

# RESEARCH IN PARAPSYCHOLOGY 1973

Abstracts and Papers from the  
Sixteenth Annual Convention of the  
Parapsychological Association, 1973

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Editors



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## PREFACE

Readers who have followed the development of parapsychology through the first eight volumes of the Proceedings of the Parapsychological Association, and in the new format of Research in Parapsychology, will have seen several trends in the field. They come out strongly in the present volume. In the years when parapsychology was preoccupied with the struggle to gain scientific recognition, there was a natural tendency to emphasize research topics which were meaningful to the critic and at the same time provided proof of the existence of psi (extrasensory perception and psychokinesis) phenomena.

In recent years there has been an increased acceptance of parapsychology and also a growing disillusionment with established science for failing to respond adequately to human needs. This has had a liberating effect on parapsychology. Research is becoming more open and independent and, not surprisingly, is actually providing more convincing demonstrations of the reality of psi than at earlier times. Striking examples of psi ability which were only known through anecdotal accounts a few years ago and given little credence are met increasingly often in the laboratory.

In the papers on psychokinesis (PK) we hear about gross movements of stationary objects the parallels of which have lain forgotten in the era of the (presumably fraudulent) physical mediums. In ESP work there is an increasing number of studies of specially gifted subjects who frequently produce such good results that the odds against their chance occurrence are astronomically high. Also ESP and PK tests with unselected subjects are reaching remarkable levels of significance thanks largely to advances in instrumentation which allow for rapid testing and assessment. The mechanization of the psi laboratory has been particularly spectacular and successful in animal research. For all this, the emphasis is not to provide proofs of psi phenomena but to explore their nature. Studies of altered states of consciousness are promising and are increasing in number and sophistication. The Invited

Address by Charles Tart and several of the research papers explore this area. At the same time, tests of the claims of mind development courses warn that there is yet no certain road to ESP success. Studies of the relationship to psi of personality traits, attitudes and response patterns are also represented and remain fruitful. The new research approaches are matched by conceptual innovations. The Presidential Address by Rex Stanford and several of the other papers call for the reappraisal of old models and the testing of new.

There are papers on the problem of survival of personality after death as approached in studies of reincarnation cases and deathbed experiences. In research on out-of-body experiences the question whether awareness can exist apart from the body (and may therefore continue after its death) is explored with the techniques of modern psychophysiology and physics laboratories. The field of parapsychology is still an underdeveloped science lacking both the manpower and resources adequate to uncover its potentials--which are the potentials of man himself. Research in Parapsychology 1973 shows the promise of the field and, hopefully, will stimulate scientists, students and others to contribute to the work.

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## THE SIXTEENTH ANNUAL CONVENTION

The Sixteenth Annual Convention of the Parapsychological Association was held at the University of Virginia in Charlottesville on September 6-8, 1973. The welcoming speech by Edgar F. Shannon, Jr., president of the University, is included as the first presentation of this volume. A total of 218 people attended the Convention. Of these 65 were Association members. Rex G. Stanford was chairman of the Program Committee, which also included Laura A. Dale and Douglas Dean. The Research Briefs chairman was Martin Johnson. Robert Van de Castle served as chairman of the Arrangements Committee.

Charles T. Tart of the University of California at Davis was the Invited Dinner Speaker. Rex G. Stanford of St. John's University, Jamaica, New York, delivered the Presidential Address. The two addresses are given in full as parts 12 and 13 at the end of the volume. Part 3 of this volume contains the research briefs which outline work in progress, research plans and other short presentations. These are followed by condensed versions of the full papers and by the symposia.





## WELCOMING REMARKS

Edgar F. Shannon, Jr.  
(President, University of Virginia)

It is indeed a pleasure to welcome the Sixteenth Annual Convention of the Parapsychological Association to the University of Virginia and for this institution to take a place among the distinguished universities where the Association has met. I am sure that your professional sessions will be profitable. I have looked over the program and wish that I could attend more of them, and my colleagues and I hope, too, that your stay here in Charlottesville will be a happy experience for each one of you.

Nearly a century has passed since a group of Cambridge University scholars founded in London in 1882 the Society for Psychological Research. Even though serious enquiry into the phenomena linked together under the heading of "Extrasensory Perception" has been continuous over the intervening years, I am merely repeating a truism when I remind you that the mainstream of the scientific community has persistently treated the parapsychologists either with open hostility or total indifference. It has ever been thus with explorers into the unknown.

Yet there are numerous and encouraging signs today of increasing interest in the pioneering work of your organization and its affiliates. The recent admission of the Association to membership in the American Association for the Advancement of Science is evidence of that fact. And in Science last month Nicholas Wade pointed out in an article, which you have all doubtless seen, that research in parapsychology is now being supported by the National Institute of Mental Health, and that courses in the subject are being offered in 75 educational institutions. Public awareness of the field was of course much enlarged when Captain Edgar D. Mitchell, an explorer of the physical unknown, performed an experiment into the mental unknown during his voyage in Apollo 14, and he has now established

his own institute for full-time study and publication in the field.

I am pleased that the University of Virginia is one of the major centers of parapsychology under the leadership of Dr. Ian Stevenson, and it seems especially fitting that your Association should meet at the University founded by Thomas Jefferson. His dream for the University was for it to be on the frontiers of knowledge, especially in science, which he thought was neglected in favor of theology in the colleges of the time. In 1820, our Founder wrote in his "100 percent American" mood to William Roscoe of Liverpool about his hopes for the University of Virginia. "This institution," he wrote, "will be based on the illimitable freedom of the human mind. For here we are not afraid to follow truth wherever it may lead, nor to tolerate any error so long as reason is left free to combat it." In that spirit I greet you on behalf of Mr. Jefferson's local successors. I wish you well in your deliberations, and I hope you will call freely upon all of us who do the day's work here for anything we can do to make your stay pleasant and to inspire you to future visits.

## RESEARCH BRIEFS\*

THE INFLUENCE OF RELAXATION AND TENSION  
ON THE PSI PROCESS

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

In a recent paper in the Journal of the American Society for Psychical Research, we presented preliminary data which suggested that progressive muscular relaxation facilitated the receptive psi process. Subjects in states of deep muscular and mental relaxation (induced by a modified Jacobson's progressive relaxation technique) scored dramatically high in seven GESP experiments using art prints as targets. In our next (as yet unpublished) experiment, we began to explore the role of relaxation in a more analytical manner. Subjects were tested while in a state of deep muscular and mental relaxation induced by tape-recorded instructions. Before the correspondences between targets and subjects' impressions of them were rated, all subjects indicated their degree of relaxation on a ten-point scale. It was found that highly relaxed subjects yielded significantly better correspondences than did less relaxed subjects. Thus, subjects listening to the same relaxation-inducing instructions actually relaxed in different degrees and their degree of relaxation was in turn correlated with degree of psi performance.

In this research brief, we report the results of a recently completed study with 20 subjects in which degree of physical relaxation was measured objectively by means of electromyographic (EMG) recordings from the frontalis

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\*Chairman: Martin Johnson, Lund University and University of Utrecht.

†Presented by W. G. Braud; in future dagger indicates speaker.

muscle group. Ten of the subjects listened to tape-recorded relaxation-inducing instructions lasting 20-25 minutes, while ten listened to tension-inducing instructions of the same length. Expectancy was equalized by telling both groups of subjects that their state would be psi-conducive. EMG recordings were taken throughout the session. Subjects also filled out a questionnaire in which they indicated their degree of physical and mental relaxation at the beginning of the experiment and during the psi impression period following the tension or relaxation instructions. Other clusters of items on the questionnaire tapped the subject's beliefs about ESP, his mood and attitude (both general and regarding specific aspects of the experiment), and certain "state" variables. The GESP procedure was similar to that used in our previous studies. Ninety-six selected art prints served as the target pool. During the experimental session an agent selected one of six prints by a random process and concentrated on it for five minutes, while the subjects tried to form impressions of its content. Following the session the subjects were shown the six prints, without being told which was the correct target. Rankings of 1 (best correspondence), 2, and 3 were scored as "hits"; rankings of 4, 5, and 6 (worst correspondence) were scored as "misses." Each subject did one trial.

The major results of the study are as follows:

(1) the tape-recorded instructions were effective in altering the subjects' physical and subjective states. Thus, the relaxation tape produced a significant decrement in EMG-defined muscular tension, in subjectively rated physical tension, and in subjectively rated mental tension; the tension tape produced a significant increment in EMG-defined muscular tension, in subjectively rated physical tension, and in subjectively rated mental tension. (2) Subjects listening to relaxation instructions performed significantly better on the psi task than did subjects listening to tension instructions. Relaxation subjects scored nine hits and one miss, while tension subjects scored six hits and four misses. (3) As we had predicted, the relaxation and tension groups did not differ in terms of other variables which might have important influences on the psi process, such as belief, mood, and attitude. Thus, the relaxation/tension effect was not confounded by differences in expectancy or other subjective variables between the two groups.

(4) There was a significant positive correlation between psi performance and (a) degree of EMG-defined

relaxation, subjectively rated physical relaxation, and subjectively rated mental relaxation during the psi impression period; (b) degree of shift toward more relaxation as defined by each of these three measures from beginning to end of the session. There was no significant correlation between initial EMG level, initial physical relaxation rating, or initial mental relaxation rating and psi performance. (5) EMG level, physical state rating, and mental state rating all intercorrelated significantly and positively whether measured in terms of their initial values, their impression period values, or their degree of shift during the session. This indicates that subjects are accurately aware of their tension or relaxation levels and that their subjective ratings correlate very well with objective bioelectrical measurements of degree of relaxation. The significantly high correlations between subjective rating and both psi and EMG levels indicate that subjective ratings may be usefully applied in future research as valid predictor and dependent variables.

In summary, different degrees of relaxation and tension may be produced by a brief tape recording and the degree of relaxation produced is positively and significantly correlated with good psi performance. This relationship between relaxation and psi occurs even when possible confounding psychological variables (such as mood, attitude, and expectancy) are measured and controlled for in the subjects tested. Thus, mental and physical relaxation appears to be an important psi-facilitating state. We continue to investigate this state in our ongoing research.

#### EXPERIMENTAL TESTS OF SILVA MIND CONTROL GRADUATES

Robert Brier (C. W. Post College, Brookville, N. Y.), Barry Savits (St. Mary's Hospital, Brooklyn), and Gertrude Schmeidler† (City College, City University of New York)

The Silva Mind Control organization advertises that it enables its graduates to develop "E. S. P." Many graduates who seem intelligent and sincere claim that they can diagnose ailments clairvoyantly, given some minimum information about an individual. We have performed two experiments to test these claims. B. S., a surgeon, selected

25 recent patients in the New York area, identified each by first name and initial of last name, age, and sex, and divided them into five groups so that there was minimal overlap of symptoms among the five members of a given group. R.B. selected five enthusiastic graduates of Mind Control, each certain that he or she could make accurate diagnoses on the basis of this minimal information, and sent each graduate the information on one group. After the 25 diagnostic readings were returned to R.B., G.S. typed them without identification of age, sex, or name and gave the typed readings to B.S. for scoring in a 25-by-25 array. B.S. rated each reading on a scale of 0 to 100 for how well it fit each of the 25 patients. G.S. evaluated the 625 scores by a two-way analysis of variance (partitioning the interaction sum of squares into one degree of freedom for the ESP hypothesis, and a residual), and found chance accuracy of diagnosis. G.S. then compiled 5-by-5 tables for each subject for the 25 scores of his five readings on each of his five cases. Again, the analysis of variance gave F scores insignificantly different from chance expectation for each subject.

Because of anecdotes about successes at the end of the Mind Control course, and also because our data showed a faint tendency for more recent graduates to have higher scores, we ran a second experiment in which we tested subjects on the day of their graduation from Mind Control. The method was the same except that B.S. selected a new set of recent patients, another person recruited the five subjects, and R.B. prepared the diagnostic readings. Again, the data of the entire experiment were not significant. However, one of the graduates, an engineer who for financial reasons was considering becoming an instructor in Silva Mind Control, had results on his 5-by-5 table that were better than chance expectation at the level of  $P = .05$ . Two of the subjects were children, aged 10 and 12, and their readings were meager and uninformative. If the scores of the three older subjects had been examined separately, they would have been significant. We should add that a sixth graduate of the same course volunteered to give readings and was assigned the same cases as the engineer. His scores for these cases were also better than chance expectation ( $P = .05$ ). We conclude that not all graduates of Silva Mind Control show marked clairvoyant ability. We conclude further that even enthusiastically affirmative graduates do not all retain such ability (if they ever possessed it). Our data still hold open the possibility that immediately after they have completed

the Mind Control course, clairvoyant diagnostic ability may be strong in some individuals. If this possibility is confirmed, it might be considered a not unexpected outcome of a training method which combines meditative and hypnoidal techniques with strong positive suggestion and high group morale.

### SOME ASPECTS OF KIRLIAN PHOTOGRAPHY

Larry Burton† and William Joines (Psychical Research Foundation and Duke University, Durham, N.C.)

In our work with Kirlian photography much of our effort has been directed toward determining the origin of the images obtained and to what extent the images differ from one object to another. To facilitate this work we have developed a method using Polaroid film. Using this technique, we have taken a large number of photographs of thumb pads of people in a normal emotional state, and have found striking individual differences in the Kirlian patterns produced. Also, an individual's Kirlian thumb pattern has certain structural characteristics which tend to remain constant from day to day.

We have found that both an inner and an outer field of a Kirlian image appear on black and white film sensitive to a wide range of ultraviolet (UV) radiation, but only the inner field transmits through a near UV filter (0.365 micron wavelength). The inner field also transmits through a dicyanin filter of the type used by Bagnall to view the "human aura." Bagnall claims that this aura consists of UV radiation of around 0.365 micron wavelength. Since infrared radiation from active nerve cells has been measured, and there are mathematically verifiable atomic and cellular processes capable of generating UV radiation, we are led to believe that the inner field may be UV radiation from the object, while the outer field is corona, or ionization of air molecules by the electric field applied to the object in the Kirlian technique. Lending support to the theory that the inner field is dependent upon cellular generation of UV is the fact that the flares of the inner field are usually much larger around living matter.

It seems reasonable to suppose that the applied electric field which produces the Kirlian image simply enhances naturally-occurring cellular radiation (the "aura") up to an intensity level that is photographically detectable, in much the same way we induce a cell to fire by applying an electric field in the form of a transmembrane potential. We have found in our later research with Polaroid 3000 panchromatic black and white film that the Kirlian image produced around people in an ordinary physical and emotional state is due entirely to ionization of gases in the air by the applied electric field. This finding does not rule out the existence of radiation from the human body, but rather indicates that either the radiation level is so low as to be photographically undetectable, or that radiation occurs only during times when the subject is in an altered physical or emotional state.

#### A DEVELOPMENTAL PROGRAM FOR THE COMPUTER-BASED EXTENSION OF PARAPSYCHOLOGICAL RESEARCH AND METHODOLOGY

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

The digital computer's use in parapsychology has to this time been primarily limited to the post hoc analysis of data gathered by the experimenter. Through the use of large-scale scientific machines, statistical analyses previously unfeasible or at best difficult and time-consuming have become routine. With the advent of small, low-cost mini-computers on the market, digital processing can now move into the actual implementation of the data-gathering and control functions of a particular experimental design rather than being restricted to the data analysis phase. As workers in the field take greater advantage of small in-house computer capability for the automation and control of studies, familiarity will be gained enabling experimenters to use machines to design as well as to implement studies previously impossible. In particular, a computer-based system can both improve controls in studies dealing with the exclusion of alternate hypotheses to psi to a degree not offered at this time by any other system, and also allow target generation, scoring, and parameter manipulation in process-oriented tasks in such a way that every human, experimenter or



otherwise, is blind to the particular parameter levels in the experiment at any time before the data are actually evaluated. It is possible to exclude from human knowledge all relevant data concerning the targets except the final results.

The basic concept of a sequential, finite state deterministic machine executing a preprogrammed set of instructions is common to all computers, no matter what their size. The separation of the program, which determines the machine's activity, from the actual hardware of gates, transistors, and diodes, which actually perform the activity, is what gives the small computer its great promise as a controller of parapsychological experiments. Once a program is loaded, it cannot be changed while running on the machine without willful intervention by an agent who has both access to the machine and the knowledge to implement the changes. The difficulty inherent in this process can be described as comparable to changing a sparkplug on an automobile travelling 60 miles an hour without stopping the car. Computers can be considered to be fairly resistant to parapsychological influences for two reasons. First, they are discrete, finite state automata, and thus are more insensitive to random statistical influences than are comparable analog systems. Second, computers are so designed that when there occurs either an error condition or an unexpected internal state of such a type as might be caused by the influences of the most powerful psychic we have seen to date, they sense such occurrences as error conditions and either report their state as being in error, or cease to function entirely.

Though the program is not particularly subject to external unwanted influences, it is nevertheless much more easily modified for the purpose of changing experimental parameters than is a hardwired controller. Also, by providing a logical rather than an electronic definition of experimental conditions, it more clearly reflects the goals of the experiment and leads the experimenter to define more clearly the experimental conditions. The more under the computer's control the experiment falls, the more completely all conditions and methods for dealing with situations must be specified, allowing for very tightly controlled and well-specified experiments. Through appropriate design, the experimenter can be removed from the test situation as much as desired, leaving the subject to interact with the machine and the target. The machine manipulates the relevant variables acting on the subject and the target as pro-

vided for in the design, which is in turn reflected in the coding of the program. Analysis of the data collected can be either on-line, with the machine providing varying amounts of feedback to the subject regarding his performance, or performed later, a task facilitated by the fact that the data recorded by the machine are also available to the machine in a form suitable for subsequent processing. By eliminating first the step of data recording by a human and later the step of transcribing the records to some computer-accessible medium, time is saved and errors are reduced.

From a hardware standpoint, small machines capable of doing much useful work are available in the same price bracket as an adequately equipped polygraph. Most systems are upward expandable, so previous investments in money and programming are preserved when the step to better facilities is taken. The small computer is becoming much more common in physiology, engineering, and biomedical engineering departments, and with the single addition of interfacing equipment to a random source, be it an electronic random number generator or a stored version of the Rand table or some portion thereof, many of these machines could be used to great advantage for short parapsychological studies allowing researchers to become acquainted with computer techniques and to evaluate the value of the machine for their particular research program.

Studies at the FRNM indicate that animal work in parapsychology can be automated under computer control with no loss in scoring level and with advantages in data storage and analysis as well as in intra- and inter-experiment consistency. Human studies patterned after non-computer-based paradigms are presently underway to investigate further the promise of expanding computer control into work with gifted subjects. The attitude of the subject to the experiment is important, and since the addition of the computer to the test situation is met with apprehension by some subjects, a carefully planned experimental program is being pursued to prevent disruption of psi performance during the transition. In conclusion, we may be confident that the global aims of research, better controls and greater understanding, will be aided substantially by what we learn through and about computer-aided methodology in psi research.

## PRECOGNITION AND TIME PROXIMITY TO THE FUTURE EVENT

Douglas Dean (Newark College of Engineering, New Jersey)

Osis has contributed papers suggesting that ESP score may decline with increasing distance even when 28 mood variables are controlled for. The corresponding question of whether precognitive ESP declines with increasing time to the future event needs to be brought into discussion. Louisa Rhine, on the basis of spontaneous case evidence, has consistently maintained that time and distance make no difference to ESP. She observed that the future event makes a different psychological impact on human beings than does the spatially distant event. Distance sharpens a sense of separation but time, at least in its longer reaches, dims it. Vaughan, using as evidence spontaneous cases of his own and of others, has suggested that there is no difference in accuracy whether the time be short or long. Osis by experiment got equally accurate precognition results when the time was 1-7 days and when the time was 7-33 days (mood changes were not controlled for). Anderson found stronger precognition results when a year elapsed before checking than when checking was done immediately. Stevenson summarized a case first reported by Myers in which a woman dreamed of the unusual death of her uncle three times during a six-year period before the death occurred. He also mentioned a case first reported by Bender involving a precognitive gap of 27 years. One kind of precognitive dream which is of interest here, which Vaughan has called "anniversary dreams," has the dream-event correspondence occurring on the same date but years apart. Thus a case can be made for time per se being relatively unimportant compared with psychological or emotional factors in triggering precognitive ESP.

A case can be made for the opposing view as well. Targ, at the 1972 Parapsychological Association Convention, suggested a model of precognition from which he predicted that accuracy would decrease with length of time from the event. He has built an ESP machine with a variable time switch (one-tenth second to one-half second) and is testing this prediction. Stevenson stated that the time interval in precognition cases is usually short; that is, a few days or hours. This is borne out in other collections of spontaneous cases. Dunne gave time data for 38 dreams, separated

into future (precognitive) and past (retrocognitive). In both categories the number of dreams declined with increasing time to the event. Kooy, a Dutch engineer, also found a decline in frequency with time in 193 apparently precognitive dreams collected over a period of two and one-half years. A Dutch lawyer, Kruisinger, found the same effect in a sample of 83 cases collected over one year. Dalton, an Irish scientist, found a decline with time in a collection of Myers's cases. Stevenson found a decline in his tabulations of precognitions of the Titanic sinking, as did Barker in his investigation of precognitions of the Aberfan coal-tip school disaster, and Soomere in his collection of 403 apparently precognitive incidents occurring in Estonia between 1900 and 1971.

These declines may actually be an artifact of memory, since verification of a case usually depends on remembering the prediction, whether it took place in a dream or while awake. It was discovered at the end of the last century that memory declines according to an exponential curve when plotted against time. I therefore analyzed the reported declines in the literature by plotting them on semi-log paper where an exponential decline would be a straight line. Most case collections in fact followed a straight line in the middle of their ranges, but showed bulges at the very short and very distant extremes. Thus there is some suggestion that time-declines are not solely a function of memory. What we need is a set of precognition data which does not depend on memory, such as that generated by a psychic predicting the future. I reported preliminary results of a study involving this sort of material at last year's Parapsychological Association Convention. Further work is in progress.

