RESEARCH IN PARAPSYCHOLOGY 1974

Abstracts and Papers from the
Seventeenth Annual Convention of the
Parapsychological Association, 1974

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Editors

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PREFACE

1974 was a year of inspiring success for parapsychology (although marred by an event discussed below). It is a hallmark of a developed science that investigators in different centers attempt to develop systematic lines of research, replicating their own and each others' observations and extending them according to a conceptual framework. Parapsychology, through lack of funds and manpower, has traditionally been deficient in this endeavor. The single most outstanding characteristic of the papers presented at the 1974 Parapsychological Association convention is their attempt to remedy this deficiency, especially in the area of internal states and psi performance.

Following the lead given by Honorton and Harper at the 1973 convention (see pp. 52-3 in Research in Parapsychology 1973), investigators this year reported seven studies attempting to reduce sensory stimulation and facilitate psi reception through use of a "Ganzfeld" technique; enough of these showed strong results to pave the way for the beginning of an empirically-based model of psi information processing. Additional support for such an approach is given by studies relating ESP and PK scoring to mental imagery, relaxation, and other internal state variables. Along similar lines, four papers explored the out-of-body experience in relation to psi performance, and several others focused on personality, perceptual, and cognitive correlates. Gary Schwartz in his Invited Address discussed ways in which a biofeedback approach could facilitate the integration of psi concepts and methods with known psychobiological processes.

Four papers on animal psi research demonstrated the viability of the anpsi approach. Several papers indicated a continuing interest in the use of automatic testing devices in ESP and PK research; other reports described further research in the area of PK on static objects and energetic recording devices. Energetic hypotheses of psi are receiving increased public attention, and physical scientists are investigating popular claims about the possible relevance for psi of such topics as auras and Kirlian photography, as well as try-

ing to develop better physical models for explaining PK effects. Two papers and a symposium dealt with these efforts.

Further contributions to theory-building included a paper describing a mathematical model of psi and the Presidential Address, in which Robert Morris proposed a set of terms and concepts which could enable us to design better experiments to distinguish between alternate routes of psi communication and alternate theories about sources of psi information.

The promising indications in this volume should not lead the reader to expect that psi can now be produced on demand. That it cannot is clear from the research reports and is underlined by a tragic event which caused several papers to be withdrawn from the convention program: a respected parapsychologist was discovered by his colleagues to be falsifying results (see Part 2, "Comments on the Levy Affair"). A promising career in parapsychology was terminated and previous work by the researcher has come into question.

This affair, sad as it is, will do some good if it helps to reduce our preoccupation with significant results and make us take more interest in work which did not come out as expected. If only the "successful" work is rewarded by publication and in other ways, we invite incidents such as this. If we ignore experiments with insignificant results, the strength of the reported work is reduced by an unknown amount and the possibility of discovering why psi was absent in the unreported work is lost.

Repeatability on demand is not always a criterion of scientific understanding. Sometimes understanding results in the ability to control; sometimes it results in the recognition that the event cannot yet be controlled, and that we do not yet know all of the factors involved. It should be noted in this context that Program Committees for P. A. conventions do not make repeatability or statistical significance a standard for acceptance of research reports.

The recent upsurge of popular interest in psychical research also necessitates a reassessment of parapsychologists' relationships with each other, with the media, and with the general public regarded both as consumers of psi-related information and as possible subjects in psi experiments. The P. A. attempted to address this need in a symposium entitled "Ethical Issues Confronting Parapsychologists," which gener-

ated a great deal of spirited discussion. The Educational Symposium which preceded the convention represented a first official step by the P. A. to try to bridge the information gap between parapsychologists and the public. It is hoped that more will follow.

The Editors

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THE SEVENTEENTH ANNUAL CONVENTION

The Seventeenth Annual Convention of the Parapsychological Association was held at St. John's University in Jamaica, New York, on August 22-24, 1974. On August 21 a day-long Educational Symposium was given for the general public. It included introductory surveys of various aspects of parapsychology by Robert L. Van de Castle, John Palmer, Rex G. Stanford, Gertrude R. Schmeidler, and Charles Honorton, and a panel discussion on unsubstantiated claims in psi-related areas. These papers are not included in the present volume.

A total of 222 people attended the convention. Of these, 66 were Association members. Edward F. Kelly was chairman of the Program Committee, which also included John Palmer and Helmut Schmidt. The Research Briefs chairman was J. G. Pratt. Rex G. Stanford and Laura F. Knipe served on the Arrangements Committee.

Gary E. Schwartz of Harvard University was the Invited Dinner Speaker. Robert L. Morris of the University of California at Santa Barbara delivered the Presidential Address. These two addresses are given in full as parts 13 and 14 at the end of the volume. Part 3 contains 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.

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COMMENTS ON THE LEVY AFFAIR

James Davis
(Institute for Parapsychology, FRNM, Durham, N. C.)

A large number of you are aware of the incidents that occurred at the Institute for Parapsychology in May and June, 1974, but in talking to some people it has become apparent that this knowledge has not been disseminated as far as I had previously thought. The crux of the matter involves events that occurred between approximately May 10 and June 12. During that time it became apparent to Jerry Levin, James Kennedy and myself that there were strong indications that the Director of the Institute, Dr. Walter J. Levy, Jr., might be engaged in some sort of covert activity involving manipulation of data in one of his studies.

The particular study involved was a rat implantation experiment in which electrodes were placed in the so-called pleasure centers of the brains of rats. The animals were first trained to stimulate their pleasure centers by pressing a foot pedal. Then the pedal was removed and a Schmidt random number generator was connected to the electrodes, such that the animal would randomly either receive or not receive the stimulation, depending upon the outcome of the generator's random decision. If the animal could exert a psychokinetic influence on the generator, it would receive pleasurable stimulation at an extrachance level. This line of research began during the summer of 1972 and was first formally reported by Dr. Levy at the 1973 Parapsychological Association convention (pp. 78-81 in Research in Parapsychology 1973).

James Kennedy, who was coexperimenter and the primary person responsible for the experiment at that time, first became suspicious in early June. Jerry Levin and I had been concerned about Dr. Levy in regard to another study involving preliminary work with stimulation levels in a series of small rodent precognition experiments, but we were really unable to make any decision because of lack of evidence.

But when it became apparent that something was possibly happening in the implanted rat study, we took steps to set up independent recording of hits in this latter study, unknown to Dr. Levy. From this record we determined that there did seem to be a major discrepancy between the official punch-tape recording of hits versus misses from the experiment, and our second source, an Esterline Angus event recorder.

After we had obtained this evidence, I took up a position hidden from Dr. Levy where I could actually observe the course of the experiment, and during that period I witnessed Dr. Levy going in and manipulating the apparatus in such a way as to produce the effects that we saw on the independent record. The following day, we spoke first with Louisa Rhine and then with J. B. Rhine, and that afternoon J. B. Rhine confronted Dr. Levy with the evidence, at which point he admitted that he had manipulated the data in the manner so described, and promptly submitted his resignation.

Dr. Levy insists that manipulation took place only in the rat study, and only during this period of approximately one month, from May 10 to June 12. However, it is of course necessary for all of Dr. Levy's work to be held suspect, especially those studies that have not actually been replicated, and for those that have been replicated to be considered on the strength of those replications alone. The same caution must be used in assessing papers given by Dr. Levy at previous Parapsychological Association conventions. Some studies, such as certain procedures in the small rodent precognition work and a maze study using human subjects, have been done successfully by other investigators. Others, such as the rat implantation and egg PK work, have not. We at the Institute are ourselves engaged in replicating these major lines of research, and also at the same time continuing the lines of research that were under development. The results of our replication attempts will be publicly reported. Beyond that, more details are available in the Comments section of the June 1974 Journal of Parapsychology.

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RESEARCH BRIEFS*

A PILOT STUDY OF ESP IN HYPERKINETIC CHILDREN

Gerald G. Jampolsky† and Maryellen J. Haight (CHILD Center Annex, Tiburon, Calif.)

One of us, G.G.J., who is a child psychiatrist, has observed that hyperkinetic children seem to have histories suggesting psi ability. One cause of hyperactive behavior in children could be a confusion and difficulty in integrating subliminal psi stimuli. In an attempt to obtain an objective assessment of these observations, a pilot study was conducted by the staff of the CHILD Center Annex.

The original hypothesis tested was that hyperkinetic children have more ESP than normal children. A second hypothesis was that percipients show learning, or training, of ESP on an ESP teaching instrument. The study was done between February and June, 1974. Two groups of children served as subjects: ten hyperkinetic males, identified by physicians, and ten normal males and females. The age range for both groups was 9 to 13 years, and they were all residents of Marin County, California. A questionnaire referring to psychological variables was developed, asking questions involving belief in ESP and past paranormal happenings, and was given at the initial office visit. A second form was checked at the end of each experimental session, with questions referring to the subject's feeling state and his decision-making process with regard to pressing the buttons on the ESP teaching instrument. The intuitive feelings of the test monitor concerning the subject's ESP score on each test day were also noted. The results of these questionnaires are not reported here.

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*Chairman: J. G. Pratt, University of Virginia, Charlottesville.

†Presented by Jampolsky; dagger will indicate speaker.

A test for clairvoyance, consisting of 50 sealed opaque envelopes containing 25 black and 25 white slips of paper, was administered to each subject prior to his work on the ESP instrument, and again six weeks later at the conclusion of the test series. The subject was allowed to feel each envelope, then indicate his choice (black or white) on a numbered sheet of paper. The test monitors did not know the key for the contents of the envelopes. The ESP instrument was a four-choice machine developed by Russell Targ and David Hurt. It is described in a paper by Targ and Cole [see pages 27-9]. Each child performed 192 runs of 25 trials each for a total of 4800 trials per child in eight test sessions. Chance expectancy was 1200 hits.

The data from the instrument were analyzed by computer, courtesy of Stanford Research Institute and Langley Porter Neuropsychiatric Institute, San Francisco. With regard to the first hypothesis, the results were mixed. Employing the Mann-Whitney U test for nonparametric variables, there was a significant difference in performance on the clairvoyance color test between the two groups. The hyperkinetic group showed a wider range of difference between their pre- and post-test scores, but no difference in overall level of scoring.

Mean scores on the ESP instrument were compared using a two-tailed t test; there was no significant difference between the mean scores of the two groups, and the mean score of the hyperkinetic group was not significantly different from chance. Analysis of variance and homogeneity of variance tests revealed no significant differences between the performance of the two groups on the ESP instrument. Of interest was the observation that nine of the ten hyperkinetic subjects had average scores above mean chance expectation. The probability of this occurring by chance is significant. The mean scores of the two groups on the ESP instrument were compared to their mean scores on the color test, using the Spearman rank correlation coefficient; the correlation between the instrument and the color test was significantly positive in the hyperkinetic group. This suggests that the two tests were measuring the same thing. The normal group did not show a correlation between scores on the two modes of testing.

The second hypothesis, that learning occurred in either group on the ESP instrument, was rejected. To show learning, we would expect an increase in the scoring rate, as

evidenced by a positive slope significant at the .05 level. The combined positive slopes of each of the groups were not significant; it is of interest, however, that the only significant positive slope exhibited by a subject belonged to a member of the hyperkinetic group. The hyperkinetic group tended to be erratic in their scoring, whereas the normal group showed evidence of settling down in a steady pattern of scoring; at 4800 trials for each subject, the curves for both groups appeared to level out at just above mean chance expectation.

In conclusion, we recommend more research and attention to the area of ESP in hyperkinetic children.

ESP AND TWO MEASURES OF INTROVERSION

Dennis Szczygielski and Gertrude Schmeidler† (City College, City University of New York)

In 1967, when Eysenck reviewed studies of extraversion and introversion in relation to ESP, he reported that extraverts tend to have higher ESP scores. Kanthamani and Rao have recently obtained confirmatory results, and in general the thesis seems to be supported (though findings are sometimes weak and occasionally inconsistent). Since most of this research tested extraversion and introversion only by questionnaires, and since such inventories are seldom fully valid, it seemed worthwhile to explore the relation further, by using both a standard questionnaire and a behavioral measure. The latter was a test of time perception which in prior research showed that introverts responded with significantly longer time reproductions than extraverts.

The subjects were 20 friends or acquaintances of D.S., who tested them individually. Only 16 completed all measures. The ESP task consisted of four runs with ESP symbols. Target order was determined from a table of random digits, entered randomly. Each target sheet held two runs, was wrapped in aluminum foil, placed in a manila envelope, and stapled closed with a response sheet on top. Forty of these assemblies were prepared, two for each subject.

The questionnaire we used was Welsh's Social Intro-

version (Si) scale from the MMPI. The time perception test was modified slightly from its original form as developed by Lynn. It consisted of ten "chained" trials. On the first trial D.S. started a stopwatch, let it run for 15 seconds with its face away from the subject, and stopped it. Starting and stopping gave clearly audible clicks. He then gave the watch to the subject, who had been instructed to make it run for the same interval without seeing its face or using other artificial aids such as counting. The subject's time was recorded, and was used as the next standard for a time reproduction; the next response was the standard for the third interval, and so on, for a total of ten trials. The score we used was the time on the tenth trial.

The subject first did two ESP runs, then took the time perception test and the Si scale. (Most subjects found these two tasks annoyingly dull.) Two more ESP runs followed. The results showed a strong decline in ESP score after the tedious interpolated tasks. The mean number of hits on the first two runs was 5.6, the mean on the last two was 4.5, and the difference was significant ($t = 3.17$, $df = 18$; $P < .01$). It was interesting to find a moderately high positive correlation between scores on the first two and the last two runs ($r = +.445$; $t = 2.05$, $df = 17$). This approaches but does not reach significance, and suggests some reliability in the ESP scores during the experimental situation.

As expected, there was a positive correlation between time scores and Si scores. It was suggestive but not significant ($r = +.458$; $t = 1.93$, $df = 15$). Each measure correlated with ESP in the direction of higher ESP scores for introverts (for Si, $r = +.362$; $t = 1.45$, $df = 15$; and for time, $r = +.433$; $t = 1.80$, $df = 15$). Neither was significant, but the time correlation was suggestive. Times were suggestively longer for subjects who scored above mean chance expectation than for those who scored at or below it ($t = 1.94$, $df = 14$). The data thus show consistent but not significant tendencies for the introverts to give higher ESP scores than the extraverts.

The fact that subjects knew the experimenter socially and were at ease with him and, in general, kindly disposed toward him, might be responsible for this unexpected tendency that runs counter to most previous findings. For introverts, a relaxed social setting with positive rapport between experimenter and subject may cancel or reverse their

usual restraint, so that they behave more freely. It is also possible that extraverts tested by a stranger or under stressful conditions will "rise to the occasion" and show good performance, but will be more than optimally relaxed (and perhaps too readily distracted) when tested in a friendly atmosphere by a friend. Further research is needed to check out these post hoc interpretations.

THE PSI CONDUCIVE SYNDROME: FREE RESPONSE GESP PERFORMANCE FOLLOWING EVOCATION OF "LEFT-HEMISPHERIC" VS. "RIGHT-HEMISPHERIC" FUNCTIONING

William G. Braud† (University of Houston) and Lendell W. Braud (Texas Southern University)

Findings from a number of different areas of study (including altered states of consciousness research, cognitive psychology, studies of man's two cerebral hemispheres, Eastern and esoteric philosophies, the mystical tradition, and parapsychology) suggest the existence of two "states," one psi-conducive and the other psi-antagonistic. Of immediate concern is the Psi Conducive Syndrome, which seems to involve seven major characteristics: muscular relaxation, decreased arousal, reduced sensory distraction and increased concentration, a more inward focusing of attention, decreased action mode--left hemisphere functioning, along with increased receptive mode--right hemisphere functioning, an altered world view, and momentary importance of psi.

The first phase of our work involved the induction of the syndrome through the use of a progressive muscular relaxation procedure. As predicted, psi was enhanced in our relaxed subjects; further, degree of psi performance was positively and significantly correlated with degree of muscular and mental relaxation. Since certain characteristics of the Psi Conducive Syndrome and the Psi Antagonistic Syndrome agree so closely with what is currently being discovered about the characteristics of the functioning of man's right and left hemispheres, respectively, the present experiment was designed to explore this possible connection.

Our hypothesis was that facilitating "right-hemispheric"

functioning in our subjects would be accompanied by a shift toward the Psi Conducive Syndrome and, consequently, the augmentation of psi. Conversely, facilitation of "left-hemispheric" functioning should be accompanied by a shift toward the Psi Antagonistic Syndrome, with a consequent inhibition of psi. Findings from lesion and stimulation studies, split-brain research, dichotic listening experiments, and lateral eye movement research together suggest that the two hemispheres may process different kinds of information and that the "operating characteristics" of the hemispheres may differ. Clusters of "cognitive" or "information processing" abilities have been associated, respectively, with the two hemispheres. Thus, the "dominant" hemisphere (the left, in right-handed subjects) appears to excel in linguistic, mathematical, logical, temporal, abstract, and analytical skills. The "minor" hemisphere (the right, in right-handed subjects) performs poorly on the above, but appears to excel in tasks involving music, facial recognition, imagery, spatial performance, and holistic judgments. Would the presentation of an "analytical" cluster of tasks engage or exercise the left hemisphere, and would the resultant shift toward the Psi Antagonistic Syndrome interfere with free response GESP? Would the presentation of a "nonanalytical" task cluster engage and exercise the right hemisphere, provoke a shift toward the Psi Conducive Syndrome, and improve GESP?

Twenty subjects were tested individually in this experiment. We attempted to evoke left-hemisphere functioning in a group of ten right-handed male college students by having them listen to and perform mental operations upon tape-recorded "analytical" material presented via stereo headphones. This 23-minute "left-hemisphere" tape included: counting the letters in words, mathematical problems, mentally diagramming sentences, solving various problems involving logic and reasoning, analogies, time-estimation, and excerpts of readings on linguistic philosophy, constitutional law, physics (mechanics and nucleonics), vocabulary, and blank verse. It was thought that listening to such material would induce an active, analytical, interpretive set which would persist during a five-minute psi impression period which immediately followed and would interfere with effective psi functioning.

Another group of ten right-handed males listened to a 23-minute "right-hemisphere" tape including: music, natural environmental sounds, and electronically synthesized sounds suggesting depth and imagery. The environmental sounds

were excerpted from disc recordings of ocean, wind, rain, thunder, crackling fire, and animal sounds (birds, crickets, meadow and forest animals). The interspersed musical selections were chosen so as to be relatively unfamiliar to the subjects and included excerpts from the symphonic works of Gustav Mahler; slow Middle East instrumental music; slow, flowing instrumentals of the musical group Pink Floyd; electronic music by Walter Carlos; and a "contemplative" sound of synthesized low frequency bells and gongs. It was thought that listening to such material would induce a receptive, nonanalytical, noninterpretive set which would persist during the immediately following five-minute psi impression period and facilitate psi functioning.

The psi task involved GESP of a color slide being viewed by a sensorially isolated agent (in normal consciousness) who was unaware of the group membership of the subjects. At the start of the session the agent randomly selected one of 15 six-picture target packs, then randomly selected one of the six pictures as the target. A corresponding slide of the chosen picture was the actual target. The overall target pool had been prepared beforehand by another person who had attempted to use simple, striking pictures with the added criteria that the six selections within a pack be as dissimilar as possible in form, color, and theme, and that none of the pictures represent material which might ordinarily be found in "nature." After the experimental session, the pack containing the target and five alternative control pictures was conveyed to the subject without the possibility of sensory leakage, and the subject compared the coded pictures with his written and/or drawn impression protocol and rank-ordered the six pictures from most likely to least likely target. Ranks of 1 (best correspondence), 2, and 3 were classified as hits; 4, 5, and 6 (worst correspondence) were misses. Binomial tests indicated the presence or absence of psi; Mann-Whitney U tests were used to contrast group performance.

To assure that the two groups did not differ on other "state" or subject variables which might themselves affect psi, the subjects completed a 16-item questionnaire at appropriate times throughout the experiment. This allowed assessment of any possible confounding variables which might contribute to a group difference. In addition, immediately before listening to his right or left hemisphere tape, each subject listened to a ten-minute progressive relaxation tape. This was done to assure that all subjects

were equally relaxed at the beginning of the experiment. All subjects were told to attempt to maintain relaxation throughout the entire experiment. The questionnaire items on "state" served as manipulation checks on the effectiveness of the relaxation tape and assured that the two groups did not differ on this crucial, possibly confounding variable.

We hypothesized that the left-hemisphere subjects would do poorly on the GESP test, that the right-hemisphere subjects would do well, and that there would be a significant difference between the two groups. These hypotheses were partially confirmed. The left-hemisphere group did show significant psi-missing (one hit and nine misses; binomial $P = .0112$), but the right-hemisphere group scored only at chance level (five hits and five misses). The Mann-Whitney U test showed a marginally significant ($P < .05$) difference between the two groups. These results, while not exactly in accord with our predictions, give encouragement for further work.

THE PSI CONDUCIVE SYNDROME: FREE RESPONSE GESP PERFORMANCE DURING AN EXPERIMENTAL HYPNAGOGIC STATE INDUCED BY VISUAL AND ACOUSTIC GANZFELD TECHNIQUES

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The experiment described here represents the third of a number of research strategies employed in our laboratory's continuing investigation of a syndrome believed to be psi-conducive. The first strategy involved inducing the syndrome through progressive muscular relaxation and suggestions for mental quietude. Psi performance was enhanced in our relaxed subjects and was found to be positively and significantly correlated with degree of physical and mental relaxation. The second strategy involved an attempt to induce psi-conducive and psi-antagonistic states through the evocation of "right-hemispheric" and "left-hemispheric" functioning, respectively (see previous paper). The rationale of this strategy was that the Psi Conducive Syndrome may be induced if a significant subset of its characteristics is strong-

ly encouraged or evoked. The subset of concern in the "hemispheric" study was a receptive, nonanalytical, noninterpretive mode of functioning.

The third strategy focuses on another important subset of characteristics of the Psi Conducive Syndrome: direction of attention away from external stimuli and toward internal processes, change in body awareness, change in sense of time, and decreased logical and increased paralogical thought processes (including increased imagery). As Honorton and Harper noted at the 1973 Parapsychological Association convention, these factors may be important ingredients of psi-conducive states. These same investigators have suggested that this subset of characteristics is an important accompaniment of the experimental hypnagogic state induced by constant unpatterned sensory input (Ganzfeld technique).

In the present experiment, we investigated the influence of the Ganzfeld-hypnagogic technique upon the psi process, in what might be called a "systematic replication" and extension of the Honorton-Harper study. Our study differed from it in the following ways. First, we added a control group, so that the effectiveness of the hypnagogic-Ganzfeld technique could be directly assessed. Second, we employed different target material (postcard-sized reproductions of paintings). Third, we changed some minor procedural details (we used loud white noise rather than seashore sounds, and white rather than red light). Fourth, we introduced a definite five-minute psi impression period (indicated to the subject by loud electrical "thumping" sounds which could be heard through the white noise of the subjects' headphones) after 30 minutes of Ganzfeld stimulation had been experienced by the subject. The subject had been told beforehand to try to receive psi impressions during this five-minute period, remember them, and record them on paper as soon as the impression period was over. Fifth, a questionnaire was used to assess and eliminate possible artifactual differences between groups on variables such as mood and expectancy.

The experimental group consisted of ten college students who were exposed to diffuse white light viewed through halved ping-pong balls and to loud (intensity was subject-adjusted) white noise presented through headphones. The subject sat in a reclining chair throughout the session. This visual-acoustic Ganzfeld was maintained for 35 minutes, during which the subject was instructed to speak continuously

about anything (s)he thought of, imagined, felt, or sensed. The last five minutes constituted the psi impression period. The subject was to attempt to receive impressions of a target picture randomly selected by the agent and viewed by the agent at that time.

A control group of ten subjects was treated similarly, with the exception that the subject spent the 35 minutes sitting comfortably (with eyes open) in the same reclining chair in a dimly illuminated soundproof room, but without ping-pong balls or headphones attached. Later the target, along with five alternative control pictures, was conveyed to the subject in coded form and without the possibility of sensory leakage. The subject rank-ordered the six pictures in terms of their correspondence with his or her own written and/or drawn impression protocol. Rankings of 1 (best correspondence), 2, and 3 were scored as hits; rankings of 4, 5, and 6 (worst correspondence) were misses. At appropriate times during the experiment, all subjects completed a 16-item questionnaire which was used to assure that the two groups were comparable in terms of possibly confounding variables such as mood, expectancy, and state.

The results indicated significant psi-hitting in the experimental group and chance performance in the control group. Under the hypnagogic-Ganzfeld condition, there were ten hits and no misses (binomial $P = .001$); the control group scored five hits and five misses (chance). The mean psi scores (ranking assigned the correct target) of the experimental and control groups were 2.0 and 3.4, respectively; the performance of the experimental group was significantly superior to that of the control group (Mann-Whitney $U = 21.5$; $P < .025$). Mann-Whitney analyses of questionnaire scores indicated that the two groups did not differ on any of the 16 items not related to the induction procedure per se, indicating that these were not confounding variables in this experiment.

Thus, our results replicate and extend the generality of Honorton and Harper's finding that the hypnagogic-Ganzfeld procedure facilitates the occurrence of receptive psi. They also suggest that deprivation of patterned visual and auditory stimulation (deprivation of exteroceptive information) and the accompanying increased attention to internal processes may be important components of the Psi Conducive Syndrome. It is hoped that further research will indicate whether these components alone are sufficient to facilitate psi or whether

their occurrence aids in a more general shift in other important psi-modulating components of the Syndrome.

BIOFEEDBACK REGULATION OF EEG ALPHA, MENTAL IMAGERY, AND PSI GUESSING PERFORMANCE

Garnet Pleshette (Maimonides Medical Center, Brooklyn)

Twenty adult volunteers served as subjects in a two-part session involving feedback control of EEG alpha rhythm activity (8-13 Hz) and psi guessing performance utilizing a binary Schmidt random number generator. In Part I the subject, after completing the Betts QMI Vividness of Mental Imagery Scale, underwent the following procedure: a five-minute adjustment period following electrode application (left occiput to right earlobe), a five-minute baseline EEG recording without feedback, a five-minute free exploration period in which the subject was given auditory feedback to the presence of EEG alpha activity with instructions to freely explore conditions associated with the presence of the tone, and finally ten trials of two-minute tone on, two-minute tone off, and two-minute rest (no feedback). The order of tone on and tone off was alternated in blocks of two trials. During the tone on periods the subject attempted to keep the tone on by inducing alpha-frequency EEG; during the tone off periods (s)he attempted to keep the tone off by suppressing alpha. Subjects with less than 10 percent alpha in the baseline period were excused from further participation in the study.

In Part II, initiated after a short break following completion of Part I, the subject completed five additional tone on, tone off, and rest periods. In addition, during Part II, (s)he made guesses on a binary Schmidt random number generator which also provided auditory feedback to hits. The subject was instructed to make a minimum of five guesses for each two-minute alpha feedback trial. No maximum was set, and the subject was encouraged to make guesses only when (s)he felt some degree of confidence as to correctness. The numbers of hits and trials were automatically registered on miniature counters on the random number generator and on the polygraph record monitored by the experimenter.

When amount of alpha during tone on periods was compared with amount during tone off periods to see if the subjects had been able to control the abundance of their alpha, it was found that 15 of the 20 subjects demonstrated independently significant alpha control ($P < .05$) in Part I, while 11 of the subjects demonstrated independently significant alpha control in Part II. Group differences in percent-time alpha (on versus off) were significant for both Part I ($P < .0005$; Sandler $A = .109$, $df = 19$) and Part II ($P < .0005$; $A = .187$, $df = 4$). No significant differences were obtained between on and off periods in psi guessing performance.

Psi guessing performance was also examined in relation to the subject's mood (as measured by the Clyde Mood Scale) and vividness of mental imagery (as measured by the Betts QMI). No significant differences in psi performance were found in relation to positive and negative mood ratings. However, a statistically significant relationship was found between the subjects' psi guessing performance and mental imagery. Subjects designated as strong imagers (below the group median on the QMI) obtained significantly lower psi performance than those designated as weak imagers (above the group median): $t = 2.22$, $df = 18$; $P = .039$. This result is directly opposite to the findings of Honorton, Tierney and Torres [see pages 157-60], who found a positive relationship between mental imagery as defined by the QMI and ESP card-guessing performance.

Mental imagery scores were not significantly related to the subjects' baseline alpha level ($r = +.06$).

THE EFFECT OF "NORMAL" VS. RELAXED STATES OF CONSCIOUSNESS ON ESP SCORING USING A GESP FEEDBACK TESTING DEVICE

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and H. H. J. Keil (University of Tasmania)

A portable, low-cost GESP feedback testing device was developed for telepathy research with sender-receiver teams. With this device an exploratory experiment comparing the effect of "normal" versus relaxed states of con-

sciousness on the ESP scoring of one male subject (J.S.) was completed. It was hypothesized that the subject would achieve a higher overall score while in a relaxed state of consciousness ("RSC"). This hypothesis was based on the reasoning that, with the amount of sensory input being lower in a RSC, there would be less "noise" to interfere with a theoretically weak signal.

The testing device consisted of two separate panels: a control panel for the experimenter and a response panel for the subject. In this experiment the subject and experimenter, with their respective panels, sat in two separate rooms about 15 feet apart and separated by a door. Their panels were connected through a small, sound-insulated hole in the wall by a wire cable.

The experimenter (H.H.J.K.) determined the target order with a computer-generated random number table which he entered using dice. After recording the 60 targets to be used in the session, he set his control panel to the first target, one of four different colored lights, and turned on a repeat cycle timer. In five seconds this timer switched on the selected target light and at the same time switched on a light located near the subject's response panel which served as a signal that he could begin guessing. The subject's signal light remained on for 20 seconds, during which time he was allowed to make two guesses, making the second guess only if the first guess was incorrect. He made his guess by pushing forward one of the four toggle switches on his panel. These four switches corresponded to the four colored lights on the control panel. This action turned on a response light on the experimenter's panel. If his guess was correct, both the subject's signal light and the target light on the control panel would go off, providing immediate feedback to the subject. The experimenter recorded whether the subject made a hit or a miss, and if it was a hit, whether it occurred on the first or second guess. It had been agreed upon before the experiment began that only hits made on the first guess would be tested for significance, the second guess being solely for the subject's benefit.

Sixty trials comprised a session for each condition, and these sessions were conducted in alternating sequence until 300 trials for each condition had been completed, resulting in 600 trials for the total experiment. The RSC condition was induced with a half-hour modified Jacobson progressive relaxation tape which the subject listened to before

each session of that condition. This condition differed slightly in procedure from the "normal" state of consciousness condition in that the subject required the assistance of an intermediate person to inform him of the beginning of each trial, enter the guesses he called out, and provide him with feedback when a hit was made. This procedure was necessary since, for the subject to maintain the RSC condition, he had to remain with his eyes closed in a reclining chair.

The results of the total 600 trials were not significant, but indicated a positive trend ($P < .11$). However, contrary to what was expected, the results of the "normal" state of consciousness when analyzed by themselves gave a significant positive deviation ($CR = 2.27$; $P < .03$), while the RSC scores were exactly at chance level. Two different tests for nonrandomness of the distribution of targets in the "normal" condition were not significant, indicating that the positive scoring was not an artifact.

It appears that at least in the case of this one subject, physical relaxation did not improve ESP scoring. However, relaxation has a mental component as well as a physical one. Thus, it is possible to be physically relaxed but not mentally, as when one is reading a book while sitting in his easy chair. Physical relaxation, though, facilitates mental relaxation, and one is likely to doze off it he gets too comfortable. But physical relaxation does not by itself calm the mind; something else is needed. Just as the physical exercises in this experiment (tightening and quickly relaxing muscles) were used to induce a state of physical relaxation, so are mental exercises needed to achieve a state of mental relaxation. The meditative exercises (such as staring at a candle) of esoteric Eastern disciplines enable one by concentrating on a constant stimulus (the effect of which is as if no stimulus is present) to "turn off" his awareness of the sensory world, thus achieving the goal of a "calm" mind.

In line with this, the possible effect of mental relaxation agrees with the subject's subjective belief that when he was able to practice certain of these meditative exercises correctly (that is, without having extraneous thoughts interrupt his concentration) before a trial began, these exercises facilitated ESP. Thus, the purpose of feedback in an ESP experiment should not be thought of as enabling the subject to discriminate internally which of the lights is the target, but as an aid in helping him discover which internal

approaches or techniques facilitate his ESP ability.

A further experiment is planned to test this idea of whether meditative exercises correctly practiced before each trial will result in higher scoring than when they are not practiced. These meditative exercises are conceived of as having a calming, placating effect on the conscious mind, enabling it to be bypassed and the unconscious mind reached, resulting in a more intuitive, spontaneous and less logical guess. Since it is not always easy to practice these meditative exercises correctly (as one can easily discover for himself by concentrating on counting each breath for any length of time) a "pass" option will be included in the testing procedure of future experiments, so that the subject can choose in advance not to guess on those trials for which he feels inadequately prepared.

USE OF AN AUTOMATIC STIMULUS GENERATOR TO TEACH EXTRASENSORY PERCEPTION

Russell Targ† and Phyllis Cole (Stanford Research Institute, Calif.)

The purpose of this experiment was to determine whether ESP learning can take place. Employing 147 subjects, it attempted to replicate an earlier pilot experiment in which one of 12 subjects working with a four-choice random stimulus generator machine showed significant learning over 672 trials at the .005 probability level. The apparatus was designed to enhance the ESP ability that may be latent in some people. Our hypothesis was that enhancement could be accomplished by allowing the user of the machine to become aware of his own mental state when he is most successfully using his extrasensory faculties. With increased consciousness of his mental state during ESP, he may be able to bring his hitherto intermittent faculties under volitional control.

The teaching machine randomly selected one of four targets on each trial. This target was not presented to the subject until he had indicated to the machine what he believed the target to be. The targets were 35-mm. color transparencies of paintings. Thus the machine was used to examine

clairvoyant perception in which experimenter and subject both remained ignorant of the machine's state until the subject had made his choice.

An important feature of the experiment was that the choice of a target was not forced. That is, the subject pressed a "pass" button on the machine when he wished not to guess. When the user of the machine indicated his choice, he was immediately and automatically informed of the correct answer. If his choice was correct, the number-of-hits counter on the machine was incremented and a bell sounded. Whether or not the choice was correct, the number-of-trials counter on the machine was incremented.

The randomness of the machine was carefully investigated. In testing the null hypothesis that the probability of the same target occurring twice in a row is one-fourth, we computed the binomial probability of obtaining the observed number of such "repeats" in a given number of trials. We also tallied the initial machine states following reset and the transition probability between any two states. In each case, the number of occurrences of each of the 16 possible transitions should be approximately equal. Before a machine was used with experimental subjects, it successfully passed these randomness tests in a minimum of 10,000 trials.

After an exploratory phase involving work with two subjects, the machine was modified to permit automatic recording of subject choices, machine choices, and "passes" on a printer or on both a printer and a Teletype paper-tape punch. Next, a screening experiment was conducted with 145 volunteer subjects; 79 adults and 66 children, mainly 12 to 14 years old. The adults and one-third of the children worked in solitude with the ESP teaching machine in a Stanford Research Institute laboratory. The remaining children worked on machines that were placed in their schools. In this phase of the experiment, all data were automatically printed on paper tape by the printer attached to the machine.

Necessary to evidence of clairvoyant learning is a statistically significant positive learning slope; that is, an increase in the average number of hits per run. Twenty-five hits for a run of 100 trials would be expected by chance. In the exploratory and screening phases of the experiment, comprising a total of 147 subjects, we identified six subjects who had a positive slope significant at the $P = .01$ level or better, using a statistic directly proportional to the usual

least-squares estimate of slope; the exact binomial probability of this occurring by chance is 3.8×10^{-3}. Excluding these six subjects, we found that the slopes of the remaining 141 subjects appeared to be normally distributed. At the other extreme, we noted with interest that no subject had a negative slope significant at the P = .01 level or better.

It should be noted that two subjects (not among the six who showed learning) had significantly high mean scores over their total testing period. One had a mean run score of 30.50 over 1400 trials, an occurrence with a binomial probability of 2×10^{-6}. The other had a mean run score of 29.57 over 2800 trials, with a binomial probability of less than 10^{-6}. These results indicated that the ESP machine can serve as a suitable screening device for those with ESP ability.

Additional work was conducted with 11 subjects who had shown the greatest ability. Experimental protocols were modified so that the noisy printer could be located at a distance from the subject. In addition, a Teletype was attached to the printer to provide data in computer-readable form. Data analysis indicated no subject's performance differed from chance expectation, nor was there any evidence of learning. Interviews with articulate, high-scoring subjects from the initial tests indicated discomfort with the Teletype connected to the printer. They believed they could perform better without the printer or Teletype recording data.

The machines were then re-tested for randomness, and six high-scoring subjects commenced work under a revised protocol without the Teletype. An experimenter recorded each subject's scores. Only one subject was able to replicate his original performance. Over 2500 trials his mean score of 27.88 had a binomial probability of 4.19×10^{-4}. At his request, during this series, this subject was permitted a few clearly demarcated daily practice sessions specified in advance. During practice sessions, his performance did not differ from chance expectation. The dramatic difference between practice and test sessions indicates the subject used practice sessions to explore man-machine interaction and to develop skills for test sessions.

REMOTE VIEWING OF NATURAL TARGETS

Harold Puthoff† and Russell Targ (Stanford Research Institute)

Research results described by Osis indicated the feasibility of a study to determine whether a subject could describe randomly chosen geographical sites located several miles away and demarcated by some appropriate means. We present here the results of such a study, termed remote viewing, carried out with Mr. Pat Price, a former California police commissioner and city councilman. It consisted of a series of double-blind tests involving local targets in the San Francisco Bay area which could be documented by several independent judges. We felt that natural geographical places or man-made sites that have existed for a long time might be more potent targets for paranormal perception experiments than artificial targets prepared in the laboratory. This hypothesis is based on opinions expressed by some subjects that the use of artificial targets involves a "trivialization of the ability" as compared with natural preexisting targets.

In each of nine experiments involving Mr. Price as remote-viewing subject and Stanford Research Institute (SRI) experimenters as a target demarcation team, a remote location was chosen in a double-blind protocol. Mr. Price, who remained at SRI, was asked to describe this remote location, as well as whatever activities might be going on there. Final judging indicated that several descriptions yielded significantly correct data pertaining to and descriptive of the target location.

In the nine experiments, the following procedures were used. A set of 12 target locations, clearly differentiated from each other and within 30 minutes' driving time from SRI, had been chosen randomly from a target-rich environment (more than 100 targets of the type used in the experimental series) prior to the experimental series by an individual in SRI management, the Director of the Information Science and Engineering Division, not otherwise associated with the experiment. Both the experimenters and the subject were kept blind as to the contents of the target pool, which were used without replacement.

At the start of each experiment, an experimenter was

closeted with Mr. Price at SRI to wait 30 minutes before beginning the narrative description of the remote location. The SRI locations from which the subject viewed the remote locations consisted of an outdoor park (Experiments 1, 2), a double-walled copper-screen Faraday cage (Experiments 3, 4, 6-9), and an office (Experiment 5). A second experimenter would then obtain a target location from the Division Director.

The target demarcation team, consisting of two to four SRI experimenters, then proceeded directly to the target by automobile without communicating with the subject or experimenter remaining behind. Since the experimenter remaining with the subject at SRI was ignorant both of the particular target and also of the target pool, he was free to question Price to clarify his descriptions. The demarcation team then remained at the target site for an agreed-upon 30-minute observation period following the 30 minutes allotted for travel. During the observation period, the remote-viewing subject described his impressions of the target site into a tape recorder. A comparison was made later when the demarcation team returned.

In general, Mr. Price's ability to describe correctly buildings, docks, roads, gardens, and so on, including structural materials, color, ambience, and activity, sometimes in great detail, indicated the functioning of a remote perceptual ability. However, the descriptions contained inaccuracies as well as correct statements. To obtain a numerical evaluation of the accuracy of the remote viewing experiments, the experimental results were subjected to independent judging on a blind basis by five SRI scientists who were not otherwise associated with the research.

The judges were asked to match the nine locations, which they independently visited, against the typed transcripts of the tape-recorded narratives of the remote viewer. The transcripts were unlabelled and presented in random order. The judges were asked to find a narrative which they would consider the best match for each of the places they visited. A given narrative could be assigned to more than one target location. A correct match required that the transcript of a given date be associated with the target of that date. The numbers of correct matches made by the five judges were 7, 6, 5, 3, and 3, for a total of 24, a result having an a priori probability of 8×10^{-10}. (By chance a total of five correct matches would be expected.)

Among all possible analyses, none is more conservative than a permutation analysis of the plurality vote of the judges' selections assuming assignment without replacement, an approach independent of the number of judges. By plurality vote, six of the nine descriptions and locations were correctly matched. This outcome has an a priori probability of 5.6×10^{-4} since, among all possible permutations of the integers one through nine, the probability of six or more being in their natural position in the list has that value. Therefore, although Price's descriptions contained inaccuracies, the descriptions were sufficiently accurate to permit the judges to differentiate among the various targets to the degree indicated.

ESP OF CARDS PREVIOUSLY SENSORIALLY FOCUSED

Carroll B. Nash† and Robert Kreb (St. Joseph's College, Pa.)

Experiments with Pavel Stepanek have disclosed his tendency to exhibit a greater deviation from chance expectancy in his scores on certain cards than on others. This focusing of ESP has taken the form of a positive deviation (psi-hitting) on certain cards and a negative deviation (psi-missing) on others.

The present experiment is an attempt to determine the presence of psi focusing in a group of 30 subjects tested individually. The subjects were freshman psychology majors in a biology course taught by C.B.N. A new ESP deck was assigned to each student. Five cards, one of each symbol, were randomly removed from the deck and the subject encouraged to identify with these five cards by holding them in his hand and looking at them. These five cards, and the other 20 cards which the subject did not handle, were each blindly placed by C.B.N. in a separate opaque envelope, the envelopes being numbered from 1 to 25. The numbers of the envelopes containing the focused cards were recorded. As the symbols on the cards in the 25 envelopes were not known by the subjects or by R.K. until the envelopes were opened after the experiment, it was a test of clairvoyance.

Each subject was tested with his personal deck by R.K. in ten runs. The envelopes were placed in random order on the basis of a table of random numbers. The number of each envelope was recorded on the subject's individual call sheet and next to it was recorded the subject's call of the ESP symbol he thought to be on the enclosed card. The number on the envelope was not known by the subject. The subjects were informed of the purpose of the experiment and a prize of five dollars was offered to the one who had the highest score on the focused cards.

Both the focused and the nonfocused cards showed negative deviations from chance expectancy. Although the difference between the two was not significant, the deviation was greater with the focused cards. The CR of the deviation of the focused cards was 2.13 with $P = .03$. Taking the subject as unit, a t-test with the focused cards also gave a marginally significant negative deviation ($t = 2.054$, $df = 29$; $P < .05$, one-tailed). The score on the nonfocused cards was insignificantly different from chance.

These results suggest that the subjects expressed ESP in calling the symbols of cards on which their attention had been previously sensorially focused. They do not indicate that psi expression was enhanced by previous sensorial focusing on the cards.

DIGITAL TRANSMISSION AND RECEPTION OF EXTRASENSORY IMPULSES INDUCED BY PHOTIC STIMULATION

David R. Wheeler (Baruch College, City University of New York)

Several researchers have reported finding that various frequencies of electromagnetic waves affect the behavior and/or patterns of electrical activity in the brains of different organisms. It seems that it is possible to transmit bits of information in the form of electromagnetic waves without the necessity of converting them into different energy forms. These electromagnetic fields could be received by some form of neural transducers. Advanced electronics allows an exacting exploration of the generation and transmission of biologi-

cal electricity.

Even with the great technological advances that have been made in electronics, there are still two problems that must be solved in order for progress to be made in the study of extrasensory phenomena. The two problems are the transmission and the decoding of electromagnetic brain impulses. Assuming that ESP is some unknown part of the electromagnetic spectrum, the two greatest obstacles to discovering and using telepathy will be the unraveling of the secret code of the brain's neurons, and the ability of one human brain to perceive the extremely minute electrical fields given off by another brain.

The experiment I propose will attempt to determine if humans can transmit and receive brain impulses. A flashing light will help solve one of the problems of transmitting an impulse in the form of a weak electrical signal. Research has shown that a flashing light causes an entire train of brain waves that respond at the same frequency as the light. It seems possible to cause several million neurons to fire at one time. With many times more firings than normal, it appears that the chance of transmitting a signal of stronger than normal intensity is increased. A flashing light can be used to make the human brain act as if it was a transmitter of digital-type information. Research has suggested that humans can produce neural electromagnetic waves that can be controlled so that a coded message results.

