperature produced target changes. This counterhypothesis may be rejected because the correlations between target and skin temperature for these half-sessions were so inconsistent: $r = -.04$, $r = +.70$ and $r = -.46$ for Ingo; $r = -.53$ and $r = +.12$ for the student volunteer. Provocative data come from the control thermistors. When a control was a few inches from the target, it sometimes showed a significant negative correlation with target changes and sometimes a significant positive correlation. Similarly, a control placed half way between subject and target might show a significant negative or positive correlation with target changes. This implies that PK affected not only the target but also a field around it, and that the size of the field varied according to psychological rather than physical factors. It further implies that the PK may operate by changing heat patterns between areas, so that as the target area grows warmer another area grows colder, or vice versa. These indications need further research. Perhaps the chief virtue of the method used here is that it permits analysis of energy transfer in PK between the target and the subject's body or the physical surround. I suggest that as a research tool it is therefore superior to a method which records discrete rather than continuous events and which fails to record simultaneous changes in the target and in other areas of interest.

AN ATTEMPT TO INCREASE THE EFFICIENCY OF PK TESTING BY AN INCREASE IN THE GENERATION SPEED

Helmut Schmidt (Institute for Parapsychology, FRNM)

An electronic generator has been built which can generate binary random numbers at high speeds, up to 1000 numbers per second. It operates similarly to a slower one I have used previously except that the basic source of randomness is supplied not by a Geiger counter but by a conventional noise generator. The numbers of "heads" and "tails" obtained are indicated by bright read-out tubes. Two output channels provide short electrical pulses with each generated head or tail, which can serve to drive a feedback display and to record the generated number sequence on an ordinary stereo tape recorder. To test for randomness I operated the machine for several long

sessions in the absence of a subject, and counted the heads and tails as well as relative frequencies of the different possible pairs of consecutively generated numbers. No deviations from randomness were found.

I began using this generator with three long-term objectives in mind. (1) After experiments with the earlier slow (one number per second) generator had repeatedly produced significant deviations from chance, I wondered whether the scoring rate could be maintained at higher generation speeds. An increase in speed without considerable decrease in scoring rate would provide a more efficient production of PK. (2) Short tests comprising a large number of trials would enable me to study possible short bursts of psychic activity. In such tests one can wait until the subject feels particularly well "tuned in" and then complete the run before the "psychic state" has time to change. (3) There is a great gap between the nonexperimental reports of "drastic" spontaneous PK effects and the rather weak statistical demonstrations of PK in the laboratory. Fast PK tests might form a bridge between these two, in that the cumulation of many rapid PK impulses can resemble static object movement in an appropriate feedback display.

Following extensive pilot work, I did an experiment comparing two generation speeds: 30 per second and 300 per second. In order to make the runs psychologically similar, those done at the slower speed comprised 100 numbers and those at the faster speed 1000 numbers. Thus runs of both speeds lasted approximately three seconds. Two types of feedback display were used at each speed. In the acoustical feedback condition, the generated heads and tails were displayed through headphones as clicks to the right and left ear respectively. The subject's goal was to receive an increased number of clicks in a specified ear. The sound volume was kept very low, and the subjects were asked to close their eyes and relax, and to concentrate on the sounds in the target ear. At the lower speed the individual clicks could be easily distinguished. At the higher speed the fluctuations in the click frequencies were noticeable as volume and pitch changes.

In the visual feedback condition, the subject looked at the pen of an ink recorder, which moved such that its deflection was proportional to the momentary difference between number of generated heads and tails. Thus the position

of the pen relative to baseline indicated the momentary cumulative score, and the motion of the pen indicated the momentary scoring rate. The subject tried to deflect the pen in a specified direction. The sensitivity of the pen deflections in the slow and fast runs was adjusted so that the random fluctuations of the pen looked practically identical under the two conditions.

The subjects receiving acoustical feedback had scored high on an initial pretest; different subjects were used for the slow and fast conditions. The subjects receiving visual feedback were three experienced psychics; they all did both slow and fast runs. A total of 200 runs was done in each of the four conditions (slow-visual, slow-acoustical, fast-visual, and fast-acoustical). All four gave CR's significant at the .01 level. The scoring rates and PQs (efficiency with respect to number of trials) were higher in the slow than in the fast condition; this was true for both feedback modes. The slow-visual condition produced exceptionally high scoring (CR = 5.3). In the fast condition, both feedback modes produced approximately the same scoring rate.

Two further analyses were done. The first used a majority-vote method in which each run was considered a trial, and was a hit if there were more hit-trials in it than miss-trials. This increased the scoring rate in all four conditions, as one would expect with statistically independent individual trials. The second analysis assessed efficiency with respect to duration of the test. The results showed that the slower rate was slightly more efficient, but both the faster and slower rates were considerably more efficient than the very slow rate of one per second used in previous investigations.

6

PSYCHOKINESIS ON NATURAL SYSTEMS*

PK ON LIVING TARGETS AS RELATED TO SEX, DISTANCE, AND TIME

Enrique Novillo Pauli, S. J. (Institute for Parapsychology, FRNM)

A phenomenon reported from ancient times is the so-called "green thumb" characteristic, the uncanny ability of an individual to influence the growth of plants. We now recognize this mysterious ability as nothing more profound than a concrete example of psychokinesis. I decided to investigate this ability experimentally in its relation to sex of the subject, distance between subject and target, and time between the PK influence attempt and its application to the plants. In all, 20 experiments have been done.

The subjects used in the experiments were unselected; most were high school children, some were adults. The general procedure in a given experiment was as follows. The targets were 600 Fescue Kentucky grass seeds planted in Baccto potting soil inside Petri dishes. To insure that all pots were given the same amount of water, a millimeter syringe was employed, and to insure use of the same water throughout, all pots were watered from a large jar filled at the beginning of the experiment. All extraneous variables were carefully controlled. The temperature and light in the room were uniform, and the position of the Petri dishes was changed daily. When they were ready, the Petri dishes were assigned by ballot to boys, girls, and control (no influence attempt). They were not assigned to individual subjects, but to the sex-group as a whole. Each group (boys, girls, and control) received 200 seeds. Both the experimenter and his assistants were blind as to target

*Chairman: Mary Rose Barrington, SPR, London.

assignment. The subjects were told to encourage the growth of their plants. In some experiments they were never present in the target room; in others they were present for several minutes under the supervision of either the experimenter, their classroom teacher, or the experimenter's assistant.

Each experiment lasted 10, 12, or 14 days, according to a preestablished schedule. Then each plant was removed from its pot, extended on a record sheet, and measured in millimeters from the seed pod to the tip of the blade. This operation was checked at least twice by different assistants. Each seed sown (i.e., each plant) constituted a trial. The results were analyzed by one-way analysis of variance (boys vs. girls vs. control) and t-tests between pairs of groups, using a two-tailed P-value.

Several experiments dealt primarily with sex differences. The subjects in the first three studies were 9 boys and 19 girls from the sixth grade at Immaculata School in Durham, North Carolina. In these preliminary studies, both boys and girls tried to influence the same Petri dishes. The difference between target and control plants was not significant. Then the sexes were separated and each was assigned its own targets. The results showed significant overall PK (a significant difference between boys plus girls vs. control group) and significant scoring by the boys alone (compared to the control group). Another experiment using older persons (two women and one man) found a significant difference between the women's scores and the man's scores, as well as a significant overall PK score.

Three subsequent studies used as subjects 18 boys and nine girls from the ninth grade at Carr Junior High School in Durham. The sexes always had separate targets. All three studies showed significant PK totals and significant sex differences. Two studies were then done using 12 boys and 11 girls with outstanding IQ's from the eleventh grade at Northern High School in Durham. In neither experiment was there a significant sex difference, although both sexes were significantly different from the control group, indicating PK.

A series of distance experiments was done in which the subjects were in Buenos Aires, Argentina, and the targets in Durham. The subjects were older persons: six men and 16 women. Five experiments were conducted in

which the subjects did not follow my instructions to separate the sexes. The results were not significant. When the separation was performed a significant sex difference emerged and the men showed a significant PK effect. The second experiment with the Northern High School students could also be regarded as a distance experiment, since the subjects and targets were eight miles apart.

Finally, three experiments were conducted to study whether water influenced by PK could have a facilitating effect on plant growth when applied some time later to the plants. A sensitive in Buenos Aires was the subject. A month later, the water he had influenced was used on a set of plants. Other plants received water from a control jar. Only the third experiment yielded a significant difference between the two sets of plants, and the overall results do not indicate that energy remained in the water.

The most striking conclusion to be drawn from these data is that sex differences reliably appear when subjects are asked to influence plant growth, and that only when sex groups are given separate targets does a PK effect emerge. In addition, the direction of the differential scoring between the sexes was not constant; sometimes boys outperformed girls, and sometimes the reverse was true. Experimenter bias is an unlikely explanation for the results; the experimenter was blind, and the results often contradicted his expectation. A further point of interest is that the magnitude of the effect seemed to be independent of the number of subjects participating; in this situation, at least, PK energy, unlike physical energy, was not additive.

RSPK ACTIVITY ASSOCIATED WITH A TEN-YEAR-OLD BOY

John Palmer (University of Virginia)

I investigated reports of alleged recurrent spontaneous psychokinesis (RSPK) activity centering around a ten and one-half-year-old Negro boy living with elderly foster parents in the rural southern U. S. The activity began in early December 1971 with knocking noises coming from the basement of the home. A plumber and electrician were called in but neither could explain these recurrent noises.

Movements of objects began on December 30 and occurred sporadically until January 10, 1972, when the boy went to live with his real mother in a nearby city.

Over 50 incidents were reported by 10 witnesses whom I interviewed, not counting the boy. Several of these incidents were witnessed by two persons, a few by more than two. They occurred in five different locales (both indoors and outdoors), although mostly in the foster parents' home. While some of the phenomena could have been produced by trickery, others apparently could not, assuming the witnesses' reports can be taken at face value. Examples of these more impressive reports are a kitchen table rising on two of its legs three times in succession, a dishpan full of dishes rising from the sink and falling to the floor, heavy chairs turning over simultaneously, a large box of food sailing over the boy's head, a comb floating around inside a pickup truck, and a series of loud "firecracker-like" noises.

With one exception, the witnesses were uneducated. Some gave very coherent, precise, and confident testimony, while others (especially the foster parents) were more cautious and seemed to have trouble recalling precise details. None of the witnesses appeared to be religious fanatics, although the foster parents gave a religious interpretation to the events. The one educated witness was the family physician, a relatively young man who appeared to be alert and critical and had no prior belief in paranormal phenomena. Along with the foster father, he witnessed in his office a door slam shut, a wooden chair turn over, and the door of a medicine cabinet pop open. Although each of these objects was within reach of the boy, the physician (who was facing the boy at all times) was "99 percent certain" that the boy did not move the objects normally.

I was present at home with the boy during parts of three days, but did not witness any convincing phenomena. On one of these days I was accompanied by Ian Stevenson. Once a chair overturned in my presence, but I was not looking in that direction at the time and the boy was positioned such that he could have turned it over normally. I do not claim that this case provides conclusive evidence for RSPK, and recognize that better cases exist in the literature. Although the boy has never been caught at trickery, there is circumstantial evidence that this may have occurred (e.g., long response latencies by the boy to the words "trick" and

"joke" on a word-association test). Nevertheless, I consider the probability that at least some of the phenomena are genuine high enough to justify a search for patterns and a comparison with other cases in the hope of finding clues to a better understanding of RSPK.

Analysis of individual incidents revealed two physical patterns. One was a tendency for objects to move toward the boy as opposed to away from him. The other was a tendency for objects to move from behind the boy when he was walking and from in front of him when he was stationary. The boy has an IQ in the 80's and is doing poorly in school, where he has a behavior problem. The Bender-Gestalt test indicated possible organic brain disease, but this was not confirmed by neurological examination. Although not clinically abnormal, his EEG revealed rather high amplitude resting alpha activity in the occipital area.

A child psychologist at the University of Virginia who examined the boy noted, among other things, temporary lapses of reality contact and a tendency to deny hostile feelings. The latter has been noted in at least two other well-documented poltergeist cases. Denial of hostility was indicated to me independently by my observations of the family interaction. Overt rejection statements by the foster mother were wistfully ignored by the boy, who claimed to be very fond of the foster parents and to want to stay with them. Conscious hostile feelings toward them might be threatening, as he knew they had adopted him when he was abandoned by his real parents. RSPK activity having the effect of upsetting the foster parents might be a means of expressing this hostility without being aware of it or feeling personal responsibility for its consequences. However, this is only a hypothesis.

RADIAL AND TANGENTIAL FORCES IN THE MIAMI POLTERGEIST

W. G. Roll† (Psychical Research Foundation), Donald S. Burdick, and William T. Joines (Duke University)

Some time ago W. G. R. and J. G. Pratt reported an

investigation of RSPK (poltergeist) phenomena in a warehouse in Miami, Florida. The 224 recorded incidents, most of which occurred in January 1967, were associated with a 19-year-old shipping clerk named Julio. The case was unusual in that it was possible to introduce experimental conditions using designated target objects and areas. On nine occasions such objects moved when both investigators had the target area under supervision, apparently ruling out fraud and other ordinary causes. In addition, there were several movements of non-target objects which occurred when the investigators, police officers, or others were present and when it did not seem possible that Julio or anyone else could have caused the incidents normally without detection. For many of these witnessed incidents, details of the movements were known. There were other cases when details of the movements were known but when the situation was not as fully controlled.

To concentrate on objects which would require approximately the same expenditure of energy to move, only objects weighing a pound or less were included in the present analysis. There were 20 such objects in the fully controlled group and 16 among those which were not as well supervised by those present. This paper is a study of some aspects of these 36 movements. Correlation coefficients were computed between all pairs of the following variables: the distance of the movement from the agent; the distance travelled by the object; the radial component of the movement in relation to the agent; the tangential component of the movement in relation to the agent; and the angle of movement in relation to the direction in which the agent was facing. Nineteen movements occurred when all five of these values were known (Series I). There were 17 incidents when the first four values were recorded but when it was not known which way the agent was facing at the time of the incident (Series II). Because of this difference, we analyzed the two sets of data separately.

In both Series I and II there were significant positive correlations between distance from agent and distance travelled, between distance from agent and tangential component, and between distance travelled and tangential component. In both series there were significant negative correlations between distance from agent and radial component,

between tangential component and radial component, and between distance travelled and radial component. The correlation coefficients were highly significant (all but one at $P < .001$). The correlations (in Series I) between the direction the agent was facing and the other components were less significant (ranging from $P = .003$ to $P = .035$). There were positive correlations between the direction the agent was facing and the radial component. There were negative correlations between the direction the agent was facing and distance from him, between direction and tangential component, and between direction and distance travelled. Nearly all the incidents occurred at the back of the agent.

The fact that the correlation coefficients were so significant, and that they were found in two statistically independent collections of data, suggested that the effects were real. A separate analysis of the 20 best controlled incidents drawn from both series gave similar results (except that the correlations between direction and radial component, and direction and distance travelled, declined to insignificance). We may interpret the overall findings as follows: first, close to the agent, object movements were predominantly short, radial, clockwise, outward, and to his right. Second, far from the agent, object movements were predominantly long, tangential, counterclockwise, inward, and to his left.

Because of the complexity of the movements, a simple "vortex field" hypothesis, as suggested in previous RSPK studies, apparently cannot explain them. Instead we offer the following analogy. If a pebble is dropped in a pool of still water, waves emanate radially outward in a manner similar to the waves radiated by an electromagnetic antenna. Two pebbles, displaced in position but striking the water at the same time, will form interference patterns with peaks of reinforcement along some radial lines and nulls of cancellation along others. If the pebbles strike the water at different times, the radial pattern of peaks and nulls is unchanged except for an angular rotation about an origin taken midway between impact points.

This interaction between water waves is similar to the interaction between two (or more) antennae generating electromagnetic waves. For example, two antennae on a plane surface fed by identical signals (like two pebbles striking the water at the same time) will combine their radiated energies within a cigar-shaped beam perpendicular

to the plane surface. However, if one signal is delayed by a specific time (like two pebbles striking the water at different times) the pattern of radiated energy is rotated about the origin, diminished in radial extent, and increased in angular extent; that is, the beam becomes shorter and fatter. Furthermore, by proper adjustment of the time delay the beam can be swept smoothly from the long, skinny position to the short, fat position and back again. This is how a phased-array radar antenna works.

Let us relate this analogy to the two findings listed above. If energy waves are radiated from different positions on the agent's body, we could obtain a radiation pattern in which three beams at the back of the agent occur at different times, varying in shape from cigar-shaped (Beam 1) to intermediate (Beam 2) to short and fat (Beam 3). Beam 1 extends to the right of the agent, Beam 2 in the middle of him, and Beam 3 to his left. Depending upon the time delay of the interacting waves (which may depend upon psychophysiological factors), Beam 1 may occur first, then Beam 2, and then Beam 3, or the time sequence may be reversed. Now, if objects under the influence of Beam 3 are carried with the beam as it becomes Beam 2, this would account for the short, clockwise, outward movements found close to the agent. Conversely, Beam 1, having a greater radial extent, would influence objects further from the agent, and as Beam 1 becomes Beam 2 a typical object movement might be long, counterclockwise, and inward.

Certain similarities and differences between the Miami case and previous cases may be explained by reference to this analogy. For instance, in Miami most of the movements originated in the back of the agent and were counterclockwise. In three previous cases W. G. R. investigated (Seaford, Newark, and Indianapolis), the movements tended to originate at the front of the agent and to be clockwise. This apparent inconsistency disappears if we suppose that a beam of RSPK energy emanates from the skull of the agent, moving from his left to his right. If the poltergeist disturbances are caused by a beam moving from left to right, objects which are disturbed in front of the agent would tend to travel clockwise and objects which are disturbed in the back of the agent would tend to move counterclockwise.

7

PSI DURING DREAMS AND ALTERED STATES*

PSI ASPECTS OF SELF-RECORDED DREAMS

Hans van der Haas (Zoeterwoude, Netherlands)

For the past two years I have consistently recorded my dreams, as soon as I awoke, on a pocket-model tape recorder. In the morning the tape recording is transcribed. I also record impressions I have received immediately before or after sleep, or in a somnolent condition during the day. In numerous cases, after an interval varying from one day to several months, the dreams or impressions were found to refer to occurrences in the past, present, or future, and in this paper I have divided the cases into these three categories. In several cases relationships were found to exist between dreams or impressions and the contents of sittings with mediums, of books, and so on.

The first category consists of dreams referring to the past, usually someone else's past. In this group, two dreams contained names, so that their content could be verified with the people concerned. An example is a dream in which I saw a friend in a room, writing on a slate. He later told me that this had happened in his past, in a Japanese concentration camp. One dream contained no reference to a person, but later on in a conversation the dream content became relevant. Theoretically, there is a possibility of cryptomnesia in these cases, since I knew the people concerned prior to having the dreams. When questioned, however, they could not remember ever having mentioned the details to me.

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*Chairman: J. Cutten, SPR, London.

The second category consists of cases in which the time of the dream and the occurrence of the event coincide, or differ only slightly. Again, these usually refer to events in other people's lives. One example is a dream in which I was walking on a tropical boulevard when I saw a woman who was having a sort of convulsion. I dreamed that a particular friend would know about the incident. When I checked with this friend later on, he told me that he had that same day been walking on a boulevard in Tunis, and had later watched a belly-dancer in a nightclub. Another example in this category involved a deliberately induced daytime impression rather than a dream. I was listening to a radio broadcast announcing that a girl had been lost. I tried to visualize where she might be, and entered a drowsy state. The fairly detailed imagery I received proved to be unrelated to the girl's location, but corresponded strikingly to details given in an article in a newspaper I read the next day. The article was located just below another article describing how the girl's body had been found.

The third category consists of dreams relating to future events, and in these cases the events most often concern myself. In one of them a location in London was specified, which I was able to verify. In another case I dreamed that my deceased wife showed me a white porcelain amulet. A few months later a medium referred to this object during a sitting. I will give a final example illustrating the complexity involved in some dream-event correspondences. I dreamed of a man in a sailboat who was having trouble with some sort of electrical system. The equipment needed to fix it was described in detail; it should include an item consisting of an iron-nickel alloy. Several months later a medium in London told me to visit a particular person in Sweden. This person, in turn, told me to visit another person in the Netherlands. This third person, finally, referred me to a man in the western part of the Netherlands who, when I saw him, told me that he was indeed working on his sailboat, devising an electrical system for investigating the "Jürgenson-voices" on tape recorders, and that he had recently included an iron-nickel alloy item to improve the operation.

TOWARD A METHODOLOGY FOR EXPERIMENTS ON OUT-OF-THE-BODY EXPERIENCES

Karlis Osis (American Society for Psychical Research)

Taking leads from the literature, my colleagues and I are attempting to develop a methodology for experiments aimed at the central hypothesis of out-of-the-body experiences (OOBE's): that there is something in the human being capable of detaching itself from the organism and operating outside the body, perceiving from a point of view outside the body, sometimes causing people in the area to perceive it, and exerting physical effects. This is called the ecsomatic hypothesis. The counterhypothesis, that OOBE's represent traveling fantasies coupled with ESP, is also within the scope of the experiments. It was recognized early by Myers and others that a person experiencing out-of-the-body projection seems to be able to perceive objects and people from the particular point in space where the projector feels himself to be, rather than from the point of view available to the physical eyes. We propose to test the properties of the spatial organization of OOBE perception in different ways.

Several systems of lenses, split-beam screens, prisms, and mirrors will be constructed within a closed stimulus-display box; these optical systems will displace the image of the stimulus picture from its actual place, or superimpose images, or select small fragments from a larger picture. If viewed from the observation windows in the box, the images of the stimuli would thus be observed in different parts of the box and their spatial relationships would be radically distorted. Such systematically manipulated dislocations and distortions would not be expected in the percept if it were organized from the point of view of the subject's body (i.e., if the subject made a kind of ESP "X-ray" of the box) rather than of the observation windows. We will provide methods of varying the optical displacements and distortions of stimulus images so that the subject will not be able to form an expectancy set and organize the perceptual data in accordance with his ideas of the box. Each time a new, unexpected distortion will be presented to the subject. We have already explored the basic ingredients of such a design, and it seems so far to produce effects somewhat consistent with the ecsomatic hypothesis of OOBE's.

Study of the psychophysiology of OOBE's has been pioneered by C. T. Tart. We have found that comparisons of voltage differences in the occipital EEG between normal and OOBE states appear to be most promising. Brain wave frequency analysis is another methodological line that we are developing. The practical aim of studying the psychophysiology of the OOBE state is the possibility of using feedback methods to teach subjects to induce the state. The experimental literature suggests that the ecsomatic projection might at some times be observable to persons in the projection area or that it might interact with the physical surroundings. Our tests will explore possible emission of light in some part of the spectrum from the ecsomatic projection, interference with air ionization potentials, possible capacity for causing air currents or interfering with them, and possible interference with infrared and other light beams.

8

ESP TESTS WITH GIFTED SUBJECTS*

FURTHER CONSIDERATION OF THE STEPANEK FOCUSING EFFECT IN THE LIGHT OF RECENT RESEARCH FINDINGS

H. H. J. Keil† (University of Tasmania) and J. G. Pratt (University of Virginia)

Pavel Stepanek (P. S.) has demonstrated the focusing effect in a number of experiments extending over his career as an ESP subject. The focusing effect occurs when a subject identifies one or more objects out of a number of similar objects which are repeatedly presented to him. The identification is revealed by nonrandom sequential responses to the objects concerned, while only random responses may be given to others used in the same test. In a large number of experiments conducted since 1966, the findings have shown that P. S. 's differential patterned responses were linked with the covers in which the white- and green-sided target cards were concealed rather than with the hidden colors. Highly significant patterned responses that were first shown in relation to the covers presented under sensory conditions later continued when those covers were presented as ESP targets hidden in larger containers.

The investigations of the focusing effect from 1966 through 1969 always involved small sets of objects, not more than ten and often fewer. This fact led naturally to the development of the hypothesis that P. S. was identifying some of the individual objects on a sensory basis and learning to associate the response "white" or "green" with each one; and that these associative habits became the vehicles for his ESP responses when those objects were

*Chairman: D. J. West, SPR, London.

concealed. A study done by Rex Stanford and J. G. P. correlated the responses given to a particular set of objects under both sensory and concealed conditions over a number of series, and showed that the patterns were highly correlated, a finding that was consistent with the above hypothesis.

Then J. G. P. and Champe Ransom reported that P. S. showed highly consistent patterning of responses when a set of 60 objects was used under sensory conditions. When the 30 objects with the most striking responses were selected and used under concealed conditions, a focusing effect was found but the responses showed no correlation with the ones given earlier to those same objects. Nor was the patterning of responses under concealed conditions consistent from series to series for the first few series. Thereafter a stable focusing pattern was established as shown by significant positive interseries correlations.

After a four-month interruption of the research we resumed the work with P. S. where the preceding research had left off, and the first test involved using the same 30 objects under the same conditions of concealment. Not only did we find a significant intraseries focusing effect when we applied a chi square test to the distribution of white and green calls for the 30 objects, but also the response tendencies on these targets were found to be significantly and positively correlated with the responses given to them during the preceding series four months previously (using a Pearson product-moment correlation).

The fact that patterning of responses occurred when so many objects were involved suggests that the focusing effect may not require the recognition of individual objects, but may involve instead (at least for some of the targets when a large number of objects are concerned) a separation of the objects into two categories on the basis of a judgment regarding whether each one shows more or less of a particular characteristic. There is no reason, of course, why the same characteristic would have to be used as the basis of evaluative judgment for all of the objects, and it is entirely possible that, even with a large set of objects, particular ones with distinctive markings will be individually identified and called the same way on most of the trials under both sensory and ESP conditions.

In short, we are not abandoning the hypothesis put

forward previously but are only recognizing that it may need to be made broader if it is to cover the occurrence of patterned responses under all conditions. We hope to make experimental tests with P. S. as well as other subjects to determine more definitely whether ESP focusing upon particular targets occurs in terms of "evaluative judgmental sorting" or "specific associative habits" or both.

SKIN VISION AND TELEPATHY IN A BLIND SUBJECT

Thelma Moss†, Jack I. Gray, John Hubacher, and Barbara Bush (University of California, Los Angeles)

Mary W., now in her thirties, has been totally blind since age eighteen. Nevertheless, she has achieved a master's degree in Russian, speaks five languages, and earns her living as a secretary and teacher of Braille. She had never attempted skin vision, and we knew nothing about it except what we had read in the literature. In spite of this, we decided to try a training program: in the eight months of experimentation that followed we progressed eventually to a point where formal experiments could be carried out.

In the first session of the preliminary training period we gave Mary two sheets of construction paper, identical in quality except that one was white and the other black. Mary was asked to discriminate between them through texture, temperature, "vibrations"--any subjective clues. After half an hour, she felt no difference, and this impression was confirmed in a series of 10 trials with only four hits. We then took a coffee break (which became standard procedure to avoid fatigue and frustration). By the end of the session she had scored eight hits in 10 trials, reporting that the black felt "rougher" than the white. In the next session, however, she described white as "rougher" than black; a reversal. This reversal of subjective impressions was repeated many times, with other colors, in subsequent sessions.

Progress was slow. After Mary learned the black-white discrimination, red was introduced, and she lost the

black-white discrimination. Similarly, when plastic materials were offered, she could not distinguish white from red. Her one swiftly learned and retained skill was "reading" black letters printed on gold. (This success may well be due, psychologically, to Mary's talent with language.) A game of anagrams was devised in which we would give Mary two letters to build a word, then add a new letter until a six-letter word was created (N + E = EN; + T = TEN; + R = RENT; + O = TENOR; + H = THRONE). This diversion was frequently used to reduce tensions and entertain observers.

Meanwhile, behavior patterns emerged. Although Mary's skill increased, she could never describe how she made her discriminations. Mary learned four colors in one material, but failed to transfer that knowledge to a different material. She would perform well on some days, and then have a session where she seemed to have lost all skills, save for the anagrams. Nevertheless a learning process seemed to be occurring, and two hypotheses were offered: (1) Mary might be getting clues from light, and (2) she might be receiving telepathically. We then arranged a test where Mary, alone in a totally dark booth, sorted 30 papers, of three colors, into three piles according to color. Her performance was as successful as in the lighted room with us. Thus, clues from light and telepathy were excluded, although clairvoyance remained a possibility. We also inaugurated telepathy tests, in which her scores fluctuated from high to low, as did her regular skin-vision tests. Finally, to avoid clues from actual skin contact, the materials were encased in plastic, with Mary feeling only the plastic. Her performance worsened, and Mary suggested discarding the plastic, avoiding contact instead by keeping her fingers one-half inch above the materials. These were termed "sensing" trials.

In the formal tests done following the training period, four different materials were used: paper, cardboard, plastic, and wool. For each material the items were identical except for color. Seven different colors were available in each material, but most trials involved fewer. Each session was designed as a different challenge. Thus in a given session we could introduce a fresh color in plastic, then repeat a four-color discrimination in cardboard, then do a sorting task with paper in the booth, and finally do some telepathy trials. Three basic categories were

designated: Touch, Sensing, and Telepathy. Records were kept of all trials.

In 22 sessions, there was a total of 1617 trials. The results in each category were divided for analysis into trials with different chance probabilities of success. In the 943 Touch trials, all subgroups showed CR's significant at $P < .001$. This was also true in the 380 Sensing trials. The 294 Telepathy trials showed significant scoring in the four- and seven-color discriminations, but not in the three-color task. Tests run with different materials all showed very high scoring. In general, the Touch trials had the highest scoring rates, averaging over 70 percent, and these did not decline as the number of choices increased. The Sensing trials had an average scoring rate above 60 percent, and again it did not decline markedly with increasing number of choices. The Telepathy trials averaged 38 percent correct scoring, with the highest scoring rate occurring in the intermediate (four-color) number of choices. The overall scoring rate for all trials combined was 64.1 percent.