#### SURVEY OF DEATHBED VISIONS IN INDIA

Karlis Osis and Erlendur Haraldsson† (American Society for Psychical Research, New York)

This survey is a cross-cultural replication of one conducted in the United States some years back by K. O. In both studies, physicians and nurses were asked to report the following phenomena: (1) hallucinations or visions in terminal patients, either of persons or of surroundings;

(2) hallucinations or visions in patients extremely close to death but who on their unexpected recovery reported "other-worldly" experiences; (3) sudden rises of mood towards serenity and peace in terminal patients. Because of the implications of certain types of deathbed visions for the survival hypothesis, it was decided to conduct another such survey in a country that differed widely in culture and religion from the United States. We chose India.

Whereas the American survey was done by mail and followed up by extensive interviews over the phone, in this survey we visited eight large university hospitals in Northern India. We addressed the medical staffs, who subsequently filled out an initial questionnaire. Those reporting relevant phenomena were personally interviewed and asked approximately 70 questions on each case of hallucination or mood elevation. We inquired about the contents of these visions, whether they were of living, deceased, or religious figures, how near the visions were to the time of death of the patient, about the latter's medical condition, diagnosis, medication, fever, etc., about the patient's psychological condition, state of consciousness, mood, etc., and about the education and religious views of the patient. Finally, we asked some questions about the respondent's background and beliefs, as these were likely to have a selective effect on his observation and reporting.

Of the approximately 700 physicians and nurses who were given our questionnaire, about 350 of them reported relevant phenomena in their patients. There were 255 cases of hallucinations of persons near the time of death. Our data have just been coded, and statistical evaluation has yet to be done. A glimpse at a frequency analysis, however, reveals some interesting cross-cultural differences. When Americans have visions near the time of death, they generally see something that pleases them, whereas Indians have more fearful visions that seem to relate to their religious beliefs. Americans more frequently have visions of females than of males, whereas Indians hallucinate predominantly males, a fact that may reflect the status of women in Indian society. The number of so-called "take away" cases, in which a patient has a vision of a deceased relative or friend who tells the patient that he has come to help him and take him away to the realm of post-mortem existence, was as great in the Indian as in the American survey. These cases constitute about half of all visions of terminal patients in both countries. The crucial evaluations of our data, such

as how contents of visions and mood elevations relate to diagnosis, state of consciousness, medication, education, religious beliefs, and so forth, still have to be done.

## ESP AND SUBLIMINALITY

Martin Johnson (Lund University, Sweden, and University of Utrecht, Netherlands)

Frederic Myers introduced the concept of the subliminal in parapsychology. John Beloff has stressed that there are several similarities between subliminal perception and ESP. In 1915 Pötzl carried out a study on the influence of subliminally exposed stimuli on dreaming. During the last two decades Sweden has been one of the main centers for experimental studies in this field. Ulf Kragh invented and developed the Defense Mechanism Test (DMT), a projective test in which "threatening" pictures are flashed by means of a tachistoscope. After each exposure the subject has to report his impressions verbally or by writing. The stimulus intensity is increased by steps. This "fractionated" procedure gives us a set of different levels of interpretation from the subject's side, starting with a rather subjective stimulus interpretation and ending with a rather veridical one. The responses are coded and interpreted according to a manual. The manual is based both on empirical findings and on theoretical assumptions within a psychodynamic frame of reference.

Over the years a number of ESP-DMT studies have been carried out by myself and associates. The principal finding is that there exists a relationship between ESP scoring direction and the degree and quality of "defensiveness" in a subject's DMT protocol. Strong signs of certain perceptual-defensive structures tend to produce psi-missing whereas a low level of defensiveness tends to yield extra-chance scoring. It has also been shown that a person's ability to recall dreams (or it may be more precise to say, his willingness to report his dreams) can fairly well be predicted by DMT data. In studies not directly related to DMT work, I obtained data suggesting that a person who ordinarily would be a psi-hitter can be manipulated to exhibit a selective psi-missing by a proper selection of target material.

If the target has a very unpleasant and personal meaning for the subject, then the chances are that the subject will avoid hitting on that type of target. These findings, tentatively interpreted within a psychodynamic frame of reference, together with the notion of the experimental induction of a "micro-trauma," constitute the conceptual background of the present attempt to try to enhance a subject's psi vigilance.

I have done two studies so far. Study 1 was carried out at Utrecht; eighteen subjects took part. The picture-motifs chosen for the induction of the "micro-trauma" by using the DMT procedure were different for male and female subjects, according to psychoanalytic assumptions. Twenty pictures were chosen for each subject. The pictures were tachistoscopically presented six times at an exposure time of 10 milliseconds. The morning after the induction, the subject carried out his ESP task. Twenty batches of opaque envelopes, five envelopes in each, were presented to the subject. In one envelope in each of the batches, there was a photocopy of one of the pictures used for the induction. In the remaining four identical envelopes there were photocopies of the same size as the one in the target envelope, but depicting "neutral" motifs. The subject was asked to try to pick out the envelope that contained the same picture as he had watched tachistoscopically the day before. The positions of the targets within the batches were randomized and unknown to the experimenter. Each subject took part in only one session (twenty trials); there were 360 trials in all.

It was hypothesized that (A) a general enhancement of the subject's psi vigilance should occur, as reflected in strong positive scoring; and that (B) this effect should be more pronounced for subjects having "good" DMT protocols (i. e., showing a low level of defensiveness). A research assistant was responsible for the DMT testing. Following the experiment, he translated the statements made by the subjects from Dutch into English, and then sent them to Kragh in Oslo who carried out his ratings of the DMT protocols. Finally, on the basis of previous findings and also on theoretical grounds, Kragh ranked the 18 subjects from the one he thought should score highest to the one he expected to score lowest on the ESP task. When this ranking was matched with the ranking of actual scores of the subjects, the rank correlation was significant at  $P < .05$ , one-tailed. No general tendency to score above mean chance expectation was observed, however. Thus the first hy-

prothesis was not borne out, but the second seemed to have some validity.

Study 2 was done at Lund, and is still in progress. It has been made possible thanks to the cooperativeness of G. Windahl who put three of his subjects at my disposal. He was also responsible for the presentation of the ESP task to the subjects. These three subjects have done varying numbers of sessions, at five trials per session. The main difference in procedure from Study 1 is that the subject gives his responses in a dream laboratory setting, and the ESP task follows immediately after he is awakened at the conclusion of the last REM period in the morning. Two of the subjects have done 20 trials each, and one has done 15 trials. The 55 trials of the three combined show significant above-chance scoring ( $P < .02$ , two-tailed), thus indicating an enhancement of psi vigilance, and supporting the first hypothesis. No between-subject comparison has been made because the number of subjects is too small. In conclusion, one can say that it is very far from proven that the process of induction of supposed "micro-traumatic" stimuli really affects the psi vigilance of a subject, but the findings are at least suggestive.

#### SINGLE-CARD CLAIRVOYANCE EXPERIMENT WITH A SPECIAL SUBJECT

H. Kanthamani (Institute for Parapsychology, FRNM)

Several successful studies have been carried out during the past year with one special subject, Bill Delmore (B.D.), at the Institute for Parapsychology. The present report deals with four series of experiments conducted using a new technique called "single-card clairvoyance." As the name implies, in this method the subject guesses one card at a time presented by the experimenter. The cards are concealed in opaque black folders, slightly larger than the cards. There are two aspects to this study: B.D.'s overall performance in this new task, and his success on confidence calls made during the task. The targets were regular playing cards. Ten decks, each consisting of 52 cards, were used. The 520 cards were thoroughly shuffled by hand and placed loosely on edge in a row in a rectangular



cardboard box. The experimenter and the subject were seated on opposite sides of a desk. The box containing the target cards was kept completely out of the subject's view. At each trial the experimenter picked up one card from the box at random and slipped it into the folder. The subject could not see this. The experimenter then held the enclosed card up to the subject's view. The folder containing the card was held at a distance of about 6 to 8 feet from the subject, the back of the card within the folder facing the subject. The experimenter took special care not to see the face of the target card, even inadvertently. Also, before holding up the folder, she made sure that the folder was enveloping the target card completely. The folder was held by its lower left corner, affording a clear view to the subject, for as long as he wished, until he made a call.

Upon presentation of the target-folder to his view, the subject usually made a quick response. Then the experimenter, still holding the target-folder up, made a quick record of the call. Following that, she slipped the target out of the folder. She then recorded the target card. Thus the subject at once knew how well he had done. After providing this feedback, the experimenter removed the target card from the table and set it aside. The same procedure was repeated for every trial. Usually only one run of 52 trials was completed during a session. A break was given whenever the subject needed, usually after 26 trials. During this interval the experimenter, in the absence of the subject, collected all the target cards used thus far in the run, shuffled them, and reinserted them into the large pile of cards remaining in the box. Care was taken to insert the cards randomly into the target pile. The experimenter was careful to deny herself any conscious knowledge of the order of the cards in the box. Also, periodically, between runs, the entire pool of 520 cards in the box was well shuffled to randomize them again. The folders used to conceal the cards were also changed after every few runs. In addition, the procedure denied the subject any opportunity to have tactile contact with the targets at any time.

In all, there were four series in the experiment. The length of each one was determined before the series was started. The first two consisted of 13 runs each (676 trials) and the last two of ten runs each (520 trials). The first series was intended as a pilot, which was followed by the second as a confirmatory series. I was the experimenter in both. A special feature was introduced in Series 3, in

which the subject was requested to make "confidence calls" whenever he experienced a strong feeling of success associated with a call. Also, a co-experimenter, H. Hanumanth Rao (H. H.), was present during all sessions of this series to assist me by careful observation of all aspects of the procedure, especially the recording.

It was planned that the fourth and final series would be carried out by a different experimenter. Accordingly, H. H. took my place in selecting the targets and presenting them to the subject. However, I was also present as an assistant in most sessions of that series. In addition to H. H., who was a regular observer in Series 2 and 3, others from the FRNM staff were present as observers at various times in Series 2, 3, and 4. Interested visitors were also allowed to watch during some sessions, since their presence seemed to motivate the subject toward better performance. The results were analyzed by Fisher's CR method for playing cards for each series separately and also for the pooled series (46 runs). This method analyzes the possible relationships of the call card to the target card on a given trial into a set of nine mutually exclusive and exhaustive classes: color only, rank (face cards vs. plain cards) only, color and rank, suit only, suit and rank, number only, number and color, suit and number (i. e., an exact hit), and no correspondence. A score is obtained for each category; these scores can either be summed to yield an overall CR value, indicating hitting or missing, or used separately in a one-by-nine chi square table where they are compared with the expected values for each class, yielding a measure of general extrachance scoring.

The Fisher's CR showed highly significant hitting in all series except the first. Further, the chi square was highly significant in all the series including the first. The pooled series data yielded a Fisher's CR of 10.73 and a chi square greater than 200, both extremely significant. When the nine categories were examined separately to assess their relative contribution to the overall extrachance results, it was found that exact hits contributed most strongly, followed by color and number hits and number only hits. The exact hits averaged three times mean chance expectation, for a CR of 13. With exact hits removed, the CR for numbers was still over 7. The suit hits showed a strong negative deviation (excluding exact hits), for a CR of -3.2. With respect to the confidence call analysis, B. D. made 20 such calls in a span of seven runs in the third series. Of

these 20 calls, 14 were found to be completely correct and the remaining six were partially correct: five were number hits and one was a suit hit. Of the five number hits, four were also color hits; and the suit hit was also a rank hit. Thus, in this study, B.D. exhibited a remarkable degree of psi ability and furthermore showed some definite signs of awareness of its presence.

#### DESCRIPTIVE ANALYSIS OF ART PRINTS TELEPATHICALLY TRANSMITTED DURING SLEEP

Stanley Krippner† and Steven Zeichner (Maimonides Medical Center, Brooklyn)

A laboratory approach to the question of telepathic dreaming became possible with the advent of EEG-REM monitoring. A study was made of 74 sessions in which randomly selected art prints were telepathically transmitted to a sleeping subject. Blind evaluation of the dream protocols and art prints (and their correspondences) determined whether the attempt was a "hit" or a "miss." Chance probability of a hit was one-half. There were 52 hits and 22 misses, producing significant evidence of telepathy (chi square = 11.36,  $df = 1$ ;  $P < .001$ ). A descriptive analysis was then done on each art print, indicating that more "dynamic" content was likely to be associated with hits, while less "dynamic" content was likely to be associated with misses. We did this by adapting the Adjective Check List by Gough and Heilbrun. Three judges evaluated each of the 74 art prints with this list, checking those adjectives he or she felt applied to the picture. An adjective was held to describe a particular art print if two judges checked that item.

Because the numbers were small, no significant differences emerged. However, the following trends were observed. Of the targets associated with hits, a higher percentage were described as aggressive, alert, blue, cold, deliberate, imaginative, interesting, masculine, and unpleasant than were those associated with misses. Of the targets associated with misses, a higher percentage were described as bright, feminine, formal, orange, pleasant, unrealistic, warm, and yellow than were those associated with hits. In addition, the simpler targets (those getting fewer

adjectives) tended to be associated with hits, and complex targets (those associated with more adjectives) tended to be related to misses.

## A POTENTIAL ANIMAL MODEL FOR PARAPSYCHOLOGICAL INTERACTION BETWEEN ORGANISMS

Walter J. Levy<sup>†</sup> and James Davis (Institute for Parapsychology, FRNM)

Research reported at two previous Parapsychological Association Conventions has given significant evidence of PK on a random number generator in a test situation in which a young chicken would receive warmth if the random generator selected a channel wired to a lamp above the animal. An identical lamp, wired to the other channel, was turned on if the opposite channel was chosen. This lamp was located in a room without chickens, and was included in order to have a symmetrical experimental design. It played an important part in our later research, however, which will be described below. The young chicken was in an environment too cool for it but not sufficiently cool to injure it, only sufficient to provide mild discomfort. Initial work used chickens of one to two weeks' age, but all subsequent research has used chicken embryos with approximately one week to go before hatching. At this stage the animals have a higher heat dependency, presumably more stable internal and environmental conditions, and are easier to care for. The several experiments done so far using this design have shown significant above-chance scoring, and we are continuing to develop the design to achieve further replication, to explore the relation of scoring patterns to individual animal variables, and to investigate more general questions about psi. The experimenter has not been excluded as a source of psi in this study yet, but we are exerting our best efforts in this direction.

The present study focused on the question of whether this PK effect could be exhibited with a single individual animal and, if so, whether the scoring rates observed in the presence of the individual animals would be consistent enough to allow an assessment of competition and reinforcement between different individual animals. The procedure consisted

of selecting psi-hitting and psi-missing animals that with repeated testing would consistently score above and below chance respectively. (In fact, animals that score below chance cannot clearly be called psi-missers, because we do not know whether they might have a lower temperature requirement or, alternatively, are actually doing something deleterious to themselves by lowering the temperature.) In the reinforcement condition an animal previously identified as a psi-hitter was put on one channel (A) of a random generator and an animal previously identified as a psi-misser on the opposite channel (B). In this arrangement, when the random number generator chooses channel A, the psi-hitter's lamp is on and the psi-misser's off, so that both obtain what their observed scoring rates indicate they are pursuing; i.e., they should reinforce each other. When the random generator chooses channel B, the psi-hitter's lamp is off, the psi-misser's on, and both would presumably be working against this event. In the competition condition both animals are put on the same channel, and thus would presumably be working for opposite effects.

In the pilot experiment, a group of one dozen eggs were tested, each individually, until an animal scoring significantly above chance (52.8 percent, with 50 percent expected by chance) and an animal scoring significantly below chance (46.5 percent) were located. These were then put in the same box on the same channel of the random generator, and a nonsignificant scoring rate of 49.5 percent was obtained. Thus they appeared to have cancelled each other's efforts in the competition paradigm. The animals were then placed on opposite channels of the same random generator, where they would theoretically be reinforced, and the psi-hitter obtained a significant scoring rate of 54.25 percent (the psi-misser thereby scoring 45.75 percent). Both animals were then retested separately. The psi-misser died in a power failure which cut off the incubator heat, and no results are available. The psi-hitter had a significant scoring rate of 53.0 percent, which was comparable to his original scoring rate. There is then in these data a suggestion of an improvement in scoring when the animals reinforced each other, and a nonsignificant result when they competed.

In the confirmatory experiment, another clutch of 12 eggs were screened individually until a significantly positive scorer and a significantly negative scorer were found. The psi-hitter had a scoring rate of 52.5 percent, the psi-misser 47.3 percent. Together in the same box these animals gave

a 50.8 percent scoring rate; when they were on opposite channels of the random generator, the psi-hitter had a 53.7 percent scoring rate. Retested separately, the psi-hitter gave a 52.5 percent rate, and the psi-misser a 47.3 percent rate, both identical to their initial scoring rates. The retest rates, however, were only marginally significant, because they were based on only 1300 trials for the two eggs (due to the shortness of time before they hatched), whereas the initial scoring rates were established with approximately 2000 trials. This experiment, then, gives suggestive evidence that relatively stable scoring rates exist in association with individual animals, and that these animals can reinforce or cancel each other when they are both benefitted by the same result or both competing for opposite results. On the simplest level, this argues against the idea that the experimenter's psi is influential, since the effects are more relevant to the animal than to the experimenter. More importantly, it suggests that this work may offer a model for testing, in efficient and controlled conditions, parapsychological interactions between organisms. This is an area which has previously been very difficult to study, particularly with humans, because of the high degree of variation and instability in their scoring.

In considering the relevance of such a line of questions, one could point to the small rodent precognition work reported at previous conventions by W.J.L. and others, which appears to be demonstrating more consistent psi scoring effects on the part of the animals than is usually found in human experimentation, as well as highly consistent relations of the scoring to aspects of the animals' behavior. This would lead one to ask why there is not more evidence of psi in natural settings and why it has not been selected for by evolution more obviously than it has, at least in terms of selecting types of behavior which favor psi expression. A possible partial explanation may lie in the fact that our tests in the laboratory have involved the animal isolated from competition, direct or implied, with other animals, which is a constant condition in the natural environment. Even if the animal in the natural environment is not directly competing for food or resources, it is not implausible to think that there may still be parapsychological competition with other animals for territory or resources. This competition may act to cancel out obvious psi effects, as our experiment suggests. Such a line of thought would seem profitably to support a line of investigation into parapsychological interactions between organisms, for which this ani-

mal model appears well suited.

### A COMPARISON STUDY OF PRECOGNITION AND PK IN GOLDEN HAMSTERS (Cricetus cricetus)

Jerry Levin (Psychical Research Foundation)

Seventeen experiments in two laboratories have consistently obtained evidence for psi in small rodents, as shown in their ability to avoid a mildly aversive electrical stimulus delivered randomly to one side or the other of a testing cage. The initial work, done in France by Duval and Montredon, consisted of three experiments in which mice crossed a low barrier which divided the two halves of a rectangular cage, one side of which received a mild electrical stimulus. For each trial, the animal's position was determined automatically and the side of the cage to be stimulated was then selected by an electronic random number generator. It was found repeatedly that the animals showed a highly significant rate of success in avoidance of the stimulus for a special class of trials called random behavior trials (RBTs). An RBT occurred after an intertrial interval during which the animal crossed the barrier at a time when he was not receiving a stimulus. Such behavior was preselected since it did not presume any behavioral reasons for the animal's choice such as side habits, fatigue, or movement in direct response to the aversive stimulus. The psi exhibited in these experiments was taken to be precognitive since an animal could score a hit if he successfully anticipated the side that would receive the stimulus on the next trial.

While gradually modifying the procedure, Levy and others have carried out fourteen experiments which confirmed the French work. Levy's modifications include constant sampling of the animal's position, a computerized testing situation, and the use of an activity wheel based testing cage which allows for a quicker rate of trials and a greater amount of available information about the animal's activity. Levy's experiments confirm the previous significant scoring rate on RBTs and in addition isolate some behavioral sub-categories in which there is improved scoring. It can be argued that the procedures used in the French and American

work may not be evidence for precognition at all. At the moment just before the random number generator selects a target, the rodent is in the same situation as the chick or egg in studies of PK to obtain warmth (see preceding paper by Levy and Davis); that is, a random generator is in control of a salient aspect of the organism's environment. If we hypothesize that the chick or egg could use PK in that situation, so could a gerbil or a hamster. The possible explanation of precognition in terms of PK has historically been a problem in work with humans as well as animals.

The present pilot study had two objectives--to provide a replication of the previous French and American work, and to use the procedure empirically to investigate the relation of psychokinesis to precognition. The experiment was done in December 1972 at the Foundation for Research on the Nature of Man, using Levy's apparatus and animal facilities. The procedure involved two parapsychologically different test conditions. The precognition procedure used was identical to Levy's procedure. The animal ran in an activity wheel testing cage; his position in the wheel was sampled every 16.6 milliseconds by a PDP-11/20 computer. A trial occurred every ten seconds during the run, when the Schmidt quantum-mechanical random number generator chose side A or side B of the cage. If the animal was in the target side, a 100-milliamp electrical stimulus was sent for half a second to the whole cage. The brevity of the stimulus prevented the animal from receiving the stimulus on the wrong side of the cage. The last recorded animal position before the target was chosen was used to determine whether there was a hit or a miss. Chance probability of a hit was one-half. The PK procedure used the identical wheel and test situation; the only change was that the random number generator, instead of selecting side A or side B, selected "stimulation" or "no-stimulation" regardless of the animal's position. The animal could avoid the aversive stimulus here only by influencing the random number generator, thus making this exclusively a PK procedure.