In this proposed experiment, two subjects will be used to transmit and receive the digital messages. At the start of the experiment both subjects will be exposed to the flashing light at the same time and frequency of ten per second. The reason for the simultaneous presentation of the flashing light is to attempt to synchronize the two brains.

Once the EEGs of the two subjects are synchronized to the frequency of the flashing light, one of the subjects will be prevented from seeing the light. The other subject will continue to be exposed to flashes of light which will cause his brain rhythms to match the frequency of the light. The experimenter will randomly flash the light over certain time periods. The subject being presented with the flashing light will act as the sender, and the second subject will act as the receiver. Both subjects will be shielded within the same Faraday cage.

One of the difficulties that is foreseen in this experiment is the control of sensory cues. Relays that emit noises and leakages of light to the receiver must be rigorously controlled so that only the transmitted neural patterns from the sender are received.

SELF-ACTUALIZATION AND PSI: PREDICTION OF PERFORMANCE USING STEPWISE DISCRIMINANT FUNCTION ANALYSIS

Alfred Scopp (Psychical Research Foundation, Durham, N.C.)

It has been suggested that transpersonal consciousness and psi ability are closely related. Maslow argued that persons who are likely to experience states of transpersonal consciousness (peak experiences) can be characterized as self-actualizers. Characteristics of self-actualizers include independence, capacity for self-support, spontaneity, self-acceptance, freedom from fear, and capacity for warm interpersonal relationships. If transpersonal consciousness and psi ability are closely related, then persons with psi abilities may have self-actualizing characteristics. It is not clear whether the non-self-actualizing person is likely to show no evidence for psi or to show evidence for negative psi.

The purposes of the present study were to identify personality variables which discriminate between ESP subjects who score substantially above chance and subjects who score substantially below chance; and to determine whether these personality variables can be successfully used to identify subjects for whom the probability of a high level of ESP is great and to identify subjects for whom the probability of a low level of ESP is great.

The data used in the present study come from a large investigation carried out by Kanthamani and Rao in India during 1967-1968. High school students were tested for ESP, personality, and mood. Personality was measured by the High School Personality Questionnaire (HSPQ) developed by Cattell. A blind-matching clairvoyant ESP test was employed, using the standard ESP cards. The subjects were tested individually in two sessions. In each session two

runs of 50 trials each were given, so that each subject contributed a total of 200 trials.

ESP Positive and ESP Negative groups were identified. The ESP Positive group included all subjects who had an ESP score of at least one standard deviation above chance. The ESP Negative group included all subjects who had an ESP score of at least one standard deviation below chance. Chance performance on the ESP task was 40 and the standard deviation was 5.7. Out of a total of 146 subjects, 21 were identified as members of the ESP Positive group, with scores of 46 or higher. The ESP Negative group consisted of 31 subjects with scores of 34 or less. Using the 17 scales of the HSPQ as predictor variables and membership in the ESP Positive versus Negative groups as the criterion, a stepwise discriminant function analysis was completed.

Discriminant function analysis is a multivariate statistical method which is useful whenever questions of comparison or prediction are raised in research. Discriminant function analysis is an important addition to multivariate analysis of variance since, in addition to determining whether two or more groups differ with respect to a set of criterion variables, it is useful in identifying the variable or combination of variables along which the groups differ most significantly.

The addition of new variables was terminated when the F ratio to enter for the next variable fell below the .05 level of confidence. Four of the 17 predictor variables had an F to enter which was significant at the .05 level of confidence. Using these four, the stepwise discriminant function analysis resulted in the correct classification of 16 out of 21, or 76.2 percent, of subjects previously classified into the ESP Positive group, and 30 out of 31, or 96.8 percent, of subjects previously classified into the ESP Negative group. A total of 46 out of 52, or 88.5 percent, of the subjects were correctly classified into either ESP Positive or ESP Negative groups.

Characteristics associated with membership in the ESP Positive group in order of their importance were: less neurotic, warm-hearted, assertive or dominant, and self-sufficient or resourceful. Characteristics associated with membership in the ESP Negative group in order of their importance were: more neurotic, reserved, obedient and submissive, and socially group dependent. The four significant

variables accounted for 54.7 percent of the variance between ESP Positive and ESP Negative groups.

The F-value for discrimination between ESP Positive and ESP Negative groups was 14.2 with 4 and 47 degrees of freedom. The discriminating power of the four scale scores was significant beyond the $P < .0001$ level. Thus personality variables were identified which effectively discriminated between ESP Positive and ESP Negative groups. The personality characteristics associated with membership in the ESP Positive group suggested that ESP Positive members had personality characteristics similar to those characterizing Maslow's self-actualizing personality type.

Because the discriminant weights used in the classification of subjects were computed from data derived from these same subjects, the percentage of correct classification might have been spuriously high. To determine the effectiveness of the four discriminant weights derived from one sample in classifying subjects from an independent sample, cross-validation was completed. The discriminant weights generated by the analysis of the first two-thirds of the sample were applied to individuals constituting the last one-third of the sample. This was repeated two more times, so that prediction was made to each third of the sample. This cross-validation procedure resulted in the correct classification of 14 out of 21, or 66.7 percent, of the ESP Positive group; and 26 out of 31, or 83.9 percent, of the ESP Negative group. A total of 40 out of 52, or 76.9 percent, of subjects were correctly classified into either ESP Positive or ESP Negative groups.

In conclusion, personality variables which successfully predict ESP performance were identified with the Indian sample used in this study. Discriminant function analysis appears to be a promising tool in parapsychology research problems in which questions of comparison and prediction are raised.

IN WHICH HAND ARE THE M&M'S? A PRELIMINARY STUDY OF ESP IN RELATION TO COGNITIVE DEVELOPMENT AND TESTING ENVIRONMENT

Sally Ann Drucker† and Larry Rubin (Maimonides Medical Center)

While there have been studies indicating that young children may be more successful in ESP guessing tasks than adults, such results have usually been ascribed to attitudinal and motivational factors, and have not been examined in relation to differences in cognitive organization between adults and children. According to Piaget, a major change in cognitive organization occurs roughly between the ages of five and seven. The earlier form of cognitive organization is referred to by Piaget as "preoperational" or "prelogical," and is characterized by the young child's tendency to deal with things one aspect at a time in terms of their perceptual appearance. Piaget refers to the later form of cognitive organization as "operational" or "logical," since it involves the ability to deal with sets of features several at a time in a structured relationship.

The purpose of this study was to explore patterns of ESP guessing success in a gamelike situation with children in relation to cognitive organization as assessed by Piaget's conservation of liquids test. The subjects were 42 children between the ages of four and seven, about evenly divided in terms of age and sex.

To provide an ESP task which would be motivationally appropriate to young children, we borrowed an idea from an old TV commercial for M&M's candies ("in which hand are the M&M's?"). The five colors used were brown, red, orange, green, and yellow. The targets were 100 M&M's, 20 of each color, thoroughly mixed in a brown paper bag. The child's task was to guess which color the experimenter would pick out of the bag immediately following the subject's guess. If the subject's guess was correct, (s)he was allowed to pick an M&M of that color out of an identical bag, to either save or eat. Each subject completed two 25-trial runs, with immediate feedback after each guess. The experimenter wore a cotton glove to eliminate tactile cues as she reached into the bag to select each target. Following each trial, the target was replaced in the bag. The bag was mixed up again after each five-trial segment. The bag was placed such that

neither experimenter nor subject could see into it, much less the bottom of it where the M&M's lay.

Following the M&M test, the subject was given Piaget's conservation of liquids test. The child was shown two 50-ml. beakers with the same amount of water in each. The water from one was then poured into a 25-ml. graduated cylinder and the child was asked if it was the same amount as the water remaining in the first beaker. The water was then poured from the 25-ml. graduated cylinder into a 15-ml. cylinder and the child was again asked if it was the same amount as in the first beaker. Finally, the water was poured into six whiskey glasses and the child was again asked if there was the same amount as in the first beaker. On the basis of their responses, the children were divided into three groups: "preoperational," "operational," and "mixed."

The overall ESP run score mean was 5.42 ($t < 1$, $df = 40$; nonsignificant). The 21 children classified as unambiguously "preoperational" obtained an ESP mean of 5.05, which is totally nonsignificant. Only six children were classified as unambiguously "operational" with a mean scoring rate of 5.83; because of the small number, this was not significant ($t = 1.24$). There were 15 children who were classified as "mixed," having made some responses which were "prelogical" and others which were "logical." This group obtained significantly positive ESP scores, with a mean of 5.77 ($t = 2.64$, $df = 14$; $P < .02$).

Internal consistency of scoring (correlation between each child's first and second run) was not significant in any group, but seemed to be positively related to cognitive operativity: Prelogical $r = -.093$ ($df = 19$); Mixed $r = +.166$ ($df = 13$); Logical rho $= +.671$ ($N = 6$). After data collection, the fact that the same experimenter tested for both ESP and cognitive organization was criticized. In the follow-up study now under way, two experimenters are testing for each separately.

We also examined the children's ESP success in relation to testing environment. Twenty-seven of the children were tested in their own homes or the homes of neighbors. These subjects obtained overall significantly positive ESP scores, with a mean of 5.74 ($t = 2.34$, $df = 26$; $P < .05$). The 15 children tested in a school situation obtained a nonsignificant ESP mean of 4.83 ($t < 1$). The difference in ESP success between children tested at home and in a school

situation was also significant ($t = 2.28$, $df = 40$; $P < .05$). These results are statistically independent of the Logical-Prelogical-Mixed group scoring differences, and are consistent with J. G. Pratt's suggestion that familiar environments for the subjects produce higher test results.

SOME FINDINGS RELEVANT TO THE CHANGE IN STATE HYPOTHESIS

Adrian Parker (University of Edinburgh, Scotland)*

One of the few functional, as distinct from correlational, hypotheses to emerge from parapsychological research is the notion that ESP is associated with a shift in the state of consciousness. More specifically, the shift is hypothesized to be a movement of consciousness from externally directed perception to internally directed imagery. The experimental evidence for this hypothesis comes mainly from the work of Honorton and his colleagues on hypnotic dreams, EEG alpha states, and states resulting from partial sensory deprivation. In all studies a self-report scale was used to assess subjective changes in the state of awareness accompanying the ESP task.

The present experiment began in October 1973 as an attempt to replicate and extend these findings using an experimental hypnagogic procedure, the Ganzfeld technique, in order to produce altered states. This is a method for producing a homogeneous field of auditory and visual stimulation. The subject listens to white noise by means of earphones while red light is shone through the split halves of ping-pong balls placed over his eyes. The resulting experience can be described as hypnagogic imagery and provides a ready means of inducing changes in the state of consciousness.

Two hypotheses were made: that those subjects who experienced relatively large changes in state during the experiment would score significantly on the ESP task, and that those subjects who reported highly altered states throughout the experiment would also score significantly. Unknown to

*Presented by Richard Broughton.

me at this time, Honorton and Harper had been experimenting with the same technique; their findings, which differ somewhat from those of this study, were reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 52-3].

Thirty subjects were selected on a volunteer basis. Twelve were males and 18 females. The age ranged from 18 to 28, and most were students. They were first instructed in the use of the self-report state scale developed by Honorton. This is a five-point scale where a score of zero indicates the subject is awake and alert and a score of four indicates he is oblivious to external surroundings.

Subjects were then exposed to the experimental hypnagogic procedure for a period of 30 minutes during which they were asked to give state reports every 10 minutes. During the final 10 minutes of the procedure the agent, usually myself, concentrated on an art picture card which he had randomly selected from a set of four such cards. At the end of the session the subject was asked to make a brief record of his imagery. The agent then handed him an envelope containing the set of four cards, one of which was the target, and left him to rank the cards in order of similarity to the imagery he had experienced. No communication was allowed between agent and subject until this had been completed.

The same analysis was carried out as had been used in a sensory deprivation experiment by Honorton, Drucker, and Hermon, reported at the 1972 Parapsychological Association convention [RIP 1972, pp. 137-9]. Ranks of 1 and 2 were counted as hits and ranks of 3 and 4 regarded as misses. The results tended to be a mirror image of the Honorton, Drucker, and Hermon experiment, but with the significance due to psi-missing rather than psi-hitting.

The total score was 11 hits and 19 misses (with 15 expected by chance), which is clearly nonsignificant. However, the mean shift in state between the first 10 minutes and the last 10 minutes was +.51, and subjects whose state reports showed a large change in state (above this mean) produced only four hits and 15 misses ($P = .013$, two-tailed). Subjects with a relatively small change in state (below the group mean) scored seven hits and four misses. Those who had overall high state reports (above the mean of 12 for five responses), indicating large alterations of state, also pro-

duced significantly negative scores by recording only one hit and 10 misses (P = .0069, two-tailed). Those who scored below the mean produced nine hits and seven misses. (Three scores occurred at the mean, one hit and two misses; they are omitted from this computation, but make no difference to the significance level.)

It remains puzzling as to why large shifts in state and high state reports should produce what is apparently a psi-missing effect here, while in previous studies the effect has been one of positive scores. Honorton and Harper's version of this experiment, which yielded positive scoring, used a recording of waves on the seashore instead of white noise, which some of their subjects had found irritating. They made no measure of change in state, but found the total scores were significantly above chance.

Some of our subjects did describe the white noise as distracting, but few if any remarked that it was displeasurable. Nevertheless this factor may have been an influence responsible for reversing the direction of the ESP effect. Whatever the cause of this, the results do suggest that change in state probably governs only the magnitude of the ESP effect and not the direction. With the proviso that one accepts the concept of psi-missing, these results can be taken as a partial replication of the change in state hypothesis.

A PILOT STUDY OF THE INFLUENCE OF EXPERIMENTER EXPECTANCY ON ESP SCORES

Adrian Parker (University of Edinburgh)*

Despite the large amount of lip service paid to experimenter effects, there has been surprisingly little actual research on this topic in parapsychology. It is true that experimenter differences have occasionally been noted and the effect often used as a rationale to explain the failure of replications. However, there has been almost no attempt to study the mechanism of this effect. An exception is an experiment reported by Honorton, Ramsey, and Cabibbo at the

*Presented by Richard Broughton.

1972 Parapsychological Association convention [RIP 1972, pp. 39-40], in which it was found that whether experimenters smiled or not, and greeted their subjects in a cold or friendly manner, had a significant effect on their subjects' ESP scores. The present pilot study was an attempt to show that the extent to which experimenters have a positive attitude toward ESP and expect high scores is also crucial to success.

The experiment was carried out as part of an undergraduate psychology course in experimental design. Eighteen students were divided on a volunteer basis into six experimenters, six subjects, and six agents. The experimenters were then further divided into two groups, A and B. Those with a strong prior bias toward belief in ESP were asked to join group A, and those with a strong bias toward disbelief to join group B. The two groups of experimenters were then separately given brief lectures on ESP and testing. Group A were told that ESP had been "proven" by scientific research using methods such as they were going to experiment with. They were then given instructions as to how to perform a GESP test and told to expect between seven and ten hits per run.

The experimenters from group B were given instructions aimed at promoting the opposite expectancy. They were told that ESP research is riddled with flaws and errors, and that there is no reliable evidence for the existence of ESP. After they were given instructions for carrying out the GESP test, they were told that if the experiments were conducted in a strict controlled manner they should obtain scores close to the chance expectation of five hits per run. Finally, both groups were asked not to discuss the experiment with members of other groups.

The procedure was for the experimenters to complete two runs of 25 trials each with their subject-agent pairs and then exchange pairs with their corresponding number in the opposite group. Thus experimenter A1 exchanged pairs with experimenter B1, experimenter A2 exchanged with experimenter B2, and experimenter A3 exchanged with experimenter B3. Two more runs were then completed by each experimenter with the new subject-agent pairs. This exchange was for the purpose of reducing the effect of subject differences on the scores. All runs were carried out in the GESP condition with subjects and agents located in separate cubicles. Scoring was checked by the experimenter, subject, and agent, and in most cases guesses and card order were recorded

separately. (Since not all the duplicate record sheets were returned, we cannot be sure there were no recording errors.) A post-experimental interview was conducted, and it was concluded from this that the expectancy instructions had been convincing and effective.

The total score was nonsignificant (118 hits with mean chance expectation of 120), but as predicted the scores of group A were significantly higher than those of group B. Group A experimenters obtained 67 hits and group B experimenters 51 hits, with mean chance expectation of 60 hits (CRd = 1.64; P = .05, one-tailed). None of the subjects produced scores which were individually significant. However, two of the experimenters contributed almost all the total deviations. One produced 28 hits with her subjects (P = .02, one-tailed), while the other produced only 13 hits (P = .05, one-tailed). It should be mentioned that the first of these had a strong preexisting bias toward accepting the existence of ESP.

Considering the sample size was so small, the results seem to demonstrate that the influence of expectancy on success and failure of ESP tests is a potent one. Obviously only a beginning has been made, and we need to know to what extent the results are due to prior biases and to what extent they were determined by the "suggestions." It also seems apparent from the results that there are neutral experimenters whose results are unaffected by such suggestions. It is proposed that the follow-up will attempt to isolate these effects and study how the expectancies are passed on to the subjects and agents.

PSI AND MENTAL IMAGERY

Richard Schechter, Gerald Solfvin†, and Rebecca McCollum
(University of North Carolina, Chapel Hill)

In recent years, a considerable amount of attention has been focused upon the relationship between mental imagery and psi. A review of the literature to date on this topic will be provided by Honorton, Tierney, and Torres in a later paper [see pages 157-60]. That review cites reports of dream occurrences, spontaneous cases, and gifted

subjects where psi was reputed to have been mediated by imagery. In addition, earlier research has shown that conditions which facilitate imagery (such as dreaming, hypnagogic reverie, and perceptual isolation) are also conducive to psi.

Honorton, Tierney, and Torres then report an experiment examining individual differences in mental imagery and psi. They found that strong imagers showed significant psi-hitting, and weak imagers showed significant psi-missing on a clairvoyant task with standard ESP cards. The difference between these two groups was also significant. These findings lent considerable support to their contention that mental imagery plays a strong role in mediating psi responses.

This paper is the result of our fascination with the Honorton, Tierney, and Torres study, and with its tantalizing but confusing results. Why did the weak imagers show significant psi-missing? We would have expected the weak imagers, supposedly forming weak and fragmentary images, to score at chance level, if imagery is in fact mediating the process. This and other difficulties led us to attempt an independent replication of this study. All of the conditions of the original study were preserved. Two experimenters ran 24 subjects each. One experimenter (R.S.) used an experimental room in the Psychology Building at the University of North Carolina, the other (R.M.) used her home. Neither of these two experimenters was aware of the other's activities or results during the experiment.

The subjects were 48 volunteers (29 females, 19 males) from the Chapel Hill, North Carolina area. Most of them were students and were known to one of the experimenters. Each subject was given six clairvoyant runs of standard, closed deck ESP cards without feedback, following one preliminary run with feedback. For the first, third, and fifth runs, the subject was instructed to rely upon mental images to make his responses. For the second, fourth, and sixth runs, he was instructed to guess the first symbol which came to mind, without relying upon mental images. In addition, he was instructed to call "check" whenever he felt that his response was correct with reasonable confidence, and to try to call about five "checks" per run. After the ESP runs were completed, the subject was administered Sheehan's shortened version of the Betts QMI Vividness of Imagery Scale. Each subject was tested individually and took about 30 minutes to complete the tasks.

The median score (94) for all 48 subjects on the Betts Scale was used to define equal groups of "strong" and "weak" imagers. A tie at the median left 23 subjects in each group. The strong imagers showed a nonsignificant tendency towards psi-missing with an average ESP run score of 4.75 hits ($t = 1.10$, $df = 22$). The weak imagers showed significant psi-hitting, with an average run score of 5.67 hits ($t = 3.92$, $df = 22$; $P < .002$, two-tailed). The difference between the ESP scores of the strong and weak imagers was significant ($t = 3.21$, $df = 44$; $P < .005$, two-tailed). As in the original study, the differential instructions (imaging versus guessing) showed no effect. In addition, there were no significant differences between experimenters. On the confidence call checks, the strong imagers scored 115 correct out of 586 calls, for an average hit rate of 19.6 percent. The weak imagers scored 122 correct out of 502, for an average hit rate of 24.8 percent.

Comparing these results with the original study by Honorton, Tierney, and Torres [see pages 157-60], we find that strong imagers scored positively in the first study but scored near chance, with a tendency towards psi-missing, in our own study. The weak imagers displayed strong psi in both studies, although the direction was reversed. Taken together, the two studies seem to tell us that mental imagery is not as simply related to psi performance as was indicated by the first study.

A further indication of this is the lack of an effect, found in both studies, of the imaging versus guessing instructional sets. If we assume that the subjects were following instructions, it made no difference whether or not they "imaged" for their response, regardless of whether they were strong or weak imagers. The confidence call hit rates seemed to separate the groups significantly in both studies, but were in fact no different than the overall group hit rates. That is, the confidence call hit rates for both strong and weak imagers were equivalent to simple random samples from each group's responses. This indicates that neither strong imagers nor weak imagers had control over, or awareness of, their psi processes.

We conclude that vivid mental imagery (self-rated) is neither necessary nor sufficient for the operation of psi, and further, that self-rated weak imagery may prove to be more reliable in predicting the occurrence of psi, if not the directionality. We feel strongly that further research is ne-

cessary to gain insight into the factors that may influence the differential psi performances of weak and strong imagers.

REPORTED DREAM RECALL, PRECOGNITIVE DREAMS, AND ESP

Erlendur Haraldsson (University of Iceland)

A positive relationship between ESP scoring and the frequency of reported dream recall has been found by previous investigators. In the present experiment 223 subjects --students in 14 classes in college or vocational schools in Iceland--were each given 100 trials of a forced-choice precognition test. The targets were the digits 0 through 9, with ten hits expected by chance. The subjects, through a multiple-choice questionnaire, were asked about their frequency of dream recall (five categories of response, ranging from "every night" to "never"), experience of precognitive dreams (three categories: "often," "sometimes," "never"), and belief in the existence of psi phenomena (three categories: "certain," "possible," "impossible"). Each of the 14 sessions was conducted by two undergraduate students of psychology. Their data were pooled for evaluation.

Since subjects who reported dreaming once a month or less frequently were relatively few in number (36, or 16 percent), they were pooled for evaluation. Eighty-two subjects (37 percent) reported dreaming every night and 105 (47 percent) not so frequently, but at least once a week. Thus the frequency of dream recall was considerably higher than has been reported in previous research. Analysis of variance showed that these three groups did not differ significantly in their ESP scoring ($F = 2.59$, $df = 2, 220$). The overall ESP scores showed nonsignificant psi-missing. Those who reported dreaming at least once a week but not every night obtained significant negative scoring ($CR = 2.37$; $P < .05$, two-tailed).

Forty-nine percent of the subjects reported having had precognitive dreams, four percent of them frequently. The three groups formed by this variable ("often," "sometimes," "never") did not differ significantly in their performance on the ESP test. A highly significant positive relationship ($P <$

.001) was found between the reported frequency of dream recall and the reported frequency of precognitive dreams. There was no significant sheep-goat effect, but the scores were in the expected direction. Those who believed that the existence of psi was certain (20 percent) obtained a mean score of 9.86, those who believed it was possible (70 percent) obtained a mean score of 9.74, and those who did not believe in the possibility of the existence of psi (ten percent) obtained a mean score of 9.27.

A MULTIPLE SESSION GANZFELD STUDY

James C. Terry (Maimonides Medical Center)

Honorton and Harper at the 1973 Parapsychological Association convention reported [RIP 1973, pp. 52-3] significant results in a free response experiment involving the regulation of subjects' auditory and visual perceptual input. A homogeneous visual field ("Ganzfeld") was used to regulate visual input, and a monotonous seashore recording played to the subject through earphones served to regulate auditory input. The present experiment involves a continuation of work with this design. The major difference is that in the present experiment each subject-agent pair completed 10 sessions, at approximately weekly intervals, rather than a single session, as was used by Honorton and Harper.

The subjects were self-selected volunteers and the agents were either friends of the subjects or acquaintances drawn from the Maimonides staff. The experimental plan called for 10 subject-agent pairs to complete 10 sessions each, following the procedures used by Honorton and Harper. Six of the 10 pairs completed their 10 sessions, but the others did not. The data reported here include only those pairs who completed all 10 sessions.

At the beginning of each session, the subject was taken into a sound-attenuated room and halves of ping-pong balls were placed over his or her eyes. A light source was placed in front of the subject's face so as to produce a diffuse unpatterned visual field or Ganzfeld. Earphones were placed over the subject's ears and a white noise recording was adjusted to a comfortable level. The subject was then

given instructions to "think out loud," describing all imagery, thoughts, associations, and so on, for the duration of the session (30 minutes). These instructions were identical to those used by Honorton and Harper.

Once the subject was safely enclosed in the sound-attenuated room, the agent was taken into a second, non-adjacent experimental room, where (s)he remained until the end of the session. The target material consisted of thematically-related View-Master slide reels. The specific target for a given session was selected randomly by shuffling a deck of 31 numbered cards corresponding to the number of target packets. Each packet contained four thematically heterogeneous slide reels. The uppermost reel of the selected packet served as target for the session. The "sending period" was then randomly selected (first, second, or third 10-minute period). At the end of the "sending period," the agent replaced the target reel in the packet and shuffled it with the three other reels in the packet. The experimenter tape-recorded the subject's continuous report during the session and took detailed notes.

At the end of the session, the experimenter obtained the target packet from the agent without exchanging words. He further shuffled the reels in the packet, then went into the subject's room, reviewed the imagery report during the session, then left the subject with the four target reels with instructions to rank them from one to four according to their similarities with his or her mentation. When the subject completed blind-ranking the target material, the experimenter obtained the identity of the actual target from the agent and gave the subject feedback as to the success or failure of the session.

The results for the six completed subjects, combined by chi square, are significant (chi square = 15.45, df = 6; P = .017). One of the six subject-agent teams obtained independently significant results with seven direct hits, where mean chance expectation was 2.5 hits ($z = 2.92$; $P = .0035$). The overall scoring rate of 45 percent compares favorably with Honorton and Harper's results in which the overall scoring rate was 43.3 percent. Chance expectation would be 25 percent. Four of the six completed subjects in the present study obtained scores which were double the chance expectancy value.

AN ESP TEST WITH PSYCHOMETRIC OBJECTS AND THE GANZFELD: NEGATIVE FINDINGS

John Palmer† and Isabelle Aued (University of Virginia)

The present experiment was basically a replication of the Honorton and Harper study reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 52-3], which used a Ganzfeld technique in an effort to facilitate psi receptivity. An exploratory aspect of the experiment dealt with whether the subject's use of a psychometric object would relate positively to ESP performance. The subjects were 40 volunteers tested individually, 11 women and 29 men. The majority were students or former students from the University of Virginia.

The subject was first shown the room and table upon which would be placed the target picture. He was then seated in a reclining chair in an adjacent room and asked to relax as much as possible. The experimenter (I.A.) then handed the subject a set of earphones to put on. Halved ping-pong balls were fitted over the subject's eyes. I.A. then switched on a white light in front of the subject's face, thereby creating a uniform white visual field. The subject was instructed to relax for a few minutes while I.A. left the room to get the target picture.

The target material included ten target sets, each set consisting of five magazine pictures. J.P. selected both the set and the picture within the set that was to be the target by referring to a random number table. He also assigned subjects randomly to the experimental and control conditions, with the restriction that 20 subjects be assigned to each condition.

As soon as the subject arrived for the experiment and was taken to the lab by I.A., J.P. (the agent), who was seated in his closed office, would concentrate on the target picture for three minutes. If the subject was to be in the experimental condition, he would also hold a ball of clay (the psychometric object) in his hands and roll it over the picture while concentrating on it. He then placed the picture in an opaque manila envelope, went to the hallway, and handed the envelope (along with the psychometric object, if applicable) to I.A. This was the first time that I.A., who had recruited the subjects, knew which condition the subject was to be in.

Upon returning to the lab, I.A. informed the subject that the target picture had now been placed on the table in the outer room and the instruction tape was about to start. In both conditions, the first part of the taped instructions was identical to that used by Honorton and Harper. The subject was asked to report all the images, thoughts, and feelings which passed through his mind, and to suggest to himself that the image of the target picture would appear in his consciousness at some time during the session. For the experimental subjects, the tape continued with suggestions that the psychometric object would help them to gain impressions of the target picture.

These instructions were followed by a 20-minute period of white noise on the tape. During this period, I.A. wrote down the subject's verbal report of his impressions. The subject was then given a six-item questionnaire which asked about his level of physical and mental relaxation, vividness of visual imagery, effort exerted to get imagery, and expectancy of success in identifying the target. For the experimental subjects two extra items were included asking how they felt the object affected their ability to gain impressions of the target picture and about their attitude toward the possibility of gaining psychic impressions from psychometric objects.

The subject was then given a questionnaire modified from one designed by Stanford to test ways in which the physical world might become known or meaningful to a person. While the subject filled out the questionnaires, I.A. took the target picture envelope to J.P.'s office and left. J.P. removed the target picture and placed it among the four control pictures in alphabetical order, according to an identifying letter on its back. The set was then handed to I.A., who returned to the lab.

After reading his mentation report back to the subject, I.A. then placed the pictures in front of him in alphabetical order and asked him to rate each picture on a 31-point scale according to its similarity to his experience during the reception period. Upon the subject's completion of the ratings, I.A. obtained the letter of the target picture from J.P. so that the subject could have feedback.

The ESP score for each subject consisted of his rating on the target picture expressed as a standard score. The

means of these scores were -0.10 in the experimental condition and -0.13 in the control condition. These means did not differ significantly from mean chance expectation (zero) or from each other. The number of direct hits (occasions where the target was given the highest rating) was 3.5 in the experimental condition, and 3 in the control condition, also not significantly different from chance.

The only significant relationship to emerge in the whole study was a positive correlation ($r = +.51$; $P < .05$) in the experimental condition between the "physical world" questionnaire and the question, "What is your opinion about psychics' belief that it is possible to gain psychic or ESP impressions from objects by holding them?" That is, those subjects who, according to Stanford's scale, had the greatest tendency to relate to the physical world through the sense of touch were most likely to strongly believe that one can obtain psychic impressions from psychometric objects. No subjects in the experiment expressed a disbelief in this, but the "true believers" had a higher score on the "physical world" questionnaire than those with a more neutral attitude.

This experiment failed to show any effect of a psychometric object on ESP performance. More importantly, perhaps, the results from the control condition failed to provide a significant confirmation of the results of Honorton and Harper using a very similar procedure. There were a few small differences of procedure between the two experiments, however, which conceivably could have influenced the results. Subjects were exposed to the Ganzfeld for 35 minutes in Honorton and Harper's experiment, and only 20 minutes in our own. But Honorton informed J.P. that in his experiment there was no significant tendency for the relevance of the imagery to the target to improve during the latter half of his experiment.

In Honorton and Harper's experiment, the agent "sent" the target picture during the session, while in our experiment he sent it immediately before the session. We suspect this difference is also inconsequential, because Honorton and Harper found that most of the target-related imagery of their subjects did not occur during the sending period.

The most likely villain, in our judgment, is the social psychological factor. For example, in Honorton and Harper's experiment, the agent either knew the subjects or

talked with them before the experiment to develop rapport. In our experiment, the agent rarely even saw the subjects, and when he did it was only briefly. Any one of a number of other situational or experimenter variables could have differentially affected the results of the two experiments. Until we develop standardized methods for interacting with subjects in our experiments, consistent independent replications of parapsychological experiments with humans will continue to elude us.

PERCEPTUAL EXPERIMENTS ON OUT-OF-BODY EXPERIENCES

Karlis Osis (American Society for Psychical Research, New York)

Are out-of-body experiences (OOBEs) real in the sense that something does indeed leave the body, is externalized, and makes observations from another point in space? We have designed two optical instruments for testing whether subjects' perception during the OOBE is consistent with the externalization hypothesis: the Optical Device and the Color Wheel. They display randomly determined composite pictures through a viewing window. Part of the picture is an optical illusion that can be seen only from one position--directly in front of the window. Thus, to score well on the illusion part of the picture, the subject in a sense would have to be present at the viewing window in an OOBE way. However, if the stimulus material is seen as is (without the illusion), we would assume the perception to be the usual ESP. Both instruments are designed to test this hypothesis but are based on different optical principles.

The subject with whom we have worked most is Dr. Alexander Tanous. In a preliminary OOBE test over a distance, a psychic observer had reported seeing Tanous hovering over the target area and bent like a jackknife. Tanous's own report also said that he could not get down on the floor, that he was bending over, head down and legs down. Impressed with this coincidence, we chose him for intensive laboratory study.

The Optical Device is so constructed that some target

aspects can be easily reached by clairvoyance as we know it; other aspects involving optical illusions or transformations can be seen only from one viewpoint outside the apparatus, and presumably require localized sensing by the OOBE projection. The subject lies down in a soundproof room at the other end of the building from the apparatus and is asked to position himself, while having an OOBE, at the viewing window.

The usual method of scoring multiple-aspect targets was used: scoring each aspect separately and scoring their combinations. We had a history of Tanous's previous performance in ESP testing. His characteristic pattern was good initial positive scoring with a decline setting in rapidly. The same happened in the test with the Optical Device: his scores quickly reached significance, and then declined. However, Tanous did not give up. He was very determined to gain conscious control of the OOBE process, and he worked hard on gaining an introspective awareness of times when he was more "out" and his perception was more effective.

At first he did not succeed, but he persisted with dogged determination in comparing his introspections with the test scores (he was told only the scores, not what the targets actually were), and eventually he hit upon criteria that seemed to work. He experiences himself to be without a body, something like being a spot of consciousness; he says it feels like light. When this light appears to be very concentrated, like a pinpoint, he feels that he will score well. The other criterion he uses is a feeling of oneness or unity with existence. After the session (before the scoring), he singled out the High-Confidence trials associated with these introspective signs.

On the High-Confidence trials Tanous approached significance on the color aspect of the target, which requires localized sensing ($P = .054$, one-tailed). The scores approached significance only in the later part of the experiment when he had mastered the test; therefore, confirmation was needed.

Next he tried the Color Wheel test. He produced the same scoring pattern as with the Optical Device test. As before, neither the overall scores nor the Low-Confidence scores were significant. The High-Confidence scores on the whole target were significant ($P = .04$), and they again improved as the subject worked through his struggle for intro-

spective awareness. In the second half of the experiment he scored significantly on several target aspects. The aspect that required localized sensing was significant at $P < .008$, one-tailed.

In summary, on the two optical systems Tanous's scores showed essentially the same sequential pattern. His results on the High-Confidence trials in the latter parts of both experiments are consistent with the OOBE hypothesis of localized viewing.

FURTHER OOBE EXPERIMENTS WITH A CAT AS DETECTOR

W. G. Roll†, Robert L. Morris, Blue Harary, Roger Wells, and John Hartwell (Psychical Research Foundation)

At the 1973 Parapsychological Association convention, R. L. M. described [RIP 1973, pp. 114-6] a series of experiments in which the behavior of a kitten changed (became more calm) when B. H. had the experience of visiting it during out-of-body experiences (OOBEs). In the present experiments we attempted to determine if the kitten, now grown into a cat, would orient toward a particular location if B. H. confined his OOBE to that location. Two short series of experiments were done. In the first, R. L. M. was the main experimenter; in the second, W. G. R. was.

In the first series the cat was placed in a circular container, built by Graham Watkins, two feet high and two feet and ten inches in diameter. The floor was divided into eight pie-shaped wedges, each of which activated a timer clock in an adjoining room when a weight pressed down on it. Thus, we could tell how much time the cat spent on each wedge during any observation period by counting the elapsed time on each of the eight timer clocks. This apparatus was located in one of the Psychical Research Foundation buildings. The room from which B. H. "launched" his OOBEs was Dr. Larry Thompson's laboratory at Duke University Medical Center, approximately one-fourth of a mile away. B. H.'s task was not to "visit" the cats directly as before, but rather to "visit" one of four locations about ten feet away from the apparatus but in the same building. The locations were cho-

sen to be roughly north, south, east, and west of the apparatus. Seven sessions of four observation periods each were conducted, two OOBE (experimental) and two non-OOBE (control). Unfortunately, the animal habituated fairly quickly to the apparatus and in later sessions simply sat wherever it was placed. There was no difference in activity level during OOBE periods versus non-OOBE periods.

On four occasions during OOBEs the cat activated five or more timer clocks; otherwise, the cat activated no more than two, indicating that it was essentially still. On the four active occasions, the cat tended strongly to spend time on the wedge closest to B.H.'s location ($P = .03$). Although the apparatus was not satisfactory, the results were compatible with the hypothesis that the cat functioned as a detector of a spatio-temporally discrete aspect of B.H.'s out-of-body experiences.

In an attempt to avoid the habituation problem, we next used a ten foot by ten foot isolation chamber at J.H.'s laboratory at Duke University. This was divided into four main areas of equal size. The procedure was as before, with two OOBE and two non-OOBE periods per session. This time, B.H. "launched" from one of the Psychical Research Foundation buildings about half a mile away. Prior to each trial, the cat was placed in a container at the center of the room. When the trial began, this container was removed by means of a cord from outside the room. The cat was observed by closed-circuit TV, the lens of the camera being focused on a convex circular mirror which made it possible to see the whole room.

We were able to conduct three sessions with four observation periods per session. The cat did not show overt evidence of responding either to B.H.'s presence or to his location through its gross spatial orientation. However, J.H., who was monitoring the TV, on four occasions had the impression that B.H. was in the room. Each time he was correct. At no time did he respond when B.H. was not present (chi square = 4.33, df = 1; $P < .05$, corrected for continuity). On one of these occasions J.H. actually was convinced that he saw an image of B.H. on the TV screen. This image was in the section of the room where B.H. was attempting to "visit" at the time. The cat was in a different section. No further sessions could be conducted since the room and TV system had to be used for other purposes.

CONFIRMATION OF A FAMILY'S REPORT OF AN APPARITION

Michaeleen Maher† and Gertrude Schmeidler (City College, CUNY)

Both members of a two-person family reported that on separate evenings, independently of each other, each had seen someone going away from her, and turning around the corner in their hallway. When investigation showed no one was there, and they could not account for what they saw in any normal way, they wondered if they had seen a "ghost." They reported it to M.M., a family friend, and agreed to let her study it.

The study followed the design of earlier quantitative investigations of haunted houses. A floor plan of the 11-room apartment was drawn, and divided into 20 comparable units. Four adjacent units were designated as the apparition's "habitat," on the basis of the family's reports. The family's description of the figure showed eight items of clothing or movement on which both family members agreed. A checklist was prepared which interspersed these items with 29 others selected by the experimenters which seemed comparable but that both family members agreed were erroneous.

It was decided to obtain data from "psychics" (persons professing to have a high degree of sensitivity to paranormal phenomena, who believed themselves capable of seeing a ghost), and also from a control group of "skeptics" (persons who believed there were no ghosts or else who believed that if there were any, they themselves could not sense one). All subjects toured the apartment singly, at a time when family members were absent or stayed behind a closed door, in a room which was not on the "tour" prescribed by the experimenter's (M.M.) instructions. A co-experimenter, ignorant of the family's description, gave the instructions, a copy of the floor plan, and a copy of the checklist to the psychics and to four of the control subjects. M.M. served this function for the other four control subjects.

Psychics were instructed to record their impressions of the ghost on the floor plan and checklist. Skeptics were instructed to fill in the floor plan and checklist "as if" there were actually a ghost ("most likely" location and description

of a ghost "if indeed" there was one).

Four psychics toured the apartment, but two were unable to fill in the checklist. We thus had six measures from psychics. Two showed significant correspondence to the family's reports: Mr. Ingo Swann on the checklist ($P = .028$), and Miss Phyllis Woodbury on the floor plan ($P = .032$). Eight skeptics toured the apartment, but three were unable to fill in the checklist as instructed. There were thus a total of 13 measures from skeptics. None was significant.

Other data make the statistical findings more interesting. Mr. Ingo Swann, for example, reported seeing the ghost coming toward him, in a location adjacent to and just beyond where the family had seen it going in the opposite direction. Though this counts as a statistical failure, it is qualitatively consistent with the family's reports.

The null data from control subjects indicate that correspondence between psychics and family was not due to preconceptions of ghostly habitat or personality, triggered by illumination, room furnishings, and so on. This seems to leave three major possibilities for interpreting the significant results of the two psychics: ESP of the family's reports, responses to a ghost, or responses to an apparition created by a living person. The present method therefore seems satisfactory only as a first step in studying purported apparitions. We need a more incisive research design to help us decide among these three interpretations.

PSYCHOKINETIC INFLUENCES ON AN ELECTROMECHANICAL RANDOM NUMBER GENERATOR DURING EVOCATION OF "LEFT-HEMISPHERIC" VS. "RIGHT-HEMISPHERIC" FUNCTIONING

Ken Andrew (University of Houston)

In an earlier paper at this convention, Braud and Braud described two syndromes which they believe to be conducive and antagonistic, respectively, to receptive psi (that is, extrasensory perception). Certain of the characteristics of their "Psi Conducive Syndrome" resemble very

closely the characteristics of the functioning of man's right cerebral hemisphere. Conversely, certain "Psi Antagonistic Syndrome" characteristics closely resemble those of left-hemispheric functioning. They suggested that a psi-conducive receptive, nonanalytical, noninterpretive set might be induced by evoking "right-hemispheric" functioning through the use of music, natural environmental sounds, and other sounds suggesting depth and imagery. A psi-antagonistic active, analytical, interpretive set might be induced by evoking "left-hemispheric" functioning through the use of tasks involving linguistic, mathematical, logical, temporal, abstract, and analytical skills. In the same paper the Brauds described free response GESP tests conducted immediately after evocation of "left" versus "right" hemisphere functioning via tape recordings.

The purpose of the present experiment was to determine the effects of the "left" and "right" hemisphere tapes upon active psi; that is, psychokinesis. Psychokinetic influences upon an electromechanical random number generator were measured while subjects were listening to the two tapes. Two groups of ten college students each served as subjects. Both groups began the experiment by listening to a brief, ten-minute version of a progressive muscular relaxation tape. The "left" group subjects than listened to and performed mental operations on the contents of a 23-minute tape including: counting the letters in words, mathematical problems, mentally diagramming sentences, solving various problems involving logic and reasoning, analogies, time-estimation, and excerpts of readings on linguistic philosophy, constitutional law, physics, vocabulary, and blank verse. While listening to this "analytical" tape, the subjects were asked to attempt to influence a random number generator. The second group of ten subjects attempted to influence the generator while listening to a 23-minute "right" hemisphere tape including: music, natural environmental sounds, and electronically synthesized sounds suggesting depth and imagery. It was hoped that this tape would evoke a receptive, nonanalytical, noninterpretive set.

The generator was an apparatus consisting of a high-speed electric motor and an electromechanical timer. While the timer timed a period of five seconds plus or minus its tolerance factor, the motor rotated a commutator which resulted in eight lights rapidly flashing. At the end of the timed period, the contact disc rotated to a halt, finally stopping in a position which resulted in one of the eight lights re-

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

The psychokinetic procedure was identical for the two groups of subjects. The subject was asked to try to make one of the lights on one side come on for 25 trials, then try to make one of the lights on the other side come on for the next 25 trials. Half the subjects did 25 left then 25 right trials, and half did 25 right then 25 left trials. If the generator stopped on the target side (right or left), a hit was recorded. Mean chance expectation was one-half, or 25 correct out of the total 50. The light coming on provided immediate feedback of success or failure.

The subjects were run individually in a light- and sound-proof room, while seated in a reclining chair, tilted slightly back from the vertical. The only light was provided by the target lamps and the only sound was provided by the tapes and the experimenter's signal to switch sides halfway through the session. The experimenter, in another room, initiated trials and recorded the stopping position of the light at his own console. The experimenter was aware of the subject's condition.

Subjects were classified as hitters if they scored above chance, or missers if they scored below chance. The results indicated significant psi-hitting in the "right" group (binomial $P = .020$), significant psi-missing in the "left" group (binomial $P = .011$), and a significant difference between groups (Mann-Whitney $U = 8.5$; $P < .002$, two-tailed). Two confirmatory experiments are now under way. In one of these, an experimenter naive about the purpose of the study is running 20 "right" and 20 "left" subjects. In the second, another experimenter familiar with all aspects of the study is also running 40 subjects. Also, the effectiveness of the right and left hemisphere tapes in actually activating the respective hemispheres is currently being assessed using

bilateral EEG recordings. The influence of the tapes on frontalis EMG and forehead-finger temperature differential is also being measured in separate studies.