Despite this astonishingly high level of performance, Mary still feels she "guesses," even on Touch trials. With more training, her performance may reach an even higher level; and with increased proficiency, she may become aware of the informational process involved. Mary does not "see" the colors; "vision" does not seem to be involved. Rather, her faculty seems to operate on an unconscious level, like so many psychic phenomena. And it is this fact that makes her proficiency at telepathy so intriguing. If her clairvoyance skill proves to be equally good, then perhaps the phenomena of skin vision and clairvoyance are related. This relationship might provide one of the connecting links between sensory and extrasensory perception.

A NOTE ON A HIGH-SCORING SUBJECT

E. F. Kelly† and B. K. Kanthamani (Institute for Parapsychology, FRNM)

A man who proved to be an exceptional subject visited the Institute for Parapsychology in February and

March of 1972. This paper reports preliminary work done with him during that period. The subject (B. D.), now 24 years old, was raised principally by his paternal grandmother, his parents having been divorced when he was an infant. The paternal side of the family appears to have been generously endowed with psychic ability, particularly his grandmother. Accordingly, B. D. very early acquired deep confidence in the reality of psi phenomena. He also, when he was a child, suffered from bouts of insomnia. In response to these he developed an unusual intimacy with a variety of states of consciousness between sleep and wakefulness, including a technique of controlled dreaming. The dream in fact seems to be the principal evolutionary force in his personality; he commonly remembers 20 to 30 dream episodes a night, and in many of them receives a sort of guidance, for example about future events or techniques for performing psi tests.

B. D. described to us a great variety of psi experiences dating back to childhood and tapping all the traditional dimensions of psi ability. It has only been during the last two years, however, that he has begun deliberately to explore his ability and attempt to control it. The principal objects of these efforts have been "tricks" performed in social contexts with ordinary playing cards, and in these he has been extremely successful. These performances are often quite spectacular; an important asset is his ability to perform at will before sizable groups of people, including skeptical or even overtly hostile people.

In our first encounters with B. D. we have tried simply to get a picture of what he can do under controlled conditions. Although he proved fairly difficult to work with, he assembled in the relatively small amount of time available a substantial quantity of high-grade evidence of psi ability. Six different kinds of ESP and PK tests were done, all yielding significant scoring levels. Three used machines designed by Helmut Schmidt, based on his random number generator: a fast PK machine (1, 000 trials; CR = 2. 6), a four-button machine allowing for either ESP or PK (5, 377 trials; CR = 6. 2), and an automated PK dart board (144 trials; CR = 2. 2). Two other PK devices, a dice tumbler and a sand timer, also showed high scoring (144 trials; CR = 4. 5 and 29 trials; CR = 3. 5 respectively). Using standard ESP cards he achieved an extremely significant CR of 7. 2 in 500 trials.

Two critical factors in B. D. 's ability emerged, confirming our impressions from the playing card performances. The first was generality: he successfully attacked a variety of task environments. The second was consistency: he showed almost no psi-missing, performed regularly at a high level, and in one case where tightening conditions produced a severe decline worked his way steadily back almost to the pre-decline level. The general suggestion which emerges is that the notorious fragility of psi is not inherent in psi itself, regarded as a process by which information enters the organism, but rather in the psychological or cognitive factors which govern the processing of that information. B. D. 's exceptional self-confidence and psi-control carried him through the sort of crisis which has traditionally been destructive for high-scoring subjects.

Some incidental observations are of interest here. Several observers independently noted apparent psi-facilitation effects in persons in B. D. 's neighborhood. B. D. also claims to be able to teach psi, and two of his pupils (roommates) gave significant evidence of psi ability in our laboratory, although not at B. D. 's level. These tentative observations should be confirmed and extended in future research with B. D. One focus should be to determine whether B. D. 's efficacy as a "teacher" depends on his physical presence or whether he communicates information about what to do that produces results. Our impression is that he conveys a few fundamental techniques. One is an imagery technique which can be used in a wide variety of situations. Another one, more basic, is what he calls "de-egoization," by which he attains a state of alert, intense passivity.

9

SPECIAL CASE STUDIES*

AN ENQUIRY INTO THE OSTENSIBLY SYNCHRONISTIC BASIS OF A PARANOID PSYCHOSIS

James F. McHarg (University of Dundee)

The concept of "synchronicity" was introduced by C. G. Jung in 1952 as an explanatory principle, supplementary to that of "causality," in an attempt to explain certain simultaneous events which, although not causally related, appeared to be meaningfully related. Jung never ceased to insist that the question of the actuality of such "synchronistic" phenomena must be decided empirically. The question of the actuality of psychopathological states of a more sustained nature, having a synchronistic basis, has not hitherto been raised; the present enquiry represents a preliminary empirical approach to it. It concerns the ostensibly synchronistic basis of a paranoid psychosis coming on in a 58-year-old woman, Mrs. L.

The question of synchronicity in the psychosis did not arise until six years after its onset and even then only in connection with the particular delusion dominating her at the time--that of a beast in the left ear eating its way into the brain. The synchronistic element arose when I made the provisional diagnosis of an acoustic neurinoma in the left ear of a man who had been transferred to my care, after I discovered that this man was a younger brother of the psychotic patient. She had been completely estranged from him for a long time. My provisional diagnosis, although rejected by ENT (ear, nose, and throat) and neurosurgical specialists, was ultimately confirmed after the

*Chairman: Emilio Servadio, Italian Society of Parapsychology.

brother's death from an unrelated condition; a specially requested post-mortem investigation demonstrated the acoustic neurinoma, on the left side, three cm. in diameter, and "pressing against the brain."

Subsequent retrospective study of the independently recorded clinical notes about the patient and her brother revealed that the hallucinatory noises in the patient's left ear, which had formed the basis of her delusion, had developed contemporaneously with the noises symptomatic of the neurinoma growing in the left ear of her brother. It also indicated that a later delusion--of a beast in her right ear--had developed contemporaneously with apprehension on the part of the brother about minor noises in his right ear. He had become abnormally aware of these noises as a result of the worsening symptoms in his left ear. These findings drew attention, next, to a much earlier delusion--that the patient was hawking up (coughing up) beasts. This was found to have developed concurrently with the brother's development of a productive cough, eventually found to be due to bronchial carcinoma. Moreover, her delusion had reached maximum intensity during the search for malignant cells in the brother's sputum which had finally clinched the diagnosis. Furthermore, it had faded away with the brother's recovery following pneumonectomy.

I recognized, nevertheless, that my failure to find evidence to controvert the complete estrangement ostensibly existing between the two could not exclude the possibility of communication through channels which simply had not been discovered. This explanation was, indeed, quite plausible in relation to the apparent reproduction, in the patient, of contemporaneous experiences of the brother which were of a purely subjective nature. Careful scrutiny of the data brought out, however, other significant features of the patient's delusions which corresponded to highly specific objective features of the brother's concurrent condition which were not subjectively known to him and which could not, therefore, have even been available for cryptocommunication. Most striking of these were the beasts which the patient believed she had begun to hawk up--for these appeared to correspond to the malignant cells which, unsuspected, must already have been present in the brother's bronchial tissues before he had started to cough. Almost as striking was the supposed destructive activity of the delusional beast in the patient's left ear eating its way into

her brain--for this appeared to correspond to the actual erosive action of the acoustic neurinoma in the brother's left ear which, undiagnosed and unsuspected, must already have begun its actual invasion of his brain. The scrutiny also indicated, finally, that the onset of the patient's hawking, and of her hearing noises in the left ear, had actually anticipated the onset of the corresponding experiences in the brother--which would have been impossible with crypto-communication.

This elucidation of positive, as well as negative, evidence for a synchronistic basis for the three successive delusions raised the question of the whole psychosis having been synchronistic; it directed attention, finally, to the one delusion not so far considered--the delusion, which had initiated the psychosis, of a sudden threat of invasion of her flat by malevolent agents. This seemed to have had no counterpart in the subjective experiences of the brother and it raised, therefore, the remarkable possibility of a synchronistic relation to the objective occurrence of the malignant transformation in his bronchial tissues. The content of the delusion showed an obvious symbolic correspondence, and the date of its onset corresponded to the date of malignant transformation--within the admittedly very broad range estimated for me by the brother's thoracic surgeon.

The findings were, therefore, very suggestive of the whole psychosis having been based upon a synchronistic partial personation by the patient of her totally estranged brother. It has to be fully acknowledged, nevertheless, that the striking correspondences could, each and all, have been fortuitous. A single-case report such as this calls for further enquiry into other sustained psychopathological syndromes in which synchronicity has not hitherto been suspected. The findings suggested also that the psychodynamic status of "synchronistic partial personation," as opposed to individual synchronistic phenomena, may be primary.

A PRECOGNITIVE "CHAIR" EXPERIMENT BETWEEN MANNHEIM AND JERUSALEM WITH THE SENSITIVE A. ORLOP

Heinz Chaim Berendt (Jerusalem)

In 1969 I conducted a precognitive "chair" experiment. The procedure involved a sensitive describing the person who would sit in a certain chair among a set of chairs on a particular future date. Mr. Orlop, the sensitive, was located in Mannheim, Germany. The actual experiment was done in Jerusalem, Israel. I was responsible for all the arrangements. Orlop had studied a diagram of the seating arrangement which would be used, and had made his prediction about the person who would sit in chair number 14. There were 16 chairs in all. Orlop tape-recorded the prediction on January 20 for the experimental evening in Jerusalem which had been arranged for January 28. A copy of the tape was sent to the Institut für Grenzgebiete der Psychologie in Freiburg as proof of the date of prediction. The original tape arrived in Jerusalem one day prior to the experiment.

On the night of the experiment, the 16 participants were seated in their chairs and Orlop's prediction was read aloud. Many items in Orlop's description coincided with objective characteristics of the lady seated in chair number 14. Taking into account the danger of selectivity, I will list some of these correspondences. Orlop described a lady about 45 years old, approximately 175 cm. tall, with dark brown hair. Her nose was Greek or Roman, and straight. She had a medium full figure. Six months previously, she had slipped while in her own home and injured her knee. Her field of activity involved giving lectures and readings with entertainment value to young people of both sexes. The occupant of Chair 14 was 42 years old, 173 cm. tall, had dark brown hair (which, however, had been dyed black), a long straight nose, and a slim figure. All the details of her accident matched Orlop's description except that she had injured her ankle, not her knee. She was an actress by profession.

Some of the other statements in Orlop's description seemed to have a sort of dreamy or blurred connection with the lady in Chair 14. Some statements, however,

seemed to be absolutely incorrect, especially those concerning the flat where she lived. I subsequently visited and photographed this place, and found that the description applied accurately to the next-door neighbor's flat. The two flats had originally been one large unit which was later divided into two by sealing one door. Another apparently incorrect statement can be interpreted in light of Orlop's contention that he is often unable to tell whether his images relate to the past, present, or future. He stated that the lady in Chair 14 had a view of the sea from her flat. The flat in Jerusalem was 60 km. from the sea, so the statement was obviously untrue, but the lady commented: "I was living for many years in Capetown and had the most marvelous view of the sea from my home."

In order to determine how well Orlop's statements fitted the other 15 participants, I asked all of them, plus the lady in Chair 14, to fill out a 46-item questionnaire. The results showed that Orlop's prediction had been fairly specific. Only five of the 16 were women; only two participants were between 40 and 45 years old; of the women, only two were between 170 and 175 cm. in height. Six persons had had accidents, four of them involving their legs. Only two persons had had an accident six months previously, and one was a man. Two persons had had accidents involving slipping. Only the lady in Chair 14 had had an accident in her own home. The results of the questionnaire were evaluated by a computer using the Multiple Scale Analysis method. The graphs resulting from this analysis showed that with respect to the characteristics described by Orlop, the lady in Chair 14 occupied an isolated position in relation to the centrally-clustered group of the other 15 participants. This provided further support for my conclusion that the correct statements made by Orlop could not be explained as pure coincidence, and that this experiment could be termed at least partially successful.

TWO COMMUNICATORS OF THE "DROP-IN" TYPE IN ICELAND

Erlendur Haraldsson† (American Society for Psychical Research) and Ian Stevenson (University of Virginia)

As evidence for survival after death, I. S. has drawn attention to the importance of mediumistic communications from communicators unknown, at the time of the sitting, to the medium and to the sitters present. Two such cases of "drop-in" communicators were reported in Iceland in 1946. Both involved the medium Hafsteinn Björnsson. E. H., in Iceland, recently checked many of the details of these cases and made further extensive enquiries which have confirmed the original reports. We summarize the results of this investigation below.

The Case of Runolfur Runolfsson. Shortly after Hafsteinn Björnsson started to hold mediumistic sittings in the autumn of 1937, an unruly and disturbing communicator appeared who was not at first willing to reveal his identity. He said he was looking for his leg. This went on for about a year. Then a new sitter, L. Gudmundsson, began to attend Hafsteinn Björnsson's sittings. He had recently moved from Reykjavik (where the sittings were held) to Sandgerdi (some 60 km. away). The unknown communicator immediately expressed his happiness at meeting Mr. Gudmundsson who, he claimed, knew about his leg which the communicator said was in the house Gudmundsson had bought in Sandgerdi. Gudmundsson was not aware of any leg in his house and refused to look for one unless the communicator revealed his identity.

At a later sitting the communicator reluctantly stated that he was Runolfur Runolfsson who had lived at Klapparkot, a small farm on the coast near Sandgerdi. He had died, he said, October 1879 at the age of 52, while on his way home from Keflavik, a village some 10 km. away from Klapparkot, during a severe storm. He had been drunk, had fallen asleep on the beach, and the tide had taken him out. The tide carried in his body in January 1880, but then dogs and ravens dismembered it. The remnants were buried in the Utskala graveyard but the thighbone was missing.

The church records of Utskala, which are kept in the National Archives in Reykjavik, were checked and the above statements verified. The records did not, however, mention that the deceased's thighbone was missing, only that his body had been dismembered. After some enquiries among older men of the community, Gudmundsson found a thighbone that had been placed in one of the walls of his house some years back. The few persons aware of its existence had never associated it with any particular man. The medium had at that time never been to Sandgerdi nor in the National Archives. This was confirmed by an examination of the guestbook of the Archives. There was no other source of written information about the details of the death of the communicator, although some of these details were known to persons living in Sandgerdi. No obituaries or announcements of his death had appeared in Icelandic newspapers.

The Case of Gudni Magnússon. At a sitting with Hafsteinn Björnsson on January 25, 1941, a man from northeastern Iceland was one of three sitters. A communicator quite unknown to all present appeared. He gave his name as Gudmundur or Gudni Magnússon and said he came from Eskifjördur, a town in northeastern Iceland. He said that he had been a truck driver, of average height, with blond hair which had already begun to thin out at the top of his head. He had been repairing his car on a mountain pass, crawled under it, and ruptured something inside his body. He managed to get home and was taken by boat to another fjord to a hospital, but died on the way. This was said to have happened some four to five months previous to the sitting. In Reykjavik, a few days later, a relative of the wife of the only practicing physician in Eskifjördur heard by chance about this communication from one of the sitters. He immediately wrote to Eskifjördur, enquired about the death of Gudni Magnússon, and obtained independent written reports from two of the sitters. The physician recognized a patient by that name and the circumstances of his death. Enquiries of Gudni's relatives verified other details.

Hafsteinn Björnsson had been brought up in northern Iceland and moved to Reykjavik (southeast Iceland) in 1937. He had never been to eastern Iceland at that time, nor did he know anyone who had known Gudni or his relatives. We found that an obituary notice on Gudni Magnússon had appeared in the Reyjkavik newspaper "Morgunbladid" on

November 7, 1940. This article, however, was more of a personal eulogy than an account of the deceased young man's life. It mentioned none of the circumstances of Gudni Magnússon's death which formed the main portion of the communication through Hafsteinn Björnsson. Further investigations of Hafsteinn Björnsson's mediumship are in progress. They have been planned with the hope that they will provide opportunities for studying other "drop-in" communicators at the time the cases occur.

10

METHOD AND THEORY IN PRECOGNITION RESEARCH*

COMPLEX PSI AND THE CONCEPT OF PRECOGNITION

Robert L. Morris (Psychical Research Foundation)

In recent years several studies have provided evidence that psi can perform complex tasks. Such studies may force us to reconsider before reaching firm conclusions about many of our research results. This is especially true of precognition studies, in which there is a time interval between the subject's response and the occurrence of the target event. During this interval a series of events takes place which leads to the occurrence of the target event. If this series of events is completely determined beforehand, the subject may be able to home in on it through complex clairvoyance and infer the target event on this basis. If the target event only becomes determined after the subject's response, there are two alternatives to the precognition hypothesis. One is that either the subject or the experimenter could exert PK on certain determining factors in such a way as to produce whatever event will match the response of the subject. The second is that the experimenter could use his own complex psi to determine what decisions he should make (such as method of entering a random number table) to produce the desired event.

If one is to take seriously the notion that an experimenter can influence the outcome of a precognition test by choices he makes after the designated subject has made his choice, then one is essentially hypothesizing that the choices of the later chooser will have more effect than those of the earlier chooser. Three studies with the psychic Lalsingh

*Chairman: K. M. Goldney, SPR, London.

Harribance provide data supporting this hypothesis. The procedure for the first study was as follows. Mr. Harribance and an experimenter, Judy Klein, sat at a table opposite each other. A barrier which ran the length of the table prevented each from seeing below the shouldertops of the other. I was off to one side, monitoring a polygraph which was recording Mr. Harribance's occipital EEG. At the start of a run, Mr. Harribance filled out two columns of 25 guesses each, using standard ESP symbols. Mrs. Klein then took a deck containing 50 ESP cards, 10 of each symbol, and started dove-tail shuffling them. When Mr. Harribance felt that the cards had been shuffled in such a way as to match his written order, he said "Stop." Mrs. Klein then recorded the order of the shuffled deck and handed it to me. Ten runs of 50 trials each were done in each of three sessions.

Of interest in this procedure is: where does the psi, if any, take place? Mr. Harribance made choices as he filled out his original 50 calls. He also made choices later as he told Mrs. Klein when to stop. If the ESP occurred in the original choices, such parameters as calling patterns and duration of calling time should relate to scoring success (as was true in earlier tests with Mr. Harribance where preference for certain symbols and consecutive calling of certain symbols were related to his scores). No evidence of such relationships was found.

If the ESP occurred instead at the later time when Mr. Harribance told Mrs. Klein when to stop shuffling, we should expect to find such factors as number of shuffles and EEG patterns relating to scoring success. For the first session, we found that Mr. Harribance scored at chance when he stopped Mrs. Klein after only two or three shuffles (Low Shuffles). On those runs when he allowed her to shuffle longer (High Shuffles), his scores were positive and significant (CR = 2.32). On the basis of this, I hypothesized that the same would be true for the last two sessions, dividing the data at the median as I had done for the first session. For the last two sessions combined, High Shuffles were significantly positive (CR = 3.37) and Low Shuffles were insignificantly negative. The EEG data have not yet been analyzed.

The second study employed the same procedure as the first for 25 runs, and a reverse procedure for 25 runs in which Mrs. Klein wrote the 50 guesses and Mr. Harribance

did the shuffling, stopping when he felt the score was correct. Completely chance results were obtained when the old procedure was used. When the new procedure was used the results were significantly positive (CR = 2.30). No measures were found to correlate with scoring success. The third study, comprising 50 runs, used a procedure similar to that of the first study, except that Mr. Harribance performed Mrs. Klein's duties. The overall results were positive and significant (CR = 2.05). Dividing runs into High and Low Shuffles as before produced significant positive results on High Shuffles (CR = 2.54) and chance results on Low Shuffles (CR = 0.40). Calling patterns (symbol preference and number of doubles) on the initial 50 guesses as well as time taken to make the guesses were completely unrelated to scoring success.

Thus on no occasion did we find any relationship between earlier-guess behavior and scoring rate, whereas we found a consistent relationship in two series using the same procedure between later-guess behavior and scoring rate. This suggests that the later guess may be the time when ESP is taking place. Should such a method continue to show evidence that the later guess is more crucial than the earlier guess, it might enable us to focus more directly on the possible role of the experimenter in precognition studies, and thereby to assess more thoroughly the past experimental evidence for precognition.

A PRECOGNITIVE METHOD FOR TESTING SENSITIVES

Douglas Dean (Newark College of Engineering)

It is a truism that sensitives find forced-choice tests, such as standard ESP card tests, very boring and do not usually perform as well as would be expected from their spontaneous psychic experiences. For this reason, many testing methods have been designed to allow a sensitive to respond freely when "reading" a person or object. The procedure usually involves a target person (TP) who is to be read by the sensitive. The TP submits a token object which the sensitive holds while making his statements about the TP. The statements about all TP's in a given study are then typed and given to the TP's, who

do not know which statements were intended for them. The TP's rate the accuracy of each statement as applied to them by making a check mark if it is accurate. Statistical methods are then used to decide whether more statements are accurate than would be expected on the basis of coincidence.

The major flaw in this procedure is that the decision about the sensitive's ability rests on how many correct check marks the TP makes. If there are too few or too many, the results are nonsignificant. In addition, the TP may check inaccurately. Often the sensitive seems to know more about the TP than the TP does about himself. This is especially true when the TP is anxious, disturbed, naive, or inhibited. An example of this occurred when I used this method with the sensitive who is the subject of the present study, Carol Ann Liaros, of the Human Dimensions Institute in Buffalo, New York. Carol, on being shown the data, expressed surprise that a particular TP whom she knew had rated seven statements in her reading as inaccurate. The TP was subsequently questioned, and admitted that the statements were indeed correct, but that she was in the process of trying to change herself and so had marked them incorrect.

Carol herself, following this failure using traditional techniques, suggested a new procedure. If we could convert the experiment to precognition, she argued, then we could safeguard against sensory cues. If we could take the decision about whether the sensitive's statement is correct off the shoulders of the TP, then he could not influence the results incorrectly. We decided then to make the decision dependent on a future event. Four series have been done with Carol as subject, using the following procedure. The TP was required to ask three questions about events which would occur in his life within the next 12 weeks, to which he did not know the answer. The questions had to be written in such a form that a yes or no answer could be given. Then Carol went down the list of questions and provided a yes or no answer to each. Twelve weeks later, the TP reported how the event actually came out. The results were evaluated by chi square using a two-by-two matrix (Carol predicted yes vs. no; event occurred yes vs. no). By this method the TP's chance of inaccurately reporting the event was very small, compared with his tendency to make incorrect accuracy ratings using the traditional procedure.

The results of the four series, which used a total of 94 TP's asking 285 questions, showed a highly significant tendency for Carol to be correct in her predictions. An example is drawn from the first series, done in the fall of 1971. The question was asked on Tuesday, October 19: "Will our union contract be accepted or rejected on Sunday, October 24?" Carol on the same day answered "Accepted." The TP was surprised by this, because all evidence indicated that the union would be on strike after Sunday. On Saturday, however, there was a complete change of attitude on the part of the union members, and they voted overwhelmingly against the strike. The contract was accepted, and Carol was correct.

Two dangers must be avoided by anyone using this procedure. First, one must guard against the possibility that all or nearly all of the questions are such that a yes (or a no) outcome is highly probable, and that the sensitive has a strong bias toward giving a yes (or a no) answer. Success under these conditions would be due not to the sensitive's psychic ability but to response bias. Second, one must obviously rule out questions to which the answer is a foregone conclusion even 12 weeks in advance. Here the results would be due to inference and not psychic ability. Controls were successfully applied for both these dangers in the present experiment. An advantage of this procedure is that one can see whether telepathy from the TP is influencing the sensitive's success, by asking the TP's to state how they expect the event to turn out. When I compared these expectations against Carol's predictions in the present study, I found that no significant correspondence existed. In addition, the TP's expectations did not relate at all to the actual outcomes, indicating that they were not precognizing the event. I concluded that Carol was using true precognition when she gave her answers.

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SYMPOSIUM: PSI, SCIENCE, AND SOCIETY

Opening Remarks: W. G. Roll, Chairman

This symposium is an occasion to reflect on the nature and purpose of parapsychology. What is parapsychology and how is it best pursued? The participants have been asked to approach this question from the vantage point of parapsychology itself, from the vantage point of some other science or discipline, or from the vantage point of society in general. Parapsychology is concerned with unknown or poorly understood aspects of man, and it seems that questions about the nature and destiny of man are being raised more urgently now than at any other time. This is of concern to parapsychologists if for no other reason than that answers to these problems are often sought in occult practices and beliefs which have at least a superficial similarity to some parapsychological researches and concepts.

THE NORMALIZATION OF PARAPSYCHOLOGY

James C. Carpenter (University of North Carolina, Chapel Hill)

A year ago at a meeting of this association I argued that fruitful work in this field had been made more difficult by a tendency of workers to forget that the central terms of the area (such as psi, clairvoyance, psychokinesis, etc.) are hypothetical constructs and to treat them instead as reified entities which had been concretely "discovered." I said then that I felt such a logical error led to a certain inflexibility in dealing with these terms, and a tendency to avoid rather than deal with the conceptual difficulties they generated. Even more important, I felt it made it more difficult to build low-level, empirically useful theory by which to guide our work, relate it to other scientific

endeavors, and give it coherence. Building such heuristic theory, it seems to me, is the most pressing need in this field today.

At least two factors would seem to have made such theory-building difficult. First, some of our most influential literature has been dominated by an inductive view of science which is forever saying that we should wait to speculate until we have garnered enough facts for them to tell us what to make of them. By this account it is nature, not ourselves, which has the basic responsibility for telling us what to make of her. There is about as much promise in this tack as in trying to build a house by collecting a great pile of bricks and hoping that they will assemble themselves into a structure. A second reason for our concreteness and reluctance to theorize seems more subtle. It has to do with the fact that the various phenomena that we have selected as our subject matter all have in common the element of anomaly. In fact, one could say, I think, that parapsychology is the study of anomalous correspondences which are psychologically meaningful.

There are logical dangers in this definition of our field. Imagine a study, if you will, ingeniously designed and carried out with thoroughness, precision and expensive instrumentation. Imagine further that, as we work our way through toward the end of the report, we find considered and abandoned one after another "normal" explanation of the observed results. A note of triumph seems to grow between the author's lines. Finally the crescendo peaks and the author concludes positively that the observations are, through his best efforts, totally unexplained and the results are therefore definitely Paranormal. Making the unexplained the goal of our scientific efforts is a risky business; and I think we are not helped really one bit by the logical trick of reifying mystery with an apparently substantive term such as "paranormal" or "psi."

One could as well declare that ours is the science of Ignorance, that we will plumb the depths of Ignorance (though more timid souls might not dare) and conclude that with statistical significance we have established the existence of Ignorance. Perhaps the situation was even a bit worse than now in those days when the goal was to discover "psi," or "prove the reality" of the "paranormal." I would assert that anyone who can utter the phrase "discover the paranormal" and not experience a very

uncomfortable sense of self-contradiction has become a bit hypnotized by his own language.

To work toward the establishment of an anomaly, and leave that hard nut of undigested mystery enthroned as our end-product, is to short-circuit our own scientific motives and give ourselves a smug feeling of completion at the very moment when we should be most goaded into hard work. If science progressed by reifying its constructs and establishing their ultimate reality, which I believe it does not, then we might keep on collecting and buttressing our miracles with some profit. If science progresses by constructing a thoroughly provisional but hopeful and useful account of events, which I believe it does, then the apparent miracle is an invitation to build. No more and no less.

Many have observed a shift in research emphasis in this field from efforts to establish the existence of this or that to attempts to frame and test predictive hypotheses. As some tell the story, this shift was justified by our having successfully done our establishing. (Others, of course, argue as heatedly that we really have not.) I would assert that the shift in emphasis is quite appropriate not because psi has been established (our designs and methods of inference are really not well-suited to such a question anyway) but because it was never a particularly interesting question from a scientific point of view anyhow. At least at this early stage of the game we will be judged most appropriately not by what we prove but by what we build. We are scientists, not lawyers.

The most interesting question in all this, it seems to me, is why the issue as to the mere existence of anomalous correspondences (or psi) should have ever seemed of such importance. Two possible factors occur to me. First, it seems that the occurrence of such events was taken as an indication or proof of a vast and significant other-order of things--a second world standing somehow just behind this everyday world of flesh and blood and love and death, but forever shut away and invisible save for those little psychic keyholes through which one could glimpse its workings. Probably the most explicit and elaborate version of this view was developed by F. W. H. Myers. In the light of such a dualistic cosmology the existence of "psychic" anomalies seemed of immense import as evidence or instantiation. Probably most parapsychologists nowadays would not consciously claim allegiance

to such a cosmology. Yet I think it has lurked dimly lit and unarticulated, implicit, in a great deal of our work. To the extent that this is so it can scarcely help having had a retarding influence on our scientific efforts. That other world was from the start supposed to be quite different from this one. To study was not to extend our sciences but to refute them. The kinds of experiences, methods and reasoning which applied to one would not, by definition, apply to the other. Approaching such mysteries scientifically might establish them but should not penetrate them. To try to work scientifically under such an implicit model would be to swim with one hand while holding on to a sinking stone with the other.

A second factor which may have made the existence-of-psi issue so important was the apparently monolithic, all-inclusive reductionistic behemoth which the physical sciences in the nineteenth century were seen to be. The white crow--to recall William James's simile for Mrs. Piper--is of such interest only in the context of some authority worth challenging which is insisting that all crows must be black. Such a simple view of science scarcely seems to hold today. The conception of the unity of the sciences, arranged in a reductionistic hierarchy, may still be given occasional lip-service today, but it hardly inhibits most psychological research, and many psychologists disown it explicitly.

The research enterprise in psychology nowadays is a problem-centered one with indigenous terminologies being developed out of intimate, persistent contact with a particular subject matter. One can scarcely find a theorist who is trying to preempt everything anymore. Most of them seem to feel lucky if they can account for anything with any reliability, and very few seem to have the energy left to go about squashing things which are left out of their particular constructions.

I think that as parapsychologists we are in precisely the same position as in other areas of psychology. We are pursuing some intriguing and confusing phenomena and have the exciting task of constructing accounts by which we can rescue them from chaotic flux and see them as intelligible and predictable. We need to make something out of them. We need to develop a terminology indigenous to our observations. Our terms are conceptual tools, not entities. Some may feel that I am throwing away the "stuff" of

parapsychology by this position. I think rather that I am urging a return to the phenomena. I am urging, in brief, the normalization of parapsychology--or rather the enrichment of psychology by our own vigorous participation in it, focusing on the phenomena we have chosen. I should think every bit of our experience and training in other areas of research should be exploited in our building. We have assumed our own irrelevance for too long.