All the experimental parameters were preset for this pilot study. There were to be 100 runs of each condition, one trial every 10 seconds, and 26 trials per run. The first trial of each run was used only to establish equal conditions for the succeeding trials, leaving 25 scored trials per run. One run of the PK condition was thrown out because of a malfunction in the testing cage, so there were actually 100 runs of the precognition condition and 99 runs



of the PK. The subjects were 13 Golden Hamsters (Cricetus cricetus), run approximately twice a day for two days in succession. Runs of the two conditions were alternated so that each animal was tested in each condition approximately twice, with about four hours between the two conditions. Since the animals were poorly marked, this pilot study did not have strict controls for identifying individual animals.

The results of the precognition procedure replicated the French and American work. The scoring on total trials was positive but not significant (52.2 percent). For RBTs the scoring rate was 55.1 percent for 782 trials (CR = 2.86), yielding significant evidence for psi. The results were even more significant for RBTs following a non-shock (NS) trial, following an intertrial interval with only one or two crosses of the barrier (called low-jump or LJ), and following both non-shock and low-jump (NSLJ). These findings have also been consistently obtained by Levy. The results of the PK procedure were positive in all categories but not statistically significant. The scoring on total trials was similar to that in the precognition condition, 52.7 percent for 1225 trials. The scoring on RBTs was 53.0 percent, yielding an insignificant CR of 1.62.

Comparing the two conditions by CRd revealed that they did not produce significantly different scoring except in the LJ ( $P = .05$ ) and the NSLJ ( $P = .01$ ) behavioral sub-categories. This suggested that although there was a difference in scoring rates in the precognition and PK procedures, it will take more testing to make it apparent whether the two will significantly diverge in all categories and the scoring will continue the trend that it has shown. In conclusion, the pilot study can be construed as a replication in a long line of successful French and American small rodent precognition work. In addition, there is positive but not significant scoring in a psychologically identical situation using a PK procedure. In light of the steady scoring rates found using the precognition procedure, the weak differences between the two conditions may well continue to diverge with more testing. A confirmation study will be done, as will further analysis of the data already collected.

## A MULTIVARIATE APPROACH TO THE PREDICTION OF ESP TEST PERFORMANCE

Kathy McGuire<sup>†</sup>, Elizabeth Percy, and James C. Carpenter  
(University of North Carolina, Chapel Hill)

Our concern in this study was the role of personality factors and mood states in psi performance. We used a clairvoyance test in which the subjects, tested in a group, were each given a scoring sheet attached to an opaque envelope inside of which was carbon paper and a target sheet; thus their written calls, using standard ESP symbols, would be automatically transferred to the target sheet to permit easy scoring. Each subject was given a separate target order, and each did ten runs of 25 trials each. There were 33 subjects in all. We also administered Nowlis's Mood Adjective Check List (MACL) and a sheep-goat question. The subjects had already taken several personality tests: the California Personality Inventory (CPI), the D-48 (a test of nonverbal intelligence), the Welsh Figure Preference Test, masculinity and femininity scales from the Gough Adjective Checklist, masculinity, author-journalist and psychologist scales from the Strong Vocational Interest Blank, and total score, verbal, and analogy scales from the Concept Mastery Test. Of special interest to us was the Welsh Figure Preference Test, which provides a four-fold typology of personality. Because Welsh's Type 2 personality is a creative and intuitive individual, Welsh predicted that subjects of this type would score higher on the clairvoyance test than the other subjects. Although there were no specific hypotheses concerning the other measures, we included them in stepwise multiple regression analyses aimed at generating equations predictive of the three criteria of total number of hits, run-score variance, and page-score variance.

The results partially supported the prediction that Type 2 persons would tend to score higher. A Type 2 person is indicated by high scores on the origence (liking for conceptual change) and intellectance (cognitive abstractness) scales of the Welsh Figure Preference Test. Both of these scales were positively correlated with hitting, and entered the regression equation with a positive loading. The regression equation for predicting the hitting criterion also contained the CPI scales for dominance and intellectual efficiency, which were negatively loaded, and achievement via conformity, flexibility, well-being, and social presence,

which were positively loaded. Concentration from the MACL was negatively loaded. Scales from the MACL were predominant in the equation to predict run-score variance. This result is congruent with the work of J.C.C. and of Schmeidler, who have both used the subject's mood at the time of testing to predict ESP performance. The equation for predicting page deviation also involved several mood scales, along with the CPI.

One puzzling finding of our study was the reversal of the sheep-goat effect for hitting. Our less than ideal testing situation may have contributed to this result. Neither of the two experimenters was feeling well, and our subjects, members of a class on psychological testing, may well have resented taking yet another test. In order to quantify mood variables which may have influenced our result, an average mood profile was obtained from the MACL for sheep and goats. Subjects of both groups produced mood profiles characterized by high levels of "social affection," "concentration," and "vigor," and low levels of "aggression" and "sadness." Our hunch about a prevailing negative mood was thus not borne out by these profiles. In comparing the profiles of the sheep and goats, a significantly higher level of "concentration" was shown by goats than by sheep, along with (as expected) a significantly higher level of "skepticism." The suggestion is raised that the situation somehow provided more of an elicitation of concentration for goats, thereby accounting in part for their superior hitting. At any rate, the main interest of the study lies in its generation of empirically derived predictive equations for three parameters of clairvoyance test performance. We feel that our findings will be more meaningful when we, and others, have applied these predictive equations to data from other subjects. Such replication is planned for the near future. We can say now that three psychological instruments, in particular, would appear to have good promise for such predicting: the Nowlis MACL, the CPI, and the Welsh Figure Preference Test.

## A DESCRIPTION OF THE PHYSIOLOGICAL VARIABLES CONNECTED WITH AN OUT-OF-BODY STUDY

Joseph Janis<sup>†</sup>, John Hartwell, Blue Harary, Jerry Levin,  
and Robert L. Morris (Psychical Research Foundation)

The object of this continuing study is to describe the physiological state of B.H. while he is undergoing an out-of-body experience (OOBE). For the purposes of this paper, it is assumed only that the OOBE is at least subjectively real. Thirteen sessions were conducted in which eight physiological variables were monitored while B.H. participated in experiments designed to detect him while out of his body in another location. These are regarded as constituting a pilot study. In a typical session, B.H. was fitted with the appropriate electrodes and placed in a soundproof room in the psychophysiology laboratory of William Erwin of Duke University Medical Center. In an adjoining room, the experimenters monitored B.H.'s physiological variables on a Grass polygraph, maintaining voice contact through a microphone. J.H. monitored the equipment while R.M., J.L., or J.J. assisted. The room was kept completely dark with white noise in the background. B.H. would proceed through several stages during the session, the first being a baseline period. The second was a relaxation and preparation period of 2 to 5 minutes which is referred to as the First Cool Down Stage. When he felt ready to have an OOBE, B.H. would signal by voice. This signal indicated the end of the First Cool Down Stage and the start of the First OOBE Stage. The First OOBE Stage would last about two or three minutes and would end when B.H. signaled by voice that he was back. Later in the session this process would be repeated, producing a Second Cool Down Stage and a Second OOBE Stage. Other stages were incorporated in the design of some sessions, but will be omitted from this discussion.

Physiological variables measured during each stage were left and right central to occipital EEG, skin potential, respiration, blood pulse volume and heart rate (plethysmograph), eye movement (EOG), and muscle movement (chin EMG). Scoring was done by manual methods for all the physiological variables except EEG. The EMG record was found to be unusable and will not be referred to further. For analysis of both the left and right occipital EEG, the taped signal was fed into an analyzer recently developed by Fritz Klein of Duke University. The analyzer derives six

parameters representing amplitude and zero crossings of the signal and its first and second derivatives. No significant difference was found when the OOBE Stages were compared with the Cool Down Stages for either the right or left EEG. Another machine scoring method is being developed by Graham Watkins, also of Duke University, in which the percent alpha of the EEG signal is determined. These data are not yet available. The data from the remaining physiological variables were analyzed by independent  $t$  tests, paired  $t$  tests, and analyses of variance. These tests were used to assess the effect of Cool Down Stages vs. OOBE Stages and differences across the thirteen sessions. The Cool Down Stages were used as a standard for comparison because of their small variance across sessions. A significant increase in respiration and heart rate from Cool Down Stage to OOBE Stage, as well as a significant decrease in skin potential, was found consistently in all three statistical tests, and therefore these results may be considered reliable. The EOG and blood pulse volume also showed statistically significant changes (EOG decreased and pulse volume increased), but not as consistently, and therefore at this time should not be considered as reliable as the significant changes in the other variables.

The analyses of variance and paired  $t$  tests further showed that the First OOBE Stage did not differ physiologically from the Second OOBE Stage, nor did the First and Second Cool Down Stages differ physiologically from each other. Therefore, it may be at least tentatively concluded that, in our pilot study, the OOBE Stage is a different physiological state from the Cool Down Stage in most of the variables measured. An outstanding exception, of course, is the EEG which thus far in the analysis has shown no significant difference between stages. It can also be said that the OOBE Stage differs from the REM dream state in that eye movement (EOG) activity during the OOBE Stage decreases from the Cool Down Stage, whereas the dream literature has shown that the REM state produces increased EOG activity. These findings, although not conclusive, have led us to plan a sleep and dream study of B.H., who also reports having OOBEs during sleep. Eye movements may possibly be a key variable in the study. We will also continue to analyze our existing data; Graham Watkins is assisting in developing machine scoring methods for all the physiological variables.

## TOWARD EXPERIMENTAL INDUCTION OF THE OUT-OF-THE-BODY EXPERIENCE

John Palmer<sup>†</sup> and Carol Vassar (University of Virginia)

This is a report of an exploratory experiment designed to examine a technique for inducing the out-of-the-body experience (OOBE) in the laboratory and to assess the effect of the procedure on subjects' performance in a free-response clairvoyance test. Sixty volunteer subjects, mostly of college age, comprised the experimental sample. Because this was an exploratory experiment, the initial strategy was to test ten subjects with one procedure and then to modify the procedure for the next ten in an effort to improve its efficacy. We continued this strategy through four variations of the procedure. J.P. then decided to expand each of these conditions to 15 subjects each and thoroughly analyze the data before attempting further modifications. The general procedure was as follows. The subject initially met with J.P. who explained the rationale and procedure of the experiment. The subject was then led to the laboratory, which consisted of two rooms. He was asked to familiarize himself with the layout of the outer room and he was shown the table on which a target picture would be placed. He then was led to the inner room and seated in a comfortable reclining chair. The windows of the room were shielded. J.P. then left the inner room to place the ESP target on the table in the outer room, while C.V. determined the speed and direction of rotation of the spiral disc (to be described later) which the subject found most effective. J.P. then returned to the room, turned off the lights, and the induction procedure began. This had been recorded on tape which was played through headphones worn by the subject as well as through the speaker of the cassette recorder. The headphones made it more difficult for the subject to localize the source of the sound, thus reducing the sense of body orientation, while at the same time allowing the experimenters to monitor the procedure from the direct output of the recorder.

The procedure consisted of three stages: (1) Pro-  
gressive relaxation technique. This was similar to the technique employed by Braud and Braud. Many people who have spontaneous waking OOBEs report being relaxed prior to the experience, and at least two persons who have claimed the ability to induce OOBEs at will mention relaxation as a first

step in inducing OOBes in themselves. (2) Audio-visual stimulation. This was designed to be conducive to creating a sense of detachment from the body. The visual stimulus was a rotating spiral which appears to travel into or away from its center, something like a vortex. The effect was created by an 18-inch fluorescent disc mounted vertically on a rotor and placed between one and one and one-half feet in front of the subject at eye level. The disc was illuminated from behind by a strobe light covered by an ultraviolet filter. The disc was rotated at approximately 1200 RPM, but by adjusting the frequency of the strobe light, it could be made to appear to rotate at the speed and direction the subject desired. The subject was instructed to stare at the spiral while imagining himself being drawn into it. When he momentarily felt himself being drawn in (as most subjects did from time to time) he was asked to take note of the experience and see how long he could make it last. The auditory stimulus was a 350 Hz. sine wave tone with frequency oscillation of + 5 Hz. The amplitude of the tone coming through the headphones was oscillated manually by turning the dial on the mixer amplifier which transmits the sound to the headphones. The sound has a numbing effect which most subjects found a helpful supplement to the disc. It also blocks out extraneous noise from the building and outside. C.V. operated the strobe light while J.P. operated the amplifier and recorder. The audio-visual exercise lasted approximately nine minutes.

(3) "Trip" to outer room. The purpose of the preceding parts of the procedure was to get the subject to "feel" what it was like to be "separated" from the physical body. During this final stage, the subject was asked to try to recreate this sense of detachment and to sustain it as long as possible while "imagining" himself traveling through the wall to the outer room, and to identify the target picture. It was during this stage that most of the variations in procedure were introduced across conditions. In general terms, we varied whether the light and sound were present during this period, whether the subject was "guided" out or went when he was ready, whether the instructions for this period were presented before or after the audio-visual exercise, and whether the subject was given an opportunity to crystallize his impressions after "returning" to his body.

The pool of ESP targets consisted of ten sets of five magazine pictures each. The set 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 round 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 C.V., 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 on a 31-point scale according to how well it corresponded to his impressions during the third stage of the induction procedure. His 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. J.P. then returned to tell the subject which picture was the target and to interview him about his experiences as time allowed.

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 filled out a short form of the Betts QMI Vividness of Imagery Scale and an objective personality inventory developed by J.P. to measure "tolerance for unusual experiences." The last 50 subjects were asked on the rating scale whether they felt at any time that they were literally outside their bodies, and 21 of them responded affirmatively. These experiences ranged from momentary feelings of separation without imagery to more sustained experiences of actually being in the outer room or more distant places. Most subjects reported feelings of detachment and alterations of consciousness during the experiment. The overall ESP results for all 60 subjects were in the negative direction, although they did not quite reach significance ( $z = -0.20$ ;  $t = 1.85$ ). However, the 21 subjects who claimed they felt they had literally left their bodies during the procedure scored significantly below chance ( $z = -0.34$ ;  $t = 2.24$ ;  $P < .05$ ), although they did not differ significantly from the other 29 subjects who answered this item. (These others produced results right at chance;  $z = -0.01$ .)

There was no significant difference in ESP scores across the four treatments. Nevertheless, it is of interest that the least negative results occurred when subjects were given the opportunity passively to contemplate their impres-



sions after "returning" to their physical bodies ( $z = -0.02$ ). Two of the subjects in this condition chose not to avail themselves of this opportunity, and both obtained negative scores. If the other 13 subjects, who did utilize the passive contemplation period, are eliminated, the remaining 47 subjects in the experiment manifested significant psi-missing ( $z = -0.28$ ;  $t = 2.48$ ;  $P < .02$ ). This analysis, although post hoc, is consistent (in a negative way) with a growing body of data suggesting that a passive, receptive set is more conducive to free-response ESP than the active, "seeking-out" set given subjects in the present experiment. This could mean that subjects somehow must be taught to adopt this passive attitude while "outside" the body if the waking OOB is to be a generally psi-facilitating state. Finally, there was a significant correlation between ESP scores and the Betts Vividness of Imagery Scale ( $r = +.35$ ;  $P < .01$ ), indicating that the ability to imagine vividly in various sense modalities apparently facilitates positive ESP scoring in this situation.

#### IS PSI CYCLICAL? INTIMATIONS OF A PSI WAVE (Psicle)

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

During an experimental parapsychology course last spring, my students and I conducted exploratory experiments with a random generator designed by Helmut Schmidt which has a display consisting of eight colored lights in a circular arrangement. On each trial a new light flashes on, either clockwise or counterclockwise from the one lit on the previous trial. Generally, the subject tries to "anticipate" which light will be lit next by pressing one of two buttons, corresponding either to a clockwise or counterclockwise rotation. In our experiments, we adopted a procedure in which the left hand was used to depress continuously the left button. As the lights began to flash, we attempted to produce continuous rotation in whichever direction they "seemed to want to rotate" during a standard run of 128 trials (flashes). Since the button was depressed throughout the run, we felt that this was almost sufficient to enable us to consider our results within a PK paradigm, and thus we labeled our experiments as "PK" studies. Because the target direction of rotation was not specified we planned to use a variance

analysis to assess the results, since rotation in one direction would produce an accumulation of "hits" on the hit counter, whereas rotation in the opposite direction would result in an accumulation of "misses" on the miss counter. Continuous rotation, varying in direction from run to run, would thus produce high run-score variance.

As we began to analyze and reflect upon our varied results, we became quite impressed with what seemed to be a very interesting periodicity in some of our data. We immediately wondered whether the periodic effects might be inherent in the machine in some way; however, given the nature of the quantum mechanical processes governing the machine's function, this did not seem probable. Then one of the students, Joseph Kazickas, made a very important observation. In contemplating the graphical results of two similar experiments which I reported at last year's Parapsychological Association Convention, he noted that the scoring of the second study seemed to start at about the same level as it had ended in the first study for the two corresponding groups. I then plotted the results of both studies on one graph, connecting the data for each of the two group categories in sequential order of runs. This produced an appearance of two cyclical processes that were about 180 degrees out of phase, particularly during the last three quarters of the two studies combined.

With this added observation of apparent periodicity from a target-guessing situation, we set out to do some more experiments with the machine. Two long-term experiments, each using a single subject (one being myself), produced further suggestive results. In addition, I began searching the parapsychological literature for suggestions of a cyclical process in published psi data. The yield was light, because most data have not been reported in a form which makes a search for periodic effects over extended periods of time possible, but it was encouraging. There are, moreover, several clues in the literature which suggest that it may be profitable to conceive of psi as a fluctuating, rhythmical process. The bidirectional nature of laboratory psi (psi-hitting and psi-missing), the variety of position effects (declines, inclines, U-shaped functions, the puzzling phenomena of mirror image curves in differential response situations, etc.), and perhaps even displacement effects are possibly all explainable as manifestations of a theoretical psi wave, which I have called the psicle.

Various data suggest the hypothesis that the psicle is a non-symmetrical wave which fluctuates not only in intensity, but also in "polarity." Three types of polarity reversal seem to be evident. (1) Single, positive reversal: in this type the first and second cycles correlate positively when the units on the X-axis (serial order of runs, segments, etc.) are taken in reverse order. (2) Single, negative reversal: in this type the first and second cycles correlate negatively when the units on the X-axis are both taken in normal order. (3) Double, negative reversal: in this type the first and second cycles correlate negatively when the units on the X-axis are taken in reverse order. According to this scheme a double, positive reversal would not exist since it would take the form of what could be called the positive, nonreversal, in which the first and second cycles would correlate positively when taken in normal order.

Empirical data drawn from both my own research and the parapsychological literature illustrate the three types of polarity reversal. A male student who was taking an individual tutorial in parapsychology with me produced data which, when smoothed by the method of running averages, provide an excellent example of the single, positive reversal. When the serial order of his run scores for Day 1 were reflected on the X-axis, a rank difference correlation of 0.85 ( $P < .01$ ) with his run scores for Day 2 was obtained. The number of data points ( $n$ ) was eight. A rank correlation of about the same magnitude (0.81;  $P < .01$ ) was obtained when the raw data were reflected ( $n = 10$ ). An additional example of the single, positive reversal was drawn from data collected by Thouless in 1949. The data points were obtained from a second-order smoothing of his data. When these points for a clairvoyance test were reflected on the X-axis, a rank difference correlation of 0.81 ( $P < .015$ ) was obtained with the points for a precognition test ( $n = 8$ ).

An example of the single, negative reversal comes from raw data taken from a recent study by myself and a student, Barry Lenk, which is in the process of being analyzed. This research utilized the Schmidt two-button machine described above and studied "PK" scores and "control" scores. In the control condition the button was depressed by way of a switch while the subject was occupied with another activity. Subjects were classified as verbalizers, visualizers, and intermediates on the basis of a word association test. Intermediates and verbalizers produced this reversal pattern (Kendall rank correlation = -0.83;  $n = 4$

in each case;  $P = .10$ ); the visualizers did not. The wave form represents the results of four experimental runs and four control runs, with the former appearing as an unreflected mirror image of the latter on the Y-axis (magnitude of run score). Another example of the single, negative reversal is taken from the raw data of a 1959 study by Anderson and Gregory. The sequence of the run segment scores for a clairvoyance experiment were correlated with the sequence of run segment scores for a precognition experiment carried out the following year. The rank difference correlation for five data points was  $-0.90$  ( $P < .05$ ).

The double, negative reversal is illustrated by smoothed data taken from an unpublished experiment which I conducted within the context of my parapsychology course last spring; the Schmidt machine previously described was again used in the "PK" mode. Subjects were classified into two groups on the basis of their performance in a pilot experiment. When the smoothed wave form of one group was reflected on the X-axis and correlated with the smoothed wave form of the other group the rank difference correlation of six data points was  $-0.87$  ( $P < .04$ ). A double, negative reversal was also found in raw run score data which I collected from a housewife who tried on three different occasions to guess the targets on a single target sheet (rank correlation =  $-0.83$ ;  $n = 6$ ;  $P < .05$ ). The polarity described here may be analogous to the electromagnetic induction of alternating current. Movement of a wire in a magnetic field induces electrical current; movement in the opposite direction produces a reversal in the direction of the current flow. "Psychological movement" from condition to condition or phase to phase of an experiment may likewise induce a change in the polarity of the hypothetical psi wave.

#### GEOMAGNETIC PERTURBATIONS AND RSPK

W. G. Roll† (Psychical Research Foundation) and Livingston Gearhart (State University of New York, Buffalo)

Recent studies of RSPK (poltergeist) phenomena have shown these to be related to psychological and physical factors. It appears that psychological tensions in the ostensible agent provide a source of energy which is radiated into the

environment where it is converted to familiar forms of energy, usually kinetic energy. The process resembles known types of energy exchange, since the number of incidents is attenuated with distance from the source and since the attenuation tends to be in the form described by the exponential decay function. The similarity of RSPK incidents to other natural phenomena suggests that RSPK may be affected by known physical factors. L.G. had noticed an apparent relationship between disturbances in the magnetic field of the earth and various phenomena such as unusual animal migrations. When he extended the study to RSPK cases, an apparent relation emerged between the onset of the disturbances and these geomagnetic perturbations. Geomagnetic changes can be produced by sunspots but most appear to be of internal terrestrial origin. They are recorded by local stations spread throughout the world. The readings are converted to three values; 0 for quiet days, 1 for average days, and 2 for unusually disturbed days. The values are averaged to one decimal place to provide an international daily figure describing the magnetic character of the earth. About fifty observations contribute to this figure.

L.G. recorded the days of inception of twenty apparent poltergeist cases from the year 1905 to 1969. At the P. R. F. we then compared the perturbations on these days with the average for the year in question. Twelve of the cases began on days which were more disturbed than the average and eight on less disturbed days. While L.G. was at the P. R. F., he found an additional five cases in the parapsychological literature; three of these were above and two below the yearly average. Following L.G.'s departure, we found another five poltergeist cases; again three were above and two below. There is often uncertainty about the day of inception of RSPK phenomena in a given case. In addition, the evidential status of some of the cases in L.G.'s initial collection was in doubt. W.G.R. was therefore particularly interested in determining to what extent the five RSPK cases he had investigated personally, and which seemed to include genuine effects, followed the trend. It was found that four of the five cases occurred on days of high disturbance and one on low.

Of the total of thirty cases, eighteen were above the yearly average and twelve below. This difference is not statistically significant. The separation remained about the same when, instead of the yearly averages, we used the monthly averages or a combined average for all the years.

There were generally more magnetic disturbances on the day following the inception of RSPK than on the day preceding inception, with twenty-two cases against eight (chi square = 5.63;  $P < .03$ ). Four of the five cases studied by W.G.R. fell into this category. The results are at best suggestive that a relationship exists between geomagnetic disturbances and RSPK phenomena. However, such a relationship is likely to be concealed because of uncertainty in the RSPK reports and uncertainty in the geomagnetic figures. The distribution of observatories over the earth's surface is uneven, especially over the ocean. Moreover, the number 0, 1, or 2 is assigned relative to the norm at a particular station, not to a general norm. Thus an average amount of disturbances at one station may be extreme at another. There is more disturbance in regions of high latitude, and there is a semi-annual period with maxima in spring and fall. This might lead one to expect more RSPK activity at those places and times. The highest concentrations of RSPK reports were in March and August which each had five cases of the total of thirty. A study of the relationship of RSPK to special areas of the globe is complicated because population density and mass media might produce more RSPK cases and reports in certain areas of the globe and thus conceal factors such as geomagnetic disturbances. This part of the study still remains to be done.

## PK EFFECT ON RANDOM TIME INTERVALS

Helmut Schmidt (Institute for Parapsychology, FRNM)

I have previously reported promising attempts to increase the efficiency of PK testing by increasing the operation speed of the testing apparatus. In these tests, using a random number generator with a 50 percent probability of a hit, I gave the subjects fast feedback of hits and misses through feedback devices such as headphones. This kind of procedure did not appear favorable for PK tests with animals, in which each miss causes the animal to receive a slightly unpleasant electric stimulus, since the animal would receive too many unpleasant stimuli. Thus the search for other, more appropriate, high speed tests seemed justified. The machine I developed for use in the tests to be discussed below has an electronic counter on top which ad-

vances during a run at the rate of 12 steps per second. After each step a random decision is made such that the counter stops with a probability of 1 in 64 and advances with a probability of 63 in 64. Thus the counter will stop after an average of 64 counts (i. e., after  $64/12$  or 5.3 seconds running time), and the actual running time in a particular run is a random variable.

The first tests with the machine used human subjects. To initiate a run the subject presses a button. This resets and then starts the counter to advance at the rate of 12 steps per second. The subject tries to keep the counter running, up to as high a number as possible. When the counter stops, the score is manually recorded and the machine is ready for the next run. As a safeguard against recording errors automatic cumulative counters register the total score for a test session, providing thus a second, independent record. The subjects are encouraged to associate with the test mental pictures of some pleasant state or process which they want to keep going on and on.

A pilot test and a two-part confirmatory test were done with human subjects. The pilot test and the first part of the confirmation used several subjects who had shown some evidence of PK ability in previous work, with myself as experimenter. The second part of the confirmation used one subject who had been particularly successful in previous tests, and a different experimenter. The total length of the confirmation was prespecified to be 50,000 trials (12 trials per second). The pilot test had 250 runs with an average length of 75 trials, where 64 was expected by chance. This gave a significant CR of 2.5 and a Psi Quotient (PQ) of 25, using the run as the unit. The first part of the confirmation had 530 runs with an average length of 77 trials (CR = 4.2; PQ = 33). The second part of the confirmation had 105 runs with an average length of 92 trials (CR = 3.8; PQ = 135). Thus all three tests showed marked above-chance scoring.

The second set of tests used animals, brine shrimp, as subjects. They were tested in groups. The generator was set so that at each step (again, at the rate of 12 per second) the stopping probability was 1 in 32 (rather than 1 in 64). A few seconds after each stop the next run was automatically initiated. Following the general pattern of recent animal PK research, the generator was connected to a shocking device so that with each stop an electric shock

was administered to the group of brine shrimp. The shock was provided by the closing of a relay which caused the discharge of a capacitor which was charged by a 600-volt potential. The discharge was conducted through a trough with salt water containing the brine shrimp. The ESP measure used in these tests was number of shocks (stops) per series, which is inversely proportional to the run-length measure used in the human work.

The exploratory series consisted of ten sessions, each of approximately 25 minutes' duration; a total of 60,095 trials were done. The observed number of shocks was 1760, significantly lower than the chance expectation of 1878 ( $CR = 2.7$ ). Thus the animals appeared to be using PK to lengthen the runs (i.e., to reduce the number of shocks). The confirmatory series employed a procedural refinement. In an attempt to reduce the possible influence of experimenter PK, control runs (with the shocking device disconnected) were alternated with test runs in an automatic manner so that the experimenter never knew whether at the moment a control run or a test run was in progress. The experiment comprised a total of 211,071 trials, approximately half of them in test runs and the others in control runs. The number of test trials was 107,104, for which the expected number of shocks was 3347. The actual number of shocks was 3164, which is significantly lower than chance expectancy ( $CR = 3.16$ ). The number of shocks in the control trials was not significantly different from chance expectancy.

#### EEG ALPHA RHYTHMS AND FREE-RESPONSE ESP PERFORMANCE

Rex G. Stanford<sup>†</sup> (St. John's University, Jamaica, N. Y.)  
and John Palmer (University of Virginia)

Rao and Feola have reported that a single subject trained alternately to enhance or suppress occipital alpha performed significantly better on a free-response ESP test during alpha generation than during suppression trials. In the only other study of which we are aware involving the relation of free-response ESP to alpha density, R. G. S. served as subject for a series of 80 trials. Although the



observed relationship between alpha density and ESP performance was positive, it did not reach significance. This may have been because the trial-to-trial variations in alpha density were likely not as great as in the Rao-Feola study, since there was no attempt to manipulate alpha density.

The present study examined density and frequency of monopolar right occipital alpha activity (referenced to the left ear or to the area over the left mastoid) in 32 male and female volunteers aged 14 to 65. Subjects first listened with eyes closed to an eight-minute recording of Indian flute music, with the instruction that they were to allow and encourage the music freely to evoke in their minds whatever images, thoughts, or impressions it would, as a kind of mental "loosening up" exercise prior to the image-reception period which would follow. When the music stopped, the subjects, while continuing to keep their eyes closed, were to let impressions and images freely develop and were to report them for tape recording. These impressions and images, they had been told earlier, would likely relate to the content of the target picture and would occur spontaneously without any effort to focus mentally on the target or to "see it." These spontaneous images and impressions would develop, they were told, because they were motivated to do well and their unconscious minds would obtain the relevant information and communicate it to their consciousness without their making a conscious effort to think about the target picture.

The target picture was randomly selected for each subject from a pool of 100 pictures taken from magazines. It was concealed in an aluminum-foil jacket which in turn was enclosed in a black cardboard folder, which was placed several feet from the subject during the music and image-reception periods. After the image-reception period, the EEG electrodes were removed and the subject listened to the tape recording of his utterances. He then rated the target picture and each of four control pictures on a 21-point scale as to the degree of its correspondence to his images and impressions. Both the subject and the experimenters were blind as to which picture was the target. Each subject's ESP score was a Z-score based on the difference between the rating given the target and the mean rating of all five pictures. Alpha density (percent-time) and frequency (Hz) measures were computed separately for the music and image-reception periods by manually scoring polygraph records of the EEG output of our electronic alpha filter.

No significant relationships were found between ESP performance and alpha frequency during the music or image-reception periods, nor between ESP performance and shift in alpha frequency from the music to the image-reception period. Thus the alpha frequency correlations found in R.G.S.'s previous studies apparently do not generalize to the present procedure. However, significant relationships were found between ESP performance and alpha density during both the image-reception (i.e., ESP test) period and the music period. One-tailed tests were used throughout because of previous research suggesting positive relationships between alpha density and free-response ESP. When ESP scores were used as the predictor, subjects with positive ESP scores ( $N = 17$ ) showed significantly greater alpha density during the image-reception period than did subjects scoring negatively or at chance ( $N = 15$ );  $t = 2.43$ ,  $df = 30$ ;  $P \approx .01$ . The above-chance scorers had a mean alpha density of 37.54 percent compared to 12.13 percent for the others. An almost identical relationship was observed between ESP performance and alpha density during the music period ( $t = 2.27$ ,  $df = 30$ ;  $P < .02$ ). During this period, above-chance ESP scorers had a mean alpha density of 41.29 percent compared to 17.61 percent for the others.

When alpha density scores were divided at the median for each period and used to predict ESP scores, no significant differences were obtained for either the music or image-reception periods. However, in both periods, subjects above the median in alpha density scored significantly above chance on the ESP test (image-reception:  $t = 1.90$ ;  $P < .05$ ; music:  $t = 2.10$ ;  $P < .03$ ). Shift in alpha density from the music to the image-reception period did not relate significantly to ESP performance. The major outcome of this study accords with the findings obtained by Rao and Feola using a within-subject design. These studies suggest that alpha density may be positively related to performance on free-response ESP tasks. The fact that ESP scores significantly predicted alpha density but not vice versa suggests that relatively high alpha density may be a necessary but not sufficient condition for success on the kind of free-response ESP test used in this experiment.

## INVESTIGATION OF SILVA MIND CONTROL CLAIMS

Alan Vaughan (Psychic Magazine, San Francisco)

An organization called Silva Mind Control has attracted nationwide attention with their advertised courses in "Mind Control," achieved, they claim, by teaching students to "enter the alpha state." At the conclusion of the course, each student is given a final examination, and awarded a diploma if he passes. According to advertisements in The New York Times, "Not a single person has failed to graduate." Silva Mind Control boasts more than ten thousand graduates. The final examination of the Mind Control course requires the student to make a distant clairvoyant diagnosis of a physically ill person whose first name and last initial, sex, age, and city of residence are provided. The student is then awarded his diploma if he passes this test. In 1970 I received a letter from a nurse in a southern city who had completed the Mind Control course and had received her diploma. She asked me to arrange for an objective test of her and her fellow graduates' distant clairvoyant diagnosing ability. Accordingly, with cooperation from a physician, I sent her the first names, last initials, sex, age, and city of residence of five patients whose conditions were unknown to me. The nurse and 20 other Mind Control graduates attempted to make clairvoyant diagnoses for these and returned them to me.

To evaluate the readings quantitatively, I selected two patients of the same sex and comparable ages, put their 21 readings each (total of 42) on coded cards, randomized them by flipping a coin, and sent them to the physician for judging, asking him to guess which of the two patients was being described by the diagnosis. Chance would give 21 "hits." The physician's judgments gave 16 "hits." In addition, he indicated on the cards any apparently correct diagnostic statements that guided him in his choice. Only one card bore a correct diagnostic statement. This card read: (1) left arm amputated; (2) left leg amputated; (3) trouble with hearing. Of these three statements, only the last was correct. I then sent the physician the remaining 63 readings (for three patients by 21 students) and asked him to report to me any additionally correct statements. He found no other striking correspondence to his patients' conditions. These findings would seem to put in doubt the claims of Silva Mind Control.

SENSORY (PERCEPTUAL) DEPRIVATION AND ESP:  
The Regulation of Auditory-Visual Input Modalities in a GESP  
Study with Continuous Interoceptive Reports

Charles Honorton<sup>†</sup> and Sharon Harper (Maimonides Medical  
Center)

Investigations of ESP in altered states of consciousness suggest that psi performance may be related to sensory-cortical inhibition. The altered states traditionally associated with ESP are characterized by a redistribution of attention toward internal processes and a concomitant reduction in attention toward somatic and environmental stimuli. C.H. has suggested earlier that when sensory input and processing are attenuated, the number of irrelevant stimuli impinging upon a subject as he attempts to respond extrasensorially to a target may also be attenuated, thereby increasing the accessibility of psi inputs (i. e., the subject's ability to detect, recognize, and respond appropriately to psi impressions). At the 1972 Parapsychological Association Convention, C.H. presented a preliminary study of GESP under conditions of partial sensory deprivation. The present study involves a continuation of this research, utilizing a different technique for regulating sensory (perceptual) inputs.

Thirty unselected, volunteer subjects each participated in a single 35-minute session in which their auditory and visual input modalities were regulated so as to maintain a constant level of unpatterned stimulation. A homogeneous visual field (Ganzfeld) was produced through placement of halved ping-pong balls over the subject's eyes, with a diffuse light projected over the eye covers. Continuous auditory stimulation was presented by way of a repetitious recording of the seashore played through headphones placed over the subject's ears. (White noise was used in pre-experimental pilot sessions, but was discontinued since subjects frequently complained that it was unpleasant and distracting.) The subject was seated in a comfortable position in a reclining chair in a sound-attenuated room. Prior to beginning the session, the subject was given these instructions: "In this experiment, we want you to 'think out loud.' Report all of the images, thoughts, and feelings which pass through your mind. Do not cling to any of them. Just observe them and describe them as they go by. Do not try to anticipate or conjure up the message. Just give yourself a suggestion right now that the message will appear in con-

sciousness at the appropriate time...."

After beginning the session, we performed a randomization procedure to select a target program and to determine which of five 5-minute periods would be the "sending" time. The target programs were 124 stereoscopic "View Master" reels, divided into pools of four reels each. Each target program (reel) contained seven thematically-related slides. An agent located in another room observed the target program, at the designated time period, through a hand-held viewer. The subject's interoceptive report was continuous throughout the 35-minute session. The report was taken down verbatim by the experimenter (S.H.) in the next room. The subject did not know the "sending" time. S.H. did not know the target. She did know, after the subject had already been sealed in the room, the time of sending in order to indicate on her notes which images occurred during sending.

At the end of the session the subject reviewed the experimenter's notes and blind-rated the correspondences between his report and each of the four potential targets in the pool. The 13 direct hits (first choices) constituted a scoring rate of 43.33 percent, which is significantly above the expected chance rate of 25 percent. The CR (corrected for continuity) is 2.11 ( $P = .017$ , one-tailed). For those who are interested in psi efficiency, the "Psi Quotient" or PQ (developed by Schmidt) is 148.4. As a base of comparison, the famous Pearce-Pratt distance series of 1850 trials was associated with a PQ of 64, and the Soal-Goldney work with Shackleton had a PQ of 45. This goes against the common myth in parapsychology that free-response methods are inefficient ways of obtaining psi.

## WORK WITH SPECIAL SUBJECTS\*

FIRSTHAND OBSERVATIONS OF NINA S. KULAGINA  
SUGGESTIVE OF PK UPON STATIC OBJECTS

J. G. Pratt<sup>†</sup> (University of Virginia) and H. H. J. Keil  
(University of Tasmania)

Nina S. Kulagina of Leningrad seems to be able to demonstrate a voluntary PK effect by moving small objects. We became interested in this subject when we both saw a film of her in action that was presented by her chief investigator, Dr. G. A. Sergeev, at a parapsychological conference in Moscow in 1968. We have therefore sought opportunities to work with Kulagina, and the purpose of this paper is to put on record the limited success achieved thus far in two informal sessions. Even though our own work with Kulagina to date is not conclusive, we consider that it is important to share our observations and impressions with our colleagues in parapsychology as well as other interested scientists at this time. There are three reasons for not waiting until the observations become conclusive for or against the PK interpretation or until it should become clear that Western scientists will not be able to reach that stage with Kulagina. One is that the widespread publicity given to Kulagina's abilities through sensational journalism has created an exaggerated impression of what is already known about her--an impression that can only be corrected by statements from serious investigators interested in the case. A second reason is the need for those who have taken advantage of the limited opportunities to work with Kulagina to make their firsthand observations and experiences available to other qualified investigators who may be drawn toward working with this subject if they should find an opportunity to do so.

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

reached a stage that justifies deliberate efforts to find new subjects nearer at hand who can achieve similar PK effects, and the better it is known what has been done with Kulagina the more likely it seems that other such subjects may become available.

A visit to Leningrad by H. H. J. K. in June 1970 was limited to discussions with Sergeev and his associate in some of the work with Kulagina. This contact served its purpose in that it revealed that later visits to work with Kulagina would be welcomed. Accordingly, J. G. P. went there for three days with Mr. Champe Ransom in October 1970. After an initial exchange of information with Sergeev, he arranged to bring Kulagina, her husband, and two other persons to our hotel room in the evening. Over a period of about two hours we observed Kulagina several times causing our own test objects (a wooden match box and a compass) to move short distances toward her on the table at which she was seated. Even though the conditions were not rigorously designed and controlled, we could not observe that the motions were caused in any normal way.

A small nonmagnetic metal cylinder that belonged to Sergeev also was seen to move under three different circumstances. Once it was standing on end on the table and supporting one end of an unexposed roll of Polaroid film while the other end of the film, the one nearer Kulagina, was resting on a small wooden block. The cylinder slid about one inch along the length of the film when Kulagina held her hands toward it and appeared to concentrate strongly. This arrangement was proposed by the Russian scientists to check upon possible "exposure" effects on the film if the object moved in its proximity. (There were no marks on the film when it was developed immediately after the event.) Subsequently this same cylinder moved two times; the movements were recorded in a motion picture. In one instance the object moved perhaps a quarter of an inch while it was surrounded by a field of aquarium gravel with a drinking glass turned upside down over it. Subsequently it moved about an inch through the field of gravel without being covered by the glass. This motion picture film and two longer Russian films of Kulagina are held in the Division of Parapsychology at the University of Virginia, where they may be viewed for appropriate scientific purposes.

In February 1971 the two of us went as tourists for what we expected to be a visit of two weeks' work with Kula-

gina. Although we had previously received assurances that our visit was expected and welcomed, unforeseen circumstances that seemed to have a political source prevented Kulagina from seeing us. Subsequently an international, multidisciplinary group of scientists was organized and requests to work with Kulagina were sponsored officially through government channels, but these requests were denied by the Soviet authorities. Following the Edinburgh Parapsychological Association Convention in 1972, we decided to make another unofficial visit during which we would contact Kulagina directly. Accordingly we dropped in unannounced at her home for a social call on September 24, 1972. We were cordially received and in a short while had arranged for a formal test session in our hotel on the following Tuesday. In preparation for that meeting we brought out some of the test objects that we expected to use and placed them on the table before Kulagina.

To our surprise Kulagina began making the characteristic motions with her hands on both sides of a small plastic jelly dish we had brought from the plane. While doing so she said that she had not done anything of this kind for about a year and she did not know whether she could succeed. After perhaps one or two minutes she stopped her motions and breathed deeply three or four times as if trying to muster energy for a greater effort. Then she immediately started in again to concentrate upon the jelly dish. Her actions were qualitatively quite different. She was obviously trying hard, which she had not appeared to be doing before. The jelly dish was resting on the open pages of a large-sized slick-paper book we had brought from Prague. Back of the jelly dish about four inches farther away from her was a small block of wood we had also put out for her to see. Suddenly the block of wood moved about one inch (two cm.) in her direction, angling slightly toward her left and rotating slowly in a clockwise direction as it did so. Both of us saw this, as we immediately confirmed with each other. Then we saw it move again in the same way. After observing the movements of the wooden block we placed a hollow but completely closed plexiglass cube (approximately four inches on each edge) before Kulagina. This cube contained two colored dice and a small quantity of red aquarium gravel. Kulagina seemed to be fascinated by this device, and she began concentrating in an apparent effort to make the objects inside the cube move. As with the earlier objects, the cube was resting on the page of the open book. The dice did not move inside the cube, but the cube itself slid about two cm. toward Kulagina.



Under the informal conditions that existed, these observations are inconclusive, but they were nevertheless impressive to us. Kulagina had not moved from the table from the time we put the objects down until they moved, and one or both of us had her under direct observation for the whole time. The objects were ours, and they were resting on our "surface," the page of the open book. We never observed any behavior suggesting that Kulagina was preparing a trick, and she would not have known to prepare it before we unexpectedly produced the objects. We were encouraged by what we had seen and we returned to our hotel with a strong feeling of optimism regarding the prospects for a successful formal session on the following Tuesday. Unfortunately that meeting did not take place. After Kulagina failed to keep the appointment for the Tuesday session, we heard that the authorities had been in touch with her after our Sunday visit and had prohibited her from meeting us again. We have no reason for discounting this explanation, though we cannot claim to have full knowledge of what happened. For a more detailed account of these observations of Kulagina and of the scientific and cultural setting of our interest in the case, see the October 1973 issue of the Journal of the American Society for Psychical Research.

#### ESP EXPERIMENTS WITH URI GELLER

Russell Targ<sup>†</sup> and Harold Puthoff (Stanford Research Institute, California)

During the past several months we have been doing a series of experiments with the gifted subject Uri Geller. Mr. Geller is a twenty-six-year-old Israeli army veteran. We found him a cheery and outgoing subject, willing to take part in any experiment we designed for him. These experiments fell into two general categories. First, there were perception (ESP) experiments, in which Uri was asked to identify and draw the contents of sealed envelopes. Further work of this type involved his endeavoring to find an object hidden in one of ten identical film cans, or to identify the upward face of a die in a box. In the second group of experiments (PK), we investigated his apparent ability to interact with laboratory equipment by perturbing the experimental apparatus without physical contact. The perception

experiments will be described in this paper, and the PK studies in the paper "PK Experiments with Uri Geller and Ingo Swann," which H. P. will present later [see page 125]. The perception experiments comprised two phases. The first, exploratory phase was undertaken for the purpose of determining the existence and extent of Geller's paranormal perceptual abilities, and is described in a motion picture film which you will see now. The film summarizes the results obtained in approximately six weeks of experiments with Geller. It is not a summary of all work performed with Geller during this period, nor is it a definitive report on the experimental protocols that were followed. The second phase of the perceptual experimentation consisted of more tightly controlled drawing studies, and will be described later in this paper.

I. Preliminary Work (Summary of Film). (A) Dice Box: a double-blind experiment was performed in which a single die was placed in a small metal box. The box was then vigorously shaken by one of the experimenters and placed on a table. The orientation of the die within the box was unknown to the experimenters at that time. Geller would then look at the box without touching it and call out his perception as to which die face was uppermost. This experiment was performed eight times, with Geller giving the correct answer each time. The probability of this occurring by chance is approximately one in a million (one-sixth to the eighth power). This experiment was actually performed ten times, but on two occasions Geller said his perception was not clear and he did not wish to guess.

(B) Hidden Object Experiment: this experiment was also done with a double-blind protocol most of the time. Ten identical aluminum film cans were placed in a row. An outside assistant not associated with the research would randomize the cans with regard to position and then put the target object into one of them. The assistant would then put the caps on all cans and leave the experimental area, notifying the experimenters that the experiment was ready. The experimenters would then enter the room with Geller, who would either pass his hand over the row of cans or simply look at them. He would then go through an elimination procedure in which he would call out the position of all empty cans until there were only two or three cans remaining. He would then announce which can he thought contained the target object. This task was performed correctly twelve times ( $P = 10^{-12}$ ). On two occasions he declined

to guess. On one of these occasions the target was a paper-wrapped metal ball bearing. The other object he declined to locate was a sugar cube. He readily located water, steel ball bearings, and small magnets.

(C) Picture Drawing Experiment: in this experiment simple pictures were drawn on three-by-five-inch file cards. The pictures were put into double-sealed envelopes. During the experiment, an envelope would be selected by the experimenter from a locked container. He would open it to identify the picture and then proceed to the experimental room with the (again sealed) envelope. Geller made seven excellent reproductions of the target pictures. No errors were made. In addition this experiment was performed, on a casual basis, numerous times with visitors and many other staff members of this laboratory. We are aware that all these preliminary picture drawing experiments are in principle subject to sensory leakage since the experimenter and subject were in the same room.

II. Later Work (Picture Drawing Experiments). In July 1973 we conducted a further series of perception experiments to try and get a more detailed understanding of the kind of perception that Geller does, and to do experiments under increasingly difficult circumstances, and under quite a variety of different shielding and distance conditions. In this series of experiments we had Geller closeted in an electrically-shielded, acoustically-shielded Faraday cage. It has an inner door and an outer door, both of which seal, making a soundproof, light-tight enclosure with solid steel sides. On an average day Geller would attempt to draw two or three targets. Before the target was selected for a given trial, he was locked inside the shielded room. The target was chosen by opening a large college dictionary at random, and then drawing the first thing that we felt was a usable target on that page. We then posted our drawing on the wall outside Geller's shielded room. Geller did extremely well under these conditions. One example is a trial in which a drawing of a bunch of 24 grapes was the target. Geller did not name what he drew, but he drew a recognizable bunch of grapes, and said he saw drops coming out of the picture. He claimed he saw purple circles and then drew exactly 24 of them. On one occasion R. T. drew the target picture while he was inside the shielded room, and Geller reproduced it excellently from outside. Another procedural variation consisted of having a computer graphics program produce the target picture, then store it in bit form

in memory, so that when Geller attempted to guess it there was no actual picture for him to "see." In three trials using this procedure he also showed success. We feel that, as a result of this series of very well-controlled experiments in which Geller was almost always separated from the target material by shielding, we can safely say that it is evident that Geller does have paranormal perceptual abilities.

#### TWO CASES OF THE REINCARNATION TYPE WITH WRITTEN RECORDS MADE BEFORE VERIFICATIONS

Ian Stevenson (University of Virginia)

In the usual case of the reincarnation type the child subject importunes his parents to take him to the family with whom he claims to have lived in a previous life. If the child gives his parents sufficient details about the family and its village or city to permit their identification, they usually take him there either to please him or to satisfy their own curiosity. After the two families have met and the child's claims have been vindicated the case reaches the attention of persons outside the families concerned, often through newspaper reports. When investigators then come to study the case, the two families may have mingled their memories and perhaps unconsciously attributed to the subject more correct statements about the previous life than he actually made before they met. This objection cannot arise when a written record of the child's statements is made before any attempt at verification. Cases of this type are extremely rare because of the mentioned tendency of the child's parents to take him to the place he talks about and their usual lack of appreciation of the importance of prior written records. There are nevertheless about a dozen cases of this rare type on record. Two (the cases of Swarnlata Mishra and Imad Elawar) were published in my first book of case reports, Twenty Cases Suggestive of Reincarnation. The following two cases add to the small but increasing number of cases in this valuable group. Both these cases occurred in Sri Lanka (Ceylon) in recent years.

In the first case the subject, Indika Guneratne, was the son of a cultivator of very modest means living in a village, Gonapola, about thirty kilometers from Colombo.

He was born on July 26, 1962. At the age of about three and a half Indika began to talk about a previous life he had lived in Matara, a large town of southern Sri Lanka (on the coast) and about two hundred kilometers from Colombo. Indika said that in the previous life he remembered he had been wealthy, had a large house, had owned estates, and had elephants. He further stated or implied that he had owned a car and that his house had electricity and a telephone. His family had none of these amenities. Altogether Indika stated about thirty details concerning the life and circumstances of the person he claimed to have been. He did not, however, mention any names of people or places other than the city, Matara, where he said he had lived and the name of a person, Premadasa, who, from the way Indika talked about him, seemed to be a servant. This lack of proper names in the subject's statements and the great distance between Gonapola and Matara prevented his father from attempting verification in Matara of what Indika was saying. Indika's father had learned from persons who knew something about Matara about a deceased man who seemed to correspond to Indika's statements, but he had not verified the statements in detail nor taken Indika to Matara. I first learned about the case early in 1968 and began its investigation in March of that year. I made a written record of all Indika's statements about the previous life as well as about certain unusual behaviors which he showed and which harmonized with his claim to have been a wealthy man in that life. I then arranged to take Indika and his father to Matara where we enquired about wealthy men whose lives might have corresponded with Indika's statements.

Since Indika had mentioned owning elephants and since only a few wealthy persons own elephants in Ceylon, the search was confined to private owners of elephants in Matara. All but two of Indika's thirty statements applied to a wealthy lumber merchant of Matara called K. G. J. Weerasinghe. He had owned elephants and had a large house with electricity and a telephone. He had had a servant, Premadasa. The description of his character obtained from two of his nephews and his adopted daughter accorded well with the unusual behavior of Indika which seemed related to his memories of the previous life. Despite the concordance of Indika's statements and behavior with what was learned about K. G. J. Weerasinghe, the lives and circumstances of other owners of elephants in Matara were examined in an effort to see whether any of their lives fitted Indika's statements as well as or better than that of K. G. J. Weerasinghe. It

was found that fourteen of Indika's statements also applied correctly to the life and circumstances of another wealthy man of Matara (who had owned elephants), but the remainder did not. This man also had no servant named Premadasa. Indika's statements did not correspond with the circumstances of the several other known elephant owners in the Matara area. It was concluded that he was referring to the life of K. G. J. Weerasinghe and no one else. Indika's family had passed through Matara on several occasions, but they had no friends or relatives there and no occasion for stopping there. (Matara is on the way from Colombo to Kataragama, a well-known place of pilgrimage.) Extensive inquiries failed to turn up any evidence that the two families had had any acquaintance with each other before the development of the case.

The subject of the second case, Sujith Lakmal, was born in Mt. Lavinia, a suburb of Colombo, on August 7, 1969. When he was about two and a half years old he began talking about a previous life which he said he had lived in Gorakana, a town about twelve kilometers south of Mt. Lavinia. Sujith narrated a great many details of the previous life he claimed to remember including a considerable number of proper names, e.g., that of the father, Jamis, he claimed to have had and a monk, Amita, he said he had known. He included in his statements some quite specific details such as that Jamis had had a damaged right eye. He also described how, in the previous life, he was knocked down and killed by a truck when he was drunk. Sujith's maternal great uncle, who was a monk at a nearby temple, heard about his statements and mentioned them to a younger monk of the temple. This monk, the Ven. Watarapolla Nandaratana, then met Sujith, his mother, and his maternal grandmother and made a dated written record of what Sujith was saying. The Ven. Watarapolla Nandaratana then went to Gorakana and, with some difficulty, eventually traced a family corresponding to Sujith's statements. A member of this family, Sammy Fernando, had been killed on January 29, 1969, when struck by a truck as he (being drunk) stepped out of a shop onto the highway. His father, Jamis, had had a diseased right eye. Nearly all the other statements made by Sujith were correct for the life and circumstances of Sammy Fernando.

In March 1973 I investigated the case with interviews of members of both families concerned and also with the Ven. Watarapolla Nandaratana concerning his interviews and

the records he had made as he conducted them. Enquiries showed no evidence that the two families concerned had had any acquaintance prior to the development of the case. Cases of this type can hardly be valued too highly, and special efforts are being made now to increase their number. It is hoped that if a larger number of them can be found a useful comparison can be made between their characteristics and those of the more numerous cases in which the two families have met before the investigation begins. If it then turns out that the cases with written records made before verification have in the main the same features as those in which the two families concerned meet before an investigation this observation will strengthen confidence in the authenticity of those cases in which investigators come to the case only after the families have met. On the other hand, if the latter type of case is found often to contain features not found in those with written records made before verification this will suggest some distortion or embellishment of these cases resulting perhaps from a blending of memories of what the subject actually said prior to verification and of the details of the life of the identified previous personality.

## PSYCHOKINESIS\*

THE ROTATING BEAM THEORY  
AND THE OLIVE HILL POLTERGEIST

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

In our report at the 1972 Convention of the Parapsychological Association, "Radial and Tangential Forces in the Miami Poltergeist," we suggested a theory for the Miami disturbances. Our analysis showed that close to the agent, object movements tended to be short, radial, clockwise, outward and clustered in an area behind the agent and to his right. Far from the agent, object movements were long, tangential, counterclockwise, inward and focused at his left front. All correlation coefficients between pairs of the five variables involved were significant. These characteristics could be explained in terms of a beam of energy produced in or near the agent's body by two transmitters which operate in and out of phase and thus cause the beam to change size and direction (in a way similar to phased-array radar). We suggested that object movements would increase in length with distance from the agent if the object were captured by a rotating beam and moved along its arc. The Olive Hill disturbances took place from mid-November to mid-December, 1968. They were investigated by John P. Stump and W.G.R. and were described by us at the 1969 Parapsychological Association Convention. The distances travelled by the objects in relation to the agent's location were recorded on a chart prior to and independently of the development of the rotating beam theory. The Olive Hill disturbances should therefore provide an independent test of this theory. However, they differed in some respects from the Miami disturbances; this prevented a simple

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\*Chairman: Gertrude Schmeidler, City College, CUNY.



comparison.

In Miami nearly all the disturbances occurred in a large warehouse room and involved merchandise such as glasses, ashtrays and so on. There were no disturbances of the furnishings of the room and there were relatively few movements of heavy objects such as cartons of merchandise. The Olive Hill disturbances were spread over three houses and included several movements of pieces of furniture in addition to small household objects. On several occasions in Miami, designated target objects moved. At other times witnesses were in a position to satisfy themselves as to the genuineness of the event. In Olive Hill, we were not able to use the target object procedure. This deficiency, however, was compensated for by the circumstance that we observed several of the occurrences at first hand and that there had been other witnesses to other incidents.

For our analysis of the Miami disturbances, these were divided into two groups according to the extent to which the movements had been supervised. The first group consisted of 20 "fully supervised events" and the second group of 16 "partially supervised events." We made a similar distinction between the incidents in Olive Hill with the exception that movements involving pieces of furniture were placed in a separate group. There were thus three groups. Group I consisted of 15 small household objects whose movements had been observed by the investigators or others and which apparently could not be attributed to familiar explanations, such as fraud. This group corresponded to the "fully supervised events" in Miami. Group II comprised 16 unwitnessed occurrences, also involving small objects; and Group III involved the movements of 13 pieces of furniture, some of which had been witnessed and others not.

In each of the incidents there was a record of the kind of object which was disturbed, the distance of the origin of movement from the agent ( $d$ ), the length of movement travelled by the object ( $l$ ), and the direction ( $\theta$ ) in which the agent was facing. On the basis of this, it was also possible to know the tangential ( $\theta$ ) and radial ( $r$ ) component of the movements. As before, when the movement was outward in relation to the agent,  $r$  received a positive value and when the movement was inward in relation to the agent,  $r$  was negative.  $\theta$  was positive for counterclockwise movements and negative for clockwise. For movements which originated behind the agent,  $\theta$  was positive,  $+1^\circ$  being at

the agent's left and  $+179^\circ$  on his right. For events in front of the agent,  $\theta$  was negative,  $-1^\circ$  being on his left and  $-179^\circ$  on his right. Correlation coefficients were calculated for each pair of the five sets of data in each group. In Group I there was a significant negative correlation between  $d$  and  $\theta$  and a significant positive correlation between  $d$  and  $l$ .  $\theta$  and  $l$  showed a negative relationship. The other correlations were insignificant. In other words, movements close to the agent tended to be short, counterclockwise and to his right. Movements far from the agent tended to be long, clockwise and to his left. In Group II there were significant positive correlations between  $\theta$  and  $r$ , and between  $r$  and  $l$ . That is, the long and short movements were both clockwise and pointed outwards. In Group III there were significant negative correlations between  $\theta$  and  $r$ , and between  $r$  and  $l$ . The short movements were clockwise and pointed outwards and the long movements were counterclockwise and pointed inwards, towards the agent.

The characteristics of the movements in Group I were similar to those in Miami. In both cases the short movements were close to the agent and to his right, while the long movements were more distant and to his left. The directions of movements were different in the two cases: in Olive Hill the long movements tended to be clockwise and the short movements counterclockwise, while the opposite had been true for Miami. But this is still consistent with the changing beam model. The movements in Group II do not, however, fit our model, while the Group III movements can be accounted for but not as clearly as those in Group I.

We were surprised at the difference between Groups I and II since they both dealt with small objects. One possible explanation is that the Group II objects were interfered with fraudulently. There was no evidence, either direct or indirect, of fraud in this case. Nevertheless fraud is a possibility for unsupervised movements of objects. Inspection of the data shows that in Group I, 13 of the 15 incidents occurred at the back of the agent while in Group II, only four of the 16 incidents were at his back. This difference probably resulted from the criterion according to which events were placed in either group: As a rule it was easier for witnesses to satisfy themselves that the agent could not have caused events fraudulently which occurred at his back and as a result, a preponderance of such movements may have appeared in Group I. The opposite was the case for the Group II incidents which occurred at the agent's

front and might therefore more easily have been caused fraudulently. There were other differences between the two groups of data which might obscure shared characteristics. In Group I, the agent was either standing or seated at the time of the occurrences, while five of the incidents in Group II took place when he was lying down. In plotting the movements in relation to the agent's position, no distinctions had been made between these three positions and this may have introduced an error into the figures. Another type of error may have entered the Group III figures. Four of the 13 pieces of furniture turned upside down or fell over in addition to moving a certain distance from their original positions while a fifth piece of furniture described a rotating motion. Again, these features were ignored on the original chart from which the calculations were made and may again have clouded the picture.

With respect to the Group I incidents on which our interest was focused, in the Olive Hill case as in Miami, the correlation coefficients between the various aspects of the movements suggest a lawful process and are consistent with the rotating beam hypothesis. In Olive Hill as in Miami the number of occurrences decreased exponentially as the distance from the agent increased. This recurrent pattern is another indication that we are dealing with a lawful process, since other natural forms of energy decay exponentially as source distance is increased.

#### COMPARISON OF PK ACTION ON TWO DIFFERENT RANDOM NUMBER GENERATORS

Helmut Schmidt (Institute for Parapsychology, FRNM)

PK action as a mental influence on the outcome of random events has appeared to be surprisingly independent of the physical nature of the underlying random process and the structure of the randomizer. Tests with dice and with electronic random generators, for example, have shown PK effects of the same order of magnitude. The experiments reported here, comprising a pilot and three confirmatory series, aimed at a quantitative comparison of the PK actions on two different binary random generators, a "simple generator" and a "complex generator." In order to have sens-

orally identical test conditions, for each trial one of the two generators was randomly selected to activate the feedback display on which the subject concentrated, and neither the experimenter nor the subject knew which generator was in action.

The simple generator reached a binary decision (+1 or -1) by stopping a fast-running modulo-2-counter at a random time. The decision of the complex generator was obtained by activating a simple generator 100 times in fast succession (which takes three seconds) and counting the majority decision of the 100 individual events as one binary event. (In the case of a tie, no output was generated.) The complex generator had been used in another experiment reported at the 1972 Parapsychological Association Convention. There, each individual event was displayed to the subject by a fast feedback device, whereas in the present experiment the feedback indicated only the majority decision of blocks of 100 events, and the individual events were not even recorded.

The two generators were stationed in a remote room, well separated from the experimental room in which the subject and experimenter were located. The subject initiated each trial by pressing a switch, which triggered both generators. Three seconds later each generator had produced a binary number. For each trial, however, only one generator, the "active" one, was connected to the feedback display in the experimental room. Depending on the generation of a +1 or a -1 by the active generator, one of two colored lamps was lit in the experimental room. The subject tried to enforce the lighting of a prespecified target lamp. In half of the sessions the target lamp was activated by the generation of a +1 and in the other half by the generation of a -1. The decision which of the two generators was active in a particular trial was randomly determined by a pre-recorded random number tape so that neither experimenter nor subject was aware of it. In the pilot and first confirmatory series the subjects were aware of my intention to compare two generators; in the second and third confirmatory series the subjects did not even know that there were two different generators.

The subjects in the pilot test and in the first and second confirmatory series were members of the laboratory staff and others who had performed well in previous PK tests. The subjects in the third confirmatory series were

selected from visitors to the laboratory by a pretest using a similar setup where, however, the simple generator was active throughout. Subjects with a non-negative pretest score who expressed a liking for the test situation were chosen. Thirty-five different subjects participated in the confirmatory series; three of these had participated in the pilot test. The length of the pilot test and of each of the confirmatory series was set at approximately 1000 trials. In the pilot test, four subjects (of which I was one) did widely varying numbers of trials. I contributed the majority of them. In the first confirmatory series five subjects, including myself, contributed approximately 200 trials each; in the second confirmatory series ten subjects did approximately 100 trials each, and in the third confirmatory series 20 subjects did approximately 50 trials each. Each subject made his contribution in one or several sessions. Breaks were taken whenever the subject seemed to lose interest. The precise number of trials made by each subject depended somewhat on external conditions. At the time the sessions with one subject were terminated the experimenter knew only the subject's total score, but not the separate scores obtained on the two generators.

The results may be evaluated in terms of three questions. (1) Is there evidence for PK acting under both conditions? For the pilot series, the simple generator produced a 58.0 percent scoring rate (where chance was 50 percent) with an associated significant CR of 3.66. The complex generator produced a 51.2 percent scoring rate with an insignificant CR of 0.54. In the combined confirmatory series, however, both generators showed significant scoring. The simple generator had 55.3 percent hits (CR = 4.4), and the complex generator had 53.8 percent hits (CR = 3.0). Therefore we can conclude that a significant PK effect was observed under both conditions. (2) Is one of the two generators generally easier to affect in the desired direction than the other? We can measure this effect in terms of the CRd, since each session contained many trials, randomly mixed, of each type. For the pilot test, the simple generator showed marginally significant higher scoring than the complex generator (CRd = 2.2); for the combined confirmatory series, the simple generator was only slightly higher (CRd = 1.0). Thus the suggestion of a differential effect in the pilot test was not borne out significantly in the confirmatory series. (3) Do some subjects score significantly differently on the two generators? This can be measured by a chi square test, evaluating the sum

of the squared CRd values for the individual subjects. In the pilot series this sum was 10.4 ( $df = 4$ ;  $P = .04$ ), suggesting an individual differential effect. The combined confirmatory series, once again, failed to repeat this finding ( $\chi^2 = 30.5$ ,  $df = 35$ ; nonsignificant). The highest CRd value was obtained by me in the pilot test ( $CRd = 2.4$ ), but I showed no differential scoring in the first confirmatory series. Only two other subjects obtained CRd values above 2. Two further tests, the sign test and the Wilcoxon test, did not suggest any difference between the two test conditions.

Thus, under sensorially identical conditions, PK effects with both generators were observed, and no statistically significant difference appeared in the two scoring rates. These results may stimulate a systematic study of the following "equivalence hypothesis": if we have two systems which generate random signals, and if these signals are in the absence of PK statistically equivalent (i. e., indistinguishable), then PK affects the two systems to the same degree, provided we use sensorially equivalent test conditions.

## ANIMAL PSI\*

THE USE OF AN ACTIVITY WHEEL BASED TESTING CAGE  
IN SMALL RODENT PRECOGNITION WORK

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(Institute for Parapsychology, FRNM)

Precognition experiments using small rodents as subjects have been done in laboratories in France, the Netherlands, and the United States, all following the same basic paradigm. In all three laboratories the animals have shown success at anticipating which half of a Skinner-box-like testing cage would receive a stimulus, when the stimulus was randomly delivered to one side of the cage or the other. In the procedure which we have evolved for the American work, the animal crosses a low barrier at will between the two halves of the cage, one side of which receives an aversive electrical stimulus during the last five seconds of each 60-second trial. The precognition measure is his ability to avoid the side which is stimulated.

The important features of the research are the use of a criterion for selecting a special class of trials called random behavior trials (RBTs), and the finding of relationships between various aspects of the animal's behavior and his psi scoring. RBTs are trials in which the animal crosses the barrier one or more times during the prestimulus period, and not within five seconds after a previous trial on which the aversive stimulus was delivered. (An animal must be in one side and not in the opposite side for one second to establish his position in that side.) These crossings are presumed to be cases where behavioral reasons such as a side habit, the tendency to remain in one place in the cage, or a movement in direct response to the stimu-

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

lus play a weaker role in motivating him to cross in comparison with the animal's psi expression. The RBTs have consistently shown higher scoring rates than the other trials. The relationships we found between scoring and other aspects of the animal's behavior suggested that low stress states or more natural conditions improved psi scoring. In particular, animals which had not received the stimulus on the previous trial ("non-shock") scored better than those which had received it, and animals which jumped over the barrier only once or twice during the prestimulus period ("low-jump") scored better than those which jumped three or more times.

Two drawbacks to the test have been the fairly slow rate at which the trials occurred, about one per minute, and the limited amount of information that could be derived about the animal's activity in the Skinner-box type testing cage with present monitoring systems. The low scoring rates we typically obtain, averaging only three to ten percent above the 50 percent mean chance expectation, require that for a significant experiment about 48 hours of testing be carried out. For comparison studies to examine variables that show only a few percent scoring difference between conditions, difficult and lengthy tests must be done requiring perhaps a week or two. To speed the pace and allow more complex studies to be done, as well as to attempt a design that provides more information about the animal's activity during the test, an activity wheel was incorporated as the testing cage. This 11-inch diameter, 4-inch wide wheel is divided so that one-half of its perimeter constitutes side A, and the other half side B of the cage. A low barrier separates sides A and B at two places on opposite sides of the perimeter. The entire perimeter is a grid. In running along the circumference, the animal typically crosses from side A to side B (and vice versa) several times a minute. This faster rate of crossing means a faster rate of RBTs (defined as before), but we wanted to make sure that this faster rate was achieved in a way which preserved the original significance of RBTs.

After preliminary testing for these and other considerations related to automation, we established a design in which a trial occurred every five to ten seconds, while RBT percentage, scoring rate, and behavioral scoring relationships remained approximately the same as with the box design. The animal runs on the wheel, crossing from one side to the other over the low barrier. In this design the position of the wheel, rather than the animal's position, is



monitored, and the stimulus is sent for one second to the entire wheel, rather than only to the side that the animal is in. Thus for each trial during the run a random number generator (an electronic device with a radioactive source to produce a random determination of one of two channels) selects either side A or side B of the cage. If the animal is in that side, a stimulus is sent for one second to the whole cage. The brevity of the stimulus prevents the animal from being stimulated on the wrong side so that, from the animal's point of view, a stimulus is sent only if he is in the side chosen and then only to that side. The entire experiment is carried out, including the changing of animals, operation of the test, and scoring of the data, by a PDP-11/20 computer located with the random generator on the floor below. The computer also carries out randomness checks on the generator during the run and is programmed to stop the experiment and call the operator in the event of a malfunction or problem of a predetermined type. We feel that the design offers the advantage of intentionally allowing subtler variation in the animal's behavior to result in RBTs so that, by increasing or decreasing his speed, he can make different guesses at the time of the trial. This is in contrast with the box, where he must make a discrete change from one side to the other at the time of the trial. At the same time, enough transition is required to preserve the separation of the two sides. In the formal experimentation which followed the refinement of the design, all experimental parameters were preset and the number of animals was approximately one-half the total number of runs for any given series. Each animal, in accordance with previous work, was tested once a day to once every other day. Home environments and handling procedures were also similar to those in our previous work. The animals used in these studies were all Golden Hamsters (Cricetus cricetus).

After successful preliminary experiments, a pilot was carried out composed of 50 runs with one trial every ten seconds, 26 trials per run. As in prior work, the first trial was used only to establish equal conditions for the succeeding ones, giving 25 scored trials per run. The results of this study gave significant evidence of ESP, with a 57 percent scoring rate for RBTs ( $CR = 3.22$ ). The behavioral subcategories of non-shock and low-jump gave increased scoring rates similar to those seen in previous work with the box-type testing cage. A confirmation was then done in which the trial time was shortened to five seconds and the run length during the second half of the test was increased

to 41 trials. In this experiment there were 25 runs in the first half and 25 runs in the second half. The overall study showed significant evidence of ESP (CR = 3.10 for RBTs), as did each of the halves (CR = 2.35 first half, CR = 2.05 second half for RBTs). The behavioral subcategories were also significant with the exception of low-jump trials in the second half. It is important to note that the scoring rates were essentially the same when the trial time was halved and when the run was lengthened. This would allow an experiment which previously took 48 hours to take place in four, and some studies which previously took seven days to occur in about 12 hours.

Another useful evaluation of the significance of the test is to look at the mean percent scoring rate averaged across the individual animals (the mean of each animal's mean score) in the preliminary, pilot, and confirmation studies and test it against the chance mean by *t* test. This also gives a high level of significance for the RBTs and the subcategories, and indicates significant performance by the animals as a group. It is important to note that although the category of total trials is significant ( $P < .01$ ) in this analysis, it is because of the large percentage of RBTs it contains; the non-RBTs are not significant. (In general in these series, total trials were not significant.) In summary, this paper presents an improved design for the small rodent precognition work that allows for more efficiency in testing, more sophisticated studies, and a testing cage that may offer different opportunities for the evaluation of the animal's psi performance. The pilot and confirmation experiments, as well as the preliminary work, give significant evidence for ESP both in the RBT data and in the behavioral subcategories, and in addition indicate that the animals as a group contributed to these effects. We would hope to be able to use the information about behavioral relationships to refine our procedure for selecting and breeding animals so as to improve their performance and stability. This may not involve selection or breeding of psi at all; such a conclusion could not be drawn at this time. But from these results some tentative support emerges for the selection of behavior factors that facilitate psi expression. Hopefully, this design will allow investigations of more complex questions and different relationships of the animal's behavior to scoring than was true with the box. Yet it apparently preserves continuity with the box studies with regard to the behaviorally related psi scoring variables examined in this research.

PRECOGNITION IN RATS AS A FUNCTION OF SHOCK  
AND DEATH

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Waterloo, Ontario)

In a precognition experiment reported in 1967, Morris exposed 19 rats to an open field situation where he measured their activity over a number of minutes. Subsequently, he randomly killed half the rats while letting the other half live. Morris found that the rats who faced imminent death were less active than the rats whose deaths were delayed for some time. He hypothesized that the lesser activity of the rats in the imminent death group was due to their natural tendency to freeze when presented with a fear-producing stimulus. The following experiment was carried out in order to follow up on and extend Morris's findings. This was done by varying time of future death as well as time of a future shock treatment. The subjects were 72 experimentally naive, female, adult hooded rats of the Royal Victoria Hospital strain which were born and raised in the laboratory. The animals were housed individually for at least a month before the beginning of the experiment. Food and water were provided ad lib. The open field apparatus consisted of a 32-by-32 inch floor surrounded by 24-inch-high walls. The floor was made of metal rods spaced half an inch apart. These rods were raised three inches above a wooden sheet divided with ink lines into 16 eight-by-eight inch squares. Each rat was started in the same corner of the open field by being placed in a topless and bottomless L-shaped enclosure which served as a start box.

The procedure involved two testing periods separated by 24 hours. On the evening of Thursday, March 2, 1972, the 72 rats were randomly divided into two Day groups. One group was tested the same night, the other group the next night. Within each group running order was randomly determined. The procedure was the same for both evenings, and all rats were tested individually. Each rat was put into the start box. After a few seconds the start box was removed and the rat's behavior observed for the next three minutes. Two measures were recorded: latency and activity. Latency represented the number of seconds from the time the start box was removed to the time the rat's nose crossed a line of an open field square when it moved in a forward direction. Activity was measured by counting the

number of times a rat's nose crossed a line of an open field square, again when it moved in a forward direction. Immediately after the second group of rats had been run in the open field, half of the rats in each of the two Day groups were randomly assigned to the Shock condition, and half to the Non-Shock condition. Each rat in each of the four resulting groups was randomly assigned a running order position, placed individually in the start box, and released. After 30 seconds, the rats in the two Shock groups were administered a current of one milliamperere for one second through the grid floor, while the rats in the two Non-Shock groups did not receive the current. During this 30-second period the number of line crossings was recorded. The experimenter handling the rats was blind as to which animals were to be shocked and which were not to be shocked.

After all the rats had been placed individually in the open field for 30 seconds, half receiving shock and the other half not, each of the four groups was then randomly divided into an Imminent group and a Delay group. The four Imminent groups were terminated with chloroform within an hour after the last 30-second period, while the four Delay groups were maintained on food and water ad lib. for at least three weeks. Analyses of variance were performed on three dependent measures: (1) latency in seconds during the first exposure to the open field; (2) line crossings per minute for three minutes during the first exposure to the open field; and (3) line crossings during the 30 seconds of the second exposure to the open field. The factors tested were Day 1 versus Day 2, Shock versus Non-Shock, and Imminent versus Delay Death.

The analyses of variance performed on latency and on line crossings for the 30-second period revealed no significant effects. However, the analysis of variance performed on line crossings during the first open field exposure did reveal significant differences. The main effects of Days and Death were highly significant, whereas Shock was not. The significant Days effect ( $F = 30.72$ ,  $df = 1, 64$ ;  $P < .001$ ) reflected a greater number of line crossings on Day 1 than on Day 2. On Day 1, the rats crossed a mean number of 16.9 lines per minute, while on Day 2 they crossed a mean number of 11.8 lines per minute. However, this difference in line crossings between the two days did not interact significantly with the effects of either of the two precognition independent variables of Shock and Death, and thus did not appear to be psi-related. The significant Death main effect

( $F = 8.81$ ,  $df = 1$ ,  $64$ ;  $P < .005$ ) reflected a greater number of line crossings per minute by the Imminent group than by the Delay group. The rats in the Imminent group crossed a mean number of 15.7 lines per minute, while the Delay group crossed a mean number of 13.0 lines per minute. A matrix of correlations was computed among the measures of latency, activity during each of the three one-minute periods, and line crossings during the 30-second period. Nine of the ten correlations were significant at  $P < .05$  (two-tailed), while the tenth was close to significance ( $P < .10$ , two-tailed). In general, these correlations revealed a negative association between latency and the four activity measures, and a positive association among the four activity measures themselves.

We can conclude that precognition in rats was demonstrated in this experiment. When exposed to an open field situation, rats were significantly more active when killed either one or 24 hours later than when killed a minimum of three weeks later. Furthermore, future shock had no significant effect on either the latency or activity measures. Activity changes in response to aversive stimuli presented in the future do not resemble those in response to aversive stimuli presented in the past. These findings appear to disconfirm Morris's hypothesis that rats presented with an aversive event in the near future will be less active than rats presented with an aversive event at some later date. It is difficult to account for these contradictory results, as the procedures used in the two studies were similar. At least three explanations are possible. First, because the rats came from different populations, their response to the Death condition may have been different. If this explanation is correct, the generalization of results between and within animal experiments would be highly restricted. A second explanation may be that the differences are due to the fact that the experimenters were different. This is a likely possibility when one considers the results of a previous experiment by J.G.C. in which the two experimenters involved produced significantly opposite results. A third explanation is more speculative. Previous unpublished work suggests that the phase of the moon may be associated with quite opposite effects. Whatever the explanation or explanations, it is now clear that time of death does have an effect on an animal's behavior. Our own experiments, along with those of Morris and Bestall, clearly demonstrate this.

These results have obvious implications for any re-

searcher working with animals either within parapsychology or without. At present, animal researchers generally sacrifice their animals when they are no longer experimentally useful. If these animals are sacrificed at different times after an experiment, the time of death may affect the outcome of the previous experiment without the researcher's being aware of it. Because time of death is not considered a factor in animal research, it is seldom if ever reported. What effect time of death has on the results of animal experiments in the medical and behavioral sciences may be a critical question. Future research in animal psi may reveal just how critical this factor is.

#### POSSIBLE PK BY RATS TO RECEIVE PLEASURABLE BRAIN STIMULATION

Walter J. Levy (Institute for Parapsychology, FRNM)

Previous work with chickens, cockroaches, and cats done by Helmut Schmidt and myself has suggested that significant PK can be exerted on random generators in test conditions where animals are the apparent subjects. This work, together with a need for and an interest in neurophysiological tests of parapsychological questions, motivated the present study. I wanted to assess in a preliminary way the feasibility of an animal model that would allow investigation of neurophysiological and physiological psychology parameters of the central nervous system in tests for ESP and PK.

The general design is as follows. Unipolar or bipolar electrodes are implanted in the brains of Sprague Dawley rats. The sites selected are "pleasure centers" for the stimulation of which an animal will perform a task, such as bar-pressing, quite frequently. (In our research we use a foot switch rather than a standard Skinner-box bar, since it requires less initial training of the animal.) With successful implantation the animal will perform the task as often as he can in preference to seeking other forms of reward such as food, water, and sleep. The sites used are the medial forebrain bundle and septal region; each animal has four electrodes implanted (one on the left and one on the right side for each of the two sites). Implantation is judged to be successful if the animal trips the foot switch for the

pleasure reward to a much greater extent when the electrodes are hooked up to the stimulator than when they are not. When the electrodes are hooked up, the animals reach moderate self-stimulation levels of approximately 200 per hour. With no stimulation the switch is tripped by the animal only five to ten times per hour. Intensity of stimulation is individually adjusted for each site in each animal to give a high foot-switch response rate.

The test apparatus consists of a lucite cage 14 inches to a side with a mercury commutator at its top in the center. This device allows a cable with a socket that attaches to the implanted electrodes in the animal's head to turn without twisting the incoming wires from a Grass model S88 stimulator and monitoring equipment. In the training section of the test, following implantation, the foot switch is placed in the cage so that the animal can receive self-stimulation from the stimulator by tripping the switch. During the parapsychological section of the test (after the animal has reached the behavioral criterion for self-stimulation) the switch is removed and, outside the test room, a random generator is substituted in its place in the circuit with the stimulator. This is an electronic device based on a radioactive source, and was developed by Helmut Schmidt. It randomly chooses one of two channels to be activated on each trial. If the generator chooses channel one on a particular trial, the stimulator fires, providing a pleasurable impulse to the animal. If channel two is chosen, no stimulus is sent to the animal. Thus, following the example of the animal's self-stimulation behavior with the foot switch, if he can use PK to cause the random generator to choose channel one more frequently than channel two, he can increase his rate of pleasurable stimulation.

The random generator is checked for randomness periodically, particularly for side bias, which is the possible problem most pertinent to this study. The sample size used in such checks is approximately four times the length of an experiment, thus allowing us to check for biases more subtle than those which would produce significance in an experimental unit. In addition, during an experiment, channel one and channel two serve as target (that is, provide pleasurable stimulation when chosen) an equal number of times, since the target channel is alternated between the two every run. This means that, even though no side bias has been observed in the randomness tests so far, any bias that is present should cancel out because of the reversal of

the channels. One pilot and two confirmatory experiments have been completed so far, all yielding significant above-chance PK scoring. In the pilot study 30-second trial intervals were used, providing a random stimulation rate slightly less than the behavioral self-stimulation rate for the animal. The length of the study was preset at 1000 trials, divided into five runs of 200 trials each. The overall results gave a scoring rate of 59 percent (590 hits;  $CR = 5.69$ ), giving initial significant evidence for PK on the random generator. A single animal was used in this test.

A confirmation experiment was then carried out using three animals, again in runs of 200 trials each using a 30-second trial interval. Each animal did one run on each of two consecutive days. This 1200-trial study gave 648 hits for a significant scoring rate of 54 percent ( $CR = 2.77$ ). No reason for the lower scoring rate is presently known, but at this early stage in the research, considering the number of variables operating, such variations are not surprising. A further study was then undertaken consisting of two parts. Four animals were used, each doing both parts of the experiment. The first part used 30-second trial intervals and was an effort to correlate foot-switch tripping rate of the individual animals in the training section of the test with scoring rate in the parapsychological section. The foot-switch tripping rates for each animal showed too much variance and overlap for the correlation to be meaningful, but the animals did show a significant PK score (455 hits out of 823 trials for a 55.3 percent scoring rate;  $CR = 4.25$ ). The odd number of trials results from a variation in the procedure introduced in this series: the experiment, instead of having a preset number of trials, is ended at the first entry of an experimenter into the room after a fixed time interval (one hour, defined as one run). This made scoring easier but was no less tight than the previous method, since the results could not be known until the experimenter had entered the room at the end of the one-hour run. The number of runs was preset.

The same procedure was used in the second part of the study. Here the trial interval was speeded up to seven seconds, so that the random generator made a choice for channel one or channel two every seven seconds. This gave 2943 trials with 1578 hits (53.6 percent scoring rate;  $CR = 3.93$ ). One possible benefit of this approach to PK testing is to demonstrate the possibility of a relatively high scoring rate with a very fast trial time, and the increase by a factor



of four in the trial speed was a step towards that. The scoring rate was somewhat lower; this is not a significant difference, so no conclusion can be drawn. The psi efficiency, as measured by the CR squared per hour, is .715 in the seven-second trial part of the study as opposed to .425 in the 30-second trial part. So the level of evidence for PK maintained itself despite the increase in speed and there was even a suggestion of an increase in efficiency.

This set of studies primarily establishes the existence of an effect which can best be explained by PK, and it offers the possibility of a test situation sufficiently stable to allow an investigation of physiological psychology questions which previously were more difficult, and in some cases impossible, to study. One example of such an investigation would be to vary the amount of information input into the central nervous system by using such drugs as curare, and observe the scoring effects. This could test an often-proposed model of psi as a weak signal made difficult to recognize by noise from other information coming into the central nervous system. Many such studies are most efficiently done, and in some cases are only ethically done, with animals. Further exploration of the relationship between motivational level and psi scoring level is another possibility, and so is pharmacological work. A basic issue here is the role of the experimenter, as in all of the animal studies, particularly those using a PK paradigm. Since this test presents only overall significance no conclusion can be drawn about the role of individual animals nor, more importantly, about the role of variables related to scoring which would be associated with the animals rather than the experimenter. The issue of possible experimenter effect is one for investigation in the future. But if this study parallels others recently done with animals, we may well find a relationship between internal factors pertinent to and known to the animal, but not to the experimenter, and scoring; this relationship would hopefully be of a sort that is significant, stable, and which, when modified, modifies the scoring. This, together with an attempt to provide a high degree of replicability of the results, seems to be the best way at present to approach the experimenter effect issue.

## ALTERED STATES OF CONSCIOUSNESS\*

## HYPEREMPIRIA: HYPNOSIS AWAKENED

Don Gibbons (West Georgia College)

I have developed a trance induction procedure which is based on specific suggestions of increased awareness, mind expansion, and heightened alertness and sensitivity, in contrast to the usual hypnotic induction based on suggestions of sleep. It is called "hyperempiria," based on the ancient Greek term "empiria," meaning "experience," with the prefix "hyper" added. Provided the subject is sufficiently suggestible, the hyperempiric procedure may be accomplished about as easily as the hypnotic one. In the former, however, the subject may be more responsive to faint sensory or extrasensory cues than in the latter, since he is feeling more alert. A study on auditory thresholds which I presented last year at the Society for Clinical and Experimental Hypnosis indicated that subjects who had undergone a hyperempiric induction did manifest a significantly greater decrease in posttest auditory thresholds than did subjects who were administered either a hypnotic induction or no induction. In addition, I have done an informal pilot study using card-guessing at West Georgia College which indicates that possibly this might be true of psychic thresholds as well. No formal parapsychological experimentation has yet been done, however.

After the hyperempiric trance induction has been completed, further alterations in conscious experience may be suggested which are likely to facilitate the subject's ability to utilize whatever psychic abilities he may possess. One of the additional suggestions which may be given is that of transcendental experience, in which it is suggested to the

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\*Chairman: Charles Honorton, Maimonides Medical Center.

subject that he is entering a new dimension, beyond the boundaries of time and space, within which he is able to reach out and make contact with universal knowledge. I am not suggesting that the subject is actually doing this, but I think that the belief or the suggestion that he is doing this may provide an ideological framework or a conceptual rationale which may engender the confidence necessary to enable him to employ his own psychic abilities to a fuller extent. It may make him into a better sheep, in other words. Another kind of thing you can suggest is mystical experience, in which the subject is given the suggestion that he is feeling some positive emotion, such as joy or love, and that the intensity of this experience is going to continue to grow until he is able to hold within himself all of the creative force contained within the entire universe. This force may then be directed toward the task at hand, such as psychokinesis. A third possible state is oceanic feeling, in which it is suggested to the subject that the boundaries of the self are temporarily dissolving, and that his consciousness is becoming one with the deity or infinite mind, as a drop of water returning to the sea.

Now if hypnosis is essentially role-taking aptitude, as Ted Sarbin says, and if the cognitive determinants of hypnotizability include the ability to think along with and to imagine vividly the suggestions which you are given, as Ted Barber has stated, then I think it is possible to conceive of a host of roles which do not fit the traditional hypnotic paradigm, and which might be profitably investigated with regard to the enhancement of psychic abilities. It is obvious that, if you have been talking sleep to a subject for ten or fifteen minutes, he is bound to feel less responsive than he normally might be, if your suggestions have had any effect at all. And I know that hypnosis has been reliably shown to increase psychic abilities. But perhaps some other kind of trance induction, such as the hyperempiric one based on suggestions of alertness rather than sleep, might work even better. Attempts have been made to get around the problem of reduced responsiveness in traditional hypnosis by using hypnotic inductions omitting the word "sleep" or "hypnosis," or by trying to induce some kind of an "alert trance." But if you do only that, then the subject is going to think, "Oh yes, this is like hypnosis," or, "This is another way to hypnotize me." And the experience which results is going to represent some combination of the explicit suggestions of the hypnotist and the implicit suggestions conveyed by the subject's own pre-existing conceptions

of hypnosis as a state resembling sleep. If suggestions of alertness are provided after a hypnotic induction has been completed, they cannot be fully effective; for if they were, and no residual decrease in responsiveness remained, the result would be to convince the subject that he is no longer hypnotized. Therefore, I have called my proposed alternative procedure "hyperempiric" induction in order to disassociate it from any previous expectations which may function concomitantly as autosuggestions.

A lot of people are concerned about how many altered states of consciousness may actually exist. My own belief is that the experience of your own awareness is a subjective phenomenon. This is what behaviorists have been saying for a long time. And if that is true, then the number of altered states or altered experiences of consciousness which have been induced by suggestion is probably equal, or just about equal, to the number of definitions of an altered state that you can conceive of, because every one of these definitions can be written up in the form of an induction and suggested to a sufficiently responsive subject. The question to ask, therefore, is not how many altered states of consciousness may exist; that is like asking how many paintings it is possible to create. The question for parapsychologists to ask is how can we create the kind of state that is desirable for our goal, which is the enhancement of one's ability to utilize one's psychic potential.

My own experience with hyperempirica during the past two years using student volunteers at West Georgia College has convinced me that this procedure may be safely and effectively employed by investigators who possess sufficient training in the conduct of suggestion-induced trances, and who are willing to employ the usual safeguards in screening subjects for research in this particular area. If the contents of consciousness are a determinant of, or an influence upon, one's ability to exercise his psychic powers, I think this technique provides a means of influencing these variables directly, given sufficiently good subjects. Since there seems to have been a considerable amount of interest generated by hyperempirica, my purpose today is to make the technique available to investigators in the field of parapsychology in the hope that parallel investigations conducted simultaneously might facilitate the exploration of whatever potential the technique may possess. For more information about the procedure, see my recent book Beyond Hypnosis: Explorations in Hyperempirica.

RELAXATION AS A PSI-CONDUCTIVE STATE:  
A REPLICATION AND EXPLORATION OF PARAMETERS

Rex G. Stanford<sup>†</sup> (St. John's University) and Brantz Mayer  
(University of Virginia)

William G. and Lendell W. Braud [see page 11] have recently reported that volunteer subjects showed significant performance on a free-response ESP task after they had undergone a procedure combining progressive muscular relaxation, suggestions of mental and physical relaxation, and suggestions that the induced state of relaxation was optimal for ESP. The present study was designed to replicate the Brauds' work and to explore how subjects' reactions to the experimental conditions relate to ESP performance. The hypotheses were that there would occur overall, significant, positive ESP performance and that measures, during the experiment, of mental relaxation (i. e., mental stillness and quietude) would relate positively and significantly to ESP performance, on a between-subject basis. The other aspects of the study were exploratory.

Twenty-one volunteer women students from the University of Virginia were tested individually by B.M. before the experiment was stopped to allow data analysis for possible reporting at the Parapsychological Association Convention. The subject reclined in a large recliner chair and closed her eyes to listen and respond to a taped orientation, instructions, and relaxation procedure. The instructions stated that after the relaxation procedure there would be a five-minute period of silence on the tape during which the subject would spontaneously develop and observe mental images or impressions. These impressions would, she was told, likely relate to the picture inside an opaque container on the other side of the room from her. She was to note her impressions and encourage them to happen. The relaxation procedure was identical in all essential respects to that of the Brauds. Subjects were put through a modified form of Jacobson's progressive relaxation technique. Muscle groups were relaxed progressively from the feet to the head, accompanied by suggestions of deep relaxation. Then suggestions for mental relaxation and quietude were given. Finally, there were suggestions that the subject was in an optimal state for psychical function, that she would be able to still her mind, make it a blank, and would wait for impressions to develop which she would carefully

observe and remember perfectly afterward. There followed the five-minute period of silence on the tape. After the tape brought the subject out of the relaxed state, she was immediately given an 11-item questionnaire concerned largely with her experiences during the experiment. Each item asked a question which the subject was to answer by putting a slash through a ten-centimeter line to indicate her response as lying somewhere between descriptors placed at opposite ends of the line.

The target pool consisted of ten target sets, each set constituted by five magazine pictures. The pictures had been chosen because of their striking qualities and had been arranged such that the five pictures in each set were maximally different one from another. R.G.S. prepared the target arrangement for each trial, selecting both the set and the picture within the set on independent random bases. He was also responsible for concealing the target in the containers. Thus B.M., who worked with the subjects, did not know the identity of the target picture, which was enclosed in an aluminum foil envelope with an outer black cardboard folder enclosing it. While the subject filled out the questionnaire, B.M. took the container (contents still enclosed) to R.G.S.'s office and retired to the hall outside, closing the door behind him. R.G.S. then placed all five pictures (four control and one target) in random order. Following this, he left the room and handed the entire set to B.M. without comment. B.M. placed the pictures before the subject in this random order and asked her to rate each on a 31-point scale according to the similarity of the picture to her experience during the receptive period. Before making her ratings she read a detailed set of guidelines designed to help her become aware of the ways ESP material sometimes manifests and to avoid some of the pitfalls of free-response judging. When the subject had completed her ratings, B.M. called R.G.S. on the intercom to obtain the designation of the target picture so that the subject could have feedback.

The overall ESP results were evaluated in a manner as comparable as possible to that used by the Brauds. In our experiment we had the subjects (as their own judges) use a rating procedure so that we could do rather sensitive tests of ESP performance in relation to psychological predictor variables. We evaluated the number of times the subjects had given the target picture the top rating among the five pictures. Nine of the 21 subjects had given the target picture the top rating, but one of these was a tie, yielding

8.5 hits; mean chance expectation is 4.2 hits. The probability associated with this is significant ( $P < .03$ , one-tailed, evaluated on an exact binomial probability basis). Analyses in relation to predictor variables used an ESP score for each subject which consisted of her rating on the target picture expressed as a standard score, computed by subtracting the mean of her five ratings from her rating on the target picture and dividing the result by the standard deviation of her five ratings. Subjects' responses to the question, "How still and quiet was your mind just before you began the relaxation process?", were, as hypothesized, significantly and positively related to ESP performance ( $r = +.38$ ,  $df = 19$ ;  $P < .05$ , one-tailed). When subjects are divided at the median with respect to their responses on this question, the efficacy of this predictor variable is quite evident. The high mental relaxation group ( $N = 10$ ) produced ESP results significantly above chance ( $t = 3.05$ ,  $df = 9$ ;  $P < .008$ , one-tailed). The low mental relaxation group ( $N = 11$ ) scored insignificantly below mean chance expectation. The difference between the two groups is significant ( $t = 2.25$ ,  $df = 19$ ;  $P < .02$ , one-tailed).

Responses to the question regarding mental relaxation at the end of the relaxation exercise showed, as hypothesized, a positive ( $r = +.15$ ) correlation with ESP scores, but it is not significant. Nor does dichotomous breakdown on this predictor variable yield significance. Questions regarding physical relaxation (at the beginning and end of the relaxation period) were not useful as predictors, yielding correlations near zero with ESP scores. After the relaxation procedure almost all subjects rated themselves near the top of the scale on physical relaxation. Questions regarding how unusual an experience the subject had by the end of the relaxation period and how she would characterize her mental activity (structured, directed, rational versus spontaneous, intense, dreamlike, bizarre) during the time she tried to use her ESP, failed to relate to ESP performance. Three questionnaire items regarding expectancy of success in the ESP task (at the beginning of the relaxation period, at its end, and during impression development) also showed no relationships to ESP performance.

A question concerned with how positively subjects felt about being in the experiment as they came into the experiment correlated negatively with ESP performance ( $r = -.30$ ), but nonsignificantly. When subjects are dichotomized at the median with respect to their responses on this

item, the ten who rated their feelings most positively scored negatively, though not to a significant degree. The 11 who felt less positively scored positively to a significant degree ( $t = 4.50$ ,  $df = 10$ ;  $P < .0012$ , two-tailed). The difference between these two groups is significant ( $t = 3.60$ ,  $df = 19$ ;  $P < .002$ , two-tailed). A question regarding general mood on the day of the experiment correlated positively ( $r = +.21$ ) but nonsignificantly with ESP performance. The fact that this general mood question and the experiment-specific mood question showed significantly opposite ( $t = 2.75$ ,  $df = 18$ ;  $P < .015$ ) relationships with ESP performance suggested that these two items might not tap a common variable to the degree that might be supposed. Responses to the experiment-specific mood item become a valuable correlational predictor when the effects of general mood are partialled out. The partial correlation for experiment-specific mood and ESP performance with effects of general mood held constant is significant and accounts for more than 25 percent of the variability ( $r = -.52$ ,  $df = 18$ ;  $P < .02$ , two-tailed).

The overall outcome of this experiment confirms the Brauds' work. The positive relationship between ESP performance and mental stillness and quietude at the beginning of the relaxation procedure suggests that psychological relaxation plays an important role in the use of ESP in such free-response tasks. Nor can this interpretation be questioned simply because the same question referred to the end of the relaxation procedure failed to yield a significant positive relationship with ESP performance. It seems likely that subjects who were mentally relaxed when they came into the experiment became more relaxed thanks to the experimental manipulation and may have been able to maintain this mental quietude for a sufficient period of time to allow them to develop meaningful extrasensory images or impressions. On the other hand, subjects who came into the experiment less calm (and who, perhaps, tended to be chronically less calm) may temporarily have become more calm because of the relaxation procedure but may have quickly reverted to a less calm state when confronted with the necessity, following the relaxation procedure, of receiving impressions. Note also that the variability of mental relaxation scores is very significantly attenuated at the end of the relaxation period as contrasted with the beginning of that period ( $F = 3.49$ ,  $df = 20, 20$ ;  $P < .01$ ). This may partly account for the reduced correlation coefficient for ESP performance and mental relaxation at the end of the relaxation period.



The correlation of ESP performance with mental stillness and quietude accords with Rhea White's suggestion that stillness and calmness of mind are important in waiting to receive psychic impressions, and with Honorton's proposals regarding psi-conducive states. Because of the near-zero correlations of expectancy measures and ESP performance, we can rule out the possibility that the relationship of the relaxation measure to ESP scores is mediated by expectancy factors. The indication that subjects who felt sure they were in an excellent mood for the ESP test scored poorly (as compared with those less exuberant) accords with earlier evidence from forced-choice studies that subjects who view the ESP task through "rose-colored glasses" tend to do poorly.

## METHODOLOGY\*

## CONSISTENT MISSING IN ESP AND SP

E. F. Kelly<sup>†</sup> (Gardner Murphy Research Institute), H. Kanthamani (Institute for Parapsychology, FRNM), and I. L. Child (Yale University)

By "consistent missing" (henceforth referred to as CM) we mean any systematic tendencies for a subject to mix up particular calls and targets. The information pertinent to identifying such tendencies in a given subject and a given body of data may be concisely presented in the form of a "confusion matrix" whose rows represent calls and columns, targets. Each trial increments exactly one cell of this matrix. At the end of the experiment, the main diagonal contains all the direct hits; these may be evaluated in the usual way, summing over the diagonal and computing the CR for total hits in relation to total trials. The off-diagonal cells contain the information relevant to CM, each such cell representing one particular way of wrongly pairing a call with a target. Our initial problem is to develop means for determining whether a given confusion matrix contains overall evidence of systematic tendencies of this sort; and, if such tendencies exist, to identify and characterize them. Consistent missing appears to have been studied first by Cadoret and Pratt. They were interested in it primarily as another form of misplacement of psi, complementary to sequential displacements. Their first step was to develop a chi square-based method for detecting the presence of overall non-randomness in the off-diagonal cells, independently of the direct hitting represented in the main diagonal. Next they applied this measure to several previously collected bodies of data, and succeeded in showing that there was in fact a CM effect in some of them.

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\*Chairman: John Palmer, University of Virginia.

Timm pursued a rather different question. He was interested in checking up on a suggestion by Nash that CM might be a causal contributor to psi-missing. He therefore took one subject with a lot of psi-missing data, and showed that the subject did indeed manifest CM and that this was concentrated in the lowest-scoring runs. He also developed a method for testing statistically the stability across bodies of data of conspicuous portions of the overall CM pattern. Specifically, he divided his data chronologically into three segments, and constructed a confusion matrix for each segment; he then took the strongest confusion in each row of one matrix as his prediction for the strongest confusion in the corresponding row of the next, and supplied an approximate test for the observed tendency of the whole set of predicted confusions to occur.

The work which we will now describe carries the subject further in that we employed methods which systematically extract the complete pattern of missing, using more of the information in the confusion matrix. Also, we hoped to use CM information to get leverage on problems involving comparisons between bodies of data, in particular between ESP data and sensory (visual) perception data. A new family of statistical procedures known as multidimensional scaling techniques (henceforth referred to as MDS) allowed us to accomplish these two ends. In using these techniques we were assisted by Forrest W. Young of the L. L. Thurstone Psychometric Laboratory at the University of North Carolina, Chapel Hill. The nearest relative to MDS among the more familiar procedures is factor analysis. In both cases we start with a large matrix of data consisting of measures of correlation, proximity, similarity, or confusability of the row and column objects, plus an assumption that underlying the given matrix is some simpler structure which contains the essential information about the relationships of the objects, but in a more compact and usable form. In practice this means that some mathematical operation on the computed underlying structure succeeds in reproducing the original data matrix, up to some criterion of fidelity. In both cases the output may be thought of as a geometric structure. A set of factors, or scales, or dimensions is extracted, defining a kind of space; and the objects in the source matrix are assigned coordinates in that space, such that their geometric relations in the space reflect the significant features of their interlocking relationships in the original matrix. For example, relatively confusable objects will end up close together in the output space.

The main practical difference between factor analysis and MDS is that MDS makes much weaker assumptions about the properties of the input data. In particular, it assumes that the level of measurement is merely ordinal; thus it tries to reproduce not the source data itself, but that monotonic (order-preserving) transformation of the source data which optimizes the whole fitting process. This flexibility allows us to force the solution into lower dimensionality than factor analysis typically achieves, in particular into two- or three-dimensional spaces, which we can visualize. Note also that since we thereby estimate fewer parameters from the same data, the estimates we make are likely to be more reliable statistically. In short, the basic idea is to allow ourselves maximum opportunity to recover systematic simple structure from noisy data.

Operationally, MDS like factor analysis uses an iterative procedure which repeatedly adjusts candidate configurations until some criterion appears to be optimized. The particular MDS algorithm we have used works as follows to obtain a solution of specified dimensionality. For each candidate configuration we can compute proximities (or distances) between all pairs of points; we can also find that monotonic transformation of the original matrix which minimizes the "stress" for that configuration, where stress is defined as some function of the sum of squared discrepancies between the (transformed) input distances and the reproduced distances. Now the partial derivatives of the stress with respect to directions in the configuration tell us how to adjust the coordinates of the points for the next iteration, and we go on like this until we find that configuration and that transformation which jointly yield a global minimum for the stress. That configuration represents our best guess at the "true" structure (in the specified number of dimensions) underlying the original data matrix.

Given such structures, two consequences follow:

(1) the scaled output from a single confusion matrix may provide insight into the character of the mechanisms underlying that matrix, through the geometry of the scaled objects in the output space. For example, the mechanisms can be assumed to be relatively sensitive to some distinction between any two objects which end up widely separated in the output structure, and what that distinction is may be interpretable on the basis of what other objects get clumped with each. Problems of interpretation in MDS are quite analogous to those in factor analysis. (2) Furthermore, it

becomes possible to investigate directly the hypothesis that a common set of mechanisms underlies two different confusion matrices. This can be done both visually in terms of the geometry of the respective scaled outputs and formally in terms of a variety of goodness-of-fit criteria proposed in the literature. The one we have used most heavily takes one output structure as a target configuration and rotates the other into optimum correspondence; then the coordinates of the objects in the two structures can be correlated dimension-by-dimension. Given overall similarity, individual elements of the CM pattern can further be tested by methods such as Timm's.

Thus, MDS provides highly general solutions to both of our main problems. Note especially the fact that the output of scaling depends only on the order of sizes of cells in the confusion matrices and not on their absolute magnitudes --or in other words, that scaling produces metrically invariant solutions for ordinal invariant data. This is particularly important in the case of card-guessing ESP data, which may plausibly be thought of as consisting of a relatively weak signal superimposed on an underlying random "carrier." We can expect the results of scaling to be nearly independent of the amount of signal, beyond some threshold amount required to approximately stabilize the ordering of cells in the confusion matrices. Certain liabilities of the methods should also be noted, however: they are in general relatively expensive, computationally, because of the iterative procedure; furthermore, the power of the technique increases with the number of objects scaled, and this is naturally correlative with computing costs. With fewer than ten objects cost is low, but the technique is relatively vulnerable to degenerate solutions; on the other hand, it is unusual to find subjects who, in tasks involving ten or more targets, can perform sufficiently well to generate data rich enough to scale.

We were extremely fortunate in having at least one such subject available, namely Bill Delmore. Like many other "sensitives," this individual strenuously insists on the quasi-visual character of his ESP experience. To investigate this claim, we have collected data both from a clairvoyance task and from a visual task in which he attempted to identify slides of the same target materials (playing cards), projected under conditions deliberately contrived to provide a visual experience as similar as possible to that of his typical ESP imagery. The ESP series is briefly

described earlier in this volume in a research brief by H. K. \* We applied the techniques described above to a comparison of the subject's ESP errors with his visual errors. The error structure in the ESP data was weak, but it could be extracted by various methods, chiefly MDS. When we did this, we found that the ESP error structure bore an overall resemblance to the relatively strong visual error structure, and that this correspondence was much more pronounced in the high-scoring runs. We tentatively interpret this result as consistent with the subject's introspections in suggesting that, for him at least, ESP information is regularly encoded in the form of fleeting visual imagery; errors appear at a secondary stage when he attempts to identify the images.

In a further study we intend to investigate error patterns produced by another exceptional subject, Lalsingh (Sean) Harribance, using different target materials but again obtaining both visual and ESP data. We conclude by stressing that the methods for studying CM are very general as well as very powerful, and could in principle be applied effectively in many other situations requiring comparisons between bodies of data. For example, we could compare psi tasks in a single subject, or compare different subjects in a single task. If particular individuals could be shown to have unique "signatures" in terms of CM patterns in particular tasks, those tasks might even be made the basis of a "survival test" of a rather complex and interesting sort.

"PT + PC = GESp"

Richard W. Brooks (Oakland University, Rochester, Mich.)

In several publications on parapsychological research, the claim is made that if telepathy and clairvoyance were separate cognitive abilities, their effect ought to be cumulative in a GESp test. The fact that the performances in GESp tests do not differ significantly from PT (pure telepathy) and PC (pure clairvoyance) tests is taken as evidence that telepathy and clairvoyance are, therefore, not different abilities, but merely reflect "different experimental conditions rather than processes differing from each other in principle," to quote from Ryzl's book Parapsychology: A Scientific Approach. In this paper, I wish to examine the

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\*See p. 24.

evidence and assumptions upon which that claim is based as well as to offer some evidence that should reopen the question. First, it is obvious that all evidence in parapsychology must be of the intrasubjective as well as intersubjective type, else there would be no way for us to verify the claims of professed "psychics" or to offer "proof" of paragnostical ability. While the "sensitive" may be able to obtain information by paranormal means, it must be such information as can also be obtained by normal means. Practically nothing has been said in the literature of parapsychology about the limitations that this imposes upon research.

But more importantly for my point, it must be noted that most of the "target" material for ESP experiments has been visual in nature. Aside from the question of whether this is really best, or even appropriate--because we have not yet settled on a theory of what kind of perception is involved; i. e., what is its field of operation and what is its limitation--the choice of visual material causes immediate difficulties in establishing PT as something distinct from GESP. This self-imposed restriction may be as much the cause of the lack of any cumulative effect as the supposed fact that PT and PC are merely (as Rhine et al. describe it in Extra-Sensory Perception After Sixty Years) "a single capacity, a mode of perception which has a range of apprehension that includes in its scope both objects and the experiences of other persons." Specifically, it may be the case that visual material is more appropriate for, say, clairvoyance tests than for telepathy tests. This, of course, does not explain why the results are approximately the same for visual material whether PT or PC is involved, but it is a factor that does not seem to me to have gotten sufficient attention. One way to establish telepathy as different from clairvoyance would be to use tests involving a sensorially different "target," e. g., unlabelled tapes of different sounds. The experimenter could himself be listening to the tapes in the telepathy experiments, while in the clairvoyance experiments the tapes could be playing to an empty, soundproofed room. If clairvoyance cannot operate under such circumstances and telepathy can, it would seem to establish them as different modes of ESP.

Second, and continuing this line of argument, there are certain types of information that are intrasubjectively verifiable (by normal means) and certain types of information that are not. For example, the existence of an object can be determined both by sight and touch, but the existence

of its color or texture cannot. It is at least worth considering that ESP may give us new information about the world in the way that, say, smell does beyond the information given by the other four senses. It is also possible that lack of general use of this channel of perception--if, indeed, it is a separate, different channel--would render it of varying degrees of insensitivity in different persons, and that practice would increase its reliability, as is the case with our other sense organs. Work is, of course, in progress along this line. Indeed, until we know its limitations, if any, we do not know with assurance whether certain experimental conditions impose such restrictions that information cannot be received in a certain way. We do not know whether it is merely lack of use that causes the results of PT, PC, and GESP tests to be approximately the same. Nor do we know whether and in what ways ESP can be deceived (analogously, say, to our inability to determine correctly the direction of a sound coming from behind us).

Third, there has been a general bias in ESP research toward its psychological aspects. This is understandable. We seem to be dealing with a perceptual process, and have enshrined that fact in our label--i. e., the "P" in "ESP." Perception is a psychological process. Hence the investigation of a paranormal perceptual process is reasonably termed "parapsychology." Whether this is a fortuitous bias or not, only time will tell. But it has led, especially in the United States, to an emphasis on the unconscious aspect of the psi process. The information the psychic or sensitive gets obtrudes itself into the conscious mind in ways that are often difficult to trace. Our search for these "hidden channels," as Louisa Rhine so aptly terms them, is the subject of much current research. This bias, however, may very well be the cause of our feeling that telepathy and clairvoyance are merely different aspects of a single capacity. To illustrate what I mean, let me make a distinction between parapsychology and parapsysics. Parapsychology, as its name implies, will deal with the psychological aspects of paranormal perception. Its "hidden channels" will necessarily be those within the mind or psyche of a single human being--e. g., dealing with the relation between the unconscious and conscious mind, with various personality traits, or with various psi-conducive psychological states (such as relaxation, motivation, belief, and so on). More recently, research has begun to deal also with the physiological correlates of psi; but this is still within the "life area," if I may call it that, of a single subject.



What happens between target object and subject's mind, or between agent's mind and subject's mind? If we were investigating questions of propagation of information signals from one point to another in the physical world, we would call the investigation physics. Russian research seems to rule out any reasonable consideration of known physical means of psi propagation. We need at least to consider the possibility of a nonphysical means of propagation (perhaps in a psi world inhabited by psi fields, which bear some relation to what we call our minds). Hence parapsysics. If this distinction has merit, it would be in the area of parapsysics, rather than parapsychology, that we may get the clearest indication that telepathy and clairvoyance are different and distinct perceptual abilities. But as long as we stay in the field of parapsychology, it would seem to me that we might well expect not to have found any statistically significant difference between telepathy and clairvoyance in the type of tests conducted so far.

Fourth, the first assumption behind the "PT + PC = GESP" formula is that the target correctly guessed by telepathy and the target gotten by clairvoyance will always differ. Statistically, this is of course not quite correct; some modification of the formula is needed, though perhaps not a very great change. It may be amended to read something like "PT < GESP > PC." But even this doesn't help, since the experimental evidence seems to be more in accord with the formula "PT = PC = GESP"; that is to say, "PT + PC = 2(GESP)." It is this fact, I think, more than any other, that has led researchers to believe that there are not really different paragnostical abilities, but merely one "psi" sense that manifests in different ways as it intrudes itself into our normal conscious processes. While that conclusion may, indeed, be correct, it does not necessarily follow from the present evidence, as I think I have shown.

Fifth, the other assumption behind the formula, now revised, is that two different sensory channels will invariably give more information than just one. For the most part, this seems to be true. But that is only because we have learned to process the information coming to us simultaneously through two--or even five--channels. This ability may be developed gradually in early childhood; the behavior of very young children seems to suggest that. But, in certain situations, when we are trying to attend carefully to the information from only one sensory channel (e.g., listening to a piece of music), we often deliberately shut off informa-

tion from other senses (e.g., close our eyes) because it interferes with the information we are trying to get. As far as I can tell, nowhere in the literature is the implication of this applied to parapsychology. If such were the case, even the revised formula could be incorrect. The following could just as easily be the case: "PT > GESP < PC." And that would be equally as convincing evidence of the difference between telepathy and clairvoyance as the original (or revised) formula.

In fact, one of my students, in the course of an informal exploratory ESP experiment, came upon a phenomenon which strongly suggests just such a possibility. The experimenter, Thomas Skylis, was trying a series of tests with different subjects under varying experimental conditions to determine which subject-experimenter pairs were best and which types of ESP tests were most favorable for his subjects. He discovered that one subject, Kathleen Shampo, scored markedly better on pure clairvoyance tests than on GESP tests (on which she scored significantly above chance even at that). After receiving the report of his experiment, I interviewed both subject and experimenter; the subject said that she preferred the PC tests because she could "see" the target more clearly--that when someone was concentrating upon it she "got confused." She felt that information was coming to her through two different channels, the telepathy channel interfering with the clairvoyance channel in her case. The question of the fundamental identity between telepathy and clairvoyance is, therefore, not established by reasoning based upon prior research, and would seem to be called into question by this particular experiment. Further research is needed along both of the lines suggested: (1) using nonvisual (e.g., auditory) target material to determine whether clairvoyance functions under such conditions, and (2) reviewing the past experiments and designing further experiments to determine whether some clairvoyance-talented people are confused, and do less well, in GESP tests.

NONINTENTIONAL PSI EVENTS:  
EXPERIMENTAL APPROACHES\*

UNCONSCIOUS PSI-MEDIATED INSTRUMENTAL RESPONSE  
AND ITS RELATION TO CONSCIOUS ESP PERFORMANCE

Rex G. Stanford<sup>†</sup> (St. John's University) and Gary Thompson  
(University of Virginia)

R.G.S. has proposed a model for spontaneous psi events according to which individuals can, without intending to do so, obtain extrasensory knowledge of events relevant to their personal needs and can use this information as a basis for modifying their behavior in a way which will be instrumental in satisfying the needs in question. This model is termed the psi-mediated instrumental response (PMIR) model, and it suggests that a person can unconsciously make psi-mediated responses which are instrumental in satisfying his needs. The present experiment examined a specific assumption of the PMIR model: that the timing of an action can be the mediating vehicle for psi-mediated instrumental response. This mechanism theoretically allows one unexpectedly to be at the appropriate place at the appropriate time to encounter a favorable event or to avoid an unfavorable one; or causes one to perform a particular action (or action sequence) at a time when the act has definite, logically unforeseen, favorable consequences. The latter function, specifically, was studied in the present experiment.

The hypotheses were (1) that all subjects tend to use the unconscious timing mechanism to avoid an unpleasant circumstance and to encounter a pleasant one (when they will definitely encounter one but only one of the two alternatives, and will have an opportunity to use the unconscious extra-

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\*Chairman: Louisa E. Rhine, Institute for Parapsychology, FRNM.

sensory timing mechanism to "choose" which they will encounter); and (2) that subjects' performance on a conscious ESP task will relate positively to the degree of their success in using the unconscious timing mechanism. That is, to the degree that a person tends to make effective unconscious use of ESP, he will also do well on a conscious ESP task. The latter hypothesis was based upon a post hoc finding of this kind in the data of an earlier study by R.G.S., and upon the feeling that the response modes involved in effective conscious ESP performance are also the response modes involved in wholly unconscious ESP. Confirmation of either hypothesis was to be construed as evidence of the unconscious timing mechanism as a functional mode for psi-mediated instrumental response. Twenty-nine volunteer males of college age were individually tested by G.T. before the experiment was stopped to allow analysis of the data for possible reporting at the Parapsychological Association Convention. The subject was told by G.T. that he was going to be given a precognition test involving an attempt to identify which of the 36 sectors of a radar-scope-like grid would later have a dot placed in them. The subject read a set of instructions aimed at guiding him toward effective performance and bolstering his enthusiasm and expectations regarding the precognition task. He then took the ESP test. Some weeks later an assistant randomly generated targets ( $p = 1/2$ ) for the ESP tests and scored them.

Immediately following the precognition test the subject was told that we were studying the relation of thought processes to performance on ESP tasks such as he had just completed. We therefore wished to give him a short free-association test to sample his thought processes. Responses and reaction times would be recorded as indicators of his form and style of response. A taped set of instructions was played immediately prior to the tape-recorded stimulus list, which consisted of 13 words. Stimuli were presented at 15-second intervals. Each time a stimulus was given on the tape, G.T. activated an electric timer. The subject was instructed to press a button as simultaneously as possible with his response. This would stop the timer, and the experimenter would then record the reaction time (RT) to the nearest one-tenth second. Unknown to the subject, the first three words of the list were not part of the PMIR task, but were used to give him time to accommodate to the task and to practice promptly pushing the button to stop the timer. The word list was administered while the subject sat in a large, comfortable chair with eyes closed to

avoid distraction.

Testing the unconscious timing mechanism assumption of the PMIR model required, in this experimental context, that the timing (RT) of one of the subject's responses be crucial in determining the subsequent quality of the experience (favorable or unfavorable) he would have in the experiment. One of the ten nonpractice stimulus words was randomly chosen as the key stimulus for each subject. The assistant who had randomly selected the key stimulus had no contact with the experimenter (G.T.). The list giving the key stimulus for each subject was turned over by the assistant to R.G.S., who kept it in his desk. R.G.S. had no contact with the subjects. The assistant who randomly selected the key stimulus also randomly determined a response-speed contingency for that stimulus: the key stimulus for each subject was randomly designated as requiring either a quick or a slow response from the subject if he was to enter the favorable as opposed to the unfavorable condition of the experiment which would follow the word-association task. The subject knew nothing about the existence of either a key stimulus or a response contingency for that stimulus, and G.T. was blind as to both the identity of the key stimulus and its response-speed contingency.

After the subject had completed the word-association task, G.T. left the laboratory on the pretext of getting further materials for a third and final phase of the experiment. He came to R.G.S.'s office, handed him the list of the subject's RT's, and R.G.S. compared the RT on the key stimulus with those of the other words and with the response-speed contingency for that subject. If on the key stimulus there was a "quick" contingency, the subject subsequently entered the favorable condition only if his response on the key stimulus was the fastest on the response list (or was tied for fastest). If the contingency required a "slow" response, the subject entered the favorable condition only if his response on the key stimulus was the slowest on the list (or tied for it). Otherwise, G.T. was told to run the subject in the unfavorable condition. G.T. then obtained the suitable materials from R.G.S.'s office, returned to the laboratory, and ran the subject through the designated condition. The dependent variable (PMIR measure) in the experiment was not, however, whether or not the subject actually entered the favorable or unfavorable condition, but was a scale measure of his tendency to do what was required by the response-speed contingency designated for the

key stimulus. The PMIR measure was the signed standard score of the subject's RT to the key stimulus. For this standard score, the standard deviation was based upon the ten RT's of the subject, and the sign designated whether or not the RT to the key stimulus was greater or smaller than the mean RT, as required by the response-speed contingency.

The unfavorable condition required the subject to circle, on three 8 1/2-by-11-inch sheets of randomly selected and ordered letters, any of three designated letters he should see. He was timed on the task, and was told that the task was given because speed on it had been suggested by "some persons" to be related to ESP performance. He was told this relationship had been studied in another lab and, although the results were not encouraging, the matter was worth further study because it could not as yet be considered settled. He was given the first sheet to work on, and then two subsequent sheets, without knowing how many more sheets were to be required of him. Each sheet had a different set of three letters to be circled. The subject was told at the start only that "Other pages of this sort will follow...."

In the favorable condition the subject was told that this part of the experiment concerned how males form impressions of females. He was asked to look sequentially at ten pictures of girls of approximately college age. Each picture showed an attractive girl in a pose which was in many instances potentially very erotically arousing. No pictures, however, showed sexual acts or were directly provocative. The girls did appear in a wide range of clothedness. The subject was asked to use an adjective checklist and to place a checkmark next to each adjective which he felt applied particularly strongly to the girl in each picture. The adjectives had been carefully chosen to assure that considering the pictures with respect to them would likely be pleasant and sexually arousing. The subject was to rate each girl separately, not to compare her with the others. This would require him to consider carefully the attribute in question by careful scrutiny of the girl depicted. This, it was thought, would be optimally arousing and pleasant and would avoid intellectualistic comparative strategies. The subjects were told that the results of this rating task were being correlated with ESP scores and were given a credible rationale for studying the possible relationship.

As evaluated by a t test the mean PMIR score of the

subjects did not differ reliably from zero. The mean score was positive but not significant ( $t = +.36$ ,  $df = 28$ ). Hypothesis 1 was thus not confirmed. Subjects as a group did not show a reliable disposition toward responding in a way which would allow them to encounter the favorable rather than the unfavorable condition. Hypothesis 2 was confirmed. The PMIR scores of subjects were positively and significantly correlated with their scores on the conscious precognition task ( $r = +.39$ ,  $df = 27$ ;  $P < .025$ , one-tailed). To the degree that persons seemed able to make effective use of unconscious ESP, they did well on the conscious ESP task. The confirmation of Hypothesis 2 provides support for the assumption that an unconscious timing mechanism can be a mode of psi-mediated instrumental response. Unless such a mechanism were effective we could not reasonably expect to find the observed correlation of PMIR scores and conscious ESP scores. The broader theoretical significance of the unconscious timing mechanism as a PMIR factor lies in opening up new perspectives regarding the role of unconscious extrasensory influence in life experience. The unconscious timing mechanism theoretically should allow possible extrasensory influence in rather rigidly preprogrammed sequences of life events in which, otherwise, such influences would not likely be possible. Confirmation of Hypothesis 2 specifically suggests that common factors may determine how effectively an individual can use ESP consciously and unconsciously. Disconfirmation of Hypothesis 1 (combined with confirmation of Hypothesis 2) suggests that it is difficult for some persons to use unconscious psi (PMIR) adaptively and that such persons may systematically misuse unconscious psi just as they psi-miss on conscious ESP tasks.

#### EVIDENCE FOR SELECTIVE TELEPATHY IN GROUP PSYCHOMETRY

Gertrude Schmeidler<sup>†</sup> and Jane Goldberg (City College,  
CUNY)

In a group psychometry session, several individuals try at the same time to have ESP impressions of a complex target. Does each respond as if he had been alone? Or is there intragroup ESP? If there is intragroup ESP, how does it function? Among the possible answers to these

questions are: (1) the subjects' records are independent; (2) there is ESP among group members which bears no relation to the target (what Warcollier called "mental contagion"); (3) one person's hits facilitate or inhibit others' hits on the same target items; (4) one person's hits facilitate or inhibit others' hits on different items. We report here data from a group psychometry experiment which support the third possibility. Seven subjects took part in the experiment. One was a psychic, Alan Vaughan. Three were Vaughan's friends, whom he brought to the session because he thought they had good psychic ability and would be in rapport with him. The other three were friends of J.G. They were interested and cooperative, but had shown no evidence of psychic ability. The experimental design was thus set up to be a comparison of scores of a psychic with scores of three experimental subjects (his friends) and three control subjects (J.G.'s friends).

J.G., G.S., and a colleague, Karen Kurlandur, acted as experimenters. There were four absent sitters. Each had handwritten a recent, meaningful dream. J.G. put each handwritten dream report into a heavy Manila envelope, held it to the light to test it for visibility, and found that the writing was not visible. She then sealed the envelopes. G.S. made a random ordering of the envelopes. The session was held at the A.S.P.R. when the building was officially closed. We described the procedure to the subjects, emphasizing differences from a prior session with the same subjects which showed no correspondence between impression and targets (though it gave some evidence of "contagion"). Vaughan gave introductory advice about how to respond. G.S. brought the first target envelope and the subjects meditated briefly about it, touching the outside if they chose. Each then went to a separate room where pen and paper were available, and behind closed doors they wrote their impressions of the dream or of the dreamer (or, if they preferred, dictated impressions to G.S. or Karen Kurlandur). After fifteen minutes the experimenters notified the subjects that time was up, and the group reassembled. G.S. collected the written impressions. The procedure was repeated for each of the three remaining targets.

With G.S. and Karen Kurlandur present, the subjects were then shown the dreams, but were not informed of the order in which the dreams had been used. Each subject made a blind rating on a scale of 0 to 100 of how well each dream corresponded to his (or her) four written statements.



J.G. typed each of the 28 readings in two ways, with and without parentheses after each scorable item. These typescripts were then dittoed and given to the dreamers (absent sitters) to score for accuracy of correspondence to their own dream. Each dreamer was asked first to score the set with parentheses by inserting a check mark into the parentheses following an item appropriate to the dream or dreamer, a question mark following a doubtful item, and a cross following an inappropriate item. The ESP score for each set was the total number of checked (appropriate) items. A second score was obtained from the dreamers by having them rate each protocol typed without parentheses on a scale of 0 to 100 for general appropriateness to their dream.

To determine the resemblance between protocols, G.S. made a blind match of similar items for simultaneous readings. This was done for each pair of subjects separately, giving 168 (42 times four) sets of figures. (Paired sets did not always show a one-to-one correspondence, since sometimes several items from one reading made separately scorable points, all of which corresponded to a single general statement in another reading.) The absent sitters' checks for their own records were then used to construct two-by-two tables for similar versus dissimilar items against checked versus unchecked items. Tables for the four protocols were pooled, yielding 42 two-by-two tables (six tables for each of the seven subjects). The tables were evaluated by chi square, using the Yeats correction where necessary, or by Fisher's Exact Method when the number of items was too small for chi square. Overall correspondence between scores and targets, disregarding the protocol-pair analysis, was evaluated separately for each subject by a two-way analysis of variance with the interaction sum of squares partitioned into one degree of freedom for the ESP hypothesis and a residual. No subject showed a significant correspondence between the target and his own ratings or between the target and the absent sitter's checked items. The absent sitter's global ratings of general appropriateness showed a significant correspondence for Vaughan's readings ( $F = 7.38$ ,  $df = 1, 8$ ;  $P = .05$ ) but were not significant for any of the other six subjects.

Data from similar versus dissimilar items were more interesting. For each of Vaughan's three friends, items similar to what Vaughan had reported were significantly more correct than items which did not correspond to

Vaughan's impressions ( $P = .005$ ;  $P = .03$ ;  $P = .05$ ). In contrast, each of the control subjects showed an insignificant relation to Vaughan's reports ( $P = .52$ ;  $P = .49$ ;  $P = .41$ ). The contrast seems especially meaningful when it is recalled that the absent sitters' checked items for Vaughan's readings were not, taken as a whole, significantly correct. The data thus imply that Vaughan's friends showed a telepathic rapport with him when he was correct but not when he was incorrect. Two possible interpretations of this finding are suggested. One is that when a psychic is correct, his impressions have a special quality which elicits a response from other (psychic) individuals who are in good rapport with him. The other is that in a group psychometry session, friends of a psychic are oriented both to the psychic and to the absent sitter, and that when the two orientations mesh they are more likely to result in a correct response than when they do not mesh. Either of these possibilities has many theoretical implications for discovering the nature of psi.

SYMPOSIUM:\*  
RESEARCH ON OUT-OF-THE-BODY EXPERIENCES:  
WHERE DO WE GO FROM HERE?

SOME NEW DIRECTIONS FOR RESEARCH

John Palmer (University of Virginia)

The first problem one must face in studying any phenomenon of nature is how to define it, and I do not believe that we as yet have adequately defined the out-of-the-body experience (or OOBEx). Indeed, any precise definition may be premature. I would suggest beginning with a relatively broad definition (e.g., the distinctive experience of perceiving the environment from a location in space outside of one's physical body) and then analyzing a large number of experiences with the purpose of classifying them into meaningful subcategories. Hopefully, we could then be in a better position to reach some consensus as to which subcategories should be retained as coming under the heading of OOBEx, thereby implying minimal criteria for such classification. These different kinds of experiences could also be related to other variables such as sex, age, state of consciousness prior to the experience, etc. Last but not least, patterns could be sought within these subcategories, such as, for example, between the quality of the experience and its content. If quantified data are available, factor analysis could be a useful tool in obtaining this kind of information.

The cases submitted to such analyses should constitute a representative sample of OOBEx. Unfortunately, samples drawn from unsolicited reports, reports solicited by advertisements or appeals, or reports obtained from special groups are particularly susceptible to sampling

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\*Chairman: John Palmer, University of Virginia.

biases. A better although more difficult, expensive, and time-consuming procedure might be to conduct a "Gallup Poll" type survey among a random sample of the population of a fairly representative community, and then obtain detailed accounts and standardized questionnaire data from persons claiming to have OOBEs as defined in the preceding paragraph. The results of such a survey probably would not be completely free from sampling biases either, especially if there were a high refusal rate, but they almost certainly would be more representative than samples obtained by the more usual methods. In addition, they would provide data on the incidence of OOBEs in the population.

However, if we are truly to understand the OOBEE, we eventually must learn to induce it in the laboratory. This is particularly true if we are to discover the psychophysiological correlates of OOBEEs, which rarely can be studied in spontaneous cases. However, even variables that can be studied in spontaneous cases, such as the psychological state of the person prior to the experience, can be studied more effectively in the laboratory where confounding variables often can be brought under experimental control. While valuable data may be and already have been obtained from a few individuals who claim to have OOBEEs pretty much "on demand," procedures must be found for inducing OOBEEs in a larger sample of subjects if generalized relationships of theoretical interest are to be uncovered. The present ambiguities concerning the definition of the OOBEE should not inhibit us from seeking such procedures. There are several approaches one can adopt for inducing OOBEEs. A number of spontaneous cases have been reported under anesthesia and hallucinogenic drugs, but the restrictions currently placed upon the use of drugs in psychological research may limit the feasibility of this approach. