RESEARCH IN PARAPSYCHOLOGY 1976

Abstracts and Papers from the
Nineteenth Annual Convention of the
Parapsychological Association, 1976

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Editors

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PREFACE

Shortly after Martin Johnson was installed as professor of parapsychology at the State University of Utrecht, Holland, the University issued an invitation to the Parapsychological Association to hold its 1976 Convention at Utrecht.

Twenty-three years before, in 1953, another international parapsychological gathering met at this university. Sponsored by the Parapsychology Foundation of New York, the meeting honored W. H. C. Tenhaeff who had been appointed professor of parapsychology that year. Tenhaeff became professor "extraordinarius," and the position would have terminated at his retirement had he not succeeded in persuading the University to create a professorship "ordinarius" in parapsychology. The University chose Johnson for the new chair. He also became director of the Parapsychology Laboratory. This is located in the same building which houses the Laboratory for Experimental Psychology and there is a close relation between the two. Tenhaeff remained head of the Parapsychological Institute, located in another part of town, so Utrecht now has two professors of parapsychology. Their approaches to the field differ widely. Tenhaeff stresses field or clinical studies while Johnson emphasizes controlled laboratory testing.

Tenhaeff, who celebrated his 80th birthday in 1974, did not give any formal paper at the Convention but the Program Committee persuaded him to head an informal presentation which included a film from Japan and a talk by Gerard Croiset describing his successful search in that country for a lost child. On the same occasion, Hans Bender paid tribute to Tenhaeff's support for the Freiburg Institute.

The program was large and varied. To encourage interaction between professional parapsychologists and persons with a lay interest, the Program Committee accepted contributions which span a wide spectrum of scientific competence. Any shortcomings could then be addressed in the discussions following the presentations. Space does not allow the inclusion of that part of the program here, but the reader can write to the authors for clarification of uncertain points.

The Editors

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THE NINETEENTH ANNUAL CONVENTION

The Nineteenth Annual Convention of the Parapsychological Association was held at the State University of Utrecht, the Netherlands, on August 18-21, 1976. A total of 145 people attended. Martin Johnson was chairman of the Program Committee, which also included John Beloff and Sybo Schouten. These three served additionally on the Arrangements Committee. The Research Briefs chairman was John Beloff.

Sir John Eccles, Nobel laureate neurophysiologist, was the Invited Dinner Speaker. Martin Johnson of the Parapsychology Laboratory at the State University of Utrecht delivered the Presidential Address. These two addresses are given in full as parts 19 and 20 at the end of the volume. Parts 2 through 9 contain the research briefs, which describe ongoing or recently-completed experiments and other short studies. These are followed by condensed versions of the full papers and symposia.

RESEARCH BRIEFS I*

THE EARTHQUAKE EFFECT: D. D. HOME'S UNIQUE AND POWERFUL PK PHENOMENON

George Zorab (Zoetermeer, the Netherlands)

The nineteenth-century physical medium D. D. Home, in the course of thousands of sittings, was never caught cheating. This is especially impressive in view of the fact that the greater part of these sittings were conducted in a good light. The principal PK phenomena witnessed were the moving about and levitation of all kinds of objects, including some very heavy ones. But there was also a PK phenomenon often reported to have occurred that surpassed in its explosive output of PK energy all the other PK manifestations of this remarkable physical medium. This so-called "earthquake effect" can best be illustrated by quoting the seance reports of acute and trustworthy witnesses. First we have that of the later Earl of Crawford describing a sitting at Florence in February 1856:

> After the rest of the circle had taken their places ... the usual manifestations occurred. Taps began almost at once on the underside of the table, and then the table began to vibrate, and then the chairs; and then the floor, and then the whole room trembled and shook, while the china rattled on the table at the further end of the room.

The writer then continued, saying that he looked under the table but saw nothing except the feet of the persons present. Directly after this enormous outburst of PK energy the reporter, who was standing apart from those gathered around the heavy seance table (seating about eight persons), saw this table "suddenly rise up to the height of four feet--it remained

*Chairperson: A. Gregory, London.

suspended in the air for about half a minute, swaying about in different directions--I again looked under the table, while it was moving about, but there was nothing visible, and then it came down gently...."

During the summer of 1855, J. J. Garth Wilkinson, M.D., attended several sittings with Home as the medium, some in his own London home. In the course of one of the latter seances, he tells us, the furniture vibrated so strongly that his little daughter once jumped up, squealing: "Oh, papa, there is a heart in my chair!" Wilkinson also claims to have seen a table roll over like a hoop, "rise waving in the atmosphere, and finally ascend to the ceiling before settling back as if it was a feather...." While this went on Home "reclined in a state of the completest muscular repose."

Another earthquake effect was observed during some test seances conducted at Amsterdam in February 1858, the witnesses being a number of very skeptical investigators. Their report remarks:

> Hardly had we seated ourselves, or within ten seconds we heard soft rappings that soon changed into loud knocks. These raps were heard to come from all sides of the large room. This was accompanied by a complete rocking movement of the room's ceiling that became so violent that together with the chairs on which we were seated we felt ourselves going up and down, as if on a rocking horse. We experienced the same movement and sensation as when seated in a carriage on springs and driving on the highroad.

The journalist Robert Bell described his experience during an 1860 sitting with Home in London as follows:

> We were sitting around a table when raps and table movements took place. Suddenly we were surprised by a strange vibration of the room. We listened and observed attentively what was happening. The vibrations became stronger and stronger. We felt it under our feet, our chairs were trembling, the floor oscillated violently. The effect was exactly the same as that happening in tropical regions during the first minutes preceding an earthquake. This violent movement of the room lasted about two or three minutes, then

it subsided by degrees. Everyone present experienced the phenomenon in exactly the same manner.

The purpose of my drawing your attention to this special PK phenomenon of Home's is that, however much we might want to stretch one or another of the several "normal" explanatory hypotheses, all such hypotheses must invariably be ruled out of court, owing to the exceptional nature of the earthquake phenomenon. The diffusion of the so-powerful manifestation throughout the seance room makes it impossible for one man (the medium) to produce it by means of bodily exertions; moreover, the size of, say, a steam-driven apparatus necessary to bring about the combined oscillations and vibrations of the ceiling and the floor of a seance room, together with all the furniture therein, would be so enormous that such a machinery could never have been set up at a moment's notice and remained undetected in the houses, hotels, and so on in which the seances took place. These houses belonged to men other than Home, since Home possessed no house of his own.

A SOUTH LONDON CASE OF RSPK

Manfred Cassirer (Physical Phenomena Committee; Society for Psychical Research)

The presently-reported case of RSPK (Recurrent Spontaneous Psychokinesis) was studied by P. M. Runnalls (P.M.R.) and me on behalf of the Society for Psychical Research during two periods of activity: the mornings of October 11, 1973, and of June 27, 1974, after which the investigation had to be abandoned. In addition, we had access to notes made by other observers. Photography was allowed, and interviews and observations could be taped. The phenomena actually observed included "simple" PK effects on stable objects as well as "complex" PK (teleportation; apportation; teleplasty or ideoplasty; direct writing). The locale was a gardening guild where horticultural produce, implements, and fertilizers were sold to members only. The premises consisted of two sheds with corrugated iron roofs having direct access to each other. The main one measured 10 by 20 feet. It had two windows and a side door (main entrance) and was well lit. Most of the business was transacted here, and it was the favored location of the "polter-

geist." The other, slightly smaller shed had a separate entrance with a metal door; the light was adequate for precise observation.

The business was mainly run by two men. T. B., aged 50 and an invalid, was the apparent usual focus of psychic activity and was therefore presumed to be the Central Party. A. C., aged 78, was the other manager. He was hostile, feeling that we were not helping to get rid of the "ghost." We found no evidence pointing to trickery, and no motivation (profit or publicity) for exploiting the phenomena. A plethora of events occurred in quick succession, covering almost the entire duration (two hours) of each observational period, though the phenomena were intermittent. Their precise order and sequence are accordingly occasionally problematic. This applies particularly to the exact position of everyone at any given time. Radial and tangential forces could not be determined. Usually only the point of impact was ascertainable, while the initial kinetic stages were invariably out of sight prior to their audible impact.

A variety of phenomena occurred, including showers of fertilizer. These were rather frequent, coming from a point above the observer or victim, and apparently utilizing material actually on the premises. T. B., P. M. R., and I were repeatedly attacked. Even more impressive were such downpours in inaccessible corners. Movements of static objects: Movements of weights of one to 14 pounds were common; some "fell off" tables, another just "arrived" after an alleged absence of several weeks. P. M. R. and I witnessed this "reappearance" of a weight; the impact was inconsistent with its having been thrown from outside. Four plastic beakers (for tea) had been placed on a car outside. While everyone was grouped around us, one beaker neatly hit P. M. R. on the head. The exact aim was remarkable in view of the lightness of the missile; a thread attached to it would have had to be 10 feet long to cover the distance.

Apports: A metal badge of unknown origin stamped out with the legend "MN" may be considered an apport. Direct writing: This was indicated when a number of chalks left the counter and, immediately afterward, I discovered a finely drawn cross on the floor. This cross was photographed. Teleplasty: A three-dimensional skull-like face was found on the counter, apparently made of sulfate of ammonia with the features cleverly picked out in "Maxicrop" fertilizer. It disintegrated, but only very slowly, the material tending to run

to the right edge of the table, whence it dropped to the floor, although firm and unaffected by blowing. No dislodgement took place under direct surveillance. Auditory phenomena: A very loud crash seemed to originate from the roof; immediate inspection revealed no normal cause.

This is one of the most sensational, in many respects unusual, and best-attested cases of poltergeist infestation on record. An unusual aspect is the age of the presumed Central Party. But were we tricked? We believe that this was not the case; in fact, there was no well-founded suspicion of foul play beyond the inherent unlikeliness of the phenomena. Such initial mental reservations evaporated into thin air as the facts became more and more inexplicable except in terms of paranormality. Also, there was an almost disarming lack of obvious motivation and opportunity for trickery which, if not totally conclusive, is at least unexpected by experienced investigators.

A POLTERGEIST CASE FROM GLASGOW

James F. McHarg (University of Dundee, Scotland)

This report concerns a poltergeist case from Glasgow about which I was asked to advise as a consultant psychiatrist. I was able to visit the family only three times. Fortunately, very effective local support and careful investigation of the phenomena were provided by the Rev. Max Magee of Strathclyde University, and Dr. Archie Roy, reader in astronomy at Glasgow University. Their recordings and other findings are to be reported in due course, and I am grateful to them for freely agreeing to have me present my preliminary, clinical impressions in advance.

The affected family consisted of the father and mother, two boys aged 15 and 11, and the maternal grandmother. The neighbor involved was an elderly gentleman living immediately below them with his wife and adult son. Apparently there had been an antagonism between the two families from the time that the upstairs family moved in, 12 years previously.

The onset of disturbing phenomena had been quite sudden at about 11 p. m. on a night in August or September 1974. The first phenomenon had been a loud bump, which had roused

the whole family, followed, for about an hour, by other inexplicable noises. Similar noises recurred intermittently over the next two or three months, and were at first blamed on the family downstairs. From November 3, 1974, except for an unexplained break for a week before Christmas 1974, knockings, scratchings, vibrations, or loud bangs had occurred nightly, usually starting while the boys were preparing for sleep. It was eventually realized that some of the tappings followed the rhythm of the Dead March; the mother, having improvised a code, elicited complex information from the tappings allegedly relating to the younger days of the elderly gentleman downstairs, as well as ridiculous messages of incitement to strangle him.

Meanwhile, the family and others had started to observe dramatic movements of furniture and other objects of all sizes. Other kinetic effects of a different kind had been observed in the boys themselves, producing twisted postures of the trunk and limbs. In the case of the older boy, these crude motility effects were reported to have evolved, for short periods, into complex motor skills he did not normally possess--e.g., conjuring with playing cards. These motility effects, experienced by the boys as imposed upon them, raised the question of exorcism and led to the medical referral.

The cessation of the really disturbing phenomena, as sudden as their onset, was alleged to have occurred at midnight one night towards the end of May 1975. The family said that they had learned on the very next day that the elderly gentleman downstairs had died and they had assumed, in fact inaccurately, that he had died that same night.

I did not find anything suggestive of fraud, neurosis, or psychosis in the affected family. In particular, the descriptions of the twisted postures of the boys did not sound like the carpo-pedal spasms of hysterical over-breathing. They were more reminiscent of torsion dystonia, which is usually attributed to basal ganglia disturbance, but the exhibition of unlearned skills, if authentic, would imply brain involvement at a higher, cortical level.

The mother indicated to me that she had felt that the incitements to kill the elderly gentleman by strangling him were not only ridiculous but specially strange, because she herself had already begun to suspect that he was hospitalized with something gravely the matter with his throat. Subse-

quently I discovered that his death, which had occurred on May 26, 1975, had been due to bronchial carcinoma. He had become seriously ill three months earlier, in February, 1975, and I presumed he had had symptoms even before that.

The ultimate origin and significance of the frank death wishes which were ostensibly communicated remain, so far, conjectural. It is clear, however, that not only had the disturbing phenomena ceased abruptly in rather close association with (but not on) the actual day of the elderly gentleman's death, but that they had also run concurrently with the entire clinical phase of his illness as well as with an unascertainable stretch (perhaps the entire stretch) of its preclinical phase. Furthermore, the overall clinical pattern of his illness, so far obtained, was consistent with the original moment of malignant transformation's having been in August or September 1974, the period when the poltergeist phenomena began.

The findings are suggestive, therefore, that the strikingly sudden onset of the poltergeist phenomena, upstairs, was in synchronistic relation to the inherently sudden moment of malignant transformation in the bronchial tissues of the elderly gentleman downstairs.

A HIGH-VOLTAGE PHOTOGRAPHY EXPERIMENT WITH URI GELLER

J. L. Hickman (Washington Street Research Center, San Francisco)*

The Kirlians and others have reported evidence of anomalous high-voltage photographic images which appear to accompany the subjective feeling of "mental energy" entering or leaving a human subject's fingertips. Formation of such luminous images by conscious mental effort occurs rarely and, in general, only with unusual individuals. A possible example of one of these rare instances is described here in an experiment I conducted with Uri Geller. Although he had done a previous pilot study on high-voltage photography, Geller had had no previous contact with the particular apparatus

*Presented by S. Krippner.

used in this experiment, and appeared to be unfamiliar with its operation or use. The experimenter and the observers could find no evidence of trickery or deception.

Geller and I met in Geller's hotel room in Palo Alto, Calif., at 9:00 p. m. on December 21, 1973. Also present were observers H. S. Dakin, J. Mayo, W. Westerbeke, and S. Kenny. During the period from 9:00 to 11:30 p. m., nine photographic exposures were made using a timer-controlled, high-voltage supply with an 8 by 10 inch aluminum plate electrode separated from the object being photographed by an 11 by 14 by 1/8 inch glass insulator plate. Only seven of the test photographs are included in this report. The other two were attempted with too short an exposure and no image appeared. For photographs 1, 2, 5, and 6, the observers and I chose an arbitrary geometric figure and one observer drew a picture of it while I requested Geller to try to make that image appear in the high-voltage photograph of his fingertip; for photographs 3, 4, and 7 I requested Geller to attempt a bioenergetic interaction with an external object.

During each trial the lights were turned off and I placed a sheet of film on the glass plate insulator, emulsion side up. I guided Geller's right index fingertip to the film surface, feeling carefully for any foreign material which might influence the photographic image; for photographs 3, 4, and 7 I also placed an object on the film a few centimeters away from Geller's finger. One or two control exposures were then taken, depending on the size of the film used. I then moved Geller's fingertip to another portion of the film, again feeling for foreign material, and the observers, Geller and I concentrated on the target image. On Geller's command, one of the observers activated the high-voltage supply. The exposed film was then placed in a light-secure box and the lights turned on.

For photographs 1 and 2, two control exposures were made at the top of the film. Geller was then requested to produce a circle and triangle respectively. Photograph 3 was taken after a watch (supplied by one observer) and Geller's fingertip were placed a few inches apart on the film. This was the control photograph. In photograph 4 the watch and fingertip were placed as before but Geller was concentrating, in his own words, on "shooting energy toward the watch." The target figures were easily identifiable from the produced images, and there was an obvious difference between the control photographs and the experimental ones.

For photograph 5, after a control exposure was recorded in the lower center of the film, Geller was requested to produce an image of a heart. Although the resultant image was not recognizable as a heart, it was sufficiently different from the control photo to encourage further attempts. For photograph 6, after a control exposure, Geller was requested to make a line appear through the middle of the high-voltage photographic image of his fingertip. Again there seemed to be a significant correspondence between the requested image and the resultant photograph.

For photograph 7, one of the observers suggested that Geller attempt to bend an object without touching it while a high-voltage photograph was being taken. I produced a metal ring which I had brought for this purpose. The ring was placed on a piece of film with Geller's fingertip one-half inch away, and a control exposure was taken. The ring was then moved to the center of the film and Geller's fingertip placed below it, an inch away. Geller concentrated for a few minutes and then signaled for the exposure to be made. When the lights were turned on, the ring was found to have an obvious bend not present earlier. As far as I could determine in the dark, Geller had not touched the ring during the entire procedure. However, the photograph is difficult to interpret since there is an obvious overlap of control and experimental images, and the possibility of incomplete surface contact between the ring and the film could account for the loss of corona discharge from the ring as seen in the experimental image.

These results appear to show high-voltage photographic effects which cannot be easily explained solely in terms of physical conditions at the fingertip-film interface. Four possible explanations should be taken into consideration during further experimentation: (1) Geller may have used sleight-of-hand techniques to introduce material underneath his fingertip which would influence the photograph. I took precautions to control for this possibility, and feel that under the circumstances no fraud occurred. (2) It has been suggested that the images could have been produced through conscious manipulation of subcutaneous musculature resulting in arrangement of fingertip surface skin configuration in the form of the requested geometric shape. Although this possibility would not explain the effects which extend beyond the skin surface area, nevertheless if Geller were capable of such extraordinary functioning, that talent alone would be worthy of extensive study.

(3) It is possible that the presence of the high electrical potential difference between finger and film was incidental and these images could have been produced by direct mental-physical energy interaction between the subject and the film. This is unlikely, since several attempts by Geller to produce images paranormally on conventional film were unsuccessful. (4) These images may be the result of a little-understood energetic mechanism which influences physical events in ways that do not conform to the known laws of science. In this case, high-voltage photography may act as a transducer for the focusing of these processes into the reference frame within which we normally operate. Or there may be certain physical energy components (e. g., electrostatic or magnetic fields) which accompany and reflect this biological functioning. High-voltage photography would then act as a method of making these field components visible. In either case, this experiment would provide preliminary evidence of both the paranormal functioning of Geller and the possible application of high-voltage photography for the study of that functioning.

THE INVESTIGATION OF "MINI-GELLERS" IN SOUTH AFRICA 18 MONTHS AFTER THEIR MANIFESTATION

E. Alan Price (South African Institute of Parapsychology)

In July 1974 Uri Geller visited South Africa and gave numerous demonstrations on the stage and over the radio. During his visit a great number of people reported that they experienced, witnessed, or were able themselves to perform psychokinetic effects similar to those performed by Geller. I conducted a survey of these claims. Among the 137 cases reported there were six children, three adolescents, and one adult who claimed that they could, and did, continue to bend metal objects independently of Geller. Of these 10 "mini-Gellers," seven were males and three females. For various reasons, it was only practical to investigate five "mini-Gellers."

The "Geller effect" manifested itself in the following manner: household items of cutlery, keys, nails, door handles, brass pestles, car bonnet mascots, etc., were bent, twisted or broken. One subject reported dematerialization

and spontaneous movement of objects. In some cases bending was observed to continue when the object was not touched by the subject. Two of the five "mini-Gellers" bent dessert spoons and teaspoons while being observed by me.

Eighteen months later I investigated these five subjects again. Copper and stainless-steel strips of 10 by 1 by 0.2 cm in size were specially prepared by a leading metallurgical firm. Some of the metal strips were fully annealed, others were half hard, and a third lot were hard. The metal strips were factory marked to indicate hardness and were then sealed in glass tubes in a glass-blowing factory. Three groups of objects were prepared. One group, containing a set of sealed tubes plus metal and plastic objects, was locked in a metal box. Another similar group was locked in a wooden box and a third similar group was left unlocked to be handled freely by the subject. The experiments took place at the subjects' homes and were unwitnessed. This was done, first, to avoid negative and skeptical observer and experimenter effects and, second, to allow the subject to work undisturbed in familiar surroundings and for as long as he wished.

The results were as follows: four of the five subjects ostensibly could still bend metal objects, but only if they could stroke and touch them. Two subjects could break metal and bend and break plastic objects. All the above, however, was performed under unwitnessed conditions. No subject could bend the copper or steel metal strips which were sealed in glass tubes. None of the objects which were lying freely in the locked boxes was deformed, broken, or altered in shape.

The following may be some reasons for the failure of metal and plastic objects to undergo deformation in the sealed and locked containers. Glass, metal, and even wood may shield the objects. Perhaps plastic containers would be better. Furthermore, direct contact may be essential for certain people who display PK ability. Additionally, complete sealing without any opening no matter how small may be psychologically inhibitory. In general, certain individuals work best in public, others only when unwitnessed. Finally, lack of motivation may be another factor. It was 18 months since Uri Geller was in South Africa. The challenge and excitement may have passed.

On the basis of interviews and visits, I do not think that the subjects perpetrated conscious fraud. If the deformation was not due to psychokinesis but rather to physical force, then I feel that this would have occurred probably while

the subject was in a dissociated state, either hypnotic or epileptic. No history of epilepsy was obtained.

PK EFFECT ON A CLINICAL THERMOMETER WITH A DANISH "MINI-GELLER" GIRL

R. D. Mattuck (University of Copenhagen)

Lena Ilsted D. is an 18-year-old Danish girl who discovered her PK abilities after Uri Geller's visit to Copenhagen in January 1974. At the time of this investigation, she was studying languages at a gymnasium (junior college) in Denmark. Lena and I conducted 11 experimental sessions together, during the period April 1974 to March 1975, in the presence of her parents. In the first seven sessions, she was able to achieve striking effects, but not when adequate controls were exercised. Some of her insufficiently controlled phenomena were: bending 3.2 mm iron nails through 45°, changing the position of the hands on a watch by several hours, and causing 15° deflections of a compass needle.

In October 1974, Lena discovered that she was able to raise the mercury in an ordinary clinical (fever) thermometer from 35° C to 40° C, while she held the end of the thermometer opposite to the mercury bulb. She demonstrated this effect in three sessions, the last of which I feel was under adequate control. It took place on March 25, 1975. The thermometer used belonged to me. It required about two calories to raise the mercury to 40° C by heating, and measurement in a centrifuge showed that a minimum acceleration of 53.1 ± 6.3 gravities, corresponding to a pressure of .79 ± .09 atmospheres, was required to increase the position of the mercury mechanically. The thermometer was heated to 36.02 ± .01° C and allowed to cool to room temperature, well before the experiment started. (Note: since this was a maximum-reading thermometer, the position of the mercury remained substantially unchanged after the cooling.) Lena sat on a well-lighted sofa in her home, one meter from me, and I handed her the thermometer at 8:06 p. m. She held it motionless between her thumb and forefinger, at the end opposite to the mercury bulb, in clear view of me, only lifting it occasionally to read it. At 8:28 p. m., she handed the thermometer back to me and it read 40.20 ± .01°C; she did not handle it further. Subsequent readings gave:

8:37 p. m., 40.25 ± .01° C; 8:41 p. m., 40.24° C; 9:10 p. m., 40.24° C; i. e., an "after-effect" increase of .05° C. This is similar to the way in which keys are reported to continue bending after being handled by Uri Geller.

I believe that the excellent observational conditions are sufficient to rule out the use of all normal physical agencies which could have caused the increase in mercury position--thermal (e. g., cigarette lighter, electric light, radiator, hot water, infrared heater, electric pad, pre-heated hair curler), frictional, or mechanical (e. g., shaking or dropping the thermometer)--which we can reasonably assume were available to Lena. The temperature of the skin of the body (34°), or the breath (32°), is too low to have produced the effect. More refined methods, such as a concealed laser beam, we feel are highly unlikely because of Lena's non-technical background.

Perhaps the most convincing indication that the increase was not caused by ordinary physical agencies is the striking after-effect. This effect does not occur when the mercury position is increased from 36° to 40° C either thermally or mechanically; such normal treatment is always followed by a decrease of around .03° C or more.

It should be noted that we cannot conclude from the above results that the rise of the mercury was caused by PK heating of the bulb, since this rise could equally well have been caused by a PK-induced pressure on the mercury. An independent measurement of the temperature--for example with a thermocouple--would be required to answer this question.

RESEARCH BRIEFS II*

BLIND PK WITH DICE

W. E. Cox (Institute for Parapsychology, FRNM, Durham, N. C.)

Early in 1976 I had occasion to reexamine the question of "blind PK" with dice, in which subjects do not know which faces are targets until after a session is completed. Considering the fact that PK cannot hypothetically be expected to affect dice without clairvoyant knowledge of how they should be revolved and tumbled, the question of how subjects might react to blind targets was one I considered important to answer. Very little work had been reported on this in early years. Dr. J. Blundun, working under Fisk in England, achieved success in totally blind PK with dice, averaging 4. 25 hits per run of 24 trials (where four hits was mean chance expectation). There was also an important element of blindness in a comparison of celluloid and lead dice which I reported in 1971. In that experiment, no subject knew that half the dice weighed seven times as much as the other half. Nevertheless, the subjects averaged 4. 17 hits per run with celluloid, and 3. 84 hits with lead dice. Dice of both weights were thrown simultaneously (and quite indistinguishably) in separate chutes.

The present question I wanted to answer was how great a difference, if any, would result when the subjects did know the targets half the time, and not the other half. I had no hypothetical reason to expect that there would be an appreciable difference between the blind series hits and the straight or "open" series hits. A single preliminary series using blind targets only, with me as experimenter, produced no deviation from chance--but I did observe an increasing decline in scoring in the fourth chronological quarter.

*Chairperson: Rex G. Stanford, St. John's University.

As a result, I employed in January 1976 a laboratory associate, B. B., to test the ability of about 20 subjects to influence dice blindly, in comparison with straight or "open" dice PK. In the blind condition, 144 runs of 24 trials each were done, in 24 sessions of six runs each. An electric tumbler containing six dice was used. Targets were taken from a random number table, but not revealed until check-up. The subject simply willed for "hits" as such, while he watched the dice being tumbled and all six faces recorded. Each "blind" session was followed by a repeat session, using the same subjects and a similar procedure, but with known target faces, for a total 144 "open" runs. Target faces ran consecutively from one to six within each "open" session.

There was an average of 3.79 hits per run for the blind targets, and 4.02 hits per run for the known targets. This difference was not significant (CRd = 1.1). But of more interest than this was the post hoc finding of an incline in the chronological quarter distribution of each session-page. For the blind series, the CRd between the first and fourth quarters was 2.20 (P = .03, two-tailed). For the known-target series, this was not significant.

One additional series of 54 runs each was done in June 1976 by a student associate volunteer, B. D. C. The blind runs exceeded the open runs in scoring by an average of .33 hits per run, which was not significant. These findings lend credence to the design and purpose of the major blind PK experiment which I am reporting at this convention, using air bubbles in water and a completely automated procedure [see pp. 129-31].

AN INVESTIGATION OF THE PSI ENHANCEMENT PARADIGM OF SCHMIDT

Brian Millar† and Richard S. Broughton (University of Edinburgh)

The theories of both E. H. Walker and H. Schmidt suggest that psi is triggered at the instant of feedback. Loosely, awareness of success at any later time allows the

†Presented by Millar; dagger will indicate speaker.

subject to cause the earlier event. Thus, according to these theorists, feedback is the pivotal point in a psi trial. It follows that if feedback from the same event is presented several times, with the subject unaware of the repetition, enhanced psi scoring might be obtained. At the 1975 Parapsychological Association convention Schmidt described a PK experiment which tested this prediction [RIP 1975, pp. 107-9]. Targets presented four times gave a much higher scoring rate (52. 95 percent) than that associated with single-feedback data (50. 82 percent). Quite apart from the theoretical importance of this finding, it suggested that a multiple feedback technique could be used as a kind of "psi amplifier." Thus for the experimental parapsychologist this procedure, if it is further confirmed, may provide a method for obtaining measurable scoring using only weak, though constant, psi sources.

Because of the promise of this psi enhancement paradigm a replication was set up in Edinburgh using a computer/random number generator (RNG) system to control the experiment from a remote location. From the start we were conscious of the possible role of the experimenter in eliciting results. Therefore two independent experiments were performed and analyzed, one by each experimenter, each using 20 subjects. Furthermore, the feedback available to the experimenter was restricted as far as possible, thus minimizing, according to the Schmidt model, the influence of experimenter psi. This was achieved by the experimenter's being in another room while the trials were being conducted and by giving the experimenter access only to summary statistics (half-session totals), not the trial-by-trial data.

As in Schmidt's experiment, each subject performed two half-sessions of four runs each, separated by a 15-minute break. Each 30-second run comprised 256 binary trials at the rate of about 10 per second. A hit was followed by a brief audible reward tone, while a miss was followed by silence. The task of the subject was to produce more tones. Unknown to him, although the even-numbered trials were generated contemporaneously, the odd-numbered trials had been produced at high speed prior to the test runs and were the same in all four runs. The totals for each half-session for single and four-times-presented trials were automatically recorded on computer magnetic tape for later analysis.

Neither experimenter's data demonstrated psi enhancement, nor was there evidence of PK in either condition. The most deviant scoring rate was 49. 56 percent (where 50 was

chance expectation). In short, despite the use of twice as many subjects overall as employed by Schmidt, and testing under what we considered to be psychologically good conditions, absolutely no psi was obtained, in marked contrast to the earlier work.

It is clearly of considerable importance to consider why this might be. Do impecunious Scottish students have systematically less PK ability than American respondents to newspaper advertisements? While the possibility cannot be entirely ignored, on the basis of both historical and contemporaneous evidence we do not believe this to be probable. It seems to us that by far the most likely explanation of the disparity between Schmidt's results and ours lies with the experimenters. Schmidt has regularly found psi in his work, while we at Edinburgh have been rather less fortunate. The disproportionate success of some experimenters has long been noted in parapsychology and this has generally been attributed to interpersonal factors. Recent evidence, however, suggests that experimenter psi may be more important. Certainly a psi-based experimenter effect seems to us highly plausible in the present case. Schmidt can himself influence an RNG. Furthermore, internal evidence suggests that he is more than a passive bystander. In some PK work conducted by him, cockroaches wired to an RNG received more shocks than chance would predict. Schmidt is said to dislike cockroaches.

It is not clear how much feedback Schmidt himself received in his psi enhancement experiment, though he has commented briefly on experimenter psi as a possible explanation of his results. It would be extremely interesting to know whether he could still elicit psi from volunteers if his own feedback were severely curtailed. Could it be that in the effort to minimize experimenter psi in our own work we have also eliminated any psi?

In summary, the strikingly nonsignificant results found in Edinburgh in two experiments by different experimenters, using a close repetition of a previously successful technique, lead us to believe that a major determinant of success may be the psi ability of the experimenter. It remains for further work to determine whether PK effects can be elicited from volunteer subjects in the absence of such a psi source.

PK EFFECTS BY A SINGLE SUBJECT ON A BINARY RANDOM NUMBER GENERATOR BASED ON ELECTRONIC NOISE

Scott Hill (University of Copenhagen)

Several years ago Schmidt invented a binary random number generator (RNG) which drove a ring of lights either one jump clockwise or one jump counterclockwise with each trial period. As the trials were automatically generated, the subject had no calls to make, and any positive results could not be due to precognition, but must be due to PK. The target direction was specified in advance for each PK trial. The device considered in the present study ("PK meter") can be considered to be a second-generation device of the type developed by Schmidt. It is, however, much lighter, portable, runs on batteries, and is based on modern integrated circuits. The major features, however, are common to Schmidt's early device: adjustable trial rate, visual feedback of each hit, optional audio feedback of each hit, and features for automatic hit and trial total recordings, to exclude experimenter error. Like Schmidt's RNG, the PK meter is based on a physical process in nature which is random and well characterized.

The PK meter is based on semiconductor noise, an effect due to varying conduction properties in solid material, consisting of components independent of and dependent on frequency. Like radioactive decay, this is a physical process which is one of nature's few natural random sources. Energetically, a few electron volts would suffice to bring a particle from the "trapped" band to the conduction band, so that we are definitely dealing with a microscopic PK effect. Although the energy is small, no conceivable application of an outside field could duplicate the random time behavior of the noise spectrum, which contains components at all frequencies. Except for the meter movement, which is an output indicator only, the PK meter contains no moving parts.

Each hit and each trial actuate (1) a cumulative meter reading, (2) a light-emitting diode light display, and (3) an audio tone. During the trials reported in this paper, all these feedback modalities were in use simultaneously. Charles Tart has noted that the amount and type of feedback, and the delay time before feedback is given to a subject, may be important parameters in ESP and PK scoring. Here the

subject received immediate feedback of both total (cumulative) and individual scores during the experiment.

The experiments were conducted in a private location provided by the subject, Mrs. XYZ, on two consecutive days in September 1975. The subject had no apparent history of ESP experiences, and indeed this study was the first extensive testing she had had. She made no claims of being able to influence the device, and was surprised when positive results were recorded. A preliminary set of 1000 trials was done on the first evening, yielding a modest success. This series was then continued for another 3000 trials, which again gave deviations in a positive direction. At this point, it was decided to commence "formal" experiments. Since the construction of the apparatus, under the control of the experimenters, allows no chance for fraud, the only change made in the protocol was moving to an adjacent and more comfortable room and providing a tape recorder for automatic registration of each hit and miss. The atmosphere throughout was relaxed and informal.

Six experimental sessions were completed. Although 15,400 trials were done, only 13,600 (including the preliminary trials) were considered for the statistical analysis, since we excluded one session where scores were self-reported by the subject. For each individual session, the number of trials was specified in advance. The number of hits exceeded mean chance expectation by 172, giving CR = 2.949. Assuming the mean of the chance distribution to be exactly 50 percent, this gave an exact binomial probability of .0016 (one-tailed). The average score was 52.5 hits per 100 trials.

To insure the RNG was functioning properly, a series of randomness trials was done by one of the experimenters and a few other persons. Several thousand randomness trials were done in Copenhagen prior to the experiment, and these agreed with factory checks performed in the plant in California. After the experiment, over 31,000 trials were done as a double-check in Copenhagen. When lumped together, this gave a total of 38,700 trials and 19,531 hits, a deviation of 181 hits above chance, corresponding to a CR of 1.84 (P = .066, two-tailed).

To check that the meter was calibrated correctly at the beginning of each run and to obtain detailed PK profiles of each run, the 9600 trials on magnetic tape were subjected

to a detailed analysis. Hits per 100 trials were graphed, corresponding to an elapsed time per block of about 40 seconds. The PK profiles reveal a steady upward trend; i. e., the overall high score was not due to a sudden large fluctuation from chance level. According to Tart's criteria, this may indicate inter-session and inter-run learning. This is presently being tested with other subjects in Copenhagen. From the feedback criteria discussed earlier, it may be expected that immediate feedback would enhance the learning effect.

Miss XYZ felt that she might score better if she were alone with the apparatus, so accordingly, sessions 4 and 5 were done without the presence of the experimenters. Session 5 was taped, but session 4 was not. It is interesting to note that session 4 (which is the only session where the scores could not be checked directly) produced, together with 5, the least significant scores. This raises questions regarding the "experimenter effect." Since the subject scored better in the presence of the experimenters, who also received feedback from the PK meter via a loudspeaker, could a "group PK" effect be present? When the device is remote-triggered, not subject to observation by any experimenter or subject, the scores seem to lie closer to the 50 percent mean. This is now being investigated post hoc.

A PK EXPERIMENT WITH A COVERT RELEASE-OF-EFFORT TEST

Richard S. Broughton† and Brian Millar (University of Edinburgh)

Laboratory lore has often provided suggestions that a subject in a psi test may suddenly do well at the moment the test has ended. Stanford has observed that such occurrences seem to follow situations in which the subject's conscious efforts proved ineffectual, and he has codified these observations into Assumption 13 of his Psi Mediated Instrumental Response model of psi: "PK as PMIR is particularly facilitated when the goal event has very recently been in conscious focus but has just left the focus of consciousness without having been realized." An experiment reported at the 1974 Parapsychological Association convention tested this assumption and

found significantly greater PK activity during a release-of-effort period than during the immediately preceding overt PK test [RIP 1974, pp. 61-3]. The experiment failed to support the idea that such an effect should be greater after unsuccessful efforts.

Such evidence is of interest not only because it may indicate where to look for psi, but also because of the light it sheds on the psychological processes which may govern the use of psi in the individual. A conceptual replication of this test of release of effort was undertaken using a high-speed, computer-linked random number generator (RNG). Subjects performed a PK task with visual feedback for a period, then received the results of their efforts. Unknown to the subject, the computer immediately began a second PK test, similar to the first but without the visual feedback or the end results. The hypothesis was that a strong PK effect would occur immediately after cessation of effort to produce the effect. Several other suggestions were to be examined informally in the first instance, among them the hypothesis that such an effect would be greater following unsuccessful efforts.

Forty paid volunteer subjects were used in the experiment. Since both experimenters were thoroughly blind to the relevant results during the experiment it was decided to examine the data from half the subjects (odd or even numbered) first; thus effects noted in the first group of 20 could be subjected to confirmatory analysis when the second group's data were examined. In this way both pilot and confirmation studies were combined in a single experiment.

The task for the subject was to influence mentally the random vertical motion of a dot on the computer's oscilloscope screen, the position of the dot being governed by a noise-driven RNG. The dot represented the momentary deviation from mean chance expectation, and at the end of a run a deviation of two centimeters represented one standard deviation. Ordinarily the movement of the dot seems much like Brownian Motion in one dimension; the subject was asked to push the dot up or to pull it down during the 100-second run. A run consisted of 10,000 trials (at 100 per second) and at the end the results were printed on the teletype beside the subject. Immediately after, the computer began an identical run to catch any release-of-effort effect, but this time there was no visual display or end results to let the subject know what was happening. To occupy this time the experimenter chatted with the subject about the scores and asked him to complete a

humorously-worded questionnaire. Afterwards, the subject was invited to do a second run (to provide further data only, as there was no manipulation between runs), followed by a second release-of-effort run unknown to him.

Data were recorded for all four tests (two overt, two release-of-effort) in five-second increments to permit examination for a possible time course in any PK effect, especially in the release-of-effort tests. At the conclusion of the experiment a program printed the reports for all the odd or even numbered subjects as directed. A binary random decision indicated the odd numbered subjects should be examined first. No evidence of any above-chance scoring was found in the data, either in overt or release-of-effort tests. Release-of-effort tests following the overt tests where a negative deviation was obtained did not provide any above-chance scoring. After exhausting all reasonable possibilities for effects, the second set of reports was printed and these replicated the findings of the first group.

The experiment failed to confirm the release-of-effort hypothesis, and in so doing it joined several other experiments conducted by the present experimenters which have failed to confirm previously confirmed hypotheses, or even give any evidence of psi whatsoever. One cannot help but observe that the present experimenters are an exact complement to the few parapsychologists who almost never fail to get results, and the suggestion that this may be due not to experimental technique, but to the varying psi abilities of the experimenters themselves, can no longer be avoided.

SEARCH FOR A RELATIONSHIP BETWEEN BRAINWAVES AND PK PERFORMANCE

Helmut Schmidt† and James C. Terry (Mind Science Foundation, San Antonio, Texas)

Let us operationally define the different brain states A, B, etc., by the occurrence of different brainwave patterns as measured under some specified conditions. In the following A and B will be, in particular, states where the frequency range and amplitude satisfy certain requirements so that, qualitatively, A is a state of high alpha activity and B, a state of high beta activity.

Consider next a binary PK trial with immediate feedback; the interesting question arises whether one particular brain state is more favorable for PK success than others. We might expect, however, any possible correlation between brain state and PK performance to interact with other parameters of the experiment so that any observed correlation might be restricted to the particular test situation. Two test situations might be most natural to explore: (1) tests where PK trials are made independently of the momentary brain state and where later analysis checks the scoring rates in the trials during which the brain state happened to be A or B; (2) tests where during one session a trial is initiated only when the brain is in a specified state, such as A.

We have chosen the second approach. In this situation, because trials occur only in the presence of a certain brain state, the subject receives through the PK feedback some information about his brain state as well. We speculated that this situation might be most interesting for the subjects and thus be particularly favorable for observing PK effects.

Sixty unselected volunteer subjects, males and females between the ages of 17 and 62, participated. Subjects came from the San Antonio area in answer to newspaper ads. Some subjects came back repeatedly; the total experiment consisted of 80 sessions. Two experimental series (I and II) comprising 40 sessions each were conducted by J. C. T. Half the sessions studied the brain state A, and the other half, brain state B. A and B corresponded to states where the EEG amplitude in, respectively, the alpha or beta frequency range surpassed a fixed threshold. The EEG was recorded differentially from the occipital lobe (with the right ear as ground), and an Alphascan 600 instrument signaled when the criterion for state A or B was met.

Each session comprised six test runs of approximately four minutes each. Test runs of two different types were alternated: these were enhancement and suppression runs. In enhancement runs, the Alphascan was connected to a binary random number generator (RNG) so that whenever the particular brain state was present the RNG produced (at a maximal rate of four per second) a random sequence of high- and low-pitched sounds, which were presented to the subject through headphones. The subject was instructed to pursue two tasks: the task of reaching the specified brain state indicated by the occurrence of high or low sounds and the

PK task of having low rather than high sounds appear. The results indicated that subjects were successful in producing more PK hits (low-pitched sounds) than misses (high-pitched sounds); overall scoring rate was 50. 7 percent; CR = 3. 12. This effect was present in both alpha and beta runs.

In suppression runs, the subjects received only one type of sound feedback and their task was to suppress the occurrence of a tone. As in the previous arrangement the binary RNG was activated whenever the specified state was reached. But now only a PK miss was signaled to the subject as a tone, whereas a hit produced no tone. The subject's goal, the suppression of the tone, could now be reached in two ways: either by avoiding the specified brain state which led to the triggering of the RNG or, if this state did occur nevertheless, by the enforcement of a hit on the RNG. The subject's emotional attitude towards the test was possibly quite different: in the previous case the occurrence of a trial was a desired event, whereas now the subject tried to avoid trials as far as possible. Overall PK results were nonsignificant (50. 2 percent), nor did alpha and beta runs produce differential scoring.

These results suggest that the enhancement condition is most promising for further work. It may require many more detailed studies to determine whether PK scoring can be correlated with brainwave activity or whether subtle psychological factors play the dominating role in determining the scores. Indeed, the one block of trials during the enhancement runs which yielded nonsignificant negative scoring might illustrate such psychological factors. This section was done last; and, by this time, the experimenter had come under time pressure so that he tested up to six subjects on a single day, whereas the previous sections were done at a more leisurely pace.

A TEST OF INTENTIONAL VERSUS UNINTENTIONAL PK

Brian Millar† and Pauline Mackenzie (University of Edinburgh)

The evocation of psi in the laboratory has long been surrounded by its own particular mystique. The importance of good social interaction between subject and experimenter and a warm protective setting in which the occurrence of psi

is taken for granted have been among conditions repeatedly stressed. It certainly seems plausible that taking someone from the outside world and in effect telling him to produce a miracle may produce psychological difficulties.

Another approach to testing is the unintentional psi task popularized by Stanford. In this kind of situation the subject is not aware that he is attempting psi. Instead he is expected to use psi unconsciously in service of his needs, for example by affecting a random number generator (RNG) of which he is unaware in such a way as to allow him to escape from a boring repetitive task. Thus far little lab lore has become attached to this kind of test. Successful results have been obtained using ordinary psychology laboratory techniques without special treatment of subjects.

If it is true that psychological factors are the major determinants of the occurrence of psi, as is generally assumed, the unintentional psi task might be expected to provide a method to lessen dependence on these delicate and chancy variables and thus provide a greater yield of psi in the laboratory setting. While a number of unintentional psi tests have been reported, we are aware of only three studies in which formal comparison between intentional and unintentional psi was made. In two of these, significant scoring was obtained in the unintentional but not in the intentional condition.

We therefore set up an experiment in Edinburgh to compare PK success under intentional and unintentional conditions. This was controlled by a remote computer-RNG system. The task employed was modeled after one reported by Schmidt at the 1974 Parapsychological Association convention, used in his case as an unintentional PK task [RIP 1974, pp. 116-21]. In our experiment PK was expected to affect the otherwise random interval between a signal (the subject's lamp's lighting) and the presentation of a barely audible "ping" embedded in white noise. The interval distribution, identical to that used by Schmidt, was such that in the absence of PK a ping was to be expected an average of 6.4 seconds (64 steps) after the lighting of the signal lamp. One second after the ping the lamp was extinguished. This indicated the end of the trial in case the subject had not noticed the feedback. After a 10-second delay the next trial began. In this way each subject contributed two runs of 10 trials each with a few minutes' break between.

Thirty unpaid volunteer subjects were allocated at random (15 each) to the two conditions. In the intentional condition the subject was told that he was to affect an RNG by PK and should press a button on hearing the ping. In the unintentional condition he was told to press the button as soon as possible after hearing the ping so that his reaction time could be recorded, and he was informed that the experiment was a test of mental speed and alertness. During the test trials the experimenter (P. M.) was in another room. The whole recruitment and running of subjects and the larger part of the analysis of results were done by her. The other experimenter, B. M., had no contact with the subjects and his only role was to design the experiment and set up the requisite hardware and software.

It was hypothesized that, in a way analogous to Schmidt's experiment, the subject's eager and expectant attention in waiting for the ping with which he would show himself to possess the desirable qualities of high mental speed and alertness might be expected to shorten the interval by PK. Chance scoring was expected in the intentional condition. In practice, neither condition differed significantly from the mean of 64 steps. The unintentional condition gave a mean of 63. 5 and the intentional 67. 3. Nor was there a significant difference between groups. Once again an experiment has completely failed to produce evidence of psi even in an unintentional condition very similar to that used by Schmidt, in whose hands the experiment succeeded admirably.

From our results we can conclude that, in this experiment at least, whether or not the subjects were aware that they were participating in a psi task was irrelevant to the elicitation of psi. Since this seems to us to be one of the more drastic psychological manipulations available we must question the widespread view that subjects' psychological factors are the crucial determinants of the appearance of psi in an experiment. Instead, our reading of the evidence suggests that subjects' psychological factors are relevant only when the preconditions for the occurrence of psi are fulfilled. One of the more important of these preconditions involves, we believe, experimenter psi. The senior author in this report took a minimal part in the conduct of the tests since he might be suspected of being "psicidal," whereas P. M. was fresh to parapsychological research and constantly optimistic about the outcome.

It is becoming increasingly clear that only a handful

of experimenters can elicit consistent evidence of psi, while others merely confirm the binomial theorem. It is, in our opinion, time to stop treating psi-based experimenter effects as a secondary problem of minor importance, which we hope will go away. On the contrary, it seems likely that the most important clues to the very elicitation of psi may well lie in this area.

A SIMPLE RANDOM NUMBER GENERATOR FOR USE WITH MINICOMPUTERS

Helmut Schmidt (Mind Science Foundation)

A reliable random number generator (RNG) can be built inexpensively from integrated circuits and a Geiger counter. Nevertheless, the construction and testing of such a device requires some experience in electronics. Most internal functions of an RNG are logic decision processes which can be performed by any of the minicomputers becoming more and more popular among hobbyists and researchers. If such a minicomputer is available, then a complete RNG can be built by the addition of a few external, noncritical components.

The conventional computer programs for generating random numbers can provide us only with quasi-random numbers in the sense that these numbers are obtained by some deterministic algorithm. Whereas these numbers may be good for determining a target sequence in a GESP test, they are unsuitable for PK tests and some other forms of psi tests. There we have to add to the deterministically operating computer some basically nondeterministic element. This can be provided easily by a Geiger counter which receives particles emitted from a radioactive source at random time intervals.

The basic structure of the RNG is as follows. The Geiger counter circuit: The Geiger tube receives a decay particle from the radioactive source; then a small current from the highly charged central wire flows to the metal case of the tube and is amplified in the transistor. The resulting signal passes a pulse shaper so that after arrival of a decay particle the output which is normally at ground level assumes for the short time of about 10^{-3} seconds a positive voltage of about 3 volts.

The computer input: A typical minicomputer can execute a sequence of logical steps which has been put into the computer memory. Furthermore, the minicomputer has electrical input and output lines to receive information from outside and to affect the course of an experiment by the closing of switches, etc. By the addition of a teletype console and further external memory the minicomputer can also be trained to do complex mathematical analyses, but for the present purpose we do not need these more expensive additions. The only requirement is that the minicomputer have an input line so that it can sense when the output is positive, i. e. when a radioactive decay particle has arrived.

The computer program: For generating a binary random number (0 or 1) we let the computer increment a binary counter at a high rate equal to $1/\tau_0$ steps per second, where τ_0 is the time required to alternate the digit. If this process is stopped by the arrival of the next radioactive decay particle then the least significant digit of the counter will, with nearly equal probabilities, be a 1 or a 0, provided that the average waiting time τ for the radioactive particle is long compared to τ_0. Thus a binary random decision has been made, based on the nondeterministic decay times of radioactive nuclei. In order to generate a random bit we first let the computer test the input line. Only if the line is at zero level (i. e., when the signal from the previous radioactive decay is past, and the Geiger tube is ready for the next count) will the computer proceed to increase the counter by one. Then the input line is sampled again and as long as the input is at zero voltage, the program loops back, incrementing the counter until the signal from the Geiger tube arrives. Then the least significant digit of the number is displayed or stored for later use. From a flow diagram and the instruction set of the particular computer the explicit program can easily be written. In the language of my IMSAI 8080 microcomputer (or the equivalent Altair 8080) this program requires only 14 words (of 8 bits each) of memory.

In order to obtain an upper limit for possible deviations from randomness, consider the case that the computer increments the binary counter, starting from zero, at the times $\tau_0, 2\tau_0, 3\tau_0, \ldots$ Assuming further that the Geiger counter is switched on at time $t = 0$, the probability for receiving a decay particle in the time interval $(t,\ t + \delta t)$ becomes:

$$P(t)\,\delta t = \frac{1}{\tau} e^{-t/\tau}\,\delta t$$

This gives, for the probability that a zero will occur in any of the time intervals during which the least significant bit of the number is 0 rather than 1, the following formula:

$$P(0) = \left\{ \int_{0}^{\tau_0} + \int_{2\tau_0}^{3\tau_0} + \int_{4\tau_0}^{5\tau_0} + \ldots \right\} P(t)\,\delta t \approx \frac{1}{2} + \frac{1}{4}\frac{\tau_0}{\tau} \quad \text{for } \frac{\tau_0}{\tau} \ll 1$$

This bias in favor of zeros was calculated under the most extreme assumption that the incrementing of the number starts always from the zero position. More generally, we can write:

$$\left| P(0) - \tfrac{1}{2} \right| \leq \frac{1}{4}\frac{\tau_0}{\tau} \quad \text{for } \frac{\tau_0}{\tau} \ll 1$$

In order to decide whether a certain constant bias

$$\delta P = \left| P(0) - \tfrac{1}{2} \right|$$

for one of the two possible outcomes is acceptable in a particular experiment let us see how many trials we would need to detect this bias (in the absence of any psi effects) with an average confidence level corresponding to a CR = 2. This number N_2 is easily calculated:

$$N_2 = (\delta P)^{-2}$$

If this number N_2 of trials at which the bias becomes visible is much larger than the number of trials made in our psi experiment, then the bias is negligible.

The numerical value of τ_0 is the time required for the computer to increment the binary number and to check whether a signal from the Geiger tube has arrived. The value of the waiting time τ can be easily controlled by the choice and the shielding of the radioactive source. For the sake of good randomness τ should be as large as possible. On the other hand τ is the average time it takes to generate one random number such that too large a τ value reduces the generation speed. It can be shown mathematically that the randomness is sufficient whenever the number of trials in the experiment is small compared to seven million.

The deviations from randomness can be drastically reduced by combining pairs of successively generated primary random numbers into one secondary random number, defined as 0 or 1, depending on whether the sum of the two primary random numbers is even or odd respectively. One can easily show that the probability for obtaining a zero using this procedure now satisfies:

$$\left| P'(0) - \tfrac{1}{2} \right| = \delta P' \leq \frac{1}{2}\left(\frac{\tau_0}{2\tau}\right)^2$$

so that the number of trials required to detect the non-randomness at the CR = 2 level becomes in our case:

$$N_2' = (\delta p')^{-2} = 1.45 \times 10^{13}$$

At the generation rate of 50 secondary random numbers per second it would take nearly 60 years of continuous operation to detect the deviation from randomness at the CR = 2 level of significance. Thus the degree of randomness is much higher than required by any realistic psi experiment. The contraction of two successively generated primary random numbers into one secondary random number can most easily be incorporated into the computer program.

MCTS: A MODULAR COMMUNICATIONS TESTING SYSTEM

John Placer and Robert L. Morris (University of California, Santa Barbara) and David T. Phillips (Science Spectrum, Inc.)*

The Modular Communications Testing System (MCTS) is a versatile electronic keyboard device designed to provide a convenient and reliable tool for the study of various psi phenomena and other forms of communication. A typical application of the device to a GESP experiment requires three people in three rooms. The experimenter operates the main control unit in the center room, the agent views a display in the first room, and the subject operates a keyboard in the third room. A random number, generated in the control unit by noise from a zener diode, is displayed to the agent. At the same time a "ready" light comes on at the subject's keyboard. The subject attempts to guess the number and enters his choice on the keyboard. The subject's choice is automatically compared with the target number and the number of trials and hits are recorded cumulatively and displayed on the main control unit.

To avoid the psychological limitations of number guessing, the random number generated by the MCTS may be used to select a particular item such as a numbered photograph from a target set. The target set may contain two, four, eight, or 16 elements as desired. The selected element then

*Presented by R. Neville.

serves as a target in GESP or clairvoyance studies. For example, the agent may concentrate on the selected photograph while the receiving subject records his visual imagery. The subject then selects the most appropriate response by examining a second copy of the target set, numbered in the same way, and enters his choice on his keyboard by pressing the corresponding button. If the subject wishes to skip the trial, he may push a "pass" button. If he has unusual certainty about his choice he may push a "confidence" button which scores the result separately. The MCTS can, if desired, display the correct choice to the subject as feedback four seconds after the choice is made. Four electronic counters record the total number of trials, hits, confidence calls, and confident hits, and display them via light-emitting diodes on the main control unit.

The MCTS has five modes of operation: GESP, clairvoyance, precognition, PK1, and PK2. In precognition studies the MCTS generates the random number after the subject enters his choice. PK studies are carried out with a preset choice designated as target. The PK1 mode selects new random numbers one at a time each time the subject presses a keyboard button. PK2 cycles automatically, generating a continuous stream of targets at rates ranging from one every two seconds to 50 trials per second. A preset-table trial counter provides automatic cutoff for fast PK studies after 16, 64, 256, 1024, or 4096 trials. The agent's display and subject's keyboard are modular units attached to the main control unit by 50-foot cables. A third keyboard, also on 50-foot cable, is used by the experimenter to set the PK target. All keyboards represent the possible targets by two rows of eight lights or buttons. The subject's keyboard includes individual lights associated with each button for feedback. The keyboard is arranged so that symmetry is maintained for target sets of two, four, eight, or 16.

The random number generator operates by amplifying zener diode noise with a two-transistor amplifier, and converting the amplified noise to logic levels with an LM339 comparator. This logic signal is divided by two to insure that equal time is spent in the high and low states. This random logic signal oscillates at frequencies up to about 200 kHz. It is sampled and clocked into a shift register at a rate of about 1 kHz whenever a new random number is required. Other types of random bit generators may easily be attached to the MCTS. Long unattended PK2 runs have shown quite satisfactory random behavior with each possible target number.

The operating cycle of the MCTS is controlled by a counter-decoder sequencing circuit which steps through six operations for each trial. Step 1 begins a new trial sequence. Step 2 generates a new random number except in precognition mode. Step 3 displays the new number to the agent (except in clairvoyant mode, in which the target is merely stored internally), resets the subject's keyboard, and turns on the subject "ready" light. The sequencer then pauses for the subject's response (except in the PK2 mode which continues automatically). If the subject presses the pass button the sequencer returns to Step 1; if a target selection is made, the sequencer advances to Step 4 and records the trial. In Step 5 a random number is generated for the precognition mode. Step 6 contains an optional pause for confidence calls, preset to two seconds, and an optional feedback interval during which the correct choice is displayed to the subject. The confidence calls, hits, and confident hits are recorded and the sequencer returns to Step 1. Each step takes about one millisecond unless a pause is selected.

Additional feedback devices have been used with the PK2 mode and a target set of two. A ring of 16 lights similar to that devised by Schmidt was constructed which employed binary random decisions to advance the illuminated light one step clockwise or counterclockwise with each decision, thus producing a "random walk" back and forth around the circle. Subjects seem to prefer the fastest cycle speed that allows them to clearly see each step, about 10 per second. At the highest rate of 50 per second, a blur of two or three lights appears to dance around the ring, and many subjects report that this was too much information at once. An audio feedback device was constructed which produced a continuous tone that rose in pitch with hits and dropped with misses. Evaluation of such feedback is not complete.

The MCTS has been employed for several preliminary PK and GESP experiments by students in university undergraduate and extension courses in parapsychology. It provides a quantitative framework for the design of experiments and greatly reduces the amount of tedious paperwork in experiments involving many trials. Student response to the MCTS has been encouraging.

RESEARCH BRIEFS III*

GANZFELD TECHNIQUES WITH INDEPENDENT RATING FOR MEASURING GESP AND PRECOGNITION

B. J. Dunne, E. Warnock, and J. P. Bisaha† (Mundelein College, Chicago)

A Ganzfeld environment was used to stimulate GESP and precognition in order to obtain further evidence to support the hypothesis that conditions of sensory inhibition are conducive to psi experiences. A second hypothesis under consideration was that a subject who displayed an ability for one form of psi would be able to evince ability for other forms as well. The experimental design was based primarily on Honorton and Harper's original Ganzfeld study [RIP 1973, pp. 52-3]. However, it was expanded to include a test of precognition as well as GESP by having the subject begin her report before the target had been selected, a procedure similar to that employed in the precognitive remote viewing experiments of Puthoff and Targ [RIP 1975, pp. 37-40]. The determination of whether precognition (or GESP) occurred was made by six independent judges using Morris's method of evaluating preferentially matched free-response material, instead of the self-rating procedure which Honorton employed with his subjects. It was hoped that this approach might provide a more objective and broader-based confirmation of the influence of psi in the experimental trials.

One female college student, age 32, acted as subject. She was seated comfortably in a quiet room softly lit by indirect lighting while halved ping-pong balls were placed over her eyes and absorbent cotton plugs were placed in her ears to produce a Ganzfeld environment of sensory inhibition. A tape recorder was placed nearby. The subject was told to relax and let her mind become empty, and at a given signal

*Chairperson: John Palmer, University of California, Davis.

to begin thinking out loud, reporting all images, thoughts, sensations or impressions which passed through her mind, without dwelling on any of them or attempting to interpret or anticipate any message. Each trial lasted a total of 15 minutes; the first 10 minutes constituted Part A, a test for precognition, and the last five minutes constituted Part B, a test for GESP. An observer, seated silently in the room with the subject, kept track of time and made a notation as to the place in the narrative which was reached after 10 minutes had elapsed.

An experimenter, with timing synchronized, was seated in a separate room, equipped with a View Master stereoscopic viewer and a target pool of 30 View Master slide reels. The slides had been previously randomized by a third party, otherwise unconnected with the experiment. When 10 minutes had passed after the beginning of the trial period, the experimenter randomly selected one of the slide reels by drawing one from the box without looking, inserted it into the viewer, and slowly rotated the reel for five minutes, concentrating on the scenes she was viewing. A total of six trials were completed in this way and the subject's narratives were transcribed and typed. This resulted in a total of 12 transcripts: six for Part A and six for Part B. The six target reels were noted and numbered in a random sequence of one through six.

Six independent judges were asked to blind rank order the transcripts of Part A by matching them with the slides and assigning a rank from one to six, one being the best match and six the worst. The same procedure was followed for the six transcripts of Part B. Six sets of scores were thus obtained, each containing six scores for Part A and six for Part B.

The statistical analysis of the experimental trials was based on the rankings given by the judges, in accordance with Morris's method for evaluating preferentially-matched, free-response material. The lowest (best) possible rank sum was six, the highest (worst) was 36. The mean rank sum for Part A was 13.166 with a mean rating of 2.19; that for Part B was 11.333 with a mean rating of 1.89. The overall significance levels of the two segments for all judges combined showed psi-hitting at $P < .04$ for Part A and $P < .01$ for Part B. A t-test showed no significant difference between the results of the two portions. Although the results of the GESP segment were somewhat better than those for the precognitive segment, ranking of Parts A and B by the same

judges could have resulted in a clueing effect. These results lend support to Honorton's findings that psi phenomena are produced at significant levels under circumstances of sensory inhibition, and that these circumstances do not appear to favor one form of psi over another.

THE INFLUENCE OF IMMEDIATE FEEDBACK UPON FREE-RESPONSE GESP PERFORMANCE DURING GANZFELD STIMULATION

William Braud† and Robert Wood (Mind Science Foundation)

If various techniques such as progressive relaxation and sensory restriction successfully reduce psi-irrelevant "noise" and increase a percipient's likelihood of attending to otherwise unnoticed psi "signals," the percipient's task then becomes one of recognizing which of his psi impressions relate to the target at hand and which relate to nontarget events. A technique which might allow a percipient to discriminate target-relevant from target-irrelevant impressions would be to provide immediate feedback for correct (target-relevant) responses emitted during an experimental session. Such immediate feedback for psi hits could result in two outcomes: an increase in the probability for hits (since these are immediately, positively reinforced), and the gradual development of an ability to recognize subtle internal cues associated with target-relevant information and hence increased feelings of confidence about whether a given impression is correct or not. Feedback could later be eliminated to determine whether these effects persist; i. e., whether "learning" has occurred.

Tart has concluded that immediate feedback for correct responses is indeed effective in the context of restricted-response GESP. This report summarizes our initial effort to apply immediate feedback in a free-response GESP experiment conducted under a sensory restriction or Ganzfeld condition which recent research suggests is psi-conducive. The design involved two independent groups of 15 percipients each who attempted to gain GESP impressions of 35-mm slides viewed by an agent in another room. Each percipient was asked to bring to the lab a person with whom he or she felt particularly close to serve as agent. The procedure for experimental (immediate feedback) percipients was as follows:

in the pretest session 30 minutes of Ganzfeld stimulation (produced by halved ping-pong balls and white noise) were followed by a five-minute, no-feedback pretest during which an agent in a distant room viewed the target slide; then came four practice sessions, each consisting of two 15-minute feedback periods (during each of which a different slide was exposed), each followed by a five-minute, no-feedback test, all conducted during Ganzfeld stimulation; finally came a no-feedback posttest session identical to the pretest. The procedure for the six sessions for control (no feedback) percipients was identical, with the exception that feedback was never provided during the practice sessions.

The percipient was signaled as to the beginning and end of target exposure. The percipient's experimenter remained in the room with the percipient and copied his or her mentation reports, which were spoken aloud continuously throughout the sessions. The agent and agent's experimenter listened to the mentation reports (through a one-way intercom) while watching the projected target slides and provided the percipient with immediate feedback (a two-second tone) for any mention of some content of the target slide. The target pool consisted of the 1024 slides of Honorton's recently devised binary target system [RIP 1974, pp. 112-5], from which targets were selected through use of a random number table. At the end of a session, the percipient coded the content of his or her mentation about the nonfeedback target slide according to the 10 categories of the binary system twice: once for the target exposure period only, and once for the session as a whole. The percipient then rank-ordered four slides (the target plus three randomly selected alternative slides never seen by the agent or agent's experimenter, delivered to the percipient's room without the possibility of sensory leakage) from most ("1") to least ("4") similarity to his or her impressions. The agent and agent's experimenter then entered the room and revealed the identity of the correct target.

Summary statistics were computed for the three psi measures: binary code for impressions mentioned during the target-exposure period, binary code for all impressions mentioned during the entire session, and target ranking. CR's were performed for feedback and control groups separately for pretest, posttest, and pre- to post-change scores (the latter were also assessed by Wilcoxon test). In addition, feedback and control groups were compared within pre- and posttest conditions by CRd and Mann-Whitney U Test.

The important conclusions to be reached from these data are that, for the exposure period binary coding measure, the feedback and control groups evinced no psi during the pretest and did not differ from each other; by the time of the posttest, the feedback group had improved significantly, now evidenced psi-hitting, and was significantly superior to the control group, which still showed no psi and did not differ from its pretest value. Similar trends are seen in the other two psi measures, but the pre-post improvement of the feedback group did not reach significance, nor did the feedback-control posttest difference. The only aberrant findings were the control group's psi-missing during the pretest on the target ranking measure and that group's marginally significant improvement from pre- to posttest. Combining all no-feedback test scores (exposure period binary codes) for all percipients across all sessions yields strikingly significant evidence for the presence of psi-hitting in the experiment as a whole for feedback (CR = 4.33; $P < .000017$, two-tailed) and control (CR = 2.07; $P \simeq .038$, two-tailed) subjects.

The feedback group's significant improvement from pretest to posttest does not seem to reflect a general habituation to the testing conditions, since such a factor should also influence the control group. We believe the improvement reflects a learning effect attributable to immediate feedback, but a psi-mediated experimenter effect remains a viable alternative explanation for these results.

EXPERIENTIAL FACTORS RELATED TO ESP IN THE GANZFELD

John Palmer†, David N. Bogart, Susan M. Jones, and Charles T. Tart (University of California, Davis)

Twenty-five students from a class on altered states of consciousness and four students plus the instructor from a seminar on biofeedback training volunteered for an ESP Ganzfeld experiment in which it was hoped to replicate the positive results of Honorton and Harper [RIP 1973, pp. 52-3]. Each subject also participated in a preliminary session where he received a half-hour of EMG feedback training to teach him to relax in a laboratory setting, and had an opportunity to become acquainted with the experimenter (S. M. J.) and the agent (D. N. B.). D. N. B. had served as agent in two previous successful ESP experiments.

When the subject arrived for the Ganzfeld session, he was seated in a comfortable reclining chair and his eyes were covered with halved ping-pong balls. During the actual session, a spot reflector lamp of adjustable distance from the subject shone into the ping-pong balls, and moderately loud white noise was played through headphones. The session lasted 35 minutes, during which time S. M. J., seated beside the subject, transcribed his mentation reports. The session was preceded by brief autogenic training (relaxation) exercises and instructions almost identical to those used by Honorton and Harper.

While S. M. J and D. N. B. were preparing the subject, J. P. entered a random number table to select one of 510 colored slides from the Maimonides binary target pool; he then placed this slide in a projector in the agent's room. D. N. B. came into this room just as the session was to begin. During one of three five-minute periods, he turned on the projector and concentrated on "sending" the slide to the subject. For half the subjects a special device was placed in front of the projector which caused the slide to be projected stroboscopically. The three sending periods and two strobe conditions were varied factorially across subjects but otherwise randomly assigned.

Following the session, D. N. B. came to J. P. 's office. J. P. then selected three control slides from a special program designed to make the four slides in the set as different from each other as practically possible. He then placed the slides in the projector in the agent's room in an order based on their sequence numbers in the pool. Meanwhile, the subject filled out a rating scale describing his experience during the session and predicted the presence or absence in the target of nine categories from the binary system (since our target pool consisted only of colored slides, the tenth binary category was not appropriate). S. M. J. then took the subject to the agent's room (since vacated by J. P.) where he rated each of the four slides on a 31-point scale according to its correspondence to his mentation. Later, two independent judges each rated the subject's transcript against eight pictures, four of which were the actual target set and four of which were the target set used for another subject (control set). The judges were blind both as to the identity of the target picture and as to the target set.

The ESP deviation scores from both the subjects' and judges' ratings were standard scores based on the difference

between the rating of the target picture and the mean rating of all pictures in the target set. The scores of the two judges correlated +.47 and were combined, the resulting scores correlating +.56 with scores based on the subjects' ratings. In addition, the judges' ratings provided a measure of displacement within the target set, using the control set ratings. The exact computation is too complicated to describe here, but is presented in the April 1977 issue of the Journal of the American Society for Psychical Research.

The mean ESP deviation score was below chance by both subjects' (-0.08) and judges' (-0.29) ratings, using the 31-point scale, the latter being significant ($t = 2.20$, $df = 29$; $P < .05$, two-tailed). Based on subjects' ratings, the mean was significantly higher for subjects from the biofeedback class than from the altered states class (+0.88 vs. -0.27, $U = 106$; $P < .02$, two-tailed), but the former mean was substantially reduced with the judges' ratings. The subjects' binary ratings did not predict target content at all.

For both subjects' and judges' ratings, there was a significant negative correlation between ESP scores and a composite of rating scale items, derived from factor analysis, that reflected the degree of consciousness alteration in the Ganzfeld (-.38; $P < .05$, two-tailed for subjects; -.47; $P < .01$, two-tailed for judges). Combined with the overall psi-missing, this result demonstrates that subjects who experienced strong altered states of consciousness exhibited the most ESP, but in the negative direction.

There was a positive correlation between ESP scores and subjects' tendency to underestimate the duration of the Ganzfeld (+.35; $P < .10$, two-tailed for subjects; +.40; $P < .05$, two-tailed for judges). Although two-tailed significance was obtained only with the judges' ratings, this finding confirms a significant relation reported in RIP 1974 (pp. 89-93) by Stanford and Neylon.

There was significant evidence of displacement in the ratings of one judge (+0.37; $t = 2.58$; $P < .02$, two-tailed). The mean displacement score of the other judge (-0.22) was significantly lower ($t = 4.19$; $P < .001$, two-tailed). The second judge paid less attention to indirect correspondences, and this seemed to be largely responsible for the difference.

The strobe device had no significant effect on ESP scores, but ESP deviation scores were higher when the tar-

get was sent during the middle sending period than during the earlier or later periods, significantly so only by the judges' ratings.

THE DEFENSE MECHANISM TEST (DMT) AS AN INDICATOR OF PSYCHIC PERFORMANCE AS MEASURED BY A FREE-RESPONSE CLAIRVOYANCE TEST USING A GANZFELD TECHNIQUE

Michael York (University of California, Santa Barbara)

The Defense Mechanism Test (DMT) has been reported to differentiate successful from nonsuccessful psi scorers. The DMT attempts to measure the level of unconscious anxiety proneness by producing a "micro trauma" which, in turn, triggers an individual's submanifest anxieties. This is accomplished by tachistoscopically presenting a visual image consisting of a central "hero" figure and a peripheral "threatening" figure. The subject's task is to describe the visual image as accurately as possible given the brief exposure period. Anxiety proneness is measured according to the degree and kind of perceptual distortions the subject projects into the stimulus picture prior to correct identification. Martin Johnson and H. K. Kanthamani found that low-anxious subjects, as determined by the DMT, performed more positively than did high-anxious subjects on a restricted-choice clairvoyance test. The present study attempted to repeat this finding using a free-response clairvoyance test. To increase the likelihood of psi-hitting, subjects went through the Ganzfeld technique as well as a relaxation tape.

Fifty-two university students participated in the two-phase design. Phase one involved the individual tachistoscopic presentation of possibly anxiety-inducing visual material as per standard DMT procedure. Final scoring of these protocols by a trained professional is now being done, so no results for this aspect of the study are yet available. Phase two required each volunteer to return at a later time for an individual psi test. Targets consisted of 100 art prints representing five thematic categories (water scenes, landscapes, abstract-surrealistic, religious, and human figures). Twenty mini-target pools of five each were then constructed by a neutral party, consisting of one print from each thematic group. Finally, individual targets were selected using a ran-

dom number generator from each mini-target pool by an otherwise uninvolved assistant.

Each subject's individual target was sealed in an opaque envelope and placed on a table in the target room. An assistant remained with the target at all times to insure its safekeeping. After a preliminary introduction aimed at putting the subject at ease and increasing confidence, the subject was shown the target envelope but was not allowed to touch it. The subject and the experimenter then went three doors down the hall to the relaxation room. A warm congenial atmosphere was maintained throughout. When the subject felt ready, the halved ping-pong balls were placed over his eyes and headphones were donned. The subject lay down on a couch underneath a fluorescent desk lamp placed so as to provide a light source free from either shadow or glare. The relaxation tape was then administered. The experimenter remained in the room to insure control and to record any impressions given by the subject during the impression period. At the completion of the relaxation procedure, the experimenter knocked once on the target room door. The mini-target pool was then reconstructed by the assistant and slid under the door to the waiting experimenter who then presented all five art prints to the subject. The subject then indicated the degree of correspondence each print had with his or her impressions, by making a mark on a continuum from no correspondence to perfect correspondence. At no time before or during the subject's scoring of art prints was the experimenter aware of the actual target picture.

Forty-nine subjects completed both phases of the experiment. Eighteen subjects rated the correct target highest of the five, and one subject rated the target and a nontarget picture equally high ($CR = 2.92$; $P < .005$, two-tailed). Despite the incomplete nature of the results (pending final DMT scoring), the significance of the overall psi scores serves as a confirmation of the Ganzfeld technique using a clairvoyance procedure. These results also lend additional strength to the use of relaxation tapes to enhance psi receptivity.

RELAXATION TECHNIQUES, FEEDBACK, AND GESP: A PRELIMINARY STUDY

Susan Mockenhaupt, Peter Robblee, Richard C. Neville†, and Robert L. Morris (University of California, Santa Barbara)

At U. C. Santa Barbara we have devised a set of automated procedures that allow subjects to engage in free response imagery to a distant complex target, be shown a target pool which includes the target, select which member of the pool is most like the subject's imagery, and be informed within seconds which member of the target pool was indeed the target. The subject can then review the target pool, examine which impressions were accurate and which were not, and move on to another imagery period for another target. To illustrate this set of procedures, we present data from an agent-percipient pilot study with a single subject. The target involved in each trial was a slide picture taken at random from the 512 color slides of the Maimonides slide pool [RIP 1974, pp. 112-5].

Prior to each run an assistant made up two sets of five closed, opaque envelopes, each containing two slides of opposite content. To do this he used a random number generator to select one slide from the Maimonides target pool. He then noted which of the nine content categories were present and which were absent, and selected the slide from the Maimonides pool that had the exact opposite distribution of categories to be the second member of the target pool for a given trial. Five such couplets were chosen, representing the target pools for the five trials done each session. One set was given to the agent, and the other set was taken by the experimenter. The agent next entered one room, and the percipient entered an adjacent room with the experimenter. The percipient then went through a relaxation procedure of his own devising, i. e. sitting down in a comfortable chair, closing his eyes, and clearing his thoughts while "reaching out" mentally towards the agent. At the end of approximately 10 minutes the percipient indicated he was ready, whereupon the experimenter signaled to the agent that the run was to begin by knocking once on the wall. The agent opened envelope #1A, removed the slides, and activated the Modular Communications Testing System (MCTS) described elsewhere [pp. 38-40]. An agent's display console then lit up one of two lights. If the light on the left was lit, it indicated the slide with the lower number

had been selected as target. If the one on the right was lit, the slide with the higher number was chosen. The agent then inserted the chosen slide in a slide projector and spent four minutes concentrating on the slide.

During this four-minute period the percipient attempted to extract impressions concerning the slide. These impressions were verbalized and recorded by the experimenter, who also acted as timekeeper. At the end of four minutes the experimenter opened envelope #1P and gave the enclosed slides (identical to those in envelope #1A) to the percipient who then viewed the slides and selected the slide he believed to be the target for that trial. The percipient's choice was then entered on a remote keyboard of the MCTS, which compared the selection with the actual target electronically, cumulatively tallied the trial and whether or not a hit was made on the central MCTS console, and lit one of two lights to give the percipient feedback. Both agent and experimenter recorded the slides, the target and the selection of the percipient. At this point, the agent opened envelope #2A, the random number generator selected a second target slide, and the second trial was initiated.

At the end of each run the agent, percipient, and experimenter discussed the agent's impressions of the target slides, the percipient's imagery, and the overall emotional and physical environment during the run. Sixteen runs were completed at the rate of one a day over a period of four months. In addition to the percipient's blind judging, two additional blind judges compared the percipient's imagery with the target and control slides.

It was hoped that by using slide pairs whose categorical composition was opposed, the judges would be confronted with clearer choices. Judges were allowed to declare no decision if no clear choice could be made. Of the 80 trials done, the percipient's rankings produced 42 hits, which was clearly nonsignificant (CR = 0.45). A consensus ranking of the 69 trials in which judges' decisions were made produced 41 hits, a stronger result but still nonsignificant (CR = 1.57). The data did produce one post hoc result which may be of use in further research with this subject. Almost all of the data were collected on Monday or Tuesday. The Monday results produced 11 hits of 30 trials (CR = -1.46) by percipient's ranking and 15 hits of 30 trials by consensus ranking (CR = 0.00). Tuesday results produced 31 hits of 45 trials by percipient's ranking (CR = 2.54; $P < .02$, two-tailed) and 26 hits

of 39 trials by consensus ranking (CR = 2.08; $P < .05$, two-tailed). Thus Mondays appeared to be poor and Tuesdays good. This may be due to a practice effect which then dissipated over the week, it may mean that the first day of the work week was not good for this particular subject, or it may indicate some yet unknown factor.

A THREE-EXPERIMENTER GANZFELD: AN ATTEMPT TO USE THE GANZFELD TECHNIQUE TO STUDY THE EXPERIMENTER EFFECT

Adrian Parker†, Brian Millar, and John Beloff (University of Edinburgh)

One of the difficulties in studying the experimenter effect in parapsychology is guaranteeing the occurrence of some ESP. The Ganzfeld ranks high among the candidates for a reliable technique of ESP induction, having shown a remarkable degree of success with different experimenters. Despite this, comments have recently been made stressing the importance of social psychological variables in the use of the technique; in particular, it is said to be sensitive to experimenter-subject interaction effects.

Since A. P. has had some previous success with the Ganzfeld in inducing ESP effects (among subjects who reported being sensitive to changes produced by it), it was decided that the technique would provide a convenient method of evaluating the effect of different experimenters with the same subjects. Twenty-four subject-agent pairs were selected on an unpaid volunteer basis, and completed three sessions, one with each of the three experimenters (A. P., B. M., and J. B.). They were allocated to experimenters in a random order and session order was also randomized, but arranged so that each experimenter had eight first-session subjects, eight second-session subjects, and eight third-session subjects. Several interpersonal and intrapersonal measures were taken. All sessions were tape recorded for the initial interaction between the subject, agent, and experimenter. During this period the purpose of the experiment was explained and instructions were given on how to respond to the Ganzfeld. The tapes were later rated on a scale which had been standardized as a measure of rapport by an independent blind judge, external to the experiment, whose ratings had

been shown to correlate significantly with those of two other judges using a sample of the tapes.

Subjects entered a 35-minute Ganzfeld period in which they were required to give a continuous report of their experiences, and also to respond every 10 minutes with a state report. During the last five minutes the agent, located in a separate room, viewed an art picture slide which had been randomly selected from a set of six by an assistant. At the end of the Ganzfeld session both subject and agent rated their expectancy of success and their mood on 10-point scales. The subject then ranked the six possible targets in each set in order of proximity to his Ganzfeld experiences. Instructions on how to do this were given and were similar to those used in previous Ganzfeld experimentation. Finally the subject responded to a brief projective test intended to reveal his feelings about the immediate experimental situation. Throughout the experiment, experimenters were blind to each other's results. Beyond keeping to the formal procedural instructions, experimenters were free to respond to subjects in their own natural style of interaction.

Overall results in terms of ESP scores, both for the experiment as a whole and for each of the three experimenters, were close to chance. The analyses of the ESP scores in relation to state reports, expectancy of success, mood, the projective measure, and rapport also revealed values close to chance expectation. Moreover, a multiple regression correlation of these variables (taken together) with hits was nonsignificant ($r = .153$, $df = 71$).

Experimenters did in fact differ in their interaction with the subjects. Quite significant differences were shown in terms of rapport, and marginal differences in terms of the projective measure and expectancy of success they elicited. However, these differences failed to show an influence on the scoring pattern. Differences between the experimenters with regard to the subjects' moods and state reports were nonsignificant.

As well as detracting from the reliability of the Ganzfeld technique, it is felt that these results argue against the plausibility of the view that psychological factors are the sole mediating variables of the experimenter effect. Since a total of 72 subject-experimenter sessions were involved, it seems probable that some of these would have involved the optimum psychological conditions for the occurrence of subject's psi.

Even a selection of those sessions in which subjects had high state scores, reported they were in a good mood, had a high expectancy of success, and were judged to have a high rapport with the experimenter failed to show a significant departure of hits from chance expectation.

THE AIRPORT PROJECT: A SURVEY OF THE TECHNIQUES FOR PSYCHIC DEVELOPMENT ADVOCATED BY POPULAR BOOKS

Robert L. Morris (University of California, Santa Barbara)*

During the winter and fall quarters of 1975, students of my introductory parapsychology course were given two possible term paper assignments, one of which was the following: "Locate a popular book on how to develop psychic abilities, of the sort you might find in bus stop or airport newsstands [hence the name of the project]. Read the book and prepare a detailed outline of the techniques suggested in the book. Then critique the book with respect to the material we have covered in class." The two classes combined produced outlines from 74 books, representing 57 authors. The collective advice was completed, categorized, and compared by myself. From these comparisons an overall picture emerged of the advice in the books.

General health. Slightly less than half the books were described as mentioning health. Those that did consistently advocated being in good physical and mental health such that one was not actively concerned with problems in these areas. Drugs were strongly discouraged. No special diet was consistently advocated, although a few did advocate meatless diets during the time of testing.

Initial attitude. All advocated an initial positive, receptive attitude, an acceptance of what was about to happen, complete confidence in one's own psychic powers and potential, and strong motivation to achieve either general or specific goals through development of one's psychic powers. Many books offered a specific belief system which should be accepted totally as one proceeds along the path of psychic develop-

*Presented by M. York.

ment. Such belief systems generally focused on some higher spiritual intelligence(s) such as God and Jesus, or else emphasized internal energy sources such as those frequently mentioned in Eastern mystical traditions. Those who did not accept the book's advocated belief system were told they were less likely to succeed, since their acceptance of what was to come would be weaker.

Nature of the developmental session. Two different strategies appeared in the books. Some advocated simply being more generally attentive to one's daily life experiences, including dreams, hunches, coincidences, and so on, such that one will be more aware of psychic events as they occur. One should take detailed notes, avoid screening out any impressions that might be relevant, and at all times be "aware of one's psychic powers." Most advocated specific developmental sessions once a day, to take place in a quiet, dark room free from disturbances. Almost all books discussing specific developmental sessions advocated sitting in a comfortable chair or lying on a bed, wearing loose clothing. At the start, one should go through a progressive body relaxation procedure followed by some sort of mind-clearing procedure designed to focus one's attention inward, yet away from the stream-of-consciousness thoughts of the day. The mind-clearing techniques varied from passive meditation upon each thought as it enters awareness until it dissolves into meaninglessness, through concentration on an external object so as to exclude competing thoughts, onto active expulsion of extraneous thoughts such that the mind becomes a totally blank screen. Some books recommended that these procedures be accompanied in part by regulated breathing exercises, generally drawn from well-known yogic practices.

At this point the advice became much more varied. Several of the books suggested allowing the body and mind-clearing procedures to lead directly into a dreamlike state or a period of quiet concentration, during which psychic impressions would gradually come forth. Others advocated the use of some specific sort of imagery to facilitate the psi process, such as imagining a beam of light or a long tube extending out towards the target and moving "energy" along that tube. Agents and receivers were advised to visualize each other. For PK, the target (or any other goal) should be visualized repeatedly. Many authors advocated the frequent visualization of light and other "energy" flowing through the body.

General advice. Readers were often advised to use a

ritual, such as the Tarot or I Ching, or procedures involving psychometric objects to facilitate concentration and/or place one in touch with greater forces. They were also advised to learn how to concentrate, keep a goal always in mind, practice a great deal, not be disappointed at initial failures, and avoid worries or fears.

Dangers. Several books warned against potential problems, such as entering too strong an altered state with resultant damage to mental health; excessive ego involvement with resultant damage to others and one's relations with them; and overestimation of one's own psychic abilities, as a result of the unwitting use of sensory cues or overinterpreting coincidences in the environment.

Much of this advice is consistent with research findings. The facilitating effects of confidence, positive attitude, progressive relaxation, and "mind-clearing" techniques have been frequent topics of research, although in each case the research is not without its ambiguities. Deliberate manipulation of confidence and goal orientation has been little studied; the same is true for specific imagery techniques. Very little developmental work with selected individuals has been done in recent years under controlled circumstances, to examine the growth in psi ability upon application of these procedures. And finally, almost no research has attempted to combine the factors discussed above, to maximize their effectiveness. Perhaps future research projects should aim for more intensive work with fewer individuals, to see the operation of these factors in more intimate detail.

A major criticism offered by the students of the books in general was that evaluation of success was left completely up to the reader. Only a handful of books described testing procedures to rule out sensory cues, expectation, personal bias, chance, and so on. It could easily be argued that the above guidelines are little more than systematic guidelines for producing fairly strong alterations of experience that are in themselves conducive to self-deception and overinterpretation rather than psi. The answer to this, of course, lies in systematic research that keeps the advice offered but provides a fair testing environment. Then, ideally, understanding will increase and the public will have the information it needs.

RESEARCH BRIEFS IV*

A STUDY OF OUT-OF-BODY EXPERIENCES USING AUDITORY TARGETS

Stuart Blue Harary† and Gerald Solfvin (Psychical Research Foundation, Durham, N. C.)

The present study is an attempt to explore the response of both naive and experienced subjects to auditory target stimuli which they attempt to experience visiting in an out-of-body state of consciousness. This is the first study of target responses during out-of-body experiences (OBEs) which deliberately sets out to utilize nonvisual target materials as a major component of the study. The naive group consisted of six students from a parapsychology seminar at the University of North Carolina who had no conscious memories of having had OBEs prior to this study. These persons participated in various group induction procedures for attempted OBEs while gathered in a room located about a quarter of a mile from the auditory target location. The experienced group consisted of two individuals, Ingo Swann and George Kokoris, who remembered having had previous OBEs, who felt they could induce these experiences at will for this experiment, and who had shown evidence of paranormal abilities in earlier visual target studies. The participants in this second group attempted to experience visiting the target location in Durham, N. C., from their homes in the New York City area.

A target pool was prepared consisting of 20 different sounds (such as bells, sirens, yodeling, musical instruments, etc.) recorded on separate cassette tapes. Each tape consisted of a one-minute silent period followed by 15 minutes of one target sound. The overall target pool was broken

*Chairperson: Stanley Krippner, Humanistic Psychology Institute.

down into five smaller pools of four targets each. Within each of these pools, the targets sounded as different from one another as possible. Each target tape was coded in order to eliminate sensory cues. At a selected time, the target area monitor randomly selected a pool and a target from this pool to be used for the evening's experiment. As a follow-up on earlier visually-oriented studies, the target monitor would flip a coin to decide whether or not to remain in the target location during the OBE period. (The monitor's presence or non-presence served as an animate visual target.) The monitor would then begin the 16-minute target tape. If he left the target area, he would not know the target for the evening since the target is preceded by the silent period.

At the agreed-upon hour, participants attempted to experience visiting the target area, to focus primarily upon auditory aspects of their experience, and to focus secondarily upon the visual presence or non-presence of the target monitor. Following the OBE period participants recorded their target impressions and then listened to a master tape of the target pool for the evening. They then rank-ordered the four selections according to their impressions. Participants also recorded whether or not they had a visual impression of the target monitor, in the target area, during the OBE period.

Throughout the experiment, participants were administered the "Questionnaire for Out-of-Body Experient" developed by S. B. H. This questionnaire is designed to record information regarding variable aspects of the OBEer's experience prior to, during, and following the OBE. When all impressions, rank-orderings, and other data were recorded and collected, the target monitor met with the rest of the group, the auditory tape was decoded, and its identity was revealed to all participants along with the visual target. Long-distance experients mailed their responses to the Foundation and these were received before the identity of the auditory and visual targets was revealed to them.

Results from experienced participants were analyzed separately from those of inexperienced participants. In preliminary analysis, Kokoris showed no significant results. Swann, however, attained significance on the target detection tasks. On the two consecutive nights that he participated, he scored direct hits on the auditory targets and on the agent-no agent (visual) condition, for a combined probability of 1 in

64 ($P < .05$, two-tailed). The inexperienced participants showed no overall significant scoring.

In addition to analyzing for overall success, it was also the plan of the study to correlate the target responses with the questionnaire items, using a computer-based correlation matrix formed from the questionnaire items and the target detection scores (ranks). We have no specific predictions about these items, since this is the first time this questionnaire, in its present form, has been used in an actual study. For both the experienced and inexperienced participants, this analysis is still being carried out and is expected to tell us more about mood and other variables possibly affecting overall scoring.

The use of nonvisual target materials may provide clues as to the "sensory" scope of OBEs. If it is indicated that psi may operate in multiple sensory modalities simultaneously during OBEs, and that the veridical quality of target responses in one modality might be positively correlated with the accuracy of responses in other modalities, this would provide testable hypotheses for further research. If verified, they might lead to an expanded definition of the OBE as a state which includes veridical multisensory psi rather than simply as a state which is defined purely subjectively. Ingo Swann's success on both the auditory and visual target tasks is encouraging because it suggests the possibility that OBEs may involve more than one sensory representation of psi operating simultaneously and positively correlated.

AN ATTEMPTED REVIVAL OF THE RÝZL TRAINING METHOD*

David P. Fourie (University of South Africa)

While I was studying the work of Milan Rýzl dating back more than ten years, as well as the attempted replications thereof, the following two ideas presented themselves. (1) Neither Rýzl nor the replicators took into account the

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fact that his psi training method used under hypnosis actually consisted of two distinct procedures, the effects of which could (and should) be studied separately. One procedure can be called "practice in vivid mental imagery," wherein the hypnotized subject tries to form an image of the target object or symbol. Feedback is provided to enable him to learn to distinguish between correct and incorrect images. The second procedure is suggested traveling clairvoyance, wherein the hypnotized subject is requested to imagine himself going to a certain location or to an object and looking at it while describing it to the hypnotist, who again provides him with feedback. These two procedures were used indiscriminately by Rýzl.

(2) Rýzl trained subjects individually, intensively, and over a long period of time. This insured the formation of a special relationship between him and every individual subject, a relationship not unlike that between a clinical psychologist and his patient. This relationship, while not totally absent in the replication attempts, seems not to have been established to the same extent.

The present study was therefore designed not only as an attempted validation of Rýzl's work, but as an extension thereof. Seven volunteer subjects between the ages of 19 and 49 (three men and four women) were trained extensively once a week for about a year. The effects of training on ESP were assessed by means of a GESP test of 10 runs using standard ESP symbols. The sender (co-experimenter) and recipient were in different rooms, each watched by an experimenter. Targets were arranged previously by an outsider using random number tables. Every individual subject was tested in this manner on four separate occasions: in the waking state, before training (Test 1); under hypnosis, before training (Test 2); under hypnosis, after training in mental imagery only (Test 3); under hypnosis, after the complete training (Test 4). The same sender was used throughout and the same experimenters were present.

Training was done exactly as described by Rýzl, except that it was strictly confined to practice in mental imagery before the third test, while thereafter practice was also given in traveling clairvoyance. Special emphasis was placed upon the establishment and maintenance of a relationship of trust between the experimenter (the hypnotist) and the individual subject.

The test data were analyzed in various ways for each individual subject. Mean run scores (direct hits) were compared among the four tests by the Mann-Whitney U Test. The same procedure was followed for the mean scores on four displaced positions (two forward and two backward). Run score variances, as well as the variances of the distributions of displaced scores, were compared between the four tests by means of F-tests. In order to detect any consistent confusion of ESP symbols at any stage, a five-by-five (targets-by-guesses) chi square analysis was done for every test.

No pattern of significant differences between any two of the tests could be found for any individual subject or for the group as a whole. As is expected by chance, about one out of 20 differences were significant at the P = .05 level. Most of these significant differences, however, were between either Test 1 and Test 3, or Test 2 and Test 3 (in either direction). Moreover, five of the seven CRs which were significant (three indicating psi-hitting and two indicating psi-missing) were obtained during Test 3, while a sixth was obtained during Test 4.

While it must be concluded that the training did not result in any significant improvement in ESP scoring (and also, in passing, that hypnosis per se did not seem to have any significant effect), the conclusion must be somewhat tempered by this last finding, which also gives a slight indication that practice in mental imagery may be more effective in eliciting improved ESP performance than suggested traveling clairvoyance.

A PSYCHOPHYSIOLOGICAL STUDY OF MEDIUMISTIC COMMUNICATORS

G. F. Solfvin†, W. G. Roll, and E. F. Kelly (Psychical Research Foundation)

Jan Cutler (J. C.), a medium with whom we had been in contact by mail, had told us she felt that her physiology was controlled by her guides when she was in trance. Exploratory tests at a local university gave some support to her impressions. On February 16, 1975, G. F. S. and W. G. R. met with J. C. and her husband in a motel room in Kalamazoo, Mich., for the purpose of collecting EEG recordings

during her trance. Mr. John E. Fetzer and Mr. and Mrs. Harold Schuster were also present and acted as sitters for the occasion.

Silver cup electrodes were attached to J. C. 's scalp using left and right Dement connections (slightly posterior to C2 and C4 in the 10-20 System) each referenced to the ipsilateral mastoid. These wires were plugged into a telemetry unit, attached to J. C. The unit, designed by Fritz Klein, transmitted both EEG signals to an FM receiver on the other side of the room. In this manner, J. C. remained relatively free to move about while her EEG was being recorded. The signal picked up by the FM receiver was recorded on one channel of a stereo tape recorder while the other channel simultaneously recorded the audio activity in the room. The tape provided a permanent record of the session which we could decipher and analyze at a later date. We used the following procedure: J. C. would rest in her seat, with eyes closed, for one minute, and then she was to concentrate on an image (also with closed eyes) for another minute. She would then "leave her body" and allow "William," one of her trance controls, to come through. After speaking with the sitter for a few minutes, "William" repeated the same procedure as J. C. This was repeated with a second trance control, "Mrs. William."

There were three steps to the analysis of the data. The first was to unravel the taped EEGs and produce a strip chart output for visual inspection. This was to give us an idea of the amount of muscle artifact and other interference in the record and the consequent feasibility of further analysis by computer. The record was relatively clear of artifact and seemed to show some rather striking differences between personalities that were encouraging for further analyses. For the second step, the EEGs (both left and right hemispheres) were broken up into contiguous one-second segments and this provided us with a log-transformed power value for each of the 32 steps in the 1-32 Hertz range. We then applied a multiple discriminant analysis (MDA) to assess the ability of the EEG variables to discriminate between personalities or conditions. The EEGs from the two hemispheres were analyzed separately.

The three personalities proved to be significantly discriminable using left hemispheric EEG. The largest differences were found between J. C. and "William," with the most influential frequencies in the range normally associated with

theta and delta waves (under 8 Hertz). The right hemispheric EEG discriminated even better, with the largest differences again found between J. C. and "William." The theta and delta frequencies were again the most influential discriminators, but the 11-17 and 23-28 Hertz ranges contributed more heavily than they had for the left hemisphere.

The MDA thus supported the impression gained from visual inspection of the EEG records that significant differences existed between the waking state of J. C. and when she was "controlled" by her two guides. The EEG change occurred in both hemispheres, and was most pronounced in the theta and delta frequency ranges. When a second trance control came in, the EEG responses again changed and became different from either of the previous records.

We cannot ask, at this point, whether "William" or "Mrs. William" are independent entities. We do not know enough about the use of the EEG as a unique identifier along the lines of a fingerprint or about the possibility of voluntary control of physiological processes. Measurable and reliable EEG changes have been used to define altered states (such as sleep) and some recent work has indicated that multiple personality cases may also show marked EEG changes between personalities. Ludwig et al. reported such a case where differences were found in alpha blocking and in the amplitude and frequency of the alpha among four different personalities (Archives of General Psychiatry, 1972, 26). In that study, the neurophysiological data taken together with the clinical-psychological and other psychophysiological measures made it "apparent that multiple personality cannot be parsimoniously and summarily dismissed by invoking traditional explanatory systems."

More studies need to be done on direct control mediumistic communications using the model of the multiple personality studies. Psychophysiological measures such as galvanic skin response to emotionally laden words, heart rate, respiratory rate, and plethysmograph can be taken simultaneously with the EEG. Psychological measures may also be used to supplement these data. A specific communicator who is unknown to the medium should be requested for experimental sessions. Ideally, this would be a deceased person for whom there are psychological and psychophysiological records available that are comparable to those being administered during the session. The Psychical Research Foundation is currently in the planning stages of a project that would incorporate these features.

SOME PSYCHOMETRIC EXPERIMENTS BY THE JERUSALEM STUDY GROUP OF THE ISRAEL PARAPSYCHOLOGY SOCIETY

H. C. Berendt (Israel Parapsychology Society)

Four psychometric experiments from two separate sessions are reported in this paper. Seven to 10 persons were present at each, almost all of them unknown to the paragnosts, and brought with them psychometric objects concealed in boxes. Two persons acted as paragnosts: Manu R. and Nava M., both of whom had begun to do psychometric experiments under my guidance. All proceedings were recorded on tape. The most important statements of the paragnosts were transcribed and judged as to their correctness by the owners of the objects, enabling a qualitative estimate of success to be made.

In Experiment I, Manu R. correctly described a religious amulet in a box handed to him by a person not previously known to him. In Experiment II, Nava M. selected a wrapped object from among several on a tray and proceeded not only to describe it with mixed success, but also to identify correctly the owner from among those present. In Experiment III, Nava M. again picked a concealed object from a tray, and correctly identified its owner, who happened to be the same man as in Experiment II. She further discerned that two separate people were associated with the outer box and its contents, respectively, and proceeded to describe both men as well as the object with a fair degree of success. In Experiment IV, Nava gave a description of an object similar to one successfully described in a previous experiment from an earlier series, the owner of which was present at the session. Neither the object nor the owner matched her description in this experiment, but in fact it turned out that the designated owner mistakenly thought that Nava had again picked his box which in fact contained the same object as before. Nava had apparently been using telepathy to describe his thoughts rather than the object she actually held.

Some interesting observations can be drawn from these experiments. Nava in Experiments II and III chose objects from the same owner, indicating that he had a dominant influence on her. In the first three experiments the initial statements of the paragnost contained more specific correct

information than later statements, indicating that deeper cognitive layers may be involved initially which then give way to more superficial logical processes as the experiment progresses.

Finally, Nava's correct unraveling of the two owners in Experiment III raises some important issues regarding psychometry. In this case the object's owner was present, whereas the owner of the box concealing the object was not only not present but was of no importance to anyone present. He was a pharmacist who had given the object's owner medicine contained in the box. The owner then used the box to conceal his object without any intention that the box itself might become the focus of Nava's attention. Nevertheless, she described the pharmacist in some detail. She might have been using telepathy to get this description from the object's owner, in which case she would be picking up material of which he was not immediately aware, but which had to be retrieved later on. Alternatively, she might have, by touching and handling the outer box, received some direct impressions about its former owner. If the latter interpretation is correct, then every object has the potential to transfer knowledge about what was or is connected with it, and all of us are living in an ocean of psychometric material.

A SURVEY OF THE ATTITUDES OF PSYCHOLOGISTS TOWARD PARAPSYCHOLOGY

David P. Fourie (University of South Africa)

In order to ascertain whether South African psychologists are interested in parapsychology, an attitude survey was conducted. A double-page bilingual questionnaire and stamped return envelope were posted to all 858 members of the two South African professional psychological organizations. This was followed two weeks later with a reminder; 290 usable replies were obtained, giving a response percentage of 33. 8. Not only were the respondents geographically well distributed, but distributions according to sex, home language and academic achievement were as expected. Negative as well as positive replies were received on all questions. The 290 respondents can therefore be regarded as fairly representative of the population under study.

The results show clearly that South African psychologists are interested in parapsychology: 59.7 percent of the respondents considered themselves either "interested" (49.7 percent) or "actively interested" (10 percent) in parapsychology. On this as on all other questions no significant differences were found between the sexes, between language groups, or between groups differing in highest academic achievement. However, it was found that of all the orientations within psychology, the personality psychologists expressed the most interest in parapsychology (92 percent were interested or actively interested), followed by the psychometricians (74 percent) and the clinical psychologists (67 percent). The physiological psychologists were the least interested (46 percent). All other orientations (e.g., social psychologists, counseling psychologists, etc.) fell between these extremes.

This finding is interesting but not surprising. Parapsychology touches on and has implications for fundamental questions, such as the mind-body problem, which also interest personality psychologists. Psychometricians, on the other hand, may be drawn by the methodology of parapsychology and its reliance on various statistical methods, while the physiological psychologists may be put off by the fact that no physiological mechanism of psi has yet been found.

The interest shown in parapsychology by psychologists seems to be largely a latent one waiting for an appropriate stimulus in order to become active. This is shown by the figure of 49.7 percent of respondents who described themselves as interested, but not actively interested, in parapsychology. The fact that, since 1970, only three theses on parapsychological topics have been written by South African psychologists is another indication of a lack of active interest. Speculation as to the reasons for this state of affairs should take into account the availability of other, less controversial and less frustrating research topics within psychology as well as the difficulty of obtaining financial assistance for parapsychological research.

The survey results furthermore show that psychologists see parapsychology as an inherent part of psychology: 76 percent of the respondents replied in the affirmative on this question. As is expected, those with a higher degree of interest in parapsychology feel so more strongly ($P = .001$) than those with less interest in this field. As psychologists would not like to think of any inherent part of their domain as unworthy of research effort, it follows that parapsychology

would be regarded as a valid field of research. In fact, 81 percent of the respondents replied affirmatively to this question. Here again the answers seem to have been partly determined by the degree of interest in parapsychology: 69 percent of those deeming parapsychology a valid research field were either interested or actively interested, compared with 14 percent of those doubting its validity. Related to this was the question of how the respondent felt toward the idea of parapsychological research in the institution where he or she was employed. Forty-two percent felt positive in this regard, although it was pointed out that many institutions (such as businesses) would not easily lend themselves to this type of research.

These relatively positive attitudes of the psychologists unfortunately form only part of the picture. Their reported knowledge of the parapsychological literature makes up the rest, which is much less reassuring. Only 21 percent of the respondents felt their knowledge of the literature to be either good (defined as a recent literature study) or fair (recent directed reading), while 79 percent admitted to either usually skipping parapsychological articles and papers or reading them almost accidentally. Again the expected correlation with interest in this field showed up clearly: 72 percent of those actively interested felt their knowledge of the literature to be either good or fair, compared to 4 percent of those little interested and 0 percent of those not interested in parapsychology. Because of this high correlation ($r = .62$) between interest in parapsychology and knowledge of the parapsychological literature, not only interest but also knowledge of the literature correlated significantly with the answers on most questions.

The main findings of this study thus are the great deal of interest (if not "active interest") displayed by the respondents, linked with the very limited extent of their knowledge of the parapsychological literature. This points clearly to the need for courses and lectures on parapsychology in respectable psychology settings such as universities. Much more should also be published about parapsychology, if possible, in psychology journals. In this way South African parapsychology could find a powerful ally in an intimately related field.

A SURVEY OF CATHOLIC THEOLOGICAL OPINION ON PARAPSYCHOLOGY IN AUSTRIA

Elmar R. Gruber (Institut für Grenzgebiete der Psychologie, Freiburg)

Following an inquiry about attitudes toward parapsychology among Catholic theologians in Germany performed by A. J. Hammers (Parapsychologie und Theologie, Bern/Frankfurt, 1975), I conducted an attitude survey among Austrian Catholic theologians employing a considerable number of items from the German questionnaire. The questionnaire was mailed to 700 Catholic theologians (graduate students of theology, priests, friars, and sisters) randomly selected by a systematic sampling method. The return rate of 35 percent was low but still considered satisfactory, since the questionnaire was rather extensive, covering 91 items.

In answer to a question about the existence of ESP and PK, the respondents indicated an extremely high degree of belief as compared with similar surveys: 95 percent of the theologians believed in the possibility of paranormal phenomena, whereas 43 percent considered telepathy, 22 percent clairvoyance, 18 percent precognition, and 26 percent PK as an established fact. Belief in these forms of psi as likely possibilities was remarkably stronger, ranging above 80 percent. The especially high percentage of belief in PK was partly due to Uri Geller's Vienna television show in 1974, about a month before the survey was performed. The appearance of Geller was preceded by an intense barrage of information about PK in the media. The questionnaire revealed that 90 percent of the sample had already heard of Geller.

There was an irresolution in the opinions of the questioned theologians regarding psi in general. On the one hand they felt that the existence of psi phenomena as independent from the religious context was confirmed to a high degree, while on the other hand they were severely skeptical when parapsychology threatened to penetrate the specific field of theology. In the latter case the affinity of parapsychology to the occult was strongly emphasized: 70 percent of the respondents agreed with the item, "Parapsychology shows a tendency to be a substitute for religion," which is a remarkably higher value than that in the German sample (42 percent). Also, the demonic influence in paranormal events was more

strongly emphasized in the Austrian (41 percent) than in the German survey (33 percent). On the other hand, 54 percent agreed with the item, "The accounts of parapsychology on the spatio-temporal independence of the soul provide a scientific support for religion." Items that were irresolutely judged showed a strong accumulation of ratings on the middle positions of the six-step attitude scale, suggesting an indecisive opinion on these items.

The possibility of training and applying the psi faculty was highly estimated by the Austrian Catholic theologians, though the accumulation on the middle positions suggests once more an incapability of making a decision: 96 percent of the questioned sample thought that parapsychology was a science, but only 49 percent considered it a science with an empirical character. This discrepancy between acknowledgment of parapsychology and its phenomena and lack of methodological awareness of its empirical grounds was also found in the German investigation. Though the old and conservative theologians in the German survey judged parapsychology most positively in itself, and also in its relevance to theology, these theologians possessed a low level of adequate information concerning experimental results of psi research. This was evident in their attitudes towards poltergeists. The German theologians assented more to the existence of RSPK the more they assumed the spiritistic and rejected the animistic hypothesis for its explanation. Here the irresolution of the Austrian theologians was even stronger because younger theologians, especially students, were slightly underrepresented in this sample, and the older respondents show the irresolutive pattern to a higher degree. The lack of information concerning experimental results was as striking as in the German sample and more distinct among younger respondents. When questioned about the animistic hypothesis for the explanation of poltergeist events, 27 percent did not respond. Though this result was expected after the pretest, the item was not eliminated and revealed in the main test a significant difference between younger and older theologians: the younger ones were less well informed.

The existence of animal psi was denied emphatically by the Austrian as well as the German theologians, as was the doctrine of the astral body. Both assumptions were incompatible with their scholastic concepts, which could be regarded as an immaterial explanation principle for psi phenomena. But the theologians hesitated to accept such philosophic-theological possibilities of explanation. Private psi

experiences of the respondents were not found to be more frequent than in the general population. Above all, the respondents judged the collaboration of parapsychology and natural sciences to be the most fruitful, while also stressing the collaboration of theology and psi research. Their high degree of belief in the existence and relevance of psi phenomena, as well as their remarkably high degree of ignorance of the results of experimental parapsychology, primarily among younger theologians, should induce parapsychologists to provide more information for the advancement of interdisciplinary work.

SUBJECTIVE REACTIONS TO THE FILIPINO "HEALERS": A QUESTIONNAIRE STUDY

Patricia Westerbeke, John Gover, and Stanley Krippner†
(Humanistic Psychology Institute, San Francisco, and California State College, Sonoma)

In an attempt to identify the subjective (or experiential) variables that characterize the reaction of the "healee" to the psychic healing experience, the present pilot study was formulated. On three occasions in 1973 and 1974, P. W. had an opportunity to travel to the Philippines with groups of indisposed persons who planned to see one or more of the well-known "healers" in that country. (No adequate scientific evaluation of these "healers" has been undertaken; and it is not the purpose of this paper to assess the reputed paranormal abilities of the Filipino "healers.")

P. W. prepared a series of questionnaires consisting largely of items with forced choices (three to seven possible responses). One questionnaire was distributed to each of the 85 members of the three groups before he or she had seen a "healer." Another questionnaire was administered within two weeks after the "healer" had been seen, and while the person seeking treatment was still in the Philippines. A total of 62 healees completed both questionnaires. Six months later, and one year later, a third questionnaire was mailed to each of the 62 persons who had answered the first questionnaires; 11 healees returned one or both copies of this questionnaire. For those healees who returned two copies of the questionnaire, the latter one received was used for data tabulation. Pearson product moment correlations were

calculated for 45 pairs of items--those which related most specifically to belief systems and the "healing" process.

The data suggest that one's post-session confidence in psychic healing (as recorded on the second questionnaire) is positively correlated with several items on the first questionnaire: stated willingness to change one's way of life if it meant being healed ($r = .953$; $P < .01$), personal experience with psi phenomena ($r = .290$; $P < .02$), and one's pre-session confidence in psychic healing ($r = .893$; $P < .01$).

The healee's post-session report on help obtained from the healer (on the second questionnaire) appears to be positively correlated with such items on the first questionnaire as pre-session confidence in psychic healing ($r = .564$; $P < .01$) and personal experience involving purported psi phenomena ($r = .304$; $P < .02$). In other words, a healee's confidence in psychic healing is, perhaps, preconditioned by experience with presumptively paranormal events.

The data from the third questionnaire indicate no perceived long-term physical, mental, or "spiritual" change associated with the healees' age, use of tobacco or alcohol, or with their religious habits. Of significance is the association between long-term physical change and the healees' confidence in psychic healing before their sessions ($r = .699$; $P < .01$), help reportedly received from the healer ($r = .855$; $P < .01$), perceived change in body "energy" ($r = .838$; $P < .01$), and personal experience with purported psi phenomena ($r = .602$; $P < .02$). It is apparent that several of the healees' responses that significantly correlate with each other are measuring similar attitudes; therefore, the correlations are not surprising. Of greater interest are the relationships between exposure to purported psi phenomena and the report of immediate help from the healer as well as perceived long-lasting physical, mental, and "spiritual" changes.

The results of this pilot study demonstrate the importance of identifying the role played by both healee expectancy and the placebo effect in any future in-depth study of psychic healing among human beings. Admittedly, the number of healees in this pilot investigation was small, and the group studied may be atypical. Nevertheless, some correlations emerged which suggest the direction that future research might take.

RESEARCH BRIEFS V*

PHYSIOLOGICAL VARIABLES AND PK

I. P. F. De Diana† and J. M. Houtkooper (University of Amsterdam)

A possible relationship exists between some parapsychological phenomena and physiological variables (e. g., Chari in Journal A. S. P. R., 1970, pp. 411-20). This may not be too surprising, since it is acceptable that parapsychological behavior, like all behavior, can be considered as a system of interacting psychophysiological variables. This means that parapsychological behavior, with regard to its possible interactional aspects with human physiology, has to be separated from other behavior patterns, such as emotional behavior, which also interact with human physiology. Therefore it is necessary to analyze physiological variables in their time relationship to parapsychological phenomena. Also, several physiological variables have to be taken into account. Furthermore, as it may be assumed that different parapsychological phenomena might have different behavior patterns, a standard automated method for detecting parapsychological phenomena has to be used.

Therefore, our present line of experimentation has the following purposes: (1) development of a research methodology that is able to operationalize hypotheses on the relationships between a form of parapsychological behavior and some physiological variables; and (2) discovery of a method to analyze whether physiological changes precede or are concomitant with the occurrence of PK, instead of following it.

The opportunity to make a start with this research came with the visit to Amsterdam of Matthew Manning, a well-known gifted PK subject (see Vol. 1, No. 2, European

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Journal of Parapsychology, pp. 6-16). The experimental set-up consisted of an electronic random number generator coupled to a computer (see Vol. 1, No. 1, European Journal of Parapsychology, pp. 3-14). The PK procedure consisted of 32 pre-run intervals, each of about 17 seconds' duration, each followed by a run of 15 seconds' duration. The 16 experimental and 16 control runs (runs with and without a target) were alternated. The following physiological measures were registered on an instrumentation recorder: occipital EEG, ECG (electrocardiogram), and respiration.

For each run of the PK series the deviation from chance level was expressed as a z score (standard normal score). From the z scores we calculated the z^2 scores, indicating the extremeness of the deviation from chance level in either direction. Summing the z^2 values, we obtained an overall measure of scoring extremeness, chi square distributed under the null hypothesis. The chi-square value obtained for all 32 runs was 50.29 ($P < .025$). From the extra variance in this value of chi square, a correlation with some physiological variable could be expected up to .60, with $P << .001$.

From the ECG, mean and standard deviation of R-peak time intervals in heart rate were calculated. The respiration data were processed to derive mean inspiration time, mean time of the total respiration period, standard deviation of respiration periods, mean expiration time, and mean respiration depth or amplitude. From the EEG, the percentage of summed energy (power) in four frequency bands was used in the analysis: delta (.5-4 Hz), theta (4-8 Hz), alpha (8-13 Hz), and beta (13-25 Hz). For each section of the experiment, a group of new physiological variables was created, consisting of the values in the preceding period minus the actual values of the existing variables. The new variables thus indicated a change in physiological state. Pearson product moment correlations were calculated over all 32 runs between the z^2 values and both the change values and the actual values of the physiological variables. Also, correlations were calculated between the z values and both the change and actual physiological values for the 16 experimental runs only.

A start has been made with a method to analyze several physiological variables in their relationship with PK. The strongest relationships we found were between z^2 and the actual energy percentage in the delta and alpha EEG

bands (positively and negatively correlated, respectively, at $P < .06$, two-tailed). However, a method is needed that is foolproof against other possible behavioral-physiological relationships such as emotion. One way to guard against this is to have some of the PK runs done without visual feedback. This will be implemented in future experiments.

PRELIMINARY PHYSICAL MEASUREMENTS OF PSYCHOPHYSICAL EFFECTS ASSOCIATED WITH THREE ALLEGED PSYCHIC HEALERS

R. G. Macdonald, J. L. Hickman, and H. S. Dakin (H. S. Dakin Company)*

Attempts were made to replicate a number of previous experiments on physical effects produced by alleged psychic healers. Three widely-known healers participated as subjects: the Rev. John Scudder (J. S.), Mr. Dean Kraft (D. K.), and Mrs. Olga Worrall (O. W.). These subjects cooperated in experiments that included cloud chamber observations, electric and magnetic field measurements, high-voltage and passive photographic film exposures, object weight measurements, Raman spectroscopic analysis of water samples, animal blood pressure measurements, growth measurements of treated and untreated plants, and electroencephalographic measurements. Occasionally, human volunteers with physical disorders acted as recipients of the healers' treatments. Most of the attempted replications did not confirm the reports of previous experimenters. However, two subjects appeared to produce significant effects on plant growth. One subject was also apparently able to cause significant electric field variations eight to 12 inches from his hands. These last two results are described more fully below.

Electric field measurements. Small changes in electric field strength near the subjects were detected by means of a 10^{12} ohm input High-Impedance Voltmeter (HIVM). This instrument cannot discriminate between voltage variations and position variations of objects. Therefore, the subject must be as still as possible. Measurements were conducted inside a Faraday Cage to minimize the effects of a 60-Hz

*Presented by J. Mishlove.

power line and other interference with the measurements. Preliminary tests with the HIVM showed its extreme sensitivity to movement of any kind and helped identify signal variations associated with heartbeat, breathing, and body movements. Therefore, an experimental procedure was established in an attempt to alleviate such sources of artifact. The subject's hands rested on a wooden table six to 12 inches below the detector. The subject was requested to move his or her hands, head, fingers, knees, and feet in order to identify the accompanying electric field changes. The subject then sat quietly while baseline measures were recorded and was then asked to enter his or her healing state of consciousness. After a brief practice session, random sequences of eight experimental (healing) and eight control (resting) trials were run. Each trial was 45 seconds long, with a 15-second rest period between trials. Body movement was continuously observed by an experimenter who triggered an event recorder at each movement.

Subjects J. S. and O. W. produced no anomalous electric field changes during controlled experimental sessions. D. K., however, was apparently able significantly to alter the electric field with almost no noticeable body movement throughout 40 experimental trials. During control trials, there was only one instance of significant electric field change without accompanying body movement, compared with 75 such events during the experimental periods. The approximate mean voltage of these unusual bursts was 4. 5 mV. The observed maximum amplitude and bandpass filter frequency limits were ±0. 006 volt and 0. 1 to 20 Hz, respectively.

Plant growth measurements. Two subjects, D. K. and O. W., took part in this experiment. We used a double-blind procedure in which sterile human physiological saline solution in sealed bottles was treated by a healer, then used to give initial waterings to 16 identical sterile, dry peat pots, each containing five rye grass seeds. A control group containing the same number of pots and seeds was initially watered with untreated saline solution. All pots were then placed close together in a random distribution such that all were exposed to the same sunlight intensity and room air conditions. Each pot was watered daily with 15 milliliters of distilled water. Three characteristics of plant growth were analyzed: the number of sprouted seeds per pot, the total height of all plants in each pot, and the mean height of the plants in each pot. The number of plants was a set of nonparametric values, so the Mann-Whitney U Test was

applied. Two-tailed t-tests were used to analyze the total and mean heights.

With D. K., only data from days 9, 10, and 11 were analyzed. On the previous days the plant growth was too scarce for comparison, and on the following days the plants had grown so tall that the shoots began to break. Data from the plants treated by O. W. were analyzed only on days 15, 16, 17, and 18 for the same reasons. On some of the days analyzed, the total and mean heights of the plants treated by D. K. were significantly less than those of the control group: total height day 9 ($P < .05$); mean height day 9 ($P < .02$); mean height day 10 ($P < .001$); mean height day 11 ($P < .001$). The total and mean heights of the treated group were on the average 15 percent and 17 percent less than the heights of the control group, respectively. There was no difference between the number of sprouted seeds in each pot on the three days analyzed.

The total and mean heights of O. W. 's experimental plants were sometimes significantly greater than those of the control group: total height day 15 ($P < .05$); total height day 16 ($P < .05$); mean height day 17 ($P < .05$); total height day 18 ($P < .05$). The total and mean heights of the treated group were on the average 27 percent and 18 percent greater than the heights of the control group, respectively. She had little influence on the number of sprouted seeds in each pot. These results lend support to some of the findings reported by Grad and others and suggest the importance of further experimentation.

RESEARCH BRIEFS VI*

SUBSENSORY PERCEPTION (SSP), EXTRASENSORY PERCEPTION (ESP), AND MEDITATION

K. Ramakrishna Rao† and Irpinder Puri (Andhra University, India)

The experiment reported here was an attempt to test the hypotheses that when subjects are tested for their extrasensory perception, using the same procedure and techniques as for testing their subsensory (subliminal) perception, a significant relationship between their subsensory perceptual scores and their ESP scores will be found; and that conditions which affect subsensory perceptual scores will also influence psi scores. Meditation was taken as a variable that might affect psi and subsensory scores, as it was believed that meditation would inhibit sensory perception and enhance one's psi ability.

The subjects were tested individually for their ESP ability and for subsensory perception by means of the Subsensory Psi Test (SPT). This was the pre-meditation session. Then for one week they took a course in Transcendental Meditation (TM) given by trained teachers. (None had been meditators before participating in the experiment.) After they were initiated into TM, the second series of SPT tests (post-meditation) was administered. Just before taking the test in the second session each subject meditated for 20 minutes. Thirty-eight subjects signed up for the experiment, but only 28 completed both the tests and the meditation course.

The SPT was designed to test the subject's ability to perceive subsensory and extrasensory targets. The extrasensory target presentation involved the projection of a "blank" slide which, it was hoped, like a projective test,

*Chairperson: H. Behrendt, Israel Parapsychology Society.

would facilitate access to the unconscious. It was further assumed that ESP received below the threshold of consciousness would enter into the determination of the perception of the ambiguous "blank" slide and that a procedure like this, by providing a suitable mediating vehicle, might not alert the defenses guarding against psi.

In this test slides are projected on a screen for a fraction of a second. The subject is asked to name the picture he sees on the screen. The picture is somewhat blurred by keeping the projection slightly out of focus. In each session the subject is exposed to one slide at a time taken from a pool of 60. The subject completes 60 trials in each session. The pool consists of 30 "blank" slides and 30 picture slides. The picture slides portray the following 10 objects: natural scene, bird, man, flower, watch, cabinet, child, book, airplane, and building. There are three slides for each object. They are used to test subsensory perception. The "blank" slides are lightly colored with black ink so as to present ambiguous visual stimuli. They are used to test ESP; each blank slide represents one of the 10 target pictures.

Separate random target sequences were prepared for each subject by a staff member at the Andhra Department of Psychology and Parapsychology, P. V. K. Another experimenter, I. P., administered the test individually to all the subjects. The subjects were given the following instructions: "The slides being shown to you contain pictures of the following 10 objects. Please write what pictures you see in the response columns serially. Do not skip any column. Make a guess if you cannot see clearly." The 10 objects were then named. After the subject had read the instructions and indicated that he understood them, the lights were switched off and the slides were projected, in the order determined by P. V. K., one after another under semi-dark conditions. The subject wrote his response after each projection on a record sheet provided for this purpose. The experimenter who presented the slides to the subject had no knowledge of the ESP target pictures corresponding to the blank slides. The person who prepared the ESP target pictures for the blank slides kept that information entirely to himself and did not involve himself in any phase of the testing.

The null hypotheses that the experimental results were expected to reject were (1) There will be no significant relationship between the subjects' subsensory and extrasensory

scores. (2) The subjects' ESP scores in the post-meditation session will not significantly differ from their pre-meditation scores. (3) The subjects' subsensory scores in the post-meditation session will not significantly differ from their scores in the pre-meditation session.

In order to test the relation between the subliminal scores and the ESP scores, the results of both the sessions were pooled and a Pearson product moment correlation between subsensory perceptual and ESP scores was computed. This gave a correlation coefficient of -. 395; $P < .05$, which suggested that the lower one's subsensory perceptual scores, the higher will be one's ESP scores. The results were then analyzed to test the second and third null hypotheses by comparing scores obtained in post- and pre-meditation sessions. The 28 subjects obtained 291 hits on subliminal targets in the pre-meditation session, an average of 10. 39 hits per 30 trials. They scored 98 ESP hits, an average of 3. 50 hits per 30 trials (chance probability was 3). In the post-meditation testing, the same subjects scored 323 hits on subliminal targets and 89 hits on ESP targets, an average of 11. 53 and 3. 18 hits per 30 trials respectively. Thus we find an increase in the subsensory scores of the subjects and a decrease in their ESP scores after they had meditated. The differences in the scores, however, were not statistically significant; thus null hypotheses (2) and (3) cannot be rejected.

SSP AND ESP WITH A WEIGHT DISCRIMINATION TEST

K. Ramakrishna Rao†, K. Gowri Sundari, Y. Jaganmohan Rao, and R. Nageswara Rao (Andhra University)

It was not clear from the experiment reported in the previous paper [pp. 77-9] that the results indicated any intrinsic relationship between subsensory perception (SSP) and psi, because of the possibility that psi might have been operating even in the SSP trials. If that were the case, the observed negative correlation might be simply a case of the psi differential effect. The following experiment was designed to test such a possibility. In the previous experiment, the sensory signals given to the subjects were sufficiently strong to enable them to identify the correct picture approximately 30 percent of the time where 10 percent would be expected by chance. It was thought that if we rendered the

sensory signals too weak to elicit veridical subliminal responses, so that the probability of success on SSP targets was no more than the probability of success on ESP targets, it would be interesting to see whether we could still obtain a significant negative correlation between SSP and ESP scores. While such a result might not conclusively answer the question whether psi and subliminal perception are so related, it would nevertheless raise a number of interesting questions and open up some new areas for research.

The present study extended the SSP work from the visual identification of projected pictures to the cutaneous discrimination of weights, by asking the subject to judge which of two weights was heavier when the difference between them was too small to permit veridical guessing at the sensory level. Twenty subjects, 10 males and 10 females, were administered a weight discrimination SSP test and also a GESP test. While the SSP subject was engaged in his task of guessing the heavier of the two weights, he was actually serving as ESP agent to another subject (the ESP percipient) in a different room who was asked to guess which of the weights in the hands of the agent he felt was the heavier. The ESP responses of the percipient were matched against the actual weights (clairvoyant condition) and also against the agent's responses; i. e., the judgment of the SSP subject as to which of the two weights was heavier (telepathic condition). Each subject did 30 trials in the ESP test and 30 trials in the SSP test.

The results of the experiment were analyzed to test the following two hypotheses: (1) There will be a significant negative correlation between subjects' ESP scores and SSP scores, where SSP scores are the scores obtained by the subjects on the weight discrimination test and ESP scores are the number of hits obtained when the subjects' responses are scored against (a) the target weights (clairvoyant condition) and (b) the agents' responses (telepathic condition). (2) There will be no significant sex differences in the ESP scores of the subjects.

Pearson product moment correlations between the subjects' SSP and ESP scores were worked out separately for clairvoyant and telepathic scores. The coefficient of correlation between SSP scores and clairvoyant scores was .13 (df = 18, nonsignificant). The correlation between SSP scores and telepathic scores was -.40, which was significant at the .05 level (one-tailed). The latter result thus confirmed the

finding of the previous study that the SSP and ESP scores were inversely related. It should be pointed out, however, that the subjects' average rate of scoring on subsensory targets was only 14.975. This score is close to chance expectation of 15. Consequently, there was no evidence that the subjects responded to subsensory stimuli. Therefore, it is not unlikely that the responses of the subjects to subsensory targets were also extrasensory. Such an assumption fits well with the differential effect hypothesis.

To test whether any sex differences existed in the ESP scoring of the male and female subjects working with male and female agents, an analysis of variance was carried out. The F ratio relating to the difference among agents was 6.6 ($df = 1, 36$; $P < .05$). This result suggests that with male agents the subjects tended to score positively and with female agents they tended to score negatively. The difference in the rate of scoring when the agent was male and when the agent was female was statistically significant.

MENTAL IMAGERY AND ESP AS MEASURED BY RAO'S SUBSENSORY PSI TEST (SPT)

R. Vidhya Rao, P. V. Krishna Rao, and K. Ramakrishna Rao† (Andhra University)

Recently K. R. R. designed a test called the Subsensory Psi Test (SPT) which provides for simultaneous testing of subsensory (subliminal) perception and nonintentional extrasensory perception in a forced-choice response situation. (For a description of the test see pp. 77-9.) The basic assumption underlying this technique was that testing simultaneously for subsensory perception and extrasensory perception would be more conducive to psi manifestation than testing for ESP alone, in that such a procedure would be unlikely to arouse natural defenses against psi. And, if mental imagery has any role to play in psi occurrence, it was hoped that the SPT might tap it. Therefore, the present study made use of the SPT in an attempt to examine the relationship between ESP and vividness of mental imagery, as measured by Sheehan's shortened form of the Betts QMI Scale.

Twenty postgraduate students at Andhra University, 13 males and seven females ranging in age from 20 to 26, par-

ticipated in this study. Some of them had taken part a few days earlier in another experiment which involved testing for extrasensory perception by the SPT. The subjects were tested individually following the administration of the QMI. They were given record sheets with the following instructions: "The slides being shown to you contain pictures of the following 10 objects. Please write what pictures you see in the response columns serially. Do not skip any column. Make a guess if you cannot see clearly. A picture can be of any of the following objects: a natural scene, a flower, a bird, a man, a watch, a cabinet, a child, a book, an airplane, or a building." After the subject had read the instructions and indicated that he understood them, the slides were projected on a screen in a semi-dark room. There were 60 slides, 30 of which were actual pictures (subliminal perception) and 30 of which were blank (ESP). The subjects recorded their responses on the record sheets immediately after each projection. All 60 slides were presented in a single session according to the order determined in advance. This order was different from subject to subject.

Experimenter I (R. V. R.), who presented the slides to the subjects, had no knowledge of the ESP target pictures corresponding to the blank slides. Experimenter II (P. V. K. R.), who prepared the ESP target pictures for the blank slides, kept that information entirely to himself and did not involve himself in any phase of the testing. After all the subjects were tested, their responses were scored for ESP ability. If the response made by a subject on the presentation of a blank slide was the same as that of the picture designated as target to that blank, it was considered a hit. Responses to the picture slides (subliminal perception) were not considered in this study, since some of the subjects had had previous experience with the test. In order to obtain an overall imagery score, the QMI was scored by pooling the ratings on all the sub-modalities.

The subjects scored a total of 76 ESP hits on the SPT. This gave an average of 3. 8 hits per run of 30 trials where 3 were expected by chance (CR = 2. 18; $P < .05$, two-tailed). This suggests the occurrence of ESP in this test. Contrary to expectation, the ESP scores on the SPT and the scores on the QMI did not correlate significantly ($r = -.129$). To find out whether vivid imagers and weak imagers differed significantly in their ESP ability, the subjects were divided into two groups, taking the group mean as the cutting point. The 12 vivid imagers obtained a deviation of

+12, which gave a CR of 2.109 ($P < .05$, two tailed). The eight weak imagers also scored positively (deviation = +4; CR = 0.86), but not to a significant extent. The difference between the means of vivid and weak imagers on the ESP task gave an insignificant t of 0.99. However, it was interesting to note that both vivid and weak imagers scored positively on the test, and vivid imagers obtained more hits than weak imagers.

The results of the present investigation do not settle the question whether psi and mental imagery are related. Nevertheless, they throw some light on our understanding of the psi mediation process. K. R. R.'s test seems to be a promising technique for testing ESP. The role of imagery in mediating the information acquired beyond the conscious level appears to be more intricate. Since all the subjects scored positively, we may infer that performance on an ESP task may not be correlated with self-reported vividness of conscious mental imagery, and that imagery as we conceive it in the mediation of psi may be different from the consciously recalled or imagined experience tested by the QMI.

RESEARCH BRIEFS VII*

PRECOGNITIVE REMOTE VIEWING IN THE CHICAGO AREA: A REPLICATION OF THE STANFORD EXPERIMENT

J. P. Bisaha† and B. J. Dunne (Mundelein College)

An investigation into the ability of untrained individuals to perceive and describe remote geographical sites before the target locations have been determined was undertaken as a replication of Puthoff and Targ's work at the Stanford Research Institute, exploring information transmission under conditions of spatial and temporal separation between percipient and communicator. Two volunteer female college students served as subjects in eight separate experimental trials (two trials for one subject and six for the other). In each instance the subject was asked to describe an unknown remote location where the experimenter was to be 35 minutes later. This description was generated during a 15-minute period beginning 20 minutes before the target was selected and 35 minutes before the outbound experimenter was to arrive at the target location.

Before each trial began the subject was informed of the nature of the experiment and the experimental protocol. She was instructed to make herself comfortable, relax completely, and try to let her mind become blank. At that point she was to try to visualize the location where the experimenter would be, to describe the images which came to mind aloud into a tape recorder, and to make sketches of these images if possible. The subject was advised not to attempt to define or identify specifically what she saw, but to stick to general impressions and descriptions. She was allowed sufficient time to relax and prepare herself before each trial began. During the trial period the subject remained in a closed room with an observer who had no knowledge of the target location.

*Chairperson: I. L. Child, Yale University.

At the same time that the subject began her description, the outbound experimenter left Mundelein College with a stack of 10 envelopes, each containing a card designating a particular site in the Chicago or suburban area lying within a radius of a half-hour drive from Mundelein. These 10 envelopes were randomly selected by another experimenter from a target pool of over 100 sites which had been compiled by a person who had no other affiliation with the experiment and who was the only person familiar with the contents of the envelopes. The outbound experimenter drove continuously for 20 minutes, or until five minutes after the subject had completed her narrative. A number from 1 to 10 was selected at random from an enclosed container holding 10 numbered sheets of paper, all folded identically. Still in motion, the experimenter counted down to that number in the pile of envelopes and proceeded to the location indicated on the enclosed card of the designated envelope so as to arrive there 15 minutes later, or 35 minutes after the subject began her recording. She remained at the target site for 15 minutes, photographed the location, and made notes as to her impressions of the site. (The photographs were taken to insure that the judges would be able to view the site later as it appeared on the day of the trial. Since most of the trials were performed in early spring, a week or two could make a significant difference in the appearance of the location due to climatic conditions and developing foliage.) She then returned to the laboratory. Typed, unedited transcripts were made of the subjects' responses and attached to any associated drawings generated by the subject.

After the eight experimental trials had been carried out, three persons not otherwise connected with the experiment were asked to act as judges. The transcripts of the subjects' narratives, together with the drawings, were presented to the judges in random order, identified only by an alphabetical letter from A to H written in the upper right-hand corner. Judges were also given, in random order, the photographs and notes taken by the experimenter while she had been at the target site. Each judge was asked to blind rank order the transcripts with the locations on a scale of 1 to 8, 1 being the best possible match and 8 being the worst. In this way the lower the values assigned, the better the match; 8 was the lowest possible rank sum and 64 the highest. The judges were also taken to visit the target locations, but were asked to rank order only in terms of the photographs.

The statistical analysis of these experimental trials

was based on the rankings given by the judges, in accordance with Morris's method for evaluating preferentially matched free-response material. The sum of the ranks assigned by the judges was 12 in two cases and 15 in one. Two judges each had five direct hits out of eight; one judge had four direct hits. The highest rank given any transcript was three. The results were statistically significant at $P < 10^{-4}$ in two cases and $P < .0005$ in the third. They would appear to provide additional evidence substantiating the existence of perceptual and communication channels which lie beyond the senses as they are currently defined.

BRAIN HEMISPHERE DIFFERENCES IN PSI-INFLUENCED REACTION TIME

Richard S. Broughton (University of Edinburgh)

I reported experiments at the 1975 Parapsychological Association convention suggesting that some psi abilities, or the brain activities which regulate them, are subject to brain lateralization effects [RIP 1975, pp. 98-102]. In this work an ESP-influenced tactual discrimination task, which took advantage of the fingertips' rather exclusive contralateral somatosensory representation in the brain, was combined with a competing task known to load on the left hemisphere. The results suggested that there was greater success in the ESP task when predominantly controlled by the right hemisphere, with the left hemisphere engaged in a competing task. These findings were in accordance with the body of material suggesting that some forms of ESP may not be equally represented in both sides of the brain.

A reasonably successful means of obtaining measures of laterality for various cognitive functions is to measure the subject's reaction time (RT). Typically, such studies employ unilateral presentation of stimuli and measure response time for recognition, decision, etc. A general finding emerging from such studies is that responses which require interhemispheric transfer of information or originate in the so-called "non-competent" hemisphere take longer. Conflicting theories of the mechanisms involved abound, but the usefulness of the technique is established.

Stanford and his colleagues have demonstrated the

efficacy of verbal RT as a dependent variable in ESP experiments. This evidence that RT could be psi-influenced indicated that it may be possible to adapt a simple RT task with an ESP component to a lateralization study. While such an experiment would differ from typical studies in that the ESP task could not be "directed" to only one hemisphere, differences between hands for the ESP component would permit inferences regarding lateralization to be drawn.

The RT task in this experiment was a button-press response to the onset of a 1 kHz tone presented bilaterally to the subject via earphones. The ESP component of the task involved an agent, a friend of the subject, who on about half the trials (randomly selected) would receive the tone 250 milliseconds before the onset of the subject's tone. These were "advance" condition trials, and for the remaining "control" trials the agent did not receive the tone until after the subject had responded. The subject and agent were not told this, but merely that the experimenter was looking for a relationship between their scores and that they should respond as quickly as possible. The experiment was controlled by computer and consisted of 160 trials per subject in eight blocks of 20 each (four blocks with each hand).

The subject was required to make responses with one or the other hand (order randomly determined by blocks) so that the response was more closely associated with one hemisphere, although it would not be possible to assume that the response originated in that hemisphere. The effect of the advance condition could not be predicted since it could either serve as an advance warning, thus priming the response, or could conflict with the subject's awareness of no tone and inhibit the response. It was expected that any effects should be consistent across subjects. One experiment was run as described in the expectation that it might elicit hemisphere differences without the need for a competing task. A second experiment included a competing task in which the subject was required to read extracts from law reports. Two groups of twenty right-handed subjects were used in the two experiments. Two-way analyses of variance (advance-control vs. right-left hand) with repeated measures were done using the means per condition for each subject. Since it was thought that an ESP effect in this case might be an occasional large influence rather than a consistent small one, it was decided to use means rather than medians as the measure. The distribution of means was checked for normality.

In Experiment I the overall means were very similar, with no significant differences. In Experiment II, identical except for the added reading task, there was no difference between the advance and control conditions, nor between hands; but there was a significant interaction ($F = 4.53$, $df = 1, 57$; $P = .035$). The means indicate that this resulted from the advance condition on the left hand causing shorter mean RTs than the control condition, while the opposite was true for the right hand. A correlated t test yielded a significant difference between control and advance conditions for the right hand ($t = 2.46$, $df = 19$; $P < .025$, two-tailed).

One post hoc finding emerged after recent indications of sex differences in laterality effects suggested that the data for males and females should be examined separately. This was done informally by subjecting the male and female data from Experiment II to separate analyses of variance. The results showed the presence of the interaction for the males ($F = 4.27$, $df = 1, 24$; $P = .047$), but not at all for the females ($F = 0.43$, $df = 1, 30$).

The failure of the first experiment corroborates the findings of my earlier series and supports suggestions that it may be necessary to disrupt some activity of the left hemisphere for psi to show itself. The interaction found in Experiment II, while not exactly expected, is consistent with previous findings. One possible interpretation is that the advance condition has in the right hemisphere the effect of priming the response while in the left hemisphere, where there is a suggestion of psi inhibition, these responses were slightly delayed. Such an interpretation is tenuous at best, but may guide future work.

The suggestion of sex differences in the data is in agreement with that found in previous experiments which seemed to indicate that the disruptive task was successful only for males. Controversy surrounds the interpretation of sex differences in laterality studies, and it is premature to attempt to integrate these findings with other work in this area; but it is sufficient to note that measures of ESP laterality in such experiments may not be equally strong for both sexes.

LATERALITY EFFECTS IN ESP PROCESSING

Michaeleen Maher and Gertrude R. Schmeidler† (City College, City University of New York)

Considerable evidence indicates that in right-handed humans, and especially in adult males with no familial history of sinistrality, the brain's left hemisphere is better at processing analytic verbal material (and exerts contralateral control of the right hand), while the right hemisphere is better at processing concrete or holistic visuospatial material (and exerts control over the left hand). This raises a question. In ESP processing, if material is clearly verbal, or clearly concrete, will responses be more accurate when both hemispheres are free to process it? Or does the less effective hemisphere contribute noise or inhibition, so that responses are more accurate if that hemisphere is kept busy with a suitable distracting task? We present data which indicate that the second alternative is correct. Our subjects were 24 right-handed adults, five males and 19 females.

We used a three-way cross-cutting design in which each subject did three tasks, each involving two kinds of ESP targets, and used each hand alternately within each condition. The three tasks: Each subject made 20 ESP (clairvoyance) choices while trying to solve syllogisms (a task expected to engage the left hemisphere); 20 choices while recognizing and tracing a visual pattern embedded in another figure (a task expected to engage the right hemisphere); and 10 ESP choices while free (no distracting task). A Greek square design counterbalanced the six possible task orders, with four subjects in each group.

The two kinds of ESP targets: Both concrete objects and words were employed as targets. The concrete pool contained five four-leaf clovers, each encased in a small transparent plastic cube, and 20 clear cubes (containing only small pieces of plastic cut to fall within the weight range of the clovers). In the verbal pool, all cubes contained a piece of paper, five with the word CLOVER, and 20 with the word WRONG!. For each task, half of each subject's trials were with concrete targets, half with words. The 25 cubes of the designated pool were put into a camera bag which was sealed and shaken. The bag permitted access to each hand through baffles, but did not permit light to enter. Subjects were instructed to pick out the clovers. After each trial,

the selected cube was returned to the bag, which was sealed and reshaken. For each group of four subjects, two made concrete object choices first, two made verbal choices.

The two hands: For the syllogism and visual tasks, one subject in each subgroup made five choices with the left hand followed by five with the right; the other subject used the opposite hand order. In the free condition, subjects put both hands in the bag and used "whichever hand found a clover first."

ESP scores differed from chance level in only one condition: during pattern recognition, the 120 choices of words made by the right hand gave significantly high scores (36 hits; CR = 2.74; $P < .007$, two-tailed). Males contributed more to this effect (35 choices, 14 hits) than females. We had expected, both from theory and from previous research, that subjects would make high ESP scores in their left-hand choices of concrete objects during the syllogism task. The reason they did not may be that strong negative affect was aroused in many by the syllogism task. Typical remarks were "I hate these!" and "Oh boy, I can see these are going to drive me crazy." We suggest that negative affect may have spread to the ESP choices associated with syllogisms, lowering the level of ESP scoring during the verbal task.

Our significant finding is of special interest because it is the mirror image of what Broughton reported at the 1975 Parapsychological Association convention [RIP 1975, pp. 98-102]. He found high ESP scores for objects chosen by the left hand during a verbal task; we found high ESP scores for words chosen by the right hand during a visual task. We suspect our failure to replicate Broughton's finding was due to our subjects' strong dislike of the particular verbal task we used. Both our data and Broughton's support the hypothesis that (for cooperative, right-handed adults) when the ESP target is best processed by one hemisphere, ESP scores are higher if the hand controlled by that hemisphere makes the ESP choices while the other hemisphere is agreeably occupied with a task that suitably engages it.

A SIMPLIFIED EXPERIMENT IN DREAM TELEPATHY

Irvin L. Child† (Yale University), H. Kanthamani (Institute for Parapsychology, FRNM), and Valeriana M. Sweeney (Crossville, Tenn.)

In the summer of 1975, all three of us were resident in Durham, N. C. At the suggestion of H. K., we planned an experiment to take advantage of what V. M. S. felt to be one form of her spontaneous psychic abilities. The experiment was modeled after the Maimonides dream telepathy experiments of Krippner and Ullman, but took a greatly simplified form capable of application anywhere without the special equipment and personnel of a dream laboratory. The two experiments we carried out were very small in scale, but gave such impressive results that they are being reported despite the failure of the later replication attempts which had to be performed at great distance.

V. M. S. acted as percipient, I. L. C. as agent, and H. K. as coordinator. All three took part as judges of correspondence between targets and percipient's reports. The targets were colored postcards, mostly of paintings. On each of the eight nights of Experiment I, I. L. C. used a random number table to select a target from a large supply, and concentrated on it for about 10 minutes beginning at 10:45 p. m. in his apartment. V. M. S., in her apartment about half a mile away, was preparing for sleep. She tried to be open to impressions during that period, and also to remember her dreams that night so as to record both impressions and dreams in the morning. After the eight nights (not consecutive) of the experiment, each participant independently ranked the eight targets in relation to each night's protocol, for the likelihood of its having been the target. A rank of 1 indicated the best match.

For each night of Experiment II, a pool of eight potential targets was drawn at random from a set of such pools, and the target was drawn at random from the pool selected. V. M. S. and H. K. were then able the next morning to rank the eight pictures in a duplicate of the pool against the protocol. I. L. C., however, necessarily had to make his rankings in the same way as in Experiment I. This experiment was continued for seven nonconsecutive nights. On two of them V. M. S. was unable to write down either dreams or pre-sleep impressions, although she did attempt rankings;

in advance of looking at results we decided to disregard those two nights altogether (except that their target pictures remained as part of the set of pictures ranked by I. L. C. against each protocol, since he had no knowledge of which nights were the ones lacking a protocol).

The summed rankings of the three judges differed markedly from mean chance expectancy, in the direction of correspondence between protocol and target (i. e., a low sum of ranks). In Experiment I, the summed rankings averaged 10. 37, with chance expectancy of 13. 5 ($t = 1.87$, $df = 7$). In Experiment II, the summed rankings averaged 8. 85, with chance expectancy of 13. 0 ($t = 2.69$, $df = 4$). We have pooled these results by the unweighted Guttman method. The outcome is a z of 2. 55 ($P = .0108$, two-tailed). The generalization justified by this significant finding does not of course pertain to a population of subjects. We can only say that the apparent success of our subject is not likely to have resulted from random variation; it was probably a genuine success.

For an outcome of this level of significance to emerge from so small an experiment, the apparent effect had to be of considerable magnitude, as of course the mean rankings show it to have been. This would not be surprising, in view of the Maimonides findings, had we been working with full accounts of impressions and dreams. In fact, however, the protocols were not at all comparable in richness with those obtained in dream laboratories. The one protocol for which all three judges agree in ranking the true target in first place said only, "Around 11 p. m., I had an image with a few lines which delineated vaguely a crowd. No dreams." On the other nights where judging was almost as successful, the reports were much fuller--over half a page of dreams--but still sparse by dream laboratory standards.

These results encourage us to suggest that the general method developed by Ullman and Krippner in their Maimonides studies may be useful even in a modified form which can be employed widely at little or no expense except of time and methodological care. Like the Ganzfeld procedure developed by Honorton, this adaptation might permit systematic experimentation which could otherwise not be done because of the great expense required.

In subsequent months we carried out similar experiments with the agent in Connecticut and the percipient in either Tennessee or Italy. These experiments showed little

deviation from chance. While the evidence of stability in our Durham results stands, any evidence about whether they will be paralleled in work with other subjects awaits actual trial.

POSITIVE GESP SCORES WITH A SUBJECT IN A STATE OF MEDITATION

Sandra Baker, Alison Jacob, and H. H. J. Keil† (University of Tasmania)

In 1975 S. B. and A. J. participated as third-year psychology students in an experimental course for which H. H. J. K. was responsible. S. B. and A. J. were asked to find a subject and to carry out a GESP test procedure which was largely based on previous experiments with the gifted subject Pavel Stepanek. The procedure allowed for the possibility that the subject might score significantly in terms of direct GESP hits, in terms of a GESP focusing effect, and in terms of a focusing effect based on sensory cues.

S. B. 's brother, David Baker (D. B.), agreed to participate as the experimental subject. The first experiment was regarded as exploratory only. D. B. scored significantly on direct ESP hits but did not show any clear evidence of either sensory or extrasensory focusing. Subsequently the procedure was simplified. Direct ESP hits were designated as the expected scores, indicating evidence for GESP, and focusing was discarded as a worthwhile line of investigation. D. B. was asked to call random sequences of four symbols displayed on target cards and, later, five photos depicting different members of his family and friends. One of the experimenters concentrated on the target cards and later on the photos, thus allowing for the possibility of telepathy as well as clairvoyance. The first test consisted of 400 trials and produced highly significant results. However, the two experimenters had placed themselves in such positions that unintentional sensory cues might have been provided to the subject. It is most unlikely that this in fact happened. Nevertheless, the results of this series were not included in the official evaluation of the evidence for ESP.

For the experiments reported here the following procedure was adopted. For each trial Experimenter 1 randomized the targets according to a random number table and

concentrated on the chosen target. Experimenter 2 recorded the subject's response and indicated with a tapping sound that D. B. had made his response. If the response was made immediately Experimenter 2 waited for three seconds to give Experimenter 1 time to complete the randomization for the next trial. A fixed time interval was found unsatisfactory, as D. B. varied his response times considerably, responding occasionally almost immediately but waiting sometimes for up to 10 seconds. Experimenter 2 was situated in the same room as D. B., while Experimenter 1 was in an adjacent room. Experimenter 1 remained silent and randomized the targets on a table covered with a blanket. D. B. 's responses were generally inaudible to Experimenter 1. Experimenter 2 sat near the door leading into the adjacent room. Experimenter 2 had no means of obtaining any sensory information about the target sequence.

In the first three experimental series which we regarded as adequate in terms of controls, four targets (geometric configurations) were used. The combined deviation for the 800 trials in these series was +76 (CR = 6. 2), which was highly significant. The following four series used five targets, photographs of friends and relatives. The combined deviation for the 700 trials in these series was +44 (CR = 4. 16), which was again highly significant. During most of the series S. B. acted as Experimenter 1 and A. J. as Experimenter 2. In some series S. B. and A. J. exchanged roles every 50 trials. D. B. did not score significantly when A. J. acted as Experimenter 1. There was nevertheless a suggestion of ESP in terms of a decline effect which seemed to occur for both S. B. and A. J. There was also a suggestion that D. B. changed his responses to particular targets when S. B. and J. A. exchanged roles. When H. H. J. K. participated as a third experimenter during some of the series D. B. continued to score at a similar level of significance.

D. B. was 17 when the tests were started, and he recently turned 18. A part-time student, he is interested in spiritualism, religious groups and meditation. He had a strong belief that he would succeed as an ESP subject. His meditation practice is based on the Ananda Marga group. At the time the experiments were started he was not any more an active member of this group, but had continued to practice meditation. D. B. estimated that during half the ESP trials he was, on account of meditation, in an altered state of consciousness. During the test he remained largely motionless with his eyes closed, and appeared to be only semi-

conscious of his surroundings. His responses were often only just audible. For a number of reasons, tests with D. B. could not be carried out frequently. Nevertheless, it is hoped that further tests will be continued. In particular, it is hoped to clarify the importance of Experimenter 1 in this particular test situation.

EFFECTS OF MEDITATION AND FEEDBACK ON PSYCHOKINETIC PERFORMANCE: A PILOT STUDY WITH AN INSTRUCTOR OF TRANSCENDENTAL MEDITATION

Charles Honorton (Maimonides Medical Center)*

I conducted a pilot microdynamic PK study with Mr. R. M., a long-time Transcendental Meditation (TM) practitioner. I used a noise-driven binary random event generator called PSIFI, and selected an auto-alternating mode of operation which automatically reversed the target definition every other trial, such that any bias in the generator would cancel out. The generator was set at two bits (trials) per second. Event-by-event feedback (an auditory tone) was provided by headphones.

R. M. was prepared for monopolar EEG recording (left occiput to right earlobe) and led to a radio frequency-shielded, sound-attenuated experimental room which was separated from PSIFI by two steel walls and a distance of approximately 12 feet. The instructions by headphones described the goals, procedures, and equipment for the experiment. They also included an experiential exercise designed to demonstrate the difference between active and passive volition. R. M. was asked to function in the passive mode during his PK task. The experiment was divided into three phases.

Phase I involved premeditation PK feedback. R. M. was given an initial PK practice run of 4.2 minutes duration (500 binary trials). This was followed by 10 experimental runs, each of 50 seconds duration (100 trials per run). In order to assess the effects of feedback on PK scoring direction, the feedback contingency was reversed between the fifth and sixth runs, such that the feedback signal was associated

*Presented by Rochelle Winnett.

with hits for the first half of the runs (high-aim) and with misses for the second half of the runs (low-aim). R. M. was blind as to the feedback contingency, and was operating throughout with instructions to keep the signal on. Premeditation results were in the feedback contingent direction, but not to a significant degree. The five high-aim runs gave a mean of 51.6, while the five low-aim runs gave a mean of 50.2, both insignificantly different from chance expectation of 50.

Phase II involved a 25-minute meditation period without PK feedback. During meditation, I gated nonfeedback PK data in relation to R. M.'s EEG activity: PK data occurring when R. M. produced 4-13 Hz theta-alpha EEG activity were automatically segregated from PK data generated while R. M.'s EEG was outside this range. Ten gated runs of 100 trials each were taken. The PK data generated during theta-alpha periods approached significance, with a mean per run of 48.8 ($t = 2.17$, $df = 9$; $P = .058$, two-tailed). The PK data generated while R. M.'s EEG was outside this frequency range were exactly at chance with a mean per run of 50.0. An interesting post hoc finding was that R. M.'s PK run score variability was significantly lower during theta-alpha periods than when his EEG was outside this range ($F = 11.14$, $df = 10$; $P = .00036$).

Phase III involved a postmeditation PK feedback task with procedures identical to those employed in Phase I. In this period, R. M. had overall significant high-aim PK results with a mean per run of 52.4 ($t = 3.54$, $df = 4$; $P = .024$, two-tailed). R. M.'s low-aim performance was nonsignificantly below chance with a mean of 49.2. The difference in PK scoring between high- and low-aim was significant ($t = 5.49$, $df = 4$; $P = .0054$, two-tailed).

To summarize, R. M. showed chance results prior to meditation and significant feedback-contingent PK results following meditation. Since he was blind to the feedback contingencies, this finding is of special interest. It shows more clearly than previous work that PK can be guided by directional feedback. At the same time, however, the nearly significant PK effect obtained during meditation, without feedback, suggests that feedback is not a necessary condition for PK, as has been suggested recently by Schmidt.

The association of nearly significant PK scoring with theta-alpha EEG frequencies and chance scoring with faster EEG frequencies is suggestive and warrants further investiga-

tion. In particular, the post hoc finding of significant differences in PK variability associated with EEG fluctuations is provocative and should be followed up.

EFFECTS OF MEDITATION AND FEEDBACK ON PSYCHOKINETIC PERFORMANCE: RESULTS WITH PRACTITIONERS OF AJAPA YOGA

Rochelle Winnett† and Charles Honorton (Maimonides Medical Center)

Following our pilot work on PK and meditation with an instructor of Transcendental Meditation (TM) (see previous paper), we formalized our procedures in a study designed to compare PK performance in meditators and nonmeditators. While we are still collecting data for this study, we have completed work with 10 meditators, all practitioners of Ajapa yoga. Ajapa is a yoga breathing technique, practiced throughout the day without imposed conditions of time and place. It is based on the belief that natural breathing is an expression of universal forces of attraction and repulsion which are balanced by a slight alteration of the breath. Ajapa meditation combines this alteration in breathing with concentration on a mantra.

The procedure was similar to that described for our pilot study with the TM instructor, with the following changes: the subjects were practitioners of Ajapa technique rather than TM; most of them were relatively recent initiates rather than long-time practitioners of meditation; and because of equipment problems, we were not able to examine PK effects in relation to EEG patterns during meditation. Subjects were given instructions identical to those used in our pilot study. The instructions described the goals, procedures, and equipment for the experiment, and included an experiential exercise to demonstrate the difference between active and passive volition. The subjects were asked to function in the passive mode during their PK task.

The session was divided into three phases. Phase I involved premeditation PK feedback. After an initial practice run of 4.2 minutes (500 trials), the subject was given 10 experimental runs of 50 seconds each (100 trials per run). As in our earlier work, unknown to the subject, the feedback

contingency was reversed between the fifth and sixth runs, such that the feedback contingency was associated with hits (high-aim) for half the runs and with misses (low-aim) for half the runs. The premeditation results were in the feedback-contingent direction to a significant degree. The high-aim runs gave a mean per run of 50.62, insignificantly different from chance expectation of 50. The low-aim runs gave a mean per run of 47.78 ($t = 4.0$, $df = 9$; $P < .005$, two-tailed). The difference between the two feedback contingencies was significant ($t = 4.63$, $df = 9$; $P < .005$, two-tailed).

During Phase II, since we were unable to gate PK data in relation to subjects' EEG activity, we took 5000 nonfeedback trials while the subject meditated. These data were nonsignificant, giving a mean per run of 49.86. The Phase III postmeditation task produced uniformly nonsignificant results. The high-aim mean was 50.52, while the low-aim mean was 50.9. The difference between feedback contingencies was in the wrong direction to a nonsignificant degree.

Thus, the Ajapa practitioners showed a significant decrement in PK performance following meditation for the low-aim contingency (pre- to postmeditation $t = 3.66$, $df = 9$; $P = .0053$, two-tailed). That is, they obtained higher scores following meditation, whereas they were aiming for low scores. This result was contrary to our expectation based on the pilot work. It is not clear whether these differences are due to differences in meditation technique, length of practice, motivation, or other factors.

RESEARCH BRIEFS VIII*

AN ATTEMPT TO DEVELOP DISGUISED ESP TESTS

Martin Johnson† and Christa Lübke (University of Utrecht, the Netherlands)

Does ESP function in everyday life without being noticed, and how could such a hypothesis be tested? A couple of years ago M. J. carried out three experiments in which university students participated. They were given a written exam in psychology. The exam consisted of eight questions, each followed by a blank space large enough for a short answer. The test sheets were attached to the front and back of a large, sealed envelope. Inside, underneath the appropriate spaces, attached to each side of a piece of cardboard of the same size as the test sheets, were copies of the front and back sheets respectively. On the sheets within, unknown and invisible to the subjects, were answers to four of the eight questions. For each subject a random procedure selected which four of the eight questions would have a target answer attached. In Studies 1 and 2, the information related to the target answers was correct, whereas in Study 3 incorrect target answers were used. In all three studies the unseen answers seemed to influence the quality of the subjects' own answers; in Studies 1 and 3 this occurred at a statistically significant level.

Recently we designed and carried out a pilot study in which a test designated as the "Hidden Dutch Cities and Villages" was used. The same rationale was used as in the case of the written academic exam. A subject was given a test sheet attached to a sealed envelope. On the sheet there was a square comprising 567 letters, arranged in a pattern of 27 by 21 lines. The letters were to a certain extent randomly arranged. At the bottom of the sheet there was a list

*Chairperson: K. Ramakrishna Rao, Andhra University.

of 18 Dutch cities and villages. The names of the cities and villages were embedded within the square of letters. In this crossword-like pattern, six of the geographical names were spaced horizontally, six vertically, and six diagonally. The test was presented as both a cognitive and a perceptual one, and each subject was given 10 minutes to try and find as many of the names as possible. Inside the envelope, unknown and invisible to the subject, was a copy of the sheet. For half the subjects, the geographical names on the target sheet were encircled and had locations identical to those on the test sheet; for the other half the names on the target sheet were encircled but displaced in relation to those on the test sheet. Fifty-four psychology students took part in this study.

The scoring level for test sets having target sheets with the names in identical locations to those appearing on the front cover was compared with that for the test sets having target sheets with the displaced geographical names. No overall significant effect appeared; the scoring levels on the test sets were not significantly different. Neither did a breakdown of data into categories such as sex and scoring on the horizontally, vertically, and diagonally spaced targets yield significant differences. However, we are not too discouraged by this negative outcome. A new disguised ESP test in which hidden figures are used is on the way, as is a reaction time test which is really a disguised test of ESP. By and large, we are working hard to develop this area.

EFFECT OF MOTHER-CHILD TELEPATHY ON A VISUAL-PERCEPTION TASK

Athena Anne Drewes† and Sally Ann Drucker (Maimonides Medical Center)

This study sought to investigate the following hypotheses: that telepathy between mother and child improves the child's performance in time and/or accuracy on a complex perceptual discrimination task, the Children's Embedded Figures Test (CEFT); that the child's intelligence affects performance on such ESP and non-ESP tasks; and that the mother's attitude toward parapsychology affects the performance of the child.

Subjects were 20 children between the ages of five

and nine, selected from a previous study on the basis of past Peabody Picture Vocabulary Test performance. The 20 subjects were divided evenly into High IQ and Low IQ groups based on the Peabody scores. They were not matched for sex or age. There was a significant IQ difference between IQ groups. One experimenter worked with the child in a separate room within hearing distance of the mother and second experimenter. The subject was to find within each of the first 11 cards a "tent" shape embedded in a picture and, after a short practice break, a "house" shape within the last 13 cards of the CEFT. The subject was instructed to find and outline with his or her finger the hidden shape while at the same time the mother would be looking at the same pictures and trying to relay the location to the subject. Each card was timed up to two minutes maximum. The second experimenter worked with the mother. The mother selected the target packet to be used from the packets the experimenter held behind her back. Each target packet contained 12 cards with the correct embedded figure shaded in (ESP) and 12 blank sheets (non-ESP). The mother was instructed to concentrate on every other shaded-in target shape and location, and to relay this to the child who was simultaneously trying to find it. The mother was instructed to relax and clear her mind on the blank sheets. At the end of the session the target packet was recorded on the subject's sheet; the packet chosen was unknown to the subject's experimenter until the end of the session. The experimenters alternated every other experiment between working with the child and with the mother, in order to control for experimenter effect. Each mother was asked to fill out a questionnaire rating her attitudes and to record any anecdotal ESP phenomena.

The children's performance was evaluated by two-way analyses of variance with respect to both time needed and accuracy in identifying the hidden figure. The High IQ children completed the task faster on both ESP and non-ESP targets than the Low IQ children, but the difference was not significant at the .05 level. There was no significant effect of ESP vs. non-ESP targets, nor was there a significant interaction between intelligence level and ESP vs. non-ESP targets. IQ, target type, and interaction between the two were also nonsignificant for mean accuracy. Each individual subject's performance was analyzed through a t test; no significant scores were found. No significant relationship was found between attitudes of the mother and child's performance. No experimenter effect was found.

Possible explanations for the overall nonsignificant results may be the low sample size and high variability. Because time scores fluctuated between one and 120 seconds within subjects, this high variability may have neutralized any possibility of significant results with respect to time. Kreitler and Kreitler found in their subliminal study (Journal of Parapsychology, 1972, 1-45) that a significant cumulative effect emerged only on a group level. Perhaps with replication using a far larger sample or a test measure with less variability such a cumulative effect may be seen and a better control of variability made. Kreitler and Kreitler hypothesized that if there are two separate non-mingling channels of information (ESP and perception) coming to an individual, attention would focus on the information flowing in through regular perceptual channels, leaving the ESP channel unattended. As long as ESP messages are in line with information transmitted through usual perceptual means, there is no effect on behavior. They further hypothesized that it would follow that if ESP messages compete for attention with contradictory messages communicated subliminally (which are approximately equal in strength to ESP communication), the ESP message would have a higher chance of being attended to and responded to.

It is our feeling that this may also apply to the present study, in which both mother and child worked on the same target goal. Each child was using normal perceptual means to accomplish the task of locating the target section. Many children did not comprehend how the mother, in another separate room, could "help" them find the target. Most subjects thought the mother was trying to find it at the same time. These factors combined might easily have inhibited any telepathic communication from being attended to or acted upon by the subject.

We also wonder whether the use of the CEFT might be a less appropriate choice than another type of perceptual task. In our previous study immediate feedback, as well as an enticing candy reward, was felt to be a positive factor in the results. Perhaps geometric figures, which do not yield immediate or intrinsically meaningful feedback to the child, are not best suited for this age group. Although the cards were colorful and interesting, they were still geared along the line of a test. Perhaps utilization of immediate feedback, candy reward, and more gamelike task would be a better procedure in future work with children.

A BLIND-MATCHING EXPERIMENT WITH A SELECTED SUBJECT USING EMOTIONALLY LOADED TARGETS

Martin Johnson (University of Utrecht)

Motivation is usually considered as a crucial factor in psi research. Needless to say, the concept of motivation is tremendously complex. By and large it is hard to measure or assess motivation in a precise way. Another factor often considered important in influencing a subject's scoring performance in a psi task seems to be the emotional quality of the target. In a study previously reported by myself, a "psychic" subject showed striking differential scoring in a blind-matching experiment, administered via closed-circuit television, between two kinds of targets. It was assumed that psi-missing would occur for target words related to unpleasant experiences in the subject's life and that psi-hitting would occur for target words related to pleasant incidents. The subject was kept unaware of the character of the target material used. This assumption was substantiated at a statistically significant level ($P < .01$, one-tailed).

In several studies I have carried out the objective has been to try to manipulate the scoring behavior of the subject by selecting different types of emotionally loaded target material. In one of the studies the outcome was in the expected direction and the difference in scoring between "pleasant" and "unpleasant" targets was clearly significant ($P < .005$, one-tailed). In still another type of ESP experiment disguised as a cognitive task, it was shown that the quality of the responses given by the subjects was significantly related to the information content and the emotional quality of the ESP targets.

The present study was carried out during the spring semester of 1976. The subject, M. Z., took part in eight sessions of 50 trials each. M. Z., a female freshman in psychology who volunteered for the experiment, was selected on the basis of her high-scoring performance in a previously reported experiment. In the present study target envelopes were to be paired with matching envelopes by the subject. The target envelopes were divided into two sets, A and B, and the matching envelopes were also divided into two sets, A and B. Each set of target envelopes (five identical-looking opaque envelopes) contained five ESP card symbols; one card was inserted in each envelope. Each set of matching

envelopes (25 identical-looking opaque envelopes) contained the five ESP card symbols, with each symbol repeated five times in each set.

"Emotionally loaded" material was added to the target envelopes of Set A only. This material consisted of more or less pornographic pictures, similar in size. One picture was added to each target envelope in Set A. A blank piece of paper was added to each target envelope in Set B to give the same appearance to the envelopes in both sets. On the back of both sets of target envelopes, as well as on the back of both sets of matching envelopes, identification numbers were written to permit the recording of the pairing between target envelopes and matching envelopes by the subject. The two sets of target envelopes as well as the two sets of matching envelopes were prepared by an assistant experimenter to keep the experimenter unaware of the meanings of the signs of identification.

Before each session the order in which the target envelopes for both sets were to appear on the desk in front of the subject was randomly determined by the use of a random number table. The order of the 25 matching envelopes within each of the sets was randomized in the same way. Matchings were done discretely for the two sets and the determination of whether Set A or Set B would come first was randomly assigned by the experimenter at the beginning of each test session. The evaluation of the results was not carried out until after the predetermined number of matchings had been completed (200 trials per set). The identification of hits and misses was independently scored by Mr. H. G. Boerenkamp of our University of Utrecht laboratory and by me as the experimenter.

It was hypothesized that the subject would manifest a differential scoring rate on the two sets and tend to manifest psi-missing on the emotionally loaded targets. The results showed nonsignificant psi-hitting on the set with blank pieces of paper and nonsignificant psi-missing on the set with pornographic pictures. The difference between the two was marginally significant (CRd = 2.00; $P < .03$, one-tailed). This outcome is suggestive. The observed scoring behavior corroborates the hypothesis. There was a rationale behind the hypothesis, based on previous findings in similar types of experiments and on studies with the Defense Mechanism Test, that emotionally provocative targets should lead to "defensive" processes in the subject, resulting in psi-missing. However, here the psi-missing effect was not per se significant.

As an alternative explanation of the outcome a possible experimenter effect should be considered. This alternative interpretation could be approached in a new experiment either by using an experimenter with opposite expectancies from mine, or by using an experimenter who is quite naive regarding the purpose of the experiment.

PSI IN THE CLASSROOM: THE VOCABULARY ESP TEST

K. Gowri Sundari, M. Sumathi Bopaiya, P. Sailaja, and K. R. Rao† (Andhra University)

Extrasensory perception has been found to be an extremely elusive phenomenon. It is likely that it would be less elusive if the testing were built into real-life situations, such as the area of educational testing. In an experiment reported in 1976 by K. R. R. and Judith Taddonio, subjects took four different multiple-choice examinations in parapsychology. Each examination had 50 items and each item contained five alternative answers to a question. Of the 50 questions, 25 had real answers in the parapsychological literature; these were designated "subject" questions. The other 25 questions were "made up," i. e., there were no true answers. These were the ESP questions, and they were written to resemble the subject questions as far as possible. The correct answers to the ESP questions were determined randomly and separately for each subject. The subject was asked to specify which of three degrees of confidence he felt in his answer to each of the 50 questions. The results confirmed the hypothesis that subjects who scored above chance on the subject questions about which they were doubtful or guessing would obtain significantly more hits in their ESP scores than those who scored below chance on these subject questions. A confirmatory series carried out with Indian students also yielded significant results in the same direction.

The present study was an attempt to replicate this finding and to develop a test that could be readily administered to large numbers. Since English is a subject taught in almost all schools in India, it was felt that a vocabulary test in English could be widely used. The Vocabulary ESP Test we developed consisted of a booklet containing 50 English-language key words provided with five alternatives. Twenty-five of the key words had real meanings, while the other 25

were made up by altering one or more letters from a real word. For each word five answers (English-language words) were prepared. For the real words, one of them was a correct synonym of the key word. The correct answer for the made-up words (ESP words) was determined randomly and separately for each subject, using random number tables, by a person otherwise unconnected with the experiment. (One word which was meant to be a real word was spelled wrongly; therefore, it was not scored.) The subject could answer the items for the real words using her knowledge of English. For the other items the subject had to use her ESP. To help the subject focus her ESP, the answers were recorded on a target sheet and enclosed in an opaque envelope, sealed and stapled to the test booklet. The subjects knew that they should use ESP to guess correctly on some items. They were not told anything about the "made-up" words.

There were three columns provided for the subject's answers. If she felt certain of her answer she circled her chosen alternative in column I. If she thought she was simply guessing, she circled in column III; and if she was doubtful of her answer, she circled in column II. Two experimenters, K. G. S. and M. S. B., distributed the test booklets to students in various classes at St. Joseph's College for Women. The instructions to the subjects were printed in the booklet. There was no time limit, but most subjects completed the test in 30 minutes. The experiment was conducted during their first class hour and therefore they were fresh and not fatigued. The subjects were 396 female students aged 16 to 20. They all had a fair grasp of the English language.

The results were analyzed to test the hypothesis that subjects who obtained scores above chance on real words answered in columns II and III (doubtful or guessing) would obtain significantly more ESP hits than subjects who obtained scores below chance on these words. Subjects scoring at chance level were omitted; 261 subjects scored above chance and 119 scored below. The obtained mean ESP score for the expected psi-hitters was 5.075 and that for the psi-missers was 5.05. The difference was not significant. The results, therefore, did not confirm the previous finding.

In retrospect, we are inclined to think that the insignificant results may have been due to any of the following factors: (1) many students complained that the test was too difficult, and this was evident from their low success in

answers to the real words. (2) The made-up words were very similar to the real words, since the words were formed by merely changing a letter or two from a meaningful word. This could have led the subject to make wrong associations and give set answers. (3) Unlike the situation in the previous research, where the subjects regarded the examination as a test of their learning in the subject of their study, the subjects in the present experiment took the test as a sort of psychology test unrelated to their course of study. This made the testing seem less related to the life situation.

We recommend that a new vocabulary test, therefore, should be prepared with words having less associative value with known English words. It should be administered in a situation where the students feel that their language skills are being tested and not as a psychology test. This can be achieved by making the class teacher administer the test. Also, the true English words should perhaps be less difficult than they were in the present experiment.

PARAPSYCHOLOGISTS' PERSONALITY AND PSI IN RELATION TO THE EXPERIMENTER EFFECT

Adrian Parker (University of Edinburgh)

It is widely assumed, with very little empirical evidence, that personality differences underlie the experimenter effect in parapsychology. It would seem that these supposed differences involve those areas of personality relating to social skills. While there is much anecdotal evidence for this, there is also some tangential support from research in which the experimenter has deliberately treated groups of subjects in an aloof detached manner versus a personalized outgoing manner, and thereby obtained differential scoring patterns. However, with the exception of the very limited work of Crumbaugh, there has apparently been no definitive attempt to test this proposition experimentally. The most obvious subjects for such a test are those parapsychologists who have established a reputation for obtaining significant results in psi research and those who have gained the opposite reputation. Further, since the evidence does not seem to implicate any specific or narrow band of factors, a global measure of personality would appear to be the most appropriate for this purpose.

Accordingly, the 16 PF, a general personality test, was given directly or sent by post to American and European parapsychologists who were active in psi research. Respondents were classified by three judges (John Beloff, Richard Broughton, and Brian Millar), when good agreement could be reached using set criteria, into those experimenters who consistently report significant results ("psi conducive experimenters") and those who characteristically fail to do so ("psi inhibitory experimenters"). "Psi conducive experimenters" were defined as those who had established a reputation for success in the field and who had published at least two papers in the last two years to this effect. "Psi inhibitory experimenters" were defined as those who were known to be unsuccessful in this respect and to have carried out at least two major studies. By this method the personality profiles of 15 well-known "psi conducive experimenters" and 14 well-known "psi inhibitory experimenters" were obtained. It was predicted that the psi conducive group would score higher on the dimensions of extroversion, warmth and sociability, confidence adequacy (self-security), and tenderness and sensitivity than the psi inhibitory group.

A rival hypothesis is that the experimenter effect is partially or wholly psi-mediated. This supposes that there are high-scoring experimenters as well as subjects who obtain their successes through their own psi. It would seem desirable to evaluate such a hypothesis using a disguised form of ESP test. For this purpose a multiple word-choice ESP test was designed and disguised as a construct inventory. The target words were chosen for each individual by computer randomization. This was issued to the parapsychologists along with the 16 PF test, which provided an ideal cover story for its use. The results were evaluated with respect to the response bias hypothesis using a method developed by Stanford. It was predicted that only the psi conducive experimenters would score significantly as a group on this test.

The results of the 16 PF showed none of the four predicted differences to be statistically significant, and only one to be in the predicted direction. A post hoc analysis of 14 other factors showed two of these to discriminate between the groups at the .05 level of significance. It was felt that these differences could have arisen by chance due to the large number of analyses carried out. However, the prediction for the psi test was confirmed. A rank difference correlation for the psi conducive group between relative accuracy on frequent responses and the number of these frequent responses given

was significant at $P < .05$, two-tailed ($r = .5$; $t = 2.08$, $df = 13$). As predicted, the same analysis for the psi inhibitory group was nonsignificant ($r = .15$; $t = 0.52$, $df = 10$).

The 16 PF cannot be regarded as a sensitive means of testing the hypothesis of personality differences between experimenters, and it is also confounded here by national differences. Nevertheless, considering the generality and power of discrimination claimed for these factors, one would expect to find some support for the hypothesis from the results of this test. The results of the psi test suggest more attention should be given to psi as a factor mediating some experimenter effects.

PSI-MEDIATED HELPING BEHAVIOR: EXPERIMENTAL PARADIGM AND INITIAL RESULTS

Rex G. Stanford† and Peter Rust (St. John's University, Jamaica, N. Y.)

Our objective was to learn whether a stranger will show psi-mediated helping behavior toward another person when the "cost" to the potential helper, of helping, is minimal. On both theoretical and certain empirical grounds we felt that psi-mediated helping behavior was likely in such a situation.

Thirty male, unpaid, volunteer subjects (persons who would need "help" in the study--henceforth, "helpable persons") were recruited for a "short psychology experiment" lasting no more than 30-35 minutes. They were told they would participate either in a "vigilance task" or in a "person-perception task," depending upon which study they were assigned to. They were not told what would determine in which study they would participate. They were individually tested.

A few minutes before the scheduled session for each helpable person, another person was recruited wherever he or she could be found (cafeteria, the hall, etc.) to participate without pay in a "short, simple test of word association," to last no more than 10 minutes. These subjects (henceforth, "potential helpers") had no idea that the word-association test was to be a vehicle through which they could render help to

a helpable person with whom they would be paired. It is virtually certain that few if any of the potential helpers were friends of the helpable persons with whom they would be contingently linked. The potential helpers had no idea that their short word-association experiment was in any way linked to the fate of another person and, indeed, did not know that any person other than the experimenter (P. R.) was involved in their study. Half the 30 potential helpers were female, half male.

While the helpable person waited in another room for the start of his experimental procedure, not knowing that the behavior of a person in another room would control his fate, the potential helper listened to 13 words on a cassette tape. His response on the words was recorded, as was his reaction time (RT) to one-hundredth of a second using an electric timer actuated by P. R. The RT measure was the basis of the dependent variable. The last 10 words of the 13-word list were the crucial words for, unknown to the potential helper, one of them had been randomly chosen--using the RAND table of random digits--as the "key word." If he produced the fastest response (among the last 10 words) on this key word, the helpable person was later introduced to a presumably quite enjoyable and potentially erotically arousing experimental condition--a "person-perception task" involving rating the characteristics of attractive young women depicted in a wide range of clothedness. If the potential helper did not produce the shortest RT on the key word, the helpable person was later introduced to a dull, tiring "vigilance task" which required very slow tracking of the lighted target of a rotary pursuit apparatus. The experimenter did not know, during the potential helper's word-association test, which word was the target.

By chance alone one subject in 10 (three in our sample of 30) would enter the enjoyable condition; in fact, seven of the 30 helpable persons were enabled to enter the more pleasant experimental condition, indicating a significant psi effect ($P = .0258$, exact binomial probability). This effect may have been of a relatively all-or-none kind (i. e., either a strong tendency to respond rapidly on the key word or no such tendency), for a z-score analysis, based upon the relative speed of response on the key word (and thus considering fast responses not fast enough to be successful), failed to reach significance. Sex of potential helper did not appear to matter. These results encourage future work in this paradigm, and a host of interesting independent variables can easily be imagined.

A COVERT PK TEST OF A SUCCESSFUL PSI EXPERIMENTER

Brian Millar (University of Edinburgh)

It has been argued that the results of many parapsychological experiments may be due to the psi abilities of the experimenter. The experiment described here was intended to detect any gross PK effect due to a single subject (R. B.) who, at least when working alone as an experimenter, has been able to elicit psi. The psi task was to "de-bias" by PK the results of randomness tests of a random number generator (RNG) which had, unknown to R. B., been adjusted by a known amount. R. B. was considered motivated to have the machine produce apparently satisfactorily random data, since if it had been found to be deficient the results of much experimental work would have had to be re-examined.

A randomness-testing procedure was used as the core of the PK test. This produced 10 chi-square values calculated from about 300,000 bits generated by our RNG in the 30 minutes the test took to run. A modification of the randomness-testing program was prepared such that before the data were printed out specified numbers were added to two of the chi-square values. Each number was calculated so that by chance alone the resultant total, believed by R. B. to be a genuine chi-square value, would seem significant (non-random) 90 percent of the time. Thus the probability of a hit (from R. B.'s standpoint, the output value being below the threatening $P = .05$ level) was 1/10. This procedure was chosen as a compromise between the extremes where no amount of PK could reduce the value output below the threatening critical value, and the alternate possibility in which many of the trials could not be expected to elicit psi, since even in the absence of PK, they would not be above the critical level. Only two values out of 10 in each test run were incremented in order that the deception did not become apparent to R. B.

In all, three runs--each with two PK targets--were secretly inserted in the course of genuine randomness tests being conducted by R. B. He had been given a list of the critical (5 percent) levels of chi square and was instructed to mark significant values. Since the PK task was to reduce the values printed out, and since part of these numbers was a known constant, the only way in which PK could act

was by reducing the genuine chi-square component of the data. For evaluation of the overall results of the six test trials of this study, the constant was subtracted from the composite to give the genuine chi-square value for each of the six test trials. These values were added. A significant value was regarded as one in which the total genuine chi-square value was significantly low ($P > .95$).

As in all PK tests the task of the subject was to bias the RNG. However, in this case the effect desired was not a simple side-bias. Instead, the complex task required the subject to decrease chi-square values representing measures of first- to fifth-order deviations from theoretical randomness. That is, he was required to cause the frequency of occurrence of overlapping strings of doubles, triples, etc. to conform more closely to mean chance expectation than would be expected by chance alone. While such a task is conceptually much more complicated than that usually required, considerable evidence indicates that "complex" psi tasks are not necessarily less successful than apparently simpler ones.

Only one trial of the six was a hit. This, in the third test, was also individually significant if considered in isolation. However, the total of the six did not differ significantly from chance expectation. No evidence of a PK effect was therefore detected.

It appears, however, that the randomness-testing paradigm is not entirely ill-suited to the detection of experimenter psi. Honorton has reported significant differences between randomness series run by him before and after experimental sessions. Honorton manually operated the machine and received trial-by-trial feedback while attempting not to influence the device. He was interested in sequential dependencies and examined the frequencies of each of the four possible pairs (00, 01, 10, 11). While all the deviations in the before and after sessions were within chance expectation, a positive deviation in the former tended to be followed by a corresponding negative deviation in the after series, and vice versa. It is as if Honorton were causing the data to be "random" (as indeed he had every reason to desire) by canceling out the chance deviations obtained in the before trials by PK action on the after group.

A major difference between Honorton's fortuitous conditions and the present experiment is that only end-of-session

feedback was employed here. Although the measures used in this experiment were based on a total of over a million binary trials, Schmidt's model suggests that a trial in fact consists of the unit of feedback. Since in this case the individual trials were never observed, and only the six chi-square values were fed back to R. B., the total test consisted of only six trials with chance probability of a hit being one in 10. If this is so, the experiment performed is very insensitive and renders unlikely the detection of any but the grossest PK effect. The amount of data that could be collected was severely limited by the necessity that the deception should not be discovered. Furthermore, only one experimenter capable of producing psi results was available in Edinburgh; thus this study was necessarily on a very restricted scale. The randomness-testing paradigm may however prove to be a useful method for covertly testing experimenters, particularly if use is made of trial-by-trial feedback.

PAPERS I*

EMPLOYING THE PARANORMAL EVENT IN ANALYSIS AS A PSYCHOTHERAPEUTIC TOOL

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I was interested in assessing two hypotheses drawn from relevant literature and research: the broader one, that the analyst-analysand relationship is a very special one and, because of the mutual needs of both, this might be a good setting in which psi phenomena could occur; and, more specifically, that within this relationship there is a specific time that is important, i. e. when there is a shift of attitude or consciousness in the analysand. Between October 11 and 20, 1975, therefore, I sent questionnaires to 3000 psychiatrists throughout the continental United States. The names were chosen at random from the Biographical Directory of the American Psychiatric Association.

The questionnaire contained two parts. Part A inquired whether or not the respondent was a psychiatrist, what his or her orientation was, and what his or her feelings were concerning the existence of psychic phenomena. It asked whether or not the respondent had ever had a paranormal experience and, if so, what type (clairvoyance, precognition, telepathy, other). Another question asked if anyone else known to the respondent had had such experiences. Part B dealt with the analytical situation, asking whether there had been any occurrences of paranormal phenomena during sessions with patients. If so, the respondent was asked to describe the experience(s) and tell at what point in analysis the experience(s) occurred (beginning, middle, or end, and after approximately how many sessions), whether those sessions might be described as turning or "breakthrough" points

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or points of resistance or denial, and whether the sessions were rewarding, painful, insightful, frustrating, or other. The therapist was asked if at the time of the occurrence(s) the relationship was good, strained, hostile, or other; if there were any significant change in the patient, a shift in attitude or personal outlook immediately preceding or following the occurrence(s), and whether or not the occurrence(s) assisted in the analysis. Answer sheets were provided for up to three patients.

By December 1, 1975, the cutoff date, 962 questionnaires were accounted for: 333 were returned undelivered, 6 returned with "deceased" notifications, 15 were returned unanswered, and 9 were answered by psychologists and were, therefore, not included in the results. The results, therefore, are based on the 609 (or 23 percent of the remaining 2647) which were returned answered. Almost a third of the sample believed strongly in psychic phenomena and almost half accepted the possibility that they exist. Over a quarter had personally had at least one paranormal experience. Precognition was the most common followed closely by telepathy. In Part B, 12 percent said there had been occurrences of paranormal phenomena during their sessions with patients.

To summarize the more detailed findings, the data tended to support the theory that when there is a shift in attitude and/or a significant change in a patient, psi phenomena are more likely to occur than at other points in therapy. In addition, psi phenomena occurred generally when there was a positive transference, commitment, or strong rapport between patient and analyst. The data also supported the postulation that psi occurrences take place at turning or breakthrough points. Almost 37 percent of the psychiatrists actually labeled the times of occurrences as "turning or breakthrough points," and an additional 10 percent answered that they were aware of the patient's shift in attitude. In further support of the "shift" or breakthrough point theory are the data concerning the point in therapy at which the phenomena occurred. Most happened in the middle of therapy which most frequently is the period of transference, "deepening commitment," "moments of intimacy," etc. In terms of sessions, psi phenomena were more likely to occur after session 15 and before session 300.

Although in more than half the cases reported, the analyst felt that the incident did aid in the analysis of the patient, there were indications in many cases that while the

psi phenomenon itself was noted by the analyst, the analyst was not aware at that time of its implications and could, therefore, make no use of any information derived therefrom. The returns suggest that most psychiatrists are unaware of what a paranormal occurrence implies therapeutically and/or how to make use of it in analysis. There is little regard for the wealth of emotionally cathected information given to the analyst on a paranormal level. What should concern the professional is that the psychic byproducts of the transference between patient and therapist not only be recognized but be utilized to enhance the skills already at the analyst's command.

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

J. Isaacs (Leamington Experimental Group, Warwickshire, England)

Most contemporary philosophers and psychologists tend to regard the question of the mind/brain relationship as having either been settled in favor of central state materialism or as being essentially undecidable and metaphysical in nature. Central state materialism may roughly be defined as the theory that the mind is the brain, and that consciousness is nothing but a type of brain activity. Many parapsychologists, however, have regarded their studies as providing evidence for some form of dualism whereby minds are not wholly reducible to brains or their functioning. There appear to be two sorts of evidence which could be regarded as constraining the choice of model for mind/brain relations, one being the "normal" evidence showing localization of brain function, or correlations between brain function and conscious experience or behavior, the other being the results deriving from research on paranormal perception. Psi functions appear to transcend the limitations applying to brains. This has led to the hypothesis that psi functions may be due to the activities of nonphysical minds.

Dualist positions of the Cartesian type have usually been subject to five main forms of objection. The first is that dualism arises from a conceptual confusion created by reificatory talk of "minds" when really "mind" talk is an indirect way of expressing quasi-lawlike statements about people's capacities, dispositions, habits, and styles of be-

havior. The second objection is that it is not necessary to posit the existence of ghostly minds, as all human information processing and conscious experience can be adequately accounted for by brain function. The third objection is that giving mental events an ontological status independent of brain states produces "nomological danglers"--events which cannot be identified with observed entities or their states and which as a result would not fit into the world as encompassed by science. The fourth objection is that the concept of a nonphysical mind entity is a curiously empty and formal notion, a metaphysical supposition not subject to empirical test. The fifth objection is that interaction between a mind entity (which by definition is nonphysical) and the brain (a physical object) must be impossible because only physical entities can interact with other physical entities.

To maintain its feasibility any dualist position must be able to meet these five objections. Clarification of the dualist position may be aided by the use of the hardware/software distinction. In any information processing system the hardware is the physical "works"; the software is constituted by the set of instructions (programs) which organize the hardware's information processing functions. The nub of the parapsychologically generated case for dualism has been that humans show software functions (psi perception) which appear inexplicable by reference to their hardware (brains). This provides an effective answer to objections one and two. Of course, should a physicalist explanation of psi be proved, for example that psi perception is due to the reception and transmission of very low-frequency radio waves, then the case generated for dualism by parapsychology would perhaps collapse.

The third and fourth objections are closely related. Dualists have to provide evidence showing that mind entities are inferred entities rather than metaphysical ones. At present, dualists can supply no empirically constrained description of the structure of mind entities, but the first step in the process of rescuing minds from having purely metaphysical status has been taken in showing that psi software functions exist which are apparently impossible for brains to perform. The fifth objection can now, I think, be met, and itself provides, indirectly, a form of test for dualism. The dualist picture which seems to be emerging is of man having two hardware items, brain and mind entity, both of which perform both hardware functions (casually interacting with the world) and software functions (processing information). The interface

between mind entity and brain is mediated by ESP, allowing the mind to scan its associated brain, and PK, which allows the mind to control the brain. Beloff has recently stated this view, and Walker has produced a detailed set of hypotheses concerning mind/brain interaction. The mind's sole hardware function according to Walker is PK, which does not involve an energy transfer from mind to matter, but an information transfer involving a reduction of the entropy of noisy energy-containing systems so as to obtain work from them. PK in this view is thus both a software function in its informational aspect and a hardware function in its causal aspect.

So far the parapsychological evidence tendered in favor of dualism has been mainly of software functions, but it is possible that very much more compelling evidence for dualism could be obtained from the mind entity's hardware function via PK. The well-established research of Schmidt and others has apparently shown that PK upon quantal processes is possible. Taking these results at face value, there is an ambiguity present in the interpretations that it is possible to place upon work involving Schmidt devices which, if clarified, could possibly produce rather powerful evidence in favor of dualist views. Supposing that the dualist scenario is true, it would not be surprising if the speed of operation of the mind entity were faster than that of the brain. The interfacing between mind entity and brain would proceed more smoothly, as error correction within the interfacing process would be more efficient if the mind entity could react more rapidly than the brain. If this hypothesis is true it provides a test for dualism, because parapsychologists could show that people can exhibit PK hardware functions which exceed in rapidity responses that could be mediated by the brain.

The relevant ambiguity in the interpretation of results from Schmidt devices is that success in influencing them may be due either to a dynamic form of PK, which would satisfy the proposed test for dualism, or to a static form of PK effect, which would not. The dynamic form of PK would involve the PK agent's mind entity monitoring the state of the Schmidt device and coordinating a very rapid PK "poke" at the noise generator within it so as to cause the noise generator to deliver a pulse at precisely the correct instant (which in the original device was a series of temporal "windows" only one microsecond long), so as to make the device latch in the target state. Such a dynamic form of PK would show that action within a one-microsecond interval was possible, and one microsecond is some five hundred times

shorter than the interval taken by one nerve impulse to cross one synapse. The static interpretation of the results is that PK causes the overall probabilities of the device's operation to shift slightly, making it latch in the target state slightly more frequently than it normally would.

What is needed to clarify the issue is a form of PK test which differentiates between static and dynamic effects. To this end, modifications could be made to the Schmidt device so that the state designated as target is changed randomly very shortly (perhaps one microsecond) before each individual trial. This would still enable a dynamic form of PK effect to score at significant levels, but would prevent static effects from achieving significant scores unless the target selection process was itself biased, which could be checked. However, even this modified test is still challengeable on the grounds that although the task appears to be a dynamic one, in reality it is static in that its aim is to cause a state to occur which will as an end product register a hit. Viewed in this way perhaps all putatively dynamic PK tasks would seem to be static in character, and PK in this view would be a process involving the increase in probability of occurrence of a given end state, regardless of the complexity or nature of the particular quantal route by which it was reached.

I do not find this possible reduction of dynamic forms of PK task to static ones very convincing, for two reasons. First, it must be trivially true that PK does increase the probability of occurrence of the target state--otherwise there would be no PK effect to argue about! Viewing the proposed dynamic form of PK task in any conventional cybernetic way must involve the assumption that the presentation of the assigned target state to the interfacing mechanism very shortly before the device becomes open to PK influence requires a rapid target state updating to be done by the mind entity, otherwise a "normal" pulse from the noise generator would latch the gate before PK action occurred and results would be random. Secondly, if PK effects originate from the interfacing mechanism between mind and brain, it is not surprising that machines which we regard as presenting complex PK tasks should be just as easy to score well on as relatively more simple devices, because either sort of device is simple compared to the enormous complexity of the neural circuits which are the more usual targets for the PK activity of the interfacing mechanism. A final objection to the dynamic PK interpretation is that precognition could short-circuit the

procedure of targets' being randomly chosen shortly before each trial. The only possible precaution one could take to rule out this counterhypothesis would seem to be to increase the trial rate, so that even if the PK target had been foreseen, the interval available for the establishment of a PK effect upon the noise generator would be less than could be effectively utilized by neural components.

AN UNCANNY TEMPORAL LOBE EPILEPSY APPARITION

James F. McHarg (University of Dundee, Scotland)

According to John Hughlings Jackson, the paroxysm of the uncinate fit, or temporal lobe epilepsy, was often very slight and while, typically, it began with a sensation of smell, its most characteristic manifestation was what he called a "dreamy state," of brief duration, in which the patient had a visual hallucination, sometimes a vivid "reminiscence" of a scene he felt he had witnessed before. The content of an apparition of temporal lobe epilepsy can sometimes be indeed pinned down to a past experience. More frequently, it cannot. In some instances such an origin even seems improbable, and occasionally the content has such an uncanny character that it raises the question of a paranormal origin. The temporal lobe epilepsy apparition now to be described exemplifies some of the difficulties of interpretation in such instances.

The apparition was experienced by a lady of 47 who, one afternoon in 1969, was being shown over the new home of a friend, and it occurred as she followed her friend into the dining room. It was of a woman, with soft brown hair, standing as if at a cooker in the far corner of the room with her left hand stirring a pot, and looking at the patient over her left shoulder. The expression on the apparitional lady's face was one of astonishment but she did not speak. She did not seem familiar to the patient. The duration of the apparition was brief, and during it the patient was still partly in touch with her friend. Immediately after the apparition the friend, when told of it, revealed that at one time there had indeed been a cooker precisely where the apparition had been "seen."

On making inquiries, the patient's friend found that some years previously the house had been occupied by two sisters, both teachers, named Blair. As soon as the friend

mentioned this name, the patient realized that the apparition must have been of a Miss Blair who, about the year 1900, had been her mother's primary school teacher, and to whom the mother, handicapped by deafness, had become very attached. In due course an old school photograph was shown to the patient from which she picked out, correctly, Miss Blair as the lady she had seen in the apparition. They also heard that Miss Blair was perhaps not dead but still alive in an old folks' home.

The patient told me during an interview that, immediately prior to the apparition, she had noticed an "out-of-place" smell, a "milky" smell, and I had no doubt (because of this as well as other confirmatory reasons) that the apparition had been of the temporal lobe epilepsy type. The patient herself felt sure that the content of the apparition did not represent the restimulation of any real-life event she had previously witnessed. To me the milky smell suggested, at first, a cryptomnesic origin from infancy. I then noted that when the patient had been only 18 months old, her mother had lost her own mother suddenly, and I speculated that the mother might have taken the sad news to the still comparatively young Miss Blair. I then realized, however, that Miss Blair had not been living at that house at that time. I next noted that, in 1955, when Miss Blair had indeed been living at that house, the patient's mother had herself died suddenly. I realized that Miss Blair might well have been standing at the cooker when she received the news of the death of her devoted former pupil--but she was then in her seventies and the patient denied having herself delivered the news.

The findings were, thus, inconclusive. Nevertheless, the veridical positioning of the cooker in the apparition did suggest a paranormal origin. It thus raised the general question whether the content of temporal lobe epilepsy apparitions may represent, on occasion, the restimulation of paranormal experiences of the individual long ago--originally, perhaps, at an unconscious level. Finally, I found that Miss Blair, although she had indeed been in an old folks' home, had sustained a fractured femur and had been removed to a hospital for surgical treatment. After two months of intensive rehabilitation, the regular progress notes had finished, on June 22, 1969, with the comment that (at 89, blind, deaf, incontinent and frail) she was "just a done old lady." The apparition had occurred only six days after this fateful medical decision, and it therefore seemed to me that its actual occasion may well have been the patient's having happened to

visit Miss Blair's former home at the very time when the latter was realizing that her own approaching death was near. She had died shortly afterwards, on March 5, 1970, in her ninetieth year.

[Shortly after the delivery of this paper, continuing inquiries brought to light an important fact the possibility of which had been unforeseen. This was that the cooker, which had certainly at one time stood where the apparitional figure had been seen, "as if standing at a cooker," had probably not been installed until after the person seen in the apparition had vacated the house. The positioning of the apparition does not, therefore, after all, have that veridical quality which it had seemed to have. A second fact brought to light is that the apparitional sister was probably not the sister who was nearing death at the time but the one who had already died. This reversal does not, however, affect the suggestion of a possible (synchronistic) relation of the apparition to the death which I found had been impending at the time of the apparition.]

THE EFFECTS OF CHRONOLOGICAL AGE ON GESP ABILITY

Ernesto Spinelli (University of Surrey, England)

The two experiments discussed below were designed to test the hypothesis that the ability to employ GESP declines as the human being learns more and more to employ and generate individually specific cognitive relations. To use a Piagetian framework, once the child enters the concrete operational stage wherein he can make real internalized connections (about the age of eight), then his ability to employ GESP should fall. What this means experimentally is that subjects up to about age eight should obtain GESP scores above chance, whereas those above this age should tend to obtain GESP scores at chance levels, since the development of cognition proper would necessitate a general "blocking" of GESP-type cognition. Also, in the age groups below eight, the younger subjects should score higher.

A total of 1000 subjects were tested in the first experiment. They were divided into 10 age groups, the three youngest ranging from age three to eight, and the oldest reaching age 70. Each group contained 50 male and 50 female subjects.

Subjects were tested in teams of two. Pairs of subjects within each age group were matched on intelligence as closely as possible. Upon being introduced to the experimental room each subject was given a set of either "sender" or "receiver" instructions. With the four youngest groups up to age 10, these instructions were preceded by a brief puppet sketch and puppets were used throughout to teach these subjects how to "play the guessing game." Subjects in these four groups were also given "thinking caps" (cone-shaped cardboard hats covered with tin foil and wire and padded inside around the ear area) which they wore throughout the experiment. They partially attenuated incoming sounds. Subjects in the remaining age groups were given straightforward instructions.

Once each subject entered his booth or experimental area he was shown his stimulus choice machine (these were called "guessing boxes"). The sender was told that once the red indicator light on his guessing box went on, he should pick up the top card in a pile of 20 cards and slot it into his machine. He would see that the card contained five different pictures, each of which was positioned so that a button from the guessing box would appear beneath it. The sender was to look at each of the five pictures and then choose the one he wanted to "transmit" to the receiver. This choice would be made by pressing the button underneath the chosen picture. If a chime rang, then that meant the receiver had guessed the sender's choice correctly; if nothing happened, then the sender was to wait for the red indicator light to go on again and move on to the next card in his pile. The receiver was given very similar instructions, with the obvious difference that his role was to "try to guess" which of these pictures the sender had chosen and was transmitting to him.

Subjects went through a total of 20 cards. At the end of the tenth trial, roles were reversed so that each subject acted as sender and receiver. Throughout the experiment a 10-track pen recorder recorded sender's choice, receiver's choice, and hits. The pictures used were 50 vocabulary test pictures and 50 pictures of abstract shapes. Each card was made up of either five different vocabulary test pictures or five different abstract shapes. Although obviously the same pictures were presented to the sender and receiver during each trial, the positional arrangement was altered so that any effects due to a positional bias would be controlled.

Simple analysis of the data revealed that the three

groups below age eight obtained scores significantly above chance ($P < .001$). Similarly, the youngest subjects (age three to four) scored significantly higher than the other two groups ($P < .001$), and the middle group (age four to five) scored significantly higher than the oldest of the three groups (age five to eight) ($P < .001$). There were no significant differences between male and female scores in any of the groups. Thus it can be seen that the two hypotheses are validated by the data. All other groups scored at chance levels.

The argument might be raised that such above-chance results were obtained because senders were allowed to choose the card they wished to transmit. The principal reason why choice was allowed in the first place was my expectation that it might be a crucial motivational factor in enhancing GESP ability. And since the data recorders were, in fact, recording all choices made, an analysis of these choices would reveal any non-random bias on the part of the senders. In fact, the analysis of subject choices revealed no such bias. But such criticisms led me to carry out a second experiment where subject choice was eliminated and replaced by randomized targets selected from the stimulus choice cards by random number tables. Personally, I was convinced that the new results would be lower than the previous ones because the sender's role became, to my mind, too passive, making him prone to boredom; but, much to my surprise, the results obtained were highly similar to those of the previous experiment.

In this second study only four age groups were tested. These were the three youngest age groups of the previous experiment and an adult (university) group. There were 50 subjects in each group, 25 males and 25 females. Once again, the three youngest groups obtained scores above those predicted by chance ($P < .001$), while the adult group obtained chance-level scores. Of the three youngest groups the youngest scored higher than the other two, and the middle scored higher than the oldest group ($P < .001$).

PARAPSYCHOLOGICAL TEMPTATIONS: A DISCUSSION ON DECLINE EFFECTS, SEX EFFECTS, AND POST-HOC EFFECTS

Nils Wiklund (Lund University, Sweden)

A well-known phenomenon in parapsychological experiments is the decline effect, the subject's tendency to get better results at the beginning of an experiment. It is quite possible that the decline effect may also be related to the experimenter. In that case it could be a contributing factor to the difficulties encountered by all parapsychologists in repeating even their own earlier results. The present experiment was designed to test the decline effect of the experimenter, and the way it may interact with the decline effect of the subjects. Eight different experimenters (four male and four female) took part in the experiment. I functioned as an independent coexperimenter, and in no case conducted actual psi-testing of the subjects. I was in a room about 100 meters away in the same house, producing the targets by means of a random number table. There were eight groups of subjects, all psychology students. First they indicated their attitude to psi phenomena on an attitude scale. In each group the three subjects with the most positive attitude were considered sheep and the three with the most negative attitude, goats. The others were excluded from all evaluation.

Each subject first did two GESP runs of 25 trials each for one experimenter and then another two GESP runs for another experimenter. There were individual targets for each subject, ESP symbols for two runs and the numbers 1-5 for two runs. Each experimenter conducted two experiments, which were separated in time by one day. So as not to confuse the possible decline effects with other effects, type of target, sex of experimenter, and time of day were all systematically varied in a balanced design, so that each of the 16 cells (two experiments for each of the eight groups of subjects) represented a different combination of factors.

Three types of decline effect and their interactions were considered as dependent variables: (1) for the subjects, comparing their first and second experiment; (2) for the experimenters, comparing their first and second experiment; and (3) from the first half to the second half within each of the cells of the experimental design. These three types of effect were tested independently for the sheep and for the

goats. A two by two by two factorial analysis of variance was used.

The results appeared completely random. None of the main effects nor any of their interactions showed any sign of a deviation from chance. However, a different effect was discovered. A comparison was made between the sex of the experimenter and the dominating sex of the subjects in each group of three persons. Since all 16 groups (eight sheep and eight goat groups) had done one experiment with a female experimenter and one with a male, it was easy to compare whether the groups had better results with an experimenter of the same sex or with one of the opposite sex. It turned out that almost without exception the groups had better results when they had an experimenter of the opposite sex. Only in one group out of 16 was there a better result with an experimenter of the same sex, and in three groups there was no difference. Statistically this is highly significant (by sign test, P = .003, two-tailed). When the results were pooled there were 520 hits for the 16 groups in the opposite-sex-experimenter situation and only 461 hits for the 16 groups in the same-sex-experimenter situation (480 hits expected by chance). The corresponding P-value for this difference is .034, two-tailed.

This result may be interpreted to mean that the sex of the experimenter is a stronger factor than the decline effect, at least if the experiments are fairly short as in the present case. The results will not decline if the subjects in the second half of the experiment get an experimenter of the opposite sex, but will possibly tend to decline if they get an experimenter of the same sex. The sex of the experimenter and of the subjects, and their relationships, have repeatedly been stressed as important factors in parapsychological experiments, and by some researchers these influences may even be accepted as proven. In this light it may be tempting to regard the result of the present experiment as interesting and as a partial support for the existence of psi phenomena.

In my opinion, however, this is not at all permissible. I will have to disagree wholeheartedly with any claim that these results in any way support an ESP hypothesis. In fact, the whole analysis of the so-called sex effect was made for the sole purpose of illustrating how easy it is to produce post-hoc results. It should not be necessary to remind professional parapsychologists of this danger. Nevertheless, my tacit assumptions are, first, that many parapsychologists tend

to pay undue attention to obvious post-hoc results and, second, that less obvious post-hoc results may be reported in parapsychological journals more often than we would like to believe. If hypotheses are not formulated with sufficient precision, if they are not even written down before the experiment is started, and if it is not decided from the start which one of all possible statistical methods is to be used, it is possible for post-hoc effects to exert an influence even if the experimenter is not fully aware of this. If post-hoc results in a particular branch of science are allowed to exert an undue influence, one necessary consequence would be that this branch of science would meet with great problems when independent researchers try to repeat earlier results. In case psi does not exist, post-hoc effects would, no doubt, be one out of many factors which have contributed to the results in "successful" parapsychological experiments.

There is, of course, also a legitimate and important use of post-hoc results, namely as inspiration for new experiments with predictions according to the post-hoc results, but this does not give the post-hoc results any evidential value. A different approach, outwardly somewhat similar to post-hoc analyses, can be seen as legitimate under very special conditions. This is the making of new predictions about data which have already been collected by others. Here the basic need is that the person who makes the new prediction knows absolutely nothing about the distribution of the data which have already been collected. If this condition is fulfilled the prediction would have the status of a "post-diction," i. e., a hypothetical statement about the outcome of something that has already happened. Post-dictions can be used, for example, in archaeological research. The use of post-dictions in principle ought to be distinguished from a post-hoc analysis, but it is a different question whether the necessary conditions for making post-dictions have been fulfilled in the famous cases which are sometimes discussed in the parapsychological literature (e. g., the quarter distributions in the PK experiments at Durham, discussed by Rhine in the Journal of Parapsychology 1975, pp. 306-25, and 1976, pp. 115-35).

In the present experiment the original hypotheses were clearly not confirmed. At an early stage, however, I decided to see if the material could not be used to illustrate the mechanism of post-hoc effects. To do this I consciously selected the group results most deviant from chance expectation. I tried to find something in common for these, and one of the first things I looked for, the combination of sex of experi-

menter and subject, turned out to be the best way to separate the high-scoring and the low-scoring groups. In this way I consciously manipulated the data in a way that in other circumstances could as well have been done unconsciously. The probability of finding some variable or other to differentiate between the high-scoring and the low-scoring subjects, of course, is very high. Combined with a slight slip of memory--I had in fact considered the influence of the sex of the experimenter!--the results might then have been presented as genuine. The fact that the result looks fairly sensible is also irrelevant. Man's mind is versatile enough to make sense of practically any pattern, be it ink-blots, or clouds, or distributions of hits in an ESP experiment.

In spite of my dictum that the so-called sex effect in the present experiment is simply my own construction, there may still be some who would like to claim that this effect after all could be genuine. They may argue that my original hypotheses were quite senseless, and that I perhaps did not construct, but rather happened to find the decisive influence in the experiment; it is rather to be expected that a new experimenter of the opposite sex would cancel out the decline effect. To test this possible counter-argument I tried to see if there was any internal consistency in the data to support this point of view. Since there seemed to be a positive effect of an experimenter of the opposite sex in the male-dominated and female-dominated groups, if this effect was genuine there ought to be a similar and even stronger effect if one excluded the individual subjects in these groups who were of the same sex as the experimenter. When I did this, the combined result for all the individual subjects with an experimenter of the opposite sex, compared with the results for those with an experimenter of the same sex, did not show any non-chance distribution. This supports my claim that the sex effect in the present experiment is purely my own construction, and an illustration of unpermitted post-hoc analysis.

PAPERS II*

EXPLORING "BLIND PK"

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

Use of the "majority vote" technique has been found generally to show greater significance than would the same raw data cumulatively totalled. In the majority vote technique, many trials are considered as one large trial, which is scored as a hit if the individual subtrials show more hits than misses. In quantitative PK, where many objects, such as dice or steel balls, can be thrown at once, the measurement of psi is thus facilitated. In research done prior to this experiment I had found that a personal knowledge of each target by the subject was not essential, and that satisfactory results could be produced with what were called "blind" targets. Here the subject's mental aim was simply for hits as such, and both time and effort were conserved.

In order to make use of our laboratory computer, I constructed a PK machine which employed air bubbles rising in a water-filled container. In continuous but irregular streams, these bubbles could occlude light to an electric eye, and this was recorded automatically in the computer memory. PK entered the picture by causing the quantity of light striking an eye to vary in the mentally desired direction (more or less light) during alternate six-second periods.

The computer compared each alternate six-second period (an "Odd" cycle) with its adjacent six-second period (an "Even" cycle) and continuously tabulated the difference, considered as one subtrial. There were five subtrials per minute. Each minute constituted a full trial; 16 full trials constituted a run. The target (whether more light should strike the eye during Odd or Even cycles) remained the same

*Chairperson: W. E. Cox, Institute for Parapsychology, FRNM.

for each full trial; thus each batch of five subtrials was aimed at the same target and could be grouped for a further majority vote analysis. The specific target order was given to the computer programmer, or else he used the order indicated in a computerized random table. A single "full trial hit" was registered if there were more bubbles recorded during each of three or more of the five Even cycles when Even was target, and vice versa. If exactly three, it was called a "Wheat" hit, and if four or five it was called a "Chaff" hit; and likewise for misses. The computer printout listed Wheat and Chaff totals separately.

I was the single subject in this experiment. Since the target objective was reversing every six seconds, I maintained only a passive PK mental effort to influence these bubbles. I looked upon the whole array of targets only collectively, if at all. The only term I used mentally was the word "Hits," or an equivalent. In the first of four experiments, 60 16-minute runs were done while I was personally present at the Institute. The PK machine was in my office, but I had no mental concern about knowing at a given instant whether Odd or Even was target (a practical impossibility). The results showed that the proportion of hits to misses in full trials was greater in the Wheat category than in the Chaff category, as I had predicted ($P = .012$, one-tailed). Subtrials themselves did not show a significant deviation from chance level.

There was still a "third order" of majority vote procedure, which used the whole 16-minute run as a single hit or miss for Wheat and for Chaff. Here Wheat hits proportionally exceeded Chaff hits sufficiently to yield the greatest significance of all ($P < .001$, one-tailed), more than making up for the necessary "loss" of considerable data.

A second series of 84 runs was done, again with myself as subject being present at the Institute, and again there was no significance for the pooled subtrials. The full-trial majority vote analysis again showed more Wheat than Chaff hits ($P = .02$, one-tailed). The full-run analysis again gave the greatest superiority of Wheat over Chaff ($P < .001$, one-tailed). Both experiments were followed by two comparable series wherein I was not present at the Institute. Though I was the subject, I did not know the hours when the experiments were performed. Neither one showed significance in any order of majority vote assessment.

Four conclusions emerged from these series as worthy of note: (1) isolation of Wheat appears to be a useful method of increasing the sensitivity of PK measurement; (2) employment of three levels of majority vote refinement is similarly advantageous; (3) fully automated equipment, together with only passive attention by the subject, seems not to be a deterrent; and (4) relatively high speeds of the bubbles increase the yield per hour of operation.

THE PERFORMANCE OF HEALERS IN PK TESTS WITH DIFFERENT RNG FEEDBACK ALGORITHMS

Dick J. Bierman† and Noortje V. T. Wout (Study Centre for Experimental Parapsychology, Amsterdam)

Theoretical considerations indicate that random number generators (RNGs) with different physical properties, and from which the output is displayed using different feedback algorithms, might have different "sensitivities" for PK influences exerted upon them. In this study the effect of the frequency of the RNG was explored. We were also interested in the so-called decline effect, the decrease in scoring rate with time. We felt that loss of "belief" might be responsible for this phenomenon, such as could be produced by feedback of poor results. We therefore decided to divide our 20 subjects into two groups of 10. For Group A the feedback algorithm was such that no very poor run scores were displayed on the screen. The computer added randomly in real time some false hits. Group B received genuine feedback. We expected that subjects in Group A would maintain their "belief" and show no decline effect.

The 20 subjects (14 male, 6 female) were all members of the NFPN (Netherlands Federation of Paranormal Healers). The main reason for using healers in this experiment was their generally positive attitude towards the paranormal. We explicitly told them that it was not our intention to investigate their claims with regard to paranormal healing; however, after the real experiment was finished, we asked them to "treat" some beans. These were to be planted, together with control beans, and the germination and growth rates would be measured. This study is not the concern of the present paper. The subjects were divided into two groups of 10 each, as described above. Both groups were matched with regard to sex and

years of membership in the Federation. We hoped that this would yield two groups with the same average self-confidence, a variable which might obviously interfere with a decline effect.

Each subject did three series (Parts I, II, and III). In Parts I and III, which were identical, the result of each trial (hit or miss) was displayed immediately after the trial. Each run consisted of 20 trials, and each part (I and III) consisted of 12 runs. In six of the 12 runs, the number of trials per second was one, and in the other six the number of trials per second was four. The probability of a hit per trial was 1 in 4 in all runs of Parts I and III. In Part II two RNGs were used simultaneously, since it was our intention that the subject should not notice a difference between the two RNGs. Actually, we "constructed" the two from the same source, using a kind of interleaved sampling and conversion procedure. One RNG had a frequency of 50 trials per second with a hit probability of 1 in 100; the other had a frequency of five trials per second with a hit probability of 1 in 10. The results for both were displayed every three seconds (corresponding to 150 trials for the high-frequency and 15 trials for the low-frequency RNG).

Subjects were tested individually in their homes, using remote terminals with a telephone connection to the computer. Both experimenters instructed the subject about the procedure. The subject then sat at the terminal and a demonstration run was displayed. This was done to get the subject acquainted with the way in which the results were fed back. The procedure was automated, but if the subject wanted it there was a short rest between runs.

We had four hypotheses. (1) Overall results will show significantly more hits than control run scores (CRs were calculated using the experimental standard deviation, which did not differ significantly from the theoretical standard deviation). (2a) There will be a difference in scoring rate between the fast and slow RNGs in Parts I and III. (2b) There will be a difference in scoring rate between the fast and slow RNGs in Part II. (3) Subjects receiving false feedback (Group A) will score significantly better during Part III of the experiment than subjects in Group B.

Hypothesis 1 was not confirmed; overall scores were generally nonsignificantly negative compared with control scores, especially in Parts I and III. Hypothesis 2a and Hypothesis 2b were likewise not confirmed; the difference between the number of hits in the two RNGs in Parts I and III

was only 34 (CRd = 0. 75), and in Part II it was only 162 (CRd = 1. 72). For both comparisons the low-frequency RNG runs resulted in fewer hits than the high-frequency RNG runs. The pooled difference for all three parts was 196 hits (CRd = 1. 88).

To test Hypothesis 3, z-scores were computed for each subject separately and for all subjects in each group for each of the three parts of the experiment. We expected that for Group A positive scoring in Part I should continue through Parts II and III, while for Group B positive scoring in Part I should decrease towards a lower scoring rate in Parts II and III. Since no positive scoring occurred in Part I Hypothesis 3 could not be tested. Nevertheless, a remarkable decline occurred in the scoring of the subjects in Group B. From an initial chance level of scoring in Part I, the psi-missing increased to a significant level in Part III (CR = 2. 15). However, the difference in scoring between Parts I and III for Group B was not significant (CRd = 1. 34). For Group A subjects the score was less negative in Part III than in Part I.

Some post-hoc analysis might give reasons why none of the hypotheses could be confirmed. The subjects were in general very cooperative but found the PK experiment very far away from their daily work of healing. All of them liked Part II the best, but did not very much like Parts I and III. This may be related to the feedback display. In the single RNG parts (I and III) the subject has an objective reference with regard to good and bad run scores. In the double RNG part (II), however, he is told only that performance is good if the graphical representations of both RNG results cross, which is nearly always the case. The generally positive attitude towards the double RNG experiment (Part II) can therefore be explained by the fact that the subject always has the feeling he is performing well. This was true for Group B as well as Group A. An explanation for the significant psi-missing of Group B subjects in Part III might be that after the positively appreciated Part II they had to repeat again the negatively appreciated Part I.

PSYCHOKINESIS, CLAIRVOYANCE AND PERSONALITY FACTORS*

J. M. Houtkooper (University of Amsterdam)**

The purpose of the experiment performed was to replicate some of the work of Kanthamani and Rao and to extend it to PK in a computerized electronic RNG setup and clairvoyance in a computerized maze test. As the personality factor of prime interest we selected neuroticism, as measured by a Dutch test, the 5PFT. We selected 21 subjects, of which 14 were highly neurotic and seven, highly non-neurotic.

The detailed hypotheses in the experiment were: five PK hypotheses (a. o. on PK training, unconsciously elicited PK and retroactive PK), three clairvoyance hypotheses (main effect, difference between random behavior trials and non-RBTs and response time effect), seven hypotheses on the relationships between different forms of psi and neuroticism and two hypotheses on the relationship between PK scoring and clairvoyance scoring behavior. Of these 17 hypotheses, only the response time effect in the maze test was confirmed ($P < .005$), response times being longer for hits than for misses. Post hoc, an overall evaluation was made by calculating a chi square. This overall psi performance is 229.32 ($df = 189$; $P < .05$).

Exploratively, we looked at the response time effect in relation with different personality factors. Significant correlation coefficients were found with Extroversion, Agreeableness and Neuroticism. The response time effect is most pronounced for subjects being low-E, low-A and/or high-N. The conclusion is that personality factors as predictors of psi scoring behavior are useful to pursue in further research.

*Verbatim abstract supplied by author.

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PAPERS III*

FEEDBACK WITH ESP SYMBOLS: REPLICATION OF UNPUBLISHED BUT NOT OF PUBLISHED FINDINGS

Margaret Jackson**, Stephen Franzoi**, and Gertrude R. Schmeidler† (City College, CUNY)

Increases in ESP scoring following feedback for hits were reported in an initial experiment and a successful replication by Honorton. He asked his subjects to call three runs of ESP cards, and on each run to say "Check" from three to seven times, to show the calls for which they felt more confidence. Scores on these runs provided a baseline. Honorton then had his subjects call three more runs, using the same procedure, except that after each correct call the experimenter said "Right." Three final non-feedback runs, similar to the first three, provided the test condition. Both published reports described significant improvement in checked calls for the latter three runs. Yoked controls given false feedback did not show improvement. A third experiment varied the number of feedback runs, and obtained results like the earlier ones for three feedback runs, but no improvement (or lower scores) after six or nine feedback runs.

We attempted to replicate the Honorton method with some minor changes required by our college setting. Our subjects made only six runs: two for baseline, two for feedback, and two for final test. A control group made six runs without feedback. A further question of interest to us was whether musicians would have higher ESP scores than other subjects. We had planned to use professional musicians, but this was impossible to arrange with the appropriate institutes.

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Therefore, it was decided that music majors would be the best alternative. The experiment was designed to examine three formal hypotheses: that checked calls would be more accurate following feedback; that total scores would be higher after feedback; that music majors would have higher ESP scores than non-music majors.

Forty unpaid volunteers, 21 male and 19 female, were recruited from the student body of the City College of the City University of New York. The first 20, recruited from psychology classes, included no music majors; the second 20, recruited from music classes, were all music majors. Ten subjects from each group were randomly assigned to the experimental, and 10 to the control condition. S. F. tested five subjects in each subgroup; M. J. tested the other five. Using a single entry point in a random number table, M. J. prepared the targets for S. F. 's subjects and S. F. similarly prepared the targets for M. J. 's. Each target list of ESP symbols was written on a strip of paper with 25 spaces, wrapped in opaque material, and put in an envelope. The envelope was sealed, and a blank response sheet was pasted on the outside of the envelope directly over the target material. For the feedback runs, special envelopes were prepared with a slot just large enough for the experimenter to see a single target.

After preliminary conversation, the experimenter described the ESP guessing task to the subject and instructed him to mark a check on the response sheet when he felt relatively confident his guess was correct. He was to try to make three to seven checks per run of 25 calls. The control subjects completed six runs in this manner. The feedback subjects were additionally instructed that they would be "learning ESP" later in the experiment. Then, following the first two runs, the third and fourth runs were done with feedback: the experimenter said "Right" after each hit. The subject was instructed to use this condition to try to discriminate between correct and incorrect calls, and to modify his guessing strategy freely. Runs five and six were identical to runs one and two.

The total scores showed no significant differences for males and females, or for subjects tested by the two experimenters, and all data were therefore pooled. Total scores were near chance expectation for all subgroups, all runs, and all combinations of runs. Checked calls were similarly near chance expectation except for feedback subjects on their

first two runs, before they received feedback. On those runs their checked calls were significantly lower than chance expectation (CR = 2.65; P < .01, two-tailed). It was clear by inspection that music majors did not score differently from non-music majors, and that total hits were not significantly different as a result of feedback. The Wilcoxon T was used to compare checked calls of feedback subjects before and after feedback. The difference was not significant.

It was clear that we had failed to replicate the Honorton findings. We had, however, obtained a significant datum: the psi-missing of our feedback subjects on checked calls before feedback. It suggested that knowing they were to try to learn ESP later on had an adverse effect on their confidence in the first two runs. We examined Honorton's articles to find if his subjects also showed psi-missing on pre-feedback checked calls, but no data were available. In none of the three articles were pre- or post-feedback scores given for checked calls. We then asked Honorton what the scores had been, and found to our surprise that they were similar to our own. In only one of the three experiments had post-feedback scores on checked calls been above chance; in the other two they were at chance level, and the finding of significant improvement was based on below-chance scores for checked calls in the pre-feedback condition. Thus we had indeed succeeded in replicating Honorton's unpublished findings although not his published ones.

A post-hoc finding which may repay later investigation is that there were rather high (+1) and (-1) displacement scores after feedback. It suggests that the feedback elicited vague, free-floating psi which approximated rather than hit the target. If this is found in other studies, it may lead to insights about the nature of the psi process. It should also be stated that one of our initial questions, whether musicians would do better at ESP than non-musicians, was not studied here. M. J., a professional musician, reports that her conversations with the music majors indicated that neither their skills nor their attitudes made it seem likely that they would pursue a professional career in the years to come.

CALL-TIME AS A NEW PARAMETER IN STATISTICAL ESP EXPERIMENTS

Wilfried Kugel (Forschungsinstitut für Parapsychologie, Berlin)

Statistical experiments on ESP phenomena carried out by the Project Group for Parapsychology (a section of the Institute for Psychology of the Technical University Berlin), particularly those connected with telepathy, led us to hypothesize that there might be a relation between the call-time (the time lapse between the request for a call and the actual call by the percipient) and the ESP score. A further question was whether the expected optimal call-time had a subjective or objective bearing on telepathic efficiency. A relation between call-time and ESP score was noted previously by Rýzl and by Soal and Bateman, but these authors had not used automatic time measuring devices. In the test series that I am discussing here, such exact time measurement was designed. We reconstructed our automatic testing device and built the new "System AASW 4."

Three test series have been conducted with this device: Series E, F, and G. Series E and G are completed. Our hypothesis for the first series (E) was that, in statistical ESP experiments, there are definite call-time intervals for a subject (or a pair, or a group of subjects) such that the score on calls given within these time intervals differs significantly from the expected mean. Our telepathy tests involved giving the subjects (percipients and agents) feedback after each call, and we soon discovered that this caused a problem: the percipients reacted very strongly to the feedback given, such that they tended to give the same call after a hit and a different one after a miss. This reaction influenced the distribution of symbols in the call row, and has been more or less strongly noted in all subjects investigated. Therefore we defined "feedback-susceptibility" as the tendency to choose the same call symbol after a hit. In Series G we attempted to correlate test scores with strength of feedback-susceptibility, measured by mathematical formulas we developed.

Three pairs of subjects were tested in Series E, which consisted of 15,867 trials. Twelve pairs of subjects were tested in Series F, which consisted of 5483 trials. Sixteen pairs of subjects were tested in Series G, which consisted of

5996 trials. Each pair was selected on the basis of having an emotional relationship. It was planned that each person should serve for the same number of trials as agent and as percipient. This could not always be reached. We studied the data of each pair individually, searching for a minimum of one specific time-interval in which the hit score should deviate significantly from chance at the five percent level, two-tailed. Series F was done to investigate the possibility of prognosticating the hit score within a call-time interval. For that purpose the most interesting interval of a first run was used as prognosis interval for a second run with the same subjects. Because of the fluctuations of call-time this principle did not work. The test device included a random number generator controlled by the decay of a radioactive source. It generated sequences of targets consisting of five slides, monitored the two consoles for agent and percipient, gave feedback of hits, targets, and calls, measured the call-time to an accuracy of 1/10 second, and recorded targets, calls, and call-time on paper tape. Randomness tests showed the random number generator to be satisfactorily random.

The call-time evaluation requires that the number of trials and hits for each call-time interval be counted. Then the difference between the expected value and the actual score is found, and the corresponding probability is calculated for a positive or negative deviation of the hit score from the expected value. To find out whether one of the calculated probabilities is small enough to draw any conclusions, one must also look at the number of time intervals investigated. Formulas were developed for assessing this information and graphs were drawn of hit score as a function of call-time interval.

The time graphs of Series G showed no significant deviations from chance, but there was an interesting concentration of hits within the call-time interval 4.5 to 5.0 seconds. In Series E there were no significant deviations at all. In Series F, which has not been fully investigated yet, there was one significant time-interval of 0.7 seconds ($P = .0004$). Overall, however, our hypotheses could not be verified. Most of the time distributions of the scores appeared as expected by chance.

For the feedback-susceptibility analysis, we divided the results of Series G into two equal groups regarding the hit scores of the subjects: one with larger deviations from chance, the other with smaller deviations from chance. The group with

nearly chance results showed a high feedback-susceptibility score; the other group showed no feedback-susceptibility. The reliability of this effect was tested by re-examining the data of Series E and F; a similar difference was obtained. We also re-examined the data from an earlier series with subjects under LSD. Those subjects scored high before and after the intoxication; under the influence of LSD the hit score declined. Correspondingly, the feedback-susceptibility was low before and after intoxication and rose during the influence of the drug as the hit score declined. In conclusion, there seems to be a strong correspondence between feedback-susceptibility and hit score deviation from chance.

PSI IN THE CLASSROOM: A REPLICATION AND EXTENSION

Judith L. Taddonio O'Brien and Dennis P. O'Brien (Institute for Parapsychology, FRNM)*

K. R. Rao and J. L. T. O. conducted a study investigating the occurrence of psi in the context of a forced-choice educational examination. Their study attempted to map the relationship between extrasensory guessing on real and on made-up items in a classroom examination. Their subjects were administered a multiple-choice examination consisting of 50 questions, each having five alternatives. Half the questions were "real"; they had real textbook answers. The other half were "made-up" questions; they were phony questions without real answers. These made-up questions were ESP questions--the students had to use their ESP to answer the questions correctly. One of the five alternatives for each of the made-up questions was randomly chosen as the ESP target. The target orders were sealed in an envelope attached to the examination booklet.

The students answered each question in a way that indicated whether they were certain of their answer, doubtful of it, or simply guessing. The investigators predicted that students who performed above chance on real questions that they were doubtful of, or were guessing about, would also perform above chance on the ESP questions; they were pre-

*Presented by K. R. Rao.

dicted psi-hitters. Conversely, students who performed below chance on real questions answered as doubtful or guessing were predicted to perform below chance on the ESP questions; they were predicted psi-missers. The results bore out their predictions: on the ESP questions, predicted psi-hitters obtained a mean run score of 5. 56, while predicted psi-missers obtained a mean run score of 3. 32 (mean chance expectation = 5. 00). A test of the difference between predicted hitters and missers was highly significant ($t = 3.14$; $P < .005$, one-tailed). Overall, a total of four separate exams were administered to the same group of subjects over a period of days in this study. Three experimenters administered the exams--one administered two separate exams, while the other two administered one each. The mean run score on ESP questions (collapsed across hitters and missers) decreased from Exam 1 (5. 42; CR = 0. 76) to Exam 4 (3. 70; CR = 2. 29). While this result suggests a decline in subject scoring over a series of examinations, the difference was not significant.

The present study was designed as a replication and extension of this experiment. A Durham High School teacher (H. H.) administered the exam to five of his psychology classes (133 students total) during the course of a single school day. There were two hypotheses. First, predicted hitters and missers would be evaluated, as in the previous study, according to performance on real questions answered as either doubtful or guessing. Predicted hitters were expected to score above chance, while predicted missers were expected to score below chance on the ESP questions. Second, because this situation involved a single experimenter (H. H.) testing different subjects over the five exams, a decline in overall ESP performance from the first to the fifth exam was predicted. With repeated testing, we felt, it would be surprising if H. H. were able to maintain a high level of enthusiasm for his task. His waning interest could result in a significant decrease in the ESP scoring rate of his students.

The examination contained 30 multiple-choice items, and each item contained five alternatives. Of the 30 questions, 15 had "real" answers in the students' psychology textbook, and 15 were "made-up" questions. The "correct" answers to the made-up questions were determined randomly and separately for each subject by D. P. O. using a PDP/11 computer. The real and made-up questions were randomly mixed. Adjacent to each question were three columns, labeled "certain, " "doubtful, " and "guessing. " Each student's

exam booklet was attached to an opaque sealed manila envelope containing the appropriate ESP target sheet, backed on both sides with construction paper.

The students were told that some of the questions were real and some were made-up so that they would have to use their ESP. They were given the following example of a real question: "Four traditional functions of psychology are description, understanding, prediction, and ... (A) trait determination, (B) naturalistic observation, (C) experimentation, (D) guidance, (E) control of behavior." They were also given an example of a made-up question: "The Hurvich-Jameson theory of personality is constructed around which of the following: (A) the apparent-process theory, (B) the nature-nurture controversy, (C) the doctrine of specific energies, (D) individual phenotypes, (E) imitation learning."

The ESP scores of the expected hitters were compared with those of the expected missers by t-test. The predicted hitter/misser effect was significant for the first class ($t = 1.84$, $df = 19$; $P < .05$, one-tailed). Of the five classes, three (1, 3, and 4) showed the predicted hitter/misser scoring trend and two (2 and 5) showed insignificant reversals. With regard to the second hypothesis, a chi-square analysis of hits and misses for Class 1 versus Class 5 was highly significant (chi-square = 10.25, $df = 1$; $P < .005$), reflecting a strong decline in the subjects' scoring rates. This decline was not surprising, given that H. H. administered the test five times in a single day. In this case, such a decline could be attributed to a psychological experimenter effect.

COGNITIVE MODE AND EXTRASENSORY FUNCTION IN A TIMING-BASED PMIR TASK

Rex G. Stanford† and Angelo Castello (St. John's University)

The possibility that psi function is facilitated by certain modes of thinking and is inhibited by others is not a new idea, but it is receiving renewed attention because some commentators have felt that the right hemisphere of the brain, a hemisphere specializing in "nonanalytic" tasks, might control a mode of thinking which is particularly psi conducive. We prefer an interaction model. We propose that ESP is not localized in either hemisphere and that whether or in what

degree a particular mode of cognitive function interferes with or facilitates extrasensory function will depend upon the interaction of that mode with what is required for extrasensory function, given the circumstances at hand. Cognitive mode is not intrinsically related to extrasensory function. Broughton reported some experimental results at the 1975 Parapsychological Association convention which can be construed as tentatively supporting an interaction model [RIP 1975, pp. 98-102].

Since we advocate an interactionist view of cognitive function and psi, an appropriate first step would be to design a study which would offer a definite opportunity for the falsification of our position. Such an experiment is one in which if the outcomes show that psi function is favored by one cognitive mode (e.g., nonanalytic activity) and inhibited by another, this outcome cannot reasonably be explained in terms of the interaction model.

For this purpose we employed a test of nonintentional extrasensory response (psi-mediated instrumental response, or PMIR) in which there was an opportunity for a goal- or need-relevant influence on the timing of the emission of a response during a word-association test. In such studies the subject responds by giving a single-word response to each of 10 words on a list, and his reaction time (RT) is recorded on each trial. Unknown to the subject, one of the 10 words has been randomly chosen as the "key word," and if on this word he responds faster than on any other word in the list, he subsequently experiences a relatively favorable circumstance. If he does not respond with an appropriately short RT on the key word, he subsequently experiences a dull and tiring experimental condition. The experimenter is blind as to which word is the key word for a given subject.

With this paradigm one could manipulate cognitive mode by assigning to different groups of subjects different classes of word-associated responses which must be given. According to the interaction model, this controlled association paradigm should not produce differential ESP success in the groups required to give different kinds of responses, at least if only cognitive mode and not mood is manipulated. The crucial factor in this paradigm is the timing of the emission of the response on a particular word, and that timing is considered in relation to RTs for other words to which the subject is responding in the same cognitive mode. The form or kind of response is therefore irrelevant. Even if one cognitive

mode generally produces faster response, this would not favor success, since it is the relative speed on the key word which is important.

We chose to compare the ESP performance of two groups, one operating under instructions to produce only logical-coordinate responses to concrete-noun stimuli and the other acting under instructions to give only predication responses to the same word list. A logical-coordinate response names an immediate member of the same class or category to which the thing named by the stimulus word belongs (e. g., APPLE--pear; PEN--pencil; SPARROW--pigeon). Predication responses of the type required here are responses which name a sensorially detectable attribute of the thing named by the stimulus word (e. g., FUR--smooth; METAL--shiny; NAIL--pointed). Extensive research in various cultural and linguistic settings has shown that in free-association the number of responses of these two classes is rather strongly negatively correlated. Persons who give many of one type tend to give few of the other. It would therefore appear that production of coordinate and predication responses involves distinctive cognitive modes. Furthermore, the production of predication responses is known very often to be mediated by imagery, especially visual imagery; not so, typically, in the case of the highly conceptual coordinate responses. The manipulation of these two distinctive cognitive modes might, therefore, actually engage different hemispheres of the brain.

The design was a two by two factorial with different subjects in each cell. One variable was cognitive set (predication or coordinate); the other, sex of subject. Equal numbers of male and female college-age volunteers were used (20 of each) and the sexes were assigned equally to the two experimental conditions. Subjects were told the test was neither a test of personality traits nor of intelligence, but was intended to study language function. They were given general instructions as to the kind of response required, were given three examples of appropriate stimulus-response relationships, and responded to an informal trial before the formal list was administered. Subjects were told they had five seconds to respond to each of the words on the list. The 13-word list was administered by tape recording with the stimuli presented 20 seconds apart. The words in order of presentation were: airplane, flute, clam, ring, butter, wool, aluminum, door, bed, robin, river, hammer, and butterfly. Unknown to the subject, only the last 10 constituted the formal test list. Subjects were told that they should not be

worried if they failed to respond to a given word within the time limit. Despite the stated time limit, subjects were not stopped if they exceeded the limit. However, if a response took longer than five seconds or there was no response, an RT of five seconds was recorded. The RT for each response was recorded to a hundredth of a second using a microswitch-actuated electrical timer controlled by the experimenter.

When a subject's word-association was completed, the experimenter left the room to consult a slip, which he had not previously seen and which had been prepared by an assistant, on which was written a number designating the key word for that subject. This number had been chosen on the basis of the RAND table of random digits. If the subject produced the shortest RT on the key word, he was told that the experiment was completed and he could now leave. If the shortest RT did not occur on the key word, he was introduced to a dull, tiring task which consisted of tracking the lighted target circle of a rotary pursuit apparatus for 20 minutes. This target was moving in a circle at the slowest speed at which it could be set to move smoothly.

The dependent variable (the ESP score for each subject) for the statistical analyses was a z score computed as follows: the logarithm (to the base 10) for each RT for a given subject was obtained. Then the subject's log RT for the key word was subtracted from his mean log RT for all 10 words in the list and this difference was divided by the standard deviation of all his log RTs (for the 10 words of the list). The resulting z score thus had a positive sign if the log RT for the key word was smaller than the mean log RT.

A two by two factorial analysis of variance revealed that neither of the main effects nor the interaction was significant (cognitive mode $F = 0.034$, $df = 1, 36$; sex $F = 1.79$, $df = 1, 36$; interaction $F = 0.50$, $df = 1, 36$). The overall ESP score did not reach significance, though it was quite highly negative ($t = 1.67$, $df = 39$; $P \simeq .10$, two-tailed). This means that there was possibly some tendency for subjects to stay in the experiment for the dull task rather than to leave early. This tendency was contributed mainly by the female subjects, whose performance was significantly negative ($t = 2.40$, $df = 19$; $P < .04$, two-tailed). This post-hoc analysis of the females' data can only be suggestive, but it does suggest the possibility that psi occurred in the study, at least among females. Despite this suggestion of psi in the

females they, taken alone, showed no evidence of a differentiation of performance in the two cognitive modes.

As a check that our experimental manipulation did not involve tasks which differed as to difficulty, we examined as the dependent variable the mean log RT for each subject. There was no effect of the experimental manipulation nor of sex upon reaction time, nor was there an interaction. The results of this study provide some support for the proposal that there is not an intrinsic relationship between cognitive mode and extrasensory response. Our results cannot, however, be said to provide direct support for the interaction model since the study was not intended to attempt confirmation of specific, positive deductions from that model. Instead, it simply attempted to falsify that model but failed to do so.

AN ESP CARD-GUESSING EXPERIMENT WITH A LARGE SAMPLE

John Palmer†, Charles T. Tart, and Dana Redington (University of California, Davis)

This experiment was part of a screening process to select subjects with exceptional ESP ability for an experiment on training ESP. The subjects were primarily students in selected undergraduate classes at U. C. Davis. Twenty-three classes were tested, ranging in size from eight to 260. In addition, 65 persons who responded to a campus advertisement were tested in special sessions. In all, 2425 people were tested.

The experimenters were upper-level undergraduates in an experimental psychology course taught by C. T. T. The students were divided into five teams of three or four students each. Team members contacted professors they knew and asked for permission to test their classes. At the beginning of each session the team members introduced themselves and distributed the record sheets. The sheets included an attitude question, "How much do you believe in the reality of extrasensory perception (ESP)?", and a mood question, "How do you feel right now?", each answerable on a seven-point scale. Subjects were asked to answer these questions before the ESP test itself.

The target material consisted of a deck of 48 playing cards containing 12 each of aces, twos, threes, and fours. The deck was separated into two decks of 24 cards, each of which was thoroughly shuffled before testing began. The subjects' task was to guess the number of each card and mark his responses on the record sheet. Chance expectation of a hit was thus one-fourth. Before each run the appropriate 24-card deck was placed face down on the table in front of the class, in back of a screen or similar barrier. Experimenter 1, who was kept blind to the target order, called out "next" every five seconds. After each signal, Experimenter 2 picked up the top card, held it for a couple of seconds, and then placed it face down on another pile.

Each session consisted of one GESP run and one clairvoyance run of 24 trials each. On GESP runs, Experimenter 2 and the other experimenter(s) present (except Experimenter 1) concentrated on the target during each trial. The GESP run was given first in the first 12 classes, after which the order was reversed. Data from the record sheets were transferred to IBM cards by keypunchers at the U. C. Davis Computer Center. The target orders, which were recorded by the experimenters after each session, were punched by J. P. A computer program was written to punch a new deck of cards containing for each subject the number of hits per run as well as his questionnaire and demographic data. Cards were punched only for subjects who made 48 scorable ESP responses, which reduced the sample size from 2425 to 1835. These cards were then fed into the computer for statistical analyses. These analyses were not corrected for stacking, because we knew of no way that the correction could be applied to the kind of statistical tests that we considered appropriate for these data. Furthermore, the effects were too strong to be substantially affected by such a correction. All P values to follow are two-tailed unless indicated otherwise.

The overall mean ESP score per subject was 12.06, which does not differ significantly from the chance mean of 12. However, analysis of variance revealed significant effects for both type of run ($F = 9.89$, $df = 1, 1833$; $P < .01$) and position of run ($F = 6.13$, $df = 1, 1833$; $P < .05$). The mean of the GESP runs (6.15) was significantly above chance expectation of 6 ($t = 2.94$; $P < .01$), while the mean of the clairvoyance runs (5.91) was below chance ($t = 1.80$; nonsignificant). The mean of the first runs (5.94) was below chance ($t = 1.32$; nonsignificant), while the mean of the second runs (6.13) was significantly above chance ($t = 2.54$; $P < .02$).

This "incline effect" may have been caused by the fact that testing conditions were somewhat chaotic and it took some subjects one run to get settled and adapted to the procedure.

Post-hoc analyses comparing these two effects among the five experimenter teams revealed a significant interaction between team and type of run ($F = 2.63$, $df = 4, 1779$; $P < .05$). The superiority of GESP scoring was found to be almost entirely attributable to one team which had expressed a strong preference for the GESP procedure. The difference between the GESP and clairvoyance means of the 627 subjects tested by this team was highly significant (6.27 versus 5.67; $t = 4.89$; $P < 10^{-6}$). Both the GESP and clairvoyance means differed significantly from chance. There were no significant relationships between ESP scores and either the mood or belief ratings, although the 22 most extreme skeptics scored significantly below chance on the basis of a one-tailed test (mean = 11.14; $t = 1.73$; $P < .05$), a finding which at least is consistent with the "sheep-goat" hypothesis. There were no significant sex differences or significant differences among the various classes tested.

It is concluded that the results of this experiment support the proposition that experimenter variables play an important role in ESP experiments.

PAPERS IV*

PSI "LEARNING" IN RATS: AN ATTEMPTED REPLICATION

J. W. Davis (Institute for Parapsychology, FRNM)

An experiment by Sandford suggested that rats improved their psi performance during the course of a run using a single-bar food reward reinforcement psychokinesis testing method with a chance hit probability of 1/10. In his study three animals were tested for five days, doing one run of 1000 trials per day. They showed no increase in scoring over days, but showed a significant increase from the first half to the second half of the run ($t = 2.43$, $df = 14$; $P < .05$, one-tailed). A second study using four animals essentially replicated this result. Since declines over the run have been noted as a common feature in human studies of both extrasensory perception and PK, and have also been noted in animal studies, Sandford's finding provoked interest, especially given his low chance hit probability and long run length. We attempted to repeat his experiment with mixed expectations regarding the outcome, initially anticipating four possibilities: (1) psi would manifest as an increase in scoring from the first half of each run to the second, confirming Sandford's work; (2) psi would manifest as a decrease in scoring from the first half to the second, a result in line with our own and others' experiences; (3) there would be overall evidence of psi, but no significant difference between halves; (4) no psi would be forthcoming, leaving the question of inclines versus declines open, but at least suggesting that the procedure is not highly replicable.

The apparatus and software used in our previous PK research at the Institute were used with modification of the chance hit probability to 1/10 and run length to 1000 trials. The operant conditioning chamber used a standard pellet

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feeder with a response bar to the left of the feeder opening, and two lamps on either side of the feeder above the bar positions. The random number generator used was a Schmidt noise diode type, similar to that used by Schmidt in his high-speed PK tests with humans. The RNG was interfaced with the pellet feeder such that the animals received food for a bar press only when the RNG had just generated a hit.

Six experienced female Long-Evans hooded rats were selected and food-deprived to approximately 85 percent of their former weight. Since in the test situation the animals would only be rewarded on the average about once every 10 trials, they needed to be trained over a number of sessions to continue bar pressing even though they were reinforced infrequently and on a random schedule. In the first session they were reinforced with every bar press, then reinforced on the average of once every other bar press (variable ratio two, abbreviated VR2), and then reinforced every third time (VR3). Then each had two to three sessions, each session consisting of a VR2 sequence, a VR3 sequence, and a VR5 sequence. These sessions were followed by a session of VR3, VR5, and VR10, a session of VR5 and VR10, and finally a session of VR10. Two of the animals would not respond under a VR10 schedule and were dropped from the study. The four remaining animals were then run for five runs each, one run per day with one or two days in between runs. A standard test of the random number generator was conducted before and after the series, and another randomness check using the software and apparatus actually used in the study was conducted with runs of 1000 targets, the total series length being four times as long as the experimental series. These three control tests were all nonsignificant, the software and apparatus test showing no overall deviations, declines, nor inclines across the 1000-trial runs.

One run of data was lost due to a computer failure during testing, and another terminated after 624 trials as a result of a power failure. Nineteen runs were therefore available for evaluation. They were divided into half-runs of 500 trials each, except for the 624-trial run which was divided into two 312-trial halves. An arc sine transformation was applied to the proportions of hits and a dependent t-test performed on the first half versus second half scores. Unlike Sandford's incline across runs, the scoring rate declined significantly from the first half of the runs to the second half, using the run as the unit of analysis ($t = 2.43$, $df = 18$; $P < .05$, two-tailed). In addition, total hits and misses for the

first and second halves of runs were summed over runs and animals in a two by two chi-square table. Here also the results were significant corrected for continuity (chi square = 4.00, df = 1; $P < .05$).

Some differences from Sandford's procedure should be noted. These were mostly caused by constraints of time and available resources. First, this study used experienced animals, whereas Sandford in his first study used naive animals. This could have made a difference, in spite of the fact that Sandford's second study used experienced animals and found the same incline. Second, the training schedule was slightly different because of the smaller hooded rats' reluctance to train to higher variable ratio schedules. Third, whereas Sandford used a 10-position electronic commutator advancing at a rate of 1000 steps per second, we "synthesized" a 1/10 probability function from a binary generator. None of these differences are substantial enough to suggest any obvious reason for a reversal. A two by two chi-square analysis of Sandford's data suggests an interesting possibility.

Comparing our percentage scoring rates with Sandford's, we noticed that they both straddled mean chance expectation of 10 percent, although in opposite directions: the deviations in the summed first half runs are 2 percent for our study and 3.6 for Sandford's; in the second half runs they are 6.8 percent for our study and 6.1 for Sandford's. The scoring rates appear as mirror images about mean chance expectation. Additionally, both studies show (though not significantly) more absolute deviations in the second halves of the runs. The two sets of data are therefore consistent if we consider the possibility of a simple directional shift between experiments with Sandford showing psi-hitting (an incline) and our study showing psi-missing (a decline). Both these results can be viewed from an experimenter effect perspective, with Sandford's expectancy leading to an incline and ours to a decline. A repetition of this experiment with three experimenters (or groups of experimenters) using an "incline expectancy" condition, a "decline expectancy" condition, and a "significant psi expectancy" condition might shed some light on the issue.

PSYCHOKINESIS IN AGGRESSIVE AND NONAGGRESSIVE FISH WITH MIRROR PRESENTATION FEEDBACK FOR HITS

William Braud (Mind Science Foundation)

Four experiments were performed which involved a new procedure for testing PK in animals. Aggressive tropical fish (Bettas and Cichlids) were given a 20-second mirror presentation (a positive reinforcer for these animals) as a feedback stimulus for hits on a Schmidt random event generator. The performance of these animals was compared with that of nonaggressive fish (catfish, tetras, and goldfish, for all of which the mirror was not reinforcing) and with the hit rate of the random generator during randomness tests conducted without the presence of fish in the apparatus. The mirror feedback stimulus was chosen because it was a positive reinforcer, rather than a punishing stimulus, which could be easily and automatically presented and terminated, did not require surgical intervention, and was not susceptible to rapid satiation effects.

The general procedure was as follows. The fish were maintained in their home tanks for one week before their first session in the apparatus. At the beginning of each run, a fish was introduced into the test tank and allowed to adapt to it for five minutes. During this pretest adaptation period, the random generator was turned off and the mirror was nonreflective. (Mirror reflectiveness or nonreflectiveness was accomplished by lighting shifts and the use of a one-way mirror.) After the adaptation period, a test run of 100 trials was initiated. The random generator was turned on and trials were run at a rate of one every 20 seconds. A hit resulted in a 20-second mirror presentation to the fish; a miss resulted in a 20-second non-mirror period. After starting the run, the experimenter (myself) left the room and did not return until a timer signaled the end of the 33.3-minute run. Hits and misses were recorded from the counters, and the fish was returned to its home tank.

In the pilot experiment, four aggressive fish and four nonaggressive fish were tested for one run (100 trials) on each of four days. In the first confirmation experiment, 10 aggressive and 10 nonaggressive fish were tested under an identical experimental protocol. In the second and third confirmation experiments, 10 aggressive and 10 nonaggressive

fish were tested for one run (100 trials) on one day, again using the same procedure.

In the pilot experiment the aggressive fish exhibited significant psi-hitting on Day 1 only, and a decline effect over the four test days which was significant for Day 1 versus Day 4. The first confirmation experiment also showed significant psi-hitting on Day 1, but the Day 1-Day 4 decline was not significant. The aggressive fish of the third confirmation experiment exhibited significant psi-hitting on their sole day of testing. The aggressive fish of the second confirmation experiment exhibited chance performance. The performance of the nonaggressive fish in each of the four experiments and the scores of the random generator on 20,000 randomness test trials distributed nonsystematically throughout the time period of the experiments never differed from chance expectation. Collapsing across experiments and combining all Day 1 scores (Day 1 procedures were identical for all fish) yielded significant psi-hitting for aggressive fish (CR = 2.81; P = .0025, one-tailed), chance performance for the nonaggressive fish (CR = 0.20), and a significant difference between aggressive and nonaggressive fish (CRd = 1.84; P = .033, one-tailed).

A "consistency test" was done by comparing the number of fish exhibiting psi-hitting with the number expected by chance. On the basis of Day 1 scores, of the 34 fish tested, 22 were hitters (scored above chance), 8 were missers (scored below chance), and 4 scored at chance. This distribution yielded a CR of 2.56 (P = .0052, one-tailed). Thus the psi hits were contributed by the subjects as a whole, not by only a few "gifted" animals.

Under the conditions of these experiments, a moderate amount of psychokinesis emerged. The obtained PK effect was small in magnitude (2 percent), but fairly consistent. The observed PK decline effect is probably contributed by a decline (habituation) in the reinforcing or incentive value of mirror-stimulation over the successive trials of the experiment. It is, of course, not certain that the animals themselves were the active agents in these experiments. Although the experimenter attempted to remove himself from the experiment as much as possible, it is still possible that he exerted some psychic influence on the results.

LONG-DISTANCE DREAM AND PRESLEEP TELEPATHY

William Braud (Mind Science Foundation)

Three experiments were conducted to explore the feasibility of conducting long-distance dream telepathy and hypnagogic telepathy experiments by mail, to compare directly the relative effectiveness of dream and hypnagogic mentation as vehicles of psi awareness, and to explore the usefulness of the new binary target system developed at Maimonides Medical Center in these experimental contexts [see RIP 1974, pp. 112-5].

Fifty subjects, most of them friends and acquaintances of mine, participated in the pilot investigation. Most of these percipients lived in Houston (a distance of 333 kilometers from the Mind Science Foundation) and other Texas locations; a few lived outside the state. A letter was mailed to each person, describing the experiment in detail and requesting participation. At 2:00 a. m. on the specified date the agent (myself) viewed a slide, randomly selected from the 1024 slides of the Maimonides binary target pool, for a 30-minute period. Percipients recorded their dreams during that night and early morning and coded the dream content in the 10 binary target categories on special forms which they mailed to me. A subject's psi score consisted of the number of matches of his or her impression (dream) code with the code of the actual target slide; five out of a possible 10 matches were expected by chance. Of the 50 scores, 43 were misses (fewer than five matches), three were hits (greater than five matches), and four were at chance (five matches); the mean score was 3.14 matches. Comparing the majority vote binary code with the target code yielded two matches, a deviation of -3, with a CR of 1.90 ($P = .057$, two-tailed).

Certain negative psychological factors which could have contributed to the apparent psi-missing of the pilot study were eliminated in the first confirmation experiment. These included an impersonal, unexpected request to participate; the fact that 40 percent of the percipients were complete strangers to me, and had been invited to participate by my friends; the possibly inappropriate 2:00 a. m. target exposure time. The target exposure time was changed, therefore, in the first confirmation study; only 10 close friends (selected on the basis of presumed interest and dedication) were asked to participate; the letters requesting participation were made more personal;

and each subject was asked to participate in a series of trials consisting of attempts to become aware of six different targets to be exposed on three consecutive days. Three targets were to be exposed early in the morning (5:30 a. m.) while the subjects were likely to be dreaming, and three were to be exposed while the subjects were awake but relaxed, just before sleep when they were likely to be entering a hypnagogic state (10:00 p. m.). Target exposure was 30 minutes in each case. Subjects recorded dream and hypnagogic content and coded and mailed the forms as before. Five of the 10 subjects returned their protocols; the remainder forgot the date or were out of town during that period. For the five participants, there were 19 hits, five misses, and six chance scores. The mean majority vote score for the six targets was 6. 83, yielding a significant CR of 2. 84 (P = . 0045, two-tailed).

The same 10 persons were asked to participate in the second confirmation experiment. The procedure was identical to that of the first confirmation with the exception that target exposure times were changed to 6:00 a. m. and 10:30 p. m. Seven of the 10 subjects returned protocols; three subjects could not participate. Three of the seven returned incomplete protocols. Of the 36 protocols received, 19 could be classified as hits, nine as misses, and eight as chance scores. Computation of majority vote scores yielded a mean of 6. 33 and a CR of 2. 07 (P = . 019, one-tailed).

Since the results of the two confirmation experiments were comparable, the data were combined to yield six majority vote presleep and six majority vote dream scores. The mean presleep score was 7. 33; the mean dream score was 5. 83. The difference between these two sets of scores was not significant. However, the six presleep scores were independently significant (CR = 3. 62; P = . 00015, one-tailed), while the six dream scores were not (CR = 1. 29). Overall, four subjects performed consistently well in the confirmation experiments and obtained significant CRs (ranging from 2. 07 to 3. 16). Four subjects produced overall chance results. Two subjects missed both confirmation series. If the majority vote scores for the two confirmation series (which yielded almost identical results) are combined, a mean binary target score of 6. 58 is obtained. This score is associated with a CR of 3. 47 (P = . 00052, two-tailed) and a Psi Quotient of 100.

SENDER RELAXATION AND EXPECTATION IN TELEPATHY

Loralu Raburn and Ray Manning (Tulane University)*

The present experimental design combined Ganzfeld stimulation of the subject with a deeply relaxed state in the agent in an effort to determine the combined effects of these states on psi performance. The design involved three experimenters, one of whom served as agent. Forty subjects were drawn voluntarily from students enrolled in Tulane University's introductory psychology classes. None of the subjects knew any of the experimenters or agents before the experiment. A Ganzfeld type of sensory deprivation similar to that used by Honorton was employed. White noise was fed to the subject's ears by stethoscope in order to facilitate blood flow monitoring. The additional physiological parameters of heart rate and galvanic skin response were monitored as well. These data were taken to determine roughly when the subject was in the desired hypnagogic state. Subjects were requested to "think out loud." Their verbal responses were recorded on tape and jotted down by an experimenter.

The 40 subjects were placed randomly in one of four groups of 10. Two of the groups were uninformed as to the nature of the experiment. They were led to believe that the experiment was merely an attempt to elucidate physiological functions associated with sensory deprivation. Instructions were read to these subjects which requested that they describe thoughts or feelings if or when they occurred. The other two groups, the informed subjects, were further told that at some point during the session an agent in another room would transmit a message to them and that it would appear in their consciousness at the appropriate time. This was termed the "expectancy" factor. For one of the informed and one of the uninformed groups there was in fact no agent sending a message. For the other group in each category an agent was employed. This was termed the "agent" factor. All informed subjects were told that an agent would be involved, whether or not one was actually used.

The message or target material was a View Master slide reel selected at random from a pool of 24. Selection

*Presented by R. Winnett.

was made using 24 poker chips, shaken together beforehand. On each chip was written a two-digit number drawn from a random number table. Using tactile information Experimenter 2 selected four chips, separating one from the rest to serve as the target. All four were placed with the numbered side face down. In this way, the experimenter had no knowledge of which numbers had been chosen. Three chips were placed in a stack on a desk in the room, and the target chip was laid next to the stack. After finishing the selection, Experimenter 2 returned to the monitoring room for the remainder of the session. The door between the outer room, where the chips and slides were located, and the monitoring room, where Experimenters 1 and 2 recorded the subject's responses, was closed except for brief moments when Experimenter 2 was entering or leaving.

Monitoring of the subject's physiological levels and verbal responses enabled Experimenters 1 and 2 to determine when the hypnagogic state had been attained. The induction period ranged from five to 15 minutes. Experimenter 2 informed Experimenter 3 when chip selection was completed. Experimenter 3 then entered the room with the slides as Experimenter 2 returned to the monitoring room. Experimenter 3 looked only at the target chip and selected the appropriate slide reel. He then proceeded to self-induce a very relaxed state of body and mind. All experimenters who acted as senders were graduates of Silva Mind Control International and had been trained to produce a state of extreme relaxation. After attaining this state, Experimenter 3 began to view the target reel. He concentrated on projecting the content of the reel to the subject. The sending period lasted three to seven minutes. For those subjects without an agent, Experimenter 2 merely selected the four chips, setting one of them aside. There was no target material.

While the subject was being removed from the Ganzfeld apparatus by Experimenter 1, Experimenter 2 reentered the outer room and selected four slide reels which corresponded to the numbers on the four poker chips. Experimenter 2 then left the room. Neither Experimenter 2 nor Experimenter 3 had any contact with the subject during and after Ganzfeld stimulation. Experimenter 1 led the subject into the outer room. He then handed the subject each slide in turn, with instructions to select the reel "which most closely matched any of the thoughts, feelings, and/or images which had been experienced while under sensory deprivation, without regard to logical reasoning on a conscious level." A

hit was defined as a correct first choice selection of the target reel; all other choices were taken as misses. Chance probability of getting a hit was one-fourth. After the subject had made his selection, Experimenters 2 and 3 returned to the room to verify the order of the reels and the correct target.

A comparison of subjects' physiological readings drawn from periods during which an agent projected target material, periods during which target material was selected, and periods wherein no such activity on the part of the experimenters occurred, showed no significant differences. Two statistical tests (Fisher's Exact Probability test and chi square) were employed to test pairs of categories constituting all possible combinations of the simultaneous presence or absence of the two variables, in addition to two overall groupings formed to test the relative strength of the two major variables by themselves. Both the agent and expectancy factors significantly affected telepathic success. There were relatively more hits when an agent was actually sending a target, and relatively more hits when the subjects were informed of the telepathic nature of the experiment. The agent-no agent effect was concentrated in the informed groups.

PAPERS V*

UNCONSCIOUS PERCEPTION OF EROTIC, NON-EROTIC, AND NEUTRAL STIMULI ON A PSI TASK

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Virtually all of the research in parapsychology has focused on the role of psi in conscious experience. If psi functions in a manner psychologically similar to other perceptual systems, it should also be important in unconscious perception. In a previously reported study I found that the participants responded differentially to erotic and neutral stimuli of which they were consciously unaware. The present study restated the question asked in that study: Can people respond differentially to stimuli of which they are consciously unaware when the stimuli cannot be consciously or unconsciously perceived by the classical perceptual systems? Additionally, it asked a new question: What is the motivational nature of the response to the stimuli perceived unconsciously? Do the participants prefer erotic (drive-reduction motivation) material or material which has been selected to satisfy exploratory-based motivation?

There were four researchers involved in the study: me, two undergraduates, and an associate professor of psychology. The participants were 22 students (11 males, 11 females) enrolled in an introductory psychology course. They had volunteered for a parapsychological study involving psi and relaxation. They never learned the true nature of the study. We insured equal groups for each sex and controlled for sex differences due to order effects.

The targets were 60 standard ESP cards, 12 of each symbol. These cards were paired with one of three types of stimuli: erotic, non-erotic, or neutral. The erotic stimuli

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consisted of 20 pictures from magazines such as Penthouse and Gallery. Ten pictures were of nude or almost nude women; 10 pictures were of both a man and a woman, both nude or almost nude. Half were color; half were black-and-white. The non-erotic stimuli consisted of 20 pictures (half color, half black-and-white) which were selected according to properties such as novelty and curiosity. The neutral stimuli were blank sheets of paper of comparable size and weight as the erotic and non-erotic stimuli. No one had conscious knowledge of what stimulus or ESP symbol was in each of the 60 envelopes. After the randomization procedure, no one had conscious knowledge of which envelopes contained the erotic stimuli, the non-erotic stimuli, and the neutral stimuli. Participants did not receive feedback until all participants had completed the experiment. The two undergraduate assistants were not told about the variables involved or the use of the erotic and non-erotic stimuli.

We also used a psi questionnaire to control for individual differences in attitude toward psi. We used a taped relaxation procedure to prepare the participants for the psi task and the State-Trait Anxiety Inventory to measure trait and state anxiety. Experimental sessions occurred approximately one hour apart in the afternoon and evening over a three-day period. Upon arrival the participant completed the psi questionnaire, the anxiety trait inventory, and the first of three anxiety state inventories. The researchers then explained the procedure in detail, dimmed the room lighting, and started the relaxation tape. The tape led directly into the psi task. During the psi task, the researcher placed each of the envelopes one at a time behind the participant. After the participant made each call, the researcher recorded it, removed that envelope, and positioned the second envelope. At the completion of the psi task, the participant completed two anxiety state measures, one for how he felt at the end of the relaxation tape and prior to the psi task, and one for how he felt at that moment. When the participant had finished, the researcher obtained any subjective feelings or impressions the participant had during the psi task. After this, the researcher thanked the participant for his participation, and gave him a post-experiment questionnaire and a stamped envelope so that he could provide anonymous feedback to the researchers informing them as to what he thought of the experiment and his treatment.

The results suggested that the participants responded differentially to stimuli of which they were consciously un-

aware and which they could not consciously or unconsciously perceive via the classical perceptual systems. F-tests indicated that the variance associated with the non-erotic targets was significantly higher than the variance associated with the erotic targets ($F = 3.61$, $df = 19, 19$; $P < .02$, two-tailed), and higher than the variance associated with the neutral targets ($F = 2.65$, $df = 19, 19$; $P < .05$, two-tailed). Analysis of the number of correct calls on individual targets further supported the hypothesis. For 11 targets, the number of hits was significantly different from chance expectation. Five targets exceeded chance expectancy separately for either males or females, or both. All of these five targets were non-erotic ($P < .01$). Of the six targets which were hit more often than would be expected by chance all were pictures, one being erotic and five non-erotic. While no definitive conclusions can be made concerning the motivational nature of the responses to stimuli perceived unconsciously, the results provided more support for exploratory motivational theories than for drive-based, need-reduction theories.

Overall psi scores, as expected, indicated no significant results. They ranged from four to 14 (chance expectation = 12). Analysis of anxiety-trait scores and psi scores using male and female data collectively indicated no significant relationship. The anxiety-trait scores for the female participants were not normative. As reported by other researchers, no significant effect of color was found. Additional analyses were accomplished to check for effects by the controlled extraneous variables. It was found that those participants who reported the largest decrease in state anxiety scored significantly higher on the erotic targets (Mann-Whitney $U = 19.5$, $df = 11, 10$; $P < .02$, two-tailed). The study suggests that researchers investigating psi and anxiety might benefit by using measures of both state and trait anxiety. The use of both measures might aid in clarifying conflicts in the literature.

One observation should be noted. In the current study four participants (one male, three female) either called "triangle" or reported strong impressions of triangles. I had found this also in my earlier study. Discussions with the participants indicated that they understood that triangles were not among the symbols. Examination of the room suggested no environmental cues which should have solicited impressions of triangles. One possible explanation is that the participants were responding through symbolic interpretation to the presence of the erotic stimuli, the triangles being feminine sym-

bols. Another possible explanation is that the participants may have been responding to the presence of triangular patterns in the erotic or non-erotic stimuli.

OPENNESS VS. CLOSEDNESS AND ITS RELATIONSHIP TO PSI

Lendell Williams Braud (Texas Southern University)*

The present study was initiated in order to develop an instrument designed to measure openness vs. closedness and its relationship to psi. The term openness refers to both the opening up and taking in of experiences (input) and the opening up and letting out or self-disclosure of aspects of ourselves (output). An 88-item questionnaire was developed consisting of a number of scales and subscales related to these concepts, and two experiments were done to refine the questionnaire and assess the relationship between scores on it and on a psi task. The first of these, the pilot study, was reported at the 1975 Parapsychological Association convention [see RIP 1975, pp. 155-9, for a discussion of the rationale and composition of the questionnaire]. Briefly, there were three major scales: Openness to unusual experiences and altered states of consciousness (Scale I); Openness and tolerance of different attitudes, cultures, races, religions, nationalities, political and economic views, behavior and change and flexibility (Scale II); and Openness to aspects of the self, non-defensiveness and a willingness to disclose aspects of the self with relevant others (Scale III). A fourth scale, Introversion versus Extroversion, was included even though it was thought not to relate to the concept of openness.

There were 32 black undergraduates included in the pilot study and 68 black undergraduates included in the confirmatory study. The majority of the subjects were students in my psychology classes at Texas Southern University. All subjects were given the questionnaire to fill out one week before the experiment. One day prior to the experiment, a student (P. H.) prepared the targets for use. Thirty-two large numbered opaque packs were used. Each pack contained four smaller numbered opaque envelopes, each con-

*Presented by Gertrude R. Schmeidler.

taining a different picture. P. H. randomly chose one of the four picture packs (without viewing the pictures). This small envelope was placed in the larger envelope and the remaining three alternative control pictures were set aside, in their envelopes, for later judging. P. H. never saw any of the target pictures, only their envelopes.

Thirty minutes before the experiment started P. H. entered a small room adjacent to the classroom and set up the targets and alternatives for use. The subjects then entered the classroom and were given brief instructions concerning the experiment and the relaxation tape to be used. Each student completed our standard belief, mood, and state form (16 questions) to assess other variables that might affect ESP performance. Then P. H. slipped the box of targets through the door between the rooms without coming into contact with any of the subjects or other experimenters. A target pack was randomly assigned to each subject. The large pack contained the target picture enclosed in the smaller opaque numbered envelope. The subject was not allowed to open the larger envelope, but was allowed to hold the target pack during the impression period. Each subject had a separate target; the task was considered one of clairvoyance.

The lights were dimmed and a 45-minute, psi-conducive tape was played. The tape included relaxation exercises, autogenic exercises, music, natural sound effects, meditation, imagery, mind-blanking and other "right hemispheric" activities. Three experimenters monitored the class to make sure that no subject opened his or her pack. A five-minute clairvoyant impression period followed the tape. Then the lights were turned up and each subject recorded his impressions. The target packs were slipped back through the door to P. H. without coming into contact with her. She placed the three numbered alternative control envelopes back into the appropriate larger envelope along with the target envelope. The placement of the smaller envelopes was always in numerical order within the larger envelope. This procedure was completed for each large pack and then the packs were again slipped back through the door without contact. Each subject received his or her large pack, opened the pack and ranked the pictures from one to four on their correspondence to his or her impressions. The subjects also rated each picture on the degree he or she liked the picture as a picture. The experiment was then concluded.

A rating of 1 for the target picture was a direct hit,

while a rating of 4 was a direct miss. Ratings of 1 and 2 were combined to yield the number of hits, while ratings 3 and 4 were combined to yield the number of misses. Therefore, the probability of a direct hit was one-fourth and the probability of a hit was one-half. The psi-conducive tape appeared to be effective. Significant psi-hitting was demonstrated in the confirmatory study (CR = 2.183; P = .015, one-tailed) and total sample (CR = 2.60; P = .0047, one-tailed). The results of the pilot study were suggestive but not significant (CR = 1.414). In addition, an analysis of the direct hits for the total sample yielded significant results (CR = 2.54; P = .0055, one tailed).

Mann-Whitney U scores were calculated for each scale and subscale comparing psi-hitters with psi-missers. Z scores were calculated so that exact probabilities could be determined. This would allow me to determine which scales looked promising but needed further refinement. All scales were in the predicted direction for both pilot and confirmatory studies (greater openness was related to psi-hitting), except for the Altered States Scale on the confirmatory study, which went slightly in the other direction. In the pilot study, the Introversion-Extroversion Scale was significant (P = .002), with extroverts scoring better than introverts. This difference was not found in the confirmatory study. No significant differences were seen on any of the scales on the confirmatory study. The total sample yielded significance on the total Scale III (P = .028) and on a subscale of III which included items from the Jourard Self-Disclosure Inventory (P = .018).

At this point in time, lack of significance on the scales is not an important factor as long as a pattern of consistently good items appears. The goal of the study was to determine which items should be included on the final questionnaire. Data on the items for all 100 subjects indicated that 70 percent of the items discriminated between psi-hitters and psi-missers in the predicted direction. Also, 74 percent of the items discriminated between direct hitters and direct missers in the predicted direction.

Therefore, the items were analyzed to determine which discriminated best. The mean for psi-hitters and psi-missers was calculated for each item and the difference obtained. Likewise, the mean for direct hitters and direct missers was calculated for each item and the difference obtained. These two differences were combined to yield a composite mean difference. These were ranked to determine the best 40 items on the

questionnaire. The item analysis indicated that the introversion-extroversion questions that did so well on the pilot study did poorly on the confirmatory study. Most of the other best items showed a consistent pattern in the pilot and confirmatory studies. The 40 best items were well distributed among all the scales.

CHANGES IN ESP SCORES AFTER THERAPY SESSIONS

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and Gertrude R. Schmeidler† (City College, CUNY)

This study involved a single subject, conscientious and highly motivated, who was a patient in analysis and who filled out a mood scale and an ESP test both before and after 24 therapy sessions. The purpose of the investigation was to find if there would be a relation between change in self-reports and change in ESP scores from before to after the sessions, and if so, whether the findings could lead to insights about the clinical process, the structure of the subject's defenses, and the psi process.

A variant of the Nowlis Mood Adjective Check List provided a 10-point scale from Definitely True to Definitely Not True for each of 30 items. They described 11 moods: aggression, anxiety, concentration, depression, egotism, fatigue, impatience, skepticism, social affection, surgency, and vigor. Scores were summed for each mood, and some of the scores were prorated so that the possible range for each mood was 0 to 27. Scores of 12 to 15 were in the neutral or "can't say" range.

Each ESP target and response sheet was a page of 20 circles. Each circle had 12 ticks at the conventional positions for a clock face. An assistant prepared the targets by drawing one "hand" into each clock face, and followed the order of a random number table to determine hand positions. The target page was wrapped on both sides with aluminum foil and put into a manila file folder. The folder was placed into a manila envelope so that the target sheet faced the front. The manila envelope was sealed and taped, a mood check list was pasted on its back, and a blank response sheet pasted on its front. The order of the envelopes was randomized. Scoring of records was blind, and was done indepen-

dently by a research assistant and by G. R. S. A "difference score" of zero was assigned to a direct hit, a difference score of one to a response which was one tick mark from the target in either direction, and so on to the maximum difference score of six. Mean chance expectation was 60 for each sheet, with lower scores showing more success. The theoretical expectation of the sum of variance scores for each sheet was 63 1/3.

Between January 27 and May 5, 1975, the subject completed 24 sets of before-and-after mood and ESP records. Each of the 24 sessions provided one ESP difference score, one ESP variance score, and 11 mood scores for the pre-session and another such set for the post-session. A computer was programmed (a) to generate a third set of scores for each session, taking post- minus pre- difference scores, post- minus pre- variance scores, and post- minus pre-mood scores; (b) to obtain the intercorrelations of the scores; (c) to obtain the multiple correlation of ESP difference scores with the 11 moods; and (d) to obtain the multiple correlation of ESP variances with the 11 moods, for each analysis.

ESP difference scores were insignificantly worse than chance expectation both before and after therapy sessions. Before therapy the mean ESP variance was 62. 8, insignificantly lower than the expected value. After therapy, however, the mean variance was 54. 5, significantly lower than the value expected by chance (if all sessions are pooled, CR = 3. 19; $P < .002$, two-tailed). The variances after the sessions were also significantly lower than the variances before the sessions ($t = 2.225$, $df = 22$; $P < .05$, two-tailed). The low variance was due chiefly to avoidance of direct hits (difference scores of 0) and secondarily to avoidance of direct psi-missing (difference scores of 6). All correlations between ESP difference scores and ESP variance totals were negligibly different from chance. Further, the range of difference scores for the whole page of calls was equivalent before the therapy session and after the therapy session. All this indicates that the change in variance deserves separate examination from change in general tendencies toward ESP hitting or missing.

Moods, on average, showed no major change from before to after therapy. Certain moods, however, were likely to have big swings in one direction or the other, while other moods were not. The multiple correlation between change in

ESP difference scores and change in mood was $R = .84$ ($F = 2.62$, $df = 11, 12$; $P < .057$), and seemed to justify further study. Of the 11 simple product-moment correlations between changes in difference scores and changes in each of the moods, five would be significant if examined separately; they indicate that ESP scores were better when the subject felt more aggression, impatience, and skepticism, and felt less depression and social affection. Examining the 55 correlations between pairs of mood changes, we found that aggression, impatience, skepticism, and lack of social affection coalesced to form a single mood pattern. Depression, the other rating scale which gave a high correlation with difference scores, was clearly independent. This implied that ESP success was less when the subject felt more depressed, and was greater when he felt hostile.

The data raise two questions. Why was there such marked avoidance of direct hits, and perhaps a tendency to avoid direct misses, after the therapy sessions? Why, in the context of psychoanalytic concerns, was there a tendency toward greater ESP success in a skeptical, nonaffiliative mood? To answer these questions, we must note that a negative transference was in effect at the time of the experiment, and we must discuss the subject's ego structure.

The subject is a relatively well-functioning and well-defended person who at the time of the experiment was employing the defenses associated with an obsessive personality. At the time the protocols were being filled out, there was a prolonged period of suspiciousness and hostility toward the therapist. Seen in this context, the results seem to make good sense. Impatience, aggression, and skepticism are generally associated with obsessive defenses; and when the subject's use of these mechanisms was stronger, his ESP success increased. These mechanisms often also serve to defend against depression (which has since developed in the subject). Both these mechanisms and the negative transference lead to a marked lack of social affiliation in the therapy situation. Thus when the defensive structure was operating most effectively and guarding against depression, ESP scores were better. The highly significant drop in variance after the therapy sessions supports this interpretation. The drop in variance indicates a marshaling of defenses as a result of the session. Put in clinical terms, at this point in the therapy the subject's resistance was increased by the threat of the therapy session and material. Thus at this period, it was a mobilization of the obsessive

defenses that represented success to the subject--and correlated with success in his ESP scores.

If this interpretation is correct, the pattern of mood change and ESP change varies both with the stage of therapy and the defensive structure. A different pattern of mood change would correlate with ESP success at a different stage of this person's therapy. Further, a subject who uses hysterical defenses and is extremely labile in mood swings would be expected to show both a more marked variation in ESP scores and also a very different pattern of mood in relation to ESP scores. Successful prediction of mood-ESP relationships thus depends on assessment of the situational demands, the dominant ego defenses employed by the subject, and clinical wisdom as to the mood changes associated with shifts in these dominant patterns of defense.

On this hypothesis, let us next consider what the data imply, first for the therapist-patient relationship and next for parapsychological research. The low variance of ESP scores after the therapy sessions seems a clear (though subtle) indicator of defensive withdrawal as a result of the sessions. It might well raise the following questions for the therapist. Is this a healthy response, evidence that the patient's affect was directed to stabilizing his defenses? Might it alternatively be taken as a healthy indication that the patient directed his affect to working through the material uncovered during the therapy session, and that this resulted in his withdrawing his affect from the ESP task which occupied his conscious attention? Or instead is it an unhealthy response to therapy, showing that he has been kept from the free exercise of his abilities; that his defensiveness has spread to other tasks and made his overall work performance withdrawn, inhibited, less effective? The short-term effects and long-term utility of depth therapy need to be examined in the context of performance in other affairs, of which ESP is one example.

From the diagnostic point of view, we should also note that the particular correlational pattern of ESP change with mood change does more than serve as confirmation that this patient's defensive structure is of the obsessional type. It additionally indicates that for him depression leads to poor or negativistic performance, rather than acting as a challenge which mobilizes counteractive tendencies. In short, the ESP scores seem usable as a quick projective test, which provides meaningful information.

From the parapsychological point of view, the most important new finding is that a significant change in variance did not correlate with change in ESP success. There seems a strong possibility that scores for variance and scores for success give complementary information, the former indicating the level of global investment of affect in the task, the latter indicating rapid shifts in affect or cognition. We may need both kinds of information to understand what brain or other body changes relate to effective use of ESP. A second parapsychological point, as stated earlier, is that studies of the relation between mood and ESP score should if possible examine the subject's defensive structure, then find if the defenses serve as a modulating variable between mood and ESP. This might be especially relevant to investigations of psychics or gifted subjects. Perhaps conclusions from their clearcut data are properly generalizable to others who have a defensive structure similar to theirs, but are not generalizable to an unselected sample of the population.

PAPERS VI*

THE APPLICATION OF SIGNAL DETECTION THEORY TO CLAIRVOYANCE AND PRECOGNITION TASKS

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The typical forced-choice ESP experiment requires that a subject discriminate the ESP targets one from another; i. e., that he recognize which of the targets is present on a given trial. In this study we were concerned not only with recognition, but also with detection. In other words, can a subject detect whether a target has or has not been presented regardless of whether, if one has, he can recognize which specific target it is? By including trials without a target (blank trials) and asking the subject to state whether or not a target was actually present, as well as identifying the target, a measure of sensitivity (d') and an independent measure of criterion or response bias (beta) can be determined. Sensitivity, or d', is a measure of ability to discriminate the presence of a signal from its absence. Beta (criterion or response bias) is a measure of how stringent is one's criterion for being willing to judge that a signal, as well as noise, is present. It may be that a subject can determine that a signal is present but cannot recognize what that signal is.

In order to investigate the range of applicability of this technique and to study possible influences of temporal factors, unknown to the subjects, both clairvoyance and precognition tasks were included. Finally, since subjects might have felt uncomfortable in first guessing what target was present and then stating whether a target was actually present, an alternative measure, a confidence rating, was also used with half the subjects. In this condition, low confidence

*Chairperson: W. G. Braud, Mind Science Foundation.

was to be treated as a "no" response and high confidence as a "yes" response.

Twenty-six male and 26 female college students, all unpaid volunteers between the ages of 18 and 28, served as subjects. Each subject received a packet of materials containing target sheets with an anti-cheating device, answer sheets, and instructions. The target sheets consisted of 400 targets recorded on two sheets of paper stapled back to back; there were 200 clairvoyance and 200 precognition trials. In the clairvoyance condition 100 targets were used with either the symbol "+" or "o" and 100 were left blank. In the precognition condition all 200 spaces were left blank except for a small dot in the upper right corner. After completion of the entire experiment, 100 of these spaces were assigned a "+" or an "o." All assignments of clairvoyance or precognition, symbol or blank, and "+" or "o" were randomly and separately assigned for each subject on the basis of thoroughly shuffling index cards. A one-half-inch square of photographic paper, which had previously been exposed to a pattern known only to the preparer, was stapled to the target sheets in a darkroom. The sheets were then wrapped in aluminum foil and inserted into a manila envelope which was then sealed. Each subject received two answer sheets laid out in exactly the same way as the target sheets.

Two sets of written instructions were prepared, with half the sample given one set and the other half the other set. In the "yes-no" procedure, subjects were told that 40 to 60 percent of the targets would be blank and the purpose of the experiment was to make the distinction between detection and recognition. On every trial they were to mark "+" or "o" and also to mark "yes" if they felt a target was actually there and "no" if they felt it was not. In the "rating" procedure, subjects were told only to mark "+" or "o" on every trial and to rate their confidence from 0 (no confidence) to 3 (very confident). In neither case was the precognition portion of the study mentioned. In both cases subjects were encouraged to work at their own pace when they felt in the mood and not to stick to a rigid schedule. Ten days were allowed for completion and subjects could take their materials home to work with them.

No evaluation was done until all materials had been returned. The envelope was opened in a darkroom and the photographic paper developed. No evidence was found that any subject had opened an envelope. At this point in the

study the precognition targets and precognition blanks were randomly assigned. For both "yes-no" and "rating" conditions the number of correct choices was tabulated separately for the clairvoyance and precognition trials. The signal detection analysis was based on the proportion of times the subject reported a target was present when it actually was present (hits) and the proportion of times he reported it present when it was not (false alarms). This was done independently of whether a correct or incorrect choice had been made with respect to "+" or "o." Under the "yes-no" condition, determination of hits and false alarms is obvious. Under the "rating" condition, a 0 or 1 rating was considered a "no" response, while a 2 or 3 rating was considered a "yes" response. Separate measures of sensitivity ($\underline{d}'$) and criterion (beta) were determined for each subject, based on the relative proportions of hits and false alarms.

The number of hits (in calling "+" or "o"), $\underline{d}'$, and beta for males and females tested under the two conditions ("yes-no" or "rating") and the two ESP tasks (clairvoyance and precognition) were determined. Hit-rate (on symbols) clustered around mean chance expectation. An analysis of variance revealed a significant interaction ($F = 4.51$, $df = 1, 48$; $P < .05$) between sex and type of ESP task. An analysis of the simple effects indicated that for the females significantly fewer hits were obtained under the precognition condition than under the clairvoyance condition.

The $\underline{d}'$ data for the "yes-no" procedure were all basically zero, indicating no differentiation between target present and target absent. Under the "rating" procedure, on the other hand, the $\underline{d}'$ data indicated some discrimination, but in the negative direction. Subjects were more likely to report confidence when a target was not present than when it actually was present. An analysis of variance verified this. Procedure ("yes-no" versus "rating") emerged as a significant main effect ($F = 7.48$, $df = 1, 48$; $P < .01$), with the "rating" procedure producing a larger (more negative) $\underline{d}'$ than the "yes-no" procedure. Using the t test for a single mean, it was found that with the "rating" procedure both clairvoyance ($t = 2.99$, $df = 25$; $P < .01$) and precognition ($t = 3.58$, $df = 25$; $P < .01$) conditions showed a mean $\underline{d}'$ significantly different from zero. Analysis of the beta scores (response bias) indicated no differences among conditions. This means that none of the experimental manipulations influenced the readiness of subjects to judge that a signal was present.

Signal detection analysis may have utility in ESP studies. In this study subjects who used the "rating" procedure could, to a statistically significant degree, discriminate the presence of a signal (ESP target) from its absence even though they systematically confused the two and tended to indicate a higher confidence when a target was not present than when it was present. Thus they could, in a sense, detect the presence of a signal even though the results of their forced-choice calling indicated that they could not reliably recognize its nature.

AN EXPLORATORY STUDY ON PSI-BASED SUBJECT AND EXPERIMENTER EXPECTANCY EFFECTS

Richard S. Broughton (University of Edinburgh)

The recent mathematical and quantum mechanical models of how psi works, by Schmidt and Walker, may go some distance in sorting out the problem of psi-based experimenter effects. A critical feature of these models is the feedback to the subject, without which there is no possibility of the subject's using his psi to influence the results. It would seem that, for the psi source in such situations, the conditions obtaining at the time the targets are generated or the test performed are not as important as the conditions which obtain at the time feedback is delivered. The implications of these models for experimenter effect are clear, and both theorists have pointed out that in all experiments there is always one certain recipient of feedback--the experimenter. Herein lie suggestions for a new methodology for the investigation of psi-based experimenter effect, and indeed perhaps the source of all psi effects, based on careful manipulation of feedback to all possible psi sources, particularly the subjects and the experimenter. Such a methodology would have to test these models in the process, and the present study seeks to make a start in that direction.

The experiments, comprising a pilot and a confirmatory series, concerned a manipulation of the subject's expectancy in a disguised ESP test. The test was presented as a study of how artifacts get into experiments. The unique feature was that the expectancy manipulation did not take place until after the subject had completed the experimental task. In a sense, the subject could be viewed as a mini-

experimenter, kept blind to the data until the end, with the added twist that he truly had no expectancies until the end. The task for the subject was a "random guessing test" in which he was to guess which of four unmarked buttons was the target. The subject listened to white noise during this task, and before the experiment proper an attempt had been made to lead him to suspect that the "artifacts" might be auditory in nature, embedded in the white noise. The only purpose of the noise was to support the cover story. There were two runs of 32 trials each; nothing was said to the subject in advance which would in any way lead him to suspect a difference between them. It was explained that as a means of keeping the experimenter blind, the subject would receive a computer printout with his scores and a message explaining the experiment in a little more detail, but the results must not be communicated to the experimenter. In this computer note, delivered at the end, the subject was given a plausible reason to expect one of the runs to be above the mean chance expectation of eight, and the other to be below.

A second aspect of these experiments controlled feedback to the subject. For half the subjects the scores reported on the printout were their actual run scores, while the remaining half received the results of concurrently run pseudo trials. This was to examine whether the psi effect was directed at the designated task or at the reported results of an unknown task. The experiments did not make use of what might be considered optimum feedback, trial by trial, but instead provided only end-of-run feedback to the subjects. For the experimenter, feedback was reduced to a minimal level, in that all he saw were the results of preplanned statistical tests. At no time did he ever see raw scores or know which condition a subject was in. It was a possibility that even this reduced feedback might prove a sufficient psi trigger for the experimenter, but at this point it seemed reasonable to expect the subjects' influence to predominate.

The main hypothesis to be tested in the pilot study was that scores in an ESP test could be manipulated by overt suggestion of differing expectancies, but in this case the effect was time-independent in that the manipulation came after the test. Additional tests were done to examine whether the subject directed his efforts to the task at hand or just to the reported scores, and if it was necessary to see the scores to have an effect.

The experiment was computer controlled and modulo-4 targets were generated by a software routine sampling a noise-driven binary random number generator twice. The subject was told that the computer would select a target button and then light a "guess" lamp. He was then to press whichever button he thought was the correct one. After 32 such trials there was a pause of about one minute, followed by another run of 32 trials. After completion of the guessing tests the computer randomly assigned the subject to either the "first run high" or the "second run high" condition, and then randomly decided whether the subject would get his real scores or the pseudo scores. (Pseudo scores were generated, once for each guess, by generating a target, then generating another and comparing them.) The appropriate note was then typed and the data recorded. The experimenter removed the note without looking at it and delivered it to the subject to be examined at his leisure. Forty unpaid volunteer subjects were used.

At the conclusion of the pilot experiment a computer program was called to perform the statistical tests on the data. Simplicity guided the choice of several t tests for the analysis: (1) a correlated t test for the high versus low scores for the pairs actually reported (as if the computer printouts were collated and analyzed in the manner of a traditional experiment); (2) a correlated t test on the high-low differences for the reported versus unreported scores to see if it was necessary to have feedback for a psi effect; (3) three t tests to see if there was an expectancy effect in the actual guessing test, if it continued for subjects who never saw the results of the guessing test, and if there was a difference between these groups.

The results of the pilot study demonstrated a significant expectancy effect in the reported scores (test 1 above) with $t = 1.918$ ($df = 39$; $P = .03$, one-tailed). The difference between the reported and unreported scores (test 2) was also significant in the expected direction with $t = 2.705$ ($df = 39$; $P = .01$, two-tailed). The third set of tests yielded no significant differences, indicating that whatever caused the significant differences in tests 1 and 2 was not operating in the actual guessing test. Therefore the third group of tests were applied to the pseudo-trial data before all data were destroyed. It was found that for subjects who saw their pseudo scores there was a weak expectancy effect ($t = 1.777$, $df = 19$; $P < .05$, one-tailed). For subjects who did not see their pseudo scores there was a significant effect in the

opposite direction ($t = -3.519$, $df = 19$; $P < .01$, two-tailed). The difference between the groups was highly significant ($t = 3.491$, $df = 38$; $P < .005$, two-tailed).

Thus the main hypothesis, that subjects can influence their scores in a direction specified after the experiment, was confirmed. The hypothesis that it is necessary to see the scores was supported by the results of test 2; however, post-hoc analysis revealed that instead of there being simply no effect in the unreported condition there was, apparently, a strong effect in the direction opposite to that which was specified for the reported scores.

A confirmatory study was run in precisely the same manner as the pilot study. Forty fresh subjects were used, this time paid volunteers recruited through a notice in the student union. While in the lab each participated in two other experiments after completing this one. Computer programs, equipment, and the method of dealing with the subjects were identical to that used in the first study. The analysis of the data differed slightly to allow an analysis of variance to be done. For the confirmatory study a program put out on punched paper tape the differences between the high and low scores for both the genuine and pseudo runs. The tape was then fed directly into the university's computer center for analysis. The analyses in this case were for any difference between the reported and the unreported scores, and any difference between the genuine and pseudo scores, as well as any interaction. Also, the original analysis program was used to perform a correlated t test on the reported scores.

The results of the confirmatory series were nil. In marked contrast with the pilot, none of the tests even approached significance. For example, in the confirmatory study the difference between the reported and the unreported scores yielded $F = 0.032$ ($df = 1, 39$), whereas in the pilot that same condition yielded $t = 2.705$ ($df = 39$; $P = .01$, two-tailed).

It seems difficult to ascribe the total failure of the second experiment to differences in methodology, since this was exactly the same in both studies. Rather than attribute this failure to replicate to gross differences either in the psi ability of the two groups of subjects or in the experimenter's ability to motivate them to use retroactive PK, it would seem more parsimonious to attribute the outcome to a psi failure on the part of a single person, namely the experimenter him-

self, for whom conditions between experiments may have changed considerably. Feedback-based methodologies may prove useful for investigating psi-based experimenter effects, although this experiment cannot be said to have "proved" their usefulness. The experiment has, insofar as the pilot is concerned, demonstrated that experimental outcomes can be affected either by subjects' seeing their results after the test or by the experimenter, who is blind to all but the statistics at the end of the experiment. Either conclusion means that a psi-based experimenter effect is very possible.

EXPLORATION OF THE PAPER MAZE TECHNIQUE FOR EXPERIMENTAL STUDY OF PSI

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We have been exploring the paper maze technique devised by Walter J. Levy, Jr., for possible study of psi. We feel the technique has some promise and are therefore reporting the outcome to date, even though no very strong conclusion is justified. In this procedure, the subject, starting at the center of a circular maze with 10 concentric walls and aiming at escape, draws a line up to each successive gap he wishes to pass through; a key is then consulted for whether the particular gap is open or closed to him. The key provides, in theory, equal probability for these two alternatives; if the subject is able to use the key as a psi target, he may be able to deviate systematically from 50-percent success. Generally, each subject did a set of four mazes; some subjects did additional sets. Subjects worked on a maze until they either reached the outside or became finally boxed in.

Two experiments were done; each involved the manipulation of two variables: whether the subject used his dominant or non-dominant hand, and whether the psi target consisted of entries in a table of random numbers or of the outcome of the subject's immediately future interaction with a random number generator. Each subject did one maze under each of the four combinations of variables; order was counterbalanced across subjects.

Experiment I was done with 72 subjects not selected at all for abilities, interest, or attitude related to psi, and

the first 48 of these subjects were not even told in advance that the experiment related to psi. Choices by the first 10 subjects showed a hitting rate of 54.7 percent ($z = 3.21$; $P = .0013$, two-tailed). The rate then declined, so that for the total data of subjects in Experiment I it was 49.6 percent ($z = 0.71$; nonsignificant). Neither the hand nor the target variable affected the results. Experiment II was done with 75 subjects who all met a minimum criterion of interest in psi; 16 of them did additional sets of mazes after the initial set. The overall hitting rate was 50.9 percent, a deviation that is significant, though just barely, at the 1 percent level ($z = 2.59$), two-tailed.

In the total data of Experiment II, where there was overall evidence of a psi effect, the predicted tendency for this effect to appear primarily at moments of "random behavior" was not found. In his work Levy had distinguished between moves that are most predictable, direct, or expectable and moves that are somewhat more surprising or indirect. The latter he called "random-behavior" moves; the former, "non-random-behavior" moves. In our study the tendency, not statistically significant, was in the opposite direction, both for explicit classification into "random" and "non-random" and for the conceptually related classification into inward and outward moves. Since the subject is aiming to get away from the center, and beyond the outmost circle, moves in the outward direction are obviously consistent with the subject's general orientation. An inward move, on the other hand, breaks up this orientation; it deviates from what is expected, and might therefore occur under conditions favorable to psi. In fact, the data in our study tended to show the opposite effect. Finally, no large or significant effects were found for the hand or target variables.

By two different methods of selection, high-scoring and low-scoring portions of data were selected for internal analysis. Here too pronounced effects of the two experimental variables and of "random" versus "non-random" moves did not appear. Suggestive evidence appeared, however, that psi information may be most frequently employed in connection with a particular kind of move: choice of an outward move along the same axis as the preceding move, where other outward moves are also immediately available. Low-scoring portions of data showed especially low scores on these moves, whereas high-scoring portions of data showed especially high scores. Speculation about this unexpected outcome suggests that emotional factors such as confidence, and elation at

previous success, may play a more important part here than the more cognitive or structural factors explicit in the "random-behavior" hypothesis.

NEW EVIDENCE SUPPORTING THE ESP INTERPRETATION OF THE PRATT-WOODRUFF EXPERIMENT

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The Pratt-Woodruff experiment, first reported in 1939, has recently been the subject of considerable controversy. The general procedure was screened touch matching. A vertical screen sitting on a small table divided it into two approximately equal areas. On one side sat the subject facing a permutation of the five ESP symbols, the key cards, that hung on a row of pegs across the face of the screen. A narrow slot across the bottom of the screen had a row of five blank cards marking the positions of the key cards. One experimenter, Woodruff, sat on the other side of the screen holding a shuffled pack of ESP cards (target cards) and watching the row of blank cards through the slot for the subject's pointer. In preparation for a new run, a second experimenter, myself, removed the key cards from their pegs and handed them to the subject to be returned to the pegs in an order that was not known to Woodruff.

When the key cards were in position and when Woodruff announced that he was ready, the subject pointed to the marker cards, one after another, to indicate where Woodruff should place the target cards. The subject's purpose was to have as many as possible of the target cards match the key cards. At the end of the run I recorded the order of the key cards while Woodruff recorded the ESP cards in the five piles back of the screen, and these two independent records were clipped together and deposited through a slot into a locked box. Then the screen was laid on its side and, observed by Woodruff and the subject, I checked the number of hits, the cards in the five piles that matched the key cards. Each experimenter independently recorded the total number of hits for the run. Then the screen was returned to its vertical position and the procedure was repeated for the next run.

C. E. M. Hansel, in his critique of this experiment, stated the rationale of his approach in the following words:

"When examining an experiment of this nature in order to see whether it is foolproof, we first assume that ESP is impossible and then seek some other cause of the high scores." His search led him to the discovery that in the records of the highest-scoring subject, P. M., the significant results were largely concentrated in the responses made to the key cards that were in the two end positions in the immediately preceding run (E-cards). He interpreted this fact to mean that Woodruff had kept track of where those cards were placed on the row of five pegs for the next run and had produced spurious hits on them through misplacement of target cards.

Woodruff and I acknowledged that the Hansel finding was highly significant, but we argued that, because of the test conditions, it could more reasonably be interpreted as an ESP salience effect. We suggested that the subject's assigned task of putting the key cards back on the pegs in a different order caused her to pay special attention to the cards that had been in the more prominent end positions during the previous run and that this special focusing of attention upon them made those symbols more salient as ESP targets and accounted for her higher rate of success on them. We showed that Hansel's claim that the E-card effect was also significantly present in the combined data of the other four high-scoring subjects was not sound since there were serious flaws in his statistical analysis of those records.

Medhurst and Scott applied another statistical test to the combined data of those other four subjects and found that the scoring rate on the E-cards was significantly higher than that on the other three key cards (M-cards). This was, however, a weak effect ($P = .012$, one-tailed) as compared with the level of significance of this analysis for P. M. ($P \quad 2\ 10^{-6}$, one-tailed). Medhurst and Scott concluded: "The evidence is not, of course, compelling. It is open to anyone to prefer the hypothesis that an unlikely coincidence has occurred or that the psychological peculiarity attributed by Pratt and Woodruff to the subject P. M. applied to more than one subject, or to produce yet another hypothesis in terms of an ESP effect.... However, it seems clear that the new evidence in this paper moves the balance at least some distance toward Hansel's hypothesis."

I have now carried out a new kind of analysis to discriminate between ESP and fraud in these data. This analysis sought to discriminate between subject ESP and experimenter fraud by using a basis of differentiation confined to

the trials made against the E-cards. I limited my research to the P. M. data, because the E-card effect was so much stronger in her data. If evidence was found from her records that clearly pointed to an ESP interpretation of the effect discovered by Hansel, we would be safe in assuming that the same would be true for the much weaker E-card effect present in the records of the other four high-scoring subjects as a group.

In the testing procedure of the Pratt-Woodruff experiment, the subject was assigned the task of replacing the key cards on the pegs before each run, and she was told to put them up in a different order from the one they had been in before. Obviously, if the subject took a naive and literal view of the requirement to make the new order different from the old one, she would have been able to carry out the instructions to change the order only to the degree that she remembered what that previous order had been. The fact that the Hansel effect occurred can be interpreted as showing that P. M., when she was rearranging the key cards on the pegs, primarily took account of the E-cards.

My idea was that the subject would not have remembered all of the key cards that were formerly in the end positions, and that when she failed to remember E-cards they should not have been different for her, psychologically, from M-cards. Those forgotten E-cards and most of the M-cards would have been replaced "randomly" upon the pegs that were still empty after the remembered cards had been put up in their changed positions. Then it occurred to me that we should be able to identify at least some of the E-cards that she did not remember: those that were left for the new run in the same position as they had been in during the last run. Since she was making a special effort to change the positions of the key cards, she would have replaced an E-card on the same peg as before only when she did not remember that it had been there.

I therefore planned an analysis which would involve comparing the rates of scoring on E-cards which were shifted to a different position with the rates on E-cards which were not shifted. On the ESP hypothesis unshifted E-cards, being psychologically like M-cards, should also show a low rate of success. The shifted E-cards, on the other hand, should contain those cards that the subject consciously remembered and replaced on other pegs in carrying out the instructions given to her. On the ESP hypothesis I therefore predicted

higher scoring on shifted E-cards than on unshifted ones. On the experimenter fraud hypothesis, on the other hand, there was no obvious reason to expect any difference in the rate of scoring on shifted and unshifted E-cards, since this distinction would have no meaning for Woodruff.

Before the data were examined, I decided to apply a chi-square statistical test based upon a two by two contingency table (shifted versus unshifted E-cards by hits versus misses) to test this hypothesis. The trials scored against the unshifted E-cards were at the level of 20.75 percent hits, while those scored against the shifted E-cards were at the level of 28.26 percent hits, where 20 percent was mean chance expectation (chi square = 5.47, corrected for continuity; $P < .01$, one-tailed). The result predicted on the ESP hypothesis was found at a level that is statistically significant. This difference between the scoring rates on shifted and unshifted E-cards is hard to reconcile with the fraud hypothesis, so the claim of the critics that the overall high rate of scoring on E-card trials was brought about through misplacement of target cards by the experimenter is not tenable as an interpretation of the Hansel finding.

NATIONAL SURVEY OF PSYCHICAL EXPERIENCES AND ATTITUDES TOWARDS THE PARANORMAL IN ICELAND

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Spontaneous psychic experiences have almost solely been studied either by investigation of individual cases or by examination of self-selected collections of spontaneous cases. Representative national surveys of psychic experiences have been rare. They include a recent national survey done by Andrew Greeley at the National Opinion Research Center in Chicago and similar surveys conducted by Gallup-type institutions in Denmark and Germany. Palmer and Dennis conducted an extensive community mail survey in Charlottesville, Va. [see RIP 1974, pp. 130-3]. The present survey was conducted in Iceland in 1974. It is the first extensive survey of spontaneous experiences and attitudes towards the paranormal, using a national random sample.

A random sample of 1157 persons between the ages of 30 and 70 was taken from the National Registry. The Icelandic population being only about 210,000, this sample contained about one-seventieth part of the population in those age brackets. As Icelanders residing abroad and foreigners residing in Iceland are included in the registry, it was decided to exclude them and deceased persons from our sample as they became known to us. Thus our original sample was reduced to 1132 persons.

The questionnaire consisted of 53 questions grouped into six categories: dreams (frequency and vividness of dream recall, self-interpretation of dreams, nature of dreams, etc.); psychic experiences (psychic dreams, waking precognition and clairvoyance, telepathy, out-of-body experiences, seeing the aura, memories of previous lifetimes, apparitions of the dead, poltergeists, hauntings, and some questions on Icelandic folklore, such as apparition of "hidden folks" or fairies, etc.); religion (religiousness, religious experiences, reading of books on religion and psi phenomena); visits to psychics (including mediums, sensitives, astrologers, and mental healers); attitudes (towards astrology, ESP, apparitions of the dead, poltergeists, mediumistic communication, hidden folks, survival of death, reincarnation, parapsychological research, etc.); and demographic information (age, education, occupation, etc.).

Nearly all the questions were of the forced-choice type and could be answered by circling the appropriate response. There were a total of 139 questions. In the spring of 1974, as we were constructing our questionnaire, we learned about the survey that Palmer and Dennis were then working on. After some correspondence with Charlottesville, we adjusted the form of our questionnaire to that of theirs for the sake of easy comparison. Our questionnaire contained more questions on spiritistic practices and beliefs, since they are widespread in Iceland; we included several questions on Icelandic folklore that may possibly be relevant for parapsychology, and included more questions dealing with religion and various attitudes.

In June 1974 a copy of the questionnaire and a prepaid reply envelope were mailed to every member of the sample. At the beginning of August the questionnaire was mailed to all who had not returned a questionnaire at that time (74 percent of the sample). A third mailing was done (to 54 percent) late in September. This mailing was followed

by phone calls and in some cases by visits when respondents could not be reached by phone. That increased the return rate considerably, and also gave us valuable information about our non-respondents. In January 1975 we closed the survey when 902 persons had returned usable questionnaires, 425 men and 477 women. We thus had a very satisfactory return rate of 80 percent of our total sample.

A psychic experience of some kind was reported by 64 percent of our respondents. The questionnaire offered a fairly exhaustive list of known types of psychic phenomena. The high return rate indicated that our respondents were quite representative of the adult population of Iceland. This result therefore strongly suggests that about two-thirds of the population of Iceland believe they have had a psychic experience at least once in their lifetime, or rather, claim to have had one or more such experiences. Of those claiming psychic experiences, one-third claimed only one type of experience, whereas two-thirds claimed two kinds or more. The women clearly outdid the men, 70 percent of them claiming psychic experiences as against 59 percent of the men. When we compare Icelanders with another Nordic group, the Danes, we find a vast difference: only 11 percent claimed psychic experiences in the Denmark survey of 1957 as compared to the 64 percent in our Iceland survey. In the German survey of 1958 this figure was 19 percent. It therefore looks as if the Icelanders are a particularly "sheepish" nation. Differences in the time and way of surveying might also possibly account for some or all of these national differences. Apparently nations can differ widely in how readily they claim psychic experiences.

Judging from the figures obtained in Greeley's recent national survey in the United States, we have reason to believe that the Americans consider themselves about as psychic as the Icelanders. Palmer and Dennis's community survey in Charlottesville also shows that over half the inhabitants of that university town claim to have had one or more psychic experiences in their lifetime.

Of the various types of psychic experiences in the survey, psychic dreams were the most common, claimed by 36 percent of our sample, with a significant sex difference: 44 percent of the females reported psychic dreams as against 27 percent of the males. Somewhat to our surprise, apparitions of the dead were the second most common psychic experience. They were claimed by 31 percent of the sample,

also with a significant sex difference (26 percent of the men and 36 percent of the women). Visual apparitions were most frequent, then tactile and auditory. Apparitions of the living were claimed by 11 percent, with no sex difference. These figures are high when compared with the other surveys. Apparitions of the living and the dead were claimed by 10 percent of the respondents in the old Census of Hallucinations, 14 percent in West's followup study in the 1940's, and 17 percent in Palmer and Dennis's survey. Having lived or spent the night in a haunted house was reported by 18 percent of the sample. This and all less frequently reported experiences did not show any sex difference. Out-of-body experiences were reported by 8 percent, poltergeists by 8 percent, seeing the aura by 5 percent, and 16 percent claimed to have been subjects in other peoples' psychic experiences.

Education was a significant factor in most of the reported phenomena, the number of reported experiences decreasing with increasing education. When asked about their opinions on the existence or non-existence of various phenomena, the college-educated seemed more sure of their judgments than the less-educated persons, and gave considerably fewer "don't know" answers. This raises the question of whether a college education suppresses psychic experiences, makes people more prejudiced in reporting them, or makes them misjudge their paranormal experiences as non-paranormal; or whether education really does make us better judges of the nature of the phenomena we observe. Other factors significantly related to psychic experiences were frequency of dream recall, a tendency to try to interpret one's dreams, and being religious. This may suggest that persons who tend to look at life as meaningful in a transpersonal sense tend also to accept more psychic elements in their lives.

Spiritualism has a strong footing in Iceland. Every third respondent had attended a mediumistic seance, half of them believing that they had communicated with the dead. Mental healers (prayer healers and mediumistic healers) appear popular, with 41 percent of the respondents having sought help from them. Astrologers are, on the other hand, rare in Iceland; only 3 percent of the respondents had sought advice from them. We asked our respondents about the usefulness of these visits. Visits to prophecy-psychics (who claim to have ESP) were rated useful by 28 percent, to mediums by 83 percent, and to mental healers by a surprising

91 percent. One wonders if visits to psychiatrists would be rated therapeutically effective by 91 percent of their patients. Thus, the areas where parapsychologists do the least research, mediumship and healing, were the most highly valued phenomena.

Iceland is definitely a country where the "sheep" dominate. Less than 1 percent of the respondents considered the existence of clairvoyance or telepathy unthinkable, and only 5 percent unlikely, as against 45 percent who thought its existence possible and 41 percent either probable or certain. Belief that the dead can be seen was about as widespread as belief in psychic dreams: 31 percent were certain of it, 26 percent thought it probable, and 31 percent possible. Belief in communication with the dead at seances was only slightly lower. At the bottom came belief in poltergeists and hauntings; 33 percent considered them impossible or unlikely. Belief in survival was somewhat greater in our sample than in Palmer and Dennis's Charlottesville sample; 40 percent were convinced of survival and only 7 percent considered it unlikely or impossible (as against 34 and 23 percent, respectively, in Charlottesville). Belief in reincarnation was slightly greater than in the Charlottesville sample, and so was a positive attitude towards parapsychological research. Many of these attitudes were negatively related to education, especially attitudes towards spiritualistic phenomena. Females were generally greater believers than males.

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SYMPOSIUM*:

PSI AND PHYSICS

CAN QUANTUM THEORY EXPLAIN PARANORMAL PHENOMENA?

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We have evolved a theory which is compatible with presently accepted physical theories, and should allow, at least in principle, numerous predictions in parapsychological experimentation. The theory is not a physical theory. It represents, instead, an extension of physics. The starting point for this theory is the epistemological question of how complex phenomena are to be described. The question of whether or not a particular state of consciousness can be linked to a given neuropsychological state, and of what the rules concerning such correlation would be, is closely connected with that of whether accurate predictions concerning the behavior of a complex system (such as the human brain) can be derived simply from knowledge of its physical behavior (neuron function and switching patterns, in this case). It has been shown that a complete description of complex systems is impossible, a result which follows from Gödel's theorem. There exists, in the description of complex systems, a lack of information which is not contingent on outside factors; rather, the lack exists in principle. There are always modes of behavior in complex systems which cannot be described by the same rules as other modes. The system of rules is not closed, so far as the behavior of the system as a whole is concerned. In order to make predictions concerning the behavior of systems' acting in ways resulting from these principles, it is necessary to have statements of information concerning the systems themselves, and not statements about the description of the systems.

*Chairperson: Helmut Schmidt, Mind Science Foundation.

The step from making statements describing a system to making statements concerning the system "itself" is epistemologically justified by the positivistic method. According to this method, it makes sense to say that a system conducts itself in a particular way if the description of its behavior says it does, so long as no contradicting description of its conduct is available. In other words, a discussion of the behavior of the system per se is pointless. If one says that a lack of information concerning the behavior of a complex system exists in principle, one is not saying that such information could, given more time and further investigation, be ascertained. Rather, it means that particular concepts lose their function and unlimited application as a result of the structure of information presented by description. In this case, the concept involved is that of behavior determined by particular circumstances. This does not mean that complex systems do not have their behavior determined by their circumstances at particular times; rather, it means that the concept of determinism cannot be applied to such systems. A similar case exists in the field of quantum mechanics. In this field, a lack of information concerning measurements of complementary observables exists in principle as a result of Heisenberg's Uncertainty Principle.

If systems are not fully describable, then a description may proceed, according to the logic of such incomplete describability described above, from the assumption that the systems concerned act stochastically. The probability function or wave function ψ was introduced into quantum mechanics for this reason. This function describes the condition and the development over time of a quantum mechanical process. The introduction of a probability function φ for the description of complex systems also seems logical. φ would, initially, be a function for which $|\varphi|^2$ would be the probability of occurrence of information in the system concerned. Since information is always connected to a carrier of information, the function φ would simultaneously be a part of the whole state function of the information carriers in the complex systems, such as that for electrons or for ions and molecules. The state of a material complex system, such as the brain, would then be described by the probability function φ, dependent, among other things, on the degree of complexity of the system concerned, and the quantum mechanical wave function ψ. We now assume that the state (here both microscopic and macroscopic) of the system examined may be described by the total wave function $\Psi = a\psi + b\varphi$ (note that the probabilities described by ψ and φ are not independent!).

The total wave function would, then, depend on the quantum mechanical state of the system, and from macroscopic status variables such as its degrees of organization and complexity.

The functions Ψ, ψ and φ have ontological characteristics already known for the quantum mechanical wave function. They are mathematical entities and they are correlated with observables of the systems concerned by mathematical operations. They implicitly contain the concept of a probability function. Probability functions are not strictly located in time and space. A probability function is reduced by a measurement or observation to a single value, which leads to a measurement value. Only the "potentiality" can be given before a measurement is made, which becomes a fixed value as a result of the measurement. Carrying out multiple measurements, using equal starting conditions, one obtains a value which is dispersed around the average value. An important distinction between the quantum mechanical probability function ψ and the function φ exists in the fact that ψ is a microscopic function, generated by the definition and behavior of microscopic particles (subatomic particles, atoms, and molecules), while φ is a macroscopic function generated by the whole system concerned.

If it were possible for a measure c for complexity to be given, then quantitative statements concerning φ should be possible. In the simplest case, for $c \rightarrow 0$, it follows $\varphi \rightarrow 0$ as well, so that ψ approaches Ψ. This means that, for simple systems, normal quantum mechanics hold.

Certain important qualitative statements about φ can be made without an exact description of its derivation. If one proceeds from the assumption of a measure of structural complexity, then even small changes in the structure of a system can result in large changes in its degree of complexity. A single missing element can render an extended switching net incapable of functioning. The same is true for functional complexity. This, as opposed to structural complexity, is a concept, which applies, in a metalanguage manner, to the structure of the function. The function concerned can only be derived from a description of the system, if it is derivable at all. The structure of the description can be transformed into one of function. A degree of complexity can be determined from this structure. This functional degree of complexity, like the structural degree of complexity, reacts in an irregular manner (like a step function) when hierarchical levels are changed. This raising or lowering occurs by

means of addition or subtraction of elements of the system from consideration in the description, or by discrimination of their functions. It follows that small changes in the system, which might be achievable without any considerable expenditure of energy, could result in large changes in φ, which is dependent on c.

Even a small change in ψ can be expected to produce a large change in the total wave function Ψ. Additionally, the complex system need, by definition, also not be limited. It could be shown that the arbitrary addition of further elements to a system only results in a small increase in the degree of complexity. These elements could also be "external" ones. The norming requirement $\int |\varphi|^2 \delta\tau = 1$ is not contradicted even when the amplitude of the function φ does not disappear outside the system. The same applies, of course, for the physical function Ψ. However, physical reasons cause this value to become so small outside the system concerned as to be negligible.

It would be possible to assume that a large number of parameters would affect the determination of the function φ, since the description of complex systems (such as the brain) contains a great many such free parameters. This could be determined from the results of experimentation, by fitting certain hypothesized parameters to the results of actual experiments. It would, however, be possible to reproduce any given experimental curve by the choice of a given number of free parameters. This would mean that the φ function was only apparently a model of the behavior of a complex system. This does not necessarily have to be the case, as is shown by observations on measures of complexity. Systems can have a large variety of structures and functions at the same degree of complexity. These structures could be considered redundant if functionally viewed, or as various alternative system types. The important thing is to have the function φ not affected by the various parameters, since it is dependent, for the most part, on c.

We have applied these concepts to a group of psychic phenomena: psychokinesis, telepathy, and clairvoyance. It can be shown that a concise qualitative treatment is possible. The following are merely the basic propositions, which are important for the discussion of the model. (1) The PK object is described by a function $\Psi = a\psi + b\varphi$ consisting of a purely physical portion ψ and a functional portion φ containing information about the brain and (possibly?) the contents of the

consciousness involved. The functions Ψ, Ψ, and φ are probability functions, and hence not localizable in terms of classical ontology. There is a non-causal correlation between the various values of the functions at different points in time and space. This results from the norming requirements for such functions. (2) The experimental subject can alter the function φ. (3) The experimental subject can perceive the function φ and its alterations.

Finally, we must state that one cannot expect a direct derivation of the fundamental equation for the function φ. This would be contradictory to Gödel's theorem. Therefore, for establishing this equation, more relations, experimentally confirmed, are required.

RANDOM FLUCTUATION THEORY OF PSYCHOKINESIS: THERMAL NOISE MODEL

R. D. Mattuck (University of Copenhagen)

Since PK phenomena generally resemble those produced by ordinary forces, most of the attempts to explain them have postulated some new type of force coming from the mind. This paper presents a different approach, based on a proposal by E. H. Walker. In this proposal, mind makes use of the energy which is already present in matter in the form of random fluctuations or "noise," reorganizing this energy in such a way as to achieve the desired PK effect. In this work I will only discuss thermal noise, but the general ideas apply to any kind of random fluctuations.

Now ordinarily, although the thermal energy is large (about 10^6 ergs in a cc of air), it is present in the form of completely chaotic movements of molecules--i. e., in a high entropy state--so we cannot use it to perform work. Walker postulates that mind makes this energy available by means of a direct interaction with matter, in which mind identifies and selects low-probability states of the material system. For example, consider a small ball floating in water. The ball is bombarded on all sides by molecules in random thermal motion. Hence the ball itself will have tiny random movements, so that its velocity will fluctuate over a range of values centered about zero. Very small velocities (about 10^{-7} cm/sec.) will predominate, but occasionally there will

be considerably higher velocities. Suppose now that a medium wants the ball to move to the right. According to the postulate, mind is able to directly identify or "see" when the ball is in one of the random velocity states with an appreciable speed in the rightward direction. The higher the velocity, the lower is the probability that the ball is in this velocity state, and the greater is the information involved. On the basis of this information, mind selects or "freezes" the ball in this low-probability velocity state. If mind is able to carry out this procedure, then the ball will acquire a velocity to the right. More generally, we can imagine that mind selects not just one, but rather a bunch of different velocity states clustered around one rightward-directed velocity state.

Thus the information acquired by mind is utilized to change the entropy of the system from high (random thermal motion of the ball) to low (average velocity of the ball nonzero). Hence we can regard the information as negative entropy introduced into the system. This process is not of the "Maxwell demon" type; i. e., it does not operate by selection of states of individual molecules, but rather by the selection of macroscopic pure states (the velocity states of the ball).

The detailed nature of the direct mind-matter interaction is not essential to the model, but a few words should be said about it. One can think of three possibilities: (1) A brain-matter link involving ordinary physical forces--for example, electromagnetic or acoustic. However, the measured energy transfer of this sort is much too small to account for the observed PK phenomena. (2) Non-local quantum mechanical correlations of the Einstein-Rosen-Podolski type between brain and matter. (3) Walker's hidden variables mechanism in which mind (consciousness) is characterized in terms of hidden variables. These are adjusted by the will, and cause collapse of the wave function into the desired state. I prefer this last possibility, but feel that it is best to leave the question open. It is important to observe that the mind-matter interaction does not appear to involve ordinary physical forces, so that unlike the case of the Maxwell demon there is the possibility that the second law of thermodynamics may be violated.

How large are the effects which can be obtained by the state selection mechanism? This will depend on the rate at which mind is able to process information. In a given time, the greater the information, the more improbable is the velocity state which can be selected, i. e. the higher is the velocity.

From neurophysiological data, we can arrive at a figure of about 10^8 bits/sec as the maximum information processing rate for the mind. Now Walker assumes that information is processed at a constant rate; applying this to our ball example, using 10^8 bits/sec, yields a very small ball velocity--only about 10^{-3} cm/sec. However, I have discovered that the state selection mechanism is much more efficient if it is done in small steps, i.e., if the average velocity of the ball is increased just a little bit at a time, by each time selecting velocity states slightly higher than those selected previously.

Doing things in small steps means that the information processing occurs in pulses. Regarding the pulse time, if mind is to interact with molecular matter, its characteristic times must match those of molecules--e.g., about 10^{-12} or 10^{-13} sec (mean time between molecular collisions in a liquid). Using a pulse time of 10^{-12} sec, it is found that a ball weighing four grams can be accelerated up to 85 meters/sec in a minute, showing that the model is able to predict macroscopic PK effects.

The pulsed state-selection mechanism for the mind-matter interaction may be developed in a general way as follows: (1) The physical system is described in terms of states labeled by one or more macroscopic variables, X. For example, X could be position, velocity, density, temperature, etc. (2) In thermal equilibrium, each variable has an average value A, and there are extremely rapid, tiny fluctuations (X - A) about this average, due to thermal noise. The root-mean-square value of these noise fluctuations is called N. The probability that the system variable has value X is a Gaussian curve with center at A and half-width N. (3) It is assumed that mind identifies and selects the states X in such a way that the Gaussian curve shifts a small amount d, without changing in width. This takes time 2p, where p is the pulse time (i.e., pulse "on" only half the time), and can be shown to involve information: $2pW = (1/(2\times\ln 2))(d/N)^2$, where W bits/sec is the average rate at which mind processes information. (4) After total time t, the Gaussian curve has been shifted t/2p times, and the total change in its average value is (td/2p). Hence the PK change in the average value of X is:

$$(1) \quad \text{PK change of } X = Nt\times\sqrt{(\ln 2\times W/p)}$$

This is completely general, applying to any variable X. It is valid for p greater than 10^{-14} sec.

Let us first apply this to our moving ball example, where $X = v$, the velocity of the object. In this case, it can be shown that $N = \sqrt{(kT/m)}$, where k = Boltzmann's constant, and T = temperature. We can now identify a PK acceleration a, and a PK "quasi-force" F (not a true force, but something which "acts like" a force, since it increases the object's velocity):

(2) PK acceleration = a = (PK change of v)/t

$$= \sqrt{(kT/m)} \times \sqrt{(\ln 2 \times W/p)}$$

(3) PK quasi-force = F = ma

For m = 4 gm, T = 300 deg, $W = 10^8$ bits/sec and $p = 10^{-12}$ sec, we find a = 850 cm/sec^2, and F = 3400 dynes. If the 4-gm object is a ball moving in water, then the PK quasi-force will accelerate the ball up to the point where F is balanced by the frictional force, $-mv/t'$, where t' is the relaxation time (about 10 sec, from Stoke's law). For $W = 10^8$ bits/sec and $p = 10^{-12}$ sec, this gives v = 85 meters/sec.

For a 4-gm cube resting on a table, the frictional force is about half the weight of the object, i. e. 1960 dynes. Hence the PK quasi-force of 3400 dynes is enough to overcome the frictional force and make the object slide on the table. If p is lowered to 10^{-13} sec, then a is increased to 2550 cm/sec^2 which is greater than the acceleration of gravity, so the object could be levitated.

Equation (2) can also be applied to PK on a harmonic oscillator, and yields the result that the PK displacement is inversely proportional to the frequency of the oscillator. This is in qualitative agreement with the experiments of Sasaki and of Taylor.

The general equation (1) can be applied to PK voltage changes in an electrical circuit with thermal noise. In a circuit with an R ohm resistor connected across a C farad capacitor, the charge Q is the analogue of the velocity, v. For C = 1 microfarad, R = 1 megohm, $W = 10^8$ bits/sec, and $p = 10^{-10}$ sec, we find that the PK charge is 5×10^{-5} coulomb and the corresponding voltage is 50 volts. Equation (1) may also be used to analyze a random number generator (RNG) based on thermal noise. Suppose we have a binary RNG which samples the noise voltage across the above RC circuit and produces a "hit" when the voltage is positive, and a "miss" when it is negative. Then, if the mind produces a

PK voltage which is much less than the noise voltage, we find that the extra number of hits/trial, E, is given by $E = (1/\sqrt{2\pi})t'\sqrt{(\ln 2 \times W/p)}$. This is independent of the temperature, the complexity of the RNG, and the trial rate, but is proportional to the time constant t' = RC. This could be tested experimentally. Equation (1) can also be used to show that PK changes in temperature of the order of a few degrees are possible.

Finally, it should be noted that in the neighborhood of the "critical temperature," where a system undergoes a phase transition, the thermal noise fluctuations, N, in (1) become enormous. For example, when water reaches a temperature of 647° K, and a pressure of 218 atmos., there are huge fluctuations in density which cause large scattering of light ("critical opalescence"). The model presented here predicts that a medium should be able to cause very large changes in the variables of such a system, which should be relatively easy to detect.

In conclusion, the thermal noise model seems to account for the order of magnitude of a variety of PK phenomena, and is able to make quantitative predictions which may be tested experimentally.

A SUGGESTED METHOD FOR DETECTING PSI WITH SUBJECTS OF RAPIDLY VARYING PERFORMANCE*

Helmut Schmidt (Mind Science Foundation)

Previously used test methods appear unsuitable for detecting psi in cases where the subjects' performance fluctuates from trial to trial in an unpredictable manner so that the average score is at chance level. Indeed, one might ask whether perhaps in this case the psi concept loses its meaning and a psi effect should be in principle undetectable. An answer to this question is offered by my recently proposed mathematical theory of psi. In this model theory, "psi sources" play the role of somewhat idealized human subjects.

*A more detailed discussion of this topic can be found in Journal of the American Society for Psychical Research, 1977, pp. 19-31.

Consider as an example the case of a binary random number generator with two output channels P and Q, with associated probabilities p and q respectively (p + q = 1). If this generator is activated repeatedly while a PK subject receives a success signal for each generated P-signal, then the relative frequency p' of the P-signals increases above its chance level p. With q' = 1 - p' we can write this as

(1) $$\frac{p'}{q'} : \frac{p}{q} = \theta, \text{ with } \theta > 1 \text{ for PK hitting}$$

We call θ the psi strength of the source (the subject). The values $\theta > 1$ and $\theta < 1$ correspond to psi-hitting and psi-missing respectively, provided the number of trials is large enough so that θ is significantly different from its chance value of 1.

From Equation 1 we can calculate p', q' in terms of p, q, θ:

(2) $$p' = \frac{p\theta}{p\theta + q} \qquad q' = \frac{q}{p\theta + q}$$

In the theoretical paper the strength of a psi source was assumed to be constant in time, but here we will admit the possibility of changes in θ during the course of an experiment. Such changes can occur either if we test many subjects with different θ values, or if we work with one subject whose psi strength depends on varying psychological or physiological factors. We can restrict our discussion to such changes in psi strength as are causally determined by the test conditions, because indeterministic fluctuations in psi strength can be shown to be, in principle, undetectable.

Let us consider the case where we have a pool of M subjects with the psi strengths $\theta_1, \ldots \theta_M$, which are constant in time, and where the subjects to be used in a PK test are prescribed by some deterministic procedure. As a simple example, we could specify that each subject is to be used exactly once. Then the average hit probability in the PK test is obtained by averaging the hit probabilities for the individual subjects (see Equation 2):

(3) $$\Pr(\text{hit}) = M^{-1} \sum_i \frac{p\theta_i}{p\theta_i + q}$$

Introducing

(4) $$x_i = \ln \theta_i \quad , \text{ i.e., } \theta_i = e^{x_i}$$

the condition for small PK effects, $\theta \approx 1$, can be written as

(5) $$|x_i| \ll 1$$

With 4 and 5, Equation 3 gives

$$\text{(6)} \qquad \Pr(\text{hit}) \approx p + pq\langle x\rangle - \langle x^2\rangle pq\,(q - p)\,/\,2$$

with

$$\text{(7)} \qquad \langle x\rangle = M^{-1}\sum_{i=1}^{M} x_i \quad , \quad \langle x^2\rangle = M^{-1}\sum_{i=1}^{M} x_i^2$$

Equation 6 shows that the values of $\langle x\rangle$ and $\langle x^2\rangle$ for the group of subjects can be determined if we measure Pr (hit) for two different p values; i. e., if we test the group with two random generators with different hit probabilities. This comparison has to be made under the same psychological conditions; i. e., the subject should never know whether a current trial is made with one or the other random generator. This can be easily achieved by offering the two types of trials in a quasi-random order to the subjects. We are interested here in $\langle x^2\rangle$ as an indicator for the existence of psi effects in classes of fluctuating psi strength. Note that the expectation value of $\langle x^2\rangle$ is zero in the absence of psi, but positive for psi-hitting as well as psi-missing.

For measuring $\langle x^2\rangle$ it is convenient to use two random generators with the hit probabilities $P_{diff} = p' < 1/2$ and $P_{easy} = q' = 1 - p'$, with p' to be specified below. Then by subtracting the hit probabilities $\Pr(\text{hit})_{diff}$ and $\Pr(\text{hit})_{easy}$ given by Equation 6 with $p = p'$ and $p = q'$ respectively, we obtain

$$\text{(8)} \qquad \Pr(\text{hit})_{diff} - \Pr(\text{hit})_{easy} + (q' - p') = p'q'\,(q'-p')\langle x^2\rangle$$

The two hit probabilities in Equation 8 are, in some approximation, given by the outcome of an experiment. Assume that we perform N trials under either condition and that we obtain H_d and H_e hits under the difficult and easy conditions respectively. Then the two hit probabilities in Equation 8 are the expectation values for H_d/N and H_e/N respectively. Introducing the random variable

$$\text{(9)} \qquad D = \frac{1}{p'q'(q'-p')}\left\{\frac{H_d}{N} - \frac{H_e}{N} + q' - p'\right\}$$

the particular experiment furnishes a particular value for D, and Equation 8 can be written as

$$\text{(10)} \qquad \overline{D} = \langle x^2\rangle$$

In order to determine whether some observed value D is significantly higher than zero (chance expectancy), or whether it is consistent with the null hypothesis (no PK in the data) $\overline{D} = 0$, we have to calculate the standard deviation for the normally distributed (in the absence of PK, at least) quantity D. A simple calculation shows that

(11) $\overline{D^2} = 2 / N\, p'\, q'\, (q' - p')^2$

For given N, this becomes smallest for

(12) $p' = (1 - 1/\sqrt{2}) / 2 = 1 / 6.83$

This is therefore the most favorable choice for the parameter p', but the more easily available p' values of 1/8, 1/6 or 1/4 are acceptable approximations. Then the CR value which measures the statistical significance of the result in favor of PK effects becomes (with (9) to (12))

(13) $$CR = D/\sqrt{\overline{D^2}} = 2\sqrt{N}\left(\frac{H_d}{N} - \frac{H_e}{N} + q' - p'\right)$$

For a given positive value of $\langle x^2 \rangle$ the expectation value for the resulting CR is easily seen to be

(14) $$\overline{CR} = \frac{\sqrt{2N}}{8}\langle x^2 \rangle$$

This equation shows us how high a value of $\langle x^2 \rangle$ is required in order to make an experiment with a certain number 2N of trials statistically significant.

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SYMPOSIUM*:
GELLER-TYPE PHENOMENA

THE GELLER CONTROVERSY: THE CURRENT STATE OF PLAY

John Beloff (University of Edinburgh)

Quite apart from its intrinsic interest, the case of Uri Geller is important first because it has led to the revival of macro-PK phenomena and second because, in consequence, it has encouraged a number of physicists to concern themselves with the problem of the paranormal, as happened in the early days of psychical research. What is here referred to as the "Geller controversy" is the dispute which still rages as to the validity or genuineness of the phenomena, not the different views which prevail concerning the correct interpretation of those phenomena assuming them to be valid.

The Geller controversy may be considered to have passed through three successive phases. First, a positive phase starting with Geller's initial impact on his worldwide audience and culminating in the Stanford Research Institute investigation by Targ and Puthoff. Second, a negative phase in which spokesmen for official science counterattacked, dwelling on the suspicious features of Geller's performances and behavior and on his involvement with conjuring and showmanship. This counterattack was spearheaded by the stage magicians, in particular by James Randi, who specialized in simulating Geller effects and whose book, The Magic of Uri Geller, dealt a severe blow to Geller's international reputation as a psychic.

It is here suggested that a third phase has commenced in which the emphasis has shifted to Geller's many imitators. The contributions to this symposium deal largely with the

*Chairperson: John Beloff, University of Edinburgh.

evidence from this source which includes the many "mini-Geller" children. The most notable exponent of Geller-type phenomena at present is the French psychic Jean-Pierre Girard, who has survived a number of stringent tests. However, not until all this evidence has been published and digested will it be possible to proclaim that the balance has shifted decisively in favor of the reality of the phenomena.

AN INQUIRY INTO THE REACTION OF THE PUBLIC TO THE "GELLER EFFECT": A QUESTIONNAIRE INVESTIGATION

Susanne Gruber-Wendlandt (Institut für Grenzgebiete der Psychologie)

In January 1974 Uri Geller demonstrated his phenomena on German television. He also did an experiment with the readers of a boulevard paper, Bild. In addition to thousands of phone calls, the editors of the paper received 1450 letters with reports of bent and broken cutlery and started watches. The television station also received thousands of phone calls and made note of 800 addresses of callers. I first performed 80 personal interviews with a casual sample of Bild readers and television viewers to assess the spectrum of reported phenomena, and then constructed a questionnaire based on my findings. Part A dealt with details of watch phenomena. Part B contained corresponding questions about cutlery. Part C consisted of questions about attitudes towards Uri Geller, the respondents' interpretation of the claimed phenomena, and their degree of familiarity with the paranormal and the occult, and their opinion on future economic and political development. Part D was a standardized personality questionnaire called FPI-K. All the items on the questionnaire were either multiple choice questions or choosing between alternatives. After a preliminary test of 150 persons, the questionnaire was sent to a random sample of 850 out of a base of about 2500 people for whom I had addresses. Return rates were 69 percent and 72 percent, which indicates a strong motivation in the questioned sample.

The evaluation of the results was carried out in collaboration with Dr. Rainer Hampel and Dr. Helmut Kury, both psychologists and statisticians. It included the following steps: counting absolute and percentile frequencies of

all single items; comparison of the findings with those of a matched control group; evaluation of the personality questionnaire; and factor analysis of 103 items of Part C of the questionnaire.

On the item analysis, 402 out of 612 persons reported 559 influenced watches or clocks, and 151 out of 612 reported 243 cutlery deformations. One-quarter of those reporting on watches claimed spontaneous starting; 17 percent of cutlery reporters described spontaneous deformation. Some people continued with the experiments. Of those who later tried to start a watch, more than three-quarters claimed success. More than half of those who tried to bend metal reported deformation. A quarter of the metal benders claimed that at least one piece of cutlery had bent without having been touched. A fifth reported broken metal objects. Slightly more than two-thirds of the watch and cutlery phenomena were actually witnessed by at least one person. Half the sample thought that Uri Geller caused the phenomena directly, a fifth considered themselves the cause of the phenomena but felt that they were triggered by Geller, and a third had no explanation. Of those who believed in Uri Geller as the cause of the phenomena, 47 percent thought that he might have done it with the help of "unknown forces"; 22 percent thought of "supernatural powers"; 52 percent regarded him as a medium with extraordinary abilities. Only 12 persons took Geller for a cheater. A representative sample of the population was chosen to serve as a control group and was questioned. The persons in this sample had not participated in any watch or bending experiment. Parallel groups were formed by age, sex, and education to the Geller sample; each resulting sample comprised 191 persons. Not surprisingly, the Geller sample reported a closer connection to an occult attitude towards life than the controls at a proportion of 50 to 30. Though none of the control group persons had observed any Geller phenomena, 95 percent had heard of him. This meant that Geller was better-known than the German chancellor! Half the control persons believed that the "Geller effect" was a "good trick," whereas only 2 percent of the Geller sample regarded Geller as a cheater.

The evaluation of the personality questionnaire for the Geller sample showed no significant deviations from the norm. Three possible alternative explanations may be given for this result: the Geller sample was indeed not different from the general population; the questionnaire was not answered honestly, which was highly improbable because the deception scale

value was not significant; or the personality questionnaire did not measure traits that were characteristic of the Geller sample.

A factor analysis was carried out on 103 items of Part C of the questionnaire. Eight factors emerged, of which the three most important were called "belief in destiny," "Geller and miracle fascination," and "intellectual interest in the occult." The last step of the evaluation was a correlation among the factors; all factors showed significant intercorrelations with the remarkable exception of the factor "Geller and miracle fascination." This factor showed no correlation with any other factor. It seemed to be a phenomenon sui generis; a person can be a "Geller fan" without having anything to do with the occult.

In conclusion, our inquiry made it possible to describe the social-psychological aspect of the "Geller effect." It still remains unexplained why Uri Geller at this particular time could cause a public reaction unique in the history of both social psychology and parapsychology.

FURTHER INVESTIGATIONS OF SPONTANEOUS AND EXPERIMENTAL PK BY THE FREIBURG INSTITUTE

Hans Bender (Institut für Grenzgebiete der Psychologie)

In addition to the survey reported in the previous paper, the Freiburg Institute has also engaged in field research and experimentation related to the Geller effect. The most remarkable case of spontaneous "Geller reaction" was reported by a family in Karlstadt, a small Bavarian town. It included up to 54 observations of deformed cutlery. I investigated this case thoroughly and found it particularly interesting in that police inspectors, who were called for help, were among those who witnessed deformations of untouched cutlery.

I also did some experiments with six "mini-Gellers" who claimed to be able repeatedly to produce metal deformations. Five of the six were found to cheat when controlled experiments were done with them. One of them produced seemingly genuine phenomena, but could not be filmed. We lay special stress on getting visual documents (films and

videotapes) which should be recorded in a way that they demonstrate as well as possible a fraud-proof situation.

Since 1975 I have done experiments with the Swiss designer Silvio, the most important genuine metal bender we have found. Sessions in which he has bent metal forks and spoons by holding them between two fingers have been recorded on videotape, as well as sessions in which he has bent a plastic spoon, silver coins, and an untouched spoon. In one session, Silvio was studied by Dr. Betz of the Institute of Physics of Munich University; he remarkably increased the resistance of an electrical device. The collaboration with physicists will be continued.

With the French subject J. -P. Girard, the Freiburg Institute executed successful experiments with metal bars hardly pliable by muscular force (28 Newton/meter were required), which he however deformed by slightly touching them. We succeeded in registering the procedure on a 3 mm. film without interruption (beginning by rolling the bar on a table). The Paris trick expert Mr. Ranky declared this filmed scene to be convincing. Encouraging pilot experiments on PK influence exercised on living organisms (such as bacteria) have been done with J. -P. Girard in collaboration with the Institute of Biology of Freiburg University; these will be continued.

SOME PROBLEMS IN TESTING "MINI-GELLERS"

A. J. Ellison (The City University, London)

In the autumn of 1973 Uri Geller first demonstrated his abilities on television in the United Kingdom--and it subsequently proved for various reasons somewhat difficult to arrange "satisfactory" scientific tests with him as the subject. However, a number of children afterwards claimed to produce similar "Geller phenomena." The following is a brief description of what was learned from a preliminary investigation in the home of two such children and of tests in the laboratories of the Electrical Engineering Department of The City University, London, subsequently organized and carried out by a multidisciplinary committee set up especially for such tests. The committee consisted of professors of experimental and theoretical physics, two psychologists, a lawyer, a medical practitioner, two of Geller's earlier investigators

(one a physicist, one a mathematician and chemist), and professors of electrical engineering. Five universities and a polytechnic institute were represented. Several committee members were also Council members of the Society for Psychical Research with many years' experience of parapsychology, and one was a member of the Magic Circle, who arranged for a second and professional magician to serve as a consultant.

The children were a boy of 13 and his sister, a girl of 11. A younger sister of seven was slightly involved. Their mother had kept a careful diary of all the Geller-type occurrences in her family since they started during a Geller television program. Claimed phenomena included bending of metal by stroking; bending of plastic and of wood; disappearance of small objects from a box, after concentration; translation of objects (including one of the children), usually instantaneously but occasionally by floating, from place to place; ESP experiences, including tracing of letters from a newspaper hidden under several layers of paper, telepathy of diagrams and words between members of the family, reading of cards by touch, and veridical "visions"; hearing a voice giving the thoughts of one of the children from a radio containing no batteries; attraction between the boy's body and objects, usually of metal (e.g., a metal sphere striking the boy on the head); rapid changes of weight of the boy on bathroom scales; and rapid opening of closed flower buds by cupping in the hand. Symptoms similar to those of epilepsy, and headaches, were also described. Later the boy described ostensible communications with his deceased grandmother.

Three of us went informally to the children's home to see for ourselves, buying heavy-gauge teaspoons in plastic bags on the way. The two older children each gently rubbed a spoon inside its plastic bag, while closely observed. One bent in a minute or so; the other did not. We ourselves tried similarly without success. The same two children each held a tightly closed tulip just above the slightly cupped hand. They opened in about five minutes; when we repeated the exercise, ours did not open. The children tried to make a button disappear from a matchbox, with no success. There appeared to be no attraction of a metal globe to a child's head. Telepathy experiments with drawings also showed no success. However, the elder girl produced a remarkable success when she tried to draw a cartoon which was in a folded newspaper brought in by me an hour or so before and which no one present had seen. The same child achieved a

remarkable success in drawing a small object held in the palm of one of us. It must be mentioned that the investigators were completely satisfied regarding the transparent honesty of the parents, who were concerned about the children's health. The parents agreed to laboratory tests in London, being reimbursed for expenses only, provided the media were not involved.

The visit to the family home had provided sufficient prima facie evidence to warrant a more controlled and scientific investigation, so the whole family (parents and three children) were brought to London for one week for tests in the laboratory. The multidisciplinary committee carried out experiments in the following areas. Metal bending: Polycrystalline spoons with locked-in stresses are of little value to a crystallographer, so short bars of single crystals of several pure metals were prepared suitable for crystallographic and metallurgical examination after paranormal bending or breaking. Also prepared were a strain detection apparatus for a continuous check during rubbing, and plastic specimens for viewing in polarized light, again to show up strain. We also arranged means for detecting current and voltage in and across metal and plastic specimens. We started these experiments with the children's attempts at the familiar spoon bending with closed-circuit television recording, and then went on to the single crystal specimens. Every specimen was accurately weighed before and after each trial. We had little success except that one of the single crystal specimens fell in half while being stroked. Apparent loss in weight of the boy: We arranged an air bed on a platform weighing machine, a manometer enabling us to record small changes in weight. A result here showed very slight promise, and was perhaps a result of hypnotic suggestion by myself.

The following experiments yielded, under the conditions described, essentially negative results. PK attraction of metal objects: We arranged rectangular specimens on an air table so that minute forces would cause movement. If a child were able to attract specimens, then infrared equipment and other equipment to detect electrostatic and magnetic fields was to be brought in. We also arranged means to detect small changes in periodicity of swinging pendulums passing a light beam to see whether the child affected gravitational mass. Flower opening: Flowers in season, a camera, and a clock were arranged to test the claim that buds opened quickly when cupped in the child's open hand. Favorable results would have led to further statistical work with

the advice of a botanist. In addition, experiments were made to see whether the children could alter the speed of a chemical reaction; i. e., to accelerate the change of color of a substance in solution. GESP: We arranged card-guessing experiments to test the claimed GESP ability, with the mother as agent. The girl was also asked to duplicate on the outsides of envelopes drawings sealed inside. Tests with a Schmidt machine and EEG tests done while the two older children attempted paranormal metal bending finished out our repertoire.

Although very few positive results were obtained, the development of the experiments and their use with the subjects brought to light a number of matters of great importance for future investigations. One of the most important was this: newly developed experimental techniques devised to detect and measure small physical quantities in new ways using the resources of modern laboratories often lead to artifacts appearing to indicate the presence of the phenomena being investigated. Future experiments must devote more attention to using experimental equipment first with subjects claiming no paranormal faculties in order to discover and remove these artifacts as far as possible. Clearly, such apparatuses should always be as simple as possible. Additionally, in view of the difficulties of producing unusual psychic phenomena in the completely unfamiliar atmosphere of a university laboratory, and especially in the case of children, "laboratory inhibition" must be expected and allowed for. Experiments which can be done in the familiar surroundings of a subject's own home are best. The bending of small bar specimens inside sealed tubes is one such experiment. Many others involving more elaborate equipment come readily to mind.

Next, there is reliable evidence that psychic faculties can sometimes be trained using hypnosis. It is important not to assume that results which the subjects were unable to achieve were impossible; they might be possible after training under hypnosis. Hypnosis would certainly be useful in removing laboratory inhibition and putting the subjects, in effect, in the familiar surroundings of home. Finally, the girl subject described, relative to ESP experiments, her hallucinatory vision of a television screen on which a point of light sometimes drew the hidden drawing which she was asked to reproduce. Uri Geller describes a similar experience. Such a psychic experience may sometimes be veridical, and it is important that the circumstances of an experiment should permit the use of such psychic aids.

REFLECTIONS ON PSYCHOKINETIC PHENOMENA

Zbigniew William Wolkowski (University of Paris)

I would like to relate the development of work with Jean-Pierre Girard, a 35-year-old French business executive whom I met in October 1975 as a result of a radio broadcast in Paris; at the end of the broadcast I asked for a response from persons capable of producing a Geller effect. At our first session with Girard my colleagues and I presented him with metal bars, quite sturdy, and composed of different metals. He began in a manner which was to become very habitual with him, holding the bar in one hand and stroking it lightly with the other; very soon the bar began to bend. We noticed informally at the beginning of the experiments that he was capable of transmitting metal bending in an auditorium, for instance, to other people: phenomena included bending a coin carried in another person's hand, or a key, or a single metal bar. According to Girard, if he could feel a certain resonance with this person, the success rate was over 70 percent.

We then embarked on a more sophisticated experiment which consisted of sealing different metal objects in Pyrex glass tubes; these were completely sealed with a torch. They were weighed with a precision of 10^{-4} grams and measured with a precision of 10^{-4} meters, and were left with Girard. Inside we had placed different metal objects such as metal paperclips and steel springs of the coil kind; when they were returned to us they were quite remarkably bent, from 10° to 30°. The steel spring, for example, which was straight at the beginning, was now so distended that at one point it could no longer move freely in the Pyrex tube. All the tubes were still the same weights and the same dimensions, and the glass blower could not detect any tampering.

From then on Girard became a collaborator in my laboratory; he explained the fragility of the phenomena involved, and he stressed the influence of the state of mood, of the environment, and of the attitude of persons present. All these factors, according to him, made the results quite unpredictable. I may add that Girard is a highly emotional, susceptible, anxious person. One of his convictions was that he could perform better and faster if he could feel the metal objects before they were sealed in the tubes. So we presented him with sturdy metal objects which he could manipulate,

then checked to see if they were still undeformed, and then sealed them immediately in tubes; the rest was recorded on videotape. Some dramatic bending occurred, but unfortunately the videotape stopped operating when the bend was occurring, so we don't have a complete record of it.

I then invited several eminent scientists to meet Girard, including some at this symposium. Since then Girard has been investigated by at least six independent groups in Europe; these studies have confirmed the authenticity of his phenomena. The kinds of effects he can produce are not limited to those discussed above. Girard in our laboratory succeeded in influencing a liquid crystal thermometer at a distance of about a meter and a half; he caused it, for instance, to shift readings from 22° to 26° C. This was also done repeatedly using encapsulated liquid crystal sheets, producing, for instance, a shift of 19° to 25° C at a distance of one and a half to two meters. Another phenomenon we observed was that a number of times Girard was able to produce a movement of an object at a distance of 10 to 50 cm. Usually he would sit down on the floor and we would place the object in front of him after checking it for invisible threads; after 10 to 15 minutes he would produce movement of these objects. He preferred cylindrical oblong objects; the movements were of the order of a few centimeters at a time, in quick jerks, and they moved just as readily towards him as away from him. Once he even induced what we thought of as a partial levitation of an object 1 or 2 cm above the surface.

As I continued my investigation I began to receive indications that the physical correlates I sought were but the tip of the iceberg. Since then I have attempted to approach this problem in a wider, more holistic manner, obtaining descriptions and interpretations from persons endowed with intuitive sources of information. These included a wide range of persons such as psychics of different cultures, yogis in India, and spiritual healers in North and South America. I began to derive a picture of the dynamics of physical phenomena of the Geller, Palladino, and Girard type. They appear to consist of an opening up to "energies," for lack of a better word, of a primitive unstructured type far below the quality and intelligence of human consciousness. The opening up to the flow of such energies creates a link, a situation of mutual dependence, between the subject and his environment. The personality profile of the circus magician, of the prankster or the conjurer, is particularly well-suited to attract such forms of energy, and create a set of what could be

called "forbidden transitions" across these boundaries (for example, as understood in particle physics). The subject has only partial control of the phenomena, since he is but the channel through which the energies may manifest themselves.

This interpretation unavoidably raises moral and ethical questions. It is no coincidence that the best-documented study of metal bending by Uri Geller comes from a weapons research laboratory. Although my remarks can be simply dismissed as meaningless speculation, beyond physics and beyond science, we cannot dismiss the moral responsibilities which we share. There is a great responsibility to train, stimulate, publicize, and finally apply such PK effects. I believe we tend to ignore the effect on the person producing such PK effects and opening himself to what may be interpreted as a flow of base primitive energies. When PK performers are a rare and precious commodity, when scientists are anxious to prove their claims, and eager to please their patrons, it is difficult to be optimistic about the outcome of such a situation in the "PK market." I feel that great care should be taken when encouraging and developing PK in subjects and I wish to stress the responsibility of the researcher in exploiting and driving the PK effect towards its possible applications.

PSYCHOKINETIC STRETCHING OF AN ALUMINUM BAR BY JEAN-PIERRE GIRARD

R. D. Mattuck† and Scott Hill (University of Copenhagen)

In January 1976 the French medium Jean-Pierre Girard was invited by G. Wikman and ourselves to participate in a series of psychokinetic metal-bending experiments to be carried out at the physics laboratory of Chalmers Technical University in Göteborg, Sweden. The experiments took place on the evening of February 21, 1976, and the afternoon of February 22. The observers present were ourselves, G. Wikman, Lic. Lett. Mlle. Maryse Delphin (who acted as French-English translator), Dr. Mogens Levinsen (Physics Laboratory I, H. C. Ørsted Institute, University of Copenhagen), J. Fjellander (Stockholm Research Center for Psychobiophysics), and Corinne Girard, Jean-Pierre's wife.

M. Girard had requested an assortment of specimens

to choose from, so he was given a wide variety of bars and rods, made of stainless steel, brass, aluminum, and copper. The rods were 11 to 25 cm in length and 5 to 8 mm in diameter, while the bars were 25 x 2 cm and 2 to 4 mm thick. Eight of the rods and bars had one or two strain gauges attached in order to measure bending, and could be plugged into an electronic recording apparatus by M. Girard, one at a time. There were two video cameras to provide a visual record of the proceedings. M. Girard held and stroked a large number of specimens and, without any apparent use of force, bent many of them through angles of up to 15°, including a 3-mm aluminum bar, a 3-mm steel bar, and a 5-mm steel rod. However, in all cases except that of the aluminum bar the controls were inadequate, so that no conclusion could be drawn. Therefore in this report we will consider only the aluminum bar, which was bent through an angle of 6.7°.

The bar was 25 x 2 x 0.3 cm and had a transverse sawcut 0.9 mm deep and 1 mm wide at the midpoint. There was one resistive strain gauge mounted at the midpoint, on the face opposite the sawcut, and another on the same side as the first, 5.3 cm from the midpoint. The gauges were held to the metal by acrylate cement and by a polyester sealing compound which covered the outer side of the gauge and the immediately surrounding metal. Each gauge was connected to one arm of its Wheatstone bridge, and produced an amplified output of 360 millivolts per 0.01 mm change in length, or 630 mv per degree of bending. In terms of bending moment, one gauge gave 111 mv/Newton-meter (9 N-m required for permanent bend), and the other gauge gave 64 mv/N-m. The output went to a six-channel Brush recorder which was located, together with the bridges, on a table about one meter from M. Girard's table. The witnesses sat around M. Girard's table at distances of one to three meters.

For this aluminum specimen we have a chart record of the voltage output before and after the period in which (at least the major part of) the bending occurred, as well as a video film of M. Girard handling the specimen in the pre- and post-bending periods. We have no chart record during the bending interval, since the bending unfortunately took place at a time when the specimen was unplugged and the Brush recorder was off. Hence we are unable to make any statement as to how this bending occurred. Nevertheless, we feel that those data we do have constitute evidence for an unexpected phenomenon: a <u>stretching</u> of the aluminum bar at the bending

point. The bend occurred exactly at the sawcut, the gauge being on the concave side of the bend. The total change in the output of the gauge during the bending period was +40 mv, which corresponds to a stretching of the concave face of about 0.001 mm. On the other hand, all duplicate specimens bent mechanically by us showed a compression on the concave face of about 0.1 mm (corresponding to about -3 volts). This indicates that at the bending point there was an overall stretching of the neutral axis of the bar of slightly over 0.1 mm which was superposed on the bending. This stretching, then, more than canceled the compression of the concave face caused by the bending.

This argument is supported by the observation that the surface of the sealing compound at the bend was as smooth as before bending, whereas all our mechanically bent specimens showed compressional ridges in the surface of the compound at the bending site. This indicated that there was no net compression of the concave side of the metal at the bending site; i.e., that the normal shortening had been canceled by a lengthening of the neutral axis to the same magnitude. Additional evidence of stretching occurred in the three-minute interval after the bending took place, when the Brush recorder and video cameras were operating continuously. This interval consisted of eight stroking periods of 5 to 40 sec in duration, separated by rest periods of 2 to 30 sec. The bending moment from stroking ranged from 0.3 to 1.0 N-m. The voltage level at the end of six of these stroking periods was between 15 and 35 mv higher than at the beginning of the stroking period, corresponding to stretching between 6.5×10^{-4} mm and 1.5×10^{-3} mm. The voltage was constant during each rest period. The total voltage change in three minutes was +110 mv, corresponding to a total stretching of 4.7×10^{-3} mm.

One alternative to the stretching interpretation is that the gauge "slipped" or "crept" relative to the aluminum bar. If the acrylate bond broke, then the gauge might creep in the slightly plastic sealing compound. Or if, due to some rare fabrication error, the phenolic resin base of the gauge was insufficiently polymerized, the gauge could creep in the base. Such slip or creep would mean that the length changes of the gauge would not follow those of the metal surface. Tests on the gauge showed that after the bending the acrylate bond was indeed broken, with the result that the calibration changed from 111 mv/N-m to 71 mv/N-m. Hence creep effects were a genuine possibility.

To find out whether creep could explain our results, we prepared 11 duplicate specimens out of the same aluminum stock. We varied those things which could be expected to influence creep: state of the acrylate bond at the start (good, poor, none), total bending time (from several minutes down to one second), thickness of sealing compound, manner of bending (using two hands, over a table edge, etc.), and physical agents applied directly to the sealing compound (pressure, tension, heating up to 170° C). We found for a mechanical bend of about 7° that the voltage output varied from about -4 volts to a good bond down to -0.3 volts for no bond. Heating reduced the magnitude of the output voltage by only about 20 percent. So poorness of bond or possible presence of elevated temperature cannot explain our results. None of the other parameters had any appreciable influence. With regard to the several small increases of voltage which occurred after the bending, we attempted to reproduce this effect by stroking the duplicates immediately after bending them, using the same bending moment as M. Girard used. We also tried this on the original aluminum bar. In no case could we obtain a total change of more than +15 mv (which fell to zero about five minutes after the stroking was stopped.) Our general conclusion, then, is that creep is certainly present, but it is not nearly large enough to account for our results.

Another possibility is drift in the Wheatstone bridge. To explain our results, this drift would have to be zero during the two-minute period prior to the bend, about 1 to 4 volts positive during the bend period, when the bar was unplugged, and between 0 and 35 mv positive in the post-bending period, and occur only during those intervals when M. Girard was stroking the bar. It is especially this last which we feel is so unlikely as to rule out the possibility of bridge drift as an explanation. Other apparatus defects, such as temporary failure of the amplifier, seem to be excluded by the fact that the stroking peaks were registered normally during the post-bending period. And of course no apparatus error could explain the absence of ridges in the sealing compound.

It requires only an 8-kg force applied 11 cm from the bending point to create a permanent bend in our specimen. But this produces a stretch on the convex side and an equal compression on the concave side, so there is no net lengthening of the specimen. On the other hand, to produce a permanent net stretching of the whole bar requires a

huge tension: 1 ton of force. (The reason for the much smaller force required for bending is that one has a large mechanical advantage equal to the ratio of the bending arm to the half-thickness of the bar, a factor of about 100). No human being can produce such a tension without special apparatus, which M. Girard clearly did not have. Of course the tension required is much less if the specimen is heated up almost to its melting point. However, such a temperature would have destroyed the strain gauge.

Another way to reduce the tension required for stretching is by irradiating the specimen with ultrasonic waves. This requires a resonator in direct contact with the aluminum bar, a 200-watt amplifier, and a large storage battery, none of which could be concealed. One more possibility is fatigue stretching. Dr. C. Crussard has tested this by bending an aluminum bar back and forth plastically through 5° to 10° several times. He found a net stretching of about .008 mm, which is too small to cancel a compression of about 0.1 mm. Dr. Crussard has observed a similar stretching of an aluminum alloy handled by M. Girard. Our data also fit in with results obtained by J. Taylor, who found that some metal specimens fractured by PK show an undisturbed grain structure near the fracture, which is typical of tensile rather than ductile fracture. Finally, it should be mentioned that it could have been just the strain gauge and sealing compound that were stretched. However, as indicated above, application of tension, pressure or heat directly to the seal did not produce any stretching effects large enough to account for our results. Note that the stretching can be interpreted as due to an anomalous softening of the bar at room temperature, accompanied by application of slight tension, with subsequent hardening. Such PK softening appears to have been produced by Geller. We are doing an electron microscopic analysis of the grain structure at the bend to investigate this possibility.

In conclusion, it seems to us that Jean-Pierre Girard produced a stretching of the aluminum bar (or conceivably of just the strain gauge and seal) without the use of any physical methods, i.e. by psychokinesis. It would be highly desirable to repeat the experiment using an independent method to measure the length of the specimen, both to eliminate the possibility of some source of error we might have overlooked, and also to decide the question of whether it was only the strain gauge which was stretched paranormally.

THE GELLER EFFECT AND PHYSICS

John G. Taylor (King's College, London)

The "Geller effect" is the abnormal deformation of metal (and possibly other) objects first brought to public notice by the Israeli entertainer Uri Geller. As demonstrated by Geller, a spoon is gently stroked at its narrowest portion; it suddenly becomes as if plastic there to such an extent that it might even break in two pieces. The phenomenon is abnormal in that the amount of pressure being applied appears far too small to cause such distortion or fracture. As is true of all other paranormal phenomena, validation of the Geller effect is non-trivial. Many cases have been reported in the media of the effect's being caused either by Geller or by other subjects who have since come forward. It is necessary to go beyond such reports, both to validate and to probe the effect further. The next level of investigation is that of close personal observation. This is still not acceptable to some, so care must be taken to exclude fraud of every conceivable sort.

Five general types of fraud might produce Geller-type phenomena; each can be and, in fact, has been prevented in various investigations in the following ways. The simplest kind of fraud is the application of unnoticed mechanical force. This can be prevented by placing the object in a sealed container or otherwise making direct contact with the object impossible. If contact is found to be necessary the pressure applied can be measured by a pressure balance or a piezoelectric pressure transducer. Additionally, the contact required for the phenomena to occur is often inappropriate to produce them mechanically (i. e., stroking the upper surface of a metal strip which then bends upward against the pressure). The second type of fraud is replacement of the object with a duplicate which has already been bent. This can be prevented by marking the object, by fastening it to the surface, or by using an object so unusual that it is extremely difficult to duplicate.

Use of chemicals constitutes the third type of fraud. Aside from prevention of direct contact with the object, one can guard against this by observing and analyzing the surface of the object after it has been bent. Characteristic cracks arise from chemicals causing metal distortion. This is an unlikely explanation for the many cases in which the subjects

are children, since such chemicals would be inaccessible to them. The fourth type of fraud is application of heat, such as by a laser beam from a concealed source. One can check on this by measuring the temperature of the specimen being distorted, for example by thermocouple. Again, child subjects are unlikely to have access to high-powered lasers. The final source of fraud is that produced by the experimenter himself, and the only protection against it is a lack of bias on the experimenter's part.

Two specific cases appeared to avoid these methods of trickery. In one of these Geller stroked the upper surface of a strip of metal which was screwed down to the top of a letter balance. The pressure he was applying could be monitored directly by the balance dial (accurate to 7 grams) as well as measured automatically by a shielded metal plate placed vertically above the metal top of the balance and connected to a quadrant electrometer (sensitive to 5 oz.). A bending of 10° occurred in one session, which I closely observed, during which no pressure greater than 10 grams was applied. In any case the bend was upwards. This experiment is presently being repeated with videotape. A videotape was obtained of a spoon bending on its own, though I was not present during the session. The subject was a 16-year-old boy. The other case avoiding fraud was that of a metal strip bent in a sealed tube. This was not done in my presence, but the seal was completely undisturbed, small marks on it being unchanged. Criticism of the sealing procedure has not proven adequate, so that this case appears a good one for validation. However, none of the above cases can be regarded as absolutely fraud proof.

There have been attempts to discover various features of the Geller effect by measuring several physical parameters, including temperature change, ultraviolet and infrared emission, magnetic and electrical fields, current flow, changes in grain size of the metal, presence of delayed bending and range of material of the object. Some provocative data are beginning to be obtained. Enough precautions were taken during these experiments to enable one to regard the results seriously. The most interesting feature discovered was that most bending occurred with non-annealed specimens, as if the internal stress energy (dislocation energy) were being utilized. Another aspect is that objects have been observed to bend even hours after initial contact by a subject.

Various theoretical explanations for the Geller effect

could be considered, the most immediate being that it is caused by electromagnetism. Electromagnetic detectors have been used to test this hypothesis, and attempts have been made to cause metal objects or crystals to bend or break by exposing them to strong fields of a range of frequencies. No success has yet been obtained.

DETECTION AND ANALYSIS OF PSYCHOKINETIC METAL-BENDING FORCES

J. B. Hasted (University of London)

A "sensor" has been developed consisting of a resistive strain gauge mounted inside a metal specimen in the form of a latchkey; the off-balance signals from an electrical bridge are amplified and chart-recorded. In this way alleged psychokinetic forces two to three orders of magnitude smaller than those necessary to deform metal specimens permanently are recorded when sensors are operated in the neighborhood of selected subjects. Strain pulses in either direction, each lasting up to several seconds, are recorded, typically at intervals of several minutes, during sessions of up to two hours. Sometimes they increase in magnitude until yield point is reached and bending occurs. When there is a force but no permanent bend the chart-record trace after the pulse continues at its previous level; but when a permanent deformation is observed, the trace shows a permanent deflection, in the correct sense, in the sensitivity range 0.1-10 mv/deg. The sensitivity depends on the relative positions of bend and sensor.

This apparatus has been used with several subjects, including Nicholas Williams (age 17) and Andrew G. (age 11). Use of two and of three sensors simultaneously with Williams at seven sessions has produced synchronous signals which suggest the following model: the strain pulses are produced when an otherwise undetectable moving "active surface" encounters and passes through the sensors. The sensors are suspended from their electrical connections so as to define a line or a plane oriented appropriately with respect to Williams, who is, so far as is possible, kept relatively stationary, being seated at a working surface and occupied in building model aircraft. He knows of the positions of the sensors and is asked to bend the latchkeys. The typical distance from

the subject to the sensors is five meters, and between the sensors, one to three meters. The subject does not in general have direct contact with the latchkeys.

The total number of strain pulse events recorded was 81. Many contained considerable "fine structure," which indicates flexibility of the "surfaces." Typically, no visible bend results. The most usual configuration of the surface is vertical, and extending outwards from the subject, as opposed to extending around him. To a small extent the subject has learned to produce synchronous signals in other configurations. Since synchronous signals can be obtained with sensors on opposite sides of the subject the surface may be considered to contain his person. A surface has been known to extend in length to about 10 meters. The rate of travel of a surface between sensors is in the range of 10 to 100 cm/sec. On encountering a screening obstacle a surface flexes its shape around it, so that partial screening of the sensor advances or delays a strain pulse; a metal screen can augment the intensity of the strain signals and bring about bending and fracture.

The folding of pairs of thin metal strips and the twisting of pairs of wires have been recorded on many occasions. This indicates that a flexible active surface can rotate about an axis in its own plane, catching the strips or wires in front of it. This process appears to be inhibited if the subject (or investigator) watches it. The speed of rotation has been measured by mounting the strips or wires in the neighborhood of the probe of a fluxgate magnetometer. One only of a pair of strips or wires is made of tinplate and is permanently magnetized. When a folding or twisting event occurs, the chart record of the time-varying magnetic field shows a number of peaks corresponding to the number of twists or folds, or to a small multiple of the number of folds, if a more complicated motion occurs. The speeds of rotation so measured in 14 events are in the range of one to three revolutions per second.

It is suggested that the paranormal fabrication of paperclip "scrunches" within glass spheres takes place by active surface rotation. A small hole must be present in the sphere, otherwise the refraction of the surface will inhibit its penetration; the axis of rotation is able to penetrate through the hole, and produce the twisted pairs of paperclips which make up the "scrunch." However we must not forget that these paperclip scrunches are not validated, in that

visual witnessing has not been achieved, and that, given time, remarkably imitative scrunches can be effected, working with tools inserted through the hole. Active surfaces might conceivably be responsible for the telekinetic horizontal pushing of small objects, the motion of which is usually reported to be jerky. Since a three-point load must be applied for distorting metal, bending surfaces would differ from telekinetic ones in possessing a single fold. It is not yet known if the force is normal to or in the plane of the surface. Subsequent studies with two sensors within a single metal specimen have provided evidence that the force involved in metal-bending is most likely to take the form of an extension pulse in the plane of the active surface. Conceivably the surfaces might be interpreted as boundaries between local universes in the many-worlds interpretation of quantum mechanics. This would require the non-physical mind to be able to subtend, but not to distinguish among, several simultaneous universes.

SYMPOSIUM*:

NEW CONCEPTS IN RSPK RESEARCH

A CASE OF RSPK IN MASSACHUSETTS: PART I--POLTERGEIST

Fred M. Mathews† and Gerald F. Solfvin (Psychical Research Foundation)

This is a report of a poltergeist-haunting case which recently took place in New Bedford, Mass. The case was brought to F. M. M.'s attention by a social welfare worker. The family comprised Mrs. S., age 67, and her three great-grandsons. For the past three months, they had endured the spontaneous activity of objects moving about the house. The social worker reported seeing the aftermath of several such incidents, although she had never witnessed the disturbances firsthand. While the initial activity remained localized in the boys' bedroom, it soon increased in duration and progressed to the other parts of the house.

Father Sullivan, the family priest, had been summoned to their home on several occasions. He described the activity in the form of rappings in the wall, dresser drawers shaking in their racks, and clothing repeatedly landing in heaps on the floor. In addition to the physical activity, Mrs. S. reported hearing shallow breathing sounds coming from one of the empty rooms. The police were called in several instances but they were unable to trace the source of the disruptions. As a means of reducing the turmoil, the family had stayed in a motel for a week. The phenomena pursued them. The boys were also placed in foster care for several weeks and although nothing occurred during their absence, the incidents resumed upon their return.

F. M. M. made contact with the family on February 3,

*Chairperson: William G. Roll, Psychical Research Foundation.

1975, three days after they had moved into a newly constructed apartment complex. The walls of the living room and those leading up the staircase were already gouged by what was said to have been the result of the continuing poltergeist activity. Although Mrs. S. had retired as a nurse's aid to become their legal guardian, Welfare was threatening to put the children in foster care if the disturbances persisted. Mrs. S. was reluctant, therefore, to have an observer on the premises. However, she realized that F. M. M. 's presence could be helpful, if only to inhibit the activity, and she agreed to the controls that would initiate the investigation. She also offered the keys to her home and allowed him to enter unannounced.

The constant stress they had to support was reflected in the mode of their daily lives. For instance, since the dishes were subject to sliding off the table, Mrs. S. limited use to only those that were immediately necessary. The boys were constantly reminded to hold the plate with one hand while eating with the other. If the confusion persisted, they would eat standing at the counter. "It's like fighting a war," Mrs. S. remarked.

Lonnie, the youngest and the most stable of the three brothers, was of preschool age. Since no disturbances occurred while he was the only child at home, the focus centered on the two older boys. Patrick, age seven, was assumed to be responsible for the abnormal activity. When he misbehaved Mrs. S warned, "Patrick, you know what happens when you get sassy" (meaning that after his being scolded, the objects would be triggered into some kind of activity). His brooding personality exploded into fits of temper for extended periods of time. In school he was extremely troublesome, although his grades were satisfactory. At night, he rocked himself to the point of exhaustion, slept in a fetal position and, like his younger brother, had nightmares he could neither recall nor relate. Michael, age nine, was the silent one and seemingly kept his emotions under control. Unlike Patrick, his report card revealed he was failing nearly every subject. As a result of the dangers the phenomena created, he was relegated to the role of household guard. Therefore, most of his activity centered in the kitchen doorway where he was in view of the stove and the objects that were drawn to it.

Two clear-cut incidents established the paranormal quality of the situation. F. M. M. 's first poltergeist encounter

left an impression on his memory unlike any other experience. The boys were at the table awaiting their dessert. The dishes had been cleared, leaving an uncluttered view of the table. F. M. M. was standing about five feet away surveying the area. A teaspoon located between Lonnie and Patrick jumped into the air, a movement comparable to that of a jumping frog, leaping without seeming to brace itself. The thrust it attained dissipated as it left the surface of the table. The boys were preoccupied, taking no notice of the object until its fall drew their attention.

F. M. M. 's second encounter occurred on another occasion as he entered the house unannounced. A floor lamp, usually located on the far end of the couch, fell into the middle of the room. In this case, F. M. M. saw only one-half of its purported trajectory. The children were seated on the floor watching television. All of them were well out of reach of it and an immediate check assured the incident had not been contrived in any way. On that particular day, RSPK was reported to have been extremely active. Moments before F. M. M. 's arrival, the family claimed that an 18-pound record player was thrust about, narrowly missing the oldest child. As it moved from one side of the room to the other, the boys strove both to control it and avoid being in its trajectory. They were unsuccessful in their attempts and the record player traveled 63 feet around the room, reaching heights of five feet before coming to rest of its own accord.

On February 17 William Roll, project director of the Psychical Research Foundation, sent G. S. to assist in the investigation, whereupon we installed 16-mm movie cameras with time-lapse mechanisms on both floors of the home. The added use of several strategically placed mirrors reflected to the camera's lens 65 percent of the areas of high activity. Peak periods were monitored automatically while the off-hour activity could be caught by the flip of a wall switch which served as the power source.

Alternately, we monitored Patrick and Michael with a portable electroencephalograph (EEG). The electrodes were connected to a miniaturized transmitter which clipped to the belt. By connecting the receiver to a stereo tape recorder, one side of the tape recorded the brain emissions while the other, connected to a microphone, simultaneously picked up the external activity that would lead to anticipated RSPK events.

No events were registered either on film or during the

telemetry recordings. Further clinical EEG examinations, completed at a local hospital, indicated no abnormality.

Dr. Julie Claire, a child psychologist who interviewed the boys without considering the reality of the phenomena, concluded they were acting out their resentment by exploiting Mrs. S.'s fears. In turn, Mrs. S.'s own convictions of the reality of the phenomena frightened them into believing their own fabrications. Our own conclusions agreed, in part, in view of Patrick's having been caught producing a fraudulent event. However, as suggested by Roll, previous observations of this kind have determined that there is a tendency to intersperse artificially reproduced events with declining genuine events. At the time, the poltergeist was on the wane.

Dr. Claire's conclusion, i.e. that the boys were seeking some vicarious form of revenge, may point to one of the genuine conditions required to initiate and direct the phenomena. An interesting side note to this case is that the boys, for fear of being ridiculed, never mentioned the disturbances outside their home. Had they manufactured them, they might have had good cause to share their success with their friends.

It was noted that RSPK functioned in varying ways, depending upon the circumstances and the type of objects involved. For instance, small objects such as toy soldiers swirled in the air repeatedly. Larger objects such as a cushion or a book seemed to be more controlled, insofar as they maneuvered around obstructions and doorways.

There also seemed to be a weaker PK force lingering in certain objects previously charged during the disturbances. Such objects did not require the presence of the focal person(s). The boys were away at the time that Mrs. S.'s rocking chair tipped over several times. If one may assume that PK follows a line of least resistance, then the rocker would be more likely to tip backward or forward, as indeed it had, in their presence, on previous occasions. Contrary to this assumption, on two separate occasions, the rocker tipped to its left side. This fixed recurring deviation suggests that the PK force may be specifically located in the object.

During the six weeks of the investigation, 184 incidents were reported. Of these, 48 centered around the stove. Michael's position as "household guard" made him responsible for keeping the dangers at a minimum. The family reported that at the onset of the trajectory, the knob would turn itself

on and the burners would ignite as the object reached the stove. Several fires occurred and numerous articles were destroyed in this manner. After a protective shield was made to secure the burners, the objects continued to soar to the stove and to wedge themselves under the covering.

We were unable to separate the children long enough to conclusively establish the agent. The patterns of the events suggest that one or both of the older boys may have acted as the agent. It is evident that Mrs. S.'s dominance over the boys, the wide age gap and the resulting lack of understanding contributed to a great buildup of tensions. Patrick's extroverted behavior dissipated some of this energy but Michael had no outlet. The rigidity of his personality, combined with his difficulties in school and at home, displayed more of the characteristics commonly associated with poltergeist agents.

In the course of debriefing the participants, we discovered that the disturbances in the original home were continuing with the new tenants. The similarity of the continued events to those reported in the early stage of the inquiry compared favorably with the new developments and suggested that a haunting may have triggered the poltergeist.

A CASE OF RSPK IN MASSACHUSETTS: PART II--THE HAUNTING

Gerald F. Solfvin† and Fred M. Mathews (Psychical Research Foundation)

We have reported this case in the chronological way our investigation proceeded. At this point the case seemed to be a classic poltergeist outbreak with all of the principal characteristics--large-scale physical effects, a potential agent (or agents) with the psychological tensions usually associated with RSPK, and the important fact that the phenomena followed the family to their new home. It was here that we began to collect background information concerning the activities in the previous residence and, as we learned more, a new picture emerged. The previous residence where the events began is a two-story frame house in which Mrs. S. and the boys occupied the upstairs apartment. Prior to August 1974 a Mr. C. occupied the downstairs and had been there for many years. Mrs. S. and Mr. C. disliked one another intensely. We later

learned from Mr. C. 's son that in the spring of 1974 Mr. C. had become particularly enraged by Mrs. S. and had told his son, "If it's possible, I'm going to come back and haunt her after I die."

Mr. C. died in August and his apartment was left intact with his furniture in place until October 1, when it was rented to another family--Mrs. T. and her three-year-old son. At this time Mrs. S. began to hear footsteps (apparently with a cane) and television noises, and on one occasion saw an apparition of Mr. C. in her kitchen. The banging noises and movements of objects followed. Mrs. T., unaware of the experiences of Mrs. S. in the upstairs apartment, was simultaneously hearing unexplained noises. In addition to the banging sounds, there were noises such as a chair squeaking, a rocker rocking on the porch, someone rattling the front door knob and the metal mailbox opening and closing. Mrs. T. 's mother, reportedly unaware of her daughter's experiences, was frightened by loud "breathing sounds" while visiting there one day.

In November, physical movements of objects began with chunks of plaster flying off the thick wall in the boys' bedroom upstairs. This is the same wall from which unexplained bangings seemed to emanate. In a week or so a rectangular area of this wall was exposed to its structural members--almost two meters wide from floor to ceiling. We spoke to the landlord about this damage and he angrily verified the extent of this area which he was forced to replaster.

In January Mrs. S. and her three great-grandsons moved to a new apartment and F. M. M. has related the continuation of the events there. The previous residence, however, continued to be active in an interesting way. Mrs. T. in the downstairs apartment, and her mother when she visited, continued to experience the footsteps, breathing sounds, and squeaky rocking-chair sounds that had been typical in the early days of the case.

In February a middle-aged couple paid one month's rent and moved into the upstairs apartment. Mrs. T. reported to us that on their second night they packed their things into their car and left in the middle of the night. Neither Mrs. T. nor the landlord knew why they left nor where they went, so we were not able to interview them.

In March, with the upstairs apartment still empty,

Mrs. T. 's apartment was broken into one night when she was staying elsewhere. The police officers, noting that many valuable items were not stolen, suspected that the thief would return soon after to finish the job and therefore decided to secrete themselves in the downstairs apartment for the night. Although they planned to spend the whole night in the house they left after only one hour there and sat in their car down the street the rest of the time because, as their official report states, "of the better vantage point." When interviewed by us a few days later, the officer in charge stated that they had really left because of all the strange noises in the house, from rats perhaps. The police were completely unaware of the paranormal activities that had been reported in the home, and the house had no history or evidence of rodents.

A few weeks later two young women, each with a preschool child, moved into the upstairs apartment having no knowledge of its history. We interviewed them after they had lived there for a few weeks, and after they had learned from neighbors about the previous happenings. They reported several instances of breathing sounds, being pinched (Mrs. S. had also reported this happening to her in that house), and some very interesting apparitions.

On one occasion, there were several light taps on the door and Ms. D. opened it but saw no one there. Her three-year-old son "saw" someone enter and go past them toward the bathroom. He followed the movement with his eyes and asked his mother who the man was. Ms. D. said she didn't see anyone and the boy became frightened and ran crying into the living room.

These women moved out shortly thereafter and the upstairs apartment was, after a long period, rented to a woman and her ten-year-old son. They reported hearing breathing sounds, footsteps, and a muffled voice on several occasions, as well as finding cabinet doors in different positions than they had left them. In December 1975 this woman attempted to communicate with the "ghost" and feels that she succeeded. She has continued to live in the house and reports that the activity has decreased.

It has become a custom in field studies to make a sharp distinction between hauntings and poltergeists. Owen cites the distinction proposed by Bozzano in 1919--mediumistic place as opposed to mediumistic person. Owen himself contemplated an attempt to make such a distinction as

part of his own studies, but "abandoned it as likely to lead to an all too extensive study!" In 1923, Flammarion separated poltergeists from hauntings by the vague distinction that poltergeists are not attributable to the dead. More recently, Roll and Stevenson have reiterated the haunted person-haunted place distinction, and pointed out the additional factors of quantity and violence of physical disturbances, duration of the disturbances, and presence or absence of hallucinatory experiences (such as apparitions, footsteps, voices).

Though the majority of cases are easily categorized with these criteria, this case suggests that the differences are not so clear. Nandor Fodor investigated a poltergeist outbreak centering around a 14-year-old girl, in which the disturbances continued after the family had moved away and new dwellers replaced them. In addition, there was some evidence suggestive of haunting prior to occupancy by either family. Eisler recently reported a case in which numerous physical movements took place in a home with poltergeist-like personality patterns, but no focal person could be identified with the disturbances and one of the most common events was the unexplained sound of footsteps.

In the New Bedford case, the facts that the RSPK followed the family to their new residence and that the haunting manifestations continued to disturb the new occupants of the former home provide us with a clue that there may be an interaction between the two kinds of events. Further, because of the sequence of the activity, there is an implication that the RSPK was initiated by the haunting. This is particularly exciting because, taken together with Eisler's and Fodor's studies, it suggests that hauntings provide a fertile environment for the development of RSPK activity.

Stevenson suggests that in our investigations of poltergeist phenomena we should be alert to possible discarnate agency. We would suggest also the converse: that we be alert to possible living agents in hauntings (and other spiritualistic) phenomena. What is needed is to apply the same experimental and observational methods to all reports of this sort. Investigations of both poltergeist and haunting cases should include detailed histories of the building, medical and psychological evaluation of each of the residents, including the former ones, and an evaluation of the sociological interactions of the family group. In addition, Schmeidler's quantitative technique for the study of an apparition might be adapted for use in poltergeist cases. Only by using standard

methodology in "field studies," be they poltergeist or haunting, will we discover what the differences are between them and how they interact with one another.

POLTERGEISTS AND HAUNTINGS

William G. Roll (Psychical Research Foundation)

Poltergeist disturbances are person-oriented while hauntings seem to be place-oriented. However, the focusing effects (the same object or area is repeatedly disturbed though the main agent is not near) show that RSPK (recurrent spontaneous psychokinesis, or poltergeist disturbances) is also place-oriented at the same time it is person-oriented. There are further similarities to hauntings in that many RSPK cases involve apparitional experiences and some RSPK cases begin in reportedly haunted houses. This latter observation was probably first made by Camille Flammarion. It also appears that hauntings to an extent may be "person-oriented" in that they may be related to one or more of the living occupants of the home. Gertrude Schmeidler has told me that the "ghost" in one of her studies resembled a living member of the family.

The focusing effect in RSPK suggests a "PK charge" in objects similar to the "telepathic charge" H. H. Price advances in his theory for apparitional haunts. Such experiences, Price suggests, result if there is a "telepathic affinity" between the contents of the mind of the percepient and the "persistent and localized images" in the house which were produced by someone in the past and are endowed with the telepathic charge. The possibility that some kind of "PK charge" may build up and be released in RSPK incidents is also consistent with the wave theory for RSPK which emerged from our studies of the Miami and Olive Hill cases. The characteristics of the RSPK movements (objects physically close to the agent tend to move in short, clockwise, and outward patterns, while distant objects move further and are counter-clockwise and inward in their directions) could result if some kind of waves were radiated from two positions on the agent's body and if they were nonsynchronous. The pattern of interaction between these waves might then become a beam which moves around the agent and varies from short and wide to long and thin. Some of the other peculiarities of

RSPK motion such as the floating or fast-slow speeds and the rotating and circular motions could result if the objects were picked up, carried along by one beam, dropped, picked up by another sweep, and so on.

This theory dovetails with the more recent observation that RSPK occurrences may be related to disturbances in the agent's central nervous system of the types which may result in epilepsy.

Epilepsy as a term, like poltergeist and haunting, implies spirit intervention. The word comes from the Greek verb to seize or to be possessed (by a spirit or a god) but long ago lost its occult connotation and is now defined as the "sudden and recurrent disturbances in mental function, state of consciousness, sensory activity or movements of the body, caused by paroxysmal malfunctions of the cerebral nerve cells" (Encyclopaedia Britannica, 1974). It does not seem far-fetched to suppose that sudden and recurrent disturbances in the brain of the poltergeist agent may be associated with the sudden and recurrent disturbances in his physical environment known as recurrent spontaneous psychokinesis, or RSPK.

The proportion of diagnosed epileptics and people with symptoms suggestive of epilepsy seems to be greater among RSPK agents than among the population as a whole, but many agents do not show any signs of the disease. Perhaps we shall come to regard RSPK as symptomatic of one type of central nervous system (CNS) dysfunction which may or may not result in other problems. For instance, RSPK may be a type of petit mal similar to the myoclonic seizures where the arm muscles contract while consciousness is usually retained. In any case the CNS theory deserves the attention of RSPK researchers. D. A. Pond says that many epileptics show "irritability ... with sudden and unpredictable variations of mood: they are suspicious, quarrelsome, egocentric, circumstantial, religious, and [show] a slowness and stickiness of thought that borders onto mental subnormality."

The theme of aggression recurs in both the personalities of RSPK agents and in the nature of the events themselves. The "stickiness of thought" or perseverance in patients with brain dysfunctions refers to their tendency to have the same thought or carry out the same activity repeatedly. This trait does not come out strongly in the personality tests of RSPK agents, but it does seem to be expressed in the

incidents themselves. The focusing effects involving repetitive incidents with an object or area are characteristic of RSPK.

In an epileptic seizure, the neural discharge begins in a particular location (the epileptic focus) and then rapidly spreads to other parts of the brain. Out of phase EEG transmissions have been found emanating from different parts of the brain, indicating one or more loci of CNS discharges in addition to the place of origin. The concept of two or more nonsynchronous loci for CNS disturbances seems to supplement the moving beam theory for RSPK.

The present theory and Price's lead to a hypothesis of dual agency for haunting RSPK: an RSPK agent who created a PK charge in the house in the past and a present occupant who releases the charge thereby causing a PK occurrence. If this idea is in the right direction, both persons may be expected to exhibit the characteristics of RSPK agents. In the New Bedford case [see previous two papers], Michael showed the aggressiveness typical of RSPK agents--a trait he may have shared with the deceased Mr. C.

We have not been able to pinpoint the hypothetical RSPK waves in terms of known energy forms. The theory, however, leads to one definite prediction: RSPK disturbances should be associated with measurable EEG effects. Solfvin tried to test this prediction in the New Bedford case but the poltergeist had left by the time the EEG equipment was ready.

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PROBLEMS, CHALLENGES AND PROMISES*

Martin Johnson

What is the use of a Presidential Address? I had to ask myself that question when I realized that I was supposed to deliver such an address. Lecturing is in many respects an old-fashioned form of communication. However, I clearly realized that it is a special privilege to have the opportunity of addressing a number of noted international colleagues and knowledgeable attendants at the Parapsychological Association convention. It offers a good opportunity to propagate some ideas which one may have pondered. Under favorable conditions the address will not only constitute a one-way stream of communication, but result in a feedback of relevant criticism. I believe that feedback through criticism is the only way by which one can hope to diminish the ocean of ignorance which we always have to face as research workers. Another excuse for an address may be that a lecture offers the opportunity for challenge. In this respect I believe that a speech can be more efficient than words communicated in print.

The areas which I have selected to touch upon in my address are admittedly arbitrarily selected. Before I enter into discussion of the issues, I would like to stress that I consider the Parapsychological Association to be an organization of crucial importance for the furthering of parapsychology as a science. In short, I consider the P. A. as the natural forum for long-term planning and policy making for the benefit of the furthering of our field.

Allow me to start with the problem of communication. There are several aspects: communication among professional workers within their professional body, the Parapsychological Association; communication between the P. A. and the mass media; and communication between professional workers or their organization and laymen. I see

*This is the Presidential Address, given August 19, 1976.

it as an important task for the P. A. to try to enhance its capacity to increase the flow of information among its members and between parapsychologists and other potentially interested categories of scientists. It has been suggested that the P. A. should try to establish a kind of archive or "clearinghouse" from which each individual researcher could obtain complete information regarding what kind of research projects are being planned and carried out by members and associates of our organization. I think the idea is very worthwhile, and perhaps such an enterprise could be coordinated with the type of data bank which Dr. John Palmer has suggested for obtaining a true picture about the ratio of planned and reported studies in our field.

Another important aspect is communication with the mass media, which, of course, have a profound impact on how parapsychology will be conceptualized by the layman. In recent years we have seen a significant increase of interest in parapsychology, especially among the general public, but also, according to opinion polls, among scientists in neighboring disciplines. It is hard to pinpoint the reason or reasons for this enhanced recognition. It can hardly be because of any specific and remarkable breakthrough within our field. Admittedly our methods have become more sophisticated in recent years. Someone has said that science is a marriage between metaphysics and technology, and I believe that we at any rate are justified in speaking of an engagement between the basically metaphysical assumptions in our field and technology. The more sophisticated methods which we have introduced into parapsychology may have influenced our image among at least some of the scientists in neighboring areas of research as well as among laymen.

However, I think that there are also other reasons for enhanced interest in the reported findings from our field. I think these reasons may be described more as irrational and may have to do with the change in the image of the scientific establishment which we have seen during the 1960's and 1970's. Even within the scientific establishment itself, one has become much more uncertain as to what is "scientific" and what is not. At any rate, we have witnessed the occult wave, and the situation is by no means uncomplicated. We have experienced a rising tide of occult and superstitious beliefs which too often are mistaken for parapsychological research findings and "facts" about which there exists a consensus of opinion among the few professional workers we have in our field. One contributing factor to this state of affairs is the attitude

of some representatives of the mass media. The mass media, not infrequently on the lookout for headlines--the more absurd and sensational, the better--publicize new and seemingly new discoveries--the more unorthodox, the better. This has, I am sure, exerted a damaging influence on the already relatively poor reputation of our field among our fellow scientists in other areas of research.

I suggest that we should see it as a problem and a challenge to institutionalize our communication with the mass media. According to my own experience, representatives of the mass media are usually very reasonable people if educated correctly on a particular subject and if one provides them with solid information. The problem is how to find time and resources for the working out of scientifically solid but still condensed press releases. This I see as a great challenge for the P. A.: writing and issuing well-produced and authoritative summaries of annual research findings in our field. Since we already have our professional publication, Research in Parapsychology, perhaps we could consider the publication of an additional summary especially prepared for the mass media.

Our organization has tried to counteract unsound sensationalism in several ways, and at the same time has tried to give the intelligent layman a chance to obtain well-balanced information about our field. As perhaps the most important expression in this connection I would like to mention the Psi Search exhibition produced by Norma Bowles and Fran Hynds. This exhibition, circulated by the Smithsonian Institution Traveling Exhibition Services (SITES), was carefully developed in cooperation with representatives of the P. A. There are indications that we will soon have an opportunity of seeing this exhibition here in Europe, adjusted of course to a European public. This I see as a promising development for the establishment of more efficient communication between the interested layman and the professionals.

By and large we have good reason, I believe, to feel a certain moderate satisfaction regarding the development within our field, but the relative attention we have received must not lead us to believe that the state of affairs is necessarily a satisfactory one. I am of the opinion that the creation of more efficient channels of information and education is of paramount importance for rendering parapsychology acceptable among the better-established branches of science. Here I see a great challenge and promise for the future.

So far I have been talking about the role that the P. A. should play in improving the quality of the external as well as internal flow of information, for the benefit of our field. Now I would like to say a few words about what I think the P. A. should see as an objective when it comes to the establishment of definitions and standards, and coordination and integration of research. As I see it, we still have to confess that even among the in-group members we are lacking a clear-cut consensus as to which observations we are justified in classifying as paranormal. I believe that all within our organization should try to clear a way through the jungle. What is meant by calling an event paranormal? Does it only imply that certain odd effects have been observed which we subjectively judge as unlikely or impossible--whatever that implies--to be explained in terms of our present knowledge within the ordinary sciences? Do the observed, diversified phenomena have something more in common than just the fact that they are hard to explain? Are we justified in calling a phenomenon paranormal only in the case where it is observed under laboratory conditions and where the design of the study is such that the observations can be subjected to a statistical evaluation? This is a very basic question, but I have reason to assume that we for the time being lack a consensus regarding the appropriate answer to these questions. With the expertise we undeniably have and can establish, I believe that it would be worthwhile to try to establish a committee for the investigation of the problems related to definitions of concepts in our field. Here I anticipate the need for an interdisciplinary approach. Perhaps the work of such a committee could be instrumental in making it a bit harder to use parapsychology as the dumping-ground for all mysterious and occult ideas, no matter how absurd or poorly validated they are.

I also believe that our organization could be helpful when it comes to the question of defining the methodological standards and publication policies with which we concur as professionals. Inquiries are already on their way regarding standards for publication. I also see it as a problem and a challenge to initiate discussions and investigation on ethical standards, regarding the relationship between an investigator and his subject or subjects. I would like to see a penetrating debate devoted to that issue. A subject should be protected and in many ways compensated for his services, but on the other hand I find it unacceptable and very far from appealing, indeed rather humiliating, for a serious researcher to be restricted to working under conditions laid down by certain more or less tricky or opportunistic sensitives. I raise the

question of whether it is worthwhile to do experiments with subjects who are quite unwilling to give one a chance to carry out well-designed, process-oriented experiments, which could give one clues as to the nature of the underlying processes involved.

I feel that the establishment of a number of work groups with well-defined tasks is badly needed for the advancement of parapsychology. I am not at all in favor of trying to create a more bureaucratic organization but, instead, I am in favor of the establishment of committees or teams working on selected relevant, long-term projects. One of the important objectives and challenges for the P. A., as I see it, is to try to help coordinate and integrate the endeavors of researchers in our field. I firmly believe that it would be good strategy to try to coordinate research projects being carried out or being considered, within the different topics of our field. I am far from sure how such coordination and integration should be achieved.

As a first, modest step in the right direction one could think of the establishment of and the issuing of a project catalog in which abstracts are given focusing on topics, objectives, choice of methods and equipment, etc., for planned investigations or investigations in progress. The catalog might also provide information regarding which hardware and software will be needed. I believe that such a project catalog could be helpful in establishing mutually beneficial contacts among researchers tackling the same types of problems. I am eager to state that I firmly believe in carefully planned experiments. I also believe very much in coordinated research programs involving long-term planning. The picture of parapsychological research today, as I see it, is characterized far too much by small pieces of research usually without any organic relationship to precisely formulated ideas. As I stressed in my Presidential Message, which was issued in early June 1976, there seems to be excessive extrinsic motivation behind the small, piecemeal projects: to get the results published, to be able to have something to communicate at the next P. A. convention, or to secure a travel grant!

This is not a phenomenon which one meets exclusively among parapsychologists. There may be a number of financial, sociological, and psychological reasons behind it. The following words by Sir John Eccles about the risk of carrying out unoriginal piecemeal work without clearly stated objectives may be educational:

> Scientific publication becomes too much occupied with the reporting of experimental observations that achieve status because they have been carried out by the latest and most expensive techniques, and not because they are designed to test some particularly interesting scientific idea or postulate. As a consequence, scientific literature is overwhelmed by mere reportage of observations that are published just as observation without organic relationship to precisely formulated hypotheses.
>
> Such observations are scientifically meaningless. They are boring and soon to be forgotten. There is thus a great wastage in every aspect of scientific endeavor, in effort of the scientists themselves, in the usage of scientific equipment and technical personnel, and in the facilities for scientific publication [in Popper, 1974].

Financial support is badly needed, but not at any price. I believe it would be rather risky to try to obtain financial support by, in my opinion, unrealistically promising that the practical application of parapsychology and paraphysics is just around the corner. I suggest that we try to be patient and, as educators, try to make clear to potential donors in governmental agencies and potential private donors the paramount importance of being given the opportunity to carry out fundamental research before we can be expected to furnish knowledge from our field that has a direct application. I have to confess that I cannot see any simple solution or easy way to attract substantial money to our field. However, there may today be some vague indications that at least a few governmental agencies exist here in the West that might consider including parapsychological experts in their futurological studies.

At the same time as I emphasize the great importance of financial means for making qualified research possible, I would also like to remind you that we must not forget that an apparatus per se can never, or at least very rarely, solve a research problem. To make my point, I would like to relate a story about an astronomer on the occasion of the installation of a gigantic telescope at an American observatory. The astronomer turned to the audience and asked which part of the instrument they thought to be most important. There were many answers, all of which he rejected. "The most important part of the apparatus" he said smiling, "is the investigator, sitting behind the lens." This story

contains a moral for parapsychology. We should do the ultimate to bring intelligent, open-minded, but still critical young people into this field. Money and technical equipment are necessary conditions for bringing about a breakthrough, but material means do not constitute the sufficient conditions.

Now I would like to touch upon a few problem areas which I consider to be especially challenging for our field. First a few words related to the mind-body problem. Could a continued and intensified study of psychokinesis throw some light on the time-honored but still unsatisfactorily solved mind-body problem? Beloff, Eccles, and Popper have independently supported the view that the mind-body problem is one that has the utmost interest to science. As early as tomorrow night you will have the opportunity of listening to Sir John Eccles. In his Invited Dinner Address he will make clear his present position regarding the mind-body issue in general. As Beloff has stated, parapsychology may be considered "the ultimate battleground on which the mind-body controversy must be fought out" (Beloff, 1960).

The importance of PK should here be considered--direct action of mind upon matter would be an excellent demonstration of the strong type of dualism in which mental entities could cause physical effects. I believe in the reality of matter. I also believe in the reality of consciousness or mind. Psi research may turn out to be instrumental in finding out how consciousness and matter interact and are related to each other. By and large I agree with Beloff when he states, "while we know a great deal about matter, even about brain-matter, we know next to nothing, in a scientific sense, about mind and its potentialities." At the same time I also concur with his statement that "it is a great mistake to exaggerate the importance of mind; the Idealists who did this by denying or belittling the reality of matter were committing the gravest of philosophical fallacies," as he puts it in his presidential address before the Society for Psychical Research (Beloff, 1976). It is quite clear that a one-way, strong mentalistic interpretation is by no means unproblematic. To give a concrete example: if mind can function independently of body, why then should a slight injury to the brain result in lasting alteration in behavior?

I would also like to direct your attention to some of Dr. Helmut Schmidt's paradoxical, challenging, and most astonishing results. To summarize, he has reported the following: In experiments involving a random number gen-

erator that apparently establish a PK effect, the distance between the subject and the random number generator does not seem to make any difference; neither does the manner in which the machine is triggered. Further, it does not seem to make any difference whether one changes the random number generator halfway or more through the experiment without informing one's subjects. Finally, and perhaps still more perplexing, it does not matter whether the output sequence has already been prepared beforehand or not. How must one make sense of these baffling results? The concept of "backward causation" has recently been introduced into parapsychology and, as far as I am concerned, I find it a fascinating and most challenging idea, but of course the concept or hypothesis must offer some sort of test implications to be of any value.

When pondering and briefly mentioning Schmidt's pioneering work, I would also like to emphasize that I consider it promising for the future of our field that more and more physicists, both experimentalists and theorists, are entering our field or at least indicating their interest. Their specific and varied background is very much needed and may be instrumental in bringing about a real breakthrough in psi research. Their expertise is much needed, especially when it concerns studies of alleged metal bending, teleportation, materialization, and psychic photography. But here I would like to add that cooperation with outstanding magicians must also be considered to be beneficial for parapsychology, at least when one is concerned with more far-fetched alleged paranormal phenomena and when one is forced to work outside the laboratory. Here I find it reasonable to conjecture that physicists in general should certainly be a bit more gullible than psychologists who have a solid background in the fields of perception and witness psychology. Therefore when it comes to experimental studies or empirical studies, I firmly believe in an interdisciplinary approach.

Further, I have over the years noticed something I would like to describe as a slight misconception among people entering our field, especially from the natural sciences: they tend to underestimate the tremendous difficulties in producing psi phenomena. I am the first one to admit the great need for theories in our field and every serious attempt must be welcomed. At the same time I have to declare that I am far from convinced as to what extent concepts from quantum physics, for instance, really can explain very much involving the paranormal. This is especially the case when it comes

to developing an inclusive theory of psi phenomena. Judged from the point of view of the history of science the reductionistic approach has usually failed. This may be especially true within the realm of biological sciences. Ernst Mayr argues as follows:

> Finally, it has never been demonstrated that reductionism works, so to speak, upwards. To be sure, most of the phenomena of functional biology can be dissected into physical-chemical components, but I am not aware of a single biological discovery that was due to the procedure of putting components at the lower level of integration together to achieve novel insight at a higher level of integration. No molecular biologist has ever found it particularly helpful to work with elementary particles [in Hull, 1974].

I am by no means dogmatic on this issue, but as I have hinted, I have serious doubts about the extent to which paranormal phenomena could be explained generally by using models from quantum physics. Why should it be easier to explain paranormal phenomena with the use of a concept borrowed from quantum physics than to be able to predict and understand phenomena in "ordinary" psychological or psychophysical perception and cognition? At the very least one has to prove convincingly that on the basis of the quantum physics models in question, one can make rather accurate predictions, and then, especially within the paraphysical field. I certainly think that one of the greatest challenges in our field is to develop an inclusive theory for psi phenomena, a theory which should then be characterized in principle by having a number of potential falsifiers. More about that later on.

One of the greatest challenges to a parapsychologist interested in theoretical issues seems to me to be to find out whether pure clairvoyance really exists. For the time being it seems next to impossible to tackle this problem in such a way that the design of the study precludes all possible alternative explanations.

Another question of considerable theoretical interest which has fascinated me for quite some time is related to the sensory qualities of the targets. Several studies reported in the literature support the view that a subject can identify a target even when it is not perceptible under normal conditions (for example, information fed into a computer, or information stored on recorder tape, or videotape). The

same issue has been discussed by Dr. Beloff in his previously mentioned S. P. R. presidential address. I may have carried out one of the first studies, if not the first, aimed at tackling this problem. I am referring to a small experiment which I carried out in 1956, here in Utrecht, with Mr. Croiset as subject (Roll, 1966). Later on, and quite independently of my small experiment, Dr. Gertrude Schmeidler carried out a much more ambitious and extensive study on the same type of problem, a study in which a computer was used (Schmeidler, 1964). Since then comparatively few studies have been carried out, or at least reported, focusing upon that issue. At the moment I think I have reason to believe that I have succeeded in developing a methodology by means of which this fascinating question can be studied in a more efficient way than has previously been possible. The method seems also to offer some test-implications for the Theta Hypothesis. At the next P. A. convention I hope to be able to report actual research findings from such studies.

Yet another challenging issue, admittedly involving considerable methodological problems, is related to studies of altered states of consciousness and how such changes may affect a subject's psi performance. During the last decades we have experienced an increased willingness to examine subjective states. This openness towards these types of studies can partly be interpreted as a consequence of the spirit of the time, but depends also to a large extent upon the technological development which has made it possible to study physiological correlates of conscious processes.

For several years I have been an admirer of Charles Tart's writings. He often generates fascinating ideas. I have followed with considerable interest his own substantial contribution to the field. Of special interest are his ideas on what he calls "state-specific sciences"; that is, certain kinds of phenomena occurring in altered states of consciousness should be investigated in those states of consciousness. The explanations and theories should also be developed and tested in those states of consciousness. Here I would like to see Dr. Tart and his followers elaborate these ideas a little more in detail. There are a lot of hurdles to surmount. What measurement should be used? How should the observations be validated? Would the demonstration of a high degree of intersubjectivity of experience really be enough? A high degree of intersubjectivity is usually considered as a "must" or at least as a necessary condition for science to be objective. However, it is not a sufficient

condition, since collective delusions have been known to occur. How will an "experimenter" in an altered state of consciousness still be able to make the necessary observations without changing his state of consciousness? Perhaps a very advanced psychophysiology of tomorrow will provide the sought-after Occam's razor. So far I find Dr. Tart's ideas very challenging, to some extent promising, but still a bit lofty.

In the closing section of this address I would like to ask some most provocative and searching questions. Are we justified in describing parapsychology as a science? What are the characteristics of a scientific discipline? How can we distinguish between a scientific and a pseudoscientific hypothesis? Does the growth of scientific knowledge depend mainly on the logic of discovery or is the progression of a scientific discipline more a question of attitudes and prejudices within the scientific community? Sir Karl Popper views the logic of scientific discovery as the major instrument for the growth of scientific knowledge, whereas Thomas Kuhn insists that "whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains" (Kuhn, 1970). Thomas Kuhn's way of viewing science and its "mob psychology" offers something of interest when it comes to the problem of explaining the rather negative attitudes toward parapsychology which still prevail among scientists in neighboring fields. The fact that scientists today are somewhat more inclined than formerly to accept parapsychology as a scientific discipline depends, I think, to some extent on the fact that parapsychologists have developed more sophisticated research methods, but I also firmly believe that this more open-minded attitude has its sociological roots, as I previously hinted.

The problem of what constitutes a science is a very challenging one. Should one work as a verificationist or a falsificationist? Could one combine the two approaches? Is science only what scientists do? Should it not be what they ought to do? Furthermore, some scientific activities are not even remotely connected with the formulation of bold conjectures or with attempts to falsify or verify these conjectures. Rather, they are activities which can be described as the mapping of facts. I suggest that parapsychology offers a wide range of problems of great interest to philosophers of science, and vice versa. I am also convinced that we as parapsychologists should collaborate much more frequently with philosophers of science.

In my June Presidential Message I urged my fellow parapsychologists to familiarize themselves with Sir Karl Popper's ideal of science. The first time I read about Popper's criterion of demarcation between science and metaphysics, I experienced it as a revelation. In short, what Popper is pleading for is that every scientific conjecture, assumption, or hypothesis should have test implications and be amenable in principle to a test of refutation. For Popper, scientific change is or should be a rational process within the realm of "logic of discovery," contrary to Thomas Kuhn's view that scientific change from one "paradigm" to another is a more irrational process, certainly not always governed by the rule of reason, which falls rather within the realm of the social psychology of discovery. Another characteristic of Popper's view of science is that we should work as falsificationists; we should try to refute our own hypotheses instead of trying to verify them. We should make bold conjectures and try to refute them and in this way improve our hypotheses.

It may be educational to give a few clues to how Popper developed his methodology. As a young man living in Vienna, he became strongly influenced by three intellectual movements: Marxism, psychoanalysis, and Einstein's theory of relativity. He soon abandoned Marxism since he felt that the revolution in Russia should not have taken place in that of all countries. It should, according to Marx, have taken place in a highly industrialized country. He reacted against the fact that committed Marxists refused to specify conditions under which they would be willing to admit that their theory had been refuted. He also found the same trend among Freudians. He states:

> The Freudian analysts emphasized that their theories were constantly verified by their 'clinical observations.' As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analyzing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. 'Because of my thousand-fold experience,' he replied; whereupon I could not help saying: 'And with this case, I suppose, your experience has become thousand-and-one-fold' [Popper, 1963].

Regarding Einstein and his theory, Popper found the situation entirely different. He says:

> But what impressed me most was Einstein's own clear statement that he would regard his theory as untenable if it should fail in certain tests. Thus he wrote, for example: 'If the redshift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable' [Popper, 1974].

Popper continues:

> Here was an attitude utterly different from the dogmatic attitude of Marx, Freud, Adler, and even more so that of their followers. Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while a disagreement, as he was the first to stress, would show his theory untenable. --Thus I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could refute the theory tested, though they could never establish it [Popper, 1974].

Popper's basic idea can be said to be the rule that a scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions. Criteria of refutation have to be laid down beforehand. The most interesting but also perhaps the most controversial feature of Popper's theory of science concerns the asymmetry between verification and refutation. According to Popper's thinking, a theory can become more or less corroborated (by passing severe tests of refutation) but a theory can never become verified. On the other hand, a limited set of observations can refute a theory. One can of course state that Popper's treatment of truth and falsehood brought him close to pragmatism, but he certainly differs from pragmatists in resisting their identification of truth with verification. Can Popper's own theory of falsifiability be put to a test of refutation? To this question Popper himself answers as follows: "... my theory is not empirical, but methodological or philosophical, and it need not therefore be falsifiable" (Popper, 1974).

One should further note that Popper requires that not only do the criteria of refutation have to be laid down beforehand, but it must be agreed also which observable situations are relevant to the refutation. To illustrate Popper's view let us consider these two examples of existential statements.

(1) There is a ten-meter-long sea monster. (2) There is a ten-meter-long sea monster to be exhibited in the British Museum this afternoon in hall A at 4:00 p. m. To Popper the first existential statement is of no interest to science, whereas the second is. The assumption can be put to a critical test of refutation.

I want to give another illustration. Assume that a researcher hypothesizes that the explanation of the fact that the moon does not leave but follows the earth is not to be found in gravitational interaction but in the effects of a small, green creature, sitting in a specific crater of the moon thinking at the earth and thus exerting such a strong PK force that the moon is inseparable from the earth. This hypothesis is of course unlikely because of the tremendously good corroboration we have from many directions of Newton's law of gravitation, but still the assumption is not entirely unscientific according to Popper, since the hypothesis in principle could be put to a critical test of refutation. However, assume that observations carried out by astronauts falsified the prediction, and then the inventor and defender of the hypothesis tried to save his cherished ideas by the post hoc statement: "Well, I am sorry, but I forgot to tell you that the green creature in question is too small to be observable. It is too small to be observable even with the aid of an electron microscope." As we can see, the defender of the hypothesis has succeded in immunizing his idea. The idea cannot in principle be put to a test of refutation and consequently it cannot be of any interest to empirical science.

Popper is often misunderstood. He has been the first to emphasize that the criterion of demarcation cannot be an absolutely sharp one but will itself have degrees. "There will be well-testable theories, hardly testable theories, and non-testable theories. Those which are non-testable are of no interest to empirical scientists. They may be described as metaphysical." Popper continues: "I stressed the fact that it would be inadequate to draw the line of demarcation between science and metaphysics so as to exclude metaphysics as nonsensical from a meaningful language" (Popper, 1969). His reservations concern what is of relevance to empirical science.

Popper's epistemological position has been criticized by a number of philosophers and philosophers of science on different grounds--by Kuhn, Lakatos and Feyerabend, just to mention a few leading names. Popper's idea about the asym-

metry between verification and falsification has been especially strongly criticized. A. J. Ayer states:

> A hypothesis cannot be conclusively confuted any more than it can be conclusively verified. For when we take the occurrence of certain observations as proof that a given hypothesis is false, we presuppose the existence of certain conditions. And though, in any given case, it may be extremely improbable that this assumption is false, it is not logically impossible [Ayer, 1946].

Admittedly, there are a number of difficulties connected with the application of Popper's criterion of demarcation. 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. Just think of the scanty repeatability we possess in most of our experiments, which reflects our lack of knowledge as to what constitutes the necessary and the sufficient conditions to bring about the phenomena. Even with these misgivings in mind, valid or not valid, I certainly think it is incorrect to describe Popper as an "emperor without clothes," as a much respected senior colleague once put it. At any rate, Popper has at least provided something which I consider as a very much worthwhile ideology for fruitful research programs.

Popper's ideal of science as expressed by his criterion of demarcation between empirical science and non-science is also an ideal of intellectual honesty for research workers. I am strongly against apologetic attempts both in parapsychology and other branches of scientific inquiry. We must refrain from the regrettable habit of more or less intentionally withdrawing our hypotheses from critical tests of refutation, and we should be very restrictive regarding post-hoc interpretations of data. Too often one meets intellectuals, and here I include parapsychologists, who are more interested in being right than in learning something unexpected and new. Too often scientists defending their own cherished ideas claim that observed discrepancies between experimental results and theory or the prevailing paradigm are only apparent and will disappear with additional data and improved understanding of the paradigm. This behavior exemplifies the despicable immunizing tactic, and would apply to scientists outside parapsychology looking at parapsychological data in general, as

well as to parapsychologists looking at more particular experimental results.

Which types of bold conjectures and refutations could we make in parapsychology, following Popper's prescription for doing efficient research work? The first issue may imply the existential aspect of psi. Do the phenomena exist? A radical, naive falsificationist would probably try to arrange a crucial test of refutation as regards the existence of psi. In formal logic or in a few areas of science where the repeatability is extremely high, perhaps one single experiment would suffice, but in our case one should have to specify beforehand how extensive the series of observations should be. That implies as far as I can see the use of a statistical approach. And therefore the data bank which Dr. Palmer has proposed should be very helpful.

If we leave the existential question, what more could we do to follow Popper's ideology of research? We have in fact almost no paradigms, but only a few corroborated correlations or results indicating what seem to be "meaningful relationships" between psi performance and certain other types of variables. For example, it is usually believed that there are "good" experimenters, that is, successful ones; and "poor" experimenters, that is, unsuccessful ones. The same belief is held regarding subjects. How could such claims be put to a critical test of refutation? A radical falsificationist might suggest that one important feature of a P. A. convention should be to carry out rigorously controlled experiments in which such claims were subjected to severe tests of refutation. This could imply that a certain number of "successful" experimenters should bring a number of "successful" subjects and run a predetermined number of trials according to their respective favorite psi tests. At the same time a group of notoriously unsuccessful experimenters should run the same experiments with notoriously dull subjects. If the conjectures passed these critical tests of refutation at a statistically significant level, the assumptions could be said to have been corroborated; if not, one should sit down and decide whether the assumptions really are worthwhile.

The exemplification I have given is perhaps a bit too wild, but I would feel relieved to subject my own ideas about the Defense Mechanism Test (DMT) as a useful tool for predicting scoring direction to a critical test of refutation. The DMT, developed by Dr. Ulf Kragh, is a projective test in which a tachistoscopic technique is utilized. I have in a few

investigations reported that there exists a relationship between the DMT sign "denial" (that is, a threat is explicitly denied in the subjects' report) and below-chance ESP scoring.

How could such a conjecture be put to a test of refutation in the Popperian sense? I have just one suggestion, which in itself is far from original. According to data on which the DMT has been standardized, at least 5 percent of the subjects belonging to the standardization population manifested "denial" in their protocols. An interesting research strategy which one could follow might be to trace as many subjects as possible who have manifested denial in their protocols in studies carried out for other than parapsychological purposes. These subjects with denial in their protocols could then be subjected to ESP studies by a number of experimenters and compared on ESP performance with a control group of subjects who did not manifest denial in their protocols. During the investigation, precautions should be taken to assure that the experimenters as well as the subjects are not aware to which category the subjects belong. If the hypothesis of a correlation between denial and below-chance scoring were to be corroborated by actual findings, this tentative hypothesis could be said to have stood up to a critical test of refutation, which however is not the same thing as saying that the concept has been verified. If falsified, I would be prepared to drop the hypothesis in question, in spite of the fact that the concept happens to fit well into the theoretical framework of the DMT.

Generally speaking, I believe that Popper's approach encourages a healthy attitude in researchers, including that of careful, rigorous skepticism regarding one's own hypotheses. The Popperian approach should lead to a continuous effort to find new ways of exposing one's ideas to critical tests. So far, the matter seems to be sound and uncontroversial. I believe that Popper's criterion of demarcation makes it quite clear that a bold and sweeping statement like "psi is fundamental to mental life and reveals the true nature of Man" offers nothing of interest to empirical science, so long as the statement does not allow for a test of refutation. The same is true about statements like "psi can never be understood in terms of ordinary psychology, whereas ordinary psychology in the future will be understood in terms of psi." But here again I must quote Popper: "we must not try to draw the line too sharply" between science and metaphysics. For "most of our scientific theories originated in myths," which "may develop testable components."

However, the difficulties start when one tries to bring Popper's ideas or methodology of science--to a considerable extent influenced by concepts in formal logic--down to the concrete realities of the everyday research situation, for instance within a behavioral science. So far, except for the DMT case, I have illustrated Popper by clearcut dichotomized issues: either there is a ten-meter-long sea monster in the British Museum at a certain time or there is not; either there is a green creature on the moon or there is not. In these two exemplifications there is no uncertainty as to the status of the observations to be made. If we turn to more practical and very complex research situations, for instance in parapsychology, the picture is much more vague because of the complexity of the situation and because virtually all our data or observations are themselves very far from uncomplicated entities, whose exact status is at best statistical. I assume that under such conditions, what we can learn from Popper as well as from the prevailing statistical philosophy of the design of experiments is the importance of trying to specify, for instance, how many experiments we are going to run, and how many subjects and how many trials would suffice.

This is of course of cardinal importance in experimental research. But not all science is experimental. In our case it is at least empirical, which seems to me to imply that in most cases of research the Popperian approach should have some relevance. However, I can imagine that some people in the audience want to stress that a scientific research program or enterprise can quite easily be characterized by collection of data, a state of affairs in which no bold hypotheses are stated or put to critical tests of refutation. In this connection one usually refers to the situation which prevailed in spectroscopy for several decades. Very few conjectures were made, but a great number of facts or at least data were gathered. Here I would like to direct your attention to the fact that even the gathering of data is guided by certain decisions as to what to observe and what to expect. I wish time had allowed me to penetrate the relation between the concept of the null hypothesis in statistics and Popper's idea of the asymmetry of refutation and verification.

Here I must put an end to my address. Generally speaking, I believe that the study of the concepts of testability, refutation, and confirmation will widen the scope and the horizon of the empirical scientist. All philosophies of science certainly have their limitations. I confess that in

the end I feel an uneasiness with a unitary conception of the "scientific method." What Popper seems to me to have shed light on are aspects of what the method should not be. At any rate, I would like you seriously to consider the potentials of the Popperian critical approach for the furthering of our discipline, especially within the experimental field. Kuhn and his followers may be quite right in their way of describing what science is about in terms of what scientists do, but I am stubborn enough to believe that it is much more important for the growth of human knowledge to ask the normative question, "How should science be done?"

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THE HUMAN PERSON IN ITS TWO-WAY RELATIONSHIP TO THE BRAIN*

Sir John Eccles

I have been interested in the problem area of parapsychology a lot of my life, and I think I have something to offer to you which will arouse quite a lot of discussion. First, I have a slight word of criticism, a feeling that parapsychologists are too concerned with phenomenology, with the description and investigation of extraordinary manifestations; that they're not enough concerned with the detailed implications of their proposed explanations. For example, I assume you all take it that these phenomena are due to the brain or the mind. Parapsychological phenomena can't be due to the body; it must be the brain or mind. But are these really separable, and how far are parapsychological investigations concerned with brain, or with mind, or with the two together? Is it dualism, or do you believe in their identity, that there is an identity as in the Identity Philosophy, or what?

This morning Dr. Taylor stated that he was giving an account of physics which was acceptable to 98 percent of physicists. My neuroscience will be acceptable to 98 percent of neuroscientists. But, on the other hand, when I come to my dualist philosophy, I think 98 percent of neuroscientists would be against me. I might just carry 2 percent. I don't mind about that. The trouble about it is, you see, that with biologists in general, neuroscientists are strict believers in the religion of materialist monism in one form or another, and it's based upon nineteenth-century physics. Physicists are much more skeptical of that religion, and I am a complete heretic with regard to it. With the philosopher Karl Popper I have written a book called The Self and Its Brain in which we claim that monist materialism is dead and that it is superseded now by what we give

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as a strong dualism. We don't think that materialist monists will cease to believe in it. People have gone on believing in dead religions until the end of their days, but I just think that as a viable religion it's dead.

Well now, we'd better get on with the story. The Popperian story of the three worlds includes everything in existence and in experience. These are quite distinct worlds; there is no fuzziness between the boundaries; this is why it is so important. World One is the world of physical states, including the whole cosmos and human brains; World Two is the world of states of consciousness, including all subjective knowledge and all inner experiences; World Three is the world of culture and civilization, all made by man. Before man came to create even the simplest stone axe, there was nothing in World Three and he was only beginning to come into World Two. Apart from that, before man came and evolved, there was only World One in the cosmos. We know of no other self-conscious beings. So this is the basic story, and I think that this is a fundamental advance in philosophy that Popper has made in the last decade.

The human brain is principally a great folded sheet of about one-half meter square, and about three millimeters thick, of cortex. There are about ten thousand million neurons in the cerebral cortex. The synaptic connections which are made by them, whereby they communicate to one another, number about 10^{13}, and the potential number of connectivities of networks and patterns is infinite. Compared with the human brain a computer looks like a little child's toy. Don't underestimate the human brain. This is the most complex and wonderful piece of matter in the cosmos. And if you want to object to that, you have to use your brain to object to it!

Whenever you experience any sensation, there is activity in your brain and then you perceive it into your mind, which is World Two, this separate world of the conscious self. It is an entity with existence, a self-subsistent entity. It doesn't exist just because of the brain; it is over and above the brain. This is a strong dualism. Don't think that your sense experiences are in the brain; they are actually in the mind. All your memories and feelings and thoughts and imaginings and dreams are in the mind, though there is a brain counterpart, of course. And there is the self or ego, which has gone on through all your lifetime, which is you, your conscious being. When you go to sleep at night, it cuts

off; when you wake up in the morning, it's the same self; you go on the next day, and so on, and this is what you are. And all of this is in the mind. Now, there is an interface between the world of matter-energy (World One) and the mental world (World Two). And this is the most horrendous interface in the cosmos. We know of nothing comparable to this in any aspect of science, the interface between what goes on in the brain, the brain events, and the mind events. This is of very great interest to parapsychology because you're operating with that interface all the time, and I want you to recognize more precisely what ideologically it should be like.

A mistake naive people may easily make is to think that as soon as some signal gets into the cortex it's experienced. That is untrue. You can stimulate on the somaesthetic area of the exposed human brain with an electrode by a series of pulses at 30 to 60 per second, each pulse being half a millisecond long. The subject doesn't feel it at all until about half a second of stimulation, if it's very weak. If it's stronger he feels it earlier. Now that's an easy experiment. The next one is very hard to explain. When you stimulate, not on the brain now, but on the finger, with an electrical stimulus, just a weak pulse, it comes up to the brain very quickly, in about a sixtieth of a second, but it still takes about half a second before it can give the experience. This is because you have to build up patterns of operation in the neural machinery over the brain before it gets through to consciousness. But in fact it can be shown by clever experiments that the subject feels the finger stimulus before that.

Now that's an extraordinary statement to make. It is called antedating by Libet. And that's precognition, you see. The experience comes before the actual neural event. How can that be? I suggest that it is something that you've learned. Let me illustrate this by comparing it to a percussion instrument on which weak and strong beats are being alternated at a steady rate. If you heard these beats as you are supposed to neurologically, you wouldn't be hearing them at equal intervals. You would get a false impression of the timing. When the weak beat comes through it takes half a second; when the strong beat comes through it takes a tenth of a second. You can imagine what this would do to any sequence of taps or any other human actions. And so you've learned somehow or other to correct for that. You've antedated the weak stimuli to make them catch up with the strong ones. And so what you feel reflects reality rather than what

is given to you by the total brain performance. You're actually feeling the physical timing correctly. And this is antedating, and if you like, precognition.

Now, let's discuss vision. The things that are with us all the time are the things you don't notice; the most wonderful things in life are the things you take for granted, like existence. You get your image on the retina; it goes through to the brain and you're looking at a field. I'm looking at this room now with the candles and the lights and the people and all the rest of it, and it's all to me a complete picture, every face I look at, every detail I look at, I can see at will. How do I do that? We have no idea. It's completely unknown. All the most sophisticated work on the retina and the pathways tells us almost nothing on this problem. You get this picture on the retina. But it never appears in the brain. All I get in the brain as I look in the visual cortex would be cells firing. Then you get more and more sophisticated processing, and all you get out of all this is just cells responding specifically to lines or angles or at the best the simplest geometrical shapes; a nerve cell might fire for a triangle and not a square, or something like that. That's the best we can do. Nowhere do we find the picture again. Nowhere do we find nerve cells which reconstitute for us this picture that is projected onto the retina. And yet the picture is there, in our minds.

My theory is that the picture is not in the brain, it's in the mind. You are making a mistake to look in the brain for a picture. If you want an ironic comment on this, it's what are called the "grandmother cells." There's a cell somewhere in your visual area that goes "ping" when your grandmother is in front of you, and you say "Ah, grandmother's there." And it doesn't matter whether grandmother is far or near or in profile or full face or what, the same cell pings for grandmother and for nothing else, and that tells you it's grandmother. Well, you know, this is all irony, but it shows you that, when we make these jokes, we never expect to get the answer, and we never will, out of the brain. The brain carries the coded information, but it doesn't carry the picture. The picture is made out by the mind scanning over the brain, and doing a most incredible synthetic job in lifting a picture out of all of this diversity of neuronal firings, hundreds of millions of cells firing in patterns. The scanning that we do to make the perceived pictures takes place from moment to moment all through our lives; you are doing it right now and you don't know you are doing it. This is the wonder, the mystery of vision.

Furthermore, you may think you can see color, but there's no color in the world, only wavelengths. There's no red. The color is created, not in my brain, but in my mind. There's coding for this, of course, the wavelengths and all the rest of the three-color process, but there's no color until the message gets through to the mind, only just wavelengths and firings of cells in the brain. The color is made in the mind, and the same is true for every other observation of the simplest kind, and even more for pattern, and more for beauty, and more for music. All of this is in the mind, not in the brain. The brain is a marvelous instrument which is useful and necessary to hand stimuli on to us in coded form so that we can make it with our minds into our conscious experienced world. We put together, then, all the splendor and magic of our visual experience in a way we don't understand, but it's lifted out of the brain and it's in the mind.

The speech area is usually on the left side of the brain, the areas of Broca and Wernicke. Ninety-five percent of people have their speech areas on the left side, and the right side has complementary functions like music and pattern. You've probably heard of the split-brain experiments of Sperry, where he cut the corpus collosum. The splitting was done to help control incessant epilepsy uncontrolled by drugs, and it worked pretty well. You cut the big linkage between the two hemispheres, each with its five thousand million cells. The linkage consists of two hundred million strands of fibers joining the two sides. When you cut this, everything that goes on in the right hemisphere is unknown to the subject. The subject cannot use this hemisphere or receive from it; he only receives conscious experiences from the left hemisphere. The right hemisphere is a very clever brain; it will do all kinds of wonderful things, but it never gives the subjects any conscious experiences, nor can they control it. They will say that they cannot control their left hand, for instance; it does what it likes, and they really just reject it; they only work with the right hand. In the same way everything in the left visual field is unknown to these subjects. And yet the left visual field projected into the right hemisphere can get actions from this right hemisphere, say by the left hand, which are very clever actions. But the important point I want to make is that the self-consciousness of the subject is restricted to the left hemisphere; the other hemisphere is lost to him for any purpose of conscious experience or action. And so the brain-mind problem is really acutely shown by this. There is a great part of

the brain which the mind can't work on at all, or receive experiences from.

Now I want to deal with psychokinesis, a real psychokinesis which can be very well controlled in an amazing way. The psychokinesis I'm going to talk about is when you will a movement and you actually trigger the brain to carry out the movement; using thought to bring about action. And I think one can prove that this is true by an empirical experiment, not some philosophical argument, but real empirical science. This was done by Kornhuber, a professor of neurology in Ulm. He has done what I regard as a work of genius. Instead of just talking about free will and all the decisions you can make and so on, which are always so complicated, he has made the simplest possible experiment. His subjects lie on a table, relaxing, and they make a very simple movement, for example a flexing of the finger. And then they wait until all memory of that has passed (they are very highly skilled, being themselves scientific investigators); and then they do it again, and so on. It's a very boring experiment, because you have to go on for a long period and they don't do it regularly, they do it quite irregularly. They've just got to lie there; when every memory is gone they, out of the blue, as it were, do it again, with no triggering signal. They are very very carefully controlled with elimination of every extraneous stimulus.

When you're bringing about any movement you have to start off from motor pyramidal cells in the motor cortex that fire down the pyramidal tract to the motor neurons, which cause the movement. That we know. That's ordinary straight physiology. I think most people naively think that when they want to move their finger what they do is just "signal" to the correct pyramidal cells and say "fire" and the finger moves, or something like that. Ask a neurologist, or a neuroscientist, how it comes about that these particular pyramidal cells and not the other ones are firing when you want to move your finger. They can't give you an answer. But this is what we're doing all the time; we are at will able to move a finger or a shoulder or do the most skillful things, and we've learned this from babyhood.

The subject has electrodes leading from the brain at four different sites on the surface of the scalp. You want to find out if something's happened in the brain in relationship to the finger movement. You can record electrically from the muscles that move the finger, and the onset of this electro-

myogram can trigger a whole computer operation, so that the computer records the EEG and the muscle action potential every time you will to move, and in addition stores two seconds before that, and a second afterwards. Now the subject does the finger movement 250 times, to lift the signal out of the noise by averaging. It takes an hour or more to do one curve. About 800 milliseconds before the movement the brain starts to go negative and gets more and more negative. This is called the "readiness potential." This is due to the firing of nerve cells that are widely spread over the surface of the brain. So when you're going to move your finger, you don't directly get at the motor pyramidal cells that are going to do the movement. Instead you have an incredible operation bringing in hundreds of millions of neurons that are slowly building up patterned firing above their base rate. They are all the time firing anyway, so you are just slightly modulating them. Indeed, this action of the will is terribly weak. It is surprising that it takes 0.8 of a second. But this is for a movement that isn't triggered by any signal. This is a movement which is initiated by the will. What happens is there's a big buildup of neurons firing, and eventually it concentrates on the specific pyramidal cells that are involved in making the finger move. Gradually the firing is channeled into or controlled into the finger area of the motor cortex.

Now, you might wonder, how can you do that? You've learned it from babyhood. A baby can't do that; it's wiggling all the time and moving around and gradually learning to control its brain. This is what babies are learning mighty fast in their first few weeks of life. They don't know it, but that's what life is! You are all the time learning to use this wonderful machine, this wonderful instrument, the brain; and now that we've learned it, we don't even know we've learned it. We just take it for granted.

Kornhuber also did an experiment in which subjects repeatedly said the word "LOTTE," chosen because it's got a very sharp beginning. This task takes longer than lifting your finger. Speech involves a much more complicated, highly organized movement, and it takes one and a half seconds now of readiness potential before it comes out. It is most surprising that just to say a word takes one and a half seconds to get the motor pyramidal cells to fire, to make the muscles move to produce the sound. Anyway, this is all empirical science. And this, I maintain, is psychokinesis. Under these conditions the mind has been able to work upon

the brain cells, just slightly changing them. If you looked at a single cell you wouldn't see anything significant; if you looked at a whole ensemble you wouldn't. You would have to average the whole performance, as in the scalp recording. The mind is making these very slight and subtle changes for hundreds of millions of cells, gradually bringing it through and channeling it into the correct target cells to make the movement. And so there is psychokinesis, mind acting upon a material object, namely brain cells. It's extremely weak, but it's effective, because we've learned to use it.

The neurons of the cerebral cortex are arranged both structurally and functionally into columns perpendicular to the surface and about 0.2 square millimeters in cross-section and three millimeters in depth. At the top of this column or module of cells is laminae one and two, an area of fine connectivity. This is where, I think, the mind is working upon the brain. Below it, in laminae three to five, are the regions where you have the strong synaptic actions. I have a theory that laminae one and two are also where memory is, in the changes at this level. So this is the subtle level, and deeper is the main control level. I conjecture that this is the level where the mind is working on the brain, slightly changing the operations at that level in the brain by what I call, poetically, "cognitive caresses." It isn't, of course, a mechanical action, but it's symbolical of that.

You have about a million columns or modules in your brain, and each of these has about 10,000 cells in it. Each column has its own little inner life. At this surface layer of laminae one and two, I conjecture, is the real frontier between the brain and the mind. A lot of imagination is required to pioneer new levels of thought where no levels of thought have existed in the past; it's a kind of exploration. And yet we have to have ideas on this. Action is going on, and experiences of all kinds exist in the mind, and not in the brain. There's no taste or smell in the brain, but only in the mind. Where is pain, but in the mind? It has pathways, of course, but there's no pain in the physical world, nothing commensurate with it. And yet it's an experience which we all know.

And then there are the inner thoughts, feelings, memories, and intents, such as the intention to move your finger. There are your memories and dreams and imaginings. They're in the mind in the first place. You might think that they're in the brain and then they come to the mind, but I do believe

that the mind has its own existence, its own independence, apart from the brain. It works intimately with the brain, but it is superior to the brain. It wasn't something from the brain that gave me the feeling I had the intention to move my finger; it came from the mind. And this is, I think, essential to any understanding of human action and responsibility.

Well, that gets us into a philosophical story, but I want you to realize that this is the greatest problem in science, this frontier between the mind and the brain. Nowhere else in science is there such an incredible frontier which has to be recognized, and accepted, and developed. This is the biggest challenge. But, you might think, what about the first law of thermodynamics? The physicists don't worry about this. Only biologists worry about the first law of thermodynamics, it seems. Physicists say, well, this is at the level that no one can ever look at in physics, and therefore we just don't know what goes on. And there could be a little gain here, and a little loss there; it could all balance out; we don't know. It's beyond anything that physicists have even thought about and tested before.

The modules of brain cells are not all open to mind influences all the time. They are opening and closing, depending on the actions of the cells firing. If the modular activity sinks, you can become unconscious, as in sleep. When you dream, a few of the modules will light up, and you will get the dream. The mind is wandering over the brain and when any modules light up it's happy to receive from them and give you whatever they're telling it and make a dream of it for you. And so this liaison brain, big as it is, is not all open to the mind; it is only partly open some of the time, and the mind is scanning it. There are perhaps two to three hundred thousand of these modules available in the liaison brain, and I should imagine that the mind scans over the whole in an amazingly fast way, perhaps like a multiple searchlight, looking at all the modules. But not only is the mind reading out from them, but it is making an integrated picture. When I see this room here I have thousands and thousands of modules, perhaps fifty to a hundred thousand, firing in some patterned array, and my mind is scanning them, and immediately makes the picture that I see in experience, my visual experience. That just gives you some idea of the problems of consciousness.

Furthermore, it isn't as if each module were by itself; each one goes to a hundred others and receives from a hundred

others, to form an immense weaving pattern of modules. It is conjectured that there is a complexity of pattern and time which we know nothing much about, which we can only visually imagine. At any given time there are open modules, closed modules, and half-open modules; some are active, and some are not active; some can receive or give to the mind, and some can't; and some are half and half. They are firing in different manners, and the mind is scanning the whole area instantaneously, and building up from it. The experienced unity comes, then, not from anything going on in the brain, but from the mind, scanning and assembling, synthesizing from moment to moment. And that is how we get the unity of our experience.

And how does it know how to do this? We experience selectively according to our intentions, and what we're interested in. I can be looking just at three candles and nothing else, and all the rest of the things in the room disappear. We do this moment by moment all the time, you see; the total ensemble of things is not what we're concerned with. We can concentrate on one little piece or another, and make that into our experience for the moment. We don't know how that happens. This is all, again, for the future. But this hypothesis of mine is one which has tremendous explanatory power, and all the other theories of the relationship of the brain to mind or free will or voluntary action have large areas of experience which they don't account for at all. But I think I can in principle account for them all on this basis. This is a big story, and I think you can see it has many implications for parapsychology. This is a tremendous change from the old materialist monism where at the best they would say that there are things going on in the brain which give you some mental action, but they would deny that the mind is an independent self-subsistent, existent entity.

When you go to sleep, there's nothing much to read out from the modules; the mind I think is scanning over the liaison area and finding nothing. When you're in a coma it's the same, or when you're under an anesthetic. You just cut down all the operations in the modules. The mind is still there, the self is there, but with nothing to read. And what happens in death? Well, that is of course the ultimate question. Does this existent self go into some other existence after the brain that it has been so intimately associated with in this long life, cognitively caressing it and receiving from it, is gone? I don't know. I would say that we mustn't be dogmatic about that on the basis of our own old beliefs in

materialist monism, namely that there is nothing in existence but material things. I believe that we have to be openminded. We don't know how we came to exist; each of us is a conscious being with our own selfhood, our own experiences, our life that we know intimately. This wonderful, beautiful, delightful experience, this wonder and mystery of life, this adventure of life, was all created we don't know how. I believe my body, of course, is evolutionarily, genetically coded, and also my brain. All of this is built in the ordinary official biological manner. But what about the conscious self, to which I have now, you see, given an independent existence? Where did it come from? It's associated with my brain in a mysterious way, in a way that we cannot understand, but we just have to accept it and not be dogmatic about the future.

We have to realize that each human being in a way has something infinitely precious, infinitely mysterious. Each human being is built really by World Three. If you cut off human beings from World Three, they don't have a World Two to any recognizable extent. This statement of mine is based upon the case of Genie. Genie was a girl discovered at the age of 13 1/2 years in Los Angeles penned up in an upstairs attic by her parents, who were psychotic. She had never been spoken to and never had had any verbal experiences, or very many nonverbal experiences either. She was just serviced in a primitive manner. At 13 1/2 years she was discovered functioning at what they judged was about a 6-month-old child level; she had no language, of course, but since then she has been mothered by foster parents and given gentle loving care for at least four years now. She has learned to speak up to a point. She has a very limited language. She speaks with the wrong hemisphere, and can put strings of only five words together. And she can use verbs and write. She's even got a sense of humor, and she has memory. But she has almost no memory of her first 13 1/2 years. Then she was not in World Two and not in World Three; she was only in World One. Only when she got language and therefore got World Three did she also get World Two and become a conscious being. Each of us is created by our cultural environment. And yet our spiritual self is independent of the brain.

I don't dare to try to solve all this for you. I've given you a very brief outline, this story that Popper and I have been developing. It is a stronger dualism than before, because we would have to insist in the first place that the mind is over and above the brain and all our ideas are in

the mind before they're in the brain. They only come into the brain when we make them into language, and thoughts, and intentions; but creative imagination is in the mind long before it gets expressed in an equation or in a sentence or something like that. That is, we are essentially primarily living in the mind; that is the great mystery, and we are so blind not to realize it. Now for parapsychology I have a message. I think that you have to concentrate much more on the role of the mind in parapsychology than the role of the brain.

For a fuller and much wider account of the brain-mind problem I refer you to the book, K. R. Popper and J. C. Eccles, The Self and Its Brain, which will be published by Springer Verlag late in 1977. It will contain all the illustrative material and all the references referred to in this published lecture.

BRIEF 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-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-BODY EXPERIENCE (OBE) 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