DIMENSIONALITY AND ESP

V. A. Firsoff (Somerset, England)

Geometrical hypotheses to account for psi phenomena, and precognition in particular, have been proposed by J. R. Smythies and Adrian Dobbs, among others. I tentatively suggest a certain interpretation of dimensionality which, though purely physical and even astronomical in origin, may have a bearing on parapsychological research. In fact, with a very different point of departure and by very different roads I arrive at conclusions that come fairly close to some ideas set forth a few years ago by W. G. Roll. Space is traditionally viewed in terms of geometry, outside a physical context, like a kind of abstract box into which various contents can be fitted at will. Mach and Einstein, however, regarded it as a physical entity, and I will follow in their footsteps.

Take the simple concept of distance. This may be so many microns, miles, or light-years; but measuring it is a physical operation, which alone gives it meaning. Assuming equal conditions, the distance from point A to point B is experienced as the difficulty--in terms of work or time--involved in passing from the one to the other. If we consider astronomical space, remote from local fields, the problem boils down to determining the time taken by a mass $\underline{m}$--and light being ponderable, this definition applies to it--moving at a known velocity, to reach B from A. Yet this velocity is inversely proportional to the inertial value of the mass $\underline{m}$. On Mach's Principle this value depends on the attraction of the remote masses of the universe. Imagine that these masses had vanished: $\underline{m}$ would become zero and its velocity infinite. It would thus be present at A and B

simultaneously, and there would be no physical way of distinguishing between the two. Extension and space would lose all meaning. This has various consequences. Mach's force is finite owing to the intervention of the cosmic red shift, which sets a limit to the propagation of energy, known as the radius of the observable universe. Any masses beyond it are inaccessible to direct physical action, but they are accessible indirectly, by relay, through action upon intermediate masses.

It is a well-known principle that the total momentum of a closed system cannot be altered from within it. The universe is such a system in respect of any mass (including a photon). If this mass m is moved the universe as a whole must execute a movement compensating for the incurred change of momentum. Yet the mass of the universe is infinite, and an infinite mass cannot be moved. It will react by relay, with a time lag due to the finite speed of propagation of energy (i.e., the velocity of light), and arrest the movement of m by a kind of "elastic after-working." I believe this to be the origin of the cosmic red shift, which has nothing to do with any expansion and measures the rate of decay of kinetic energy. It thus constitutes an objective measure of universal time. The decay occurs within the Hubble Time of the order of 10^{10} years, and it limits any direct macroscopic action to the radius of the observable universe.

If, however, we consider the microscopic situation at the subatomic level we encounter hadronic forces, which are 10^{40} times stronger than gravitation, but have a very short range. In the light of the above reasoning it is logical to assume that the decay of these forces is likewise due to an "elastic after-working" of the universe and, if so, the propagation of the corresponding energy must be enormously faster than that of light or gravitation.

Time on this scale has a different dimension. Subatomic events are measured in micro- and nanoseconds of our time-scale and, since subatomic time flows faster, the effect of a given cause in the microscopic system will precede that in our macroscopic system. Thus if the progress of a missile is referred to the latter, a nucleus about to be hit by it can explode before the missile has reached it: we thus have a reversal of time and causal sequence. Relativistically, of course, a movement that is faster than light involves a time reversal (in our macro-

scopic system). Better to visualize the situation, let us take a galaxy 20 million light-years away. Suppose we could receive signals from it at twice the speed of light; we would thus acquire knowledge of events in that galaxy 10 million years before they could be telescopically observed.

Now ESP phenomena suggest the existence of mental fields and interactions of a peculiar kind involving precognition--i. e., time reversal--and thus a situation very similar to that found at the subatomic level, or indeed in special relativity. In the latter the flow of time is a function of speed; but the flow of parapsychological time would be determined by very different parameters, as psychological time undoubtedly is. My mind has always been exercised by the three-dimensionality of visual space. If dimensions are a function of physical fields this dimensionality must have a physical cause. It is not just a matter of abstract geometry. Consider light. We have here an electric and a magnetic vector oscillating at right angles to each other. But light has mass--a gravitational property--and so exerts pressure in the direction of its propagation, at right angles to the other two vectors. Here is a Cartesian system of three mutually orthogonal coordinates, with the fourth Einsteinian coordinate of time determined by the velocity of light. This seems to account for the dimensionality of perceptual space. The approach is fruitful, as it leads immediately to wave mechanics and special relativity. Dimensions are determined by interactional forces.

We do not perceive things, but only events, i. e., changes in dimensional vectors. Once we bear this in mind and remember the suggested nature of cosmic time, it becomes clear why in the equations of relativity the square of the time increment appears with a negative sign: it stands for negative energy. This is also why anti-particles in subatomic reactions move along a negative time axis. But wave mechanics tells us something more. On paper, mathematically, a change in one dimension only is possible. Not so in the real universe. Here dimensional vectors are conjugated. A moving mass generates, indeed becomes, an electromagnetic oscillation; whence, incidentally, the indeterminacy of its position. In a space continuum any event must have all the dimensions of the continuum simultaneously. Every interaction defines a dimension, so that the movement of our mass will have strong hadronic and weak nuclear (neutrino) effects as well. The reason why these remain unobservable is, firstly, their short range, and

secondly, the fact that mass, representing gravitation, is negligibly small in relation to nuclear forces. If, however, our assumptions regarding mind and mental interaction are correct, there must be a mental effect as well, without which clairvoyance would be physically impossible.

Thus in a sense mind is an epiphenomenon of physical events, but the same reasoning applies in reverse, so that physical events are also an epiphenomenon of the mind. Furthermore, since mental effects of physical events are small, it would seem that mental forces resemble the nuclear in being much more powerful than those of the macroscopic physical scheme. Indeterminacy is the consequence of conjugation of dimensional vectors, and so will be multiplied with each further dimension. Is this an idle flight of fancy or a blueprint of the physics of the future? Only the future knows the answer.

A CHANGE OF METHODOLOGY IS NEEDED

K. Ramakrishna Rao (Andhra University, Waltair, India)

Parapsychology, in my opinion, has suffered a great deal by its association with psychology. Modern parapsychology, while proclaiming itself to be anti-establishment, has followed very much the same models and methodology as psychology has used. This kind of a beginning might have had its own strategic significance at the time when parapsychology was largely confined to the seance rooms of suspicious mediums. Also, this phase of research has brought a good deal of evidence to convince at least some men of science of the reality of psi. And this is surely no small achievement. Yet we have not crossed the impasse; we have not convinced many scientists; and we can hardly make any claim to understand the nature of psi and its place in this universe. Our "psychological" approach boomeranged. On the one hand, parapsychology became suspect in the eyes of psychologists. On the other hand, the inadequate methods and inappropriate models borrowed from psychology had a strangling effect on the healthy growth of our science.

I come from India, a culture which is very different

from the Western culture where my parapsychological training took place. When I contemplate these two influences on my career as a parapsychologist, I can see very well the difference between their approaches. Under the influence of my Western training I was brought to a position where my thinking had become tight, narrow, limited, and attached to objective criteria. I was obsessed with manipulating variables and obtaining predicted results. At the same time I knew I was dealing with a phenomenon which was radically different, qualitatively different from what I had known in psychology. With nearly two decades of involvement in parapsychological research, I do not believe I am always clear in my mind when I talk or write about psi. Let me illustrate this point by referring to the status of evidence in our field.

When we raise questions about the status of evidence in parapsychological research, whether ESP is established or not, we are approaching them from various philosophies of science and various personal standpoints. I had been taught that science is value-free, that science is objective, but I have become totally convinced that this is not true. There can be no science without the personal involvement of the scientist, and there is no science which is completely value-free. Now when we ask the question "Is ESP an established fact?", do we mean by it "Does the scientific community by and large accept ESP as an established fact?" Or are we asking the question "As a reasonable individual who is acquainted with the ESP literature, do I think ESP is an established fact?" We must make clear which point of view we are taking. I find myself shifting sides from time to time.

I am sure most of you would agree that, if we take the scientific community as the reference group, the answer is no; ESP is still a controversial phenomenon, it is certainly not established. But if I take myself as the reference person, I would answer yes; in my judgment, ESP is an established phenomenon. I think the answer is very clear. For those who are acquainted with the literature there are only two possible alternatives: deception or the reality of the existence of ESP. I don't think we can ever do an experiment in which deception can be totally eliminated, so one's personal judgment must suffice.

The point I would like to raise here is this: if I am convinced that the evidence is sufficient to prove the

existence of ESP, is it necessary that there should be a repeatable experiment? Should parapsychology deploy all of its resources in finding a repeatable experiment, or should we use these resources to do something else? Here, I think, we should follow our own points of view. As for me, I feel that unique things exist in the universe; some of them may not be repeatable, but you cannot deny them. Even if an event occurs only once, if it does not give you a scope to experiment with, or if there is only one witness to say that he observed it, we still cannot say that it did not take place. Now if we accept in some meaningful sense that ESP is unique, we have to consider ways of studying it, and certainly the methods that we borrow from others become inappropriate.

When I talk to physicists and engineers, I often find them willing to accept the parapsychological position. When I talk to psychologists, I find them more reluctant to accept it. Why? For the very simple reason that the psychologist is working with outdated, obsolete Newtonian models, thinking in absolutist rather than relativistic terms. Parapsychological phenomena represent a potentially healthy challenge for the psychologist. They can force him to wake up and reevaluate his position. If he doesn't accept this challenge, it is to his own disadvantage because he will continue to work in a very limited sphere and his conclusions will have only limited validity.

In my search for appropriate methods to study these unique parapsychological phenomena, I have found myself returning to the traditional approaches of my Eastern background, which I had long abandoned. In India, a couple of thousand years ago, people began to look at these phenomena in a very scientific way. However, they did not use a stimulus-response model with its implications of an external object influencing a subject; instead, they used a model which placed emphasis on the individual's participation in these events. They developed a methodology, a training procedure, which might be called introspection, but not in the Wundtian or Titchenerian sense. It was a method by which they could control their internal processes and become aware of things going on inside themselves. This method would seem to me to be particularly relevant to our field.

We need to shift our efforts from looking at what happens outside to exploring and understanding what is going

on inside ourselves. We have today many psychophysiological techniques that can shed light on these phenomena. To the extent that they can be used to validate claims made by the yogis of ancient India, they lend scientific credibility to the approach of applying some of these yogic techniques to the study of ESP. I am convinced that the traditional parapsychological approach which considers the "target" to be a stimulus outside the subject, and the subject's guess to be the observable response, will not work any longer. We have to have a new model.

The time has come for this intellectual challenge to be taken quite seriously. Appropriate methodology must be designed to explore the inner processes that are going on when someone has a paranormal experience. A more existential and phenomenological approach to the kinds of things we are dealing with in parapsychology is bound to yield better results than the kind of behavioristic, mechanistic, cause-effect relationships we have been studying.

PSI AND PSYCHIATRY: THE NEED FOR RESTRUCTURING BASIC CONCEPTS

Montague Ullman (Maimonides Medical Center)

Parapsychological data cannot be absorbed into the mainstream of science through any additive strategy. We seem to be dealing with a situation where we are called upon to go beyond an examination of the data in their continuity with related fields of investigation and make the more strenuous effort of resolving their apparent discontinuity. To do this will in all likelihood involve a "paradigm shift" of the kind described by Kuhn. We are faced then with two tasks. The task of examining the continuities is addressed to the question of how our available knowledge helps us understand parapsychological data. The task of examining the discontinuities is addressed to the question of how our parapsychological data can help us develop a more profound understanding of what we now regard as our store of available knowledge.

An illustrative example of the problem occurs in the field of psychiatry. For some time now it has been known

that psi data impinge in some way upon psychopathological processes and are apt to be encountered in the course of the psychotherapeutic endeavor. Freud in a tentative way and Jung in a more all-out sense were the forerunners of a handful of analysts who sought to identify the areas of congruence--i. e., the continuities between psi events and the psychodynamics of the therapeutic situation. This led to an appreciation of the role of anxiety, need, interest, and, more specifically, transferential and countertransferential factors in eliciting psi responses.

We can add to our understanding of the continuities experimentally by examining the nature of central processing once paranormal information impinges upon the central nervous system. Sinclair, and later Warcollier, made important and similar observations on the kinds of changes that occur in the course of the paranormal transmission of images. The fragmentation of the ESP stimulus described by both authors has also been noted in some of our pilot dream studies of telepathic transmission. The possible relationship of these effects to the studies of Evans on the fragmentation of the stabilized retinal image suggests possible similarities and continuities between ESP and visual perceptual processes.

More is obviously needed than the establishment of psychological continuities at the level of meaning and motivation, and structural continuities at the level of central nervous system processing. We are still left with the tantalizingly difficult task of coming to terms with the discontinuities. These discontinuities surface as events transcending spatial and temporal limitations. Considering this challenge just within the field of psychiatry itself, it seems to me more and more apparent that any accommodation of paranormal data will involve the elaboration of a radically different conceptual base for our understanding of psychopathological syndromes.

If we limit our discussion to just two such syndromes, namely the schizophrenic and manic-depressive psychoses, we may have to reconceptualize our understanding of both of these psychoses in terms of the spatial and temporal aspects of character organization. There has been some tendency in recent years to merge the two psychoses, but I think that symptomatically, temperamentally, and perhaps constitutionally they remain distinctive. The manic-depressive maintains affective ties to the world, but cannot modulate

them. The schizophrenic deploys his affective capacities in the service of maintaining distance between himself and the world. The schizophrenic is future-oriented in terms of his unrelenting vulnerability to unpredictable threats to and assaults upon his isolation. The manic-depressive is past-oriented in the depressive phase, relating in terms of past failures, and present-oriented in the manic phase, relating in terms of a sense of unreal successes.

Time plays a different role for each in still another way. Magical thinking and omnipotence of thought play a key role for the schizophrenic. Normal processes, extending in time, have to be bypassed to arrive at magical solutions. Real time, in a sense, doesn't exist for the schizophrenic. This is in contrast to the manic-depressive for whom time is either retarded or accelerated, depending on the phasic variation he is experiencing. In the depressive phase, there is a severe limitation in the contextual field and time is retarded. In the manic phase, there is a tremendous expanse in the contextual field and time is accelerated. In the depressive phase the past overshadows the present and the future, and in the manic phase the present overshadows both the past and the future. Is this difference in orientation reflected in differences in ESP performance? Would precognition be more apt to be associated with schizophrenia, telepathy or clairvoyance with the manic psychosis, and retrocognition with the depressive psychosis? Would other but equally consistent relationships obtain where compensatory abilities might play a role--i.e., the schizophrenic sensing things paranormally in the present in connection with his excessive vigilance concerning the future?

Space can be thought of as content, as context, and in a certain sense as encompassing the qualities of palpability, endurance, and sameness in contrast to the qualities of impalpability, elusiveness, and change associated with the concept of time. In this sense the schizophrenic is time-oriented--the changing aspects of reality are more important to him than the enduring ones, since it is change and not sameness or familiarity that evokes vigilance operations. For the manic-depressive it is space, context, and sameness that predominate over time and change, since the manic-depressive does invest himself in his human context but unfortunately as an expression of the fantasy either that it cannot change or that it is changing too fast for him to keep up with.

This differential weighting of space and time in the two syndromes might provide potential clues for further research efforts. Might it not be better to seek effects in the psychological realm with schizoid individuals and psychokinetic effects with those closer to the manic-depressive end of the spectrum (and here I would include hysterics)? In the first case we are testing the limits of the schizophrenic's omnipotence of thought, his need to know in the service of vigilance operations, and his preoccupation with the temporal or changing aspects of reality. In the second case we are testing the limits of the individual whose interaction with the world is much more invested in space, context, bodily and motoric involvement.

I have not yet engaged in any systematic study, but I have developed the clinical impression that from a psychopathological point of view good ESP performers as encountered in the clinical situation are on the schizoid side. I haven't had the opportunity to study sufficient PK performers, but I would offer the speculation that they fall within the manic-depressive or hysteric syndromes.

A "DIFFERENT" APPROACH TO PARAPSYCHOLOGY

Emilio Servadio (Italian Society of Parapsychology)

In the professional fields that are most familiar to me, psychoanalysis and psychotherapy, there have been two major turning points: first, the primary importance that had to be given to psychological reality over a so-called objective reality; and second, the recent abandonment of an analytical "model" postulating an observer (the analyst) and an object of observation (the patient). Whereas in the early days of psychoanalysis the so-called transference of the patient was recognized as an important phenomenon, while the counter-transference of the analyst was seen as an undesirable disturbance, it is commonly admitted nowadays that transference and counter-transference are the complementary components of a whole, of a Gestalt where a sharp distinction between subject and object of observation can no longer be maintained.

In the physical and natural sciences, a similar

abandonment of the classical position implying an observer and an independent object of observation is still, for many, little more than theoretical, in spite of the well-known affirmation of the indetermination principle by Werner Heisenberg in 1926. The fact is that it is very difficult to overcome the classical, empirical model of scientific observation. This is true in spite of the fact that philosophical speculation has ever and ever contended that such overcoming was an unavoidable step in modern thought, and a long due re-evaluation of certain traditional teachings. Several recent essays have stressed the necessity of a similar change in our approach to reality. The last I have come across is a book by Joseph Chilton Pearce, The Crack in the Cosmic Egg, where the author maintains that reality can actually be shaped by the creative scientist, and that--to quote his own words--"concepts direct percepts as much as percepts impinge on concepts."

On more than one occasion, I have asked myself if and to what extent parapsychologists have accepted the aforesaid views, and I have had to admit that, with a few exceptions, their approach has largely remained in line with the classical, subject-object model. The exceptions are represented by Lawrence LeShan, Jule Eisenbud, John C. Poynton, and very few others. Perhaps the answer to many present difficulties in parapsychological research might consist in pursuing inner changes in us, the observers; getting more and more familiar with the beyond in our very depths; approaching the world of psi by probing our inner paths. The famous student of Eastern traditions, Sir John Woodroffe, said: "If we would know what some other than ordinary experience is, we must actually shift not our speculative thought on to it but our whole being into it."

The obvious although implicit reluctance of parapsychologists--in their majority at least--to accept the aforesaid shift in their search (implying as we have seen a bypassing of the empirical "observer and observed" model) seems particularly unjustified when one considers what is occurring in other areas of science. Manifold investigations of altered states of consciousness and of various techniques aimed at the discovery of new inner horizons have been either dug out from oblivion or newly invented in recent years. They command the attention of an increasing number of Western students, including more than a few parapsychologists. I need only mention the development of research on yoga, the bio-feedback contrivances that can

prolong alpha states more or less at will, that sort of Western version of yoga which is Schultz's "autogenous training," hypnotic regression, Desoille's "rêve éveillé," the psychedelic states obtained with or without drugs, the self-metaprogramming designed and recommended by John C. Lilly, De Ropp's so-called "creative psychology," etc. -- not to speak of some less-known practices which have long been adopted in esoteric or "occult" associations and groups. I admit that it is not very easy to find one's way in such a variegated series of descriptions and alleged experiences. However, it is my conviction that underlying them all there is the definite indication of a fully justified pursuit: that of establishing different relations between man and reality, and developing brand new ways of creative thinking and living.

Of course it is far too soon to predict in what ways the aforesaid change in our views and techniques might allow us to obtain different and better knowledge of psi phenomena. However, it seems to me that if we admit the possibility of penetrating--by our own efforts--into different kinds of experience, into realms that can be, and often have been, compared to Alice's "Wonderland," psi phenomena could well appear to us as ubiquitous, erratic signs and manifestations of such realms, of such diverse lands. To bring them steadily and concretely under our scrutiny "this side of the looking-glass," following the traditional scientific approach, would then appear a conceptually impossible pursuit. It would be comparable to the process used by a natural scientist who should insist, in carrying out a thorough study of fishes, on methodically isolating them from their natural element, water.

Even more difficult to foresee are the gains at large that might be expected from the above-described "new look" in parapsychological research. But I have a distinct idea that experiencing "from within" the premises of psi phenomena, and their underlying mechanisms, should bring about great changes in our whole evaluation of fundamental "categories" such as mind, matter, interpersonal relations, nay, life itself. Contrariwise, I do not think that a refinement of our present "orthodox" scientific techniques of parapsychological investigation could effect any substantial change in human ideas or behavior, nor that it would be in any particular way beneficial to the human race. As always in history, great human changes can be expected if fertile ideas are sufficiently spread, and become conditioning

factors of thought and behavior (take for example Marxism, or psychoanalysis). Parapsychologists, who have by definition a typically "open" attitude toward new territories and new paths, seem to me particularly prepared to be among the pioneers of what might well be a profound revolution in the history of the human mind.

RESEARCH OBJECTIVES AND METHODOLOGY

John Beloff (University of Edinburgh)

Public interest in the paranormal has in the past mainly been of a practical kind, so that mediums, fortune-tellers, water-diviners, witch-doctors and other assorted magicians have always been able to count on plying a lucrative trade. In modern times, however, it has been largely the ignorant and uneducated who provided the bulk of their clientele. It is somewhat alarming, therefore, to learn that the so-called "occult revival" of recent years has spread even among university students. Parapsychology has always stood for the scientific approach to the paranormal and it would be unfortunate, perhaps even in the long run disastrous if, out of a temptation to cash in on this new trend, we were to lend respectability to what, in effect, is rank superstition.

This is not to say that parapsychology might not one day generate a genuine para-technology of its own. I can envisage, for example, early warning systems based on mass precognition tests or, let us say, a clairvoyant detection service based on systematic token-object readings; but such thrilling possibilities must, for the time being, remain the province of the science-fiction writer. Parapsychology is still too backward to offer yet anything much in the way of a technological spin-off.

Meanwhile, there are two main objectives on which parapsychologists should now concentrate. The first is trying to convince the outside world and, more especially, the uncommitted scientist that we are onto something real, that the whole field is not just a mirage. The second is to learn as much as we can about the phenomena themselves.

In present circumstances these aims are of equal importance; neither should take precedence over the other. But--and this is my main point--the two demand quite different research strategies. For the purpose of impressing outsiders the kind of experiment one needs is something easy to understand and as free from complications as possible. For the purpose of enlarging our knowledge of the phenomena, on the other hand, a more sensitive and subtle approach may be necessary. For the former objective, it is a great advantage to have the entire procedure automated so as to reduce the role of the experimenter to an absolute minimum. The new-style automated animal experiment is likely to prove invaluable in this connection. There may also be untapped possibilities in developing automated gambling machines that could be specifically designed to test various ESP and PK hypotheses. For the latter objective, to learn more about the phenomena, the more promising avenues are likely to come from the intensive testing of special subjects or of subjects under special conditions. Recent interest in moods and altered states of consciousness as well as dream experiments testify to a desire to broaden the scope of parapsychology.

There is an obvious affinity between psi phenomena and the phenomena of depth psychology. For example, symbolism and imagery play a large part in both. For this reason there is much to be said for free-response tests in this connection rather than the forced-choice design of the conventional card-guessing tests which were ideal so long as one wanted only to demonstrate ESP. This does not mean, however, reverting to the sloppy old-fashioned style of test which simply ignored statistics. The point is that statistical methods are available which can handle free-response data using matching techniques, which allow for an exact estimate of significance.

I come now to my last point, the importance of parapsychology. For a long time to come, its importance as I see it is neither practical nor in any ordinary sense scientific. It is, rather, metaphysical. By this I mean that parapsychology represents the only body of empirical evidence which has a direct bearing on the traditional mind-body problem. ESP and PK are the only psychological phenomena which so far defy the brain-computer analogy which, rightly or wrongly, is being applied to all our other perceptual, cognitive or motor processes. This is an aspect which the pioneers of parapsychology, from Frederic

Myers to J. B. Rhine, appreciated very clearly. It is regrettable that it is often lost sight of or even repudiated by many contemporary workers who prefer to consider the paranormal as simply the physics of the day after tomorrow.

PARAPSYCHOLOGY AND CONSCIOUSNESS

Charles Honorton (Maimonides Medical Center)

While searching for models and theories, we parapsychologists have been reluctant to venture beyond the conceptual system with which our findings conflict. Unless we discover the equivalent of an ESP end organ with peculiar properties such as time displacement, we are apparently postulating direct nonsensory interchange between man and his environment. Any notion of survival after death requires us to postulate "something" which could survive. Thus, we have tacitly defined our field as the study of consciousness as it extends beyond the organism.

Herein lies our claim to the attention of society as well as the source of our difficulties with science. The "ESP controversy" has less to do with the adequacy of experiments than with the challenges they pose to epistemological assumptions which underlie Western science. In Kuhn's terminology, we are engaged in a "paradigm clash." While this has all been said before, we are still attending to the content of the critics rather than to their message: psi phenomena are anomalies in the prevailing reality view or paradigm. As long as we cling to the hope that psi can--somehow--be accommodated into the present paradigm, we continue to design new "key" experiments which test nothing more than the ingenuity of future critics who say that it cannot. This loop can only be broken through the emergence of a new paradigm.

Drawn by the gravitational influence of physical science, Western psychology quickly settled into a frame of reference sufficiently limited to permit adoption of physical science methods, tools, and constructs. Mental phenomena, judged complex and difficult to measure through these techniques, were given in custody to the clinicians. Behaviorism has given psychology a sense of status if not accomplishment.

Seeing it currently embodied in B. F. Skinner's best-selling manifesto, Beyond Freedom and Dignity, we are reminded of C. D. Broad, who 50 years ago characterized behaviorism as "so preposterously silly it could only have been proposed by very learned men who were themselves too confused to know what they were saying." Societal pressure is mounting for psychology to demonstrate clearly its competence and value, and to transcend a tradition which emphasizes pathology over potential. Isn't it odd that there are national institutes for mental health but not for mental potential?

Humanistic psychology, the psychedelic movement of the sixties, and current fascination with "occult" practices reflect attempts to erect a new paradigm. Some go too far, abandon too much, and provide dubious alternatives. There is developing, however, a new and deep interest in states of consciousness and techniques for altering them. Already there is a market for portable biofeedback devices--some of which actually work. Research on Indian meditation techniques has become not only acceptable, but quite popular. In fact, current biofeedback and meditation studies may provide a methodological and conceptual bridge between the dramatically different paradigms of East and West.

Eastern psychology, such as yoga, emphasizes the development of mental potential through the control of consciousness. In Tart's terminology, yoga is an empirically-based "state-specific science." The siddhis or "powers" attributed to yogic development represent a catalog of parapsychological claims. The techniques through which these "powers" purportedly become manifest are sophisticated and systematic. Patanjali refers to concentration-meditation-absorption as the process leading to siddhis: freedom from external sensory distraction and internal ego structuring which permits uninterrupted "flow" of attention to its object and, if maintained for a sufficient period, absorption in and by it. This last phase, absorption, is associated with a state of consciousness generally described as "transcendental" and in which, it is claimed, space and time cease to exist.

Preliminary explorations of ESP in altered states of consciousness including hypnosis, dreaming, alpha feedback, and sensory deprivation suggest that psi (or, perhaps, subjects' detection and recognition of psi) is facilitated by attenuation of externally-directed attention, by "calmness

of mind," and by relaxed vigilance. The points of convergence with yogic descriptions are sufficient to invite more thorough examination. The yoga aphorisms of Patanjali may provide fertile ground for parapsychological hypothesis-testing and construct-building.

One's range of possibilities (reality orientation), according to Vedantic tradition, is determined by the fixation of consciousness. Identification with and absorption in the narrow range of frequencies we call "sensory" has given us control ("power") over phenomena operating within their jurisdiction. The tremendous success of this sensory-fixated concentration-meditation-absorption has led Western man to assume that this frequency range is the spectrum, and that all else is illusion. Similarly, the internally-fixated yogin, whose concentration-meditation-absorption is directed away from the sensory frequencies, comes to accept developing paranormal powers as his reality-base. To him, our phenomenal-sensory world is illusion. Is it possible that the increasing focus of our culture on "occult" practices and consciousness alteration is increasing the frequency of psi detection and recognition?

We tend to think of psi as rare, but perhaps it is only the recognition-detection aspect which is rare. We become conscious of psi when we isolate ourselves from the sensory processes. It is possible that psi is an important mediator of behavior below the level of recognition and detection. Eisenbud, with apologies to Freud, speaks of the "parapsychology of everyday life." Important studies on psi-mediated unconscious behavior have been reported by Stanford and by the Kreitlers. The implications of their findings cannot be underestimated as we go about the business of developing a new paradigm.

Finally, we can learn something from the way the parapsychologist is depicted in the fiction of our culture. He is a private investigator searching for the inexplicable. His role is to witness it, have others witness it, demonstrate its inexplicability, and then "make it go away."

SCIENTIFIC METHOD AND SOCIAL ISSUES IN PARAPSYCHOLOGY

Martin Johnson (University of Utrecht)

As a parapsychologist I have often been asked to give my opinion regarding what implications the existence of psi phenomena may have for philosophy and society. There are a number of issues of speculation, as I would prefer to call them, such as: will psi research lead to a kind of new knowledge as regards the "nature" of man? Will such knowledge have a bearing on the "ultimate" questions like the "destiny of man"? In addition to these two classical "speculative" questions, and very closely related to the second one, is the survival question.

These speculative questions are usually considered to be meaningless and unscientific by most representatives of modern linguistic philosophy, such as Ryle and his followers. Other more specific questions are often asked, such as: will ESP have an impact in the future on such things as space-communication among widely separated spacecraft and between spacecraft and earth? Will PK be used for medical treatment and/or for destructive purposes? What use will be made of precognition in the future by society and by its science and technology?

Several of these questions have been asked for a comparatively long time, but there seems to exist today an enhanced public interest in these matters. It may be relevant to ask about the reasons why the interest may have increased over the last few years. Here I can only formulate a few conjectures. Some contributing factors may be: the diminished impact of institutionalized religion on the individual and society; the insight that man may constitute the greatest threat toward himself and his planet; a decrease in confidence in the scientific enterprise and its claims of being "impartial" and "objective" (this trend toward an anti-intellectual approach has been emphasized especially by some representatives of what usually is identified as the youth movement); and the publicity surrounding Captain Edgar Mitchell's ESP experiment from the Apollo 14 spacecraft.

I do not believe that the increase in public interest in parapsychology could have anything to do with a distinct and specific breakthrough in our field because, to the best

of my knowledge, we have not experienced any such real breakthrough. Is it scientifically sound or legitimate to ask the types of questions that have just been listed? The answer seems to me to be intimately related to whether or not there exists a useful and valid line of demarcation that can be drawn between what is scientific and what is not. To the best of my knowledge there doesn't exist any such perfect instrument that can easily be used. This does not mean that I believe that "anything goes" in science. On the contrary, I believe that at least in the realm of empirical and experimental science, we do have a powerful instrument that can help us to distinguish between scientific and pseudo-scientific hypotheses or statements. This is Karl Popper's criterion of refutability or falsifiability. According to this criterion, every scientific conjecture, assumption, or hypothesis should have test implications and be amenable in principle to a test of refutation.

Within the behavioral sciences it is not easy to formulate a problem in such a way that Popper's criterion can meaningfully be used. Even when it turns out to be possible, we are confronted with other types of problems when we try in a practical way to falsify a hypothesis. As John Beloff recently stated, we are very far from the repeatable experiment in parapsychology. This fact can be interpreted in different ways. One interpretation, which is favorable toward parapsychologists and parapsychology, would be that at present too little is known regarding the necessary and sufficient conditions under which psi phenomena can be demonstrated for it to be possible to test any paradigm within our field for falsification.

Relying on Kant and even more on Popper, I cannot see how psi phenomena could have implications for speculative issues like "does God exist?" "what is the ultimate nature of man?" "will we survive our bodily death?" and so on. The same holds for the problem of whether or not research findings from the field of astronomy or cosmology have implications for questions like "is there a God?" "Does the architecture of the universe speak in favor of the existence of a God or a creator?" "Do quasars and black holes have any implications for the classical speculative questions?"

Like most linguistic philosophers I would feel inclined to say that these questions are meaningless from a scientific point of view. How could their supposed implica-

tions be tested? One should, however, notice that all irrefutable statements or suggestions need not be improbable to the same extent. One should also consider that a problem that even in principle cannot be formulated today in such a way that it can be put to a test of refutation, may become in the future amenable to such a test. Let us, however, admit that the classical speculative questions elicit rather strong emotional reactions in most of us, which is understandable psychologically. If we look at it from an existential point of view, these questions may be terribly relevant. Strong sentiment indicates strong needs. These needs may be of the greatest importance in motivating us to carry out the scientific inquiry. Popper has stated that science starts in myths, and by an almost endless chain of conjectures and refutations develops into more and more efficient and beautiful models or constructs for predictions and explanations.

We should also consider that there is always a phase in the development of science when there is a place for science-fiction-like conjectures. I would guess that this situation is prevalent to a considerable extent in parapsychology today. Finally, one should observe that all philosophers of science do not share my own view of the usefulness of Popper's criterion. What is scientific for Popper is unscientific for Thomas Kuhn, and what Kuhn claims is scientific is unscientific for Popper. Leaving philosophy aside, we note that there exists an interest in parapsychology on the part of society. There are several interesting issues in this connection which can be labelled sociological, such as: why does this interest vary in different countries, societies, and professional groups? and why are some men more tolerant of the psi hypothesis than others? Research on such questions would seem to me quite relevant and interesting.

TO WALK IN THE DARK, KEEP YOUR FEET ON THE GROUND

J. G. Pratt (University of Virginia)

The mission of parapsychology can be characterized in a general way in the same manner as that of any other

branch of science: to study certain aspects of natural phenomena with the purpose of obtaining a better understanding of them. The phenomena with which parapsychology is concerned are very diverse, and a wide range of scientific methods are required for studying them. The present situation is not one in which we could expect agreement among the research workers on what is the high-priority research. Personally, I would be opposed to the assignment of a priority rating to research areas as a policy intended to influence what parapsychologists should do. Our knowledge is still too limited, and the danger is too real that the designation of high priority areas would lead to the neglect of important lines of work that happen to be unfashionable at the time.

Rather, I would place the strongest emphasis upon pursuing different approaches simultaneously and achieving the widest possible coverage of the phenomena that raise parapsychological issues and should therefore receive our attention. This is not to say that every investigator should spread himself thin by diversifying his individual efforts. Indeed, I think it is highly appropriate that each research worker should have his own personal priority objectives. He may be excused if he occasionally states what they are, but the better course would be to let them be recognized from his actions.

I would not like to see the situation develop in which each research worker attempts to influence other active parapsychologists to concentrate their efforts along the particular line or lines that he prefers. The approaches open to parapsychologists fall within three general divisions of the field: the study of spontaneous cases; the experimental investigation of selected subjects with outstanding psi abilities; and the planning and carrying out of experiments with unselected subjects. All three approaches lend themselves to process-oriented research. My own personal preference is for the second of these approaches. I think working with selected subjects has its advantages for me, but I do not claim that it is generally more meritorious than other ways of doing research in parapsychology and that everyone should work in this fashion.

Parapsychologists should stop saying that we do not expect other scientists to take our findings seriously until we have accomplished particular objectives, such as the development of an experiment that can be repeated on

demand, the formulation of a comprehensive theory of psi, the presentation of an adequate explanation of the phenomena, or the like. What we need is to get on with the research. Efforts by the active research workers to anticipate the course or rate of scientific discovery in parapsychology have been singularly unsuccessful. Without pausing to document this statement, I move on to ask: Why should we expect otherwise? Pioneering research such as we are engaged in is an effort to enter and explore intellectual territory where no one has been before. We may speculate on what we are going to find, but we have no reason to be surprised when the reality proves us wrong.

The importance of our problems speaks for itself. Not even our harshest critics have said that the questions we raise are trivial. But I foresee no new direction for the research that could magically bring a quick end to all of our difficulties. Even if we should discover how to get a convincing demonstration of ESP from anyone whenever we ask for it, this would not guarantee a quick understanding of the nature of the process, any more than the universality of memory has eliminated the core mystery about that process. I agree with Dr. Beloff that the major effects of parapsychology are likely to be philosophical rather than technological and that their greatest importance will likely be the bearing of our findings upon the body-mind problem.

THE SUBJECT MATTER OF PARAPSYCHOLOGY

W. G. Roll (Psychical Research Foundation)

Parapsychologists have trouble deciding about the nature of psi phenomena. Our President, Dr. Beloff, even seems uncertain as to whether or not psi phenomena exist. I find it difficult to doubt that ESP and PK occur but it is obvious that we have not determined the nature of psychic phenomena. We do not know what the subject matter of our field really is.

When I came into parapsychology, I expected to explore aspects of human mind and consciousness which reach beyond the physical organism. But in the day-to-day ESP

and PK testing there was little if any talk of mind or consciousness and when there was, such talk seemed superfluous in the context of what we were doing. The same was even true for survival research. In mediumistic studies we were more interested in evidence for continuation of memories after death than with evidence for continuation of consciousness. Psi research did not seem much different from explorations in psychology, biology, physics and so on. More than that I began to wonder if we were actually not working in one or more of these other disciplines. Obviously our methods of research, whether in laboratory or field, together with our statistics, were borrowed from the other sciences. More importantly, it seemed that whenever we made a discovery, we were using the tools of one of the established sciences and we were using the concepts of that science to describe our results. When we found that sheep-goat attitude was related to ESP scores, we were using psychological tools and our finding was essentially a psychological one. The same with moods, personality traits and so on. More recently the encephalograph work is converting our laboratories into psychophysiological ones. Others are becoming animal laboratories. Even such an exotic creature as the poltergeist appears to accommodate itself to the research methods and concepts of established science. It seemed to me that parapsychology was disappearing and that it was disappearing in direct relation to our research successes.

When parapsychology, for better or worse, appeared to fade into the other sciences, there was a new development. In the space of a short time consciousness revived as a topic of research interest in the field. This development did not originate in parapsychology. The interest in consciousness was part of the search for a new foundation on which to base personal and social values in Western culture. In the scientific community this interest was expressed for instance by the EEG studies of Joe Kamiya.

There had earlier been studies of such altered states as dreaming, hypnosis and mediumistic trance as avenues to psi but the emphasis had not been on the aspect of expansion. There were anecdotal accounts, especially from oriental religions, that expanded states of consciousness were associated with psi abilities and that methods to induce such states might stimulate these abilities. But no systematic research had been done. The discovery that different states of consciousness have different physiological

indices and, further, that sensory awareness of these indices could help a person enter such states, made this a scientifically meaningful and promising area of psi exploration.

Other scientists, particularly psychophysiologists and psychologists, are making important contributions to the study of altered states. The unique contribution which parapsychology can make is to explore whether expansion of consciousness is real. It seems to me that we cannot determine this point unless there is evidence either that a person is aware of events at the distant scene to which he feels he has expanded or else evidence that he can interact physically with things there--i. e., unless evidence for ESP or PK is associated with the feeling of expansion. And of course parapsychology is especially equipped to explore whether consciousness can be independent of the body to the extent that it may continue after the body's death.

Some parapsychologists have regarded ESP and PK phenomena as instances where mind interacts with matter. But often the ESP or PK subject is unconscious of the process so a mind terminology has seemed unnecessary. If the psi process can be made conscious, however, mind or consciousness may reappear as something real. Would this be a reality separate from physical reality?

I suspect that instead of mind-matter dualism we shall be talking of mind-matter complementarity. In the same way as a quantum of light sometimes behaves like a particle and at other times like a wave, so the world may appear as consciousness when seen from one point of view and as matter when seen from another point of view. When the world is approached through the sense organs and the instruments which are extensions of the sense organs it is seen as something outside and material. When the same world is approached through consciousness it is seen as something inside and mental. Physics becomes the science which is taking the outer route (and, according to some physicists, is now coming around to the view that the physical world is also a mental world) and parapsychology becomes the science which is taking the inner route.

In the explorations of the world as matter, scientists have been developing physical instruments such as cloud chambers, electron microscopes and so on. For scientists who wish to explore the world as consciousness, an

important tool may be consciousness itself, particularly its wider reaches. Scientists who are capable of personally experiencing such states could gain insights which would not be possible or take longer to reach for scientists who lack this capacity. A recent article in Science by Charles Tart on "state specific sciences" explores the pros and cons of such an approach.

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SUBJECT VARIABLES IN UNSELECTED INDIVIDUALS*

EXTRASENSORY PERCEPTION OF EROTIC AND NON-EROTIC TARGETS AS A FUNCTION OF CONTROLLABILITY OF SUBJECT'S VISUAL IMAGERY, RUN POSITION, AND "AGENT" MOOD

Alan D. Price (Wesleyan University)

Two studies (one a pilot) were undertaken to investigate relationships between GESP and several variables. In the pilot study, 16 unselected subjects attempted, as a group, to reproduce a single target sheet consisting of four runs of erotic symbols (the first letters of five erotic words) alternating with four runs of nonerotic symbols (the standard ESP symbols). The target sheet was prepared by the author, but was concealed from all present during the test. The subjects had previously taken the Gordon Test of Visual Imagery Control, but were unaware of the intention to study the relationship between imagery control and ESP.

The following results were obtained. First, there was a marginally significant overall negative deviation ($P < .05$). The negative deviation for erotic targets was independently significant ($P < .05$), whereas that for nonerotic targets was insignificant; the CRd between target types was also insignificant. Statistics were corrected for the stacking effect. Second, the session score variance of the subjects manifesting uncontrolled visual imagery (autonomous imagers) was significantly greater than that of the subjects exhibiting controlled visual imagery (controlled imagers) at $P < .01$. Third, a three-way analysis of variance of GESP scores (imagery control type by run

*Chairman: R. H. Thouless, SPR, London.

position by target type) with repeated measures on the last two factors disclosed that none of the main effects of the factors was significant, but that the interaction between imagery control type and run position was marginally significant ($P < .035$). This suggested that controlled imagers showed a different scoring pattern across runs than did autonomous imagers.

In Study II, using 14 volunteer subjects, confirmation and extension of these results were sought. A different target sheet, shielded and sealed within an opaque envelope, was used for each subject. The targets were prepared jointly by the author and a graduate student assistant. The author served as the "erotic agent," preparing the erotic targets (four runs, comprised of new symbols), and the assistant served as the "nonerotic agent," preparing the nonerotic targets (four runs of ESP symbols). A fortuitous observation of the assistant's extremely negative mood on the last day of target preparation provided a fourth variable for study: scores on targets prepared on that day were compared with scores on targets prepared on the previous two days, when the assistant was in a neutral mood. Otherwise, the procedure was similar to that used in the pilot study.

Neither overall scoring nor erotic target scoring showed the significant negative deviations found in the pilot study, but it was not hypothesized that they would. All of the hypotheses tested in Study II were either marginally or significantly confirmed, and the results were as follows: (1) autonomous imagers scored significantly below chance on erotic targets ($P < .05$, one-tailed) but not on nonerotic targets; controlled imagers did not deviate significantly from chance on either target type. (2) The differential effect between the scores on the two target types was statistically significant for autonomous imagers ($P < .01$, one-tailed) but not for controlled imagers. (3) A significant interaction was found between imagery control type and run position, as in the pilot study ($P < .009$). (4) Autonomous imagers manifested significantly greater run score variance than controlled imagers ($P < .04$, one-tailed). (5) Autonomous imagers manifested significantly greater session score variance than controlled imagers ($P < .05$, one-tailed). (6) The mood state of the nonerotic agent differentially affected GESP scores ($P < .007$).

Hypotheses (2) and (3) were tested by means of a

three-way analysis of variance (imagery control type by run position by target type) with repeated measures on the last two factors. Hypothesis (6) was tested by means of an additional three-way analysis of variance (nonerotic agent mood by run position by target type) with repeated measures on the last two factors. The probability value reported for Hypothesis (6) refers to the interaction between nonerotic agent mood and target type. If this value is corrected for selection because of the generality of the hypothesis, it is still marginally significant ($P < .021$). As a result of finding this interaction, an analysis was made of simple main effects of mood and target type. For the nonerotic targets, there was a significant difference between the GESP scores associated with the two mood states of the nonerotic agent ($P < .009$). A state of negative affect, egocentricity, and hyperdistractibility (negative mood) produced a mean run score of 6.33, while an affectively neutral, task-oriented state (neutral mood) produced a mean of 4.89. For the erotic targets, the difference was in the opposite direction, with both means insignificantly below chance. When target types were compared within each mood group, the means associated with the negative mood showed a highly significant differential effect ($P < .0015$). These run score means for nonerotic and erotic targets were 6.33 and 4.25, respectively. For the neutral-mood targets the difference was insignificant.

The interaction between imagery control type and run position found in both studies was taken as evidence for a "differential position effect" between groups differing in controllability of visual imagery. In addition, the data of both studies suggested that imagery control type was predictive of variability in GESP scores, as anticipated from studies of imagery control and other "normal" cognitive processes. Finally, the striking, serendipitous finding of a mood effect in Study II associated with the assistant gives support to the notion that the intrapsychic processes of persons connected with an experiment, other than the subjects who provide the raw data, may be crucially involved in the dynamics of the ESP system.

THE PSI QUIZ: A NEW ESP TEST

John A. Freeman (Campbell College, Buies Creek, N. C.)

In an effort to design an ESP test that would be interesting and challenging to the subject, and at the same time would enable me to study psi differential effects, I developed the Psi Quiz. This precognitive test somewhat resembles an intelligence test in that it comprises a series of questions that require some degree of reasoning, memory, or mental calculation. There are 25 questions in all; each is followed by either five or 10 locations for an answer. I will list three of the questions to illustrate. (1) "If one pound troy weight is greater than one pound avoirdupois, put an X in any of the five circles; otherwise put two dots in any one of the circles" (five circles follow). (2) "If the square root of 64 is less than the square root of 25 multiplied by 2, put a circle in one of the five squares. If it is equal to or more than this number, put a square in one of the five circles" (five squares and five circles follow). (3) "If it is snowing, the temperature is below 40 degrees. The temperature is below 40 degrees. Therefore it is snowing. Circle one of the five T's if the last statement is logically true, or one of the five F's if it is false" (five T's and five F's follow).

In those cases in which 10 answer locations are provided, they are grouped for analysis into five pairs, such that the first T following Question (3) is paired with the first F, for example. The ESP task consists of selecting the correct one of the five locations (or one of the five location-pairs) for each question. Each question constitutes a trial. It is possible, therefore, for a subject to fail on the intellectual aspect of the trial, but still make an ESP hit. Alternatively, he may answer the question correctly but miss the ESP target, miss both, or get both correct. The entire 25-item test constitutes one run.

The Psi Quiz was first administered to the members of the Psychology Club of a local, predominantly Negro, university. I spoke to the group for 20 to 30 minutes on parapsychology, answered their questions, and then distributed the test to them while explaining the directions. After they had completed the tests, I took them to the Institute for Parapsychology, FRNM, under whose auspices the experiment was conducted. There the tests were

alphabetized and duplicated. Two people were involved in finding an entry point in a random number table used to select the targets, and also in scoring the data. Each subject had a different set of targets. The overall results were not statistically significant ($P = .08$), but there was a trend for subjects who missed (intellectually) more questions than the average for the class to score above mean chance expectation on questions they answered correctly, and below mean chance expectation on questions they missed. On the other hand, the subjects who answered more questions correctly (intellectually) than the average for the class scored positively on questions they missed and negatively on questions they answered correctly. Although not significant, this trend showed suggestive consistency across subjects. I therefore predicted that this differential effect would appear in a second group.

Forty-one students in a local business college class were the subjects of the second study, using the same procedure as before. They completed a total of 40.76 runs (not every subject completed the test) with an overall positive deviation of 14.2. I separated the students into those above and those below the mean number of questions answered correctly for the class. Those who were above the mean had the equivalent of 14 runs with a deviation of -6 on questions answered correctly, and the equivalent of 4.84 runs with a deviation of +12.8 on questions missed. Those who were below the mean had 12.84 runs with a deviation of +13.8 on questions answered correctly, and 9.08 runs with a deviation of -6.4 on questions missed. When both groups were combined, the 17.68 predicted positive runs had a deviation of +26.6, and the 23.08 predicted negative runs had a deviation of -12.4. The difference between predicted positive and predicted negative runs was highly significant ($CRd = 3.24$; $P = .001$, two-tailed). An analysis for subject consistency was also significant ($P < .01$), indicating that the effect was generalized throughout the group.

The interpretation of these results can only be speculative at this stage. Why did students who answered more questions correctly than the average for the class score negatively on questions correctly answered? Perhaps because they were so preoccupied with getting the intellectual aspect right that their preoccupation inhibited ESP performance. These same students scored positively on questions they missed, which may indicate that ESP could play the

part of a compensatory mechanism. On the other hand, the students who did poorly on the intellectual task may have realized their performance would "show them up"; when they came to a question they could not answer, their frustration was compounded and psi-missing occurred. However, when they felt confident about being able to answer the question, this could have been such a relief that ESP was able to operate in a positive manner. Whatever the correct interpretation of these results may be, I have at least introduced a new testing technique which may be more involving for subjects than those currently in use.

TELEPATHIC INFORMATION TRANSFER OF EMOTIONAL DATA

Howard Eisenberg (Sunnybrook Hospital, Toronto)

I conducted a telepathy experiment using a multivariate design which employed a novel stimulus medium to generate potent vicarious emotions. Forty-eight unselected volunteer subjects, ranging in age from 14 to 44, were arbitrarily divided into "senders" and "receivers." Each sender was paired with a receiver, and each pair was tested individually. Some of these sender-receiver pairs were tested during the day (Alpha group) and some at night (Beta group). To permit an examination of possible experimenter effect, different assistants were used in the Alpha and Beta groups. Each sender-receiver pair did a maximum of seven experimental sessions on seven different days. All subjects gave pretest arousal, confidence, and affect ratings at the beginning of each session. In addition, they completed an ESP belief rating scale and took four personality tests: the Myers-Briggs Type Indicator, the Eysenck Personality Inventory, Byrne's Repression-Sensitization Scale, and Allport's Study of Values.

The target material consisted of seven 6.5-minute excerpts from different motion-picture films with accompanying sound tracks, which were selected for their poignant emotionally stimulating character. One of the films was shown in each of the seven sessions, the presentation order being individually randomized for each sender-receiver pair. The experimenter assigned to the receivers did not know these presentation sequences.

During each session, the sender and receiver were located in different rooms, separated horizontally by about 250 feet and several concrete walls, and vertically by one storey. While the sender was being exposed to the motion-picture stimulus, the receiver was asked to relax and free-associate. Following this, both sender and receiver individually completed several response indicators describing their experiences during the stimulation period: these included written descriptions of their free-associations as well as detailed questionnaires; they also ranked a deck of five photographs in order of approximation to their predominant experience during the stimulation period, and described their rationale for each particular rank assignment. The picture-ranking procedure was used to statistically test for telepathic transfer. There was a separate deck of five photographs for each of the seven motion-picture films; unknown to the receivers, one photo in each deck (the target photo) was actually taken from the corresponding target film. These photo decks were prepared by an elaborate procedure to insure that the target photo would not appear more attractive than the four non-target photos.

There were 151 complete sessions, 62 in the Alpha group and 89 in the Beta group. The hypothesis that valid telepathic information transfer would occur was tested across subjects by comparing the obtained number of correct correspondences between the receivers' first-ranked photo and the actual target photo with chance expectation, using the CR. Overall scoring combining the Alpha and Beta groups was extremely high ($P < .000073$), though only the Beta group was independently significant ($P < .000003$). An additional evaluation used subjects rather than trials as the unit of analysis, and consisted of a one-tailed t-test with data normalized by arcsin transformations. This also showed significantly high scoring for the total group ($P < .005$) and for the Beta group alone ($P < .0005$). The Alpha group by itself was nonsignificant with respect to both analyses.

To test for the presence of a significant correspondence between the rankings of the four supposedly "non-target" photos by senders and their respective receivers in those sessions where the target photo was correctly ranked first, Kendall's tau coefficient of rank correlation was employed. A two-tailed t-test was then used to evaluate the obtained tau, which was significant for the Beta group

only, at $P < .02$. This provided independent confirmation of the psi hypothesis and also demonstrated that more information was transferred than had originally been expected.

ESP belief could not be tested as a direct correlate of experimental psi performance, as it was excessively negatively skewed in the Beta group. However, a two-tailed t-test between the ESP belief ratings of the Alpha and Beta groups was significant for senders in the expected direction at $P < .01$, an indirect indication that a relationship might exist. Several personality and attitude measures, when tested by discriminant analysis and regression statistics, were significantly associated with successful senders, but not with receivers. Finally, qualitative analysis of the written subjective experience descriptions revealed several remarkable correspondences between senders and their paired receivers, though for the most part these descriptions were characterized by a high signal-to-noise ratio and abundant transformational distortions.

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COGNITIVE CORRELATES OF PSI PERFORMANCE*

SHIFTS IN SUBJECTIVE STATE AND GESP: A PRELIMINARY STUDY UNDER CONDITIONS OF PARTIAL SENSORY DEPRIVATION

Charles Honorton†, Sally Drucker, and Harry Hermon
(Maimonides Medical Center)

In previous studies, C. H. has found a relationship between magnitude of directional shift in state of consciousness, measured by subjects' self-reports, and success in the ESP task. In this study a new method of altering subjective state was tried. Thirty volunteer subjects (12 males and 18 females) participated in one 30-minute session each, involving partial sensory deprivation. Isolation was achieved by having the subjects suspended in a swing-like cradle, standing upright, and immobilized by straps. Sound-attenuating headphones and a lightproof blindfold were used to greatly reduce visual and auditory stimulation. The cradle, commonly called a "witches' cradle," was maintained in rotating motion by involuntary movements on the part of the subject. Subjects were tested individually. We hypothesized that subjects whose state was substantially altered by this procedure would produce higher GESP scores.

Shifts in subjective state were assessed by a state report scale which C. H. had developed and used in previous research. It ranged from zero, indicating a normally alert and awake condition, to four, indicating a profoundly altered state in which the subject considers himself oblivious to external surroundings. The subject was instructed to call out the number corresponding to his internal state. Through-

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out the session, these state reports were elicited at five-minute intervals. During the last 10 minutes of the session an agent, in another room, concentrated on a randomly-selected target picture. When the 30-minute period was over, the subject reported the imagery he had experienced while in the sensory deprivation condition. He was then given eight pictures, one of which was the target picture, and asked to rank their degree of correspondence to his imagery. Subjects who gave the target picture ranks of 1 to 4 (better match) got a hit; those who ranked it 5 to 8 got a miss. Chance expectation for the 30 subjects was thus 15 hits and 15 misses.

While the overall results were not significant (19 hits and 11 misses), the hypothesized relationship between GESP success and state reports was confirmed. Subjects with overall high (above mean) state reports obtained 13 hits and four misses (76.4 percent hits), which was significant at $P < .025$ (one-tailed). Subjects with overall low (below mean) state reports produced six hits and seven misses (46.2 percent hits), which was not significant. Mean shift in state of all subjects from the first to the last 10-minute period was +0.83. Subjects with a high degree of shift obtained 13 hits and three misses (81.2 percent hits), significant at $P < .011$ (one-tailed). Subjects with a low degree of shift obtained six hits and eight misses (42.8 percent hits). Subjects who got hits had a mean state shift of +1.16, while those who missed had a mean shift of +0.45. The difference between these two groups was significant at $P = .05$, one-tailed (Mann-Whitney U Test).

This is the third study C. H. has reported using state reports to assess the effect of consciousness-manipulating techniques on ESP score. Each study has involved different techniques for manipulating consciousness (biofeedback, hypnosis, sensory isolation), and different ESP tasks (clairvoyant card-guessing, clairvoyant and GESP free response methods). In each study the same relationship was found between ESP and state report measures. We believe that further confirmation and extension of these findings may lead to the following empirical generalizations about the ESP process: (1) ESP success will be greater under conditions which are associated with attenuation of external sensory stimuli. (2) Strong shifts from external to internal stimuli will be associated with greater ESP success than small shifts. (3) Hypnosis, EEG biofeedback,

and partial sensory deprivation will augment ESP performance to the extent that they are associated with attenuation of external stimuli and shifts from externally-directed to internally-directed attentive activity.

EFFECTS OF FEEDBACK ON DISCRIMINATION BETWEEN CORRECT AND INCORRECT ESP RESPONSES: A FURTHER REPLICATION AND EXTENSION

Ellendale McCollam and Charles Honorton† (Maimonides Medical Center)

Two studies have been reported by C. H. in which subjects were asked to indicate the ESP calls they felt relatively confident about in a given run. These were termed confidence calls. In both studies, the subjects completed three runs of DT clairvoyance with ESP cards while making confidence calls (prefeedback condition), three runs with immediate feedback to successful responses (feedback condition), and three additional DT runs with confidence calls (postfeedback condition). The hypothesis was that subjects would show significant increments in their proportion of correct confidence calls (confidence hits) following feedback. This hypothesis was confirmed in both studies. Further, subjects in the first study showed significant increments in overall scoring following feedback. This was not found in the second study. Both studies incorporated control groups using subjects who underwent the same procedure but were given false feedback (were told incorrect responses were correct) instead of correct feedback. As expected, the control subjects did not show significant increments in either confidence hits or overall scoring.

The present investigation represents an attempt to replicate the above findings with a different experimenter, assess the effects of different feedback contingencies upon the subjects' success, and explore subjective concomitants of confidence hits. The four experimental groups used differed only in the amount of feedback given during the feedback condition. Fourteen subjects were assigned to each group. Group 1 received no feedback, Group 2 received three runs of feedback, Group 3 received six runs of feedback, and Group 4 received nine runs of feedback.

Group 2 was considered the replication group, since it employed the same number of feedback runs as had the previous studies. Our hypothesis was directed to this group in particular.

All subjects were tested individually and received the same instructions used in the previous studies. They did three runs of DT clairvoyance with confidence calls prior to and following the feedback condition. They were instructed to make from three to seven confidence calls per run. The target material consisted of standard closed decks of ESP cards which had been shuffled and enclosed in opaque containers by C. H. prior to each subject's arrival in the laboratory. Following the postfeedback condition, the experimenter (E. M.) conducted an interview with the subject in order to ascertain subjective criteria used in making confidence calls. Two ESP measures were used. The confidence hit score represented the difference between the proportion of correct confidence calls in the pre- and postfeedback conditions. The score was positive if the postfeedback level was higher. The overall score represented the difference in number of hits in the pre- and postfeedback conditions. Again, the score was positive if the postfeedback level was higher.

Significant overall scoring was obtained only in the replication group, Group 2. In this group, 13 out of 14 subjects obtained higher overall scores in the postfeedback condition ($P < .01$, Wilcoxon matched-pairs, signed-ranks test), and the postfeedback condition yielded independently significant psi-hitting ($CR = 2.93$; $P = .0034$). A one-way analysis of variance performed on the pre- to postfeedback difference scores for the four experimental groups was significant ($F = 5.72$, $df = 3$ and 52; $P < .01$), indicating that the groups differed in amount of change. When Group 2 was paired with each of the other groups, it showed significantly greater scoring increments than all of them. The greatest difference was between Group 2 and Group 1, which had received no feedback.

When we examined the confidence hit scores we found that, once again, significant effects were present only in the replication group. In this group, 11 out of 14 subjects obtained larger proportions of confidence hits in the postfeedback condition than in the prefeedback condition, and the difference was significant ($P < .01$, Wilcoxon test). The original effect was thus replicated again. A one-way analysis

of variance performed on the pre- to postfeedback difference scores for the four experimental groups was not significant ($F = 1.54$, $df = 3$ and 52). When Group 2 was paired with each of the others, we found the greatest difference between it and Group 1, as had been true for overall scoring. Of the 42 subjects in the three groups receiving feedback, 30 provided responses to the interview questions dealing with how they perceived confidence calls as different from other calls. Those who reported using visual imagery as their criterion for making confidence calls showed no increase in proportion of confidence hits pre- to postfeedback; those who reported using intuitive or multiple-modality cues showed a nonsignificant increase in confidence hits; and those who could describe no criteria showed a nonsignificant decrease in confidence hits.

The original hypothesis has now been confirmed in three studies with two different experimenters in the same laboratory. The logical next step would be to see if it can now be replicated independently in another laboratory. The lack of correspondingly greater increments with more feedback is difficult to explain if the observed effects involve learning. Although it is possible to postulate some type of inhibitory mechanism post hoc, only further research can clarify the nature of the effects. The subjective material relating to confidence hits, while provocative, must be viewed as exploratory and needs to be replicated. If successful replications are forthcoming, the implication that confidence hits represent true discrimination rather than secondary ESP effects will be difficult to resist. The fact that overall scoring increments following three runs of feedback have occurred in two of the three studies, in addition to the confidence hit increments observed in all three, leads us to be optimistic about the utility of this design.

ALPHA RHYTHM AND ESP IN A FREE RESPONSE SITUATION

K. Ramakrishna Rao† (Andhra University) and José Feola (University of Minnesota)

Several studies have shown relationships between amount of EEG alpha rhythm, associated with a relaxed

state of mind, and level of ESP scoring. Most of these have used between-subject comparisons. In the present study we hypothesized that a relatively high alpha state would be conducive to psi-hitting and a relatively low alpha state would cause psi-missing within the same subject. We tested this hypothesis by comparing the ESP scores of one subject when he was in an induced high-alpha condition with those obtained in an induced low-alpha condition, with these conditions being presented in a random sequence.

The subject, a painter, had practiced Hindu meditation for 25 years. He had been given alpha feedback training by Dr. Joe Kamiya and proved to be successful at controlling his level of alpha production. The experiment was done at Kamiya's laboratory at Langley Porter Institute in San Francisco in 1969. A double-blind procedure was followed. The agent (J. F.) sat on the second floor of the laboratory. He held a set of eight target pictures (cut from magazines) which had been enclosed in opaque envelopes and randomized by shuffling before the experiment. The subject was seated in a room in the basement of the laboratory and was hooked up to a Beckman Type R Dynograph. As soon as K. R. R. gave the signal for the first trial, the agent opened the envelope numbered one. He looked at the picture and thought about it until the signal to stop was given. When given the next signal to begin, he proceeded to the second envelope, and so on. The experiment was conducted on two successive mornings, with eight trials each morning, using different sets of pictures.

After receiving a few minutes of alpha feedback at the start of the session, using a reinforcing tone, the subject was asked during each of the eight trials to produce a high or a low level of alpha according to a previously prepared random list. The EEG recording of the subject's alpha activity was taken for a period of two minutes, coinciding with the agent's period of concentration. At the completion of each two-minute period, the agent was signalled to stop looking at the picture and relax, and an assistant, usually Kamiya, went into the subject's room and asked him to report what had gone through his mind. The subject's report was tape-recorded. He was then given alpha feedback for about five minutes before proceeding to the next trial. During the first day there were five high-trials (attempts to induce a high-alpha state) and three low-trials (attempts to induce a low-alpha state). On the second day, there were five low-trials and three high-trials.

The agent did not know whether a given trial was low or high.

At the conclusion of the eight trials on each of the two days, Kamiya transcribed the reports of the subject for that day onto index cards. He then randomized these cards, gave them code numbers, kept the code with him, and gave the cards to K. R. R. The agent was then led to the basement and the subject was taken to the second floor where he was given the eight reports and the eight pictures. After he had read all the reports and observed all the pictures, he selected one report and picked out the picture which he thought gave the best correspondence. That picture was given a score of eight for that report. He then selected the next most likely picture (given a score of seven), and so on until the eighth picture (scored a one). The subject then selected another report and did the same until all reports had been matched with all pictures. He was told that he was free to pick out the same picture as the best match for more than one report.

After the subject completed the matching on the first day, the agent was brought back to the second floor and the subject was asked to wait outside. Following the same procedure the agent also did the matching. Then Kamiya and K. R. R. together decoded the pictures and the reports into their original order. The subject's score for a given report-picture pairing was added to the agent's score, and the sum of scores for the correct report-picture pairs was compared between high-trials and low-trials. This procedure was repeated the next day.

When both days' results were combined, the sum of ranks for the eight correct report-picture pairs during the high-alpha state was found to be 78 as compared to 54 for the low-alpha condition, indicating better ESP performance in the high-alpha state. The average scores of the high-trials and the low-trials were 4.875 and 3.375 respectively, the theoretical mean in either case being 4.5. The CR of the difference between them was 1.85 ($P < .05$, one-tailed). The results were also analyzed independently for each of the two days. The high-alpha trials for the first day had an average score of 4.70 and the low-alpha trials an average of 2.00 (CRd = 2.29; P = .01). On the second day the mean score for high-alpha trials was 5.17 and for low-alpha trials 4.20. The difference between the two was not significant. When agent and subject scores were analyzed

separately, they were found to contribute equally to the high-low alpha differential. This finding argues against the possible criticism that the subject's awareness of which trials were high-alpha and which were low-alpha might have influenced his ratings. A final check was made on the actual alpha level during the two conditions. As we had hoped, the high-trials indeed showed more alpha than the low-trials ($P < .02$).

14

MOOD, MOTIVATION, MEDITATION, AND ESP*

VALIDATING RESEARCH ON A MOOD-ADJECTIVE SCALE FOR PREDICTING RUN-SCORE VARIANCE

James C. Carpenter (University of North Carolina, Chapel Hill)

Following preliminary investigations, I have done five studies using an empirically derived scale of mood-adjectives designed to predict precognition run-score variance. The first study was an effort to validate the scale in a population and with a task somewhat different from those from which the scale had been derived. The others attempted to heighten the predictive power of the scale by considering what have come lately in personality research to be called "moderating variables." As used here, variance refers to the tendency of individual run-scores to depart in either direction from the theoretical central tendency as defined by the binomial theorem. Several studies by David Rogers, my wife, and myself seemed to suggest that, in precognition guessing, fresh and energetic states of mind appeared to favor large variance, while bored or fatigued states favored small variance.

I therefore selected some mood-adjectives which had been found by Nowlis to discriminate elated and sedated states. These were combined with some filler adjectives to form a mood-adjective check list (MACL). The subject is instructed simply to check those adjectives that describe his mood at the moment. I have used the same basic experimental procedure in all my studies. Each subject is given a packet of ESP record blanks with space for five precognition runs of 24 (sometimes 25) calls each. Stapled

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behind each five-run blank is an MACL. The subject is asked to do the runs at times of his own choosing, picking a time when he can do all five without interruption, and filling out the MACL immediately after finishing the sitting. When all sittings are done, he is to return the packet to me. When all packets are returned, targets are taken from a random number book with an entry point derived from weather information.

The results of the first preliminary study showed that a simple separation of MACL scores into "positive" and "negative" moods did not significantly discriminate run-score variance. However, when the MACL scores were divided into "moderate" and "extreme" scores, moderate positive vs. negative scores discriminated as expected, while extreme scores discriminated in the opposite direction. Two other preliminary studies were carried out to cross-validate this rather complex method of scoring, and the results obtained were, indeed, replicative.

However, at this point I was hoping to find a more direct, less arbitrary, and more powerful way to treat the MACL scores. I pooled all data from these three preliminary studies (representing 74 graduate or undergraduate students at Ohio State University), and subjected them to an empirical test-construction technique called the Wherry Test-Selection Method. This is a version of a stepwise multiple regression analysis, which selects items on the basis of their independent predictive relation to a criterion. Three items predicted positively to run-score variance: aloof, fearless, and lazy; while the following eight predicted negatively: adaptable, amiable, carefree, downhearted, drifting, intoxicated, task-involved, and tired. I decided to use these 11 items, equally weighted, to predict large vs. small run-score variance in further studies.

The first attempt at validation of this scale was carried out in a situation different in at least three respects from that in which the original data had been collected. The subjects were younger, being high school juniors. The targets were binary (the designs 0 and +), while previously the five standard ESP symbols had been used. Finally, all guessing, unknown to the subjects, was done at a single list of targets as opposed to the individualized target lists used previously. Seventeen subjects did four sittings of five runs each. The two extreme quartiles of the MACL data were used in predicting large vs. small variance in

the corresponding ESP data. The predicted large variance data showed a variance not significantly different from chance, the predicted small variance data showed a smaller variance than chance, and the difference between them was significant ($P < .05$).

Experiments 2 and 3 explored the possibility of enhancing the predicted variance effects by using binary targets with highly salient personal meaning for the subject. The Kelly Role Construct Personality Inventory was used to elicit from each subject an array of role-titles and personal constructs which, when factored, could provide a picture of that person's main cognitive dimensions by which he structures his interpersonal experience. In this test the subject supplies names of persons he knows and rates them on his own construct dimensions. The first (main) factor was extracted from each array, and the two persons most strongly representative of the two opposite poles of the dimension were taken as the personally salient targets for that subject. The neutral targets were + and 0. Experiment 2 involved one subject who was a recent high school graduate with a strong interest in ESP. She performed 12 sittings (five runs each) of precognitive guesses using the salient targets, and six using the neutral targets. The variance prediction was confirmed: the obtained F-ratio of predicted large to predicted small variance (PL/PS) for all runs pooled was marginally significant ($P < .10$), while that for salient targets only was stronger ($P < .05$).

Experiment 3 employed four subjects (all University of North Carolina students in my personality class); three subjects did three sittings each of salient and neutral targets, and the fourth did two of each. Using extreme-quartile scoring of the MACL there were no salient-target sittings with a predicted large variance. Because of this, a median MACL split was used. The PL/PS F-ratio for all runs pooled was not significant, although it was in the expected direction, while the F-ratio for salient targets only was marginally significant ($P < .10$).

Following a suggestion of Schmeidler that mood and attitude variables should be studied in interaction, and noting that the great majority of the original 74 subjects who had generated the MACL scale had been sheep, I decided to conduct an experiment to investigate the possibility that the scale should be more predictive of run-score variance for the performances of sheep than for those

of goats. Twenty-five subjects (University of North Carolina undergraduates) were tested; 15 were sheep and 10 were goats in terms of the question: "Do you believe ESP is possible given the conditions of this experiment?" Each subject did four sittings of five runs each. The PL/PS F-ratio for all subjects pooled was not significant. For the sheep alone the predicted effect occurred at a marginal level ($P < .10$), while the goats showed a reversal of the predicted effect at an approximately equivalent strength.

Thayer has reported finding that authoritarianism, as measured by the California F-scale, was a significant discriminator of a subject's ability to be an accurate internal observer in an alpha wave conditioning situation. High-authoritarian subjects did poorly. Reasoning that the MACL requires internal observation and report, I did a fifth experiment using three groups of subjects in which F-scales were completed along with the sheep-goat question and four sittings of five runs each.

The PL/PS F-ratio of all subjects pooled was significant ($P < .005$). This time the sheep-goat division did not increase the power of the discrimination. In fact, the F-ratio for goats was somewhat (but not significantly) larger than that for sheep. The F-scale, however, was a powerful discriminator. Using a median split, low-authoritarian subjects produced a PL/PS F-ratio that was quite significant ($P < .001$). Among the low-authoritarian subjects, the sheep did produce a somewhat stronger effect than the goats ($P < .001$ as opposed to $P < .025$). The high-authoritarian subjects showed no significant variance effect, although the sheep did show an insignificant trend toward reversal of the effect, and goats a slight trend toward the predicted effect. Investigation of each group's data separately showed that the superior predictability of low-authoritarian subjects was borne out consistently.

MEDITATION AND ESP SCORING

Hamlyn Dukhan and K. Ramakrishna Rao† (Andhra University)

The object of this investigation was to explore the relationship between the practice of yogic meditation and

ESP scoring in the context of two commonly-held assumptions. First, it is believed that the practice of meditation and yoga exercise help to control the fluctuations of the psyche, which in turn results in enhanced psi ability. Second, the practitioner is warned that his paranormal abilities stand in the way of his spiritual or yogic development and that therefore he should scrupulously avoid using them. By testing subjects at two stages of yogic training, we could investigate the extent and relative strength of these two possible influences on ESP performance.

The term "meditation" in this study refers to a combination of several aspects of yoga practice which enable the subjects to attain a meditative state. These are: Mandala Gazing, Pranayama (breath control), Laya Yoga (kundalini arousal), Japa Yoga (mantra chanting), and Karma Yoga (performance of specified duties as a means to self realization). The investigation comprised a pilot experiment and two confirmatory experiments using as subjects students of the Ananda Ashram in South India. They were done between December 1971 and April 1972.

The pilot experiment illustrates the general procedure used in all three studies. The 27 subjects were classified as juniors or seniors. A junior subject was one who had been recently inducted into yoga practice or who had had no intensive training in yoga. A senior subject was one who had already attained some experience in yoga and a fair level of yogic development. All subjects were Westerners attending a residential Yoga Teacher Training Course conducted by the Guru of the Ananda Ashram, Swami Gitananda, at the Pondicherry branch. They had volunteered to be subjects.

The experimenter (H. D.) resided in the ashram through the entire investigation. It was agreed that each subject would do eight rounds of testing, each consisting of a pre-meditation session and a post-meditation session. Each subject thus served as his own control. The subjects decided what meditative approach they would use on a particular day. They were divided equally into three groups (A, B, and C) which were tested in that order, one group each day, until the eight rounds for each group were completed. (Three subjects completed seven rounds each and one subject only six rounds.) Subjects were tested individually and, on the average, once every three days.

Two clairvoyant ESP tests were given during the pre- and post-meditation sessions; a picture target test and a blind matching test using standard ESP cards. Only the results of the latter will be reported here. It consisted of two double runs (four standard ESP runs) in each session. The testing was done in a closed room. The experimenter and subject sat facing each other on a straw mat on the floor. The experimenter used a high table situated at the back of the subject to insert the target cards into black opaque envelopes before each double run. This manipulation was completely out of the range of vision of the subjects. Checking and recording of hits were done by the experimenter in the presence of the subject. When the experimenter returned from Pondicherry all the score sheets were rechecked by K. R. R. and his assistants.

The 15 junior subjects did 464 standard ESP runs of 25 trials each during the pre-meditation session and an equal number of runs in the post-meditation session. They obtained 2,198 hits (-122) in the pre-meditation and 2,411 hits (+91) in the post-meditation session. The difference was significant ($t = 2.97$; $P < .01$). The 12 senior subjects did 376 runs before and 376 runs after meditation and scored 1,891 (+11) and 1,860 (-20) hits respectively, an insignificant difference. From these results it appeared that the junior group tended to obtain better ESP scores during the post-meditation session. We wondered whether the failure of the senior group to show this effect might have been due to their greater awareness of the stigma attached to the acquisition of paranormal powers by yogis.

Two confirmatory experiments were subsequently carried out. The first was again conducted at the Pondicherry branch of the ashram, and used the same procedure as before. The nine seniors were originally the juniors of the pilot experiment. The nine juniors were new students at the ashram. Again, all the subjects were Westerners. Testing of each group was done on alternate days until the eight sessions were completed. The junior subjects obtained 1,292 hits (-148) in 288 pre-meditation runs and 1,580 hits (+140) in 288 post-meditation runs ($t = 7.57$; $P < .001$). The seniors obtained 1,345 hits (-75) and 1,501 hits (+81) in 284 pre- and 284 post-meditation runs respectively ($t = 3.92$; $P < .01$). In this study, then, we found both the junior and senior groups obtaining significantly better scores after meditation.

Since the seniors in this experiment were from the junior group of the pilot experiment, we needed to see whether a fresh batch of seniors would show the same effect.

The second confirmatory study took us to the Bangalore branch of the Ananda Ashram, also taught by Swami Gitananda. This time, the subjects were all Indians and practicing Hindus. They did not live at the ashram; most of them were working people, and lived in their homes. All 14 subjects (seven seniors and seven juniors) were tested daily, usually after work. Testing was done in the Mandir where there was adequate provision for meditation. The junior subjects obtained 998 hits (-82) in 216 pre-meditation and 1,127 hits (+47) in 216 post-meditation runs ($\underline{t}$ = 2.62; $P < .05$). The senior subjects did 224 runs before and 224 runs after meditation and obtained 1,043 hits (-77) and 1,235 hits (+115) respectively ($\underline{t}$ = 4.13; $P < .01$). Thus this group, like all the previous ones except the seniors in the pilot experiment, supported the hypothesis that meditation facilitates high ESP scoring. Only the pilot study seniors, with their chance-level scoring, lent support to the hypothesis that yogic practitioners avoid using their psi.

THE ROLE OF TEST PREPAREDNESS IN PSI EXPERIMENTS

P. Sailaja and K. Ramakrishna Rao† (Andhra University)

We have reported in two previous studies that subjects obtained more hits in an ESP test given immediately following a scheduled interview that would determine something important to them than in a test given just before the interview. This superior performance in the post-interview session may be explained by any of four hypotheses. (A) For successful ESP performance the subject needs to be psychologically at ease and devoid of tension and anxiety. The subjects who came for the interview were anxious and tense during the pre-interview session and thus psi-missed. In the post-interview period they were relatively more at ease and therefore scored higher. (B) A set of psychological preparedness is necessary for successful ESP performance; when it is lacking the subject tends to psi-miss. The subjects had not been told in advance that they had to take an ESP test during the pre-interview session, so they psi-

missed. They were prepared, however, during the post-interview session, and therefore psi-hit. (C) The superior performance in the post-interview test may have been an effect of learning because it always came later than the pre-interview test. (D) The observed results might have been caused by the experimenters' expectation.

The object of the present study was to find out which of these hypotheses most appropriately explained the observed differential scoring. Our design differed from that used previously in three important respects. First, the subjects were tested in only one session, either before or after the interview. This eliminated the learning hypothesis. Second, the experimenters did not know whether the subject had been interviewed prior to testing, or to which experimental group he belonged. Thus the effect of experimenter expectation was controlled. Third, the subjects were divided into three groups.

The subjects in Group I, like the subjects in the previous experiments, lacked a set of test preparedness. They were not told of the test in advance, and were tested as soon as they arrived for the interview. The subjects in Groups II and III were told in advance that they had to take a psychology test in connection with their interview. Therefore we assumed that they came with a set of psychological preparedness to take such a test. The subjects in Groups II and III differed in that the former were interviewed after the ESP test whereas the latter were interviewed before the ESP test. If the tension of the impending interview was a key factor then the performance of Groups II and III should be significantly different. If a set of psychological preparedness was crucial, as we felt it probably was, then Groups II and III should both perform significantly better than Group I. We predicted, in accordance with this last hypothesis, that Group I would psi-miss and Groups II and III would psi-hit.

All subjects, as in the previous studies, were applicants for admission to psychology and library science courses at Andhra University. K. R. R. conducted the interview, and P. S. and B. K. Kanthamani administered the ESP test, which was a blind matching task using a deck of 50 ESP cards. Each subject did two runs of 50 trials each. There were 21 subjects in Group I, 23 in Group II, and six in Group III. (We were unable to have comparable group sizes because a great many subjects in Group III did not arrive

for the interview.) Group I obtained 397 hits (-23) and averaged 9.45 hits per run of 50 trials. Group II scored 494 hits (+34), an average of 10.74 hits per 50 trials. A t test of the difference between Groups I and II gave a t of 2.15 ($P < .05$). This confirmed our expectation that subjects who took the ESP test with a set of test preparedness would perform better than those without such a set.

The six subjects in Group III obtained a total of 132 hits (+12) and averaged 11 hits per 50 trials, which was slightly higher than the average of Group II. Both Groups II and III scored positively and there was no significant difference between them. Together they obtained 626 hits (+46) in 58 runs, an average of 10.79. The difference between this combined score and Group I's score was marginally significant ($t = 2.37$; $P < .05$). It is interesting to note that the combined score of Groups II and III was independently significant ($CR = 2.14$; $P < .05$).

These results clearly suggest that if an applicant is prepared for a psychology test, and then given an ESP test, he will tend to score positively. This seems to be true whether he is interviewed before or after the test. His assumed state of tension and anxiety about the impending interview cannot therefore be an adequate explanation of his psi-missing in the pre-interview tests of our previous experiments. It is likely that the differential effect we found was due to the presence of a set of test preparedness during the post-interview period and the lack of such a set during the pre-interview period.

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EXPERIMENTS WITH ANIMALS*

THE EFFECT OF CONTINGENCY ON PRECOGNITION IN THE RAT

James G. Craig (University of Waterloo, Ontario)

There is mounting evidence that a variety of animal species are capable of exhibiting what appears to be psi behavior. In the animal psi experiments concerned with precognition, the general experimental paradigm employed to date has involved measuring changes in an animal's behavior in the present as a function of the presentation of an aversive event, such as shock or death, some time in the future. The paradigm can be divided into two subsidiary paradigms, which I have labelled "contingent" and "noncontingent." In the contingent precognition paradigm, the animal subjects have control over whether or not the aversive event will occur. In the noncontingent precognition paradigm, they have no such control. I have done two experiments comparing rat precognition performance in each of these conditions.

The dependent measure in both studies was rat running behavior in a foot-long T-maze. A trial began when the plastic divider in the start box was lifted; it ended when the rat's nose crossed into the left or the right choice area at the top of the "T." To help reduce sensory cues from the experimenter to the rat, a thin metal plate covered the length of the T-maze. Because the rats were assigned to groups only after all of them had been run, the experimenter was blind as to the group identity of any particular rat. Three measures were taken of the rat's behavior in the T-maze: turn direction at the choice point,

*Chairman: Robert L. Morris, Psychical Research Foundation.

running time, and frequency of fecal bolus emission.

After all the rats were run, they were randomly assigned to the Contingent or the Noncontingent condition. Then, within each of these conditions, they were assigned either to Imminent Death or Delay Death, administered by an overdose of chloroform. Death for the Imminent animals occurred within a few hours of the experiment. Death for the Delay animals occurred no earlier than one month after the experiment. In the Contingent condition, rats were assigned to Imminent Death if they made an "incorrect" choice and to Delay Death if they made a "correct" choice. The correctness or incorrectness of the choices was determined by matching the actual choices against a series of random numbers. Within the Noncontingent condition, rats were assigned randomly to Imminent or Delay Death regardless of their T-maze choice.

The procedure for the random assignment of animals to groups was as follows: immediately after all the rats had been run, I turned on the radio and waited for the weather report. When it came on, I recorded the current local temperature. I then calculated the square root of the temperature and used the seventh digit to the right of the decimal place to enter a table of random numbers, which was used to assign the animals to groups. In the first experiment, 69 Holtzman rats ran one trial apiece. On the basis of their choice behavior, the 36 rats in the Contingent condition showed no evidence that they could discriminate correct from incorrect turns. Twenty-one made incorrect choices, putting themselves in the Imminent Death group, while only 15 made correct choices, putting themselves in the Delay Death group. In the Noncontingent condition, there were 17 rats in the Imminent and 16 rats in the Delay Death group. The fecal bolus emission data, like the choice data, did not differentiate between groups.

Differences occurred, however, in running time. The Contingent-Delay group took much longer to run the T-maze than any of the other three groups, but only the difference between it and the Contingent-Imminent group approached significance. These two groups differed in running time by 37 percent, a sizable amount, but the large degree of between-subject variability kept the significance level down. I therefore decided to do a second experiment using two trials per rat instead of one, reasoning that the first trial could serve as a baseline while the second trial would

be the decision trial. In this way the large between-subject differences could be replaced by smaller but more consistent within-subject differences. Fifty-nine Holtzman rats were used in this second experiment. After all the rats had been given their first trial, they were then given their second trial. As in the first experiment, the choice behavior results (based on the second trial) showed that the 32 rats in the Contingent condition could not discriminate correct from incorrect turns. Sixteen placed themselves in the Imminent group and 16 placed themselves in the Delay group. Of the 27 rats in the Noncontingent condition, 12 were Imminent and 15 were Delay.

An unexpected result caused me to change my plans for analyzing the running time data. Not only did both trials produce similar results, but the first trial showed a stronger effect. While on both trials the Contingent-Delay group again took longer to run the T-maze than any of the other three groups, the only significant difference was that on Trial 1 between the Contingent-Delay group and the Noncontingent-Delay group ($P < .02$). It appeared as if there was precognition within a precognition: the animals behaved on Trial 1 as if they were already responding to the contingent condition on Trial 2.

In order to evaluate overall precognition in the two experiments, I did a three-way analysis of variance (Experiment by Contingent-Noncontingent by Imminent-Delay), using as the dependent variable the combined running time data from the first experiment and Trial 1 of the second experiment. The interaction between the latter two factors was significant ($P < .01$), with the Contingent-Delay group taking considerably longer than the other three. The three faster groups were not significantly different from each other. I concluded that a rat's behavioral reaction to future death is determined not only by death's nearness in time, but also by the degree of control it has over that event. If the occurrence of death is contingent upon the correctness of a rat's response, that contingency will produce different behavioral effects than if the occurrence of death is not so contingent.

FURTHER STUDIES ON THE RESUSCITATION OF ANESTHETIZED MICE

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(Institute for Parapsychology, FRNM)

For the past two years we have been doing research in which "talented" subjects, who have demonstrated PK ability previous to the experiment, attempt to awaken one of two anesthetized mice. The procedure we have evolved consists of etherizing both mice simultaneously at the start of each trial; the first to lose consciousness is randomly designated either experimental or control. Both mice are then placed on a table, one on each side. An experimenter assigned to each mouse measures the time elapsed until it awakens, which is defined as taking its first step. The subject sits behind a one-way mirror so that he can see the mice but cannot be seen by the experimenters. According to a prearranged target order unknown to the experimenters, he concentrates either on the mouse on the left side of the table or on the mouse on the right. The trial ends when both mice are awake. Sixteen (or 24) such trials constitute a run; the target for the first eight (or 12) is the mouse on one side of the table, and for the second eight (or 12) the mouse on the other side. A 15- to 30-minute break separates the first and second halves of the run.

Our recent experiments have employed a full factorial design using five dichotomous variables; the mice at both levels of each variable are equally distributed among both levels of every other variable. The variables are experimental vs. control mouse, left vs. right side of the table, one experimenter vs. the other experimenter, first vs. second mouse to lose consciousness initially, and male vs. female mouse. The dependent variable is time to awaken measured either in seconds or log seconds. The effects of the five independent variables on time to awaken are assessed by analysis of variance.

Seven such series have been done. The first was an attempt to replicate our previously-reported research using two new experimenters (in the previous studies G. K. W. and A. M. W. had been the experimenters). The attempt was successful at producing high scoring: the main effect of the experimental vs. control variable was significant at $P = .026$, indicating significantly faster awakening

for the mouse on which the subject concentrated. The second series attempted to raise scoring levels by reducing the error variance. The mice were pretested on time to awaken, and pairs were selected whose times were similar. Furthermore, both mice were anesthetized in the same container, which was a standard etherizing device. The results showed a high level of scoring ($P = .002$).

In the third series we used "non-talented" subjects, who had not demonstrated PK ability in previous work. The results were completely insignificant, but a relationship was found between the subjects' PK scores (mean difference in time to awaken between experimental and control mouse) and their performance on a battery of psychological tests. This battery included the Eysenck Personality Inventory, the Lüscher Color Test, and an attitude questionnaire. Nine variables extracted from these tests were used in a stepwise multiple regression analysis with PK score as the dependent variable. Two of the factors, both taken from the attitude questionnaire, showed a significant relationship with PK score. Belief in ESP was positively related to scoring ($P = .05$), but attitude toward success in the experiment was negatively related: those who did not think they would do well in fact did better than those who had a more confident attitude ($P = .01$).

Two series were carried out to investigate the physiological state of the subjects during the experiment. The five variables monitored in the first such series were EEG, EKG, respiration, finger plethysmograph, and GSR. The GSR showed no significant changes during the test session, and was therefore dropped from the second series. In both series, the subjects showed significantly increased heart rate, decreased pulse amplitude, increased respiration rate, increased irregularity of respiration, alpha block in the occipital EEG, and muscle tension in the skeletal muscles. The T-wave in the EKG was significantly higher at the end of the session than at the beginning for females; for males, it was significantly lower. Both series yielded significant PK scoring ($P = .004$ and $P = .00003$).

Finally, two series were done to find out why there had been chance results in previous work when the target side was randomly changed from trial to trial, rather than remaining the same for each half of the run. The subjects had complained that they could not shift their focus of concentration fast enough, which led us to hypothesize that a

"lag effect" might exist in which the PK effect exerted on the previous trial would continue to have an influence on the mouse's performance in the current trial. To test for such an effect we had the subjects in these two series do half of a normal run, concentrating exclusively on one side or the other. The subjects then left the room and occupied themselves with other activities while the experimenters, who were of course unaware of which side had been the target, continued the procedure to the end of the run. In the first of these series the same experimenters performed both halves of the run; in the second, as an added precaution against experimenter effects, two different experimenters conducted the second half. Physiological monitoring was used in the second series.

Linear regression analyses showed the two halves of the run to be similar in overall scoring. This was true in both series ($P = .02$ in each). In addition, both halves of the run showed independently significant high scoring levels in each series. In the first series the first half was significant at $P = .003$ and the second half at $P = .002$. In the second series the significance levels were $P = .04$ for the first half and $P = .05$ for the second half. The physiological data were significant in the same way as those obtained in the earlier series. The fifth and seventh series had used automatic timing of the mouse's arousal: the mice were placed on photocells and the photocell output was recorded on a polygraph chart. Arousal was defined as the point at which the mouse got off the photocell. In one of the series timing was also done manually with a stopwatch. No significant difference was found between the two methods, attesting to the reliability of each.

EFFECTS OF FACTORS INTERPRETABLE AS HIGH AND LOW STRESS STATES ON PRECOGNITION IN SMALL RODENTS

Walter J. Levy†, Brian Artley, Carol Williams, and Becky Owens (Institute for Parapsychology, FRNM)

Previously-reported studies by Duval and Montredon in France, and by W. J. L. and others in Durham, North Carolina, have given evidence that mice and jirds (commonly

called gerbils) can to a significant extent avoid being in a location where they will receive an electric shock. In our experimental design the animal is placed in a small test cage divided in half by a low barrier. Each half has an independently-wired grid for a floor. For each minute throughout a 26-minute run, a random target generator selects one of the two grids to receive a shock during the last five seconds of the minute. The first minute does not count as a trial, so there are 25 trials per run. The animal's position is monitored continuously with respect to which side he is in. In our previous work we found that selecting only "random behavior" trials increased the scoring rate. A random behavior trial is one in which the animal jumps over the barrier one or more times during the 55-second preshock period, but does not include a single jump made during shock at the end of the previous trial. About half the trials meet this criterion. All results reported below refer solely to random behavior trials.

This paper reports further analyses on our three previously-reported studies, plus an additional 11 experiments carried out during 1971 and 1972 using hamsters as well as mice and jirds. All showed significant positive scoring levels. An analysis of internal effects on the three early studies showed that two factors were important: animals which had not been shocked on the previous trial scored better than those which had been shocked, and animals which jumped over the barrier only once or twice during the preshock period scored better than those which jumped three or more times. These two variables are not independent, since shock on the previous trial would make the animal more likely to jump. Both previous shock and a high jump rate were associated with chance-level scoring, and both can be taken as indicators of stress for the animal. The possibility therefore arises that stress inhibits psi performance in this situation. These analyses are currently being done on the first five of the 11 recent studies, and have been significantly confirmed in the last six. An additional observation from the previously-reported work was that animals whose cage environment was frequently changed scored better, indicating that adaptation to a changing environment decreased the stress of being placed in a new environment at the time of testing.

Two other possible indicators of stress have been related to scoring level in the recent experiments. The first was testing rate. We normally tested an animal once

every other day, and noticed that scoring level seemed to drop when it was tested more frequently. We therefore selected a group of animals who were scoring significantly above chance and tested them four times a day for six days. The remaining animals, whose scoring rate was almost as high, were tested every other day, as usual. The scores of the first group dropped to chance level, whereas the second group continued to score significantly above chance. This may provide a parallel to the decline effects commonly found when human subjects are tested too frequently.

The second stress-related variable which emerged from the recent data was time of day. Several of the series were run continuously, 24 hours a day. In all of these, the scoring level was highest between midnight and 8:00 A. M. This is the time when these small rodents are normally most active, being nocturnal animals, and therefore testing them during this time would disturb their rest least and produce least stress. Several other factors are now being investigated which significantly relate to scoring level and may also relate to stress. One is the time of the "committed jump." This is the last jump the animal makes before the five-second shock (or no-shock) period at the end of the trial. In two series analyzed so far the animals scored significantly above chance only when the committed jump was within 13 seconds of the next trial. This might indicate a lower degree of stress in that the animal is further removed in time from shock on the preceding trial. Another effect we have observed is that a large amount of extraneous light and noise in the room seems to decrease the scoring level, perhaps through elevating stress.

The various measures which have been associated with significant scoring in these studies seem to imply that stress has an important and consistent influence on the animals' ability precognitively to avoid shock. To confirm the hypothesis, these measures need further testing and more extensive interweaving, using a more detailed definition of stress for these animals. The efficiency of the research can be increased by speeding up the trial rate (if random behavior percentage and scoring rate can be maintained), by finding more factors which affect scoring, and by selecting and breeding high-scoring animals. The result will hopefully be a sufficiently clear definition of factors affecting scoring level to produce a stable and repeatable psi experiment which can be used as a tool for investigation in many areas.

We are now in the process of doing further replications and encouraging others to do so. A group of researchers in the Netherlands have achieved a significant confirmation of the main effect of the studies, which is overall high-scoring on random behavior trials. Independent replications make it more difficult to attribute the results to experimenter effects. One of our recent studies was conducted by B. A. while W. J. L., the principal investigator in the research, was in Georgia. In addition, the presence of internal stress-related effects would imply that scoring level is more dependent on the animal than on the experimenter.

The fact that our experiment is to a great extent automated further attenuates the experimenter's influence. His only role is to place the animal in the test cage and then remove it after the session; shock generation, recording of the animal's position and activity, and registration of success in avoiding shock are all done by machines. The experimenter does not usually observe the animals during the test, and is often in another room. We are currently attempting to phase out the experimenter entirely, using a variety of equipment controlled by an in-house real-time computer. This will allow the animals to be inserted and removed from the test cage automatically, and will enable several experiments to be run at the same time.

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GROUP ESP TESTS*

A FREE RESPONSE SHEEP-GOAT EXPERIMENT USING AN IRRELEVANT TASK

Mary Rose Barrington (Society for Psychical Research)

On March 23, 1971, the ESP Committee of the Society for Psychical Research (S. P. R.) conducted a telepathic group sheep-goat experiment using as subjects 49 S. P. R. members who had volunteered to take part. At the beginning of the session they graded themselves on a six-point scale denoting degree of belief in ESP. They were asked to choose the most extreme statement with which they could agree. Our purpose in designing the scale was to separate the purely intellectual sheep from those dyed-in-the-wool sheep who felt that ESP was part of their personal way of life.

The six statements were labelled as follows, listed in descending order of belief: White Sheep ("I think I should score above chance in an ESP test"); Grey Sheep ("I have had experiences I definitely ascribe to ESP"); Black Sheep ("I am almost sure that some events are due to ESP"); White Goats ("I am not quite convinced by the evidence for ESP"); Grey Goats ("I very much doubt the existence of ESP"); Black Goats ("I regard ESP as little more than superstition"). As I had expected, sheep predominated, but they were sheep of relatively little faith. There were no White Sheep, 17 Grey Sheep, 26 Black Sheep, two White Goats, three Grey Goats, and one Black Goat. One of the Black Sheep and the sole Black Goat were disqualified later on because they did not produce usable data, so I was left with 47 subjects representing

*Chairman: Gertrude Schmeidler, City College, CUNY.

four intermediate sheep-goat categories.

A fundamental consideration was how to extract any above-chance scoring from a group of English percipients who were at best tepid sheep and, as the sheep-goat scale indicated, none of whom believed that he could score in an ESP test. Something more devious than a forced-choice routine was called for. In a spontaneous ESP experience the subject becomes aware of something that arrives in his mind unsought while he is going about his ordinary business. In this experiment "ordinary business" was represented to the percipients as the experimental task, but it was in fact an irrelevant task.

The percipients were given notebooks bearing a row and seat number. At the start of each trial they were shown a display card, a white board 34 inches by 28 inches, divided into nine rectangles. Each rectangle contained a simple line drawing of an object, each drawing bearing a title such as "Book" or "Ashtray." The display card was shown to the percipients for a few seconds, while a recorded voice read out the titles, and then it was withdrawn. The percipients were then asked to count backwards in threes nine times from a given number, and after that they had to write down in their notebooks, in any order, as many of the titles shown on the display card for that trial as they could remember. They were to use a separate notebook page for each trial. They were told in the vaguest possible way that six agents, to whom they were briefly introduced before the trials began, would be in an adjoining room trying to influence their recollections. The directions throughout the trials were given by means of a pre-recorded tape, and anticipating a certain sales resistance the voice urged the percipients to keep on writing even after their certain memories were exhausted. There were 18 trials, each lasting two minutes.

Meanwhile, at the start of each trial a target card was placed in front of the agents. This resembled the percipients' display card in layout, but instead of nine random and unrelated subjects as were shown on the display card, each of the titled drawings on the target card related to a single target theme. On Trial A the target theme was "Animals," and the target card showed drawings entitled "Cat," "Dog," "Cow," and so on. The 18 target themes were: Animals, Vegetables, Buildings, Trees, Wheeled Traffic, Furniture, Fruit, Clothes, Birds, Games,

Electrical Equipment, People, Boats, Road Signs, Insects, Mathematics, Music, and Aircraft. Knowing nothing about the true role of agents in telepathy tests we used three different methods of presentation, the agents looking at the target card for one minute on some trials and for shorter periods on others. (As it turned out, the shorter exposures produced better results, but not to any significant degree.)

It was predicted that the agents would influence the percipients by infiltrating into their minds false recollections corresponding with the target card instead of with the display card. Thus if on Trial A a percipient recorded that he thought he saw an elephant, this would be a hit, since no elephant would have appeared on the display card, nor would a suggestive word like "Trunk." But if he recorded "elephant" on any other trial, this would depress the scoring level. The measure of success was to find whether percipients had consistently written down more false recollections of Animals on Trial A than on any other trial, more of Vegetables on Trial B, and so on. The assessable material, then, consisted of all the false recollections culled from each of the 47 notebooks collected from the percipients at the end of the experiment.

Over 700 assessable items were accepted, the average contribution being 14 or 15 items per percipient. An item-by-target matrix was prepared, with the items listed trial by trial down the margin of a page. Each contributor was identified only by his seat and row number. Across the top of the page the 18 target themes were listed in alphabetical order. Three judges assessed the degree of correspondence between each item and each target on a scale ranging from 0 to 5. Their three scores were then summed, and the data were divided into groups based on the self-rated degree of belief in ESP of the percipients contributing the items. For each of the four sheep-goat categories, a new matrix was prepared in which were entered total marks for each target theme for each trial. In these four new matrices the target themes were again listed across the page, but this time in their correct order, while the letters denoting the trials were listed down the side of the page. Thus the diagonal running from upper left to lower right showed correct target-trial pairings. The marks awarded on this diagonal were compared with those awarded for that theme on other trials.

The most reliable method of assessment was

considered to be the Monte Carlo method, and a computer printout was obtained, using 1000 random samples, for each sheep-goat category matrix. The results showed a linear relationship between degree of belief in ESP and ESP score: Grey Sheep scored above chance ($P < .05$); Black Sheep scored slightly lower; White Goats scored lower than Black Sheep; and Grey Goats scored lowest of all (just below chance level). The probability was .042 that the four groups would have come out in that order. In the Grey Sheep matrix, but not in any other, there was a noticeable decline effect. When the first half of the trials were compared with the second half, the difference was significant ($P < .001$). I concluded that, standing alone, the results of this experiment would be of little consequence, but bearing in mind the absurdly small populations involved and the rather half-hearted beliefs expressed, the degree of differential was consistent with sheep-goat tests carried out in other countries where more significant results have been obtained.

THE TIME FACTOR IN EXPERIMENTAL ESP

Arthur Thomson (Dundee, Scotland)

Since 1968, I have been running group ESP experiments investigating time displacement scoring effects in relation to trial rate, using a telepathic procedure similar to S. G. Soal's. These tests have been carried out in the classroom using as subjects adults who were attending extramural classes in parapsychology which I have been teaching under the aegis of Glasgow University. In all, 11 different classes have been held at different towns in Scotland but, for the purposes of this report, tests done in the first two are treated as pilot investigations and the data presented below are based on the remaining nine groups.

The procedure was essentially that used by Soal in his Shackleton series. The agent, a member of the class, was seated on one side of a table placed at the rear of the classroom behind a screen opposite the experimenter (myself), and out of sight of the class. Between the experimenter and the agent was a special screen pierced with a row of five holes. At the start of each trial the

experimenter signalled to the agent by shining a torch through one of the five holes according to a prepared sequence of random numbers. On the table in front of the agent lay a row of five cards face downwards. As soon as the signal was received the agent picked up the corresponding card, inspected it, and returned it to its original position.

Four runs of 25 trials each were done in a session. To facilitate displacement scoring, two extra targets were sent at the beginning of each run and two at the end. The class did not call these targets; they were used later in scoring (± 1) and (± 2) displacement hits. At first, the standard ESP cards were used as targets, but later a set of animal cards like those used by Soal was adopted. The signal for each trial was given by a bell attached to a metronome which rang on every fourth metronome beat. Initially, rates of 3.5 to 4 seconds per trial were used ("slow" runs), but later I used a faster rate of 2.5 seconds per trial ("fast" runs). In all, 1526 runs were done at the slower speed, and 1094 at the faster speed.

On the basis of the pilot results I hypothesized that when hits were considered as a function of target displacement they would be found to form an inverted U-curve with a peak at the zero position and a trough at the (± 2) positions. This was subsequently found to hold only for the slow trial rates; when the faster rate was used, the effect was reversed, so that the maximum positive score occurred at the (± 2) positions. Accordingly, a second prediction was made with respect to groups run exclusively on the faster schedule. All scores and computations were double-checked by a collaborator, an honors graduate in mathematics.

The following scoring pattern was obtained for the 1526 slow runs: for (-2) displacement, CR = -1.43; (-1) gave CR = +0.37; direct hits gave CR = +2.91; (+1) gave CR = -2.57; and (+2) gave CR = -3.03. Thus the prediction of an inverted U-curve was borne out. The direct hits were significantly positive at the .002 level and the overall chi square obtained by combining the CR's was significant at the .001 level (chi square = 26.44, df = 5), indicating overall ESP. The four displacement CR's were themselves significant when combined (chi square = 17.97, df = 4; $P < .01$). I concluded that my hypothesis regarding slow trial rates was confirmed: there was not only psi-hitting on the direct target but also psi-missing on the displaced targets.

Turning next to the results of the fast runs, the scoring pattern was as follows: for (-2) displacement, CR = +2.01; (-1) gave CR = +0.80; direct hits gave CR = -0.23; (+1) gave CR = -2.01; and (+2) gave CR = +0.78. The positive scoring on the (±2) positions partially confirms the hypothesis for the fast trial rate. However, the overall chi square of the five CR's combined (9.38, df = 5) fell short of the .05 level of significance, so I could not demonstrate that overall ESP was occurring, and my conclusion was therefore only tentative. Nevertheless, these results do provide evidence that the previous results from the slow runs were unlikely to have been due to scoring error (a tendency to overlook hits in the displaced positions). In addition, they provide a parallel to the shift in scoring which occurred in the Shackleton series when the rate of presentation was increased from 2.8 seconds to 1.4 seconds per trial.

GESP IN THE CLASSROOM

Dick J. Bierman† and Bert Camstra (Study Centre for Experimental Parapsychology)

We wanted to design a repeatable experiment using unselected subjects in which variables such as sex, age, and type of ESP test could be related reliably to ESP scoring patterns, including time displacement and position effects. For this purpose a classroom setting is ideal; it enables us to test large numbers of subjects and thus control for random error variance. In order to increase the efficiency of such an experiment, an automated procedure was designed which could be used for either a GESP or clairvoyance test.

A tape containing a sequence of 75 random numbers (from 1 to 5) was fed into a tape reader which controlled five signal lamps corresponding to the five numbers. Above each lamp was a stimulus card on which was a picture of a car, tree, house, apple, or sailboat. The agent (the classroom teacher) sat behind a screen shielded from the percipients (the pupils). When one of the signal lamps was lighted, he concentrated on the corresponding picture card. At the same time a buzzer signalled the pupils to

record their guesses on a scoring sheet. By disconnecting the appropriate apparatus, the teacher could be cut off from knowledge of the target, thereby converting the test into the clairvoyant mode. Comparisons between clairvoyance and GESP could thus be made without the pupils' knowledge.

A pilot and a confirmatory study have been completed so far using this design. The pilot study, done in 1971, used as subjects 88 pupils aged 11 to 17. No significant overall ESP was found, nor was there a difference between boys and girls. The GESP mode produced higher scoring ($P = .003$) than the clairvoyant mode. A marked decline effect was found, resembling a U-curve. A tentative curvilinear relationship was established between scoring level and age. There was significant (-1) displacement scoring ($P = .02$). No correction was made for the stacking effect. On the basis of this pilot study a number of minor modifications were made, including a shift in the sequence which we hoped would convert the (-1) displacement scoring into direct-hit scoring. In the confirmatory investigation, using these modifications, 1402 pupils were tested from 12 schools: seven primary schools and five secondary schools. Each pupil did 75 trials (three standard runs). Scores were corrected for the stacking effect using a formula worked out by D. J. B.

The results showed that our modifications had been successful in producing significantly positive direct hitting. The subjects gave a mean score of 16.034 where mean chance expectation was 15 ($CR = 11.7$; $P < 10^{-30}$). Girls from the secondary schools contributed most of the positive scoring, and girls in general did slightly better than boys ($P = .042$). Again, the GESP condition produced higher scores than clairvoyance, but not nearly as strongly as in the pilot study ($P = .089$). Finally, a marked linear incline effect was obtained across runs, which followed the formula: score on run (i) $= 5 + [.35 \times (i-1)]$.

There was no evidence of bimodality in the overall scoring distribution (i. e., no groups of psi-hitters or psi-missers). Instead, the distribution resembled a normal curve, which one would expect if chance were operating, except that the entire curve was shifted 1.034 score points to the right. The variance was only slightly larger than the theoretically expected variance. This evidence led us to feel safe in concluding that, in this population at least,

everyone had some ESP ability, and the psi distribution was rather narrow. This represents a vindication of the approach which uses unselected subjects. A final consistency analysis was done in which, assuming the scoring level found in the confirmatory investigation, we computed the number of subjects needed to have a 50 percent chance of finding a significant positive deviation ($P < .001$) in any study of this kind. This number turned out to be 110 subjects. We then tested this prediction on the schools in our sample having at least 110 pupils. Of the seven such schools, five gave a positive deviation significant at $P < .001$, indicating marked inter-school consistency and supporting our prediction.

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HISTORICAL STUDIES*

ESP PERFORMANCES OF D. D. HOME

George Zorab (The Hague)

D. D. Home (1833-1886) should certainly be considered the most powerful PK subject of the last two centuries, and he is noted in parapsychological history as a medium who produced very remarkable physical phenomena. But what is still very little known is that he was also an outstanding ESP subject. I feel that the time has come to draw attention to his remarkable ESP performances, and thus supply in addition some more evidence for the oneness of the psi factor. Psi may manifest itself in various aspects, but they all well up from a single source, as is evidenced in Home's extraordinary mental (ESP) as well as physical (PK) abilities.

Two kinds of ESP performances can be distinguished during Home's sittings. ESP was at work either as information allegedly received from deceased persons, or as information coming from the living, generally the sitters present at the seance. The function of the former was to enforce the sitters' conviction that a certain spirit was present and communicating, thus greatly supporting the spiritualistic hypothesis professed by Home. ESP from the living occurred mostly when in the course of a sitting those present felt themselves touched, patted, pinched, etc. on various parts of their bodies. A sitter's demand to be touched could be left unspoken but would still be carried out correctly, thus giving evidence of ESP functioning with Home as the percipient. The same could occur during the production of that typical phenomenon of early spiritualistic

*Chairman: Hans Bender, Institut für Grenzgebiete der Psychologie.

times: the raps. In accordance with the sitters' unspoken wish Home could produce these raps in every desired number, rhythm, and loudness. Over and over again the demand, which had only been thought, would be correctly fulfilled.

ESP phenomena also occurred when the sitters' requests concerned deceased persons. In Mrs. Peck's report on sittings with Home in Geneva in 1873 we read: "By permission I put several mental questions, each of which was promptly and correctly answered, with the full names of friends and relatives deceased, and circumstances which could not have been known to any of those present; all, as I have stated, having been previous to the past 24 hours strangers to me." In general, reports of Home's sittings record only his remarkable PK and materialization phenomena, and seldom mention anything regarding ESP performances. The reverse however occurred when in 1855 Home was staying in Florence at Mr. A. Trollope's house. Trollope was mainly interested in spiritual manifestations, and it seems that Home readily adapted himself to the attitude of his host. In one of Trollope's books we find several instances mentioned of truly remarkable ESP performances which the author considered as a matter of course to be authentic communications from the deceased person concerned.

In addition, Home had a remarkable manner of combining his ESP communications with phenomena of a physical nature (psychokinesis and materialization), the sitter thereby being immensely impressed and convinced of the real presence of the deceased person announced to be communicating. An interesting instance of such a combination of ESP and materialization is given by the French Empress Eugenie who describes her seance with Home in a letter to her sister in March 1857. She writes:

> On March 15 we conducted a seance. Nobody, with the exception of myself, knew which anniversary it was that day. Well, from the very moment we sat down at the seance-table, a hand had, not for a single moment, refrained from pressing mine, or from pulling my dress, urging me to give it my hand. Amazed at its insistence, I asked: 'You do love me, don't you?' And at once it answered with 'yes,' pressing my hand quite distinctly! 'Do I know you?' 'Yes.' 'Will you

> please tell me the name you had on earth?' By rapping out the letters of the alphabet it answered: 'Today is the date of my decease.' Then all the other sitters wanted to know what all this was about. I answered: 'It is my father.' Directly I had said this the hand pressed mine with great feeling, and gave me permission to press its hand in my turn. Then the hand with its finger made the sign of the cross three times on my hand.

Finally, with reference to the recent experimental interest in tracing relationships between EEG alpha activity and ESP scoring, I would like to point out that Home was able to produce his psi phenomena at all levels of consciousness. He could relish a dish of "baked rice" while powerful PK phenomena were going on. Whether he was calmly sitting at the seance-table, laughing or joking with sitters, or in a state of trance, the same powerful PK and ESP phenomena manifested themselves.

THE USEFULNESS OF HISTORY

Fraser Nicol (Lexington, Massachusetts)

Hegel is reported to have said "What do we learn from history? We learn from history that we do not learn from history." If it is true that in parapsychology we do not learn from history as much as we ought to, there are honorable reasons for this ignorance. When Myers wrote his classic volume Human Personality, he had to have a good working knowledge of approximately 70 volumes of psychical journals in several languages, in addition to a modest number of books. To cover the history of our field today, I estimate that one would be required to have at least a fair knowledge of 800 volumes of journals and perhaps an equal number of books. The ordinary practitioner of parapsychology cannot be expected to get on speaking terms with so great a mountain of information. But even if there are honorable reasons for our neglect of history, the penalties are severe. I would like in this paper to show how historical facts have been misrepresented, to illustrate how progress has been retarded by our

neglecting to use the available knowledge from the past, and finally to propose a remedy.

Many examples of erroneous parapsychological history are available. I have selected one as an illustration. At a sitting with the medium Mrs. Piper in 1909, Professor G. Stanley Hall asked the medium's "Hodgson" control to produce his niece Bessie Beals. The "niece" thereafter made a brief appearance as a communicator. Subsequently Hall said he had no such niece--the name was a pure invention. Since that time numerous writers have repeated Hall's story. An examination of the evidence, however, shows that Hall concealed some of the facts in the case, and that there was indeed a person named Bessie Beals who was not without significance in the matter.

Next, I will describe several examples of early research achievements and observations which would have facilitated investigation in many areas if modern parapsychologists had been aware of them. THE SPHYGMOGRAPH. More than 90 years ago Dr. John E. Purdon, an English physician with wide experience in psychical research, performed experiments on percipients and agents which may be described as forerunners of the plethysmograph work done by Figar and others in recent years. Using the sphygmograph to measure pulse rate, Purdon claimed that the rate varied with the success or failure of telepathic transmission. He also believed he had found that if two people in the same room happened to think of the same thing, their sphygmograph records would show it. A self-critical man, Purdon appealed to researchers better equipped than himself to pursue the investigations further. No sustained attempt was made to do so, and for three-quarters of a century his ingenious ideas were lost to history.

CARD GUESSING VIA AUTOMATIC WRITING. This is a case illustrating an "altered state of consciousness." In 1910 Miss Lisi Cipriani informed James Hyslop that she could clairvoyantly, through automatic writing, guess the order of five letters inscribed on small sheets of paper. Hyslop tested her claim, taking all necessary precautions. Through four laborious sessions he shuffled the target papers; neither party knew the target order until after the lady's hand had recorded her psychic impressions. In 85 sets of the five targets Miss Cipriani obtained 103 hits, which was 21 percent above the chance expectation of 85. The associated P-value of .06 is at least interesting for so small a

number of trials. Further, the probability of getting all five targets in a set right by chance is only one in 120; in 85 sets one would hardly expect even one to be right. Miss Cipriani got four sets right (P = .006).

The most interesting aspect of this automatic writing research, however, is the evidence it provided of a struggle apparently going on at some deep level of Miss Cipriani's mind as she tried to get true clairvoyant impressions of the invisible targets. Sometimes the subconscious seemed confident of its impressions, but at other times it seemed uncertain; the writing hand would switch from one place in the order to another while Hyslop waited for its final decision. If automatic writing could be a means of getting an illuminating view of what happens deep in the mind when psi is actually manifesting, the Hyslop research might be used as a starting point--after being forgotten for 60 years.

HYPNOTISM. In the old days it was a common thing for subjects to be put into a somnambulic state, in which paranormal occurrences were both frequent and remarkable. Nowadays it seems to be rare for percipients to pass into a somnambulic condition, and accordingly paranormal manifestations are sparse. Much could be learned by studying the writings of the old hypnotists, especially Esdaile, and old volumes of The Zoist. DECLINE EFFECTS. First noticed 83 years ago, their psychological importance was never followed up. They were disregarded for half a century until they were rediscovered at Duke. CONSISTENT MISSING. Although this phenomenon first appeared in experimental work at the Society for Psychical Research in the middle of the 1890's, it was neglected by the investigators and their successors. Forty years later G. C. Barnard called attention to the subject and offered a psychological explanation, but nothing further was done for many years.

TELEKINESIS. The current Russian investigations might be better understood if studied against the background of the older work done with Miss Tomczyk and Dr. Kharis's subject Mlle. Melita P. in the early 1900's. HYPERAESTHESIA. Myers in 1900 held the view, and offered evidence, that this physical condition of abnormally increased sensitivity formed the junction point between the psychical and physical parts of the human personality. If so, it should be a supremely important subject for investigation today.

Because of the immense growth of information, hundreds of meaningful researches and theoretical discourses have vanished out of sight. It has recently been suggested that to meet the needs of students and researchers an International Bibliography should be prepared. No plans have been made, but I would assume that such a work would consist not merely of hundreds of pages of listed books and papers but would be sufficiently annotated to be a ready guide through the literature in all its multifarious aspects.

SYMPOSIUM: WHAT EVIDENCE, IF YOU HAD IT, WOULD CONVINCE YOU OF SURVIVAL?*

A CROSS-CULTURAL APPROACH

Karlis Osis (American Society for Psychical Research)

Looking back over the years, I ask what evidence for the survival hypothesis has impressed me so much that I was ready to put in years of hard work investigating it? When I was younger, I searched passionately through the literature for a definitive experiment or a definitive case that would clinch the issue. I was quite impressed by the cross-correspondences, such as the Ear of Dionysius case, and by mediumistic cases of the proxy sitting type, especially those where the motivation for a particular message seemed not to be within the range of those living persons who were present. I was impressed by Barrett's deathbed observations and Carrington's word association tests with mediums in trance, as well as by spontaneous case reports of apparitions. But, under closer scrutiny, none of the experiments or the cases looked like a really definite proof of survival.

Do we need, however, to demand definitive evidence in this sense? Are matters of comparable complexity in the behavioral sciences, such as the nature of psychoses and neuroses, ever settled by one crucial experiment? It is a large array of research facts coalescing around central ideas and ideologies that seems to convince behavioral scientists--at least, some of them. If the idea of survival after death has some basis in reality, the facts in that area will also begin to coalesce around it. This is the way to confirm the idea. In my opinion, a very rewarding survival

*Chairman: Ian Stevenson, University of Virginia.

research strategy at present would be an intensive search into those phenomena which are prominently used to support belief in survival. I would choose phenomena which seem to cut across cultural differences, since they would appear to be rooted more in human nature than in beliefs and superstitions: out-of-the-body experiences of the living, apparitions of the living and dead, and the twilight states between life and death. Some years ago, I made a study of deathbed observations which suggested that in these twilight states sensitivity to out-of-the-body experiences and apparitions seems to be greatly increased.

Evidence from disconnected phenomena is less compelling to me than evidence from various areas that makes sense when organized by a unifying theory. Myers attempted this on a magnificent scale and utilized knowledge of the psychology and medicine of his day. But his day was more than 70 years ago. There is a great need to break loose from the methodology and theoretical outlook of the Victorians and expand into new areas and new ways of thinking, as Stevenson's reincarnation research has done. It is time to research these phenomena using the new methodologies, new technologies, and new theoretical outlooks now available. For example, the out-of-the-body experience might consist of introspective awareness or it might be the same thing that appears as an apparition to the outside observer. Dying patients, having increased sensitivity to the inner and the outer, might constitute a connecting link for us between introspective awareness and the exteroceptive seeing of apparitions of the dead. I would be more convinced of the survival hypothesis if such seemingly various phenomena could be brought together under some encompassing survival theory.

THE GAP BETWEEN BELIEF AND PROOF

Hans Bender (Institut für Grenzgebiete der Psychologie)

The topic of this symposium is a very personal one and provokes a very personal answer. As a matter of fact one's attitude toward the survival problem--this most emotionally loaded question for mankind--is generally a biased one. Thus, it is necessary to reflect upon one's

own bias before entering into a discussion of the possible evidential value of facts. I confess that in coping with the problem of survival I feel as if I had a sort of split personality. My emotional attitude seems to be a naively positive one: I simply cannot imagine that bodily death should be the end of the great adventure of the maturation of a personality in which a mind receives experiences and deploys its faculties. It would be a terrible waste, and we need not think in specifically religious terms to find it absurd. On the other hand I regard myself as a scientist and as such I feel it is my primordial obligation to try to abstain from the will to believe. The scientific approach has a dignity of its own and scientific methods, as controversial as they may be, are on the whole a protection against wishful thinking.

In the light of this preliminary confession I must bluntly state at the very beginning that I actually do not see the possibility of getting conclusive evidence for survival in the sense of a definite proof which excludes any other interpretation but the manifestation of a discarnate agency. I lay special stress on the words "definite proof" and do not hesitate to admit that for some selected cases the spiritistic hypothesis seems to be the most plausible one, in William James's sense. Furthermore, I admit that in the same way that we cannot definitely verify the survival hypothesis we cannot definitely falsify it; so it might make sense that someone well-informed and intellectually responsible, in weighing the whole bulk of claimed evidence, will come to the conclusion that the survival interpretation is more probable than the animistic one. I do not think that I will agree with him, as I am too much impressed by the similarities between ESP and PK achievements of the living and the claimed evidence of survival.

We all know that the increasing knowledge of the realm of psi which experiments provide invalidates the older argumentation which refers to telepathy between the living only as a counterhypothesis to survival. All the mediumistic cases of the proxy sitting type, the identity proofs, book-tests, and so on are obsolete. The cross-correspondences will forever remain a most impressive demonstration of the paranormal, but the modern concept of a psi field makes an animistic interpretation more probable. I feel bound to the old scholastic principle "entia non sunt numeranda praeter necessitatem" (entities should not be augmented beyond necessity). All paranormal experiences

involve a psyche in a living organism. That is what we observe and check and control. But we have no direct experience that a psyche may exist as an entity without a body.

I agree with J. B. Rhine when he points out in his article "Telepathy and Human Personality" that further research on the problem of survival can only be based on a direct proof of the existence of mind or psyche without a body. Nevertheless I am ready to foster every attempt to expand into new areas of research which might throw light on the survival problem, and in this connection I am particularly thinking of Ian Stevenson's reincarnation research and the new approaches to out-of-the-body experiences which were described by Karlis Osis earlier in this convention.

Let me close with a remark on some exploratory experiments which I have done with F. Jürgenson. They involved the production of unexplainable voices on tape, allegedly belonging to persons who were dead. I got genuine voices not explainable in terms of physics. I am even persuaded that Jürgenson got voices of persons who had recently died, which seemed to be identical with their voices while they were living. To me it seems most likely that this is a PK effect stemming from the unconscious mind of the experimenter, in whose memory the sound of the voice is preserved. I can imagine that one day I might hear the voice of a late dear friend telling me something which only he and I knew about. I would probably get humid eyes, but would not be convinced that the surviving personality of my friend was the origin of the communication. I will not finish before emphasizing that the evidence for psi that we actually have may be interpreted as supporting religious beliefs in some kind of survival. Since we know that under certain conditions the psyche may transcend space and time, such an interpretation becomes less improbable in a scientific sense. But I actually see no way for a cogent proof.

A NEW APPROACH TO MEDIUMSHIP RESEARCH

John Palmer (University of Virginia)

The only evidence that would completely convince me

of survival would be my own survival, because to me survival implies a continuity of self-consciousness which in principle is unverifiable by objective means. However, I believe scientific evidence capable of altering the subjective probability I would attach to the survival hypothesis can be obtained by collecting data--of various kinds--from which the existence of discarnate spirits can be reasonably and parsimoniously inferred. I cannot comment on all these kinds of evidence here, so I will restrict my remarks to one research area I am somewhat familiar with, namely, mediumship.

The traditional approach in mediumship research has been to equate survival evidence with the quantity and quality of paranormal phenomena produced by the medium that seem to transcend the limits of "normal" psi. This approach has created problems because of the difficulty in specifying these limits. If we understood the mechanisms underlying psi, the task might not be so difficult. Lacking such understanding, we must fall back on a sort of primary generalization from the data of specific experiments or accounts of spontaneous cases that occur in non-survivalistic contexts. But how far can we generalize? Given our present knowledge of psi, I do not think we should attach much weight as survival evidence to the mere acquisition of factual information about a person, living or dead. On the other hand, it seems unreasonable to me to generalize from current psi data to the demonstration of skills, such as responsive xenoglossy, that would seem to require more than a knowledge of the instruction manual. But this is only an opinion. Since we have few if any rational principles to guide our generalizations, they are essentially an exercise in speculation. It is therefore not surprising that the "super-psi" controversy remains unresolved.

At least until we are better able to define the limits of psi, I see nothing to be gained from pursuing the "super-psi" controversy in the traditional manner. What I believe is needed at this stage is the testing of concrete predictions from specific models of mediumship that assume the active agency of discarnate spirits. By specific models I mean models that allow one to specify what particular kinds or constellations of paranormal phenomena should occur in a given situation. A particularly attractive model to me is the familiar "possession" model, because in its pure form it attributes the causation of all paranormal phenomena to a spirit and does not assume telepathic or other similar

ability on the medium's part. Specifically, such a possession model assumes that the discarnate spirit of deceased person X (retaining to at least some degree the memories and personal characteristics of X) temporarily replaces the incarnate spirit or psyche of the medium, thereby providing her with these same memories and characteristics. While this model could benefit from further elaboration, it does represent the kind of model that is conducive to the kind of research I feel is needed.

The above model is supported to the degree that the medium is able to personate convincingly, in a manner she could not do normally, the deceased individual who is claimed to possess her. The model requires not only that she possess the requisite factual knowledge, but also that she manifest his mannerisms, dispositions, abilities, tone of voice, and other characteristics that distinguished him as a unique individual. Perhaps the closest we have come to obtaining such convergent evidence from mediums is in the case of Mrs. Piper's Pelham control.

If such convergent phenomena could be presented convincingly, I think the burden would fall on the anti-survivalist to provide empirical evidence that mediums (and, preferably, the particular medium in question) might be able to produce these specific kinds of phenomena without the aid of discarnate spirits. I would find two types of evidence rather persuasive in this connection, although there could be others. First would be evidence that during "possession" the medium can paranormally manifest knowledge or abilities not characteristic of the "possessing" individual when alive. Second, and even more compelling, would be evidence that the medium can paranormally personate with comparable proficiency a living individual, such as in the famous Gordon Davis case.

Neither of these kinds of phenomena are necessarily inconsistent with the view that the convergent phenomena characteristic of the "possessing" individual originated with his discarnate spirit. They are, however, damaging to the survivalist's position because they provide evidence that discarnate spirits may not be needed to produce such personations. Given such evidence, the onus would fall back on the survivalist to demonstrate the necessity of discarnate spirits in the production of those phenomena he cites as evidence for survival, perhaps by using a different model.

SURVIVAL RESEARCH WITH THE LIVING

W. G. Roll (Psychical Research Foundation)

The question posed by the title of this symposium suggests that we still lack convincing evidence of survival. Before we look for new evidence, I think we need to examine the assumptions about survival we made in exploring for the old. Perhaps our frustrations are not so much due to the data as to our interpretation of them. One assumption is that the memories, disposition, and so on of a person who has survived death, exist independently of the physical universe. A second assumption is that the surviving consciousness is essentially the waking consciousness of ordinary life with its clear-cut boundaries between each person. Since in part we identify a person by his memories, dispositions and so on, we suppose that in a deceased person these are clearly distinguished from the memories and dispositions of other surviving entities. Similarly we suppose that the consciousness which "goes with" the memories and dispositions of a living person will continue to be associated with them after his death and to be separate from the consciousnesses of other persons.

When we then seek evidence for survival after death, we find that it does not conform with our ideas about survival. Take the drop-in cases discussed by Alan Gauld in the Proceedings of the Society for Psychical Research. Six of the ten verified drop-ins had either lived in Cambridge where the operators had their homes or in areas where they had visited. The remaining four drop-ins came from other parts of England; there were none from abroad. This geographical focusing effect is found also in other drop-ins and in reincarnation cases. The situation seems similar to mediumistic sittings where the linkage is usually through a person who knew the deceased or by an object belonging to him. Such a linkage or focusing effect suggests a dependence on physical space. The second assumption is also not supported by Gauld's drop-ins. There is much confusion about the ownership of memories. For instance, one communicator may "remember" an event in the life of another. Other times one communicator pretends to be somebody else; or fictitious communicators enter the scene and interact with the "genuine" ones.

In the face of such conflicts with our assumptions

about survival, we are likely to reject the data as evidence for survival. For instance, we may say that the mixed-up memories, the fictitious communicators and so on are produced in the unconscious of the medium. It then is a short step to say that this is also true for the verified communicators and that they are built around a core of ESP information which the medium has obtained from existing sources. When there is object linkage between the medium and such sources, the non-survival interpretation is strengthened. Instead of rejecting--or accepting--the data as evidence for survival let us use it to reformulate our survival hypotheses. For instance, we might hypothesize that the deceased remember by means of physical objects associated with past events and that the consciousnesses of the deceased are not sharply separated so that memories (and perhaps other aspects of personal identity) may "belong" to more than one consciousness.

Since consciousness may exist independently of memories, etc., it is not enough to demonstrate the continuation of these after death. There may be other indices of disembodied consciousness but it is not known what they are. It is easier to find such indices for a consciousness which is associated with a body. We say that a living person is conscious by comparing his behavior and his descriptions of his states with our behavior and state when we know ourselves to be conscious. Since the dead cannot be directly observed and since we do not know whether or not the entities which speak through mediums are who they claim to be, we are unable to tell whether consciousness continues after death. But in this hypothetical continuity lies the solution to the problem. Since the consciousness which may survive death presumably is present before death, it should be possible to study the characteristics of the after-death consciousness in the living. We already know what some of these must be. First, a survivable consciousness must be able to exist apart from the organism. Second, if it is to interact with others after death--and perhaps, with the living, as in mediumistic communications--then it must possess ESP abilities (or perhaps itself be a "state" of ESP awareness). The most promising subjects for this study would be those who feel that they can extend their consciousness beyond the body and at the same time can interact (by ESP or PK) with the environment to which they have expanded. Such people might include mediums, meditators, persons capable of out-of-body experiences, and persons in deep hypnosis. Parapsychologists now have

access both to suitable ESP tests and to physiological instruments which may indicate whether expanded states of awareness can take place independently of the body (it is also possible that there are detectable changes at the place to which a person such as an OOBE subject has "expanded").

Is the apparently survivable consciousness of the living similar to the consciousness of ostensible communicators? It seems that a person's ESP may not be entirely independent of the physical universe but tends to focus on near or familiar persons and places. If a distant target is involved, ESP may be facilitated by object linkage. Secondly ESP sometimes involves the sharing of mental contents. As H. H. Price has said, once an image or idea has been produced in a mind it no longer belongs exclusively to that mind. This sharing of memories and ideas leads sometimes to the feeling of shared or expanded consciousness. When Mrs. Garrett used her ESP, she had "no sense of I and any other, but a close association with, an immersion in, the phenomena."

Paradoxically, we may obtain stronger evidence for survival through studies of living subjects than of ostensible communicators. More importantly studies of the living may give us easier access to the psychological and, possibly, physical parameters of the consciousness which may survive and of the space in which it survives. This may result in a more detailed survival hypothesis than we now have with which to explore possible after-death states.

THE "PERFECT" REINCARNATION CASE

Ian Stevenson (University of Virginia)

Most parapsychologists today believe that extrasensory perception on the part of a living person, even if of a highly complex type, suffices to explain the evidence that some of our predecessors, although by no means all of them, took to support a belief in survival after death. But these judgments are usually made with regard only to informational features by which it is thought a deceased person has provided evidence of his survival. There exist, however,

types of data other than cognitive which may indicate survival and which may not be so readily explained by the hypothesis of extrasensory perception on the part of a living person. I refer to correspondences between the behavioral aspects of a deceased person, especially skills, and those of a living person; and to aspects of the deceased person's physical body, such as wounds, to which birthmarks or deformities on the living person correspond. Many reincarnation cases have features of one or both of these kinds. All such cases have weaknesses due to their dependence on human testimony, but I think that in principle they may provide important evidence of survival. To illustrate I shall describe what I consider the "perfect case" of the reincarnation type. (I shall describe the case in the past tense, but it must be remembered that I have not yet found the "perfect case.")

The subject was a boy born in a village of France who had several distinct birthmarks. When he began to speak he indicated that in his previous life he had been shot and killed with bullets hitting him at the sites of these birthmarks. As he acquired more powers of speech he filled in details. He named the assassins, and said that one of them had accused him of cheating at cards just before shooting him. He gave his own name, and the names of his parents, his siblings, and a girl friend. He also stated where he had lived and where he had been shot, as well as many other particulars of his previous life.

During this period the boy showed several behavioral traits which made him unique among the other children of the family. He resisted learning French and preferred talking a language incomprehensible to his parents. He took food to his mouth with his hands instead of using utensils; he rejected his family's meals and called for rice and hot curries; he demanded a piece of cloth which he wore like a Ceylonese sarong instead of the short pants his parents offered him. He showed agility in climbing the trunks of tall trees and said that he used to pick coconuts. When he could not climb trees he wanted to play cards. He also asked for a drink he called "arack" and said it was his favorite beverage.

His parents made a written record of the child's statements and his unusual behavior. They had no connections with Ceylon and knew nothing about the person their son described or the strange language he was speaking.

When the child was between three and four years old he had mentioned enough details of the previous life so that his father called in an experienced investigator. The investigator made additional notes of his own about the statements and behavior of the child and had the language he was speaking identified as Sinhalese. He then travelled to the Ceylonese village the boy had named and there found that a coconut picker with the name the boy said was his, had been murdered by shooting several years before the birth of the boy in France. The body of the murdered man had been examined by a physician who had recorded the locations of the wounds on his body. These corresponded exactly with the sites and appearances of the birthmarks on the boy's body. The boy's statements were found to be correct regarding facts in the life and death of the man killed in Ceylon. Moreover, this man had exactly the habits shown by the French boy, including a fondness for arack and an enthusiasm for playing cards.

This case would thus seem to show the reproduction in a living person of three distinct features of a deceased person: imaged memories of the events in the life of the deceased person; behavioral traits, including skills, such as speaking a language not normally learned by the subject and the ability to climb up tree trunks easily; and correspondences between wounds on the physical body of the deceased person and birthmarks on the subject. Could it not be said then that to a considerable extent the subject had reproduced important features of the deceased man he claimed to have been? And in that case, would not the interpretation that this deceased man had, in fact, been reborn seem more probable than any other interpretation of the case? Does anyone think that all its features could be fully accounted for by extrasensory perception on the part of the living subject? In conclusion, I wish to emphasize again that I do not have such a "perfect case" and have more hope than expectation of finding one. I can say, however, that I have found actual cases which have features resembling this one although not equal to it in all respects.

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BELIEF AND DOUBT*

John Beloff (University of Edinburgh)

At last year's convention my distinguished predecessor in office, Dr. Gertrude Schmeidler, carried out an opinion poll among members of the Parapsychological Association. The first item of her questionnaire read as follows: "ESP is so well established that new research which asks only, 'Does ESP occur?' is uninteresting." It turned out that almost 90 percent of our membership agreed with this statement (Schmeidler, 1971). When I read this I was amazed. Had we now become such a tight little in-group, I wondered, that we had lost all touch with reality? Or had our recent admission to the American Association for the Advancement of Science gone to our heads? For how was it possible to talk of ESP being "so well established" when the official organs of science and psychology are still extremely reluctant to publish any positive findings, when the standard textbooks of psychology rarely bother to mention ESP unless it be in a derisory manner, and when the national research foundations which, in effect, control scientific policy are not as a rule prepared to subsidize research on the topic?

ESP may well exist, we may have excellent reasons for thinking that it does, and its existence may prove to be a fact of supreme importance; but, clearly, if words are to retain their meanings, we cannot talk about ESP being well established. And if this is true of ESP, it is presumably even more true of any other paranormal phenomenon. Yet I can scarcely suppose that my fellow members of the Parapsychological Association are ignorant of the facts of scientific life. What, then, could have prompted them to endorse this item? One can only surmise, but perhaps they reasoned somewhat as follows: "There is by now a vast body of

*This is the Presidential Address, presented September 3, 1972.

evidence which points to the existence of ESP. If this has so far failed to carry conviction then accumulating further evidence of the same sort is unlikely to make any difference. In any case, only when we can offer scientists a plausible explanation for ESP will they begin to take notice. Our research should be directed to this aim rather than to the mere demonstration that ESP occurs."

This all sounds reasonable enough, though I should have thought that, in a field such as ours, one just could not have enough good evidence. Moreover, I have my doubts, personally, whether anything that we shall ever discover about ESP will be more extraordinary and more exciting than the sheer fact of its existence. But what really worries me when I hear talk of this sort is that it betrays a certain complacency about our situation vis-à-vis the scientific community as a whole. For I feel strongly that if ever we give up trying to win the support of the uncommitted and decide to go it alone we shall very quickly degenerate into an insignificant clique of tiresome pseudoscientists. Accordingly, I decided to devote my Presidential Address to an attempt to dispel the illusions and misconceptions which I believe to be rife among parapsychologists about the reasons which lead people to reject our evidence. I did not imagine that such an address was likely to enhance my popularity with members but since, unlike President Nixon, I shall not be running for a second term, I am free to speak my mind.

The first illusion which I want to challenge is the idea that there is some merit in being a believer. We naturally like to think of ourselves as somehow more imaginative, more finely attuned to nature's mysteries than other scientists and we look upon the skeptic as, by contrast, a dull dog, hidebound in his narrow and outdated materialism. Such an attitude, I submit, is a hangover from religion and has no place in science. Theologians have argued that the reason why God does not reveal Himself to us more plainly is that, if He did so, He would deprive us of the splendid opportunities we now have to exercise our faith in Him. The proudest boast of the faithful was to be able to exclaim, with Tertullian, "credo quia absurdum," which freely rendered might be translated as "I believe, not because of, but in spite of the evidence!"

In science, on the contrary, there is no merit in belief and, equally, there is no merit in disbelief. If merit

is anywhere involved it lies in following the implications of the evidence even when these run directly counter to one's fondest beliefs and expectations. The idea that the skeptic is prejudiced because he differs from us in his interpretation of the evidence finds little support in reality. Take the case of the late E. G. Boring of Harvard, the eminent historian of psychology. His long career encompassed in its span practically the entire history of modern psychology and his writings showed him to be a man of the widest sympathies who could discuss with understanding and appreciation the most diverse schools of psychology. Yet, for whatever reason, he drew the line at parapsychology. Almost the last thing he ever wrote was an enthusiastic introduction to Professor Hansel's book on ESP (Hansel, 1966) in which he contrived somehow to appear even more negative and destructive than the author himself!

I can well understand how infuriating it must be when, after great efforts, you have succeeded in obtaining evidence that would undoubtedly be regarded as conclusive in any other branch of science only to find that your colleagues either ignore your results or, worse still, question your credibility. But before you dismiss the skeptic as willfully obtuse, consider how it looks from his point of view. If we did not have evidence that ESP exists, nothing in all of science would have suggested it; on the contrary everything we know would tell against it. Is it therefore so unreasonable if the skeptic prefers to think that there must be some flaw in your evidence even if he cannot spot it, some factor X which you have forgotten to control for which would allow for a normal explanation of your findings? I have come to the conclusion after reading and talking to large numbers of parapsychologists that there are in fact very few experiments if any which they would all accept as conclusive. What satisfies one, another will find wanting. We can hardly blame the skeptic therefore if he refuses to accept some piece of evidence at its face value even though he cannot think of a plausible counterexplanation.

My friend Dr. Robert Thouless has pointed out that as more people carry out parapsychological experiments conviction must grow, because however suspicious you may be about the honesty of other experimenters you can scarcely doubt a result which you yourself have obtained (Thouless, 1972, page 97). Certainly, I imagined that a successful investigator, one who had, so to speak, been touched with the wings of glory, would forever after be freed from the

doubts that assail his less fortunate brethren. As I shall proceed to show, however, even this is not enough. Unless success can be sustained belief will sooner or later yield to doubt. Consider first two experiments which must surely rank among the most remarkable in the whole of our experimental literature.

In the year 1935 Dr. B. F. Riess, then of Hunter College, New York, was one of many psychologists of that period who were highly skeptical of the claims being put forth by the Duke University Parapsychology Laboratory. He decided very sensibly to try a card-guessing experiment for himself. Indeed J. B. Rhine personally encouraged him to do so. He found himself a suitable subject in the person of a young woman music teacher who had some reputation among her friends for being psychic. Every evening at 9 P.M. Dr. Riess, in his study at home, would go through two packs of ESP cards with himself as agent while his subject, in her home a quarter of a mile away, would record her guesses. The outcome, which is now a matter of history, is that at the end of 74 runs the girl had scored an average of 18 hits out of 25 trials (Riess, 1937). I wonder how many of you here have ever witnessed even a single run of 18 out of 25 under strict conditions. Yet here was a girl who could average 18 out of 25 on 74 successive runs! What happened next was as follows. The girl fell ill and when she recovered she was dismayed to find that she was no more capable of scoring above chance than any of the other 67 students whom Riess had tested.

What was the effect of these events on Dr. Riess himself? At a meeting of the American Psychological Association the following year he had the candor to declare publicly that although this finding outraged his own scientific philosophy, nevertheless he could see no possibility of explaining it away. The score sheets had been kept locked in his desk at home, Gardner Murphy had inspected the records and had been able to spot only a single instance of a recording error and, in short, the only person who could have cheated was himself (Pratt, 1964, page 65; Rhine et al., 1966, pages 167-169). To the best of my knowledge Dr. Riess has never recanted or disowned the experiment as being invalid and yet, for some reason, he quietly backed away and from that day to this has never so far as I know evinced the remotest interest in parapsychology.

My second example concerns an experiment carried out in 1919 at the University of Groningen in northern Holland by Dr. H. I. F. W. Brugmans, Dr. G. Heymans, and Dr. A. A. Weinberg. Drs. Brugmans and Heymans were, incidentally, two of the earliest pioneers of the modern statistical approach to the study of personality. The subject was a student of mathematics and physics at the university named A. van Dam. The set-up involved a modified chess board and the subject's task was to point to the particular square which had been randomly selected as the target for that trial. The experimenters, who were also the agents, were situated in a room above from which they could observe the board through a glass panel in the floor, although they could not be seen by the subject. Under these conditions van Dam was correct on 32 out of 80 trials--i. e., 40 percent of the time--as against a chance expectation of about 2 percent. On those sessions when he was given alcohol his scoring rose to 22 out of 29 trials--i. e., 76 percent correct. In his published report of 1921 Brugmans wrote: "We can therefore say without any reserve that the existence of a transfer of thoughts under circumstances which completely exclude communication by sense organs is proved by this" (translated from the original by Zanstra, 1962, page 107).

One might have thought that this declaration would have signalled the start of a great era of experimental parapsychology in the Netherlands before the Duke Laboratory had yet been thought of. Nothing of the sort happened. The incredible van Dam, like all his kind, soon lost his ability and Dr. Brugmans drifted away from parapsychology. He died not long ago and by then, so I gather from my Dutch friends, his position was one of open contempt for the whole field. [I have since been informed by Dr. H. C. Berendt of the Israel Parapsychology Society, Jerusalem, that A. A. Weinberg emigrated to Israel where he recently died. Unlike Brugmans his interest in parapsychology seems to have persisted to the end.]

There is one more example I would like to mention which is perhaps the most revealing of all, for it concerns two of the most learned men who have ever devoted a considerable portion of their lives to the study of the paranormal. They are Dr. Eric Dingwall and Dr. Theodore Besterman. The name of Dingwall is probably familiar to most of you here unless you happen to be newcomers to the field. Besterman, however, is less likely to be known

to you since he abandoned parapsychology in the 1930's. Since that time he has become an authority on the literature of the French Enlightenment, has recently published a big biography of Voltaire, and has edited Voltaire's letters in an edition which, I gather, runs to 106 volumes!

Now the relevant facts which I want to bring to your attention are as follows. Both men were at one time research officers of the Society for Psychical Research. In this capacity both had an opportunity to test the celebrated Polish clairvoyant, Stefan Ossowiecki (Besterman, 1933; Dingwall, 1924). They used very similar techniques: both prepared a crude little drawing which they then elaborately packaged and sealed. In both cases Ossowiecki succeeded, under observation in reproducing the drawing with almost perfect fidelity. Both men duly published statements saying that they were satisfied on all counts with the results and that the precautions they had taken ruled out any possibility of a normal explanation.

Eventually, and this is the whole point of my story, both men developed doubts. I thought it would be instructive to discover why; so, not long ago, I wrote to them and asked them. Dingwall replied that for a few hours prior to the critical session the target-packet had been in the possession of Dr. von Schrenck-Notzing, the well-known German parapsychologist. Since he no longer had any confidence in Schrenck-Notzing he could no longer vouch for the experiment. I must explain at this point that, at the time, it was thought important to demonstrate that Ossowiecki's powers were clairvoyant rather than telepathic and so, although Dingwall travelled to Warsaw, he could not be present at the critical session. This was how Schrenck-Notzing came into the picture; Dingwall had deputed him to act in his place. This was methodologically the right thing to do in any case, because if Dingwall had been present himself it would now be impossible to know how much information he might unwittingly have transmitted to the subject through facial expressions, lip-movements, and so on.

Because of this consideration, Dingwall's attempt to shift responsibility to his erstwhile colleague struck me as somewhat disingenuous, and I wrote to him again pressing him to be more specific. I inquired, in particular, whether he was suggesting that Schrenck-Notzing had conspired with Ossowiecki to fool him, or whether he thought that Ossowiecki

had somehow outwitted Schrenck-Notzing. I asked how, on either hypothesis, he proposed to explain his own published statement to the effect that the day after the critical session the target-packet was returned to him intact, with all its seals and private code-marks undisturbed. To this Dr. Dingwall replied that it was obvious that, like most parapsychologists, I adhered to what he called "the magical way of looking at the world" and, this being so, it would be a mere waste of his time to answer any more of my silly questions. So that was that.

Dr. Besterman, though he was not peevish like Dingwall, was a good deal less forthcoming. He confirmed that he had developed reservations but he wanted to make it clear that these in no way reflected upon his coexperimenter, Lord Charles Hope. This, I may say, is a crucial point because, in this instance, it was Lord Charles Hope who both supervised the test and opened the target-packet at the session in Warsaw while Besterman stayed behind in London. Thus to me, at least, it was clear that neither of these two great scholars really had any reason whatsoever for changing their minds, otherwise why would they have been so evasive? Yet their doubt was quite sincere.

You may say that this only goes to prove what unreasonable people skeptics really are. This may be so; certainly skeptics are just as capable of being irrational as believers. Yet, can we altogether blame them? The point is that, when he perished in the Warsaw uprising of 1944, Ossowiecki, arguably the greatest sensitive on record, left behind him no successors. The law of cognitive dissonance suffices to explain the sequel; in the fullness of time every miracle, no matter how incontrovertible it may once have appeared, will lose its luster.

No discussion of the problems of belief and doubt would be entirely frank without touching upon the distasteful and embarrassing question of experimenter fraud. There are some of you, I know, who feel strongly that this whole question is so outrageous that we should outlaw it from all our deliberations. While respecting the sincerity of your feelings, I cannot agree with this opinion. Of course, no worse calamity could befall a person in academic life than to be suspected of fraud. But, in the circumstances, this is an occupational hazard that we must accept. If you are too worried about it you would be better sticking to some safer occupation. I believe that I have a reputation

for honesty and yet I have no illusions that, if ever I were to make a claim that was worth doubting, a smear campaign would soon be mounted to suggest that I was a person of unsound mind. When all is said and done parapsychology is too important, too much is at stake, for it to be left entirely in the hands of gentlemen.

During this past year one of the ablest critics of parapsychology, who rightly believes that truth is more important than anybody's feelings, has been carrying out a minute examination of the famous Soal-Shackleton series of 1941-1943. I prefer not to divulge his name until he is ready to publish his own account, but I may say that the primary object of the exercise was to see whether there was any substance in the allegation made by a certain woman, who had been an agent in some of the sessions, to the effect that she had seen Soal altering figures on the score sheet. As part of his investigation, this critic initiated a multiple correspondence with six Council members of the Society for Psychical Research, including myself, who shared his interest in this particular episode of our history. Whenever any of us had any comment to make we would post copies of it to all the other members of the group. The experience gave me a unique glimpse into the mind of a highly sophisticated skeptic.

It eventually became apparent that no agreement could be reached. For although there were indeed some suspicious statistical irregularities in the data these might represent a peculiar pattern of the subject's ESP scoring. Only if this possibility were ruled out would the evidence be sufficient to condemn Soal. Hence, ultimately, the verdict was going to depend on which of the two alternatives you found the more incredible: that Soal was a fraud or that Shackleton was genuine. My critical friend did not pretend that it would have been particularly easy for Soal to have cheated throughout, to have hoodwinked everybody including his lynx-eyed coexperimenter Mollie Goldney, but he did quite sincerely believe that this was more likely than that Shackleton possessed ESP. And, from one point of view, the logic of his position was unassailable. We know for a fact that people do lie and cheat but, as I have said, it still remains for us to prove that there are people who have the gift of ESP. I realized that the only argument which would have carried any weight with my friend would be for me to produce for him another Shackleton whom he could test for himself, but that is precisely where I was completely powerless.

The special irony of this particular case is that the Soal-Shackleton series is among the few major investigations that was deliberately designed to be proof against experimenter fraud and yet, over the years, it has been a constant focus of suspicion! There is, however, one lesson we have learned as a result of this unfortunate affair and it is this: if you have a good subject do not keep him to yourself; let someone else test him before it is too late. Good subjects are so rare that you are naturally tempted, when at last you find one, to monopolize him. However, if you loan him out, as Dr. Ryzl did with Stepanek, for example, or Dr. Eisenbud did with Serios, then you avoid having to bear alone the whole onus of suspicion.

It may strike some of my forward-looking American colleagues as strange, not to say perverse, that we British parapsychologists should be so obsessed with our past. I think this is a reflection of the fact that, when it comes to the phenomena, the Atlantic does seem to represent some kind of a divide. There have been occasions this year when, getting together with my friends at the Society for Psychical Research in London, we have asked one another, in mournful tones, whether perhaps ESP is not just something that happens in America! Certainly, since Soal retired from the field some twenty years ago there has been precious little sign of it in these parts, at any rate in the laboratory. Of course part of the explanation lies in the sheer magnitude of the American research effort relative to our own, as this conference so strikingly illustrates; and yet I wonder whether this can be the whole story.

I must beg my American friends to believe me when I say that we have not been entirely idle over here. I do not wish to cite my own paltry efforts in this context (I may just be an incompetent investigator), but I cannot help recalling my indomitable friend Dr. George Medhurst who unhappily died about a year ago of an illness. In addition to a distinguished career as an electrical engineer, Dr. Medhurst devoted practically all his leisure hours to parapsychology. Over the years he tested literally thousands of individuals, always hoping that another Shackleton might turn up or, at the very least, that he would obtain some firm evidence for the reality of ESP. He died, alas, a disappointed man. Yet his experience was so common by British standards that it never evoked any comment; no one suggested, for example, that he had a negative effect on the phenomena. I do not know why it is that British

parapsychology, which once led the world, has sunk to such a low ebb. Presumably there is a tide in all these things and, hopefully, the tide may one day turn again. Meanwhile, however, I think it is important to understand that there are these unexplained national differences within our field. No doubt the situation on the Continent is different again.

Perhaps, in view of all that I have been saying, we should start to pay more attention to that growing body of opinion which believes that we should strive to gain a better theoretical understanding of the phenomena rather than to continue piling up evidence. Supporters of this view often take their stand on the Kuhnian interpretation of scientific history according to which science goes through a succession of revolutionary transformations. The implication is that, after the next revolution in science, parapsychology will fall naturally into place and what now appears to us as anomalous or "paranormal" will then become an integral part of the scientific world view. Sometimes reference is made to an Einstein-figure whose genius will bring order out of chaos. No one could deny that this is an alluring prospect, and I do not wish to deflate it, but I must confess that I can see no sign that such a revolution is imminent and if it means that parapsychology must mark time until the revolution arrives, I fear that the outlook is very bleak indeed.

Fortunately, there is another way; and if it is less grand, at any rate it is a good deal more practical. I refer to the search that has been going on for a repeatable parapsychological experiment. To avoid cavil, let me quickly add that, by "repeatable," I mean that the predicted effect must work at least 50 percent of the time and, even more important, must not depend on the availability of a particular individual as subject. Now, it may be optimistic to think that such an experiment is even possible, but I do not think that anyone could call it utopian. Though it may never come about it may alternatively be lurking just round the corner. It may, indeed, be implicit in one or another of the papers we shall be hearing at this conference. I am thinking especially of the exciting recent developments in the field of animal experimentation as well as of the possibilities inherent in Dr. Schmidt's ingenious machines.

Once such an experiment became public knowledge things really would start to move. In one university after

another research workers and Ph. D. students would be clamoring to try it out; journals like Nature and Science would open their columns to them; psychology textbooks would hurriedly be revised and the national research foundations would loosen their purse-strings. At the end of it all we might still not understand ESP but, then, do we understand any psychological phenomenon in any fundamental sense? At any rate, ESP would at last have become a demonstrable phenomenon along with hypnosis, conditioning, the reminiscence-effect and even subliminal perception.

Such an outcome might, of course, have some rather disconcerting repercussions. It is conceivable that this organization of ours would be swept aside. What became, I sometimes wonder, of the Interplanetary Travel Society of my youth when the space age actually arrived and NASA came into being? One parapsychologist with whom I once discussed the point told me that so far as she was concerned a repeatable experiment would take all the fun and adventure out of parapsychology and it would then become just another routine science. Undoubtedly progress always exacts a price, but of one thing we can be certain: when that day arrives the long-drawn-out struggle between belief and doubt will at last be over.

While preparing this address it occurred to me that the city of Edinburgh forms a fitting backdrop to my theme. For not only is Edinburgh the city of David Hume, the great philosopher, but it also has associations with his near namesake Daniel Home who was born in the village of Currie just outside. David Hume, whose life fell wholly within the eighteenth century, was the author of a famous essay on miracles which is still one of the best statements of the skeptical position ever written and ought to be compulsory reading for every aspiring parapsychologist. Daniel Home, whose life fell wholly within the nineteenth century, was a man of mystery. A scion of the noble house of Home (of which the present British Foreign Secretary is the most illustrious living representative), he was, by all accounts, the greatest miracle-monger in the history of the human race. We have heard something about this unique individual earlier in this convention in Mr. Fraser Nicol's paper "A Psychic Tour of Historic Edinburgh." Later on we shall be hearing more from my friend Dr. Zorab of the Hague who has spent many years researching his life (see "ESP Performances of D. D. Home"). And so, poised

betwixt the twin pillars of belief and doubt, of Hume and of anti-Hume, I must now take my leave of you and it only remains for me to wish you all every success in all your enterprises and a very pleasant conference.

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OUT ON A TIGHTROPE: PARAPSYCHOLOGY AND PHYSICS*

Arthur Koestler

Some of you may remember a remarkable periodical published in the 1950's by the physicist Dr. Irving J. Good, called the Journal of Half-Baked Ideas. A selection of articles from it was later published as a book with the title The Scientist Speculates: An Anthology of Partly-Baked Ideas (Good, 1962b). The paper I am about to deliver is a sort of club-sandwich of partly baked ideas, and I can only hope that it won't give you indigestion.

A club-sandwich has several layers. The top layer, on which I shall start, is a fully baked crust which through long exposure has almost hardened into cliché. The subsequent layers will be half-baked, quarter-baked, and so on, until we reach the bottom layer of shamelessly raw speculation.

In a recently published book I tried to make the point that the unthinkable phenomena of parapsychology appear somewhat less preposterous in the light of the unthinkable phenomena of modern quantum physics (Koestler, 1972). This argument is by no means new; it has been so often and so brilliantly demonstrated that it has almost become a commonplace. But it can also be applied, less fashionably, to classical pre-Heisenberg physics. To mention a single example: from the point of view of naive common sense, the type of action-at-a-distance called telepathy is no more mysterious than that other action-at-a-distance called universal gravity. When Kepler, eighty years before Newton, came out with the wild suggestion that the tides were caused by the attraction of the moon, even Galileo dismissed the idea as an occult fancy, which contradicted the

*This is the Invited Dinner Address, presented September 4, 1972.

laws of nature. And Newton himself vehemently rejected the concept of universal gravity unless there existed some interstellar medium which transmitted it. In his third letter to Bentley, he wrote: "That one body may act upon another, at a distance, through a vacuum, without the mediation of anything else ... is to me so great an absurdity that no man who has ... a competent faculty of thinking can ever fall into it" (Turnbull, 1961).

Yet fall we all did, as schoolboys in the classroom, without becoming aware of our fallen state. Such is the power of mental habituation. To live with a paradox is like being married to a nagging bitch: after a while you become deaf to her nagging and settle down in comfortable resignation.

Thus even classical physics could only make progress at the price of insulting common sense and by breaking and re-making the previously sacrosanct laws of nature. Modern physics had to repeat both offences in even more brutal ways. And parapsychology has to carry a similar burden of guilt. Einstein, de Broglie and Schrödinger between them have dematerialized matter like the conjuror who makes the lady vanish from the box on the stage. Heisenberg replaced determinism by uncertainty and causality by statistics; Dirac postulated holes in space stuffed with electrons of negative mass; Thomson made a single particle go through two holes in a screen at the same time--which, Cyril Burt commented, is more than a ghost can do. Photons of zero rest-mass have been observed in the process of giving virgin birth to twins endowed with solid rest-mass; Feynman made time flow backwards on his diagrams; and these are but a few glimpses from the surrealistic panorama which quantum physics has opened up for us. To paraphrase an old saying: inside the atom is where things happen that don't.

The astronomers are having an equally gay time. The big bang versus continual creation controversy would have delighted medieval theologians. Radio astronomers claim that they can hear background noises which must have originated with the pristine bang of creation. More recently the universe became pockmarked with black holes into which the mass of collapsing stars is sucked at the speed of light, to be annihilated and vanish from our universe into the blue yonder. The universe is turning out to be a very odd place indeed, and we no longer need ghosts to make our hair stand on end.

My purpose in reminding you of these well-known developments was to underline once more the fact that the mechanistic and deterministic world-view, which is still dominant in sociology and the behavioral sciences as well as in the public at large, has no longer a leg left to stand on; it has become a Victorian anachronism. The nineteenth-century clockwork model of the universe is in shambles and, since matter itself has been dematerialized, materialism can no longer claim to be a scientific philosophy.

As a side-effect of this philosophical crisis, we may observe a curiously reciprocal development in the exact sciences on the one hand, and parapsychology on the other. For the last fifty years our leading physicists have been playing around with more and more obscure mental constructs, whose quasi-mystical implications are camouflaged by technical jargon and mathematical formalism. If Galileo were alive, he would certainly have accused them of dabbling in occult fancies. At the same time he might have looked with a benevolent eye at the parapsychologist's increasing reliance on hard statistics, rigorous controls, mechanical gadgets, and electronic computers. Thus the intellectual climate in the two camps seems to have been changing in opposite directions: Rhine's successors, because of their statistical orientation, have sometimes been accused of scientific pedantry, while Einstein's successors were accused of flirting with ghosts in the guise of particles which possess no mass, nor weight, nor any precise location in space.

I believe that this apparent convergence is more than a surface phenomenon. But one must be careful in drawing conclusions from it. The time for physics and parapsychology to fall happily into each other's arms is not yet. What both increasingly have in common are the two negative attributes that I mentioned a minute ago: both defy common sense, and both defy the previously accepted laws of nature. They are both provocative and iconoclastic. And, to say it once more, the baffling paradoxes produced by one make the baffling paradoxes of the other appear a little less preposterous. If whole stars can vanish into black holes there may also be singularities in the continuum which produce poltergeists.

One might call this a kind of negative affinity. In concrete terms it does not amount to much. But philosophically and emotionally it seems to me significant. It helps

to put one's nagging doubts at rest. It is encouraging to know that if the parapsychologist is out on a limb, the physicist is out on a tightrope.

But unavoidably the question arises whether there are any signs on the horizon of a positive affinity or convergence between post-materialistic physics and post-spiritualistic parapsychology? I think one can distinguish two such signposts, the first of a subjective, the second of an objective, nature.

An impressive number of eminent physicists, including several Nobel laureates, have shown an inclination to flirt with parapsychology--witnessed by the list of past presidents of the (British) Society for Psychical Research; and, as in other subversive movements, the number of fellow-travellers far exceeds that of the card-carrying members. Thus, for instance, the discoverer of the electron, Sir Joseph J. Thomson, was one of the earliest members of the Society. Now why should physicists in particular show this proneness to infection by the ESP bug? The answer can be found in the autobiographical writings and metaphysical speculations of the greatest among them. The dominant chord which you can detect in nearly all of them is a pervasive feeling of frustration, based on the realization that science can only elucidate certain aspects or levels of reality, while the ultimate questions will always elude its grasp, vanishing into infinite regress like images reflected in a hall of mirrors. "Physics is mathematical," wrote Bertrand Russell, "not because we know so much about the physical world, but because we know so little; it is only its mathematical properties that we can discover" (Russell, 1927). This resigned agnosticism leads either into a spiritual desert--Schrödinger in his middle age gave up physics in disgust--or, more often, to a new open-mindedness, a sophisticated kind of innocence on a higher turn of the spiral.

So much about what I have called the subjective aspect of convergence. The next step is to look for objective convergences, areas where the domains of physics and parapsychology might enter into direct contact. But this step means digging into a deeper layer of the club-sandwich, which is no longer fully baked. There is no need to dwell here on earlier abortive efforts to provide a physical explanation of ESP by radio waves and the like. They were honorable attempts at dressing the wolf in sheep's

clothing, and inevitably failed. With the advent of quantum theory, however, these attempts became considerably more sophisticated--we might say that they progressed from the quarter-baked to the half-baked stage. Examples are Axel Firsoff's hypothesis of extrasensory communication by means of mindons--the hypothetical particles of an all-pervasive mind-stuff, with properties somewhat similar to the neutrino's; Martin Ruderfer's complex theory of a neutrino sea interacting with matter; and the late Adrian Dobbs's psitrons, swarms of particles of imaginary mass, travelling along a second, imaginary time dimension, and capable of impinging directly on neurons in the percipients' brains.

These theories are of considerable ingenuity and I do not think that physicists could find any obvious fault with them. Yet, like other similar efforts, they fail to satisfy because they give the impression of improvised bridges across the nasty abyss, supported by ad hoc hypotheses. One cannot help feeling that these imaginative efforts are stimulating but premature, and that they suffer from what Whitehead called "misplaced concreteness." To put it differently, their authors seem to remain under the spell of the physicist's concepts and categories, instead of creating their own, autonomous conceptual systems, a universe of discourse commensurate with the phenomena in their own field--as biology has done to some extent. I would like to quote here a speaker at an ESP forum sponsored by the American Society for Psychical Research, Henry Margenau, professor of physics at Yale:

> I have probed physics for suggestions it can offer towards a solution of the sort of problems you seem to encounter. The positive results, I fear, are meagre and disappointing.... But why, I should like to ask, is it necessary to import into any new discipline all the approved concepts of an older science in its contemporary stage of development? Physics did not adhere slavishly to the Greek rationalistic formulations that preceded it; it was forced to create its own specific constructs....
>
> The parapsychologist, I think ... must strike out on his own and probably reason in bolder terms than present-day physics suggest [Margenau, 1967].

This does not mean of course that parapsychology should cut itself off from the mainstream of scientific research, and retire into an ivory tower. But that mainstream itself is now flowing into bold new directions which seem to point to an indirect sort of convergence in the future--not by premature shortcuts, but by a sort of isomorphism or Gestalt affinity. I am referring here to what one might call the mentalistic trend in biology and physics with its explicit or implied admission of the power of mind over matter. This trend seems to be an indirect consequence of those paradoxical developments in physics which I have mentioned before. In its still early days, Sir James Jeans made his celebrated pronunciamento: "Today there is a wide measure of agreement, which on the physical side of science approaches almost to unanimity, that the stream of knowledge is heading towards a non-mechanical reality; the universe begins to look more like a great thought than a great machine" (Jeans, 1937).

This statement was not meant as a poetic metaphor; it was the embarrassing, but inescapable conclusion emerging from the physical laboratories. There were several aspects to it. One of the most fundamental was the principle of complementarity. It stated that the smallest constituents of the universe are ambiguous, Janus-faced entities which under certain conditions behave like hard little pellets, under other conditions like waves in a non-material medium. These two types of behavior mutually exclude each other, but also mutually complement each other. Heisenberg was apparently the first to recognize that this complementarity may be regarded as a paradigm of the dualism of matter and mind. In his autobiography he was even more explicit. "Atoms are not things," he wrote. "When we get down to the atomic level, the objective world in space and time no longer exists" (Heisenberg, 1969). From here there is only one step to the realization that the contents of mental experience also defy definition in terms of space, time, and substance, yet are somehow linked with the material brain--as the wave-function of the electron is somehow linked with its material aspect.

Eugene Wigner, another Nobel laureate, carried the analogy one step further. He reminds us that in the last century it was taken for granted that matter can produce and influence light, but light can never influence matter. Yet the discovery of the Compton effect proved that light exerts pressure on material bodies, and later it was found

that it can even give birth to material bodies (the pair-formation of electrons and positrons mentioned earlier). Mutatis mutandis, he argues, the physical world influences the mind, but the mind also has a direct influence on the physical world. For "we do not know of any phenomena in which one object is influenced by another without exerting an influence thereupon" (Wigner, 1962).

Niels Bohr, von Neumann, and others went even further. One of the difficulties which bedeviled quantum theory from the beginning was the inevitable interference of the observer with the process observed, which led into insoluble paradoxes. The mental processes of the observer assumed the status of an irreducible datum--not for the trivial reason of the observer's fallibility, but because of the logical conundrums inherent in measuring events on the quantum level and describing them in probabilistic terms. David Bohm, in a recent anthology, Quantum Theory and Beyond, following in the footsteps of Niels Bohr, explicitly rejects "any kind of description which considers the 'observer' and the 'observed system' as separately existent" and maintains that "it has no meaning to say, for example, that there is an 'observed object' that interacts with the 'observing instrument' " (Bohm, 1971).

Now this, as Ludvik Bass has pointed out (1971), is sheer Vedantism--which teaches that the objects you behold cannot be separated from the mind of the beholder; the observer and the thing observed constitute a single, indivisible fluid reality: I am thou and thou art I. To quote Wigner again, "all the possible knowledge concerning any object can be given as its wave function [Schrödinger's psi]" (Wigner, 1962). But psi is a symbolic abstraction based on another mental construct, namely probability theory --and not a model of reality in the sense in which a road map is a model or the mechanistic clockwork universe aspired to be a model. One might conclude, with Dr. Good (in The Scientist Speculates) that "the physicist's psi function [Schrödinger's basic wave equation] is mysterious enough to provoke the conjecture that it may in some sense explain features of the mind. Perhaps the psi of quantum physics depends on the psi of the parapsychologists" (Good, 1962a). Other scientists have pointed half jokingly, half seriously, at the hidden sympathies between the two psi's.

Thus there is not only a negative convergence between

the two domains in the sense of a shared contempt for common sense and mental smugness; there are also portents of a tentative positive convergence--which, however, is more implicit than explicit, potential rather than actual, intuitive rather than logical--a sort of Gestalt affinity as I called it before. It should not be hurried or forced; I am old fashioned enough to believe that courtship should precede copulation. The great syntheses in the history of thought emerge when the time is ripe for them--when all the components which are to go into the new synthesis are already present. Neither science nor para-science appears to have reached that stage.

One might add here, as a footnote, that in biology too there is a growing tendency to recognize the power of mind over matter. Some twenty years ago, Sir John Eccles created quite a stir when he proposed that the exercise of conscious volition--a dirty word in behavioristic psychology--could, by affecting a single neuron, trigger off changes of activity in large areas of the cortical network (Eccles, 1953). Since then, other researchers have shown that mental volition, assisted by various types of biofeedback apparatus, can influence the activities of the autonomic nervous system and bring on the alpha wave rhythm of the brain.

* * *

We have now arrived at the last layer of the club-sandwich, which is almost completely unbaked. I am approaching it with a certain amount of trepidation, the more so as I must now revert to some anecdotal material--which, however, may come as a relief after so much quantum jabberwocky.

When my recent book, The Roots of Coincidence, was published I received, as one might expect, a good many letters from people anxious to relate their experiences. I shall quote from two of these, which seem to me remarkable in their different ways.

The contents of the first I must relate in a slightly camouflaged version to spare the feelings of those involved. It came from an architect whose younger brother, a brilliant student, had suffered a nervous breakdown and thrown himself in front of an incoming train in a London tube station. He suffered a fractured pelvis, punctured abdomen

with extrusion of intestine, lacerated back and severe bruising, but survived. They had to jack up the train to get him out; he had been under it, but the wheels had stopped just short of his body. However, according to the hospital doctor's account to the victim's relatives, which was later confirmed by an official of London Transport, the train was not stopped by the driver applying the brakes (the time lag was apparently too short for that) but because a passenger in the train, quite unaware of what was happening, had pulled the emergency handle.

I passed the case on to a friend who was willing to investigate it--Mr. Tom Tickell, who works on the editorial staff of The Guardian--the son of Renée Haynes, Editor of the Journal of the Society for Psychical Research. Mr. Tickell contacted London Transport, but ran into the traditional barrier of red tape. The identity of the passenger who had pulled the emergency handle was allegedly unknown. The name of the driver of the train was eventually disclosed, but not his address. A letter to the driver, addressed c/o London Transport, remained unanswered. Thus, as so often happens, the case petered out, leaving but a muddy trail.

The next case is in a different vein. What follows is an extract from a letter J. B. Priestley wrote me on February 7, 1972, after he had read my book. No doubt you know that Priestley is married to Jacquetta Hawkes, the archaeologist.

> My wife bought three large coloured lithographs by Graham Sutherland. When they arrived here from London she took them up to her bedroom, to hang them up in the morning. They were leaning against a chair and the one on the outside, facing the room, was a lithograph of a grasshopper. When Jacquetta got into bed that night, she felt some sort of twittering movement going on, so she got out and pulled back the clothes. There was a grasshopper in the bed. No grasshopper had been seen in that room before, nor has been seen since. No grasshopper has ever been seen at any other time in this house.

The first of these stories might possibly be explained by ESP which prompted the unknown passenger to pull the emergency handle; the second, in common with many coincidental happenings, defies explanation in both conventional

and parapsychological terms. They are equally baffling to the theologian, for if the passenger's action is to be credited to providence, what prompted providence to put a grasshopper in Mrs. Priestley's bed? I have never heard it suggested that providence has a sense of humor.

Whether one believes that such highly improbable meaningful coincidences are manifestations of some unknown principle operating beyond physical causality, or are produced by the proverbial monkey at the typewriter, is a matter of inclination and temperament. I have found to my surprise that the majority of my acquaintances--among whom scientists predominate--belong to the former category, although some are reluctant to confess it, for fear of ridicule, even to themselves. Carl Jung had the same experience among his patients, which was perhaps not surprising; more surprising is that Nobel laureate Wolfgang Pauli (one of the chief architects of quantum theory, who predicted the existence of the neutrino) cooperated with Jung on the latter's famous treatise "Synchronicity: An Acausal Connecting Principle." Jung defines synchronicity as "the simultaneous occurrence of two or more meaningfully, but not causally connected events"; and the acausal factor behind such events is said to be "equal in rank to causality as a principle of explanation" (Jung, 1952).

The origins of the Jung-Pauli synchronicity concept can be traced back partly to Schopenhauer, partly to the Austrian biologist Paul Kammerer who published a book (which put an end to his academic career) called Das Gesetz der Serie (Kammerer, 1919). Kammerer's concept of seriality referred to the recurrence or clustering of meaningfully but not causally connected events--familiar to all gamblers and insurance companies. He postulated that coexistent with causality there is an acausal principle active in the universe which tends towards unity. It is in some respects comparable to universal gravity, but whereas gravity acts indiscriminately on inert mass, this force correlates by affinity, or a kind of selective resonance. "We thus arrive," he writes, "at the image of a world-mosaic or cosmic kaleidoscope, which, in spite of constant shufflings and rearrangements, also takes care of bringing like and like together."

Now this sounds pretty wild talk in the twentieth century, but in fact the concept goes back all the way to the Hippocratic "sympathy of all things": "There is one

common flow, one common breathing, all things are in sympathy." This doctrine that everything in the universe is hanging together, not by mechanical causes but by hidden affinities which account for apparent coincidences, was not only the foundation of primitive magic, of astrology and alchemy; it also runs as a leitmotif through the teachings of the Pythagoreans, Neoplatonists, and the philosophers of the early Renaissance. Jung's dualism of causality and acausal synchronicity was neatly formulated by Pico della Mirandola:

> Firstly there is the unity in things whereby each thing is at one with itself, consists of itself, and coheres with itself. Secondly, there is the unity whereby one creature is united with the others and all parts of the world constitute one world [Mirandola, 1557].

The scientific revolution put an end to this type of thinking and proclaimed mechanical causality as the absolute ruler of matter and mind. Yet three centuries later we are witnessing a swing of the pendulum in the opposite direction. On the quantum level the absolute rule of causality has come to an end; and Schrödinger's psi function, which defines a single electron, is spread out, Mirandola-wise, over the whole universe. On the cosmic scale Mach's principle, endorsed by Einstein, stipulates that the inertial forces on earth are governed by the total mass of the universe around us. Whitehead commented:

> It is difficult to take seriously the suggestion that these domestic phenomena on the earth are due to the influence of the fixed stars. I cannot persuade myself to believe that a little star in its twinkling turned round Foucault's pendulum in the Paris Exhibition of 1851 [quoted in Sciama, 1959].

But there it is. Mach's principle has become an integral part of modern physics, even though it smacks of the Hippocratic "sympathy of all things." For it implies not only that the universe at large influences local events, but also that local events have an influence, however small, on the universe at large. Everything hangs together; microcosm reflects macrocosm and is reflected by it.

In biology, too, there is a search for new principles --or, perhaps, a revival of earlier insights--which would

provide a more satisfying approach to the creative aspects of evolution than Neo-Darwinism, for all its historic merits, has been able to provide. Jacques Monod's Chance and Necessity (1971) may turn out to be the swan song of a rash generation of scientists who claimed that chance mutations plus natural selection provide the complete explanation of the emergence of higher levels of organization, of more complex structures and forms of behavior. Today more and more biologists are coming to realize that random mutations may provide part of the explanation, but not the whole explanation and perhaps not even an important part of it.

At the same time, the tyranny of the Second Law of Thermodynamics with its implied tendency towards transforming cosmos into chaos seems to be approaching its end, with the realization that the law applies only to so-called closed systems, whereas in open systems such as a living organism, an opposite tendency seems to be at work --creating order out of disorder, cosmos out of chaos, designing patterns where none existed before. This ubiquitous constructive principle has been proposed by various authors under various names; it carries echoes of Galen's and Kepler's facultas formatrix, Goethe's Gestaltung, and Bergson's élan vital; in more recent times the German biologist Woltereck proposed the term "anamorphosis," which von Bertalanffy adopted, while L. L. Whyte called it the "morphic principle." It is related to Schrödinger's concept of organisms feeding on negative entropy, which again is related to what I have called elsewhere the Integrative Tendency.

What all these tentative formulations have in common is that they regard the morphic, or formative, or Integrative Tendency, the striving towards higher forms of unity-in-diversity, as an essential factor in biological and mental evolution, and as an irreducible principle, which is as fundamental to the sciences of life as its antagonist, the Second Law of Thermodynamics, is to inanimate matter. Whether you call such a principle causal or acausal is a matter of semantics.

I would like to end this talk by mentioning briefly two somewhat bizarre, way-out experiments. A young graduate student named Stuart Kauffman at California Medical School created quite a stir some four or five years ago by setting up a system of several hundred simple binary on-off switches; each switch had the inputs coming

into it from two other switches chosen at random; and each input channel had one of the functions of Boolean logic--yes, no; and, or--assigned to it, again at random. Then he fed an electric impulse into that chaotic system and watched what was going to happen. What happened was that the system soon settled down into a cyclic routine, the impulses going round in a complex stable pattern, or one of several alternative patterns--order had been generated from disorder. What's more, when the system's routine was disturbed, the pattern soon righted itself--the originally random system manifested a kind of homeostasis (Kauffman, 1969).

The second experiment is something of a skeleton in the cupboard of the Society for Psychical Research. I am referring to the famous Spencer Brown controversy of twenty years ago. Brown claimed that by matching pairs of digits at random, the first digit symbolizing an ESP guess, the second the target card, he obtained a significantly higher number of hits than chance expectation. Mr. Arthur T. Oram, an expert statistician, aided by several volunteer workers, then undertook the task of verifying Brown's results. His team matched no less than 50,000 digits taken from random tables. The result was strictly according to chance expectation, so all seemed well. But then Spencer Brown made a thorough analysis of Oram's tables--and discovered the classic decline effect of hits with odds against chance of the order of 7,000 to one (Brown, 1957). It should be pointed out that Brown did not question the validity of the results obtained by ESP experiments--which he accepted at face value; but he thought that they pointed to some anomaly or hidden factor in the very nature of randomness. He did not elaborate on the nature of this suspected anomaly, but the idea bears a close resemblance to Kammerer's seriality and Jung-Pauli's synchronicity--the morphic or patterning or Integrative Tendency invading even the sober realm of random tables--as it invaded Kauffman's anarchic random circuits. It seems that nature is fond of blowing smoke-rings.

Sir Alister Hardy, in his Gifford Lectures, seems inclined to believe that Spencer Brown was on the right track. This, he pointed out, would by no means invalidate the evidence for "true" ESP in spontaneous cases and also in some laboratory experiments. But he surmised that the results of the majority of card-guessing and other statistical experiments "may be due to something quite different from

telepathy ... something no less fundamental and interesting ... something implicit in the very nature and meaning of randomness itself.... Let me say that if most of this apparent card-guessing and dice-influencing work should in fact turn out to be something very different, it will not, I believe, have been a wasted effort; it will have provided a wonderful mine of material for the study of a very remarkable new principle" (Hardy, 1965).

I may add that Hardy himself has in the meantime produced a substantial body of evidence, to be published shortly, for that hypothetical new principle--which dates back, as I said, to Hippocrates. It seems to be guided by E. M. Forster's motto: connect, always connect. How it works we do not know. We only know that it cannot work within the framework of classical causality any more than the quantum phenomena can be fitted into it. Perhaps it is somehow related to the physicist's god of the gaps. Perhaps the roots of coincidence sprout from those gaps. To try to explain by it how the grasshopper got into Mrs. Priestley's bed would be a grotesque exercise in misplaced concreteness. But the little mystic who is hidden inside each great scientist, longing to be let out, may perceive a connection which earlier cultures have always taken for granted. If I were asked to sum up in a single sentence these partly baked ideas at the end of my talk, I would propose this paraphrase on Spinoza: "Nature abhors randomness."

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GLOSSARY

AGENT. In telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the target.

ASTRAL PROJECTION see OUT-OF-THE-BODY EXPERIENCE

CALL. An individual guess to a specific target.

CLAIRVOYANCE. ESP of a physical event.

DECLINE EFFECT. A decline in scoring during a series of trials.

DIFFERENTIAL EFFECT. A differential scoring rate between two procedural conditions within the same experiment.

DISPLACEMENT. An ESP response to a target other than the intended one.

DT [Down Through] PROCEDURE. The clairvoyance method in which the cards are called down through the pack before they are checked.

ESP [Extrasensory Perception]. Information obtained by a person about an event without the use of known means of information.

ESP CARDS. Cards bearing one of five standard symbols: circle, cross, square, star, and wavy lines.

FREE VERBAL RESPONSE METHOD (FVR). Any procedure in which the range of targets is not known to the subject, such that he is free to make any response he wants.

GESP [General Extrasensory Perception]. Any method designed to test the occurrence of ESP which permits

either telepathy or clairvoyance or both to operate.

MATCHING PROCEDURE. Any procedure in which the subject matches one set of cards (or objects) against another.

OUT-OF-THE-BODY EXPERIENCE (OOBE). A state in which one's "self" is experienced to be located at a specific place outside the physical body. Also called astral projection.

PERCIPIENT. The person who is receiving information through ESP, especially information coming from an agent or sender.

PK see PSYCHOKINESIS

POLTERGEIST. A type of spontaneous case characterized by localized household disturbances, especially unexplained movements of objects.

PRECOGNITION. ESP of a future event.

PSI. Psychic ability in general, including ESP and PK.

PSI-HITTING. Exercise of psi ability in a way that hits the target at which the subject is aiming.

PSI-MISSING. Exercise of psi ability in a way that avoids the target the subject is attempting to hit.

PSYCHIC. Pertaining to psi; also, someone who is a sensitive.

PSYCHOKINESIS (PK). A physical effect produced by a person without known intermediaries.

PSYCHOMETRY. The ESP method in which an object (known as a token object) is used to obtain information about events associated with it.

RETROCOGNITION. ESP of a past event.

RUN. A group of consecutive trials.

SENSITIVE. An individual who purportedly has strong psi ability.

SPONTANEOUS CASE. An unplanned natural occurrence apparently involving psi.

SUBJECT. The person whose psi ability is being tested.

TARGET. The aspect of the subject's environment toward which he is asked to direct his psi ability, such as an ESP card or a rolling die.

TELEPATHY. ESP of a mental event.

TOKEN OBJECT see PSYCHOMETRY

TRIAL. A single attempt by the subject to use his psi ability.

NAME INDEX

*Convention participants are identified by asterisks.

SUBJECT INDEX