The results of this study suggest that a "right-hemisphere functioning," receptive, nonanalytical, noninterpretive state may be conducive to PK; while a "left-hemisphere functioning," active, analytical, interpretive state may be antagonistic to PK. Other work in our lab, employing identical tapes, indicates that the tapes do not result in different degrees of physical and mental relaxation, when these latter are self-rated by the subjects. Thus, these results would not appear to be confounded by tension level differences. Since the experimenter was aware of the subject's condition, his own possible PK effect on the target may confound these results.

AN EFFECT OF RELEASE OF EFFORT IN A PSYCHOKINETIC TASK

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

The objectives of this study were to determine whether a particularly strong PK effect would occur on a target system immediately after cessation of effort to produce an effect (as contrasted with any PK effect shown during the period of effort), and to examine under what conditions any such effect would be maximal. The hypotheses were: first, that such an effect occurs generally; and second, that this effect is maximal on trials in which persons perceive themselves as failing whereas on earlier trials they had thought their efforts successful (that is, a "success-followed-by-nonsuccess" condition).

Thirty-six unpaid volunteer subjects were tested, mostly of college age and representing both sexes. Each subject attempted to influence the electrical resistance of a light-stimulated photocell housed in a light-tight box, during successive 10-second intervals spaced 70 seconds apart. Each subject did 30 such trials. The changes in the photocell were continuously registered by a Grass Model 70 polygraph recording in the GSR (electrical resistance) mode.

Twelve subjects were tested in each of three experimental conditions: (a) no "feedback"; (b) noncontingent (bogus) feedback indicating to the subjects that they were showing a moderate but increasing level of success (first 21 trials) followed by constant success (trials 22-26) and then by constant failure (trials 27-30); and (c) noncontingent feedback similar to (b), but with perceived failure on trials 22-26 and continued perceived failure on trials 27-30.

For conditions (b) and (c) the noncontingent (bogus) feedback was delivered by variations in the frequency of an auditory stimulus which signaled the trial (effort) period, with increases in pitch signaling "success" to the subject. In all three conditions subjects were instructed to cease concentration on the target box (housed in another room) as soon as the tone signaling the trial had ceased. To help insure distraction from the task during the between-trial (between-effort) intervals when the tone was off, the subject read aloud from a magazine furnished him by the experimenter. During the sequence of 30 trials, the experimenter sat with the polygraph and the target box in a separate room from the subject.

When all subjects had been tested, a map measure was used to measure the length of the readout line (reflecting changes in electrical resistance). The measured length of the readout line during a given time interval (for example, during an effort period) was divided by the length of the polygraph chart for the same time interval, and this supplied the basic dependent measure of the study. For each trial the chart was measured for the 10 seconds of effort and for 20 post-effort seconds. The dependent measure was taken only for trials 22-30. The person making the measurement was blind as to which of the three "feedback" conditions a subject was tested under, but it was impossible to make him blind as to whether he was measuring an effort or a non-effort period. The measurement, however, seemed straightforward.

Each of the three experimental conditions independently showed significantly greater activity during the release-of-effort period than during the effort period, as measured by Sandler's A test, a computationally simpler exact equivalent of the t-test for correlated data. For the non-"feedback" condition, the mean difference for non-effort versus effort was 7.2 percent ($P < .02$); for the success-followed-by-non-success condition the difference was 3.3 percent ($P < .001$);

for nonsuccess-followed-by-nonsuccess the difference was 2.7 percent ($P < .05$). The disproportionality of P-values and mean differences is due to differences in variability of difference scores in the three conditions. The results thus confirm the hypothesis regarding the occurrence of a release-of-effort effect.

The second hypothesis failed to be confirmed, for there was no superiority of the success-followed-by-nonsuccess condition over either of the other conditions for the crucial trials (27-30). For those trials, there is a significant effect of the three experimental conditions (as measured by analysis of variance), but it is contributed largely by the superior release-of-effort effect in the non-"feedback" condition as contrasted with the other conditions.

Further work seems warranted on the release-of-effort hypothesis and the psychological conditions of its occurrence. The outcomes of this study fit in well with the so-called "lag" effect noted by Pratt and Woodruff in a portion of their PK work with dice. Such an effect would seem to be of considerable interest and importance, if it is real.

A SCRUTINY OF URI GELLER

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

The purpose of this scrutiny of the static-PK claims of Mr. Uri Geller was to supplement the findings reported at the 1973 Parapsychological Association convention by Targ and Puthoff [RIP 1973, pp. 57-60 and 125-8]. Their most significant results with Geller were found in ESP experiments, since only a small portion of their tests for PK on stable systems were under reportedly satisfactory conditions of control.

Through the good offices of Mrs. Judith Skutch, I was able to spend an hour alone with Geller on April 24, 1974, in his New York apartment. My hope was to witness static-PK under what could be deemed adequate safeguards. I also wished to allow certain openings for fraud, in the event he intended to employ such means. He was told that I contributed to the research work of the FRNM, but not that I was also a magician.

Three tests of Geller's static-PK ability were made. For these I had purchased two keys of specific types, plus an internal obstruction for my pocket watch. The first test was with a flat steel key of the safety deposit box type, which I was quite unable to bend by hand. I intentionally allowed Geller to handle it, whereupon he asked if I did not have an ordinary key instead. I said "No," and he replied, "Well, I'll try to do something with this one." He laid my key on his transparent, glass-topped coffee table, and stood up between his sofa and the table. I sat at one end of the table. We both noted the absolute flatness of the key upon the glass. I placed my right forefinger lightly on the larger end, and Geller gently stroked the remainder with his right forefinger. The key began to bend slowly at a point just beyond my finger. He ceased stroking when it reached an angle of five or six degrees. Any pressure of his naturally was against the direction of bend. I removed my finger, and watched him rock the bent key upon the glass.

By this time I had obtained a mirror with my left hand, and again held the same key as before. Geller stroked it again, and it was seen gradually to bend to a total of 12 1/4 degrees. His stroking was light, since it caused no upward increase in pressure against my forefinger. All of this took approximately a minute. With the mirror I could see very clearly under the key. My sight distance was about 15 inches. No semblance of fraud was detected. The strength required to bend this key represented an upward force of 40 pounds at Geller's end, and about 100 pounds at my end, as was subsequently determined with a similar key and weighing scales.

Another key was similarly bent later in the hour. This was of the ordinary three-inch skeleton variety, made of a soft zinc alloy. As before, I handed it to Geller, and he returned it to me in a perfectly straight condition. The key was placed on the coffee table, my forefinger pressed against the toothed end (which naturally elevated the handle end an eighth-inch or more above the glass). Geller stroked only an inch of the handle end's upper surface, and this time a bend slowly appeared near that end, a full inch away from my finger. It conspicuously continued until it reached 36 degrees. Again, there was no noticeable pressure upward against my finger, and the time required was less than a minute.

The final experiment involved my Hamilton pocket

watch. It was handed to Geller, with its chain. (I had proposed this test earlier by telephone, having said only that I was going to alter the speed regulator and challenge him to readjust it "so that the watch would run again.") He expressed uncertainty, held it to his ear, did not shake it unduly, kept it clearly within my view, then again listened, and ejaculated "It's ticking, it's ticking!"

This watch had been specially prepared in advance as follows: A strip of tinfoil, an inch long and at least a sixteenth of an inch wide, was inserted upon the balance wheel bridge, but beneath the speed regulator arm, extending one-fourth inch over the balance wheel, with the arm set at "F." The remaining part of the strip was folded out of the way, and allowed to extend down from the "F" toward the stem. The back had two lids, one extremely difficult to open. Ten minutes before my session with Geller I depressed the shorter portion well into the spokes of the wheel, and thereby stopped it.

Following Geller's accomplishment, I retrieved the watch and listened. On opening the back, we found the regulator arm to have been moved to slightly beyond "S," despite its normal tight fit. This had lifted the shorter end of foil up from within the wheel. The remaining three-fourths inch of tinfoil was separated, and had itself been moved to where its nearer end was half an inch farther away from the "F---S" section than when I prepared it, and had been turned 90 degrees clockwise as well. It was in fact stuck slightly, since there was some candy-wrapper wax on the underside of the strip of foil.

This dual effect within my watch was even more impressive to me than the keys. Due to the permanent removal of a delimiting set-screw, the regulator arm could be pushed as much as 40 degrees; and it was found to have moved to this full extent. Geller himself was visibly surprised by the unexpected feature, and summoned an associate to examine this effect.

If I may be permitted to insert the following personal notes, I have been a student of both psychical research and sleight of hand for 40 years, and confidently believe that deception may be ruled out as the explanation of these effects. Other magicians whom I have consulted have not altered this opinion, but criticism of the same will be welcomed.

Due to the simple and conspicuous movements in all three of the static-PK effects, I have no special regrets about the absence of any assistant observer. For psychological reasons I did not solicit a movie photographer and, in fact, turned down an offer by a fellow magician to accompany me. My firm conclusion now is that increased research interest in Geller is warranted.

NINA S. KULAGINA: A STRONG CASE FOR PK INVOLVING DIRECTLY OBSERVABLE MOVEMENTS OF OBJECTS RECORDED ON CINE FILM

H. H. J. Keil† (University of Tasmania) and Jarl Fahler (Helsinki, Finland)

Reports at the 1973 Parapsychological Association convention [RIP 1973, pp. 54-7 and 121-5] suggest that Nina Kulagina possesses the ability to move static objects by PK with a fair degree of control. We visited her in Russia for the purpose of making further observations and recording the phenomena on movie film. The object movements reported here are shown on the film we made. We had brought along the following objects: (1) A clear plastic cube (about 10 cm. on each side) containing a nonmagnetic cylinder resting on the bottom surface and fastened to one side of the cube with a very weak thin expansion spiral steel spring. The spring was nevertheless strong enough to restrain the cylinder to a certain range of positions. Kulagina had moved this cylinder by itself previously. The cube had one open side, which was usually at the top. (2) A similar clear plastic cube with one open side, which had a table tennis ball suspended (with a similar steel spring) from the center of the top, and about 15 mm. from the floor. (3) A compass weighing approximately 35 grams.

Throughout the demonstration J.F. was able to watch Kulagina without any obstruction or diversion of his attention. He remained within a distance of 70 to 100 cm. from the objects. H.H.J.K. was to some extent occupied with taking movie films, and was not always able to observe all phenomena. All demonstrations were carried out under good illumination conditions. We observed the following movements

of our own and other objects: (1) An inverted glass about 65 mm. by 35 mm. large had been placed over a green wooden object; both were inside a larger inverted wine glass. The smaller glass moved approximately 20 mm., from one edge of the larger glass to the other. The direction of movement was towards Kulagina, with one short interruption approximately halfway. The compass was on the line of movement between the glasses and Kulagina. It showed no movements of the sort one might expect if an invisible string had been used. The glasses were about 25 to 30 cm. from the edge of the table. No movement of the green round wooden object relative to the smaller glass was noticed. The green object was resting against the end wall of the small glass, such that as soon as the small glass moved, the green object moved as well. If it were argued that the green object had a steel interior and that it could be moved with a strong magnet from underneath the table, then it was precisely in the wrong position to produce the observed movement of the glass.

(2) The whole compass housing, situated almost at the edge of the table, made several turning movements in a counterclockwise direction, turning approximately 70 degrees and also sliding perhaps 15 mm. closer to Kulagina.

(3) The table tennis ball in the cube first moved down and then towards Kulagina (the open side of the cube being on Kulagina's side and the cube being situated close to the edge of the table). This movement was slow and can in no way be explained by oscillations. After a short period of resting on the plexiglass surface directly beneath the point of attachment of the spring, the ball moved a very short distance (approximately 5 mm.) towards Kulagina and then made two or three vertical movements without quite returning to its normal suspension position. The ball then made contact again with the floor of the surface and continued to slide on this surface towards one side of Kulagina until it reached almost the edge of the cube.

It must be kept in mind that the ball is normally suspended about 20 mm. above the bottom surface. Consequently, even when it appeared to rest on this surface a force was necessary to keep it there, because the table tennis ball did not return to its normal position. The spring was considerably expanded when the ball, still in contact with the floor of the surface, was finally moved almost to the edge of this surface. As soon as the force holding the ball was released

the spring jerked the ball back with a quick movement.

(4) The small green object and the small inverted glass discussed above again moved. The green object was situated approximately 25 cm. from Kulagina, and moved in a direction towards her. In the line of movement was the small glass, the nearest edge being approximately 4 cm. away from the green object; and further in the line of movement was the cube containing the cylinder. The nearest edge of the cube was approximately 15 mm. from the small glass. The green object moved in a fairly straight line towards the small glass (that is, towards Kulagina). After the green object touched the glass they both moved together 15 mm. further until the glass touched the cube. There was no noticeable movement of the glass or of the cube while the green object was moving by itself, and there was no noticeable movement of the cube when both the green object and the glass moved together.

The total movement occurred in several stages consisting of three to five more or less clearly separated jerks. The way the movement of the green object together with the glass occurred, it is possible the glass was pushed along on account of the green object and not moving on its own. It could be argued that this movement could have been produced with a magnet from underneath the table if the green object contained an iron center. While this possibility is mentioned, there was not the slightest indication that anything like that took place. A very substantial magnetic force would have been required to act in this way from underneath the table.

(5) The same small inverted glass moved along inside the inverted wine glass. The situation was essentially the same as in movement (1) above, except that the green object was not present. Therefore, magnetism can be ruled out with certainty.

While no field study even under excellent conditions can be regarded as absolute proof, the observations of some of the details discussed as well as our overall impressions of the total performance gave both of us very strong confidence in the observed phenomena as genuine occurrences of PK. It now seems desirable to conduct research with Kulagina in a somewhat different manner. Up to now a major consideration has always been to insure that the movements were genuinely of paranormal origin. While future work should also try to maintain controls to the extent that it is

possible to do so, it seems reasonable to assume that the movements produced by Kulagina are indeed based on PK. Consequently it seems desirable in the future to test the dimensions of PK as well as other theoretical aspects for which a suitable test can be devised.

For this purpose it would be desirable if all scientists who are likely to visit Kulagina in the future could keep in touch with each other and with anybody else who is interested in the explanation of PK and related theoretical considerations. To find an important question that could be answered under the limitations that exist during such a field study is probably as important as the field study itself. Moreover, it seems highly desirable to present Kulagina with a motivating challenge rather than with the request to produce the same phenomena once more.

"MINI-GELLER" PK CASES

H. H. J. Keil† (University of Tasmania) and Scott Hill (University of Copenhagen and Danish Society for Psychical Research)

The mass media as well as the masses in Europe responded to the Geller performances far more than in the U.S.A. Uri Geller made headlines in Europe to an extent that newspapers in the U.S.A. would find difficult to approach even with Watergate material. It is impossible to say whether public interest was triggered by the more extensive coverage through mass media or vice versa. There is little doubt that Geller made a considerable impact on the European population which will have its repercussions on parapsychology whether we like it or not. In the June 1974 issue of Popular Photography the professional magician and longtime critic of parapsychology James Randi, together with C. Reynolds, made serious allegations which suggest that some of the phenomena produced by Geller may be based on trickery. Psychology Today in July 1974 published an article by A. Weil which also suggested trickery as an explanation which ought to be seriously considered.

We have heard during this convention one paper (by Cox) which reports a positive PK result in a Geller investi-

gation. Although it would be foolish to assume that a public performer may not use opportunities to obtain results by trickery, this should not rule out the possibility that some of his results are genuine. We should keep in mind that evidence for directly observable PK has increased in recent years, and we should welcome any careful observations which might add a clue to the PK puzzle. Nevertheless, there is the problem that Geller appears not to be interested in scientific investigations, and many claims and reports are likely to remain controversial.

While Geller himself remains a difficult subject to pin down for long controlled experiments, he may have provided us with an unusual opportunity to find new cases which will allow us to study directly observable PK to a far greater extent than has been possible during the last six decades or so. This is due to his ability as a showman to enchant mass audiences to such an extent that they are willing to overcome their inhibitions and at least try participating in mass PK experiments. Thousands in Europe reported success in bending spoons in the Geller fashion by paranormal means.

Obviously, after the event it is difficult to ascertain whether spoons were bent paranormally or by normal means, with or without the awareness of the subjects concerned. Given, however, that several thousand people reported success, it seems unlikely to us that all of them should have obtained these results by normal means. H.H.J.K. had an opportunity to visit a number of cases in the Freiburg region of West Germany which were investigated by the Freiburg Institute, and S.H. followed up a number of cases in Scandinavia. A few points emerged from these investigations. A substantial number of the spoon cases are real rather than imaginary. That is, spoons were really bent, often more than 90 degrees; they had not just been previously slightly bent from normal use, and then under excited examination found to be bent.

It was evident to the investigators that many people believed the spoons bent on account of Geller, and that they themselves only provided some sort of help for him to tune in. Consequently they did not expect to be able to bend them by themselves. This seems an important point. In the first place, inhibitions were effectively removed by their belief that Uri Geller was capable of PK, and by the suggestion that they provide only some minor assistance in enabling him to perform his PK feat at their homes. In fact, probably

several steps are required to enable most of these subjects to perform on their own. Obviously, video tapes of Geller encouraging them to go on with his help would be most desirable in future experimentation with these people, perhaps ending with the suggestion that Geller's energy will remain effective with them for years.

Since the interest in Geller in Europe was and probably still is a mass movement in the general population, sociological and general psychological variables have been emphasized. The limited finances provided for a substantial survey which is being carried out by the Freiburg Institute restricted this study somewhat to the sociological aspects. Obviously success in finding new PK subjects might provide opportunities for more scientific investigations of directly observable PK. S.H. is trying to follow up a number of cases, and a few seem promising.

FURTHER INVESTIGATION OF PK WITH TEMPERATURE RECORDS

Gertrude Schmeidler†, Janet Mitchell, and Nancy Sondow
(City College, City University of New York)

This is an extension of the research on continuous monitoring of PK effects reported by G.S. at the 1972 Parapsychological Association convention [RIP 1972, pp. 63-5]. In that study a gifted subject showed marked ability to change temperature recordings in the desired direction. The present study represents an attempt to study this phenomenon in a general population. Subjects tried to change the temperature of an insulated thermistor, sealed into a thermos. The thermistor was connected to a polygraph which made a continuous record of its temperature. Changes in the neighborhood of one-hundredth of a degree were scorable.

The research design was to have each session consist of two series, separately scored, with a recess between them. Each series was to consist of 16 periods of 45 seconds each, half test and half rest. In four test periods, the subject would be instructed to make the thermistor hotter, and in four to make it colder. Each test period would be preceded by a rest period. Hotter and colder instructions

would be counterbalanced in an ABBABAAB pattern. In each session, one series would take A as "Hotter" and the other would take A as "Colder." Odd-numbered sessions would begin with A as "Hotter"; even-numbered ones with A as "Colder."

Six other thermistors would give supplementary information: one attached to the subject's inner wrist to monitor skin temperature; one covered with a transparent plastic case and attached to the target's thermos as a visual guide for the subject; and each of the others sealed into its own thermos and placed adjacent to or a few feet from the target, to monitor ambient changes and to serve thereby as controls for the target thermistor. Temperature records would be scored by a blind judge at five-second intervals. The significance of the difference between target changes in the hot and cold periods of each series would be evaluated by analysis of variance.

The subjects were interested volunteers, many of whom were recruited by J.M. because of their apparent psi ability. There were 23 subjects and 31 sessions (62 series). All subjects took the Hand test just before their first series. The Hand test is a projective measure designed to elicit the subject's dominant needs and response tendencies. Several series departed from the planned design. Some of these deviations were trivial, such as having a rest period last for 55 instead of 45 seconds, and their data seemed usable. Other deviations were major, and seemed to make the data unusable, such as having the polygraph pen not register temperature during a test period. We now consider that the usable data comprise 47 series, contributed by 22 subjects. Four had one usable series; 15 had two; two had four; and one had five.

Fourteen of these 47 series showed significant target changes between hot and cold periods; seven in the instructed direction and seven in the opposite direction. Eight were significant at $P < .001$; two were significant at $P < .01$; and four were significant at $P < .05$. Eleven subjects gave significant data; 11 (with one to four series each) did not. Our reading of the introspections did not find any differences between successful and unsuccessful subjects or series.

No predictions had been made about the Hand test. Post hoc analysis showed that subjects who were successful at producing extrachance results on the day of the Hand test

were characterized by more "active" responses, and fewer responses that were "passive," "tension," "crippled," "fear," or "refusal," whereas unsuccessful subjects had the opposite traits. The pattern has face validity, and a median test comparing successful and unsuccessful subjects shows it is significant at $P < .05$; it deserves further study.

Comparison with the gifted subject previously studied shows that the major difference in results was that his significant changes were all in the instructed direction, whereas in the present study only half were. The difference in correctness of direction for significant data was significant at $P < .03$. Two major similarities in the two studies were: sufficient significant data to show that this is a practicable means of studying PK; and evidence for specificity of the PK effect on the target location, since in both experiments the target thermistor often registered a long-term decrease in temperature while an adjacent thermistor registered a long-term increase, or vice versa.

AN ATTEMPTED CONFIRMATION OF THE RODENT ESP FINDINGS WITH POSITIVE REINFORCEMENT

Richard Broughton and Brian Millar† (University of Edinburgh)

In recent years considerable research on psi in animals has appeared in the literature. In the bulk of this reported work, however, only negative reinforcement (electric shock) had been used. Schouten did an experiment in which mice had to use psi to choose which of two levers would lead to positive reinforcement (drops of water), but obtained only marginally significant results. Last year in Edinburgh Adrian Parker conducted a positive reinforcement experiment with gerbils pressing one of two levers to obtain sunflower seeds, and achieved an overall scoring rate of 52.8 percent ($P = .004$). He found a success rate of 55 percent ($P = .022$) for those trials in which the animal's response did not appear to be caused by behavioral biases or previous stimulation (called "random behavior trials," or RBTs). Parker also found suggestions of possible differences in the psi ability of individual animals. We, therefore, undertook a replication with an experimental design which enabled us to examine both

individual differences in animals and a possible experimenter effect.

The Schouten and Parker studies seemed to indicate that animals could use psi in positive reinforcement situations; they also confirmed the random behavior effect found in the negative reinforcement experiments. However, the behavior of the animals in our experimental setup led us to suspect, on the basis of pilot trials, that the reward RBT was not the same as the shock RBT. The RBT criterion in the shock experiment sorts out the trials when the animal moved for no apparent reason on the assumption that psi may be operating then. In the reward experiment the animal has to move from the response key to another side of the cage to obtain the reward. Thus, in the shock experiments the RBT is an example of the animal overcoming static behavior, but in the reward situation it is an example of the animal overcoming an operant conditioning bias caused by the previous trial. Because of this problem, and also the fact that the RBT analysis is particularly susceptible to optional stopping by the animals unless the total number of RBTs is prespecified, we concerned ourselves chiefly with the overall scoring rate.

The gerbils had to work for their food by guessing which of two response keys would operate the feeding mechanism and let them take a seed. The correct key for each trial was determined by a random sequence previously coded on a paper tape which was read only after the guess was made. Five animals were used in the pilot, and four in the main experiment; they were given a fixed number of trials each day to a preset total. Experimenters were alternated daily to examine experimenter effect.

We ran a pilot study of 2400 trials and a main study of 3200 trials. Our results, unfortunately, did not amount to a confirmation of the previous work. The pilot yielded a scoring rate of only 50.46 percent, and the main experiment 50.44 percent, where 50 percent is chance expectation. The RBTs did not bail us out either, since we obtained a lower scoring rate of 47.38 percent for the pilot, and only a slightly higher scoring rate of 51.5 percent for the main experiment, neither of which was significant.

The experiment was designed to examine several variables, and suggestions emerged from the data analyses which may help in understanding the apparent lack of above-

chance scoring. They are, however, only suggestions, and we are reporting them in the hope that it may lead to possible corroboration in other experiments, successful or not.

In the pilot the data of one experimenter (B.M.) yielded a slightly below-chance scoring rate of 48.79 percent, and the data of the other (R.B.) a slightly above-chance rate of 52.12 percent. Neither the scores nor the difference between them was significant, but the effect continued in the same direction, though with less magnitude, in the main study with rates of 49.81 percent and 51.06 percent, respectively.

A two-way analysis of variance examining animal differences and daily variation on the pilot data yielded an effect of animal differences significant at the $P < .05$ level ($F = 3.184$, $df = 4, 20$), but no effect of days. The latter would have been expected if a simple experimenter effect was present. In the main study this finding was reversed with no effect for animal differences but one for days, significant at $P < .05$ ($F = 3.051$, $df = 7, 21$). To pursue this effect further, we performed post hoc two-way analyses of variance on the results obtained by each experimenter in the two studies. For experimenter B.M., the lower-scoring experimenter, there were no significant differences at all, but with R.B., the higher-scoring experimenter, the data yielded the same effects as the overall data. This would seem to suggest that most of the variance in both the animal differences and the day differences was concentrated in R.B.'s data.

Caution is necessary in interpreting these findings. They may only be chance fluctuations of the data. On the other hand, possibly psi was operating but subject to a complex type of experimenter effect, such that one experimenter increased (or the other decreased) the overall variance in the scoring by the animals. Perhaps one experimenter was "psicidal," killing off psi entirely, while the other allowed greater fluctuations of the animals' abilities in either a positive or a negative direction.

DISPLACEMENT EFFECTS IN THE RECORDS OF SUBJECT C.J. FROM THE UNIVERSITY OF COLORADO ESP EXPERIMENTS

J. G. Pratt† (University of Virginia), and Dorothy R. Martin and Frances P. Stribic (University of Colorado, Boulder)

In 1940 D.R.M. and F.P.S. published a comprehensive report of an extensive series of ESP tests conducted at the University of Colorado. After a Preliminary Series in which 39 students were tested individually using the "down through" (DT) technique, the project was continued with 13 subjects in the Main DT Series. Then one subject, C.J., was selected for further intensive research during his last two years at college. In all, he carried out a total of 3659 runs under a variety of testing conditions. The rate of success on the direct target averaged 6.80, where five is expected by chance, reaching an astronomical level of statistical significance. After the Preliminary Series, all work was done with the cards screened from the subject.

Using a method of analysis applied previously to the ESP records of Mrs. Gloria Stewart, the present study examines the C.J. data for forward (+1) and backward (-1) displacement. To avoid confusion with direct target effects, pairs of trials with one or two direct hits were not included in the analysis. Pairs of misses with duplicate calls or duplicate targets were also omitted, since they cannot show displacement hits. The remaining pairs of misses were evaluated for displacement by a chi-square test applied on the basis of the expected theoretical distribution of the number of pairs with no displacement, (+1) only, (-1) only, and both (+1) and (-1).

No significant displacement effects were found in the Preliminary Series, even though C.J. showed highly significant scoring on the direct target. Thereafter displacement scoring in the pairs of misses occurred during the next three series, but the pattern was different from series to series. In the Main DT Series, the principal effect was positive scoring on (-1); in the next (identified as DT Variations), there was a highly significant negative score on (+1); and in the last (Main UT Series, in which C.J. called "up through" the pack), the predominant effect was a negative score on (-1). The effect observed is at a high level of statistical significance in each series, with $P < 10^{-6}$ in each

instance. These results are more variable and more difficult to interpret than the displacement effects in the Stewart records, where the findings showed positive scoring on both (+1) and (-1) to about the same degree. Then, in the third and final year of working as an ESP subject, C.J. showed almost no displacement scoring even though he continued to succeed in hitting the direct target.

CHARGE BUILD-UP ON THE BODY AS A BASIS FOR THE "HUMAN AURA" AND CERTAIN PK EVENTS

Brad Stevens†, Larry Burton, and William Joines (Duke University and Psychical Research Foundation)

Bagnall describes the "human aura" as a light blue or gray glow surrounding the body extending an average of four inches from the body's surface. Recent reports claim that during PK events Russian scientists observed sparks emanating from the subject's fingertips. The fact that ionized air emits blue light suggests that the "aura" is electrical corona about the body. The sparks in PK events would then be merely an extension of corona to breakdown, and the movement of objects would be the result of electrostatic forces. These possibilities led us to investigate naturally-occurring electrical corona about the human body.

First we calculated the charge necessary to produce corona. Reports of the "aura" indicate that it is rather evenly distributed about the entire length of the finger. The index finger is roughly a one- to two-cm. diameter cylinder with a hemispherical tip. The tip portion may be reasonably well modeled by a one-half- to one-cm. radius sphere. For the sake of initial calculation, assume that all charge present on the finger resides on the surface and is uniformly distributed over the spherical model. Since occasions on which the "aura" has been reported did not specify that the hand was near a ground plane, the calculations of the electric field with respect to ground at infinity will suffice. Using standard field equations and the fact that the breakdown strength of air is approximately 3000 KV/M, the amount of charge buildup on the body necessary to produce corona may be calculated. These calculations yield a figure of 10^{-8} coulomb. Further calculations show that the corona would

be present up to a maximum of 4.8 cm. distant from the fingertip.

The question also arises, "What force can this charge exert on another object?" Typically, persons possessing static-object PK abilities move small objects up to 20 cm. distant. The force between two charged objects is given by Coulomb's law. We have calculated the amount of charge on the fingertip. When in the presence of the E-field set up by the charged sphere model, a dielectric object will have an equal but opposite charge induced. Hence we may calculate the force of attraction between the finger and object as a function of distance. By this calculation, one could just move an object with a total rolling frictional resistance of 0.43 mg. (impulse effect of hand motion could overcome any reasonable inertia) at a distance of 15 cm. if sufficient charge for a surface E-field of 3×10^6 V/M were present on his finger.

Now that we have considered the amount of charge necessary to cause corona about an idealized model of the finger, we should consider the physiological possibility of this charge being generated, and the effect that the presence of this charge would have on the ensuing physiological function of the body. It is commonplace for one to build up static charge on the body and then to observe its discharge in the form of a visible spark and sensation of pain. The fact that a spark occurs indicates the presence of an E-field in excess of 3×10^6 V/M. The fact that there is pain indicates an adverse effect on physiological functioning.

Since it is apparent (and the experimental evidence proves this) that enough charge can be accumulated by static means on the body to give rise to an E-field of 3×10^6 V/M, let us calculate the field distribution occurring as a result of this charge. We may then consider the feasibility of this charge being generated by physiological processes. In order to do this, we must know the net charge differences between the various parts of the body and the structure and electrical properties of the body.

If we model the finger as a layered cylindrical capacitor, we find that if charge sufficient to cause corona were present entirely on the surface of the body (that is, if the charge were produced by means other than static charge buildup), an electrical potential between the epidermis and the dermis of between 7.5 and 30 KV would have to be main-

tained. These values of potential differences taken across a 1.0 mm. thickness of skin with a dielectric constant of eight times that of free space yield field intensities of 1.5 to 6.0 x 10^6 V/M. Since one commonly finds electric fields of the order of 10^6 V/M in biological systems, these values appear at first to be reasonable.

There is a flaw, however, and that is the result of a semipermeable membrane in the skin. Studies with micropipets reveal that the bulk of the epidermal-dermal resistance lies in a two-micron thickness of the stratum disconjunctum or the germinating layer. This is due to a membrane structure which as a result of its selective permeability for sodium and chlorine ions acts as an ion pump. Thus the resistance between two surface points of the epidermis is largely due to the bulk resistance of the epidermis, yet the bulk resistance of the epidermis is negligible compared to the transmembrane resistance. More accurately then, we should use a thickness of two microns rather than one mm. in the field calculation. This yields fields of 0.75 to 3.0 x 10^9 V/M.

Typically maximum resting potentials of the membrane are 100 mV. Therefore the membrane routinely withstands a field of 5 x 10^4 V/M. Even allowing for ten times the normal required strength, the membrane could not support even the smaller electric field. We can now make two observations. There is a system available in the form of the membrane which can act as a charge pump for the physiological production of charge. In order for the body to sustain a surface E-field of 3 x 10^6 V/M, the body must be floating above ground by nearly 30,000 volts as the membrane cannot support the necessary field strength for the dermis to reside at ground potential.

For corona to exist about the body, the charge must either come from static sources, or the body must expel charge against the force of charge equilibrium in order to gain net charge with respect to ground. In the event of net charge on the body, the discharge would cause current flow through the skin. This current could then produce the sensation of electrical shock. The fact that the body could and does maintain potentials on the order of 100 millivolts with respect to ground offers interesting possibilities for microvolt electric field interaction between people and objects.

The object of our experiments was to verify or disprove the theoretical predictions outlined above. The results

showed two things. First, static charge is capable of producing visible corona on the body, and measured potentials on the fingertips agree well with predicted values. Second, experiments with a subject who is apparently capable of inducing physiological sensations in other people did not produce potentials great enough to cause corona. She and others, though, commonly built up static or other charge to increase their potential with respect to ground to a maximum of 50 volts.

For studies of static charge, a dry day was chosen for the experiment. Rubbing a wool shirt with an acrylic-lined coat, a subject would become statically charged. Once charged, movements by the subject could be readily detected at a distance of three meters. Potential differences of 30,000 volts with respect to ground were consistently recorded on the subject's fingertips. Upon touching a ground, a spark discharge was observed and the subject felt a sharp pain. When the procedure was repeated in a darkroom, visible corona was observed between spark discharges. PMT responses validated these observances.

In the study to determine whether or not the charge necessary for corona onset could be produced physiologically, the subject was grounded prior to all experiments. The probe was placed in close proximity (three cm.) to the subject's hands and potential at the probe was recorded. As mentioned previously, no potentials capable of producing corona were observed. However, though corona was not generated, some sensations induced by the subject in others correspond to sudden changes in the potential near the probe by about 2.5 to 5.0 volts. These results reinforce the prediction that corona around the body is usually not caused by physiological production of charge. However, the correlation of sensations to small potential changes suggests that there may be some validity to the idea of microvolt E-field interaction between people and objects.

We conclude that corona can exist around the human body due to static charge buildup. Upon discharge, this static charge disrupts the dermal-epidermal membrane potential and also causes the sensation of electric shock. It is very improbable that normal physiological processes can produce charge at the potentials necessary to cause corona onset. Interaction between people and objects as a result of the microvolt electric field of a person seems probable.

PRECOGNITION IN WATER-DEPRIVED RATS

James C. Terry (Maimonides Medical Center) and Susan A. Harris† (Mount Holyoke College, Mass.)

The purpose of this experiment was to test for rodent precognition using positive reinforcement. The subjects were five water-deprived Wistar rats (Rattus norvegicus) approximately 190 days old, with previous bar-pressing experience. The apparatus was a Skinner box enclosed in a sound-attenuated chamber with a ventilation fan acting to mask outside noises. A locally-built relay programming equipment with the Gellerman-Fellows prearranged chance sequence was used to determine which of two bars would deliver access to water in each trial.

Each rat did two precognition runs of 100 trials each, for a total 200 trials per rat, or 1000 trials for all five rats combined. Each rat was tested on two consecutive days, one run per day. A trial lasted 10 seconds. It began with the presentation of two stimulus lights; the rat's first response, pressing a reinforcement bar under one of the two lights, allowed reinforcement in the form of water to be delivered for a correct prediction.

The overall scoring rate for total trials was 49 percent, which was statistically nonsignificant (50 percent was chance expectation). When the data were evaluated in terms of random behavior trials (RBTs), trials in which the animals changed response bars for no apparent reason, thus excluding those trials in which the behavior was explainable by conventional reinforcement theory, the results revealed a significantly positive scoring rate of 68.1 percent (CR = 3.57; $P < .0001$). Thus we concluded that the rats were using psi to obtain water in this experiment.

4

ALTERED STATES OF CONSCIOUSNESS*

COMPARISON OF EXTRASENSORY AND PRESLEEP INFLUENCES ON DREAMS: A PRELIMINARY REPORT

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(Maimonides Medical Center)

The purpose of this study was to compare extrasensory and presleep sensory incorporation of target (stimulus) material in dreams; to assess the quality and degree of incorporation in relation to emotional characteristics of the target material; and to examine individual differences in cognitive style in relation to target incorporation under sensory and extrasensory conditions of presentation.

A preliminary one-night dream session with art print targets served as the sole basis for selecting 40 subject-agent pairs for the formal study. Subjects were accepted into the formal study if they ranked the correct target in the upper half of a judging pool of six potential targets. One trial with chance probability of a hit being one-half is obviously less than an ideal basis for selecting subjects for more intensive work. This practice was necessitated, however, by the budget available for this study. Subjects and agents were all young adult volunteers. The agents were usually (though not always) friends or prior acquaintances of the subjects with whom the subjects felt they could easily communicate. There were an approximately equal number of men and women. The subjects were paid $50 upon completion of the formal series. The preliminary screening session also served as a laboratory adaptation night for the subsequent formal series.

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For the formal series, the targets were four 16-mm. motion picture films, each about 10 minutes in length. The films were selected from previous studies of presleep influences on dream content by H. A. Witkin and associates, project consultants. Two of the films had been found to be emotionally arousing in previous studies by Witkin, and on the basis of psychological and physiological measures: a film illustrating the Malstrom Vacuum Extractor as a method of delivering children at birth; and a film illustrating a subincision rite practiced by an Australian aboriginal group. The other two films had previously been found by Witkin and associates to be emotionally neutral: travelogues of London and of the Far Western United States. One emotionally-arousing film and one emotionally-neutral film served as targets for each of the two conditions of presentation (ESP and presleep). Order of presentation of the four films was determined randomly for each subject-agent pair on the first night of the formal series. Subjects were blind during the ESP condition, but not of course during the presleep condition, as to the emotionality variable.

In order to avoid stimulus residues from earlier sessions, the two ESP nights were run first for all subjects. The subject was prepared for EEG and EOG (eye movement) monitoring and enclosed in a shielded, sound-attenuated sleep room for the night. The agent was then taken to a room at the opposite end of the laboratory and shown one of the target films. Subject and agent then both went to sleep. In order to determine the subject's latency to sleep onset, the experimenter recorded the EEG page numbers from the initial lights-out period to the occurrence of the first sleep spindle in the EEG. The subject was awakened by the experimenter at the end of each REM period for a dream report, elicited according to a standard interview format. Additional associations were elicited in a postsleep interview in the morning. Dream reports and postsleep interview material were tape-recorded and subsequently transcribed.

The final two nights of the formal series served as the presleep condition. On these nights, the subject was prepared for sleep monitoring and then shown one of the two remaining target films prior to going to bed. The agent was not present during the presleep nights. The procedure was otherwise identical to the ESP condition.

Four measures of stimulus incorporation were utilized in this study: content correspondences involving blind ratings

by three independent judges of the degree of correspondence between each of the subject's four dream transcripts and each of the four target films, using a 100-point rating scale. The judges were also blind as to condition (ESP or presleep). Content analysis of emotionality and anxiety in the dream transcripts. This has not yet been completed; it is being evaluated on a blind basis at the University of Cincinnati using the Gottschalk-Glaser method of content analysis. Latency to sleep onset in relation to target-type (emotionally-arousing or emotionally-neutral), determined on the basis of number of minutes from lights out to the first sleep spindle. Pre-to-postsleep mood shifts for both subjects and agents, using the Clyde Mood Scale. This has not yet been completed.

In addition, subjects and agents were administered Witkin's Rod-and-Frame Test and Embedded Figures Test. Field-dependent subjects' mean incorporation scores on each of the four incorporation measures for each condition and target-type will be compared with those for field-independent subjects.

We have presently completed analysis of content correspondences and latency to sleep onset. For the content correspondence analysis, the judges' ratings for each session were converted to z-scores and the mean z-score for the three judges was obtained for each session. Degree of incorporation was assessed for each condition and target-type by t-test. For the presleep condition, both the emotionally-arousing and the emotionally-neutral target material were incorporated to a statistically significant degree. The difference between the two is not statistically significant. For the ESP condition, neither the emotionally-arousing nor the emotionally-neutral target material was incorporated to a significant degree. In fact, the results for the ESP condition are clearly nonsignificant. Latency to sleep onset (presleep condition) was not significantly affected by the emotional characteristics of the target, contrary to previous findings by Witkin and associates.

PSI AND THE IMAGINARY DREAM

Edward A. Charlesworth (University of Houston)

Spontaneous cases and laboratory studies indicate that certain variables may be related to the receptive psi process. These include physical relaxation, passivity of mind, decrease in externally directed attention, and perhaps an increase in attention to internal sensations. In addition, the dream state has been shown to facilitate psi in laboratory research. The present study is an attempt to induce a psi-favorable state allowing more experimental control than the normal REM dream, and including certain variables related to the receptive psi process. This was accomplished by the use of a 33-minute experimental tape recording which I designed to guide the subjects into an altered state of consciousness by the induction of an imaginary dream.

The tape comprised four components: a modified Jacobson's relaxation technique, deep breathing exercises, guided visual imagery exercises through different environmental scenes, and environmental sounds to increase the "reality" of the visual imagery. The progressive relaxation technique consisted of instructions to alternately tense and relax the various parts of the body. The deep breathing exercises simply had the subject focus on breathing deeply and slowly for a few minutes.

The visual imagery exercises were introduced by suggestions that the subject imagine a dream that he might have had as a child. He is told that in this dream he will get out of bed, walk across the room to a closet, find a door in the back of the closet, and open it. It is then suggested that he find himself in a green, grassy meadow. At this instant environmental sounds associated with a meadow are incorporated into the tape recording. He is then guided through the meadow and eventually through a forest, up a mountain, and down the mountain to a beach. Eventually on the beach he is instructed to return to his normal waking consciousness. Within each of the environmental scenes it is suggested that he see something unusual or out of place for that scene. For example, when the subject is in the meadow, the tape suggests that he look across the meadow and see something unusual, something he would not ordinarily find in a meadow. This constitutes the ESP test.

This tape was used in two experiments. Sixty individuals participated in the first study, each one serving as both agent and subject. Forty of them listened to the experimental tape to induce an imaginary dream, and 20 served as a control group. The majority were students at the University of Houston, who were either enrolled in an introductory psychology class and received experimental credit for participating, or responded to a handout asking for participants. The majority of the subject-agent pairs were acquainted with each other prior to the experiment. None of the subjects had participated in such research before, nor had had any prior training as a subject or an agent. Questionnaires answered by all subjects indicated that both experimental and control groups would be classified as "sheep."

The experimental group was tested individually, with one partner serving as the agent first and the other as the subject. The roles were then reversed. In an attempt to avoid any bias on the second trial, by familiarity with the experimental tape, the participants were told in advance exactly what the tape contained and the environmental scenes that they would imagine. Both roles were explained to both partners. The agent was told he would hear the same tape as the subject, and that he was to do all of the visual imagery just as the subject would be doing it. The difference was that when the tape suggested that the subject see something unusual, the agent was to incorporate the target picture into his imagery, and actively attempt to "send" it. The subject was to let his visual imagery flow passively, and see what unusual sight appeared.

After the subject-agent pair had decided who would take which role first, they were separated by approximately one hundred feet, six rooms, and three halls. Both the subject and the agent sat on a desk-type chair with a swivel base. The experimenter randomly selected a package of six target pictures and placed it in the room with the subject. The subject then placed a set of headphones on, the experimenter left the room, closed and locked the door, and no more verbal contact was made with the subject until the trial was over. The experimenter then went back to the room with the agent and randomly selected a slide of one of the pictures in the selected target pack for the agent to "send." The slide was then projected in the room with the agent (and remained projected throughout the tape's duration), headphones were put on the agent, and the experimental tape started. The experimenter then left the room and

locked the door behind him.

Both subject and agent heard the tape simultaneously. When the experimental tape was over and both subject and agent had been told (via the tape) to return to a normal state of consciousness, they answered questionnaires and filled out protocol sheets, and then the subject ranked the six target pictures from most likely to least likely to have been sent. The subject and agent then rejoined the experimenter, learned how they did, and then the roles were reversed for another trial.

The target pool consisted of 15 target packs, each containing six pictures in separate opaque envelopes. There was a slide made of each target picture, so that the agent never had to touch the picture itself. The target packs had been prepared beforehand by the experimenter; an attempt was made to use simple, yet striking pictures. The criterion for placing six pictures within each pack was that they were as dissimilar as possible (especially in form, color, and theme). All of the pictures were selected with the idea that they would be considered something not normally seen in any of the environmental scenes to be imagined.

The control group of 20 subjects was also divided into subject-agent pairs. Each pair did two sessions (reversing roles between sessions) with only a five-minute impression period, and two sessions with the modified progressive relaxation portion of the tape and a five-minute impression period.

Rankings of the correct target of one through three were classified as hits and rankings of four through six as misses. There were 28 hits and 12 misses for the experimental group, yielding a binomial probability of .0089, for significant psi-hitting. The control group scored at chance for both the five-minute impression period and the progressive relaxation and five-minute impression period. The subjects' questionnaires showed that both the imaginary dream induction tape and the progressive relaxation tape were effective in reducing physical tension and mental tension (all were significant at $P < .005$ by the Wilcoxon test), but the impression-only condition was not.

There was no significant difference between the experimental group and either of the two control groups in terms of initial physical relaxation rating, initial mental re-

laxation rating, impression period physical relaxation rating, or impression period mental relaxation rating (Mann-Whitney U test). There was no significant difference between the experimental group and the progressive relaxation control group on questions concerning similarity to or difference from waking consciousness, or bodily awareness. Both groups indicated that awareness of their bodies was altered, as was their consciousness in comparison to their normal waking consciousness. The impression-only control group did differ significantly from the experimental group on these two questions, and appeared not to be altered in consciousness or bodily awareness to a significant degree. The impression-only control group also differed significantly from the experimental group in belief that the state they were in would facilitate psi. The control group tended to believe that just the impression period did not facilitate psi. The experimental group did not differ significantly from either of the two control groups in variables which might have important influences on the outcome of the experiment (belief, mood, attitude, etc.).

The results of this first experiment suggest that the imaginary dream is conducive to psi. The target-protocol correspondences suggested that psi was mediated through both visual and auditory imagery evoked by the imaginary dream. I then did a second experiment using same-sex twins as subject-agent pairs. There were 14 female and six male identical twins, and ten female and ten male fraternal twins. The mean age for the identical twins was 15.3 years, and for the fraternal twins 16.1 years. Both groups would be classified as "sheep" by their scores on questionnaires. The design of this experiment was identical to that of the first experiment.

For the group of identical twins there were seven hits and 13 misses, for nonsignificant psi-missing. The group of fraternal twins scored 15 hits and five misses, yielding significant psi-hitting (binomial $P = .021$). There was a significant difference between the two groups ($P = .0125$) when mean ranks were compared by Mann-Whitney U test. The groups did not differ significantly on any variables which might have important influences on the results of the experiment (belief, mood, attitude, and certain other states), with the exception of their self-rated introversion and extraversion. The fraternal twins were significantly more extraverted than the identical twins (Mann-Whitney U test, $P < .01$).

Perhaps the identical twins did poorly because the role of being an identical twin carries with it an immense need to individualize. Jan Ehrenwald mentions the erection of barriers against telepathy within the mother-child relationship when the child starts a phase of separation-individuation. If this individuation need is strong in the mother-child relationship, then that strength must be many times increased when one has another person who is his genetic duplicate. Identical twins may therefore shut off psychic encroachment from each other.

EXPERIENTIAL FACTORS RELATED TO FREE-RESPONSE CLAIRVOYANCE PERFORMANCE IN A SENSORY UNIFORMITY SETTING (GANZFELD)

Rex G. Stanford† and Ann Neylon (St. John's University)

Honorton and Harper reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 52-3] that they had obtained positive, extrachance performance when subjects were given a GESP free-response test in a Ganzfeld setting. In such a setting the subject relaxes with a uniformly translucent hemisphere over each eye and is subjected to relatively uniform auditory stimulation through earphones (e.g., the sound of waves on a beach or white noise). He is encouraged to report freely all the images and other experiences he has during the period he is in the Ganzfeld.

One objective of the present study was to learn whether the Ganzfeld setting would facilitate performance in a clairvoyance free-response task as it appears to do in free-response GESP work. Another objective was to measure some of the subject's psychological responses to the test situation to learn whether reports concerning certain internal states might be related to degree of success in the ESP task.

Forty college-age volunteer subjects, including both males and females, were tested individually. A.N. was in charge of the testing. The subject reclined in a padded chair with half a ping-pong ball taped over each eye and with "pink" noise (white noise with high frequencies attenuated) administered to both ears via earphones. A fluorescent-tube desk lamp was lowered in front of him at eye level

with the length of the tubes extended crosswise relative to his body. This lamp was adjusted such that the subject reported uniform luminance over his entire visual field and such that the lighting level was comfortable. The pink noise was also adjusted to be comfortable, yet loud enough to avoid distraction from external sounds.

The subject was to respond to a target picture placed on a table opposite him. The picture was entirely enclosed in a heavy aluminum-foil wrapping and this package was in turn placed inside a black, opaque folder. The subject was told not to try to concentrate on the picture itself, but to focus on, be aware of, and report whatever was happening in his mind. His "contact" with the picture would, he was told, take place automatically because of his desire to succeed, so he should focus on inner experience rather than trying to "see through" the wrapper and look at the picture.

The target picture was randomly selected from a 50-target pool consisting of ten target sets, each made up of five pictures. The Rand table of random digits was used by R.G.S. to derive, first, the target set (from the ten possibilities), and then the picture from that set (from among the five possibilities). R.G.S. had prepared the list of targets for all the subjects prior to the beginning of the study and had used a single random number table entry point to do so, allowing subsequent digits to determine the target for subjects in the order of their testing. This was done to reduce the role of possible experimenter psi-bias in selecting targets. The initial entry point was determined by the place in the table at which R.G.S.'s last study involving the table had terminated.

R.G.S. placed the selected target in its containers prior to each session. He had no contact with the subjects during the experiment until after their judging of target and control pictures had been completed. A.N. was blind as to the identity of the target picture. The subject remained in the Ganzfeld for 25 minutes, but he had not been informed of the length of time the Ganzfeld would last. A.N. made notes, during the Ganzfeld, of the subject's reports. Immediately after the Ganzfeld the subject was asked to estimate how long it seemed he had been in it. Then he responded to a series of 11 questions about his experiences during the Ganzfeld, all phrased in terms of what percent of the time he had had a particular type of experience. While the subject responded to the questionnaire, A.N. took

the target picture (still concealed) to R.G.S. and left his office while he removed the picture from its containers and placed it in random order with respect to the four other pictures in its set. He then returned the set to A.N.

The subject read instructions designed to help him avoid some of the pitfalls of the judging process, and he was asked to rate each picture on a 31-point scale of similarity to his Ganzfeld experience. The notes regarding his reports during the Ganzfeld were read to him immediately before he began the judging. When he had completed his judgments, both he and A.N. went to R.G.S.'s office to ascertain the target picture for the session. The ESP score for each subject was the z-score for his rating of the target picture (based upon comparison of that rating with the mean of all five ratings divided by the standard deviation of all five ratings).

The overall ESP results did not reach statistical significance as measured by a t-test (of all z-scores against theoretical mean chance expectation). The direction of the results was negative ($t = -0.53$, $df = 39$). Subjects' estimates of time in the Ganzfeld showed a highly non-normal (skewed and rather leptokurtic) frequency distribution, both by visual inspection and by Geary's test of non-normality. This means that application of the product-moment correlation method to the relationship of ESP scores and time estimates could be misleading, so the correlation of these variables is not being reported here (though it is reported for the relationships of other variables and ESP performance examined below). However, the t-test is known to be robust in the face of non-normality. Thus, ESP scores were dichotomized at the median, and mean time estimates for the two levels of ESP performance were contrasted using the t-test. The 20 subjects in the higher-scoring group underestimated time in the Ganzfeld to a degree that was significantly greater than for the 20 lower scorers ($t = 2.31$, $df = 38$; $P < .03$, two-tailed).

The higher-scoring group underestimated time, on the average, by about 12 minutes, whereas the lower-scoring group underestimated it by about four minutes. The ESP scores of subjects who underestimated time in the Ganzfeld ($N = 33$) were compared with those ($N = 7$) who did not; the latter group tended to psi-miss, whereas the former performed more positively ($t = 2.57$, $df = 38$; $P < .02$, two-tailed). Subjects who did not underestimate time scored sig-

nificantly below mean chance expectation ($t = 5.13$, $df = 6$; $P < .005$, two-tailed).

The percentage of time that random, disconnected thoughts were reported during the Ganzfeld correlated positively and significantly with ESP scores ($r = +.336$, $df = 38$; $P < .04$, two-tailed). Percentage of time that changes in body image were noted during the Ganzfeld correlated negatively and significantly with ESP scores ($r = -.376$, $df = 38$; $P < .02$, two-tailed). None of the other questionnaire items correlated significantly with ESP performance.

The results of this study suggest that the high performance level found in free-response GESP tests with the Ganzfeld may not easily generalize to a free-response clairvoyance task. Nonparapsychological investigators using the Ganzfeld have felt it tends to promote a "transference-like" relationship with the experimenter. Perhaps this kind of relationship develops with the agent in Ganzfeld GESP studies. If this is true, it may explain why such strongly positive scores are obtained. The Ganzfeld's sensory uniformity may set the stage for effective utilization of internal cues which carry psi information, but the special relationship with the telepathic agent proposed above may explain why there is such a degree of focusing on the target as opposed to control pictures (displacement). In the present clairvoyance study both of us were struck by the dramatic character and abundance of seeming displacement (that is, focusing on a control picture rather than the target), which led to psi-missing.

The finding that level of ESP performance predicts degree of experienced "time contraction" in the Ganzfeld and vice versa may provide an insight into the perennially troublesome displacement phenomenon in free-response work. The finding indicates that subjects who tended to displace in the study were largely those who showed no time contraction in the Ganzfeld. Perhaps the measure of time contraction was really a measure of the degree to which the subject became deeply absorbed in the experience in an affectively positive (or at least neutral) way. If a subject who is not involved very deeply or positively in the Ganzfeld experience feels the task is "running on a long time," he may actually "look ahead" in time toward the end of the session, a time when he will confront all the pictures in the set. This may orient his ESP toward the larger target set and may result in displacement.

Results involving the questionnaire items which correlated significantly with ESP performance fit in well with the model of internal states and ESP performance which Honorton has been developing. "Random, disconnected thoughts" may represent a freedom from the organized, rational forms of mentation which seem to hinder ESP performance, and may be vehicles for extrasensory information. "Changes in body image" are perhaps most likely to be reported by subjects who have not successfully tuned out or reduced bodily sensation. Such sensation may be a source of noise which can interfere with the utilization of psi-mediated cues.

PSI-MEDIATED IMAGERY AND IDEATION IN THE GANZFELD: A CONFIRMATORY STUDY

Charles Honorton† and James C. Terry (Maimonides Medical Center)

In an earlier report [RIP 1973, pp. 52-3], C.H. and Sharon Harper found statistically significant incorporation of pictorial target material (View-Master slide reels) in a free-response GESP study involving regulation of visual and auditory perceptual input. Thirty volunteer subjects each contributed a single session in which their perceptual input was regulated by a homogeneous visual field (Ganzfeld) and a repetitious seashore recording played through headphones. The subjects gave continuous verbal reports of ongoing imagery and ideation following instructions to "think out loud." A distantly located agent, at a time unknown to the subject, viewed a randomly selected View-Master target reel, attempting to extrasensorially influence the subject's ongoing mentation.

The present study is an attempt to confirm the findings of our previous investigation. Eighteen undergraduate honors students enrolled in C.H.'s parapsychology honors seminar at St. John's University participated. The students were divided into six experimental teams, consisting of three students per team. The experimental protocol called for each team to complete ten experimental sessions with one member of the team serving as subject, another as agent, and a third as recording experimenter. Due to a hospital

workers' strike which closed the laboratory building, only one of the six teams completed all ten sessions. The other teams completed between three and seven sessions each. Twelve of the students served as subjects, completing between one and four sessions each.

During the session the subject sat in a shielded, sound-attenuated room with halved ping-pong balls over the eyes, a source of light in front of the face, and stereo headphones connecting to a cassette playback deck in the adjacent monitoring room where the recorder monitored the subject's verbal report by intercom. Instructions to the subject were identical to those used in the earlier study. White noise was used in the present study instead of the seashore recording used earlier.

Once the subject was enclosed in the shielded room, an agent stationed in a second experimental room carried out the randomization procedure to select the target. The procedure consisted of shuffling and cutting a deck of 31 numbered cards, corresponding to the 31 target pools. Each target pool contained four thematically different View-Master reels. The uppermost reel in the pool served as target for the session. The agent then shuffled a second deck of numbered cards to determine which of three ten-minute periods would serve as the sending period. These procedures were supervised and observed by either C.H. or J.C.T. in 27 of the 38 completed experimental sessions. At the end of the sending period, the agent replaced the target reel in the packet with the other three reels in the pool and shuffled the four reels in the packet. In the 27 supervised sessions, either C.H. or J.C.T. further shuffled the four reels in the packet before delivering the packet to the recorder at the end of the session.

The recorder then took the packet into the subject's room along with detailed notes of the subject's mentation report. Following a review of the mentation report, the subject blind-ranked the four reels in the pool according to perceived similarities with mentation during the session. Upon completion of the blind-ranking procedure, the agent was called in to reveal the correct target reel.

The results given here comprise only the 27 sessions supervised by C.H. or J.C.T., in which we could be sure of adequate procedural controls. A hit was defined as a correct first or second choice from the pool of four slide

reels. One of the teams (Team A) obtained independently significant positive results (P = .008), and the overall results pooled across teams show significant psi-hitting when combined by Fisher's method (P = .014). Unlike the earlier study, the number of direct hits (correct first choices) is not independently significant, though of the same order of magnitude. With 25 percent expected by chance, the present study had 41 percent direct hits, and the earlier study 43 percent.

Below are excerpts from subjects' mentation reports for sessions in which the correct target reels were selected as the subject's first or second choices. The excerpts represent sections of mentation reports which the subject identified as providing the basis of similarity with the target reels; they are divided into mentation occurring in the presending, sending, and postsending periods.

TEAM A. Target: "The Flying Nun" (second choice). Presending: "... An archbishop's hat. Tiny people, far away.... Floating ... a lot of clouds and a skyline.... A 3-D statue of a girl's face, with short dark hair.... A blue sky, people with umbrellas, looks like it could be France ... Red Riding Hood. A little girl in a bonnet, she's gone now." Sending: "Arches. A church. A lot of buildings. An aerial view, moving to the ground fast. A road with hedges and towers. A hand, buildings, a church and columns. A French church with Doric columns...." Postsending: "... A window up high in a stone wall, more bells. A peaked roof...."

TEAM A. Target: "Gunsmoke" (first choice). Presending: "... Now a desert, probably because I'm thirsty. There's a skull of a longhorn cow ... a pool of water, buzzards flying over it ... a grey horse and rider...." Sending: "... a tepee and two Indians, holding a corn grinder ... a hand gun, from around 1860 or 1870, like from the Westerns ... a pinto horse, tied to a hitching post...." Postsending: no apparent correspondences.

TEAM C. Target: "Lancelot Link Secret Chimp" (second choice). Presending: "... A chimpanzee from '2001' jumping up and down...." Sending: "... apes and pre-human lifestyles...." Postsending: "... The images of prehistoric dogs, pre-human ape men, what a rough existence they must have had. An image of a family friend who is a policeman, blue uniform and badge...."

TEAM E. Target: "Rare Coins" (first choice). Pre-sending: "... now I see circles--an enormous amount of them. Their sizes are not the same ... some are really large, and others are very tiny--no larger than a penny. They just keep flashing in front of me--all these different sized circles." Sending: no apparent correspondences. Post-sending: "Now I see colors--a complete array of colors. Two ... in particular--gold and silver seem to stand out more than all the others. I sense something important. I can't tell what but I get a feeling of importances, respect, value."

5

ANIMAL STUDIES*

THE EFFECT OF THE EXPERIMENTER ON PRECOGNITION IN THE RAT

James G. Craig (University of Waterloo, Ontario)

There is a growing concern among parapsychologists with the effect of the experimenter on the results of psi research. This concern has been especially marked when animals are used as subjects. Do animals have psi ability like humans, or do only humans have psi, and use it to influence their animal subjects? While there is evidence in psi experiments that experimenters influence human subjects there is, as yet, no evidence that experimenters influence the performance of animal subjects. The following experiment was planned to investigate this question. It is similar in design to previous research I reported at the 1972 Parapsychological Association convention dealing with precognition in rats run in a T-maze [RIP 1972, pp. 154-6].

In animal psi experiments concerned with precognition, the general experimental paradigm involves measuring changes in an animal's behavior in the present as a function of the presentation of an aversive event, such as shock or death, sometime in the future. This may be divided into two subsidiary paradigms, which I have labelled Contingent and Noncontingent. In the Contingent precognition paradigm the presence or absence of the aversive event in the future is contingent upon some aspect of the animal's behavior in the present. In the Noncontingent precognition paradigm, animal subjects have no control over the future onset of the aversive event.

*Chairman: J. G. Pratt, University of Virginia.

In the experiments I have done previous to this one, the differential effect of this Contingent-Noncontingent variable was explored. Within each condition animals were assigned to Imminent Death or Delay Death. Imminent Death followed the running of the experiment; Delay Death meant that the animals were terminated no earlier than one month after the experiment. Within the Contingent condition, animals were assigned to Imminent Death if they made an incorrect choice and to Delay Death if they made the correct choice in the T-maze. The correctness of the choices was determined by matching the actual choices of the rats against a series of random numbers. Within the Noncontingent condition, assignment to Imminent versus Delay Death was made without regard to the rat's choice.

The results of my early experiments, though complex, indicated that the Contingent-Noncontingent factor interacted with the Imminent-Delay Death factor to produce consistent differences between groups in running time in the T-maze. When these results were shown to my adviser at the University of Waterloo, Dr. R. V. Thysell, he had two concerns. The first was that I was biasing my data in the desired direction by incorrect time recording. The second was that I was biasing my data by somehow influencing the animals. Both concerns involved my using precognition and/or psychokinesis to achieve the desired results. To test his hypothesis, he agreed to participate with me in a replication of my first experiment, in which we made two minor modifications. First, each of us recorded running time. Second, each of us handled only half the animals.

Most of the 96 rats employed were male Holtzman. Of these, 34 comprised the Delay rats used in the first experiment. All animals had been used in previous learning experiments. They were maintained on food and water ad libitum. We used the same T-maze I had used before. All rats were randomly assigned a running order position. Then, they were randomly assigned to each of the two experimenters, myself and Thysell, the only proviso being that each experimenter was assigned half the animals. Each rat had one trial.

The T-maze was placed on a table which stood about five feet from the ground. I stood to the left of the top middle of the T-maze, while Thysell stood to the right. These positions corresponded to the left and right turns of the rats in the maze, and were maintained throughout the experiment.

Each experimenter was responsible for picking his assigned rat out of the rat's home cage, moving to his assigned position, depositing the rat in the start box of the T-maze, and raising the door to the start box so the rat could enter the maze proper. After a rat had completed its run Thysell reported his observations of the rat's running time, T-maze arm choice (right or left), and number of fecal boluses, if any, to me; I recorded Thysell's data, then my own observations. While I was doing this Thysell transferred the rat to its home cage. After all the rats had been run, each experimenter's 48 rats were randomly assigned to four groups: Contingent-Imminent (C-I), Contingent-Delay (C-D), Noncontingent-Imminent (NC-I), and Noncontingent-Delay (NC-D).

Of the 96 rats, ten were dropped from further analysis because they took longer than two minutes to run the T-maze. Of the remaining 86, 45 were in my groups and 41 were in Thysell's groups. A correlation performed on the running times recorded by the two experimenters showed almost perfect correspondence ($r = +.9999$; $P < .001$). The two running times for each rat were, therefore, averaged to produce one score. Observations of turn direction and fecal bolus count tallied exactly.

A three-way analysis of variance (Experimenter by Contingent-Noncontingent by Imminent-Delay) on the dependent variable running time gave evidence of two kinds of experimenter effects. First, there was the suggestion of an interaction between the Experimenter variable and the Contingent-Noncontingent variable ($F = 3.57$, $df = 1, 78$; $P = .06$). With me as experimenter, the two Contingent groups had a mean running time of 39.7 seconds, while the two Noncontingent groups had a mean running time of 29.7 seconds, which was 25 percent less. For Thysell, on the other hand, the two Contingent groups had a mean running time of 32.2 seconds, while the two Noncontingent groups had a mean running time of 46.4 seconds, which was 31 percent more. Furthermore, while the mean running time for my own NC-D group was 29.7 seconds, the mean running time for Thysell's NC-D group was 46.4 seconds, which was 36 percent more ($P < .10$ by Duncan's Multiple Range Test). In general, my own results tended to replicate my earlier experiments, whereas Thysell's tended to contradict them.

Another kind of experimenter effect was observed in a more complex interaction involving the C-D groups versus the other groups. When I handled the animals, the C-D

group had a consistently longer mean running time than the C-I, NC-D, and NC-I groups. On the other hand, when Thysell handled the animals, the C-D group had the shortest mean running time of any of the four groups.

Besides the experimenter effects, there was the suggestion of an interaction between the Contingent-Noncontingent variable and the Imminent-Delay variable ($F = 2.89$, $df = 1, 78$; $P = .09$). Under the Contingent condition, the Imminent group had a mean running time of 39.0 seconds, while the Delay group had a mean running time of 32.9 seconds, which was 16 percent less. Under the Noncontingent condition, on the other hand, the Imminent group had a mean running time of 30.2 seconds, while the Delay group had a mean running time of 45.9 seconds, which was 34 percent more. These results tended to contradict my earlier experiments.

In summary, the hypothesis that my initial results might have been due to measurement errors was disconfirmed in the present experiment by the finding of a very high correlation between running time scores recorded by both experimenters, and by the totally nondiscrepant measures of fecal bolus emission and turn direction. On the other hand, the hypothesis that my results might be due to my somehow influencing the animals was confirmed. It was apparent that the direction of the differences between the experimental groups was a function not only of the Contingent-Noncontingent and Imminent-Delay variables, but of the Experimenter variable as well. It would appear, therefore, that the results were a product of both subject and experimenter. The message for experimenters working with animals as subjects is clear. Unless the effect of the experimenter is measured, conclusions about the meaning of animal psi experiments must be tentative.

PRECOGNITION AS A FUNCTION OF ENVIRONMENTAL ENRICHMENT AND TIME OF THE LUNAR MONTH

William C. Treurniet and James G. Craig† (University of Waterloo)

Previous research by ourselves and others suggests

that there may be a lunar cycle which affects scoring in animal psi tests, and also that novelty, an enriched environment, and the opportunity to interact with other animals may be important. The hypothesis tested in the present experiment, therefore, was that time of testing in the lunar cycle interacts with environmental conditions prior to testing to influence the expression of behavior attributable to precognition of the animal subject's own death.

Eighty 28-day-old, male, Swiss Webster mice were the subjects. Forty mice were housed individually in plastic cages containing nothing but approximately one-half inch of bedding material consisting of wood shavings. These mice were considered environmentally deprived. The other 40 were housed in groups of five in larger plastic cages containing a variety of articles. Every day, these articles were rotated from cage to cage, and fresh bedding supplied. These animals were considered environmentally enriched. All animals were placed on a cycle consisting of 12 hours of light followed by 12 hours of darkness, with the lights being turned on at 5:00 a.m. These conditions were maintained for 30 days before testing was begun.

Each mouse had one trial. Activity was evaluated in two ways: by observing the number of line crossings made by the animal on a grid drawn on a "hole board," and by counting the number of "head dips" the animal made. A mouse, when placed on the board, has a natural tendency to dip its head into the various holes. The frequency of head dipping is presumably indicative of the animal's exploratory tendencies. Number of line crossings and number of head dips were measured simultaneously for three consecutive minutes with counts being obtained for each minute.

Half of the animals from each housing condition were tested at a point in the lunar cycle when previous data obtained by J.G.C. suggested that rats that were going to die would be more active than controls. The other half of the mice were tested at a point in the lunar cycle (three days later) when previous data suggested that rats that were going to die would be less active than controls. All testing was done between the hours of 8:00 p.m. and 12:00 midnight. Groups were randomly selected from the enriched and deprived animals immediately following completion of each day's testing, and were sacrificed with chloroform within an hour. These four groups (one from each test-day-by-housing-condition category) will subsequently be referred to as

the "Imminent" groups. The animals in the four remaining groups (henceforth referred to as the "Delay" groups) were all maintained in the solitary condition for a period of one month following completion of the experiment.

The experimental design was composed of three between-subject factors: the Enriched-Deprived condition, the Imminent-Delay condition, and the Day of test condition. The three scores obtained over the three minutes of test constituted the only within-subject factor for each of the two dependent measures of line crosses and head dips. Analysis of variance of the line crossing data revealed no significant effects; however, analysis of the head-dipping behavior showed a significant three-way interaction ($F = 6.25$, $df = 1, 64$; $P < .025$): the enriched and deprived animals scored in opposite directions in response to the Imminent-Delay treatment on both test days, and the direction of the response to the Imminent-Delay treatment on the first day was opposite to the direction on the second test day. Pearson product-moment correlations between number of head dips and number of line crossings suggested that the two measures were relatively independent.

The significant Enriched-Deprived by Imminent-Delay by Day of test interaction obtained for head-dipping behavior, the absence of any effect involving precognition with the line crossing variable, and the low correlations existing between the two measures suggest that the exploratory component of activity may have been responsible for any precognition effect obtained in our previous animal research. In the T-maze apparatus used by J.G.C., for example, the animals' exploratory tendencies may have correlated highly with time to traverse the maze. Running time was only operationally defined as reflecting "activity." It may equally well have quantified exploratory tendencies. Apparatus design, then, because of its influence on behavior, becomes an important consideration in a precognition experiment with rodents.

In conclusion, the prediction that environmental conditions prior to testing would interact with time of testing in a lunar cycle to influence a precognitive behavioral response was supported.

6

THEORY AND METHODS*

A CONCEPT OF A MATHEMATICAL PSI THEORY

Helmut Schmidt (Institute for Parapsychology, FRNM)

Many of the observed psi phenomena, for example precognition, appear quite inconsistent with our basic intuitive concepts about nature, so that an understanding of psi may require major changes in our thinking patterns. Modern physics has coped successfully with similar situations. Relativity theory has shown, for example, that our naive concepts of space and time were only approximately correct, and quantum theory has made it clear that our naive pictures of particles with well-specified locations and velocities are not applicable to the particles encountered at the subatomic level.

Physics was able to overcome the limitations set by our naive intuition with the help of mathematical theories. It was found that even where our intuition fails a mathematical formalism can still guide us to develop a new, more appropriate kind of understanding. This reasoning suggests that we try to use a mathematical approach to the understanding of psi, in the sense that we first develop mathematical models for psi and then depend on the mathematical formalism rather than on intuition for the proper "interpretation" of the phenomena. In the following I will outline a specific example of a mathematical psi model. By "model," I mean a preliminary theory which provides a logically consistent outlook and suggests further experiments.

Let us assume the statistical viewpoint of quantum theory that nature is governed partly by chance. This means

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that the present state of the world does not uniquely determine the future world history, so that many possible future world histories are consistent with a given present state. Quantum theory specifies the probability for any one of these possible histories to be the actually realized history. The psi axiom to be introduced will modify the values of these probabilities. Let me introduce the axiom in connection with a specific example, which will easily lead to a more general formulation. Consider a binary random number generator with the two output channels P and Q so that, whenever the generator is triggered in the absence of a psi effect, the channel P or Q emits a signal with the probabilities p and q respectively.

Next, let a subject, whom we will term the "psi source," be linked to the generator in such a manner that with every P-output signal the subject receives a rewarding input signal, whereas a Q-output signal has no effect on the subject. In the case of a human subject who is instructed to enforce a high rate of P-outputs this signal could be simply an indication of success, and for an animal the rewarding input signal might be provided by the administration of food, warmth, etc. In the case of psi-hitting, then, the probabilities p and q for a signal to appear at P or Q respectively are changed into p' and q' with $p'/q' > p/q$. Then we can introduce a number $\theta > 1$ so that:

$$(1)\quad p'/q' = \theta \cdot p/q, \text{ or}$$

$$(2)\quad p' = p\cdot\theta/(p\cdot\theta+q) \text{ and } q' = q/(p\cdot\theta+q).$$

Let us call θ the strength of the psi source. For $\theta = 1$ there would be no PK effect and in the case $0 < \theta < 1$ we would have PK-missing (reduction of the P-output signal frequency). Previous to the triggering of the generator we have to consider, from the statistical viewpoint of quantum theory, two possible classes of future world histories, the P-class where the P-channel carries a signal which is transmitted to the subject, and the Q-class where the Q-channel carries a signal which does not affect the subject. The presence of the PK-hitting subject changes the quantum theoretical probabilities p and q for these two classes into p' and q' as given by equation (2) above.

Let me now lay the basis for our model theory by postulating the existence of certain psi sources with properties of idealized PK subjects in the sense that the strength

of each psi source is constant in time. This implies that, for the present, we disregard fluctuations in a subject's performance due to exhaustion, mood, etc. In comparing the model with experimental results we can later at least in part correct for this simplification. The constancy of the strength of θ does not imply that the psi source scores equally efficiently in all psi test situations. The θ-value of a psi source may be considered as an internal parameter, similar to the p and q values of a binary random generator which are specified by the generator's internal structure, but can be different from the signal output probabilities in a PK test setup. The psi axiom specifies in a general and mathematically simple manner the interaction between a psi source and the rest of the world, which contains the previous example as a special, typical case.

Let us formalize our psi axiom. Consider a situation in which the outcome of a random process decides between two possible classes of future world histories P and Q. Let the probabilities for P and Q to occur in the absence of the psi source under consideration be p and q. Next, let the psi source of strength θ be coupled to the system so that the psi source receives a signal, or is "stimulated," for each of the P-histories (sometime in the course of the history) while the Q-histories do not stimulate the source. Then the original probabilities p and q for the two classes of histories are changed as follows: (a) the new probabilities for the class P and Q are p' and q' with values shown in equations (1) and (2) above; (b) the relative probabilities of the different histories within the class P and within the class Q are not changed.

For a phenomenological psi theory we do not have to discuss the internal structure of a psi source. But the model certainly suggests a search for the most elementary psi source in nature. From the psi axiom we can derive a large number of specific predictions, for example an addition theorem for PK effects, which can be tested experimentally. Furthermore, the axiom implies some general features of psi which might be mentioned.

Complexity independence. The scoring rate of a psi source in a PK test depends only on the p and q values of the random generator and on the strength of the psi source. Thus in particular the scoring rate is independent of the internal structure or the complexity of the random generator. This aspect of the model is in accord with a large number

of experiments which have pointed out the surprising degree of independence between the subject's scoring rate and the complexity of a psi task.

Space-time independence. The psi axiom makes no reference to the spatial separation between the random generator and the psi source, nor to the time interval between the activation of the random generator and the presentation of the stimulus to the subject. This implies a space-time independence of psi which has also been experimentally observed. Furthermore the time independence of psi in our model leads to the existence of precognition: the psi sources which appear at first sight as only PK sources can, in the model, be utilized to make predictions of future random events similar to a successful subject in a precognition test.

The importance of feedback. A psi source can have an effect on the world history only if the source can be stimulated somewhere in the course of the world history. The stimulus corresponds to the feedback signal indicating success to a subject in a psi test. Thus in the model feedback at some stage is necessary to make psi operate. A psychic could not continue to perform if he never learned (not even from some later newspaper article) how well he performed in a psi test. This feature of the model suggests a more careful study of the role of feedback in psi tests, and it is possible that these studies will lead to some modifications of the proposed model.

In conclusion, this discussion was based on the working hypothesis that the fundamental laws of nature are mathematically simple and that in particular the often complex appearance of psi results from an interplay between a simple psi principle and the complex brain. The main objective was to develop some model within which psi-like phenomena could be discussed in a logically consistent manner. Such a model could be derived from a mathematically simple psi axiom. This axiom was formulated in terms of psi sources; that is, structures with axiomatically defined properties. At the present stage, the model could not be expected to give an accurate description of psi, but only to serve as a basis for further theoretical and experimental studies. However, the model displays a large number of features of observed psi effects, and it is not yet clear where the model disagrees with experimental evidence.

KIRLIAN PHOTOGRAPHY AND ITS RELEVANCE TO PARAPSYCHOLOGICAL RESEARCH

Larry Burton†, William Joines, and Brad Stevens (Duke University and Psychical Research Foundation)

Kirlian photography is a process whereby an electric field is applied to an object near, or in direct contact with, a photographic film. The process is conducted with the film in total darkness, and if the electric field is of sufficient intensity (about one million volts per meter) a pattern of electromagnetic radiation within the region surrounding the object is displayed on the film. Several factors are responsible for the parapsychologist's interest in Kirlian photography. The unusual glow around the object image is in some respects similar to the human "aura" as described by psychics. This possible connection between the image and the "aura" was reinforced by reports of the "phantom leaf" effect and the Kirlians' claim of drastic image change prior to the onset of disease.

From the intuitive point of view, the occurrence of psi phenomena indicates an energy transfer (or at least an information transfer) between the subject and his surroundings. The possibility that Kirlian photography might be a means of detecting this energy has motivated our efforts to determine the physical mechanisms occurring during the Kirlian process. Finally, the recent rise in publicity of parapsychological research has linked Kirlian photography with parapsychology.

To date, published works represent little progress toward a clear understanding of the physical mechanisms involved in Kirlian photography. Two general directions of research have evolved. The first direction is to focus on the psychological and physiological aspects of the process while assuming that whatever the physical mechanisms of the process may be, they remain constant from one experiment to the next. Following this line of research, Moss and Johnson have reported that the Kirlian image fails to correlate with various physiological changes, while noting a persistent correlation with emotional changes. The second direction of research is to concentrate on the physical mechanisms of the process. Following this, Tiller has deduced that the image production is the result of electrical corona. Other investigators claim that the image is a record of the

human "aura." All published reports to date fail to examine the possibility of induced radiation from the body. The Kirlians implied that the image could be used for the early detection of disease. They were also responsible for the report of the "phantom leaf" effect, in which the Kirlian photograph of a leaf from which a part has been cut reveals the complete leaf outline. Finally, published experiments to date have been difficult to repeat by other investigators.

In order to fully understand the results of any experiment, it is necessary to isolate and describe the effects of as many variables as possible. With Kirlian photography, this is especially necessary since apparently contradictory results are often obtained. Furthermore, if Kirlian photography is to be used as a tool in parapsychological research, we must fully understand the physical mechanisms of the process if we are to design a reliable and repeatable experiment. In order to accomplish these goals, we must first know the source of the radiation which causes the image production. Second, we must understand the process by which the radiation is produced, and the influence which changes in technique can have on the production of the radiation.

The obvious question one asks is: where does the radiation originate; within the object being photographed, within the medium surrounding the object, or within the photographic film? All material substances (including air, photographic film, and biological tissue) can react to an applied electric field by virtue of the fact that all materials contain charge carriers which can be displaced (that is, all material can have an induced dipole moment). This reaction to the applied electric field increases the energy of the material. To release this added energy, the material could then radiate at frequencies within the infrared (IR), visible, and ultraviolet (UV) range which are detected by the photographic film.

There are two outstanding possibilities for induced radiation from the human body by virtue of this mechanism. First, polar molecules comprising the nerve cell membrane have a dipole moment of such magnitude that if one dipole in an array were "flipped," the resultant dipole radiation would be in the range of near UV radiation. The fact that IR radiation has been observed from the blue crab nerve, and that UV radiation near 255 and 285 nm severely inhibits normal cell firing, lend much credibility to this possibility. The second possibility relies on a similar process in con-

nective tissue of the skin. It is an established fact that various substances in the skin will fluoresce when irradiated with light of near UV frequencies. Given a source of external radiation (either the proposed radiation from nerve cells or from electrical corona which will be described later) this fluorescence could take place. Also under the influence of the applied electric field, various bond angles in tissue structures could be stretched and relaxed, thus yielding radiation. Of course, even if the radiation is generated within the body, there is the problem of attenuation of the radiation before it could reach the exterior of the body. From the optical properties of the epidermis, we can deduce that visible radiation and some very near UV radiation might escape the body.

A similar process could occur in the photographic film, but owing to the types of ions present in the emulsion and the local gradients of the applied electric field, the possibility of radiation from the film is remote. It is possible that the field itself might reorder the crystal structure of the film emulsion, but we have been able to show experimentally that the Kirlian image does not originate within the film emulsion.

The final possibility for radiation production is electrical corona in the air about the object. Although the process for the production of corona radiation is basically the same as in other solid materials (that is, one elicits dipole radiation from the electric contribution to the dipole moment), the name corona is reserved for radiation from gaseous mediums. As the dielectric constant of the body and the film reduce the applied electric field by a factor of ten, the corona process is always far more intense than either of the above possible processes for any value of applied field strength. Furthermore, if the gas composition is known, then one may use the methods of plasma spectroscopy to calculate the frequency and intensity of the radiation given off by the gas. Using the data from spectroscopy, we have developed charts relating field strength to the various colors obtained in the Kirlian process. These predictions agree well with our experimental results, and also with the descriptions of visible corona given by other investigators.

Using the corona theory, we can now predict and reproduce the Kirlian images obtained in a given experiment. Hence, we feel certain that the Kirlian images we and others have obtained are a result of electrical corona and not in-

duced radiation from the body or the film. Calculations show that even if the various mechanisms in the human organism which could emit IR, visible, and UV radiation were triggered, the intensity of this induced radiation would be so low as to be completely absorbed by the corona radiation.

To date, most reported research with Kirlian photography has used an AC electric field. Usually these AC sources were of an inductive discharge nature, such as Tesla coils and ignition systems. Seldom, if ever, were the waveshapes reported, and in the case of certain Tesla coils, frequency reports were misleading. As a consequence, it is virtually impossible to repeat any reported experiments with certainty.

Since we have shown in our experiments that electrical corona is the basic mechanism of the radiation production in the Kirlian process, we are able to present an optimum Kirlian photographic system which will offer several advantages to the parapsychological researcher. Briefly, this system results from consideration of the following facts: High frequency AC currents are inherently safer than DC currents. DC corona allows easier interpretations of visible corona data. Corona production is quite sensitive to the shape of the applied wave-front of the electric field. Tesla coils, spark systems, and other inductive discharge systems do not in general give consistent outputs with respect to frequency stability and amplitude. Corona production is influenced by factors such as humidity, pressure, gas composition, and background radiation.

In our system we use a variable 60 KV DC power supply with regulation better than one percent. The potential is pulsed by means of a vacuum relay operated by an adjustable timing circuit. The high voltage is applied to a 1/16 inch copper clad fiberglass circuit board. Return current from the object being photographed is monitored simultaneously with the applied potentials.

Using this system, we have been able to explain and verify several reported phenomena. Three subjects were used in our experiments; the results were the same for all three. Indeed, a predominantly blue Kirlian photograph does indicate a relaxed emotional state, and a high GSR which correlates well with this phenomenon offers additional support for this statement. As a person becomes emotionally aroused, the Kirlian photograph will change to a predomi-

nance of red. This is the result of two major changes. First, the lower skin resistance of the person makes the body a better conductor, consequently tending to increase the electric field in the surrounding air. The increased sweating increases the concentration of sodium in the air, also tending (by virtue of the effect of sodium vapor on the first and second Townsend ionization constants) to increase the electric field. However, under the increased electric field, local ionization of gases creates a space charge which drastically reduces the local electric field seen by the gaseous molecules. The overall effect is the shift in radiated color from blue (indicating a high local field strength) to red (indicating a low local field strength). Of course, mixtures of these two extreme states are possible. Likewise, the emission of other wastes from the body via the sweat glands, or the presence of foreign substances, could lead to different emission spectra, and offer a tentative explanation of the elusive green and yellow Kirlian photographs reported from the U.S.S.R.

From the experiments we have performed, we cannot say that IR, visible, and UV radiation from the body never occur. There are numerous mechanisms within biological tissue, including the energy release mentioned earlier, which could account for such radiation should it be detected. Indeed the physical or emotional state of the subject may determine whether or not there is radiation from the body. Our experiments do, however, show that electrical corona is the mechanism by which the predominant radiation is generated in the Kirlian photographic process. Thus Kirlian photography in itself is no longer a process which can be called paranormal. Kirlian photography may, however, find a use in parapsychological research as a detection mechanism. The process is sensitive to a large number of physiological changes in the human body, and thus could be used as a fast indicator of physiological changes occurring in the subject. Such a device would allow screening of sensitives during "fishing expedition" experiments and would give some indication of the probability of success of more elaborate, specialized monitoring devices.

The fact that the Kirlian process depends upon a number of physical parameters of the medium surrounding an object allows the possible use of the technique to investigate changes in an area's physical environment. An obvious example of this would be in the investigation of out-of-body experiences and hauntings. The Kirlian process has possi-

bilities, but there are certain disadvantages to its immediate use in any area. It is a new process. Consequently, there are virtually no data for comparison of experimental results. Also, in order for results to be reproducible, the process will require a more elaborate equipment setup than is now being used by most investigators.

OBJECTIVE DETERMINATION OF STIMULUS INCORPORATION IN ESP TASKS WITH PICTORIAL TARGETS

Charles Honorton (Maimonides Medical Center)

Free-response methods seem to yield consistently stronger psi effects than forced-choice methods in terms of the number of trials required for statistical significance, probably because they are associated with greater emphasis upon the individual target and are more often employed in conjunction with techniques designed to optimize the subject for psi retrieval. Yet at the same time, the standard blind-judging procedures used in free-response studies are extremely wasteful of data.

Typical free-response methods involve blind judgment of similarities (correspondences) between a series of targets and subject reports. Correct identifications ("hits") have a probability of occurrence on each trial of $1/N$, where N is the number of matching items in the series. While blind-judging methods control against judge-bias which could artifactually inflate target-mentation correspondences, they are grossly insensitive to genuine effects, since a perfect correspondence receives the same weight as a partial correspondence. Such procedures, in addition, are time-consuming and provide no quantitative data concerning the information content of the target, the information content of the subject's mentation report, or the information content which is shared by the target and mentation report.

The new method proposed here provides an objective basis for quantitatively describing the information content of the target, the subject's mentation report, and the amount of correspondence between them. It permits description of information content in digital (binary) terms, eliminates the

need for judges and target pools, and provides the possibility of statistical significance for a single target trial.

This method requires construction of a special target series consisting of permutations of content in ten fixed content categories such that each target picture represents a different permutation of the ten categories, 1024 target pictures in all. The categories are: nature scenes, architectural objects, body parts, food, artifacts/implements, human characters, animals, mythical characters, activity, and color. They have been adapted from Hall and Van de Castle's system for content analysis of dreams, from which specific criteria defining permissible content for each category have been developed by Ellen Messer of the Maimonides laboratory.

The ten categories can be coded for each target on a binary basis (content present = 1, content absent = 0). The target picture having no content (i.e., blank) is coded in binary terms as 0000000000; the one containing items of content in each of the ten categories is coded 1111111111. The total target series must contain one each of the 2^{10} (i.e., 1024) target pictures. The categories and binary scoring system can be illustrated by coding a sample painting. Van Gogh's "Portrait of Lt. Milliet," portraying a bearded army officer in color, would be coded in binary terms as 1000100000. If this picture were black and white, it would be coded as 0000100000. If it were in color and included animals and mountains in the background, it would be coded as 1001100001.

The 1024 target pictures are constructed from visual material of every type, including magazine photographs and illustrations, cartoons, art prints, merchandise catalogs, comic books, and so on. We are currently in the process of selecting these pictures. Each potential target picture is screened by three judges who independently code its content in terms of the presence or absence of content in each of the ten categories. In some cases, original pictures are edited to eliminate extraneous or ambiguous content, or embellished to add content items required for a particular coding. In order for a given picture to qualify as a target in the digital series, the three judges must reach unanimous agreement as to its content (i.e., the same ten-digit binary number). All targets accepted on this basis are transferred to two-by-two inch slides, with a reference number across the cardboard casing providing access to the digital code of

its information content. Several judges will also rate, on a one to ten scale, each target picture for degree of emotionality or affect, and will give each a z-score based on this rating. Targets for specific experimental sessions may be selected by electronic random generators, random number tables, or in other ways, from the total series of 1024.

During an experiment the subject's free-response mentation is sampled as desired (for example, by dream reports, hypnotic imagery, or free association). At the completion of the report period, the subject is asked to code his or her mentation according to the presence or absence of content in each of the ten categories which characterize the target population, and is provided with explicit criteria for doing so. Thus, free-response mentation may also be expressed as a ten-digit binary number.

It is important to note that since the target series is constructed such as to guarantee intercategory independence, no assumption need be made concerning the independence of content items in the subject's mentation report. Given independence of the target items, it has been demonstrated that a subject's response bias can only reduce the likelihood of significant departures from binomial expectation, a conservative error.

Each target trial thus constitutes ten independent binary trials with a scoring range of zero to ten, and a mean chance expectation of five. If the subject's coded mentation report corresponds completely with the coded target picture (that is, results in the same ten-digit binary number), each of the ten binary trials counts as a "hit" and the associated probability for that target trial is 1/1024, or $P < .001$. Similarly, nine out of ten target-mentation correspondences are associated with $P = .011$, and eight out of ten with $P = .055$.

To illustrate, consider the earlier example, van Gogh's "Portrait of Lt. Milliet," the content of which was coded as 1000100000. During a hypnotic dream study I did with John Stump, the following imagery was reported by a subject in a session in which this painting was used as a clairvoyant target: "I had the impression that this was a close-up of a face--probably that of a woman or child, half profile, facing to the right. I tried to see more and I see kind of an orange and gold cobble."

According to the binary coding system, this report contains items from two of the content categories: human characters and color. The report is thus coded as 1000100000, which is a perfect match with the target. Assuming independence of items in the target, this target trial would be associated with $P < .001$. Using conventional blind-judging procedures and a conventional target pool, it was associated with $P = .25$. In fact, the overall significance of the 28 target trials in this study was only $P = .016$, by the conventional blind-judging procedure.

7

PSYCHOKINESIS WITH HUMAN SUBJECTS*

OBSERVATION OF SUBCONSCIOUS PK EFFECTS WITH AND WITHOUT TIME DISPLACEMENT

Helmut Schmidt (Institute for Parapsychology, FRNM)

The existence of PK and its largely subconscious operation suggest that a researcher who studies some subtle statistical process may become unwittingly more than a passive observer and may change the process by some PK mechanism. In order to demonstrate such an effect explicitly a number of human subjects were placed in the role of the observing scientist. These subjects had to record barely audible sound signals which were presented through headphones at random time intervals. I hypothesized that the subjects' eager and expectant attention to each signal might affect the underlying random process, leading to an increase in the average signal frequency (i.e., number of signals per unit time).

For a better understanding of the subconscious PK effect and the range of its practical implications one has to study the psychological and physical conditions under which the effect may occur. The second part of this study is concerned with the specific question of whether the PK effect would disappear when the signals were presented to the subject only indirectly, through a prerecorded tape. For this purpose randomly-spaced signals were generated and recorded on magnetic tape in the absence of a subject or experimenter, and only several days later did the subjects listen to the weak signals obtained by a playback of the tape. Thus, when the subjects entered the picture, the spacing of the signals seemed no longer susceptible to a PK influence. But

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even in this case a PK effect was found. This result appears to be in agreement with earlier experiments which showed that psi is to a surprising degree independent of space and time limitations. The result is also consistent with my mathematical psi theory described in an earlier paper. More experiments might be required, however, until we can feel confident in committing ourselves to any particular interpretation of the reported experimental results.

In order to obtain randomly-spaced signals one could mount a weak radioactive source next to a Geiger counter and make an arrangement so that each arriving decay particle triggers the generation of the signal. Then, under ideal conditions the signals would arrive at random times, in the sense that the signals are statistically independent and that the probability for a small time interval to contain a signal is proportional to the length of this interval. Such an arrangement has, however, some minor technical disadvantages, such as the need for frequent re-calibrations of the basic counting rate, and the need for shielding the Geiger counter against radiation from radioactive watch dials. Therefore in the present experiment a digital random time generator was used which has practically the same randomness features as the above apparatus under ideal conditions.

When this digital generator is triggered to produce a time interval of random length, an electronic display counter starts advancing from zero, at the rate of ten steps per second. After each step, the internal random device (a "64-sided electronic die") decides whether to stop the counter (probability 1/64) or to proceed to the next step (probability 63/64). Thus the counter stops after an <u>average</u> of 64 steps or 6.4 seconds, but the actual running time varies randomly. This process constitutes one run. When the counter stops a barely audible sound is presented to the subject. The subject does not see the display counter, so all he is aware of is the sound.

At the beginning of the experiment the theoretically expected average run length of 64 steps was experimentally verified. For this purpose 20,000 time intervals were automatically generated and the total number of counter steps was recorded. The average run length was found to be 63.8 steps (6.38 seconds), not significantly different from chance. A similar randomness test was made between the first and second part of the study, with an average run length of 63.5 steps, again within the range of chance fluctuations.

The first part of the study comprised a pilot and a confirmatory series. After some exploratory studies I decided to have 20 subjects contribute 20 test runs each to the pilot test. These subjects were members of the laboratory or visitors who expressed interest in the test. Acting as experimenters were myself or one of two assistants. The task, as presented to the subject, was to perceive very weak clicks presented through headphones. The challenging nature of the task, the need for extreme alertness without physical tension, and the value of cultivating the sense of hearing were mentioned to the subject. He was told that he would learn all the details of the test at the end, but some subjects might have guessed that, in some way, psi was involved in the test. Next, the sound volume was individually adjusted so that the signals were barely audible and the subject missed at least one but not more than three signals in 10 signals presented. No attempt was made to shield the room against external sounds.

At the beginning of each of the 20 runs the experimenter told the subject to be ready and activated the random generator, whereupon the display counter started advancing and stopped after a random number of steps. At this time the signal was presented to the subject. The average run length over the 400 runs was 54.2 steps, significantly less than chance expectation of 64 steps (CR = 3.34; P = .001). Thus it appeared that in their eager concentration on the next signal, the subjects had subconsciously mobilized a PK mechanism to make this signal arrive sooner than expected by chance.

For the confirmatory test the recording of the results was automated and some other changes were made in an attempt to reduce the experimenter involvement. The only task of the experimenter, apart from instructing the subject, was to push a button at the beginning of each group of ten runs. Then, automatically, the turning off of a signal lamp indicated the beginning of the first run to the subject and the display counter started advancing. As before, an auditory signal was presented to the subject at the end of the run. The subject was instructed to register the signal by a pencil mark on a note pad. This instruction was given only to distract the subject from the PK aspect of the experiment, and the pencil marks were not evaluated. A few seconds after the arrival of the sound signal, the signal lamp was turned on so that the subject could tell whether he had missed the sound signal. At this time the counter reading was record-

ed on paper punch tape for later computer evaluation. Shortly afterwards the turn-off of the signal lamp indicated the beginning of the next run.

The prespecified number of 30 subjects (all were visitors to the laboratory who expressed interest in the experiment) was tested under these conditions by myself as experimenter. Each did 20 runs. A rather quiet room was used for this test. The subjects sat in a comfortable chair, usually very still because body movement produced enough noise to mask the signals. The average run length over the 600 runs was 55.3 steps, significantly below chance expectancy ($CR = 3.59$; $P < .001$). Thus the randomness of the arriving signals was again significantly affected. A t-test showed the consistency of the effect over the 30 subjects ($t = 3.62$, $df = 29$; $P < .001$).

The second part of the study investigated PK effects under time displacement. The psychological conditions in this and the previous test were very similar. There was, however, a basic physical difference. In the previous study, the random decisions determining the length of a run were made while the subject was actively waiting for the outcome. In the present test the generator was activated in the absence of subject or experimenter, at a time when the subject did not even know that he would participate in the experiment. The decisions made by the generator were stored on magnetic tape and only later played back to the subject.

Let us look at the steps of the test procedure in their proper time sequence: first, the random generator was activated automatically in the absence of subject or experimenter to produce a group of 60 test runs, which were then labelled A. The runs were separated by intermissions of a few seconds. Each counter step in a run was recorded on one of two channels of a magnetic recording tape, and the end of each run was recorded by a signal on the other tape channel. The counter readings at the end of each run (the number of steps in the run) were also printed on paper punch tape. Thus we had two independent records of the length of each run. In the same manner another 60 runs, labelled B, were recorded on a magnetic tape, and a corresponding paper punch tape.

These records were left untouched until two days later, when the experimenter learned which of the two groups was to provide the test tape and which the control tape. The

predetermined but previously unknown decision was made by calculating the square root of n with n = 2 on a 12-digit calculator, so that group A provided the test tape if the 12th digit was even. For decisions on later groups, n = 3, 5, 7, and other successive prime numbers were used.

Next, the first subject was selected and the test tape (A or B) was played back while he listened for the weak signal at the end of each run from one of the tape channels. The indicator lamp, lit at the beginning of a run, had the same function as before. The individual counter steps as recorded on the other tape channel served to advance a display counter so that the experimenter could manually record the length of each run. This manual record (which could have been easily rechecked by another tape replay) agreed with the independent record available from the paper punch tape. A minor psychological difference between this and the previous test was that the subject did not have to make checkmarks, but only told the experimenter of the arrival of the signal.

After three subjects had participated in this manner (20 runs per subject), the paper punch tape records for the tests and for the controls were evaluated by a computer which printed out the lengths of the individual runs. This procedure was repeated until the predetermined number of 30 subjects (members of the laboratory and visitors who expressed interest in the test) had participated in 20 runs each. Remember that the test runs and control runs were prepared under the same conditions, at a time when the experimenter did not yet know which runs were to serve as tests and which controls.

The following results were obtained: for the test runs, an average run length of 56.4 steps was found. This was significantly (CR = 3.14; $P < .001$) below the chance expectancy of 64 steps. Furthermore, a t-test showed the effect to be consistent over the 30 subjects ($t = 3.66$, $df = 29$; $P < .001$). For the 600 control runs, which were not displayed to a subject in a psychologically challenging situation, but only presented to the experimenter in the form of a computer printout, the average run length was 62.9 steps, not significantly different from chance expectancy (CR = 0.4). A t-test for the 30 blocks of 20 runs each gave $t = 0.5$ ($df = 29$), which again was not significant.

In the context of my recently-developed mathematical

psi model theory, one might tentatively interpret the results as follows: Just as, in a precognition experiment, a later random event affects the earlier state of mind of a subject, in the present experiment the subject's mental effort affected an earlier random event. We can not dismiss the possibility that it was the experimenter who subconsciously produced the effect at the time the random events were generated and recorded, but this issue might be easily resolved after more detailed studies of PK effects under time displacement have been made.

8

OUT-OF-BODY EXPERIENCES*

ESP AND OUT-OF-BODY EXPERIENCES: THE EFFECT OF PSYCHOLOGICAL SET

John Palmer† (University of Virginia) and Ronald Lieberman (Goddard College, Plainfield, Vt.)

In a previous experiment reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 38-41], J.P. and Carol Vassar found that subjects reporting out-of-body experiences (OOBEs) resulting from a special induction procedure scored significantly below chance on a free-response clairvoyance task. It was suggested that this negative scoring might be attributable to the fact that the subjects were given an "active" set (to go out and "look at" the target picture) as opposed to the more common "passive" set (to allow an image of the picture to enter their consciousness). We decided to test this hypothesis by contrasting two experimental conditions. The "OOBE condition" (active set) was essentially the same as in the previous experiment. The "ESP condition" (passive set) employed the same induction procedure, except that the subjects were told nothing about OOBEs. Instead, they were told to allow imagery to enter consciousness passively, and that the purpose of the induction procedure was to make it easier for them to do this.

This design also gave us an opportunity to explore the importance of psychological set in eliciting reports of OOBEs following the induction procedure. To the degree that such a set is a necessary factor, there should be more reports of OOBEs in the OOBE condition (where the set is present) than in the ESP condition (where the set is absent). Finally,

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we wished to explore further two significant correlational findings from the previous experiment. These were an "inverted U" relationship between ESP and expectancy of ESP success prior to the induction, and a positive relationship between ESP and the Betts Vividness of Imagery Scale. The latter finding may have been artifactual, because subjects received their ESP scores prior to taking the Betts. As a check on this possibility, half of the subjects in the present experiment received their ESP scores after taking the Betts, and half before.

The sample consisted of 40 subjects, most of whom were first-year students at the University of Virginia. To minimize their knowledge of the two conditions of the experiment, we recruited subjects from a set of undergraduate dormitories divided into two sections. Subjects recruited from one section were automatically placed in the OOBE condition and those from the other section in the ESP condition. Four subjects who were not undergraduates were placed in the OOBE condition, by virtue of prior arrangements.

Immediately before the experiment all subjects were given a description of the procedure and an explanation of why inducing an altered state of consciousness (and, in the OOBE condition, inducing an OOBE) should facilitate ESP. The briefing in the OOBE condition was identical to that used in the previous experiment. The induction procedure was essentially the same as in the previous experiment, except for one revision. Pilot testing had indicated that the spiral disc used in the previous experiment was not necessary for eliciting reports of OOBEs. Therefore, a "Ganzfeld" was substituted in this experiment. It consisted of placing halves of ping-pong balls over the subject's eyes and having him stare into a white light. It was hoped that the Ganzfeld might facilitate the visual component of the OOBE, which in the previous experiment was not as strong as we would have liked.

After the briefing, the subject was led to the laboratory, seated in a reclining chair, and the ping-pong balls were attached. The first stage of the induction procedure was an adaptation of Jacobson's progressive relaxation technique. The room was darkened during this stage, as in the previous experiment. The relaxation exercise was followed by the instructions for the second stage, which differed for the two conditions. Subjects in the OOBE condition were asked to imagine themselves travelling through the white space they would see in front of them and into the adjacent

room where they were to identify a magazine picture placed on a table. Subjects in the ESP condition were asked to allow imagery to enter their consciousness passively, and to suggest to themselves that this imagery would correspond to the magazine picture.

The second stage was identical for both conditions. The white light was turned on at the beginning of this stage and the subject was asked to open his eyes and stare straight ahead into it. The auditory stimulus was identical to that used in the previous experiment. It consisted of a pulsating 350 Hz sine wave played simultaneously through headphones and the speaker of a cassette recorder, with amplitude modulation through the former only. The second stage lasted approximately nine and a half minutes, as in the previous experiment. The instructions, the relaxation induction, and the tone were all tape-recorded.

The pool of ESP targets consisted of ten sets of five magazine pictures each, selected from a photography magazine. The set of targets to be used for each subject and the picture within each set which was to be the target for that subject were determined in advance from a random number table. Prior to the induction procedure, J.P. removed the target from its packet and placed it face-upward on a large wooden table in the outer room. Following the final stage of the induction procedure, J.P. returned to the outer room, replaced the target picture in its packet in its previous position, and left the whole packet on the table. J.P. then left the room, and R.L., who did not know which picture was the target, conducted the rating task.

The subject was shown the five pictures and was asked to rate each picture on a 31-point scale according to how well it corresponded to his impressions during the second stage of the induction procedure. His ESP score was a z-score obtained by subtracting the average rating of all five pictures from the rating given the target and dividing by the standard deviation of all five ratings. Before he made his ESP ratings, the subject was asked to fill out an extensive questionnaire dealing with his reactions, experiences, and expectancies at various stages of the experiment. Following the ESP ratings, the subject was administered a shortened form of the Betts QMI Vividness of Imagery Scale and a Draw-a-Person Test, which has not yet been scored. For alternate subjects, these scales were administered before or after J.P. returned to inform the subject of his ESP score.

The mean ESP scores in the two conditions did not differ significantly from chance (zero) or from each other (OOBE: +0.19; ESP: +0.11). The difference between the two conditions in subjects' mean ratings of the amount of effort they exerted in trying to identify the ESP target, although in the proper direction, was not as large as expected (OOBE: 1.95; ESP: 1.55; nonsignificant). This finding suggests that the two psychological sets may not be as different on the active-passive dimension as was originally thought. However, there was a strong tendency for more subjects to report OOBEs in the OOBE condition than in the ESP condition. In the OOBE condition, 13 of 20 subjects answered "yes" to the question: "Did you at any time during the experiment have the feeling that you were literally outside your body?" In the ESP condition, only 4 of 20 subjects answered "yes." This difference is associated with a corrected chi square value of 6.55 ($df = 1$; $P < .02$).

As a more refined measure of subjects' belief that they had OOBEs, those who answered the preceding question "yes" were asked to rate on a four-inch continuous scale how confident they were that they had literally left their bodies. The mean rating was 2.41, which is slightly beyond the midpoint of the scale in the direction of strong conviction. Subjects in the OOBE condition also had a greater expectancy of success on the ESP test prior to the induction than did subjects in the ESP condition ($t = 2.41$, $df = 32$; $P < .05$), which suggests that the explanation given the OOBE subjects during the initial briefing of why having an OOBE should facilitate ESP was at least somewhat effective in bolstering their confidence.

Correlations between ESP scores and other variables were first computed separately within each condition. If these pairs of correlations did not differ significantly, the data were pooled and the correlations recomputed across conditions; i.e., containing subjects from both conditions. The only variable for which significantly different correlations with ESP scores were found in the two conditions was expectancy of success on the ESP test prior to the induction (OOBE: +.38; ESP: -.55; $z = 2.69$; $P < .01$). However, since mean expectancy differed significantly in the two conditions, it was also possible to interpret this finding as a curvilinear relationship across the two groups combined, as will be discussed later.

The 17 subjects in both conditions who reported

OOBEs had a mean ESP score of +0.44, which is significantly above chance ($t = 2.60$, $df = 16$; $P < .02$). The other 23 subjects had a mean ESP score of -0.06. These two means are significantly different ($t = 2.11$, $df = 38$; $P < .05$). A somewhat surprising post hoc finding was a rather strong tendency for subjects who had never read anything about OOBEs before coming into the experiment to score more positively than subjects who had read about OOBEs previously ($t = 3.05$, $df = 38$; $P < .01$).

For subjects who received their ESP scores before taking the Betts imagery scale, there was a significant positive correlation between imagery and ESP ($r = +.59$, $df = 18$; $P < .01$), which is comparable to the +.35 correlation found in the previous experiment. This relationship did not appear, however, for subjects who only received their ESP scores after they had completed the Betts ($r = -.17$; $P =$ nonsignificant), and the two correlations differ significantly ($z = 2.50$; $P < .02$). This finding strongly suggests that the correlation found in the previous experiment was artifactual. It would appear that knowledge of ESP scores created either differential demand characteristics or changes in mood that affected subsequent performance on the Betts.

Perhaps the most noteworthy outcome of the present experiment is that it represents a significant reversal of the ESP results of the previous experiment with respect to two separate predictors: reports of OOBEs and expectancy of ESP success prior to the induction. To demonstrate these reversals, analyses of variance were computed comparing the results of the two experiments, using "experiments" as one of the factors. As neither of these predictors showed significant mean differences between the two experiments, it was possible to treat them as orthogonal factors with "experiments" in factorial designs.

Persons reporting OOBEs scored below chance in the previous experiment and above chance in this experiment. This reversal is reflected in a significant interaction in the analysis of variance ($F = 6.33$, $df = 1, 86$; $P < .02$). In the previous experiment, there had been a significant "inverted U" relationship between ESP and expectancy of success prior to the induction. An almost exact mirror image of this relationship appeared in the present experiment. This reversal is reflected in a significant interaction in a second analysis of variance ($F = 5.27$, $df = 2, 87$; $P < .01$). It is important to note that expectancy of success and report of an

OOBE were linearly and curvilinearly independent in both experiments. Thus, the reversal appeared with respect to two independent predictor variables.

We have no logical explanation for these reversals. The main difference between the two experiments (outside of the differences between the two conditions of the present experiment, the results of which were pooled for the analyses in question) was the use of the spiral disc for the visual stimulus in the previous experiment and the Ganzfeld in the present experiment. The coexperimenter was a female in the previous experiment and a male in the present one. The likelihood of this latter difference being important, however, is lessened by the fact that there were no significant sex differences in either study.

A STUDY OF THE PHYSIOLOGICAL VARIABLES ASSOCIATED WITH OUT-OF-BODY EXPERIENCES

John Hartwell, Joseph Janis†, and Blue Harary (Psychical Research Foundation)

This paper describes the physiological state of one of the authors (B.H.) during a series of experiments in which he voluntarily induced what is commonly termed an out-of-body experience (OOBE). While B.H.'s physiological variables were being monitored, experiments designed to detect his self-described OOBE presence in another building were also being carried out. Preliminary results of both the physiological study and the detection study were presented at the 1973 Parapsychological Association convention [RIP 1973, pp. 36-7]. This paper describes further analyses done in an effort to identify and describe psi-conducive states and their possible relevance to the survival problem. A complete physiological description of the OOBE state would appear to be of central importance to future work in the survival area.

Physiological data were gathered on 13 evenings over a period of about three months. The following measures were taken: EEG (electroencephalogram; left and right occipital each referenced to ipsilateral central, according to the international 10-20 system); EOG (electrooculogram; right eye as referenced to the right mastoid); EMG (electromyo-

gram; bipolar from electrodes on the chin); skin potential (DC level from palmar surface of the left hand as referenced to the left forearm); heart rate and blood pulse amplitude (from a transmission-type digital plethysmograph affixed to the index finger of the right hand); and respiration (from a strain gauge positioned about the chest).

All of these measures were recorded on a 12-channel Grass Model Seven polygraph along with a rectified and integrated version of the EMG data, a frequency-limited (2-7 Hz) version of one EEG channel, and several timing and event marking signals. Six channels of data along with one timing signal were also recorded on an FM tape recorder to permit automated analysis later.

In a typical session, B.H. reclined within a large soundproof testing chamber after being fitted with the appropriate electrodes. The chamber was completely darkened, and low-level white noise was provided by the voice intercom system. The experiment proceeded through several distinct stages during each session. The initial baseline period was followed by a relaxation and preparation period of two to five minutes. This was referred to as the First Cool Down Stage. When B.H. had entered a state he recognized as conducive to an OOBE, he would signal by saying "soon." This event was clearly recorded on the polygraph by the EMG measure derived from the chin. It marked the end of the First Cool Down and the start of the First OOBE Stage. The word "back" similarly denoted the end of the OOBE. Later in the session this process was repeated, producing a Second Cool Down Stage and a Second OOBE Stage.

Several types of multivariate analysis, including analysis of variance, were applied to the data. Each analysis showed that the OOBE Stage differed from the Cool Down Stage in three of the variables measured. For the 13 experimental sessions respiration rate and heart rate showed a statistically significant increase from the Cool Down Stage to the OOBE Stage, while skin potential decreased significantly. Large increases in blood pulse amplitude occurred too early within most Cool Down periods for consistent detection by the various analytical procedures. The EEG data were analyzed for both alpha density and frequency for both the left and right hemispheres. No statistically significant differences were found between the Cool Down and the OOBE Stages. The EMG and EOG data similarly showed no significant differences for this experiment. It was noted that

none of the variables showed a significant difference between the First and Second Cool Down Stages nor between the First and Second OOBE Stages.

The state achieved by B.H. during these experiments is marked by deep relaxation. His day-long preparation prior to the experimental session is consistent with this result. He generally arose around noon, and spent the day quietly and without eating prior to the evening's experiment; he was, however, temporarily aroused by conversation preceding each Cool Down Stage, and the physiological measures reflect this. Several measures showed a rapid transition to very relaxed values during the first 30 seconds of the Cool Down Stages. Skin potential, for instance, declined by approximately 15 millivolts during this time, while vasodilation increased substantially. Respiration was marked by several large slow breaths at the start of the Cool Down Stage, and became shallow and extremely regular by its end. A small but significant increase in heart rate is attributed in part to the sinus arrhythmia accompanying the large breaths, and in part to a parasympathetic response to decreased blood pressure arising from the large vasodilation.

The OOBE state achieved by B.H. differs from sleep in a number of the measures taken. The EEG tracings during the out-of-body experience were read as normal, waking, eyes-closed by an experienced clinical electroencephalographer. The special low-frequency trap did not show an increase in activity that would be associated with the deeper sleep stages. When averaged over all sessions, eye movements actually decreased during the OOBE Stage. Neither they nor the high alpha content of the EEG seem compatible with light sleep or REM dream periods. The EMG recordings indicate that muscle tonus was maintained at relaxed levels. They differ in this respect both from REM periods and from states of deep anesthesia. All-night sleep recordings have been made on several occasions since the OOBE experimental data were gathered. Normal sleep patterns including REM periods were observed.

The state achieved by B.H. for these experiments is interpreted as deeply relaxed, but waking. The Cool Down and OOBE Stages show a high degree of consistency over the 13 experimental sessions, and differ from each other significantly in several of the measures taken. No unique physiological indicators of the OOBE state emerged in this study. All of the significant changes were gradual and occurred prior to the onset of the experience.

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SPONTANEOUS CASE TRENDS*

A COMMUNITY MAIL SURVEY OF PSYCHIC EXPERIENCES

John Palmer† and Michael Dennis (University of Virginia)

There have been a number of surveys of spontaneous psychic experiences reported in the parapsychological literature. However, all of these surveys involved selected samples which might be atypical of the population as a whole. Louisa Rhine, for example, bases her findings on people who voluntarily mail descriptions of their experiences to her as a result of general appeals in the media. Other studies, such as the British Census of Hallucinations and several surveys of out-of-body experiences, involved asking questions about specific psychic experiences to more or less intact groups like classes of college students or persons from a particular social class. Perhaps the most representative study is an unpublished national interview survey of mystical experiences by Andrew Greely and William McCready of the National Opinion Research Council, which unfortunately dealt only superficially with psychic experiences.

As a national survey was beyond our resources, we decided to make a community questionnaire survey of Charlottesville, Virginia, and surrounding suburbs. Although this sample was also self-selected to the extent that some persons declined to return their questionnaires, we feel our data are more representative than most reported in the parapsychological literature.

Our initial sample consisted of 1000 persons randomly selected from two sources. Seven hundred persons were selected from the 1973 Charlottesville City Directory. The

*Chairman: William Joines, Duke University and PRF.

names in the portion of the Directory we used were obtained by door-to-door canvassing of all numbered street addresses in Charlottesville and surrounding suburbs. All persons over 18 years of age living in each household were included in the Directory. The other 300 names were selected from the 1973-1974 University of Virginia Student Directory, which proved to be a more complete and up-to-date source of student names than the City Directory. We excluded student names from the sample we took from the City Directory. The 300/700 ratio of students to non-students reflects our best estimate of the proportion of students to non-students living in the area we sampled.

The questionnaire consisted of 46 questions, all of which could be answered by circling the appropriate response. The questions can be placed in five categories: Demographic: sex, race, age, family size and birth order, political and religious beliefs, occupation, level of education, family income, marital status. Experiences and practices of possible relevance to psi: recall and vividness of dreams, lucid dreams, mystical experiences, self-analysis of dreams, meditation, visits to psychics, drug use. "Psychic" experiences: déjà vu, psychic dreams, waking ESP experiences, apparitions, out-of-body experiences, seeing the "aura," memories of previous lifetimes, poltergeists, hauntings, subject of others' psychic experiences (that is, agent). Attitudes: toward astrology, survival of death, reincarnation, parapsychological research. Sociological questions: effect of psychic experiences on personal attitudes and actions, whether a psychic experience had ever saved the respondent or someone he knew from a tragedy or death.

Many of the questions, especially those asking about psychic experiences, had from one to seven subquestions which the respondent was to answer only if he had ever had the experience in question. These questions asked such things as how frequently the respondent had this type of experience, and how many of these experiences had certain characteristics (for example, in how many out-of-body experiences did the person see his physical body; in how many psychic dreams did he tell someone of the dream before learning of the verifying event).

On March 1, 1974, one copy of the survey, along with a postage-paid "business reply" return envelope, was sent to each of the 700 persons sampled from the City Directory. The first mailing to the 300 students was on March 11.

There were two additional follow-up mailings to persons who had not yet returned their questionnaires. Each mailing occurred three weeks after the preceding one. It consisted of a new copy of the questionnaire, a new return envelope, and a supplementary letter exhorting the person to return his completed questionnaire. A fourth postcard mailing was then sent to those persons who still had not returned their questionnaires. The postcard listed a number of reasons why people might not have filled in their questionnaires and asked them to check those which were applicable to them. Each questionnaire had a three-digit code number stamped on the lower right-hand corner of the back page. This number keyed the person's name on our mailing list. When a questionnaire was returned, this number was circled on the mailing list, and the date we received it was recorded. If a questionnaire was returned to us by the post office as undeliverable, or if someone else returned it indicating the person was deceased, a new name was sampled and the questionnaire was sent to the new individual. This was done only following the first two mailings. If a person returned an uncompleted questionnaire or indicated a refusal to cooperate, we simply treated that person as a "no-return" and did not re-sample.

We received usable questionnaires from 89 percent of our student sample and 51 percent of our town sample. Although the representativeness of our final town sample might be questioned, we are encouraged by two factors. First, the percentages of townspeople reporting various types of psychic experiences were very similar to the percentages for students. Second, there were no significant tendencies on questions involving psychic experiences for persons returning their questionnaires after three proddings to respond differently than persons who sent them back right away.

Déjà vu experiences were reported by 68 percent of the townspeople and 88 percent of the student sample. This difference can be attributed to the age variable: 84 percent of the townspeople under 30 claimed to have had the experience. A significantly greater percentage of women than men reported waking ESP experiences in the student sample (48 versus 34) and in the two samples combined (45 versus 35). There were no significant sex differences for any of the other basic psychic experiences assessed.

The question on apparitions was almost identical to that used in the Census of Hallucinations and in a follow-up

study in the 1940s by D. J. West. Our percentage of positive responses (17) is slightly higher than (although comparable to) the ten and 14 percent figures reported in these earlier surveys, and we did not attempt to screen out questionable reports as they did. Our findings agree with theirs in revealing a greater incidence of apparitions reported by females, but our sex difference, while comparable in magnitude to that found in the Census, was not statistically significant because of our smaller sample size. The most striking difference between our findings and the earlier ones was that, while the experiences reported in the earlier surveys were predominantly visual, ours were more equally balanced, with the tactile and auditory modes actually being most prevalent.

A greater proportion of students (25 percent) than townspeople (14 percent) reported out-of-body experiences, perhaps because this experience was significantly related to the use of "mind-expanding" drugs, and a greater proportion of students than townspeople reported using such drugs. The only other psychic experiences significantly related to drug use were seeing the "aura" for the student sample, and poltergeists and hauntings for the townspeople.

The poltergeist results do not support the conclusion that teenagers are overrepresented in such cases. However, our percentages are based on a very small number of cases about which we have little detailed information. There was a strong tendency for the same people to report the various types of psychic experiences, and for psychic experiences to be most prevalent among persons reporting good dream recall, vivid or lucid dreams, and mystical experiences. Finally, it must be emphasized that our results reflect claims of psychic experiences. How many of these experiences involve genuine paranormal processes is a question that cannot be answered by the data at hand.

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SYMPOSIUM:* ENERGY FOCUSING AND LINGERING EFFECTS IN POLTERGEIST CASES AND EXPERIMENTAL STUDIES

Introduction

William Joines (Duke University and Psychical Research Foundation)

In this symposium some experimental findings will be presented which lend further support to observations that have been made by many researchers: PK and ESP effects may be focused and may linger within physical areas. An example of area focusing would be a poltergeist case where objects within one area of a room are repeatedly disturbed while the same kinds of objects in another part of the room are never disturbed. An example of the linger effect would be the situation in which a PK subject deflects a compass needle by 20 degrees, the deflection remains until the compass is removed from the immediate area, and the needle returns to the deflected position when replaced in the original area. The focusing effect seems to be independent of time, while the linger effect may last for a few seconds or several minutes.

EARLIER RSPK CASES

W. G. Roll (Psychical Research Foundation)

In three early poltergeist cases (also called Recurrent

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Spontaneous Psychokinesis, or RSPK), my co-investigators and I found that there was a tendency for the phenomena to concentrate on certain objects, on certain areas, or on certain types of objects. This focusing effect was but one of the patterns that we found. The most striking pattern was the attenuation or decrease in the number of incidents with distance from the agent; this pattern has recurred in a number of cases. We don't know whether it is typical of PK in general, or something peculiar to these unusual household disturbances. We were not able to carry out any statistical study of the apparent focusing effect in these three early cases, which were the Seaford case that I investigated jointly with Dr. J. G. Pratt, the Indianapolis case, and the Newark case.

There were two more recent cases, the Miami and Olive Hill cases, which have been described at other meetings of the Parapsychological Association [e.g., P.A. Proceedings 1967, 1968, and 1969; RIP 1972, pp. 72-5; and RIP 1973, pp. 64-7], but which we have not previously analyzed for these effects. I thought it might be interesting for you to hear these more recent analyses, and to see if we can begin to make sense of this aspect of poltergeist disturbances. RSPK focusing, if it exists, is interesting for several reasons. First, it might be another possible characteristic of PK that there is an influence, which lasts for a period of time, associated with a certain object or a certain area. Second, there is a possible relevance to the survival question, in that there seems to be a continuation, a "survival" of something associated with a physical area, a physical object. Conceivably this kind of phenomenon might also exist in so-called haunted houses. Now let me describe these two cases.

The analysis of these spontaneous cases is difficult in that it is often hard to determine whether or not we are dealing with genuine parapsychological occurrences. In the Miami and Olive Hill cases I was more confident than in any of the previous cases that there were genuine parapsychological occurrences. In the Miami case, Dr. Pratt and I were able to isolate certain areas for periods of time, and there were occurrences in these supervised areas. In addition to ourselves there were many other witnesses to the occurrences, including police officers and other observers. In the Olive Hill case we did not have such experimental areas, but there were a number of things that happened in full view of the investigators. Again, there were two parapsycholo-

gists on the scene, John Stump and myself. In addition, there were several neighbors and visitors to the home, and again things happened when they were present.

One problem in studying the focusing effect on objects, that is, the likelihood that an object that once has moved will then again be disturbed, is that in poltergeist cases many of the objects are breakables. When they move once they won't move again until they are swept out of the house. Also, the family is likely to try to rescue the more precious belongings, and put them away under beds, or in trunks, and so on. Thus these objects perhaps are less likely to be involved again.

In the Olive Hill case, there were altogether about 200 recorded incidents spread over three houses, and most of the incidents, 158, occurred in one house, where our investigation was concentrated. Of those 158 events, 58 involved pieces of furniture. Usually these pieces of furniture would only turn over, but sometimes they would move some distance, all apparently for no sufficient physical reason. As a rule these pieces of furniture, being reasonably sturdy, did not break, and so we had several objects on which we could concentrate our attention. Out of these 58 furniture incidents, 23 involved a small wooden kitchen cabinet, and 11 involved a coffee table in the living room. This suggested a focusing effect. However, we could not be certain of this, because of the other effect that I mentioned earlier, the distance effect: Objects that are close to the apparent agent are more likely to be disturbed than objects further afield.

The agent here was a 12-year-old boy. I myself witnessed one of the furniture movements when the boy was standing right next to the kitchen table in the house. I saw this kitchen table lift from the ground, rotate about 45 degrees, and then settle on the backs of the chairs that were standing around it, and at the same time I watched the boy. There seemed to be no physical connection between the two, and there was no one else in the room.

When we concentrated on the movements for which we had definite measurements, we found 26 which were all in the 1-5 foot distance grouping from the boy. There were eight movements of the kitchen cabinet, five of the coffee table, and one or two each of another ten objects. This distribution is significant (chi square = 23.8, df = 11; $P <$

.02), and suggests focusing on the cabinet and table. A closer examination shows that these two objects tended to be closer to the boy than the others, the average distance being one foot two inches versus two feet four: Conceivably proximity contributed to the focusing on these objects.

That this is not the full explanation, however, is suggested by reports of ten furniture occurrences when the boy was not in the house at all. None of us witnessed any of those, but the family claims that there were movements when he was not present. Seven of these involved one of the 12 objects mentioned before, including three movements of the kitchen cabinet and two of the coffee table. That there was an influence that remained for a period before it died down, was also suggested by the distribution of the events. Five of the cabinet occurrences and four of the table occurrences happened in close succession.

In the Miami case we had a different situation. There were no furniture disturbances; the 224 recorded events involved cartons of merchandise, but mainly individual glasses, ashtrays, and other small items. As you perhaps recall from the report of this case, we are dealing with a warehouse containing Florida novelty items such as glasses with palms painted on them, ashtrays with alligators, and so on, all made in Taiwan or Japan. These objects were observed to move various distances. Again, there was an association with a person; in this case, a 19-year-old shipping clerk.

Because there was so much breakage, it was difficult to get a clear picture of focusing on individual objects. However, these events were apparently concentrated in certain areas of the warehouse. At the center there were three wooden tiers of shelves, each about three feet by twelve. Tiers 1 and 2 had three shelves, and Tier 3 had two. The shelves were separated along their length with planks and there was access to both sides of the tiers. The tiers were all well stocked with merchandise and since they seemed similar in other respects, I decided to concentrate on them in a study of area focusing. For this analysis, each tier was divided in the middle, thus giving four areas. There were striking differences in distribution of events over the three tiers. For instance, 41 occurrences took place in the northwest quarter of Tier 1, and only one in the southwest part of Tier 3.

We are now faced with the same question that we

were faced with before: Could the effect be due to proximity or some other factor? I noticed that the fewest number of incidents occurred on Tier 3, which had only two shelves, as against Tiers 1 and 2, which each had three. If we exclude Tier 3 and only look at occurrences where we had measurements of the agent's distance from the objects, 22 incidents remain. Sixteen of these were in the northwest quarter of Tier 1, six were in the northeast corner of Tier 2, and none were in the other six areas--a highly significant distribution (chi square = 84, df = 7; $P < .0001$; because of the small number of incidents, the chi square gives a rough approximation to the P-value). The effect cannot be attributed to agent proximity: The four distance groupings, 1-5 feet, 6-10, 11-15, and 16-20, had respectively six, eight, five, and three incidents. There is indication of a decline, which we would expect on the basis of previous RSPK investigations and on the basis of our analysis of the Miami incidents, but the number of occurrences between six and ten feet, and between 11 and 15, were about the same as the occurrences right next to the agent.

Another possibility presents itself. There was apparent focusing on certain kinds of objects in the Miami case: Beer mugs, ashtrays, and so-called "Zombie" glasses were especially often moved. If the 22 incidents consisted of such objects, we might be dealing with a type of object focusing, rather than with area focusing. When these three types of objects are excluded, the incidents in the two areas are reduced to eight and three, still a striking distribution (chi square = 42, df = 7; $P < .0001$, with the same qualification as before).

The Olive Hill and Miami cases support indications from earlier studies that there are focusing effects in RSPK. I tentatively offer two "laws." When an object has been involved in an RSPK occurrence, this object is, all other things being equal, more likely to be involved again than an object which has not previously been affected. And, when there has been an RSPK occurrence in a given area, there is, all other things being equal, more likely to be another occurrence in that area than in an area in which there has been no previous incident. Also, it is likely that object and area recurrences will happen closely to each other in time. It is encouraging that the focusing phenomenon is now also emerging in laboratory PK, as we shall shortly hear.

The focusing and attenuation effects have important

practical and theoretical implications. It is obviously easier to make controlled observations if one knows that the incidents are likely to occur close to a person and to involve certain objects or locations than if they might happen anywhere and involve anything in the general area. On the theoretical side, the focusing effects fit well with the attenuation phenomenon and the rotating beam theory, as Dr. Joines will show [pages 147-9].

THE BRONX POLTERGEIST

William Eisler (Psychical Research Foundation)

On March 15, 1974, I began investigation of an active poltergeist case in the Bronx, New York City. The troubled family consisted of 62-year-old Mr. Robbins, his wife, age 57, and their 8 1/2-year-old adopted daughter Ann, around whom the phenomena centered. (These names are not their real ones.)

As reported to me, the disturbance started on Saturday night, February 9. Because a neighbor's five-year-old son was staying at the apartment that weekend, Mrs. Robbins and Ann were both sleeping in Ann's bed, while Mr. Robbins and the boy were in the parents' bedroom. As they were going to bed at about midnight, four framed pictures hanging over Ann's bed fell to the floor. Mrs. Robbins was not in the room at the time, but came in and hung the pictures back on the wall. A few minutes later they fell again, this time while Mrs. Robbins was in the room. Mr. Robbins came in after this second incident, and saw them fall two more times. During this time Ann was in the bed. After falling the fourth time, the pictures were left on the floor. Soon after this Mrs. Robbins witnessed the lamp fall off Ann's night-table. When she put it back, it fell again about five minutes later. Then the furniture in the room started falling over: the wooden night-table, a desk and chair, a toy chest, a portable television, and two lamps. One lamp fell when Mr. Robbins was the only person in the room, and the other fell when nobody was in the room.

As the family moved about the apartment during the remainder of the night, objects fell in other rooms. While

Ann was sitting on the bed in the parents' bedroom, Mrs. Robbins witnessed an ashtray fall, the night-table and a lamp each fell over twice in a row, and a clock-radio several feet behind Ann fell from a table. In the living room, Mrs. Robbins says she saw a chair balance on two legs for several seconds when she was the only person in the room. A living room lamp fell and a coffee table overturned. In the kitchen, the toaster, cookie jar, and garbage can moved. The hamper, scale, and garbage can in the bathroom also fell over, each more than once. All told, 39 incidents occurred the first night, which was the most active period of the disturbance.

The next day items continued to move. Several bottles in the bathroom, the hamper, and Ann's desk fell. A neighbor described an incident in which he, Mr. Robbins, and Ann were standing in the foyer by the front door when a living room chair ten feet away was seen to balance on two legs. He went over and pushed it down, and reported later that no one was near it. This was the same chair Mrs. Robbins had seen lean the night before. A dresser in Ann's room fell three times, while she was alone in the room.

From witnesses' accounts I have tabulated 94 events occurring between the inception of the disturbance on February 9 and the start of my investigation on March 15. Objects throughout the apartment were affected, especially items in Ann's bedroom and the bathroom. Several neighbors witnessed events, and a policeman saw Ann's desk chair fall when no one was in the room.

Soon after I arrived on March 15, Ann went into her room to put on her pajamas, while the parents and I talked in the living room. Shortly we heard a crash in her bedroom. We found her desk and chair overturned on the floor and Ann standing on the other side of the bed undressing. If Ann had knocked the desk over, she would have had to run either around or over her bed. If she had gone around the bed, I would have seen her, since her door was open and there is a view straight from the living room into a part of the room she would have had to cross going around the bed. I did not hear any noise from the bedsprings. I picked the desk back up and returned to the living room. Three minutes later we heard another crash and found the desk had fallen again. When I got to the room Ann was in the same place she had been a few minutes before, but by

now with her pants off. For this event I am certain Ann could not have run around the bed without being seen, and again I did not hear any noise from the bedsprings. It seemed unlikely that she would choose such a time to play a trick.

I lived in the apartment for six days, during which time 43 events took place. My presence did not seem to deter the phenomena. The disturbance was primarily in Ann's room, but objects in the kitchen and bathroom also moved. Generally events occurred when Ann was alone. I did not see any entire object movements, though I did see several objects in flight. One event took place when Mrs. Robbins was with Ann in her room. They were by the headboard of the bed, looking for something on the floor, when the lamp five feet away fell from the dresser. On the morning of March 22, Mr. Robbins and Ann went to Virginia for a week's vacation, during which time nothing occurred, and when they returned home the activity lessened. On April 6 W. G. Roll spent one day and night at the apartment, but nothing happened during his stay.

Events only occurred when Ann was home, and almost always in the room she was in. This was not true of anyone else in the household. As exactly as it can be estimated, Ann's average distance from the object that moved was 3.96 feet (calculated from the start of the movement). This is much less than is generally true in poltergeist cases: W. G. Roll has computed average distances of 9.0, 7.9, and 13.5 feet in three of his previous investigations. There was a dramatic and clear-cut attenuation of the number of events with increasing distance from Ann, a further indication that she was the source of the disturbance. Of the 106 incidents where Ann's distance from the object that moved was known, or can be reliably estimated, 100 were within ten feet of her. A breakdown of this ten-foot range into five two-foot intervals shows a marked attenuation effect. There were 37 incidents within two feet of Ann, 29 from two to four feet, 17 from four to six feet, eight from six to eight feet, and five from eight to ten feet. (In four cases a two-foot estimate could not be reliably made.) This attenuation pattern is significant at the .005 probability level ($r = .99$; $t = 26.97$).

Objects generally moved toward Ann ($N = 51$) rather than away from her ($N = 21$). In some cases it was impossible to assess a clear directionality. There was a tend-

ency for movement to originate behind Ann. In incidents where her relative position was known, there were ten movements in front, 20 behind, seven on her right side, and 12 on her left. Where it can be determined, 39 events took place while Ann was walking or standing, and 54 occurred when she was sitting or lying down. In agreement with a trend found by John Palmer in a recent case reported at the 1972 Parapsychological Association convention [RIP 1972, pp. 70-2], objects tended to move from behind when she was walking, and from in front when she was still. The same objects moved in different directions, and there does not appear to be any compositional pattern.

There was evidence of a "focusing effect." Objects that moved tended to be those that had moved before. Ann's desk fell over a total of 17 times, her lamp fell 12 times, her night-table fell eight times, the bathroom hamper fell eight times, one of her dressers fell six times, and the cookie jar moved five times. Ann's pictures fell four times, and then were never put back on the wall. The toy chest, kitchen garbage can, and bathroom garbage can were each affected three times, and 11 items moved twice.

There was also evidence of a "lingering effect." On 19 occasions an object fell, was put back, and fell again soon after being replaced. Ann's pictures fell four times in succession. Her desk and dresser each fell three times in a row. Events occurred in all five rooms of the apartment, and since basically the same objects moved each time, this would suggest that objects were focused on rather than areas. However, an area on Ann's dresser where her lamp normally was seemed particularly prone to disturbance. The lamp was the second-most frequently moved object, and three times when other items were in its place they moved, so it is unclear whether area or object focusing would be involved.

Psychological factors are believed to be of primary importance in causing poltergeist, or RSPK disturbances; they have been theorized to be the product of repressed tensions. The present case is in accordance with this view. During my investigation I had a talk with a psychologist who had recently begun seeing the family. Her opinion was that Ann was not getting the attention and affection she wanted from her parents, especially her mother. The psychologist felt that Ann's relationship with her mother was the core of the problem, and that Ann's frustration and hostility were directed more towards her mother than her father. This

fits in with the pattern of RSPK activity; for the most part, someone's presence with Ann stopped the phenomena from occurring, but this was not true for Mrs. Robbins. She was with Ann when 30 incidents occurred, as opposed to Mr. Robbins' presence at 16, and visitors' presence at six.

The theory that RSPK is displaced aggression makes sense here. The phenomena give Ann the attention she wants while at the same time providing a psychological release. The disturbances may represent an outlet of repressed emotions directed in particular towards the mother. At the same time, Ann is innocent, and therefore escapes punishment, since she is not knocking anything over herself.

A biological factor of possible relevance is the fact that Ann is a hyperactive girl. She was almost constantly moving or fidgeting. This is a medical diagnosis, and is a physiological condition with several adverse side effects, such as vomiting, stomach trouble, and bed-wetting. Palmer has also noted that the agent in this case was a very active child.

Despite my physicalistic emphasis, I have serious doubts about the adequacy of a purely energetic theory as a comprehensive explanation of RSPK. In this case, events did not take place at school or anywhere else outside of the home. This suggests that a psychological set--which in this case was only created at home--may be necessary for the production of RSPK. If RSPK were simply an energetic process, it would be expected to occur more randomly.

LINGER EFFECTS IN SEVERAL PK EXPERIMENTS

Roger Wells† and Graham K. Watkins (Psychical Research Foundation and Duke University)

The possibility that an area may become "sensitized," or retain some sort of effect which may have been previously imposed on it, was suggested in PK research with anesthetized mice which we did while working at the Institute for Parapsychology, FRNM [see, e.g., RIP 1972, pp. 157-9]. We placed a similarly-etherized pair of mice on a table in one room while a subject with presumed paranormal healing

abilities observed from an adjacent room through a one-way glass, attempting to influence the mouse on one side to recover prior to the mouse on the other. Both mice were monitored for recovery time; a hit occurred when the target mouse awoke first.

In the first series of experiments with this "healing" effect, we noted that the subjects failed to produce a significant effect when the assigned target side (right or left) was randomly varied from trial to trial. They did well, however, when one side was used as target throughout half of a given run. It is unlikely that this was due to bias in the experimental system. It could be explained, however, if the effect which was causing the accelerated waking of the target mice did not immediately dissipate when the subject ceased to concentrate, but rather lingered on for a certain period of time. This idea was reinforced by the finding that a rest period of approximately 30 minutes was required between halves of the run to insure a successful second half, when the target side was changed from first to second half.

Two series of experiments were then conducted to test this hypothesis. In the first, eight runs of 24 trials each were performed; the experimenters were G.K.W. and Anita Watkins. The target side for each run was determined by a Schmidt random number generator. The subject was asked to leave the building upon completion of the first half of each run, and the second half began immediately, with pairs of mice continuing to be placed on the table as though the subject were still present. Timing was manual, by stopwatch. The first halves showed significantly faster awakening for the mice on the target side ($t = 3.74$, $df = 95$; $P < .001$), and in the second halves the mice on that side continued to awaken first ($t = 4.69$, $df = 95$; $P < .001$). The run-by-run regression of mean time differences between target and control mice on the first halves versus second halves yielded a t value of 3.21 ($df = 6$; $P = .02$), and a regression coefficient of 1.08. The correlation between first and second halves was +.79.

To investigate the possibility of an experimenter effect, a second series was done consisting of eight runs of 16 trials each. G.K.W. and Anita Watkins were the experimenters for the first halves of the runs, and R.W. and Judith Klein were experimenters for the second halves. At the end of the first half of each run G.K.W. and Anita Watkins removed all data so that R.W. and Judith Klein, when they

began their trials, were blind both to the target side and to the outcome of the first half. As before, the subject was present only for the first half. Automatic timing by photocell was used in this series.

Both halves of this series were significant in the expected direction when tested by analysis of variance (P = .024 for first halves; P = .015 for second halves), but only the second halves were significant when analyzed by t-test (t = 1.84, df = 63; P = nonsignificant for first halves; t = 2.01, df = 63; P = .05 for second halves). The regression analysis was again significant (t = 3.41, df = 6; P = .02), and the regression coefficient remarkably close to the first series (1.07 as compared to 1.08). The correlation between first and second halves was +.81.

This second series of experiments seemed to indicate that the "linger effect" is not only a reality but may be a more reliable finding than the main effect. Certainly it explains the failure of the subjects in our earlier research to achieve significant results in those runs in which a random side sequence was used rather than one side continuously.

These results may be compared to some static-object PK results G.K.W. obtained in rather informal experimentation with Felicia Parise. These were reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 132-4]. After Ms. Parise had produced a compass needle deflection by mental concentration, the needle remained offset when she moved away from the area. It was not affectable by either a bar magnet or a steel knife blade; it appeared that the needle was stuck. When, however, the compass was moved to a distance of approximately one meter away, it returned to North and became normally sensitive to the magnet and blade. When it was moved back, it again moved off North and appeared to lose its normal sensitivity to metal. It is of considerable interest that this effect also took about 30 minutes to decay, over which time the needle gradually moved back to North and gradually regained its sensitivity.

Finally, a third experimental design we have developed over the last two months has given suggestions of an intriguingly similar linger effect. This research is being done in the laboratory of Dr. Larry Thompson at the Duke University Medical Center. Since the work is in a preliminary stage, we will not fully present the results, but the basic concept involves a human subject exerting a mental effort to influence

the discharge time of a capacitor being automatically discharged every ten seconds. We cannot recommend the procedure as an especially sensitive one, since we have screened half a dozen potential subjects, several of whom scored well in the anesthetized mouse experiments, and still have found only one subject who can demonstrate an effect on this device.

However, this one subject evidences quite a strong ability; during his concentration periods the discharge time of the capacitor decreases by as much as ten percent, although the variation generally holds at near 0.1 percent during control periods. In all instances during which the effect was observed, the capacitor discharge time returned to control period levels immediately upon termination of concentration by the subject, with one exception: a session for which we had enclosed the target capacitor in a grounded metal box in order to eliminate the possibility of the effect being directly due to electrical field fluctuations induced by the subject. In this session, the capacitor discharge time decreased relatively gradually over a period of some 30 minutes, even after the subject had left the room and, at the end of the session, even with the metal box removed. At this point, the experimenters concluded that the capacitor was malfunctioning and terminated the session. However, in the next session this same capacitor was functioning normally, and has shown no signs of "drifting" in any such manner since. We feel it is a possibility that the presence of the enclosing box allowed a field effect to be established around the capacitor; we assign a high priority to determining if this effect has any degree of repeatability, and exploring its spatial and temporal characteristics if it does in fact recur.

In two subsequent sessions in which the capacitor was similarly shielded, no effect at all was recorded. (Unshielded results have continued at a spectacular level.) We now consider it much more likely that the initially reported effect was due to equipment malfunction.

We have simultaneously monitored static electrical field fluctuations in the experimental room during two sessions with the capacitor, using a high-impedance electrometer with output recorded on tape and polygraph chart simultaneously with the capacitor discharge time. In these two sessions, no activity other than 60-cycle hum was recorded during the pre-session control period. Beginning during the subject's concentration, and continuing after he had left, synchronous fluctuations of 5-10 Hz, resembling EEG theta

activity, were observed on the polygraph chart output of the electrometer. We feel this electrical activity could also be viewed as a lingering effect, although it seems implausible that it could directly affect the capacitor.

We have since determined that the specific frequency of the 5-10 Hz oscillations is an artifact of the electrometer arm length and can be duplicated by slight mechanical jarring of the equipment. It remains a fact that no such effect occurred prior to concentration, and that the effect continued sporadically with no one present in the equipment room. However, it is most certainly not an electrical field effect per se and has therefore become of less interest to us.

A lingering effect in PK experiments should at least be considered as a possible source of interference which can be overcome with appropriate design, and may additionally be a functional principle which will allow reliable manipulation of at least one type of paranormal phenomena.

A WAVE THEORY OF PSI ENERGY

William Joines (Duke University and Psychical Research Foundation)

We have enough experimental evidence to indicate a repeated pattern for focusing effects and lingering effects involving psi energy. I wish to discuss a theory that is new only in that it has not been applied to explain these particular PK effects. Let us simply assume that psi energy (whatever it is) exhibits wave motion in transmitting from subject to object. This is a reasonable assumption because all forms of energy now known to us manifest themselves as wave motions. These wave motions, like electromagnetic waves and sound waves, can be described by similar graphic or mathematical relationships. Using this generalized wave theory we know how to reflect, transmit, reinforce, and cancel these waves, even though the wave sources are quite different. We might also expect that even if psi energy is a new form of energy it should also exhibit a wave-like behavior, and might therefore be describable by presently-known wave theory.

What predictions can we make by assuming that psi energy is transmitted in waves? (1) The psi wave should be attenuated exponentially with distance from the source, since other waves are attenuated in this way. This could account for the attenuation effect universally found in poltergeist cases, in which number of object movements decreases with distance from the purported agent. (2) The rotating beam theory, which seems to adequately fit the data on directions of object movements in many poltergeist cases, depends upon an interference of waves and follows directly from wave theory. This was discussed in relation to the Olive Hill poltergeist case at the 1973 Parapsychological Association convention [see RIP 1973, pp. 64-7]. (3) All the different waves we know about can be focused within an area. If psi energy is a wave, area focusing should be expected even if we do not know what causes the focusing. (4) The linger effect can be explained as the combined effect of a rotating beam and wave reflection from nearby obstacles.

It may help to visualize these phenomena if one imagines water waves. Exponential attenuation takes place when waves generated by two sources are superimposed, producing an interference pattern. The rotating beam theory postulates phase differences between the waves from two sources which intersect to produce a beam sweeping from left to right (or vice versa) which becomes wider and less intense as it sweeps. Area focusing occurs when a wave is reflected back from a concave barrier to a focal point, thus concentrating the energy in a small area. An analogy to the linger effect might be produced if you took the rotating beam and combined this with a reflection of the water waves back toward the source. This would produce a funnel-shaped disturbance or whirlpool which stays in one position and slowly dies out as time passes. Now picture a wave which travels through three-dimensional space (as would a psi wave) rather than along a two-dimensional surface; the combination of rotation and reflection should cause a spherical disturbance which lingers within a given position.

How could these effects be detected by electromagnetic instruments like photographic film, gauss meter, compass, or radio receiver? The same process which triggers a psi wave might cause an accompanying electromagnetic wave. We see this happening in an atomic explosion where not only a shock wave is generated, but a very intense electromagnetic wave as well. Also, the electromagnetic wave may be caused by the psi wave itself. A relevant example is a tor-

nado, which generates an electromagnetic wave due to the swirling electrical charges within the funnel shape. This wave is now used by meteorologists to detect the approach of a tornado.

Taking the linger effect model as an example, if our spherical concentration of psi energy can contain electrical charges as does a tornado, the region would behave as a tuned electrical circuit and would radiate at a frequency depending upon the size of the spherical region. During a poltergeist case I helped to investigate in 1971, I detected a spherical region of space which apparently emitted radiation at a frequency of 146 MHz. This detection was made on two separate occasions using a radio receiver. The region was about two feet in diameter, which is consistent with the 146 MHz frequency, and detectable transmissions persisted for about one minute. The intriguing possibility exists that this might have been a physical basis for the linger effect.

I hope I have made the point that wave theory is consistent with many features of PK phenomena. Certainly more data are needed to complete the picture or form a new one. Perhaps your own research will supply these data. Perhaps also we can learn to use psi energy as effectively as we now use electromagnetic energy.

11

ESP AND "NORMAL" COGNITION*

A STUDY OF THE MEMORY-ESP RELATIONSHIP USING LINGUISTIC FORMS

H. Kanthamani† and H. Hanumanth Rao† (Institute for Parapsychology, FRNM)

The present study attempted to investigate the relationship between memory and ESP on a trial-by-trial basis. The point of interest in this investigation lay in separating correctly recalled stimuli from those which were not correctly recalled and studying ESP in response to both these classes of stimuli. Such an analysis was made possible by the use of a special ESP test which enabled us to bring the two processes, recall and ESP, as close in time as possible.

The material consisted of a list of 20 Telugu (a south Indian language) words plus 20 associated pairs of English words, which were translated homonyms of the 20 words in Telugu. These English word-pairs were used to test both ESP and memory. The main reason for using foreign-language words and their homonyms in English was to make the test interesting to the subjects. However, since the word-pairs had to be translated homonyms, it was not possible to select them from lists of words whose frequency of use was controlled according to the available norms.

The general procedure used in the pilot and confirmatory series was as follows. Each subject was required to memorize the list of 20 paired-associates in a short period of time. Different durations of learning time were tried initially, before arriving at an optimum of 180 seconds. The subject was told that he would be required to recall the word-

*Chairman: Martin Johnson, University of Utrecht, Netherlands.

pairs at a later period. The learning period was followed by a brief interference lasting approximately 2 to 3 minutes, during which the subject was told the nature of the words he had just learned. Afterward he was asked to recall the pairs of words.

One word in each English-language pair had been selected randomly as an ESP target. A target sheet containing the list of 20 Telugu words and the 20 associated English target words was enclosed in a sealed opaque manila envelope. The subject's response sheet consisted of the list of 20 Telugu words, opposite to each of which was one of the pair of English words, randomly selected in a separate procedure. Thus the English word on the response sheet might or might not be the same as the ESP target word on the target sheet. The presence of Telugu words on the target sheet and the response sheet served only as an indicator of the trials. The subject was required to write down the missing word in each pair from recall, prompted by the English word provided. The ESP guesses of the subject were then obtained by asking him to circle one of the two English words (either the prompting word or the word he had recalled), the one which he thought was the target word written on the sheet inside the envelope.

The subject was asked to attempt ESP guesses for all 20 pairs, irrespective of his success in recalling them. No time limit was stipulated for recall. When a subject failed to recall a word, he was instructed to indicate his ESP guess by circling in the empty space for the missing word if that was his choice or, alternately, circling the prompting word if that was his choice.

A short preliminary test was carried out using a modification of the above procedure. The main purpose of this series was to arrive at a suitable technique of testing for memory, in particular the period of time to be allowed for learning the list. Six subjects were tested individually. Each was given a list of Telugu words along with their English word-pairs. A learning period of five minutes was tried in this series, and the subjects were required to recall and write both the English words next to each Telugu word on the response sheet. The subjects made their ESP guesses immediately after writing down each recalled pair by circling one of the two words. They attempted ESP guesses on all 20 word pairs.

The six subjects--a heterogeneous group--together made 120 ESP trials in which they obtained 48 hits, 12 less than chance expectation. The negative scoring is by itself significant ($P < .05$). However, the main focus of interest was on the ESP scoring rate for those trials with recall correct as contrasted with those with recall wrong. The recall-correct trials contributed most to the obtained ESP effect. Out of 58 trials only 19 hits were made, which was significant psi-missing ($P < .01$). The scoring on recall-wrong trials was at chance. This result suggests that memory seems to affect ESP differentially, depending on whether a particular word is correctly recalled or not.

The preliminary test was followed by a pilot series in which the task of recall was made easier for the subject by providing him with a prompting word selected randomly from each English pair he had learned. Consequently, the learning time allowed was reduced to 180 seconds. This series was planned to include 20 high-school students as subjects, tested individually. As it neared completion, it was found difficult to get the required number of high-school students, so the series had to be completed by accepting other available subjects. The analysis, however, was done with the data obtained from the 16 high-school subjects only.

The overall scoring in the ESP task was found to be positive but not significant. However, as before, the trials recalled correctly contributed all the ESP effect. Out of a total of 150 such trials 91 hits were made, which is significantly more than chance expectation, as analyzed by the Wilcoxon test ($Z = 2.21$; $P < .05$); scoring on the recall-wrong trials was not significantly different from chance. The difference between recall-correct and recall-wrong trials was not significant ($Z = 1.42$).

The pilot test led to two further confirmatory series to test the hypothesis that the ESP effect, if any, would be concentrated in those trials which were successfully recalled. Each series was planned to include 20 subjects, tested individually for both memory and ESP. All subjects were high-school students, both boys and girls. The procedure for making ESP guesses was slightly altered in the first confirmatory series. The subject was given the choice of writing the recalled word either in capital letters or in small letters, depending on how he thought the target word had been written. The preparation of targets was altered in accordance with this change: the target sheet inside the en-

velope contained the missing word, written either in capital or small letters, determined on a random basis. In this way the subject was forced to make his ESP guesses immediately prior to writing his recalled word. If he was unable to recall any word, he still indicated his ESP choice by writing "C" (for capital) or "S" (for small).

The overall ESP score of the 20 high-school students was significantly positive ($P < .05$). In accordance with expectation, this effect was concentrated in the correctly recalled trials. Out of 279 such trials there were 160 hits, 20.5 more than chance expectation ($Z = 2.72$; $P = .003$, one-tailed). On those trials which were not successfully recalled, the ESP results were close to chance. The difference between the two categories was not significant.

Since the change made in the mode of ESP guessing in the first confirmatory series proved to be too cumbersome for the subjects, this aspect had to be changed once again in the second confirmatory series. As before, the record sheets consisted of 20 Telugu words, with one of the two English homonyms to the right of each one. Either to the left or to the right of each English word there were two adjacent blank lines. The subject was instructed first to select one of the blank lines as his ESP choice and only then to write the recalled word in the space chosen. If he was not able to recall the word, he was instructed to make a check mark in either of the spaces to indicate his ESP choice.

As before, 20 subjects participated in this test. The overall ESP results were at chance. When the recall-correct trials and the recall-wrong trials were separately analyzed, once again correctly recalled trials yielded psi-hitting ($Z = 2.05$; $P = .02$, one-tailed). However, the recall-wrong trials also yielded significant ESP in a negative direction ($Z = 1.96$; $P = .05$, two-tailed). The difference between the two categories of trials was also significant ($Z = 2.30$; $P = .02$, two-tailed). Thus, both confirmatory series upheld the hypothesis that ESP success would be concentrated in those trials which were successfully recalled. The second series suggested additionally that recall-wrong trials tend to yield psi-missing.

The main finding of this investigation, that successfully remembered trials also tend to be successful ESP trials, remained fairly consistent across the various series.

In the pilot and two main series, successfully recalled trials always carried significantly positive ESP effects. However, the results of the preliminary experiment proved to be an exception in that the recall-correct trials tended to give rise to psi-missing. Perhaps this anomaly can be partially explained as being due to the more difficult learning task assigned the subjects in the preliminary series as compared to the other series.

Although our primary purpose was to analyze the results of the two tests (memory and ESP) trial by trial, it was thought worthwhile to use the present data for certain analyses considering subjects as units. For this purpose the four series were pooled to obtain a fairly large sample size. It may be noted that all of the following are post hoc analyses. There were in all 62 subjects tested in the four different series. First, a product-moment correlation coefficient was worked out between the total recall scores and the ESP scores of the subjects. The obtained correlation was +.284, which corresponds to a t of 2.876 (df = 60; $P < .01$, two-tailed). Thus, there seems to be a significant positive relationship between recall success and ESP success in these data.

Further, it was observed that when subjects were divided into two groups on the basis of their memory scores the below-median group showed psi-missing and the above-median group psi-hitting. The difference between the two groups was significant ($t = 2.29$, df = 60; $P < .05$, two-tailed). The hitting of the above-median group was independently significant ($P = .02$), with a reliable consistency ($P < .002$). However, the below-median group was not independently significant.

RESPONSE TENDENCIES AND STIMULUS STRUCTURE

H. Kanthamani† and H. Hanumanth Rao (Institute for Parapsychology, FRNM)

The objective of this study was to explore how a subject's ESP performance is affected by his response to the nature of the stimuli, especially when some of the stimuli are conspicuous. The subject's response could be one of in-

hibition and bias against the prominent aspect or, alternately, one of spontaneous attraction towards it. In order to study this question, we employed an ESP test based on the general psychological principle that individuals tend to prefer meaningful stimuli when these are combined with non-meaningful or less meaningful stimuli.

The ESP test consisted of a list which contained 63 three-lettered syllables, arranged in 21 rows of three syllables each. Two of the syllables in each row were nonsense syllables of the CVC (consonant-vowel-consonant) type. The other three-lettered syllable in each row was a meaningful word; these words were not controlled with strict consideration to their standard frequency level. The positions of the three syllables in each row were randomly arranged. Thus, in all there were 42 nonsense syllables in the list, and 21 meaningful words.

The subjects were all high-school students. We were invited by local high schools to give seminars on parapsychology. On three such occasions, the ESP test was introduced and those who wanted to participate were asked to stay in the classroom for subsequent testing. All those who volunteered for the experiment were given the test in a group situation.

The test was introduced as a precognition experiment. The subjects were asked to select one of the stimuli from the three in each row. The instructions required them to read all the syllables in each row, and then choose one of them as their ESP response by circling it. They were told that the actual targets would be known sometime in the future, and their goal was to make as many correct hits as possible. The subjects were not informed explicitly of what the list contained, but it was quite obvious that they would notice the presence of two types of stimuli--meaningful stimuli (MS) and nonsense stimuli (NS).

Although the test was presented to the subjects as one of precognition, strictly speaking it was a test of clairvoyance. The digits in a random number table which would determine which of the three syllables in each row would serve as target had already been selected, but the targets had not yet been "prepared" at the time of the test. The targets were converted from the random digits on the day following each test. Separate sets of targets were generated for each subject. In all, three series were carried out with students

at three different high schools in Durham, North Carolina. The first series included 30 subjects, the second 25, and the third 36. Both H.K. and H.H.R. were present as the experimenters for all three groups. In addition to the two experimenters, a teacher from the school was also present, who helped in making sure that the students understood the instructions.

On the basis of a preliminary study, we decided to divide the subjects into two groups, taking the number of MS checked by them as the criterion. Since the proportion of MS was one-third of the total stimuli, the theoretical expectation was that a subject would check seven such stimuli in a run of 21 trials. Using seven as the cutting point, two groups were formed and compared. Group 1 included all those who checked less than seven MS, and group 2 was made up of those who checked more than seven MS. Those who checked exactly seven MS were deleted from the analysis.

In the first series, the 12 subjects who constituted group 1 obtained an average of 5.58 hits per run, which is less than the mean chance expectation of seven hits. Group 2, on the other hand, with 15 subjects, obtained an average score of 7.87 hits per run, which is higher than mean chance expectation. The difference between the scoring rates of the two groups was significant ($t = 3.47$, $df = 25$; $P < .01$). The results of the second and third series follow the same trend. In the second series the ten subjects in group 1 obtained an average of 5.5 hits per run, while the 13 subjects in group 2 scored over seven hits per run. The difference in their scoring rates was again significant ($t = 3.07$, $df = 21$; $P < .01$). In the third series the 20 subjects in group 1 obtained an average of 6.1 hits per run, the 16 subjects in group 2 averaged eight hits, and the difference between the two was significant ($t = 3.16$, $df = 34$; $P < .01$).

Thus in all three series those subjects who chose a smaller number of meaningful words as their ESP guesses in general obtained less than chance scoring averages, whereas those subjects who checked a larger number of meaningful words obtained higher than average scores. Further, the psi-missing among group 1 subjects was found to be marginally significant ($P < .05$) in the first two series, but not in the third. The hitting tendency of the group 2 subjects did not reach significance in any series. However, when the results of all the three series were pooled in a post hoc analysis, the psi-hitting on the part of group 2 sub-

jects reached a significant level ($P < .01$), and the psi-missing of group 1 subjects increased in significance ($P < .001$).

We were also interested in looking at the difference in scoring rates between MS and NS in the two groups separately. If group 1 was "biased" against MS, as indicated by checking only a few of them, group 2 was "biased" towards MS. We wanted to see if these biases would affect ESP scoring level. Unfortunately, no such difference was noticed between average ESP scores on MS and NS trials in the two groups. The averages for MS and NS trials tended to be in the same direction; they were below chance expectation in group 1 and above chance in group 2. At this stage the results fail to allow any speculation about the application of a response bias hypothesis to these data. However, one interesting feature emerges. Combining all the series, we observed that MS trials yielded psi-hitting and NS trials psi-missing. Average run score was 7.6 on the former (not significant) and 6.27 on the latter ($P < .05$). Thus, the subjects in general tended to hit on meaningful words if they chose them as their ESP responses, and to miss to a more consistent degree on the non-meaningful words. This is a post hoc observation and therefore needs further exploration.

THE ROLE OF MENTAL IMAGERY IN PSI-MEDIATION

Charles Honorton†, Laura Tierney, and David Torres (Maimonides Medical Center)

How is psi information mediated into consciousness? Unlike sensory experience, psi has no distinctive modality. Psi occurrences are determined not through introspective awareness, but "after the fact" on the basis of correspondences between external events and the manifest content of sensorially-noncontingent, internally-generated processes. Tyrrell referred to such content as "mediating vehicles," observing, "The mediating vehicle is not a paranormal phenomenon but is the product of psychological machinery which all possess.... The same vehicle which mediates paranormal cognition also mediates subconscious expectations and beliefs or normally acquired knowledge which has not reached consciousness independently."

Cross-cultural studies of spontaneous cases indicate that psi information is mediated through mental imagery in a substantial majority of cases. Only 15 to 30 percent of the cases in these surveys involved imageless impressions. The vast majority of impression cases occur during normal wakefulness, while imagery cases occur predominantly in dreams. The degree of correspondence between target event and experience is more complete and less fragmentary in dreams than in waking psi experiences. Phenomenological accounts by gifted psychics also place strong emphasis on the development and utilization of mental images in deliberate attempts to obtain psi impressions. In 1966 W. G. Roll suggested that memory images may serve an analogous function in psi communication to the role of sense data in normal sense perception. According to this theory, psi retrieval should be enhanced under conditions which facilitate imagery, and in individuals whose cognitive styles are dominated by imagery. If this is correct, we should expect psi performance to be affected by both intra- and inter-subject differences in imagery.

Let us look briefly at the conditions associated with spontaneous imagery and facilitation of imagery. Spontaneous mental imagery is most frequently associated with dreaming, hypnagogic reverie, and conditions of perceptual isolation. In the absence of perceptual stimulation, and given a level of arousal sufficient to maintain awareness, the individual generates quasi-sensory imagery which serves an inherent need for stimulation. These are the same conditions which have been discussed elsewhere as psi-conducive states. Recent experimental studies of psi communication in dreams, hypnosis, relaxation, and sensory isolation are consistent with the pattern suggested on the basis of spontaneous cases and the phenomenological descriptions of gifted percipients.

If mental imagery is a major modality for psi mediation, individual differences in imagery should also be found to affect psi performance. Kelly, Kanthamani, and Child in 1973 administered a battery of imagery and other cognitive measures to a highly successful ESP subject, Bill Delmore, and concluded that his inner world was characterized by an enormous richness of complex and vivid imagery. In further work, reported at the 1973 Parapsychological Association convention [RIP 1973, pp. 90-4], these investigators found significant similarities between Delmore's style of processing visual and psi information. In a hypnotic dream study involving psi incorporation of free-response targets by unse-

lected subjects, C.H. found significantly more psi correspondences (that is, direct hits) by those who rated their hypnotic dream imagery as vivid and dreamlike than by those reporting less vivid and dreamlike experiences. There was also evidence in this study that hypnosis facilitated imagery.

There is also evidence suggesting that individual differences in sensitivity to internal processes are related to psi performance in waking state card-guessing studies. Such measures as frequency of dream recall and creativity have been found to be related to psi performance. These measures are also reported to be positively correlated with vividness of mental imagery. In a card-guessing study done in 1973 C.H. found that the availability of internal cues influenced subjects' success in discriminating between correct and incorrect ESP impressions.

The present experiment was designed to test directly the hypothesis that mental imagery facilitates ESP performance. We used Sheehan's version of the Betts QMI Vividness of Mental Imagery Scale and a standard ESP card-guessing task. The QMI contains seven subscales for visual, auditory, cutaneous, kinaesthetic, gustatory, olfactory, and organic modalities. High intermodality correlations among the subscales suggest that individuals vary primarily along a dimension of imagery vividness.

The subjects were 50 volunteers (23 males and 27 females) ranging in age from 16 to 45. In most cases they were casual visitors to the laboratory, although about half were to some degree acquainted with one of the experimenters (L.T. and D.T.) prior to the experiment. Twelve had participated in earlier ESP experiments involving free-response methods. However, subjects were selected for participation in the current study only on the basis of interest and availability.

Each experimenter ran 25 subjects in individual sessions. The target material consisted of standard (closed) decks of 25 ESP cards which were shuffled and enclosed in opaque containers prior to the subject's arrival. The subjects were given an informal off-the-record run to acquaint them with the symbols and calling procedure (DT clairvoyance). They were then given the following general instructions for making confidence calls: "During each run I want you to call 'check' whenever you feel relatively confident that your guess is correct. You may not feel a great deal

of confidence on any of the trials, but some may seem different somehow from the others. I would like you to make about five 'checks' on each run...."

The concealed target deck for each run was placed on a table or desk in front of the subject. The experimenter recorded the subject's responses on standard record sheets. We compared two instructional sets, one involving imagery (in which the subject was instructed to make responses on the basis of visual images of the ESP symbols), the other involving imageless guessing (in which the subject was instructed to guess the first symbol that came to mind without looking for mental images). Three runs of each type were completed with order alternated by subject, for a total of 150 trials per subject. The QMI was administered prior to checking the card-guessing scores.

Subjects with QMI scores below the group median (88) were designated strong imagers, while those with above-median QMI scores were designated weak imagers. There were 24 subjects in each group. The strong imagers obtained a mean ESP run score of 5.44, a statistically significant level of psi-hitting ($t = 3.38$, $df = 23$; $P < .005$, two-tailed), while the weak imagers obtained an equally significant level of psi-missing with a mean run score of 4.65 ($t = 3.45$, $df = 23$; $P < .003$, two-tailed). The difference between strong and weak imagers was also significant ($t = 3.14$, $df = 46$; $P < .004$, two-tailed).

Nine subjects (nearly 20 percent) made no confidence calls despite repeated urgings by the experimenter. Strong imagers made an average of 3.8 confidence calls per run, compared to 4.6 per run for the weak imagers. This difference was not significant. Strong imagers, however, obtained a mean of 22.7 percent correct confidence calls, compared to 17.3 percent for the weak imagers ($t = 2.20$, $df = 39$; $P < .05$). There was no significant difference between the two instructional sets in ESP performance.

PSI AND SUBLIMINAL PERCEPTION: A REPLICATION OF THE KREITLER AND KREITLER STUDY

Christa Lübke† and Winfried Rohr (University of Freiburg, West Germany)

Kreitler and Kreitler have reported that performance on certain standard psychological tests can be influenced by extrasensory communication. In one experiment the Kreitlers showed that subjects recognized significantly more letters projected subliminally when there was a sender concentrating on the corresponding letter, although the subject was unaware of the sender's existence. Further analysis of the data revealed that the effect was due mainly to letters which, in the control condition, were definitely below the threshold of perception. On the contrary, no ESP effect could be demonstrated with regard to letters which, in the control condition, were around the threshold or above it.

The present study was designed to replicate this experiment. Our design was as similar to theirs as possible. Thirty-eight undergraduate students in psychology (16 males, 22 females) took part in the experiment, 31 (11 males, 20 females) as subjects and seven (five males, two females) as senders. Six students participated as senders in four to six sessions each, and one student was a sender in just one session.

Slides of three identical sets of 20 letters each, and one set of 20 landscape-pictures, were used. The letters represented the alphabet except for the letters D, G, I, J, O, and Q, which had been eliminated by the Kreitlers on the basis of pretests. Two sets of letters were exposed subliminally to the subject. The first was used to determine the subject's threshold of perception so that the exposure time could be individually adjusted for each subject in the experiment that followed. The second set of letters was used in the actual experiment, projected once in the given order of the slides and once in the reverse order, for a total of 40 trials. During these 40 trials the third set of letters and the 20 pictures were exposed for 20 seconds each to the sender. In the experimental condition the sender looked at the letters, which were the same letters the subject was trying to perceive, and in the control condition the sender looked at the pictures. Letters and pictures were arranged alternately in the sender's cassette; whether

a letter or a picture began the series was randomly determined. Thus, subject and sender were exposed to 40 projections, with each letter in the subject's cassette corresponding once to the same letter in the sender's cassette and once to a landscape-picture.

Three experimenters participated in each session. The first worked with the subject, the second with the sender, and the third, who had no contact with subject and sender, prepared the slides according to randomization rules some time prior to the experimental session. The three experimenters could not communicate with each other during the session. The subject was told that he was participating in a study on the effects of fatigue on the threshold of perception. Then the first set of letters was projected by means of an electronic tachistoscope at intervals of 20 seconds to determine the subject's threshold of perception. The exposure time, initially 3/1000 second, was modified to control for individual differences in absolute threshold of perception. If the subject did not recognize at least one out of the first four letters, the exposure time was increased. If the subject recognized at least three of the first four letters, the exposure time was reduced. The same procedure was followed for the next four slides, and again for the following four slides. The remaining eight slides were used for recording the number of correct identifications without any further changes in exposure time. The average number of correct identifications was 2.8 out of eight, or 34.3 percent (as compared to 27.8 percent in the Kreitlers' study). Following this, the second set of letters was projected without interruption, again one slide every 20 seconds. In this part of the experiment, the experimenter could not see the projected letters, nor did he know the order of the slides.

Meanwhile, the second experimenter was working with the sender in another room. Each slide, letter or picture, was presented to the sender for 20 seconds. He was asked to concentrate on it, and to try to transmit his thoughts to the subject. The 40 slides were projected without breaks in between. The experimenter could not see the slides, nor did he know their order. The sender did not meet the subject, nor did he know who the subject was. He was asked not to talk about the experiment. Synchronizing of the subject's and the sender's projections was achieved by means of a stereophonic tape recorder connected to both rooms. The first slide was presented to the sender from exactly ten seconds before to ten seconds after the subject was exposed to

the first letter of the experimental trials.

A t-test for correlated means comparing the number of correct letter identifications in the experimental and control conditions for each subject across subjects revealed a significant difference ($t = 1.83$; $P < .04$, one-tailed). The hypothesis that there would be more letters identified correctly with the sender's than without the sender's communication was confirmed. An analysis of variance was performed to test the second hypothesis, that ESP would be manifested mainly with regard to response tendencies characterized by a low probability of occurrence. The letters were classified into three groups according to the frequency with which they were recognized by the subjects in the control condition. The difference between the three groups was significant ($F = 6.88$, $df = 2, 17$; $P < .007$); the subjects recognized significantly more letters in the experimental condition than in the control condition when letters were concerned which, in the control condition, were below the threshold of perception (that is, were recognized by less than one-third of the subjects).

Comparisons for each letter separately between the number of times it was perceived correctly with and without the sender's communication yielded only one significant finding: the letter H, which was more often recognized with than without a sender (binomial test; $P = .02$, one-tailed). Separate comparisons for each subject and for each sender between the number of letters that were identified correctly with and without the sender's communication were not significant. An analysis of variance was performed to test for sex differences. Male and female subjects did not differ in recognizing more letters under the experimental condition than under the control condition.

Separate comparisons were made for sessions in which the sender started with a letter, and for sessions in which the sender started with a control slide. A positive effect of the sender showed up only in sessions in which the first slide projected to the sender was a control slide ($t = 1.86$; $P = .04$, one-tailed). To evaluate the difference between the two groups of sessions, an analysis of variance was performed which yielded a significant difference ($F = 5.8$, $df = 1, 29$; $P = .02$).

The outcome of this study confirms the Kreitlers' work, and supports Rex Stanford's PMIR hypothesis that peo-

ple use their ESP unintentionally in the service of their needs (presented at the 1973 Parapsychological Association convention: RIP 1973, pp. 99-103). Subjects correctly identified more letters with than without a sender's communication. The positive ESP effect was caused mainly by letters which, under the control condition, were recognized by less than one-third of the subjects.

The effect did not depend upon the sex of the subject, and was cumulative across subjects and letters. Only one letter out of 20 was recognized more often with the sender's influence than without it, and there was no individual subject performing significantly better with than without a sender. The finding that the effect showed up only in sessions in which the sender started with a control slide cannot be explained at this point.

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SYMPOSIUM:* ETHICAL ISSUES CONFRONTING PARAPSYCHOLOGISTS

DRIVING SUBJECTS CRAZY

David P. Rogers (University of North Carolina)

The majority of my daily work is done in a mental hospital, in direct patient care and teaching. I also work in the Psychology Department at the University of North Carolina. Being a parapsychologist and clinical psychologist has given me some clients with a mixture of psychic and emotional problems.

In the state mental hospital there's a fellow I'll call "Joe." Joe is in his middle thirties, has lived in the rural South most of his life, and just before I came to this convention I went to visit him to see how he was doing, as I've done for two years. He is on a locked ward, and most of his adult life has been spent in a mental hospital. One day when he was walking down the street years ago the "air changed" suddenly, as he puts it. The atmosphere became dense, and then lightened, and suddenly his body became permeable. His thoughts began filtering out--people knew what he was thinking and he knew what they were thinking. This loss of control caused him considerable concern, and continues to keep him in the hospital.

Perhaps at a convention like this one we can see telepathic events as being fascinating and worthy of further study, but to Joe they're devastating; they've ruined his life. He became panic-stricken; he didn't want people to have mental telepathy; he didn't care if it was interesting. He just knew that he didn't like people knowing what he was

*Chairman: David P. Rogers.

thinking, and he didn't like to know what they were thinking either, because a lot of the things they thought weren't "very nice."

He began to try to run away; he went to various cities, he changed his name (he thought maybe people wouldn't know who he was), and eventually he became depressed because he couldn't get away. So he came to the hospital and was immediately diagnosed as paranoid schizophrenic. He hasn't been able to go outside on a visit for more than a couple of days. Whenever he does, the paranoid ideation returns, the fear comes back, and on occasion he has seriously attempted suicide. Presently he's a "chronic" patient on a closed ward, and I think you can best describe his experiential world as an open wound. He feels open, and he wants to close it up. When I visited him a couple of days ago, I remember being struck by his pain; he had a loaded gun which I took from him, because he claimed he was about to shoot himself in the head.

I am telling this story to point up dramatically an important issue in research: What we see as fascinating and intriguing when analyzed from an objective and scientific point of view can be very terrifying to people when experienced from an internal framework. We should ask ourselves sometimes what it would be like if we lived in a world of ever-occurring psi phenomena rather than just talked about it and studied it. Some people handle psychic experiences well; I'm talking about people who are scared of them. I think there are more people than we realize who are scared of psi when they walk into our laboratories.

Therefore there's a bind that many subjects are put in: The ways that experimenters have been coached to extract ESP from subjects often draw out behavior which, if exhibited in society, would be diagnosed or personally experienced as "crazy." Subjects are very well aware of this split, even if we sometimes ignore it. They are aware that what we're telling them to do is considered crazy by a lot of other people, and I've known persons who have come to the mental hospital, having been subjects at the local parapsychology laboratories, who will not tell you that they've been subjects for fear you'll think they're crazy.

Another problem is that we not only encourage performance in our subjects, but we encourage them to believe in what we're doing. We often tell people that something

exists and that they can have it, that they can do it. I don't think this is quite as common in some other fields as it is in ours. I don't usually tell people that personality exists when I give them a Rorschach, for example. I'm not saying that people shouldn't motivate subjects. We have to. What I'm saying is that we also have to be aware of and ethically responsible for the consequences that this kind of generated enthusiasm can have on somebody once he leaves us.

I recently had a fellow come in who had done two standard GESP runs at a local laboratory. He had scored two sevens, where fives were expected by chance. The experimenter, who acted impressed by these scores, had told him that he was doing above chance, that people with ESP abilities do above chance, and that perhaps he was demonstrating psychic skills. He had been rather confused, anxious, and suspicious beforehand, and such feedback had a harmful effect on his often tenuous grip on reality. He became more confused; he began to act strangely around his friends. He began to think that now he had ESP, so he would stare at his friends and smile, and assume that both parties were experiencing telepathic communication. His friends thought he was becoming very weird and didn't want to associate with him. He became anxious, depressed, and isolated. People began to see him as "crazy." He began to believe it. There was a lot of pain involved in this process of estrangement.

The point here is that before the experiment, his attitudes in terms of ESP were curiosity and speculation. But when an official parapsychologist told him that he had it, it was no longer a matter of conjecture; it was a matter of fact. In recent years, a few people who have been subjects in local ESP experiments have come to me in a therapeutic context convinced of their ESP skills, apparently based on their experience as subjects. In every case I have felt that reports of their performance did not justify their inflated beliefs in their psychic skills.

Too often experimenters do not realize the powerful ways that they can influence subjects. One woman thought the experimenter was so powerful that he must know how to control ESP because of the prestige position that he had in an important parapsychology laboratory. So she began to think that the experimenters could actually control her and send her messages. I remember getting a frantic phone call from her during a P.A. convention that was held in her

town begging me to tell the experimenters to stop sending her messages. As a beginning experimenter, I remember I tried to encourage people, convince them, and work them up. I hinted that perhaps they had special abilities. Then I sent them away from the experiment never to see them again. I would like to say now, in retrospect, that I think that my behavior then was unethical.

Why do we do this sort of thing to our subjects? First, I think many of us have a naive fear based on the powerful and magical qualities that we often secretly and irrationally attribute to psi. I have a hard enough time dealing with my conscious thoughts, communications, wishes, etc. I personally do not want a lot of messages impinging on me from people that I don't know and perhaps don't care to know. If psi manifests itself with the frequency and in the fashion that we seem to find it empirically, I'm not sure if I'm ready yet to embrace it emotionally. I'm not willing to leave this convention and incorporate the research findings presented here on a personal level, to act on them. What I'm going to do is more research, more reading, more thinking, trying to make more sense of it; and then some day when I can really be convinced that it can be personally meaningful and useful to me, I'm going to deal with it. I think many experimenters fear psi on this crude kind of level and deal with it in this detached and intellectual fashion.

The second problem that I think makes us sometimes not face what we're doing to subjects is the lack of confidence we have in our phenomena. The problem of repeatability is an obsessive thing with parapsychologists. It does not seem to plague my colleagues in psychology as much. If an experiment there is not replicated, fraud or artifact are not immediately suspected. More likely, differences in design or some social psychological variables are cited. If something doesn't repeat in parapsychology we often (or at least our critics) don't use these kinds of explanations. We begin to question the legitimacy of the finding. I think that most social scientists do not expect or require exact replication as much as we do. The legitimacy of their field does not ride on positive findings in every study. But in psi we continually stress replication, and we become distressed if we don't get it. I think, in a sense, we have set higher standards of believability than most other groups.

Therefore we are confronted with a dilemma: an irrational fear of the power of the phenomena, and a super-

rational lack of confidence in the phenomena. I was dramatically reinforced in this conviction some years ago when I was in another theoretical symposium and Montague Ullman made the statement that he had had some personal ESP experiences which he had felt sufficiently comfortable about to reveal at another, non-parapsychological convention, but wouldn't tell parapsychologists about. I began to wonder why. And when I began to think about it, I realized that I wouldn't either. I asked him later why he wouldn't, and he said to do so among experimental parapsychologists would be "unsanitary."

I've noticed in myself and in my colleagues something that I can't avoid: a feeling of discomfort that comes over me when I meet a psychic who confronts me with a multitude of rich data. I either try to imagine how I could test what the psychic tells me, or I begin to wonder if I am going to be annoyed by another "weirdo." I think if we note our reactions to psychics, many of us will realize our own ambivalence about psi. The positive value of this ambivalence is that it drives us to more intensive study. It spurs commitment for me--this area has too much meaning and potential to ignore. The negative value, which is the focus of this paper, is that in order to justify our study, in order to clear away the ambivalence, sometimes we try to convince ourselves of our work by convincing our subjects. In a way, we live through their beliefs, and we justify our own beliefs by having followers.

But this is an ethical symposium, and I would like to conclude by talking about some ethical issues stemming from my previous remarks. If we upset subjects without knowing it, we have a special responsibility towards them. I think in science a test is usually tentative; we assume the null hypothesis. I think we are lying to our subjects when we pretend that the tentative is a fact, when we tell them that they can do something that we haven't even discovered yet.

I think that we can handle the situation. All we have to do is debrief subjects. I think that if we encourage and support and drive a subject to do well, we must afterwards tell him why we did it. We need to tell him that this doesn't necessarily reflect on his ESP skills, since we don't know about them. We should give him precise information in terms of probability statistics about what it is that he actually did. We should give him short resumés of what the project is attempting to do, if he wishes.

This leads me to a more broad concern, and that is the need for professional ethical standards in the Parapsychological Association. Do we need them? I, of course, think we do. Some people may think that this will hamper free investigation and that experimenters must have full independence. I think that we need some quality control in our experimenters. We need to educate experimenters as to what should and should not be done with subjects. I have seen too much negative spinoff from the lack of such standards to feel that the establishment of standards and a professional ethics committee is an irrelevant task. If what we're doing as experimenters is respectable and important, I think that we must make our expectations and our rules of conduct explicit, rather than implicit, and begin to test the maturity of our relationships to one another.

THE PSYCHICAL RESEARCH COMMUNITY: CONCERNS AND RESPONSIBILITIES

Blue Harary (Psychical Research Foundation and Duke University)

Before exploring questions which confront us specifically as members of the psychical research community, it is pertinent to dwell generally, for a moment, upon the concerns of most scientific groups. On their own, individual scientists lack the perspective or energy to have a meaningful impact upon the quality and direction of the social and scientific endeavors in their chosen field. In an organized community, however, scientists may keep track of each other's work and opinions through publications and conferences. The scientists in a given field can embody their concerns in a constitution and by-laws which state the scientific and social expectations of the group for itself and for individual members. The members heavily depend upon, yet are responsible for, the effectiveness of their organization to reach their collective objectives.

The Parapsychological Association (P.A.) provides a context and frame of reference for psychical researchers. Without our actively concerned participation, however, the P.A. would be impotent and unable to meet our needs. In terms of responsibilities which should concern a decently

organized scientific community, and for which it should have mechanisms, the P.A. falls short in some areas in its ability to cope with present-day needs of psychical researchers. Although the early constitution may have been adequate for helping to serve the formative needs of the P.A., it is now in need of careful revision, in some areas, in order to help equip the P.A. to handle more global obligations of the present time in our history.

Although each of the points that follow relates to certain passages in the P.A. constitution, it is not necessary to quote the constitution directly. It might be meaningless to simply alter the constitution to any degree in the thought that this act alone would significantly affect the practical workings of the P.A. community. It might also be somewhat extreme to avoid altogether laying down flexible guidelines in either the constitution, by-laws, or an accompanying ethics guidebook (similar to the one in use by the American Psychological Association) which would be available to all of us for referral. The workings of any such organization as the P.A. are smoothest when crises can be coped with through accepted and understood legislative channels rather than through means that must be first arrived at in the midst of difficulties. More importantly, a well organized but flexible community avoids crises by channeling its operations along the safest possible route and fulfilling its responsibilities by the most efficient and acceptable means with the least procedural confusion possible.

Responsibilities of the P.A. include fulfilling both scientific and social obligations. Some important areas with which the P.A. might concern itself include quality control in all aspects of research including selection of personnel, publication, and eventual possible application of psychical research findings; the relationships which psychical researchers have with one another and with other scientists and members of the general public community; the relative standing of parapsychology among the sciences; and the integration of psychical research findings with those of other branches of science.

Social responsibilities of the P.A. may not be bounded solely by self-interest in the field's own advancement. The organized community may wish to accept other social responsibilities as well. As psychical research progresses, the P.A. may need to become more concerned about the theoretical or social implications of findings and methodology.

The possible eventual application of the field's findings may, in the future, be governed by ethical concerns and directives of the community as a whole.

The most critical concern of the P.A., the quality of the research effort in parapsychology, is dependent upon the scientific qualifications and personal integrity of members and associates. Yet, once an individual has been admitted to the organization, these aspects of his professional contributions and relationships are somewhat difficult to guarantee under the present system. Since the P.A. has been conceptualized as a responsible agent for the advancement of the field of psychical research, as psychical researchers we may wish to empower the P.A. to take a more active role in the defining of specific social, ethical, and scientific expectations of the interactions and contributions of the membership. The P.A. may also need to outline carefully the mechanisms and criteria by which an individual may be judged to have fallen so short of those expectations that his continued participation in the P.A. is no longer desirable, on a permanent or a temporary basis. Regrettable recent events make it evident that such a serious circumstance can arise.

An organized scientific community may be expected to fulfill the responsibility of regularly providing the proper conditions for creative and intellectual exchange between its members. This can be accomplished through the medium of professional publications and in the context of community conferences. In the case of the P.A. the annual convention has, in the past, consisted primarily of sessions which have been almost entirely dedicated to the reading of papers that would later be published in detail anyway, as well as social gatherings. There has usually been little time allotted for collective discussions of specific presentations by the group as a whole. In the best interests of psychical research, perhaps the annual convention should be construed as serving both scientific and social functions for the P.A. There may be ways in which the annual convention can be made to be a more effective time for sober reflection, for a free-flowing open exchange of ideas and criticisms, for the considering of how well, or poorly, the P.A. may be meeting its objectives and responsibilities, and for the defining of new directions and commitments by the group.

To be capable of performing its functions most efficiently, the Program Committee for the annual convention needs to be able to define, at least generally, minimum

standards for acceptable research. At the present time, the depth with which members of the Program Committee are to go about fulfilling their task, and the minimum standards to which they are to adhere in so doing, are mostly matters of personal opinion. Perhaps it would not be practical for the P.A. to define its criteria for scientific quality control too strictly. How equally impractical is it for the P.A. not to define its criteria for acceptance or rejection of particular pieces of work at all? The relationship which the Program Committee is intended to have with the Council may also need to be carefully spelled out to avoid unnecessary possible confrontations over the limits of each group's powers.

The 1974 annual convention has dedicated one full day to education. It is hoped that this will set a meaningful and positive precedent. The P.A. is taking steps to improve the quality of open discussions of presentations by making efforts to familiarize participants with the nature of presentations prior to the actual convention date. There are indications of a trend toward increasing numbers of regional professional symposia concerned with issues and research in parapsychology. These commendable, socially responsible, acts suggest that psychical researchers are keenly interested in fulfilling their responsibilities as members of the scientific community.

If, as psychical researchers, we do not become our own most severe critics, scientists from other disciplines, as they have demonstrated in the past, may be more than pleased to be severely critical for us. Becoming our own severest critics may mean empowering P.A. committees (such as the Program Committee) to examine the validity of experimental design and statistics and to explore the ethical nature of research efforts. The current position of psychical research seems to be such that, unless researchers are to enjoy an unfortunate collective reputation, when scientists from other fields and individuals from the general public examine even the fringe endeavors of this field (and especially widely publicized research) our endeavors had better not be found to be scientifically or ethically lacking. Perhaps it might be refreshing for the P.A. to openly condemn sloppy research and publicly censure shady "researchers," sensationalist publications, and unethical practices related to this field, instead of following an apparent policy of "see no evil." On our own, as individual scientists, we are not capable of effectively taking a stand on what occurs in the psychical research community. The Parapsychological As-

sociation must, therefore, take our stand for us.

THE ETHICS OF COMMUNICATING IN A SUPER-COMMUNICATING SOCIETY

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

My topic is the ethics of disseminating scientific information to the public in a society such as our own, which has powerful communications media, well entrenched in the power structure, that are constantly searching for new material. In earlier days the relationship of science to the media was more favorable than it is now, and even today the "hard" sciences have several advantages over the behavioral sciences.

For example, in the harder sciences, communication is relatively easily restricted and controlled by the researchers themselves, and professional publication as a matter of course precedes media publication. When an article appears in a professional journal it undergoes, in its course of acceptance and immediately following, a peer review system which acts as a buffer between the researcher and the very reasonable errors that he easily might have made. When the information is later disseminated to the media, those errors have been corrected through the review system, so that a buffer exists between the researcher and the public. The public, although it can't possibly appreciate all of the details of the research, is then in a better position to use the information.

When media publication occurs, it should be done by qualified serious writers, rather than sensational writers. Again, the hard sciences have the advantage here. In recent years the National Association of Science Writers has been active. It is an affiliate organization of the American Association for the Advancement of Science. One must meet certain standards to be a member of that association, and they pride themselves that their writers take the time to insure the accuracy of their stories. Most major publications have science writers drawn from this association. This is an ideal system in that the researchers involved don't disseminate information until they are ready.

Scientists in the past have been successful in avoiding sensational media coverage because the words used in most branches of science are basically beyond the vocabulary of the lay individual. A phrase from a report in a technical journal has little apparent direct implication for the conduct of people's daily lives, so sensational writers haven't taken much interest. Also, in earlier days the media were not very extensive. When radio and TV came along the media found ways to make hard scientific findings a little clearer and more interesting.

With the growth of the behavioral sciences, a problem arose. People were confronted with familiar language. Many journal articles within the behavioral sciences could be understood in part by a wider variety of professional writers. You don't have to be a science major to be able to get information from an article in the behavioral sciences that will titillate your readers. Also, lay people now have better backgrounds and are able to demand more information.

Today the media can legally report on anything that is in print or that they hear at a symposium; they can at any given moment call a researcher to request an interview or TV show, and often the researcher finds it hard to explain why he thinks it would be premature. Effective media regulation would, of course, seriously curtail freedom of the press.

What kind of relationship has existed between the media and the Parapsychological Association (P.A.)? P.A. members have in the past been by and large passive users of the media. We speak when spoken to. The P.A. to my knowledge has never called a press conference, has never issued a position paper on anything. Very few individual researchers within our field have taken that step. Some of our members, of course, do not even speak when spoken to. If someone calls up for an interview, they reply, "I'm not ready yet." Or else, "I seriously doubt that what I would have to say would be something that you would be interested in."

What about non-P.A. members? Some are the "super-quiet" people that we P.A. members will never know about, who are off in their labs trying to repeat a result. Often they are afraid of getting positive results; if they succeed they won't talk about it. If they don't succeed they tell a few close friends that ESP doesn't exist. These people don't

ever deal with the media; they are never approached by the media and they never do the approaching themselves.

There are also the "super-noisy" people who try to attract the media to any results that come along, knowing that many of the media are not oriented toward careful research evaluation. These people often achieve considerable media attention, proportional to how interesting they can make their results seem. Making results interesting generally means avoiding details of research design or analysis. Media people differ, of course, and some of them honestly don't know how to interpret a piece of research. Some, on the other hand, recognize the difference between science and non-science and, for their own purposes, what they really want is enough of the former to justify the latter. They go in and get a little dab of "science," which then "validates" just about everything else that they really wanted to talk about in their article.

I think we have a serious problem here. Since so much incorrect, partial, and superficial information is being disseminated, society now has two definite needs from the parapsychology research community. The first is a description of the findings of parapsychology as they may apply in useful ways. This is a positive need, one that we can generally meet by using standards of responsible reporting; the scientist continues his research without fanfare until it reaches a point where he feels it can be publicly discussed; then he reports it along with its potential applications.

The second need is for a description of what parapsychologists don't know, and why it is that we don't know it, despite others' claims that we do know it. This is an old problem, but one of recently-acquired seriousness. By using the old ethical standards we may possibly not be able to meet this need adequately.

The solution that I tentatively propose is to form an active partnership with responsible elements in the media, especially science writers. Right now we don't have the funds or the personnel for it. It's worth articulating now, though, so we can begin to make plans. We should have workshops for media individuals; this year's educational symposium was a good start. How about symposia in which we interface specifically with media people? We need to educate writers, we need especially to educate members of the National Association of Science Writers, because most of them

don't really know much about parapsychology. I have rarely been interviewed by a member of that association, and the same I'm sure is true for my colleagues.

An alternative course is that parapsychologists themselves take the time to learn to become communicators. We badly need to be able to get our messages across to people. When irresponsible popular books are published, instead of merely complaining about them in clever book reviews or privately, we should take steps to make our opinion known. This would include giving the public specific details about why the books are wrong. Perhaps the P.A. should seriously consider requesting press conferences or issuing position papers upon appropriate occasions. I think we should develop and insist on using effective ways of talking about the inherent complexity of our studies, because it certainly is rampant in all that we do. We should take the initiative in warning consumers about shady practices popularly associated with our field.

Two recent developments show promise along these lines. One is that the P.A. now has an Information Services Committee, headed by Dr. Robert Van de Castle, which is just starting to get off the ground. We welcome all suggestions you may have as to the roles it could reasonably serve within the obvious restraints of our Association. The second development is that we will be presenting at the January, 1975, A.A.A.S. convention a symposium called "The Application and Misapplication of Findings in Parapsychology." Several of the papers will deal extensively with the simulation of psi phenomena, the difficulties in courses that claim to teach psi ability, some of the theoretical problems involved in research in the field, and so on.

This is only a beginning. There are obviously a lot more things we have to do, but as we start to think about how we drive our subjects crazy, perhaps we should also think about what we, through misinformation or silence, have done to them before they even decide to become subjects.

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TACIT COMMUNICATION AND EXPERIMENTAL THEOLOGY*

Robert L. Morris

Unlike past Parapsychological Association presidents, I don't have an extensive research career behind me that has given me insights into some specific research area. Therefore, today I will share with you some of my beginning thoughts about the field in general and its problems. What follows is Version One of what will undoubtedly develop into several different versions. Let us start off by focusing on difficulties in the terminology of the field. Many of us view parapsychology as an interdisciplinary problem area. The term "parapsychology" would seem quite inconsistent with an interdisciplinary approach. Along similar lines, some of us attempt to shift the emphasis to more "solid" disciplines. "Biocommunication" and "paraphysics" are examples that come readily to mind. Each arbitrarily declares the centrality of one discipline as opposed to the others.

A lot of the more specific terms that we use seem to have built-in assumptions behind them: extrasensory perception, for instance. Much of our research calls into question the notion that what we are studying is a form of perception; on the other hand, just because we have not histologically delineated a biological sense transducer for "psi messages" does not enable us to conclude that we never will. Also, terms such as the "fearsome foursome" of telepathy, clairvoyance, precognition, and psychokinesis have lost some of their capacity to define distinct concepts or even distinct operational procedures.

Consider the situation in which one organism attempts to communicate information to another organism. If the two species involved are of similar central nervous system de-

*This is the Presidential Address, given August 22, 1974.

velopment, we generally label that telepathy. For instance, if one human being is trying to influence the thoughts of another, that's called telepathy, and we generally regard the receiver as the one whose psi is being tested. If in fact the "receiving" organism is considerably lower in central nervous system development than the "sender," suddenly we have what many would call a psychokinesis study in which the subject, whose psi is supposedly being tested, is trying to influence the behavior of a small animal, which is obviously inferior to him (e.g., Metta, 1972).

Thus telepathy and psychokinesis can each be used to describe basically the same procedure. The use of one but not the other of these terms in describing a study to which both could be applied may confuse some of the basic ways in which we go about trying to understand the processes at work.

Many writers in the field consider the agent superfluous in telepathy studies if in fact there is an objective record of the target, and feel that successful telepathy studies can be taken as evidence for psi only in the receiver. In psychokinesis studies, the "influencer" is the one whose psi is being studied. We generally do not regard the target as having shown psi.

I'm sure most of you are well aware of the difficulties we've had in general in trying to isolate a "pure" telepathy study, a "pure" precognition study, or a "pure" psychokinesis study, since these difficulties have been explored in detail elsewhere (e.g., Rhine & Pratt, 1957). Briefly stated, studies of supposed pure mind-to-mind (telepathy) communication fail because to achieve public validation of the success of any such communication attempt, there must be independent publicly available records of the sender's message and the receiver's response. Given such a public record, clairvoyance (object-to-mind communication) becomes an alternative explanation. Studies of clairvoyance could all be due to precognition.

Studies of precognition have difficulties because once the subject has registered his guesses, the targets must somehow come into being in such a way that they could not have been influenced by anyone else's psi ability, including the experimenter's. Given that we at present can set no theoretical limits upon what psi ability can do, we cannot completely guarantee that no later psi influences were exerted. To test for pure psychokinesis, we must guarantee that

no one involved in determining the PK target order could have used precognition of the actual target event outcomes, such as to preselect a favorable assigned target order. Unless very large effects are obtained, such as movement of static objects, PK is hard to separate from ESP.

There is also disagreement over what terms and concepts belong in parapsychology. The influx of the term "paraphysics," I think, is something that illustrates this. We don't know what processes are involved in the production of psi. One possibility is that some form of energy transfer is involved that we don't understand well yet. As we look at the world around us, if we see some unusual distribution of energy or some unusual manifestation of an apparent energetic force, it becomes tempting to say, "Hey, we don't really know too much about that, maybe that's it, maybe that's what we've been looking for." So we then rush in, measure it, and correlate it with events and experiences in the surrounding environment.

We don't really know what to call what we're doing, however. Is it paraphysics? Is it physics? How do we really want to use the term energy in this context? Some writers have even attempted to use the concept of "non-physical energy." The mixture of terms from physics and biology with others having their origins basically in philosophy, psychology, and our ordinary language system has failed to produce a set of terms that reasonably bridges these disciplines. Perhaps one of the reasons why an interdisciplinary approach to psi hasn't been tried too much before is that the terms psi researchers use are not really shared by the disciplines mentioned above.

I would now like to explore with you the possible development of a more general conceptual system and a terminology accompanying it. The specific question to be asked is: Are there certain basic underlying threads of meaning, amongst all the things that we've come to regard as possibly parapsychological? What is psi, basically? Psi has been described on some occasions as a meaningful coincidence. Two things seem to coincide in a way that is meaningful, and yet we cannot understand the apparent linkage between them. One way of saying it is that psi represents an apparent coincidence between something going on in an organism and something going on in an element of the environment, in such a way as to imply communication between the two. There seems to be a flow, or an exchange, of information

between organism and environment in a way that we don't really understand, and that doesn't seem amenable to a ready understanding.

Since what I'm saying is that communication is implied here, let's try to take the most basic simple-minded model of communication that we have, one which uses four concepts: a source, a receiver, a channel, and a message. These are all defined fairly simply in ways that make sense in terms of the way we usually use the words. A source is an aspect of the environment that is capable of emitting information; specifically, a source is something that in any given system does in fact originate information. A receiver is an aspect of the environment that is capable of receiving and transforming or transducing that information such that it becomes overtly manifest that the information has indeed arrived.

The message essentially is the information or pattern or meaning transferred from the source to the receiver. The channel is the means by which the information is transferred. As I speak to you, I am the source, you are the receivers, the message is the content of my words, the channel is sound waves. So this is in some ways a fairly dull set of terms, but they may have a fair amount of generalizability.

These definitions that I've given you must be modified slightly in order to accommodate what we mean by psi. In order to do this we need the concept of the channel environment. The channel environment of the receiver is that portion of the total environment to which the receiver has access at a given moment via presently-understood channels of information flow. My channel environment, then, is anything going on out there that I potentially can know about through understood channels, understood means of communication. Psi becomes an event in an organism which coincides with an event external to the channel environment of the organism in such a way as to imply a flow of information from environment to organism or vice versa. More simply, psi becomes apparent information flow for which no channel can be specified.

Since no channel is specifiable, communication itself has an element of ambiguity to it. Thus it may be appropriate to identify psi as an implied or apparent or tacit communication. It is <u>as if</u> communication took place, but one

cannot say for sure. There seems to be a receiver, and a message, and a source; we don't know whether there is truly a channel or not.

I'd like to go now to applying this set of concepts to the fearsome foursome of telepathy, clairvoyance, precognition, and psychokinesis. These are the terms that have stuck with us the longest and certainly have intruded liberally on our presentations throughout this convention. In telepathy procedures, basically, two organisms are placed outside each other's channel environment. One organism is then fed a message, and the other is examined for evidence indicating the presence of that message. In clairvoyance procedures a physical object or event is designated as source and placed outside the channel environment of a designated receiver organism. The receiver is then monitored for evidence that information from the source was acquired by the receiver.

In precognition procedures an object or event (including perhaps an organism) is designated as source, and an organism is designated as receiver. The receiver is monitored for evidence of information about the source only in advance of the presence of the message information in the source, thereby automatically placing it outside the channel environment of the receiver at the time of its monitoring. Of course, it's necessary to insure that other determinants of the eventual designated message are also outside the receiver's channel environment up to and through the time of monitoring. In psychokinesis procedures an organism is designated as source and an object, or another organism, is designated as receiver. The source organism must be outside the channel environment of the receiver at the time the receiver is being monitored. A message is fed into the source (e.g., "Make the dice come up sixes," or "Keep the light on"), and the receiver is monitored to see if its subsequent performance reflects the message presented to the source.

Even with this simple system, however, we still have a time problem. Given that we cannot specify a channel, and therefore cannot at present fix in time the moment of communication, we introduce a certain ambiguity with regard to the true source and receiver, especially if we are willing to take precognition seriously. We also must view the spatial and cognitive limits of the potential set of messages we could communicate as unknown or uncertain right now. I'd

like to use two examples to illustrate this: the "chair test" and the "psychic shuffle."

Chair test procedures vary (e.g., Berendt, 1973; Eisenbud, 1973), but the following is representative. Someone designated as the source, generally a psychic, is given the number of a chair in a lecture hall and asked to describe the person who will sit in that chair at a future lecture. The chair number is then sealed inside an envelope and placed with a similarly sealed transcript of the statements that the psychic gave about the chair's future occupant. Later, notices about the lecture are put up, people decide they want to come to the lecture, they come, they arrive at the door; as they arrive at the door they are assigned chair numbers, and they sit in those chairs. Then they listen to the lecture, and at some time during the lecture the transcript is read. The people are then asked, "OK, anybody who feels that's you, please stand up, and also please rate to a certain extent the degree of correspondence between you and that description." So then there are all those people in all those chairs, one of whom is sitting in the right chair, and they're each being asked essentially to respond to or relate themselves to information that is right now inside an envelope in the front of the room. If the right person stands up, the study is considered successful.

Such studies are generally called precognition studies. We designate the psychic as the source. We say the information is flowing from the future back to him at the time he makes his statements. Supposedly the psychic is looking ahead into the future, seeing someone sitting in the chair, and responding. There's an ambiguity to this, because there are all kinds of potential sources and receivers in such a study. Let's explore some of them very briefly. Once the psychic has given his responses, there's a whole batch of information now on record; somebody's typed it up, he's thought about it, it exists probably in the memories of several people, it exists in typed copy and perhaps on duplicate copies, and so on. Each of these may now serve as a potential source, or the total of them may serve as a set of potential sources.

Consider next all the people who have just read the advertisement for that lecture, or who have otherwise heard of it. Maybe some people who fit that description especially well are drawn towards attending the lecture. Maybe some information is flowing to them, maybe they're now the re-

ceivers, and the psychic is the source, or some of these other records of his statements are the source. They then decide to come, and other people are undoubtedly now deciding to come, others who don't resemble the psychic's description so much, but who would still like to see what happens.

So here are all these people making all these decisions about when they're going to arrive at the lecture hall, and in what order they're going to be given the number. Maybe they're serving as receivers at those moments, as they're deciding how long to take brushing their hair, or whatever. Other people have already made decisions about how the number is given out. All wend their ways toward the lecture, and things get tighter and tighter, and finally bingo! Mr. or Ms. Right winds up right on time, gets that number, goes in, sits down, and will surely respond to the statement when it is later read, thus producing a successful study, but not a true precognition study.

Another alternative possibility can be seen if we consider what happens after the transcript has been heard. Now everybody's sitting there trying to decide whether or not he or she is that person, in which case at that moment the number sealed inside the envelope could serve as source, with the receivers being all those in various numbered chairs who are deciding how high they should evaluate their own correspondence to that description. All things being equal, whoever is sitting in the designated chair may be more impelled to respond than an equally plausible person in the next one, thereby guaranteeing success in a different way. And because we can't say when the information flow is taking place, and can't specify a channel, in many kinds of studies such as the chair studies we also cannot specify in fact who is the true source or who is the true receiver, or at what time they are functioning as such. We may have to deal with a set of potential sources, and a set of potential receivers.

Now let's go to the psychic shuffle. The psychic shuffle was a testing technique that originally arose from the early attempts to study precognition. In the early precognition studies the designated subject was asked to guess the order a deck of cards would have later on. Then somebody had to figure out how to order that deck of cards later on, in such a way that he was not using his own psi to produce a favorable order. One way was for the experimenter to

shuffle the cards and lay them out. In this design the subject was construed as the receiver and the post-shuffle card order was the source.

It was argued that maybe the person shuffling the cards was serving as the receiver while shuffling, was using those already-existent guesses as source and was unconsciously using psi to help him shuffle the cards so as to have them match the guesses. So a study was done in which subjects were given a deck of cards and a concealed target order. The subjects' task was to shuffle their cards, such that they matched the concealed target order as much as possible. Some people seemed to be able to do this, enough certainly to place that whole set of procedures for selecting precognition targets in doubt (Rhine, Smith, & Woodruff, 1938). That procedure isn't used in precognition studies any more. Recently, however, H. K. Kanthamani (1974) and I (Morris, 1973) have made use of this technique as a psi testing procedure in and of itself. Both of us focused primarily on the gifted subject Lalsingh Harribance, who had previously shown ability at this test. Dr. Kanthamani's work was done in a variety of different ways; I'll be describing my own work here.

We used a situation in which two people sit on opposite sides of a table with a wooden barrier in between, so they cannot see below each other's eye level. One of these people (Harribance) takes out a sheet of paper and makes fifty standard ESP card guesses. The other person then takes a deck of fifty unused ESP cards and starts shuffling them in a series of shuffles. Harribance is asked to stop the shuffling by calling out whenever he thinks the just-shuffled order extensively matches what he's just written. If the two orders match, we can say that ESP has been shown and that information flowed from one side of the table to the other.

There are several possible source-receiver-message combinations in such a procedure, as in the chair test. One possibility is that when Harribance first makes out his fifty guesses, he's serving as the receiver right then and the source is in the future. He just simply looks ahead and sees what the order of the shuffled deck will be when it is stopped, and fills out his calls accordingly. Another possibility is that once he's filled out his initial calls, then the person doing the shuffling may shuffle in such a way as to give a succession of strong correspondences to Harribance's earlier

guesses, such that whenever Harribance ends the shuffles, the results will be positive. Maybe that person is then serving as receiver for the original guesses as source. A third possibility is that whenever Harribance is trying to judge those shuffles and decide when to say, "OK, stop now," he may be serving as receiver and the order of the shuffled cards at the end of each shuffle serving as source.

In this particular study, there are two possible occasions in which Harribance could be showing evidence of ESP by serving as receiver. One is whenever he makes out his original list of fifty guesses, the other is whenever he tells the shuffler when to stop. We can look for aspects of his behavior on those two occasions to see whether or not they correlate with success at the final matchup. We found that if we looked at Harribance's patterns of behavior the first time around, such as how long it took him to make his calls, whether or not there were certain calling patterns in his guesses, and so on, we found no relationships with scoring success. If, however, we looked at some of the aspects of his behavior at the later time of choosing, specifically how many shuffles he allowed the shuffler to make before being stopped, we found that at that time Harribance's behavior really mattered. If the shuffler was stopped after less than four shuffles, no information flow seemed to take place. If four or more shuffles were allowed, however, the results were good. This indicated to us that perhaps information flow was occurring in only one place in that design, i.e. during Harribance's last time of decision-making, and not in the other possible places. Of course, we would have to look a lot further to verify that hypothesis.

In order to decide what long-term or short-term variables affect the receiver's performance, obviously we have to know which organism is the receiver and at what period of time it is serving as such. Otherwise we could easily collect a lot of information that doesn't really bear very much on anything. We could know all the psychophysiology in the world about somebody, but if that person wasn't in fact involved in the real information exchanges, it's not very useful information. In order to learn what short-term variables, such as mood or directedness of attention, affect the receiver's performance, we have to know what responses of that receiver should serve as our focus of attention. We not only have to know <u>who</u> the receivers are, we have to know <u>when</u> they do the receiving in order to know what data are <u>most</u> pertinent. For analogous reasons, in order to learn

about any source variables we must be able to identify the source and the time period during which it serves as source.

A point of experimental design arises here. If we can identify with certainty neither source nor receiver, we must at least be able to specify that no member of the total set of possible sources falls within the channel environment of any member of the set of possible receivers, in order to know that we even have a genuine parapsychology experiment. It becomes necessary to examine the experimental procedure in real detail, so that we have taken into consideration all of the possible source-message-receiver combinations. If we don't we may miss a real weakness in design, such that some kind of ordinary information flow takes place.

Given what I've just said, which may serve in some respects as basically the definition of a true parapsychology experiment, or of a psi process in general, we should attempt as best we can within a given study to trim the number of possible source-receiver combinations. Since we cannot separate information transfer from information manifestation, until our precognition question is solved, we should aim many of our theoretical studies in the direction of constructing models of specific time-locked information flow, such that we can then generate predictions from them.

In my opinion, therefore, in describing or characterizing our studies, I think we should drop the old favorites such as telepathy, clairvoyance, parapsychology, precognition, and psychokinesis, except on social occasions. I think we should talk in general about psi, or tacit or implied communication, or implied information flow. Also, we should use terms such as designated source, designated receiver, and designated message, referring to those components of the communication system construed as source, receiver, and message by the experimenter. At the same time we should specify other sets of potential sources, receivers, and messages as they may pertain to the specific procedure employed. When we do a study, we should discuss any conclusions we make within the context of how well we feel we've really delimited what sources and messages and receivers we're talking about.

Of course, a main concern of ours when we observe such communication is how do we explain it, and what can we learn about it? By applying the kind of simple-minded communication models that I have just presented, I think I

can come up with at least nine kinds of theoretical possibilities of how information flow might potentially take place. None of these possibilities deserves to be called a theory yet; far from it. Very few of them are even creeping towards model status. But I think conceptually they may be useful to consider. In general, what I'm talking about here is explanations for the successful outcome of a general experiment. We select a receiver, designated as such, select a source outside the receiver's channel environment, manipulate the source, monitor the receiver, and assess the receiver's output with respect to what we put into the source.

Our first two possibilities have nothing to do with parapsychological theories. The first one, which we shall call Defects in data treatment, says that the logic of the mathematical comparison between source and receiver is flawed, such that information only appeared to go from one place to another; i.e., the match between source and receiver was not meaningful at all whenever appropriate mathematical criteria were applied to it. This is essentially, of course, a subset of objections to certain kinds of psi studies with which you're already familiar, such as arbitrary selection of data, optional stopping, using statistical tests whose requisite assumptions are not met by the data, using too many ways of looking for psi, and so on. With regard to the latter point, sooner or later you're bound to find something, if you look for tight variance and expanded variance and psi-hitting and psi-missing and displacement to the 25th order; only a fool would not be able to find "psi" somewhere in the data as a whole, especially if several different conditions are involved. Some have argued that it might be possible to demonstrate mathematically zero information transfer under certain circumstances, but I doubt it.

Number two, which we shall call Defects in procedure, is the possibility that the source, or its determinants, were not truly outside the receiver's channel environment. Here we have simple flaws in the design of the study or new applications of old principles of information transfer that have been neglected or forgotten. We heard a description this morning by Stevens, Burton, and Joines ("Charge Build-Up on the Body as a Basis for the 'Human Aura' and Certain PK Events" [see pages 77-80]) of how perhaps human beings can physiologically build up static electricity charges on certain of their body surfaces in some way. If the person then exerts a great deal of mental concentration plus abrupt physical movement such that a little ball moves a little bit,

perhaps he was essentially deploying such a static charge in some way so as to induce movement through means that we already understand (i.e., Faradic induction), but haven't seriously bothered to apply in this circumstance.

Number three is the possibility that some new kind of channel or channels exist which allow the source to emit or originate information through some pattern of impulse, a particle or wave or a combination of the two, or some kind of analog, similar to the way that we obtain other information through sensory transduction of information. The transducer would therefore be at the physical site of the receiver. One might call these in general "neo-energetic" systems of communication. Implicit here is that some sort of force or something that might truly be deserving the concept of energy is involved; i.e., that there's a bona fide impulse-type propagation. The Greek word othein means "to push"; it comes from osmos, which means "impulse." So I came up with the term Otheistic communication to refer to such neo-energetic communication systems.

The fourth possibility, which we may call Otheistic scan communication, is that perhaps certain receivers may emit impulses of some kind which reach the source, interact with it in such a way as to acquire information, and then return to the receivers à la sonar, thereby carrying information. This would still be an energetic mode of some sort; it might be regarded perhaps as a subset of the otheistic mode just talked about. Such a mode could be used as a device to scan the environment for relevant information, as has been suggested for psi by several researchers (e.g., Stanford, 1974).

Number five, which we may call Self-extension communication, is the possibility that organisms may have the capability to extend some real aspect of themselves beyond their physiological boundaries. This aspect, if extendable in this way, may have the ability to transduce information directly from sources beyond the channel environment of the physiological organism. In this case, the actual transduction of the information may occur at the locus of either the source or the receiver, depending on which is extending or expanding to which. The emergence of the message at the physiological receiver would then be due to information propagation within the greater boundaries of that receiver, plus some kind of intra-receiver processing. We would then in our research be obligated to study the nature of intra-re-

ceiver information flow channels in addition to our usual studies of receiver-environmental interactions.

Number six, which we may call Otheistic self-extension communication, is just a combination of three and five, which would simply assert that organisms may have the capability of an extension beyond the physiological self and can transduce information outside of the physiological body, but they must transduce some kind of information carried by some new form of impulse, or energy, that we don't yet know. This represents little more than a logical extension of the other two possibilities, plus a recognition that they are not necessarily mutually exclusive.

Possibility number seven, which we can call Superordinate source communication, is that there may be some kind of coordinating source entirely external to the designated communication system. Such hypotheses basically assert that the construance of a limited source-receiver system is inappropriate. This external coordinator may take the form of a common source, with the designated source and the designated receiver both serving in fact as receivers. Such hypotheses can deal very much with terms that presently exist within various theological systems. For instance, there may be some super-entity or super-power of some sort, having intelligence, and capable of coordinating and regulating information in such a way that it somehow imposes coincidences between designated source and receiver in ways not apparent to the "experimenter." Here may also be included various other systems of theory or belief which talk about various intelligent organizing principles, basic mind-stuff from which matter has evolved, the collective unconscious, extraterrestrial beings, and so on.

Number eight is what I'm calling Information slosh communication. The basic suggestion here is that perhaps information is oscillatory in some way that we don't presently know or understand. Being not a physicist nor a mathematician, I don't know very much about the use of the term "dimensions." I know we can have fifth dimensions in mathematics that are very nice and tidy; we can have them in music that are very nice and tidy. Whether we can have them within the context of such a system as this, I don't know. It is possible, for instance, that there is some sort of slosh of information back and forth within an extradimensional context, as has often been suggested for time (e.g., Dunne, 1938). Information slosh models can essentially ap-

ply to any kind of proposed system which posits that information is really being exchanged rather rapidly among apparent subsystems within the overall source-receiver system.

Number nine deals with what's called Message ubiquity, which means that all or maybe some information is in some real sense present everywhere at any given moment, and just simply needs to be released by some change of state in either source or receiver or both. There is, therefore, no true transfer of information at all from any source to any receiver under this last set of models. The entire source-receiver-channel model is hopelessly inappropriate. The information is all over the place. Such possibilities will be difficult to investigate for some of the same reasons that the ether hypotheses were difficult to investigate a long time ago. If information is in fact all over the place, we cannot isolate a condition of no information for comparison purposes. In any kind of experimental study, sooner or later we seek a restriction to the ability under study, a limit of some sort which enables us to examine the effects of different variables, and so on.

These nine general categories, as stated earlier, represent groupings of potential theoretical explanations of psi. Defects in data treatment represents a set of specific hypotheses drawing from present knowledge of data treatment procedures rather than any new theories. A possible exception is the notion of Spencer Brown (1957) that the usual laws of probability need to be modified because occasionally one may get unusually large numerical deviations from calculated chance expectation for purely mathematical reasons that we do not yet fully understand.

Defects in procedure also represents a set of specific hypotheses drawing from our present understanding of physics, biology, and psychology about the various methods by which information can be exchanged between organisms and their environment. Such hypotheses when explored in detail have often led to increased understanding of such largely ignored signaling systems as subvocal whispering, small body movements, and weak biological fields surrounding tissue (e.g., Stevens, Burton, & Joines, already cited), and will undoubtedly continue to do so, thereby serving a valuable function in themselves.

Otheistic communication systems would involve either

new energetic forms beyond those we presently understand, or at least a drastic modification of our understanding of present forms of energy. Several suggestions along such lines have already been made (e.g., Berger, 1940). Any specific theoretical developments along these lines, if supported experimentally, would represent genuinely new additions to the body of scientific knowledge. The same would be true of Otheistic scan communication.

Self-extension communication and Otheistic self-extension communication would involve either some sort of spirit-soul theory or theories about psi fields surrounding organisms and objects which interact to exchange information. Spirit-soul theories are common to almost all major religious systems and have often been elaborated in great detail, but not in such a way as to lend themselves to predictions. A few psi field theories have been formulated in crude detail (e.g., Wasserman, 1966), and Roll's (1964) has been specifically aimed towards generating testable hypotheses. Advances in this conceptual area should lead to advances in both science and religion.

Superordinate source communication implies some sort of major organizing principle or entity more inclusive or more complex than the communication system designated by the experimenters plus the experimenters themselves. Such hypothesized superordinate sources may well therefore be impossible to assess, at least for the time being, by the relatively simple system of the experimenter and his research tools, especially if the superordinate source possesses intelligence. Conceptual advancement in developing theories about such sources would probably represent advances in religious thought; collecting empirical evidence bearing on any such theories would probably lead to advances in religion, science, and the interface between the two.

Information slosh communication and Message ubiquity are perhaps the hardest of all to describe specifically and may call for ways of collecting data about our universe that are considerably outside our present conceptual schemes. Perhaps we must find ways to pursue suggestions such as Tart's (1972) that we develop "state-specific sciences" such that we make systematic use of the seemingly enhanced (or at least modified) perceptions of reality that often accompany strong imagery periods, drug intake, meditation procedures, and so on.

As can be seen, these broad classes of potential theories vary considerably in their present amenability to investigation, conceptualization, and so on. Yet they can all be at least crudely formulated, such as to lead hopefully to improvements in conceptualization. Some of them bear strong resemblance to concepts from within religion; the problems of expressing them concisely may well contribute to our understanding of the problems we face when we attempt to relate science to religious experience.

As promised in the title, I will go from one area of hopeless naivety into another, and try to relate the above to what I call experimental theology. There are many important aspects to religion, most of which I don't pretend to have any real feeling for yet at all; two, however, may be of particular importance to discuss within the present context. One is that a large part of religious experience and religious writing has to do with the attribution of external intelligent causality for forceful and unexplainable events and meaningful coincidences in the environment. Storms, lightning, seasons, and things like that were formerly inexplicable and powerful events which could influence people's lives a great deal. Many belief systems arose which essentially said, "All right, it seems to happen, it happens a lot, it seems to help or hurt our crops, it therefore has meaning, and so it may well be the result of an external and all-powerful cause"--or, what I would say now, a Superordinate source. And they would impute very often a certain amount of intelligence and directedness to the superordinate source. This of course is true in a lot of our present religious systems. Coincidences in general are sometimes readily attributed to a particular external organizing principle or super-source.

A second important aspect is that a great many religious experiences and teachings basically involve the acceptance of the validity of some form of expanded awareness experience, either experienced directly by oneself or one that is described by others, and which has served as the basis for a set of writings or teachings that one accepts at some emotional level even though one may not have had the same depth of experience as the original writer of the passage. These expanded awareness experiences can include some of the ones described in the meditation literature: experiencing a godhead, being at one with the universe, ecstasy, an all-pervading feeling of love, a revelation experience of some sort which is oftentimes a combination of apparent

psi, a lot of confidence, and a feeling in many cases of ecstasy. There can of course also be negative experiences: bad trips, apparent demonic images, and so on.

Can we interface these and other aspects of religion in some meaningful way with the reconstruances earlier developed? If we go down the list of the nine groupings of psi explanations, we come closer and closer in many respects to religion-related concepts, which still seem to make a little bit of sense even when something as pedestrian as a source-receiver-message model is imposed upon them. Number seven, Superordinate source communication, could have as one of its hypotheses the existence of some form of godhead or external, very extensive intelligence. If in fact there is a superordinate coordinator serving as a common source, one then essentially is saying that everybody else is a potential receiver. One can then look for shared characteristics during religious experiences, since in fact they may all be functioning as receivers. Such a common source may also, of course, serve as a receiver--e.g., during the act of prayer and/or as a scanner and purveyor of general earthly events.

Also, it is possible that in some situations we may have a sort of hierarchy. For instance, in healing studies we generally construe the whole process of healing either as something psychosomatic coupled with suggestion, or we say that the healer was the source of some kind of psi information imparted to the organism being healed which in itself facilitated the healing process. One may also apply the notion of an external super-source to healing paradigms. Under such models, the healer still serves as the immediate source of information or influence for the person being healed; however, that healer becomes in turn the receiver of information or some kind of specific influence from yet another source which is the external godhead or some comparable additional source. Such a model would be suggested by the statements of many healers that they are merely the vehicles for the transmission of spiritual energy from a godhead to the person needing to be healed.

Certainly it is easy to construe many kinds of religious experiences as very much related to psi-conducive expanded-awareness experiences. An individual uses drugs or meditation to induce an experience of either expanding beyond his physiological boundaries or at least of becoming aware of his extension beyond such boundaries. The experi-

ence then provides the person with access to a great deal of additional information, and becomes interpretable as a religious experience. That ties in with the set of models that were numbered five and six on the above list.

The source for a miracle involving any physical effect (e.g., water becoming wine) could be either human or an external source. The receiver in the system would be in general that part of the environment which was affected physically. In psychic revelation the seer or prophet becomes a possible receiver which would draw from a wide variety of possible sources of information for the message constituting the "revelation." Alternatively, we can consider the prophet's statements as analogous to those of the designated psychic in the chair test, thus opening up additional potential routes of information flow.

Religion or theology in general is, it seems to me, basically receiver-oriented in that we're all regarded as receivers of certain kinds of information; i.e., as constantly influenced by some general source of "higher intelligence," and so on. One of the main kinds of examples of a human as source, of course, would be the notion of prayer, in which a human attempts to impart information to an external coordinator, in this case construed as a super-source as well as super-receiver.

I must apologize for imposing such pedestrian models on questions that are of such a great deal of importance. I do so mainly in the hope that they may contribute to the interfacing of a range of studies, from religious studies on through sociology, anthropology, psychology, biology, physics, mathematics, and so on. Although I don't advise that everyone immediately adopt these terms and this way of looking at psi phenomena, I think it may potentially serve a unifying function that will allow people of different disciplines who would themselves personally prefer other terms and other concepts drawn from their own disciplines to communicate better with each other. I would suggest in general that we can at least try applying these kinds of models, look for analogs within the religious literature, and apply parallel research techniques. In this way we may come closer to having what might truly be called some day an experimental theology.

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BIOFEEDBACK AND PATTERN SELF-REGULATION IN BIOLOGICAL PERSPECTIVE: A Critical Analysis of Extra Sensitive Perception*

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Introduction

Although we do not usually think about it, we are constantly self-regulating complex patterns of neural and visceral responses in our dynamic interchange with our environment. It is curious that despite the fact that man is uniquely equipped, both biologically and culturally, to recognize this simple truth, he has not only frequently ignored this fact, but in the past has at times gone to great pains to deny or disguise it (e.g., Skinner, 1971). How often do we consciously ponder the multiplicity of biological processes we must coordinate and orchestrate in order to perform everyday acts such as creating and writing a sentence? Not very often, because it is characteristic of man's nature to take for granted those things he can readily do. Also, we usually direct our attention to the immediate task or goal at hand, rather than actively reflecting upon the interacting set of processes we are in fact self-generating in order to produce the complex behavior in question. However, if a skill is unique or unexpected, such as feats of bodily and cognitive self-regulation claimed for certain yogis or meditators, our fascination about the nature of self-regulation is rekindled.

This is the case for biofeedback. By providing humans or lower animals with information or feedback for internal responses such as heart rate, blood pressure, and EEG, and incentives or rewards for changing or controlling the feedback, subjects can learn to regulate the specific re-

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sponse(s) in question. Biofeedback studies have raised the question of voluntary control of responses previously considered to be involuntary, and have stimulated interest in enhancing human potentialities as well as determining the limits of self-control (e.g., Miller et al., 1974). However, despite the plethora of research in this area, it is curious that little serious attention has been paid thus far toward explaining how self-regulation develops, or what the possible psychobiological mechanisms and constraints are. Equally interesting is the fact that most of the research deals only with the self-regulation of single responses, or response systems. It does not address the more complex question concerning the nature of self-regulation and coordination of multi-physiological processes.

In this address I will discuss the issue of learned specificity versus patterning of responses and the relevance of pattern self-regulation toward understanding the psychobiology of human behavior and consciousness. Drawing from research conducted by my colleagues and students over the past six years, including new, yet unpublished data, I will consider the implications of pattern biofeedback procedures for the control of combinations of responses in relation to the broader question concerning the underlying bio-cognitive mechanisms involved in normal multi-response self-regulation. As we will see, pattern biofeedback procedures can provide a unique and powerful research tool for investigating the interrelationships among physiological systems in the intact human, and assessing the role of physiological patterns in subjective experience (Schwartz, 1972; 1973; 1974; 1975).

This address will not emphasize clinical applications per se (see Schwartz, 1973), but rather basic psychobiological processes involved in pattern self-regulation and its potential implications for parapsychology. Unfortunately, the biofeedback field suffers many of the same kinds of problems traditionally facing parapsychology. There are fantastic claims, a lunatic fringe, and an element of magic. As a result, some of the more traditional scientists either withdraw from or reject the field. The approach I have found useful for biofeedback is to place it back within a more integrative framework by emphasizing a psychobiological perspective. What I would like to suggest here is that a similar strategy may be usefully applied to what has traditionally been called "ESP."

Although I am not a parapsychologist, I do have an interest in the uncanny capability of the human brain to both

process and self-generate subtle and unusual forms of information, as will be described below. In this sense I am an agnostic about what I prefer to think of as "extrasensitive" perception. After discussing biofeedback, meditation, and other bio-cognitive self-regulation procedures, I will offer some speculations concerning a conceptual as well as research strategy applied to one particular area of parapsychology. This perspective grows out of our research on the self-regulation of patterns of physiological responses.

Specificity and the Brain

The capability of the human brain to self-regulate different and dynamic patterns of neural and visceral processes grows out of its extraordinary capability for response specificity. Common sense tells us that it is adaptive for animals, including man, to have developed a nervous system capable of changing itself in such a way as to require the least effort for producing a given consequence in its environment. The brain is a highly efficient organ in this respect, for it is quite extraordinary how the brain typically recruits and coordinates only those sensory, autonomic, and motor processes needed to perform a given task or skill. There are many examples of this phenomenon in the motor skills and motor physiology literature, and recently in biofeedback. For example, Basmajian (1972) has gone so far as to teach humans to control individual motor units in the finger. The fascinating thing about this research is that initially when subjects learn to control a motor unit, they show corresponding activity in adjacent motor units. However, with training the adjacent motor unit activity diminishes, and only the specific unit controlling the feedback and reward is self-regulated.

The motor system is a good analog for studying the self-regulation of autonomic and electrocortical responses, because it clearly demonstrates the general principle of learning progressing from partial integration to greater specificity with training. Recently, psychophysiologists such as Lang (1974) and Brener (1974) have applied a motor skills model to biofeedback learning. In our laboratory, we have also considered this perspective. For example, Schwartz, Vogler, and Young (1975) have investigated two different cardiac self-regulation skills. Typically, the skill studied in biofeedback research has been a "strength and endurance" task, e.g., "increase your cardiac rate and hold it for a

minute," or "decrease your heart rate and hold it for a minute." Yet to be investigated are other varieties of self-regulation skills such as reaction time and precision control (Fleishman, 1966). Based on the motor skills literature, we posited that practicing cardiac strength and endurance would result in the learning of a different skill from practicing, for example, cardiac reaction time.

Twenty subjects were pretested in their ability to both raise and lower their heart rate without feedback (but with full instructions) for one-minute trials (the strength and endurance paradigm). In addition, subjects were pretested for their ability to perform a new cardiac reaction time task without feedback; subjects were instructed that whenever the trial light came on, they were to try to moderately raise their heart rate (or lower their heart rate) as quickly as possible. Three seconds of criterion cardiac control turned the trial lights off, and response time was computed. Following the pretest, half the subjects were given practice on strength and endurance training, with a meter display of beat-by-beat changes in heart rate as biofeedback. These subjects practiced maximally raising and lowering their heart rates during the one-minute trials. The remaining subjects were given practice on the reaction time task. These subjects practiced raising (and lowering) their heart rate to 50 percent of strength values as quickly as possible, using the same meter biofeedback display. Finally, all subjects were posttested without feedback on strength and endurance and on reaction time.

Interestingly, the results indicated that the subjects who had practiced strength and endurance showed small increases in strength and endurance control above pretest levels, but no improvement in cardiac reaction time. Conversely, the subjects who were trained on cardiac reaction time showed a 50 percent improvement in cardiac reaction time, but no improvement in strength and endurance. In other words, subjects learned two different cardiac skills, specific to the task they had practiced. This finding of specificity of skill learning within a single autonomic response underscores the power of biofeedback procedures (above and beyond instructions per se) in tapping specific capabilities for learned self-regulation normally not exercised by human beings. In the process, the biofeedback procedure provides a model system for studying the similarities and differences between motor and visceral self-regulatory processes.

However, this leads us further away from the issue of self-regulated patterns of physiological activity, because single response biofeedback typically results in fine learned specificity. As will be described below, the specificity issue became particularly focused when David Shapiro, Bernard Tursky and I began research on the self-regulation of blood pressure and heart rate.

Self-Regulation of Systolic Blood Pressure vs. Heart Rate

In our initial studies (Shapiro, Tursky, Gershon, & Stern, 1969; Shapiro, Tursky, & Schwartz, 1970a; 1970b) subjects were given simple binary (light-tone) feedback for relative increases or decreases in systolic blood pressure or heart rate. After subjects produced a designated number of feedbacks, they were given an incentive or reward in the form of slides. Whereas the early studies used only pictures from Playboy magazine as rewards, later studies switched to a potpourri of rewards, including pictures of Boston and foreign countries, and monetary bonuses. It is important to point out that these subjects were given minimal instructions; they were not told what response the feedback displayed, nor the direction (increase or decrease) it reflected.

In the first two studies (Shapiro, Tursky, Gershon, & Stern, 1969; Shapiro, Tursky, & Schwartz, 1970a), subjects were given feedback for increases and decreases in systolic blood pressure while heart rate was simultaneously monitored. The results showed that after about 15 one-minute trials the two groups began to diverge, with the decrease blood pressure feedback group significantly lowering its blood pressure relative to the increase blood pressure feedback group. Interestingly, however, when heart rate was examined, no differences were obtained. In other words, subjects had learned to control their systolic blood pressure without simultaneously changing their heart rate, thereby demonstrating learned specificity within the cardiovascular system. Also interesting was that analysis of post-experimental questionnaires revealed no apparent differences in cognitive strategies between the two groups. In fact, most of the subjects believed they had little or no control over the feedback.

After replicating these unexpected findings (Shapiro, Tursky, & Schwartz, 1970a), we turned the paradigm around:

subjects were given binary feedback and reward for increases or decreases in heart rate while systolic blood pressure was simultaneously recorded (Shapiro, Tursky, & Schwartz, 1970b). The results showed that subjects now learned relative control of their heart rate, without corresponding changes in systolic blood pressure. Again, analysis of subjective reports revealed no apparent differences between groups.

Self-Regulation of Patterns of Systolic Blood Pressure and Heart Rate

In pondering the explanation for this degree of learned specificity, it occurred to us that the data might be uncovering something about the natural relationship between these two responses (Schwartz, Shapiro, & Tursky, 1971; Schwartz, 1972). For example, if it were the case that blood pressure and heart rate were always naturally changing in the same direction (integration) so that whenever blood pressure increased heart rate increased and vice versa, then if feedback and reward were given for blood pressure, heart rate would also be receiving the identical feedback. Therefore, we would predict that both responses should be learned and in the same direction. Conversely, if blood pressure and heart rate were always changing in opposite directions (differentiation) so that whenever one increased the other simultaneously decreased and vice versa, then if feedback and reward were given for one, the other would be receiving the opposite contingency. Again, in this case both responses would be learned, but in opposite directions. However, the empirical findings produced neither of these results; rather, specificity was obtained. Therefore, this would imply that systolic blood pressure and heart rate are unrelated such that feedback for one results in some form of simultaneous random reinforcement of the other.

If we assume for the moment that this conclusion is justified, the question that arises is: how would we teach a person to control both of them? One approach that follows from the above pattern analysis is to give feedback and reward only when the desired pattern occurs. This requires a method for measuring phasic patterns of physiological responses in real time. Toward this end we developed a procedure for assessing the four basic patterns of systolic blood pressure (BP) and heart rate (HR) at each beat of the heart: $BP^{up}HR^{up}$, $BP_{down}HR_{down}$, $BP_{down}HR^{up}$, and $BP^{up}HR_{down}$

(Schwartz, Shapiro, & Tursky, 1971; Schwartz, 1972).

Using this pattern procedure, a biofeedback experiment was performed in which subjects were given feedback and reward for one of the four BP-HR patterns (Schwartz, 1972). The results were both interesting and surprising. Concerning the integration conditions, subjects now learned to control both their blood pressure and their heart rate simultaneously. However, in comparison to the prior findings of specificity, integration learning occurred more quickly (within the first five trials) and the magnitude of control was somewhat larger. In other words, controlling both responses simultaneously in the same direction led to greater learned self-regulation than training in either single system alone. Conversely, the results for learned differentiation, although statistically significant, resulted in poorer separation of blood pressure and heart rate than training in either one alone. To summarize, the results suggest that by training subjects to self-regulate patterns of responses, it is possible to uncover natural constraints operating between responses not noticed when training in single systems alone. As we will see, this accidental discovery proves to have major theoretical as well as practical importance (Schwartz, 1975).

An additional, unexpected finding supporting the pattern concept concerns post-experimental reports. Unlike the previous single system results, it turns out that when subjects are trained to lower both their blood pressure and their heart rate simultaneously ($\overline{BP_{down}HR_{down}}$), they now spontaneously begin to report feelings of calmness, relaxation, and other cognitions one would expect to be associated with more general multi-response relaxation. Given that these subjects were minimally instructed, this finding is even more striking. Altogether, the results suggest that in the process of trying to understand and extend biofeedback techniques to patterns of responses, the research uncovers more about the nature of the physiological systems themselves and their relationship to subjective experience.

Space does not permit a detailed presentation on more recent research involving the self-regulation of patterns of diastolic blood pressure and heart rate (see Schwartz, 1974). However, two additional points should be briefly noted. One is that just because two responses tend to naturally co-vary, this does not necessarily require that they be causally linked. The only way to test whether two responses are causally re-

lated is to try to pull them apart, for example using pattern biofeedback training. The second point is that to really assess the strength of a biological constraint, multi-sessions of training are needed. Unfortunately, with few exceptions (e.g., see below), such studies have yet to be carried out.

Coordination Training and the Self-Regulation of Patterns of EEG and Autonomic Responses

If pattern biofeedback training can be effectively used to study relationships within the autonomic nervous system, can we not hypothesize that the pattern approach may have more general use in investigating natural integrations and constraints across sensory, autonomic, and motor systems? In the past couple of years my laboratory has become interested in using the pattern biofeedback approach as a means of examining the role of cortical processes in the self-regulation of autonomic activity. For example, what role do cortical processes play in controlling one's heart rate? In designing this research, it became clear that if a single person could be taught to control all the patterns within himself, this would eliminate cross-subject variability, and would give a measure of stability within an individual over time.

Consider the following situation. How does a person go about learning to rub his stomach with one hand and pat his head with the other? Self-reflection will reveal that this is a difficult task; one way to learn this skill is to practice each response alone and then coordinate the two by practicing them together. The same procedure can be applied to the training of EEG and autonomic patterns. One advantage of this kind of procedure is that simple single channel biofeedback devices can be used for pattern training. (Although we currently use a PDP-11 computer system for analyzing complex patterns of multi-physiological responses, such equipment is not necessary for basic coordination training.) Another advantage is that coordination training implies the concept of "play"; what the subject does is learn how to control a given system, at his own pace, and then he practices putting them together. If he tires of raising one and simultaneously lowering another, he can practice controlling a different pattern, or one at a time, or just rest. Interspersed between such "free play" periods are specific test trials. We find that the typical college student experiences this learning situation as interesting, challenging and highly motivating.

Using this training procedure, Hassett and Schwartz (1975) taught subjects in one session to control their occipital alpha over the right hemisphere with full instructions. After EEG alpha training, all subjects were trained in heart rate control with full instructions. Then, in a second session, subjects were given dual feedback free play periods and tested for all eight patterns. The results indicate that the typical subject can in two sessions, within certain limits, learn to produce all eight possible EEG-heart rate combinations on command. He can raise and lower his occipital alpha with little effect on his heart rate; he can raise and lower his heart rate without affecting his occipital alpha; he can raise the two or lower the two, or make them go in opposite directions on command, for one-minute trials.

However, there are interesting occipital EEG-heart rate constraints that emerge, particularly when pattern control is required. Simply stated, occipital EEG control has a unidirection constraint on heart rate. For example, subjects produce larger increases in heart rate when simultaneously blocking their occipital alpha ($Alpha^{off}HR^{up}$) than when increasing heart rate alone (HR^{up}). However, alpha suppression is greater with alpha control alone ($Alpha^{off}$) than with $Alpha^{off}HR^{up}$ regulation. In other words, within certain limits it appears that the heart is sensitive to the needs of the visual cortex. However, the heart is a "slave to many masters," and therefore it is not surprising that whereas the occipital cortex can drive the heart, the heart can change without necessarily involving the occipital cortex.

Three subjects have been trained for eight days, and this unidirectional constraint is consistently found, especially during pattern self-regulation test trials. We have also post-tested two of these subjects seven months later, and they retain the same extent of learned EEG-heart rate control. Analysis of subjective reports provides further support for the pattern notion. Subjects experience the $Alpha_{on}HR_{down}$ pattern as most relaxing, the $Alpha^{off}HR^{up}$ pattern as most arousing, and the two differentiation conditions ($Alpha^{off}$-HR_{down} and $Alpha_{on}HR^{up}$) as falling in between. Such data illustrate the potential of applying biofeedback pattern procedures to the study of physiological patterning and subjective experiences.

Cognitive Control of Patterns of Physiological Responses

Given that subjects can learn to self-regulate patterns of physiological responses, the next question that arises is: how do they do it? What kinds of cognitive processes do subjects self-regulate in order to produce specific patterns of responses? In an early review article, Katkin and Murray (1968) went so far as to conclude that in order to demonstrate "instrumental conditioning" of an autonomic response in humans, it was necessary for the subject to be paralyzed by curare (to remove motor components) and to be rendered unconscious (to eliminate cognitive mediators)! In reply to this article, Crider, Schwartz and Shnidman (1969) pointed out that there were surprisingly few experimental data from which to argue that cognitive events could influence discrete physiological responses in the first place.

However, over the past five years significant progress has been made concerning the cognitive control of both single systems and patterns of responses. For example, in 1971, I developed a new time-locked procedure which I thought might even convince B. F. Skinner that thoughts could elicit discrete autonomic responses (Schwartz, 1971). Essentially, the procedure had the subjects self-generate four affective words capable (when spoken overtly) of producing an emotional response. The words were: sex, rape, a monosyllabic slang word for intercourse, and death. Subjects self-generated these words covertly in synchrony with an externally-paced tone. Subjects also self-generated the letters A, B, C, and D, and the numbers 1, 2, 3, and 4. The hypothesis was that by time-locking the different thought categories, it would be possible to uncover differential cardiac responses to purely internally-generated stimuli. The results clearly demonstrated small but significant cardiac accelerations that accompanied the self-generated affective words.

Excited by these findings, I went running to make an appointment with Skinner, naively thinking that these results might change his basic views. Skinner's response upon hearing these data was essentially as follows: "Excellent! So you have empirically demonstrated that cognitive events can elicit autonomic responses. But I've known that all along! I can hook myself up to a polygraph, think emotional thoughts and my heart rate will increase also. I am just not interested in that problem. However, if you are interested, go out and study it using your paced procedure as well as biofeedback procedures. I'm just going to continue doing my

own thing, and you should do yours." It might be noted that he has never mentioned these data again, because he really does not care about them.

However, we continued research on this problem. For example, Schwartz and Higgins (1971) raised the question: if a thought can have stimulus properties, can it also act as a response? In the experiment, a subject was required at the offset of a five-second warning stimulus to push a button either deliberately fast or deliberately slow. Anticipatory cardiac responses were measured preceding the two different button-pushes. However, on half the trials the subject was instructed to do a similar task purely "in his head"; that is, the subject was required to think the word "stop" either deliberately fast or slow, comparable in speed to the overt motor response. The results showed that during the overt motor-push trials, anticipatory cardiac decelerations occurred just prior to the time that the subject actually pushed the button. In fact, the difference between fast and slow in the lag of the heart rate correlated quite highly with the actual difference in the overt motor button-pushes. Interestingly, the results for the covert thought trials produced smaller, but similar results. This was interpreted by Skinner as supporting his contention that thoughts are just very small overt responses.

In light of the above data, the question that arises next is whether cognitive (mental) events can elicit specific patterns of physiological activity. And if so, are these the same mechanisms underlying biofeedback control of specific patterns of responses? As it turns out, recent data concerning both the self-generation of patterns of motor responses and electrocortical responses provide compelling support for the notion of cognitive control of patterns of responses.

Imagery, Emotion, and Self-Generated Patterns of Facial Muscle Activity

Just over 100 years ago, Darwin published his classic book about patterns of facial expression and emotion (1872). Darwin observed that different facial expressions are associated with discrete innate emotions that are consistent within different species. Recently, Ekman (1973) and Izard (1971) have replicated and extended these findings, taking a more neuropsychological approach. Izard theorizes that patterns

of facial and postural muscle activity are themselves parallel-processed by the brain and combine to make up an important component of the conscious experience of emotion.

Based on these observations, we have raised the following questions: what happens when a person thinks happy thoughts and begins to feel happy? Is it possible that, although not readily apparent to the casual observer or the person himself, in the process of generating happy imagery a subtle smile is elicited (a "covert" facial expression of happiness)? And if so, by measuring specific patterns of facial muscle activity, is it possible to differentiate between different emotional thoughts and moods? In a series of studies, we have found that by recording EMG from four carefully-selected muscles on the face, it is possible to differentiate when subjects are specifically thinking happy versus sad versus angry thoughts and feelings (Schwartz, Fair, Greenberg, Foran, & Klerman, 1975; Schwartz, Fair, Greenberg, Friedman, & Klerman, 1974; Schwartz, Fair, Greenberg, Mandel, & Klerman, 1974; 1975). Interestingly, if normal subjects are asked to simply think about a "typical day," the facial EMG pattern elicited is one of happiness. However, when depressed subjects are asked to think about a typical day, the resulting facial EMG pattern is one of sadness.

There are three important implications of these data for the present discussion. One is that specific self-induced cognitive states can elicit discrete bodily patterns. In fact, these heretofore unnoticed somatic patterns may serve as a major physiological mechanism allowing imagery to elicit the subjective feelings associated with different emotions. The results with the depressed subjects bring home another point; a person's ability to self-regulate different patterns of physiological responses will depend in part on his emotional state at the time.

Emotion and Patterns of Hemispheric Asymmetry

Another example that illustrates physiological patterning and self-generated emotion emerges from our recent research on hemispheric asymmetry. Previous research indicates that the left hemisphere (in the normal right-handed person) is involved with processes like verbal thinking, whereas the right hemisphere is involved with spatial, musical, and affective processes. If talking is left, and whistling is right, then what is singing? We predicted that sing-

ing is a coordinated skill or pattern involving both hemispheres. Schwartz, Davidson, Maer, and Bromfield (1974) recorded patterns of EEG activity across the two hemispheres during whistling, talking, and singing. As predicted, the EEG revealed that whereas talking produces relative left hemisphere activation and whistling produces relative right hemisphere activation, singing results in a relative equalization of the two hemispheres.

Consider another example illustrating the pattern perspective. If a person is asked a simple verbal question such as "What is the primary difference in the meanings of the words 'logic' and 'thought'?," the typical right-handed subject will tend to look away to the right, suggesting that the left hemisphere is involved. Conversely, if a person is asked a simple spatial question, such as "Picture a rectangle in your head; draw a line from the upper right-hand corner to the lower left-hand corner. What two figures do you now see?," the typical subject will tend to look to the left while answering the question, suggesting that the right hemisphere is involved (Kinsbourne, 1972). We raised the following question: what would happen if, on top of this, emotional content were added to the task? It was predicted that this would add an additional right hemispheric component to all the questions. We therefore created both verbal-emotional and spatial-emotional questions (Schwartz, Davidson, Maer, & Bromfield, 1974). An example of a verbal-emotional question is "What is the primary difference in the meanings of the words 'anger' and 'hate'?" A spatial-emotional question is "Imagine that you are crossing the street from north to south and a car coming from the east hits you. Which leg will be broken first?" The results of this experiment completely support the concept of hemispheric patterning in complex cognitive (affective) states. Besides demonstrating main effects of right hemispheric involvement for spatial questions and affective questions on the average, analysis of the separate questions reveals the greatest left hemisphere involvement for verbal-non-emotional questions, and the greatest right hemisphere involvement for the spatial-emotional questions.

The importance of data such as the above is that they lead us to break complex cognitive states into their underlying sub-components, and then put them back together again to determine how they combine to produce unique physiological patterns and subjective Gestalts. However, the question still remains, can we generalize from these data and argue

that biofeedback control of the cardiovascular, electrodermal, and motor systems involves similar cognitive referents? Or, are only certain kinds of responses or patterns associated with specific cognitive states? I would suggest that in fact persons typically do not normally use specifiable cognitive referents when they, for example, control their motor system. When asked, "How do you move your arm?," most people cannot tell you. However, it is reasonable to hypothesize that at least certain patterns of physiological responses are associated with discrete cognitive referents, and that in generating the specific response pattern this will be accompanied by the specific subjective state.

For example, is there a difference at the level of the brain between controlling one's heart rate by "tensing and relaxing one's muscles" and controlling one's heart rate by "thinking arousing thoughts"? Are these processes different, or are they the same? In a recent experiment working within the left hemisphere, we have separately recorded EEG from the occipital cortex and from that part of the sensorimotor cortex involved in the movement of the right hand (Neyers & Schwartz, 1975). Simply stated, we find that when subjects control their heart rate by tensing and relaxing their muscles, they produce corresponding changes in the EEG over the sensorimotor cortex but not over their occipital cortex! On the other hand, if subjects control their heart rate by thinking emotional thoughts, little change is observed over either the left sensorimotor or left occipital cortex. We predict that for affective imagery, EEG changes will be found over the right hemisphere. Such data provide strong support for the concept of response patterning in complex self-regulation strategies.

Meditation and Modes of Relaxation: A Pattern Analysis

The concept of pattern self-regulation has direct clinical implications for our understanding of meditation and other relaxation self-regulation therapies (Davidson & Schwartz, 1975). Wallace and Benson (1972) have described "a hypometabolic state" occurring in transcendental meditation (TM). This state is unusual in that it produces decreases in multi-physiological systems; blood pressure, heart rate, oxygen consumption, and EEG, to name a few. The pattern is also unusual in that whereas the EEG looks somewhat like stage 1 sleep (and it is claimed that the oxygen consumption decrease is even greater than that during

sleep), the meditator reports himself to be awake and alert. Benson has relabelled this pattern the "relaxation response," demonstrating that a very simple meditation procedure (attending to breathing and saying the word "one" after each breath--an English mantra) produces marked decreases in oxygen consumption (Beary & Benson, 1974).

Stimulated by such findings, many biofeedback researchers were tempted to formulate the following hypothesis: since low-frequency EEG occurs in passive meditation, and subjects can learn with biofeedback to self-regulate such EEG patterns, then biofeedback training for these EEG changes will lead to deep relaxation and an "instant electronic Yoga." The major fallacy in this logic is that single-system biofeedback training typically leads to specificity, not patterning. If a subject is given feedback for occipital alpha, what he learns to do is to control occipital alpha. He does not necessarily learn to control the pattern of changes actually occurring in these complex meditative states. As described earlier, learning to decrease both blood pressure and heart rate simultaneously ($BP_{down}HR_{down}$) results in greater subjective relaxation than lowering either one alone. Similarly, in our EEG alpha-heart rate pattern self-regulation study, $alpha_{on}HR_{down}$ is found to be very relaxing. In fact, one of our subjects who was in long-term training liked that particular pattern so much that she began practicing it on the subway back and forth to Harvard as a means of relaxing.

I would suggest that patterns in meditation are even more complicated than currently recognized, and this will have important implications for parapsychology (see below). In the recent volume Psychophysiology of Zen, Hirai (1974) provides data from Japanese subjects for the concept of "relaxed awareness." Similarly, in America, Goleman and Schwartz (1975) have found evidence to support the hypothesis that a major effect of transcendental meditation (TM) is the self-generation of a unique and somewhat paradoxical pattern of cortical and limbic arousal, roughly equivalent to the differential subjective experiences of perception versus emotion. More specifically, we have hypothesized that passive meditation practices can lead to heightened cortical arousability plus decreased limbic arousability at the same time, which expresses itself in human personality and subjective experience as heightened perceptual awareness and simultaneously reduced emotional arousal and stress. Were this, albeit oversimplified, conclusion generally correct, the following set of important issues would arise: is it possible using

biofeedback techniques to mimic this particular psychophysiological state? What combination of responses and biofeedback training procedures would be necessary to match the pattern of psychophysiological changes which occur naturally during such meditation techniques? And if it were possible, is it worth the effort?

My own response to these questions splits according to the needs of basic research versus clinical applications. At the level of basic research, the pattern biofeedback approach provides a new procedure for investigating how patterns of physiological systems combine to produce unique subjective Gestalts and behavioral correlates; at this level, the approach promises to be quite fruitful. However, as for clinical application, practical and naturalistic considerations lead me to the conclusion that if the psychophysiological state produced by passive meditation techniques is of therapeutic value (e.g., by reducing limbic stress and therefore its many expressions in different diseases; Selye, 1973), then it should be induced and practiced using the non-electronic, easily portable and generalizable machinery of our own biocognitive system.

When we consider the phenomenon of relaxation still more broadly, it becomes clear that different combinations of cognitive, attentional and somatic strategies can be brought into play, and that different relaxation procedures emphasize the self-regulation of different combinations of processes. Davidson and Schwartz (1975) have outlined how different relaxation paradigms utilize different combinations of strategies, which will be reflected in different patterns of physiological responses. In a similar manner, it is possible to classify different kinds of anxiety, involving patterns of cognitive, visceral and somatic components. We predict that the most appropriate and effective relaxation procedure may depend upon the pattern of anxiety the person is experiencing at the moment.

Consider a case of high cognitive-low somatic anxiety --in this state, for example, the person may be physically tired and cannot move, trying to fall asleep except that his mind is racing with disturbing images and thoughts. The age-old treatment for this anxiety state is to visualize sheep and count them--this cognitive self-regulation procedure, we have posited, is particularly effective because it blocks both unwanted visual (right hemisphere) and verbal (left hemisphere) images at the same time, thereby dually aiding the

person in falling asleep. Another type of extreme anxiety is the person who feels somatically tense and jittery, but nothing in particular (no specific images) is bothering him. For such cases of low cognitive-high somatic anxiety, effective "relaxation" strategies include jogging, gardening, or other self-generated, somatic activities that serve to block the undesired somatic state and use up some of the unwanted somatic metabolism at the same time, thereby producing fatigue. This pattern orientation to anxiety assessment and relaxation treatment is not unlike Lazarus's (1973) recent concept of multi-modality therapy. His approach is to classify for the individual the patterns of responses that need to be modified, and treat them either singularly or in combination, recognizing that the selected treatment of one component will not necessarily lead to reductions in others. Specific applications of the pattern approach to biofeedback therapy are described in Schwartz (1973; 1974; 1975).

Pattern Self-Regulation and Human Consciousness

Beginning with experiments on the self-regulation of patterns of cardiovascular responses, followed by research on the voluntary control of patterns of EEG and heart rate, and finally research involving self-regulated patterns of cognitive and affective processes and related clinical applications, we have seen both the value in and complexity of examining voluntary control of combinations of processes and their interactions. One major thesis that has slowly emerged from all this research is that patterns of physiological processes can be both self-generated by the brain and also processed by the brain, in this way producing unique cross-system interactions and perceptual Gestalts that make up a significant component of human subjective experience. This concept of pattern does not simply refer to our seeing combinations of physiological responses in isolation, but rather goes beyond the individual responses making up the pattern so as to recognize the novel, interactive or emergent property that patterns can acquire. Simply stated, the whole can be qualitatively different from the sum of its parts, yet be dependent upon the organization of its parts for its unique properties. This phenomenon is not restricted to psychophysiology; on the contrary, it is seen at all levels of physics and chemistry, and extends through biology and neuropsychology (Weiss, 1969). Sperry (1969) illustrates, for example, how at room temperature the liquid water has a unique set of properties that are clearly different qualitative-

ly from the individual properties of the two gases that comprise it; yet water is totally dependent upon the specific combination of two atoms of hydrogen and one atom of oxygen in order to maintain its particular property as a whole.

This concept of emergent property is what I mean by pattern. Current neuropsychologists concerned with the biology of consciousness employ a pattern/interaction/emergent property notion when they speak of cell assemblies (Hebb, 1974), neural engrams (John, 1972), holograms (Pribram, 1971) or dynamic neural patterns (Sperry, 1969). At a psychophysiological level, it is not surprising that emotion was long ago described by William James (1890) as the perception of specific patterns of autonomic consequences of action. More recent researchers such as Schachter and Singer (1962) have added the importance of perception and cognition to the autonomic arousal as an integral part of this interaction. Today, theorists like Izard (1971) stress the interaction of patterning of combinations of neurophysiological systems, including discrete patterns of postural and facial muscle activity, as the mechanism underlying the subjective experience of emotion. The concept of interacting neuropsychological systems in the makeup of human intentionality and consciousness is brilliantly illustrated in a recent volume by Luria (1973).

Research on biofeedback and the self-regulation of combinations of responses adds a new vista to this basic concept of pattern by providing a novel paradigm for systematically and parametrically investigating physiological relationships in the intact human. Self-regulation as a general research strategy is particularly useful because it enables researchers to isolate component parts of systems and then examine how they may combine to produce unique physiological and associated subjective states. Our laboratory has shown that the self-regulation of patterns of responses can have different consequences from those observed when controlling individual functions alone. As I have illustrated, this simple principle proves to have important basic as well as clinical ramifications. With the aid of self-regulation integration and differentiation techniques, it is possible to assess natural restraints and constraints between different physiological systems in the normal person. And by requiring uninformed subjects to self-regulate different patterns of physiological responses, it is now possible to determine empirically how separate physiological components can combine to produce unique changes in consciousness and

human performance. It is not inconceivable, however, that the very act of self-regulating a pattern of responses may have somewhat different consequences from those found when a similar pattern is elicited by other means. Should future research prove this to be the case, it will limit the general applicability of the approach. On the other hand, such a finding could well provide a key to further uncovering the unique nature of the self-regulation process itself.

Applications to Extra Sensitive Perception: A Psychobiological Pattern Perspective

What relevance does the above perspective have for theory and research in parapsychology? The analysis to be described was triggered in part by a phone call I received from Time magazine last year. Time asked me whether I would be willing to talk about "ESP," and I said "NO." However, after the phone conversation, the question that bothered me was, "Why not?" In considering my motives, I decided that the issue did not involve the data per se. Rather, my opposition was that I hated the term "Extra Sensory Perception." I hated the concept because it was so antagonistic to my conception of psychobiology and self-regulation. It occurred to me that a major stumbling block for parapsychology (which has similarly occurred in the history of science, applying to such notables as Darwin, Freud, and Skinner) is that a specific term (e.g., ESP) evokes certain negative images which are completely counterproductive to scientists' accepting it.

I therefore decided that it might be useful to attempt to place parapsychology, like biofeedback, in a more psychobiological framework. If I could take just one such area in parapsychology and place it on a firmer psychobiological ground, I might then be able to convince myself that there was something worth considering, and in the process I might also be able to convince some of my colleagues as well. The area I chose was "ESP." Let us make the not unreasonable assumption that all information (both normal and paranormal) that influences behavior is processed by the nervous system in one way or another. Let us for the moment, therefore, not use the term "extrasensory" (since this is not only, I would suggest, a little narrow, but it is also antibiological), but rather label the phenomenon as extrasensitive perception (or even describe it as extrasensitive cortical perception, to give it a more specific physiological base).

Once stated in this way, the following questions emerge: (1) Does the brain typically respond to very low levels of normal sensory stimulation (beyond levels of normal consciousness) that elicit differentiation or patterns of neural activity in the brain? (2) Are there specific brain states in certain individuals and/or conditions (e.g., during relaxation) that accentuate the brain's capability to register and process very low levels of normal sensory stimulation? If questions (1) and (2) can be answered in the affirmative, question (3) becomes particularly relevant: (3) Can the brain respond to low levels of stimulation provided by other energy sources (e.g., microwave) that bypass the traditional five sensory effector systems? What I would like to suggest is that based on current data in psychophysiology and psychobiology it is possible to answer each of these questions in the affirmative.

First, let us consider the "normal" sensory systems and low levels of sensory input. What immediately comes to mind is research on "subliminal perception," the "Drink Coke" phenomena. However, the problem with this research is that the term subliminal, like ESP, is neither theoretically nor empirically compelling, and it is not psychobiologically based. It has all the unwanted overtones of questionable science, and when we combine subliminal perception with ESP, we only further compound the initial problem. On the other hand, there are recent physiological data using EEG measures that provide a compelling psychobiological model for the extrasensitive cortical perception of normal sensory inputs.

Consider auditory stimuli. We are constantly bombarded by low-frequency sound below the level of consciousness. Nonetheless, not only is it processed by the brain, but if it is sufficiently intense, it can cause headaches and influence our cognitive and affective processes. In point of fact, the body is built in such a way that all kinds of physiological inputs influence our behavior; while we cannot sense them directly, we can nonetheless perceive consequences. Consider breathing. If asked to hold our breath, we can voluntarily inhibit our breathing. Although we can try very hard to keep holding our breath, after a while our brain starts to "fight back," and finally it forces us to take another breath. (This, by the way, is a very adaptive mechanism.) However, we do not directly experience via our chemoreceptors that carbon dioxide is building up in our blood. What we experience instead are the consequences on our body; for example, the opposing constraints on the chest as we try to compete against the normal mechanism initiat-

ing the taking of another breath. This illustrates what should be obvious, that a model in biology for normal unconscious cortical perception is already available.

A particularly relevant example concerns research on visual stimuli. Is there any evidence for differentiated EEG responses to unconscious or sub-threshold visual stimuli? The answer is beautifully provided by Shevrin, a clever psychoanalytically-oriented psychophysiologist (Shevrin & Fritzler, 1968). Shevrin was interested in assessing whether the brain was capable of registering a light so dim that a subject could not detect it. Shevrin demonstrated that the EEG could in fact register such a stimulus not detected at a conscious level. More important, however, was the next question he posed; namely, whether the brain could register differences in two weak visual stimuli, one "meaningful" and the other not meaningful? The stimuli selected by Shevrin were one picture containing both a "pen" and a "knee," and a second picture of similar shapes but not recognizable as specific objects. In some conditions, Shevrin presented these stimuli at such brief exposures that subjects could not even detect that a light had flashed. The results indicated that the brains of his subjects accurately discriminated between the two stimuli as measured by the EEG, even though the subjects could not report that any stimuli had been presented.

Probably the most creative and daring aspect of this research was that Shevrin had the subjects "free associate" after each set of stimuli. He posited that clang associations to the meaningful stimulus might be detected. So he looked for responses such as "penny," or "money"; all varieties of associations that a psychoanalyst might find relevant. Most interestingly, Shevrin reported that when subjects were presented with the sub-threshold meaningful stimulus, they spontaneously reported relevant free associations. In addition, Shevrin found that subjects who had an abundance of resting alpha in their EEG tended to show greater discrimination of the EEG response to the meaningful stimulus, and also greater discrimination in their verbal reports. The parallel of these observations to similar claims of state-dependent alpha research in the area of parapsychology is intriguing.

Given that extrasensitive cortical perception can occur with normal visual stimuli, it is then possible to ask more specific questions about the mechanisms of this effect. For example, what specific states of the brain are more conducive for such information to be registered by the cortex? And

what individuals are more prone to register such information and have it accurately processed? Let me suggest that we first consider those sources of energy that can be readily quantified at a physical and biological level, and then consider parallels of these data to forms of energy yet to be readily quantified.

It is often claimed in parapsychology that subjects tend to be better at extrasensitive perception at certain times than at others. However, is there any evidence in current psychophysiology using normal visual stimuli that parallels this phenomenon? Let me suggest that the answer to this question may be "Yes." For example, recent data from our laboratory suggest that in certain forms of meditation (e.g., TM), a self-regulated state is produced resulting in heightened cortical arousability plus decreased autonomic/limbic arousability at the same time (Goleman & Schwartz, 1975). This implies that the brain has reduced its level of autonomic and somatic internal "noise" or "distraction," thereby opening up its sensory systems. I would predict that if low level visual stimulation were presented after TM, accentuated EEG responses reflecting extrasensitive perception would be found comparable to Shevrin's individual difference effects. The parallel of such findings to applications of meditation training in parapsychology are obvious.

What about individual differences in extrasensitive perception? It is claimed in parapsychology that certain individuals are better than others, but what are their brain characteristics? Are there any physiological data for normal modes of sensory stimulation that provide a model paradigm for such research? The answer again is "Yes." For example, research on EEG augmentation and reduction to overt stimuli illustrates that subjects reliably differ in their reactivity to different intensities of overt visual stimuli (Buchsbaum, 1974). We can then raise the related question, do subjects also differ in EEG response to the intensities of stimuli they cannot see? And are those the subjects who tend to be more responsive to parapsychological extrasensitive states? Such data have yet to be collected.

If we assume for the moment that this general orientation may yield data that could be of theoretical and practical value, we should move to question (3). Is there any evidence from a psychobiological perspective that forms of energy other than those activating the five senses can influence the brain in ways that we normally do not think about? Again,

the answer appears to be "Yes." There is a large literature on biological rhythms for which the mechanisms of energy influence are not clear. However, it is well known that cells are responsive to many forms of energy. For example, the body is composed of 70 to 80 percent water. It has been suggested that we may suffer from "biological tides" as the moon goes around the earth. If this were true, what consequence might the changing pull of the moon on our cellular structure have on our brain in terms of its capability to process low levels of other energy? This question has yet to be answered.

Another example relating to question (3) concerns electromagnetic fields that can influence the brain. Adey and his colleagues (Bawin, Gavalas-Medici, & Adey, 1973) have reported that when they pulse electromagnetic waves at a frequency comparable to a monkey's normal alpha rhythm, this energy can have the effect of driving the monkey's EEG at the alpha frequency, and this in turn influences the monkey's behavior. Of course, it must be recognized that Adey used fairly intense sources, but this is not the main issue. The point is that electromagnetic waves can, under certain conditions, influence the brain, and that these effects can and should be studied.

Adey's findings relate to parapsychological phenomena recently reported in Nature (Targ & Puthoff, 1974) claiming desynchronization of the EEG in one person when a second person in another room is having his EEG driven by photic stimulation. Not surprisingly, when scientists typically hear about such extrasensory perception, they throw up their hands and say, "Although I would like to accept the data, it still doesn't make any theoretical sense." Well, what would happen if we took a more psychobiological perspective instead and performed the following experiment: we take the device into the subject room and turn the intensity of the photic stimulator so low that the subject cannot even see the light flashing. The question then becomes, does the subject's brain react to this sub-threshold stimulation? And more importantly, are the subjects who do show an EEG response to this sub-threshold photic stimulation the same subjects who show EEG responses when the stimulator is placed in a second room using the more standard parapsychological paradigm?

Such data could stimulate researchers to ask whether there is something special about certain brains, and even

whether energy from one brain can influence another brain. Still, the question remains, what kind of energy? One model comes again from psychophysiology, this time from Cohen (1972), who has developed a sensitive and unique instrument for recording electromagnetic radiation from the brain without using electrodes attached to the skull. Apparently, the normal brain does generate electromagnetic activity; the intensity is very low, but it is there. However, the alert skeptic can reply: "O.K., maybe all this is true from a biological perspective. But how are you going to convince me that the brain is capable of discriminating between all the electromagnetic noise occurring in its environment? I still refuse to believe it."

I would suggest that research such as Shevrin and Fritzler's (1968) evoked potential studies, coupled with new research on biofeedback, amply illustrates the capability of the brain to discriminate among the "noise" of normal stimulation. Although the human brain has a remarkable capability for specificity and patterning, the typical psychologist and psychobiologist is reluctant to accept this. Yet why do we question this? For example, the recognition of a human face is far beyond what the most sophisticated man-made computer can do, yet there is something quite effortless and automatic about the normal brain's capability for processing and categorizing such information. In other words, maybe we need no longer ask the question, is the brain capable of such perception? Rather, we should ask: How is the brain capable of such perception? Under what conditions, for which persons, is the brain extra sensitive to various forms of energy, and what are the limits of the processing of such stimulation? From this perspective, I myself move from being a nonbeliever in "ESP" to become a more open-minded agnostic about Extrasensitive (cortical) Perception.

Summary and Concluding Remarks

This address began with biofeedback and specificity, and moved to the important issue concerning the self-regulation of patterns of responses. We then considered self-regulation in a broader perspective, bringing cognitive and emotional processes into the research, and considered the self-regulation of patterns of responses from a psychobiological perspective. And finally, we have turned to a special case of patterning from a psychobiological perspective, what I have called extrasensitive perception.

Despite the wealth and complexity of the data and theory presented here, it is hoped that two general conclusions will be recognized. First, pattern self-regulation viewed from a psychobiological perspective has the potential to stimulate basic as well as applied research, previously considered out of the realm of serious behavioral science and clinical practice (Schwartz, 1975). For example, we can begin to ask how patterns of physiological information are processed and self-regulated by the brain in such a way as to produce unique experiences of human consciousness.

The second conclusion, of more direct concern to researchers in parapsychology, is that it is now feasible to develop a psychobiological pattern approach to the study of paranormal phenomena. My own conclusion as a psychophysiologist is that if one were to take a psychobiological pattern perspective and conduct systematic psychophysiological research within the framework of extrasensitive, rather than extrasensory, neuro-perception, this could possibly have the effect of stimulating new psychobiological methods and findings concerning both the underlying mechanisms and limitations of so-called paranormal perception. In the process, such findings could provide the necessary heuristic and conceptual links that would bring more of the mainstream of science into the purview of parapsychology.

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

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

PK see PSYCHOKINESIS

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

PRECOGNITION ESP of a future event.

PSI Psychic ability in general, including ESP and PK.

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

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

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

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

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

RETROCOGNITION ESP of a past event.

RUN A group of consecutive trials.

SENSITIVE An individual who purportedly has strong psi ability.

SPONTANEOUS CASE An unplanned natural occurrence apparently involving psi.

SUBJECT The person whose psi ability is being tested.

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

TELEPATHY ESP of a mental event.

TOKEN OBJECT see PSYCHOMETRY

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

NAME INDEX

*Convention participants are identified by asterisks.

SUBJECT INDEX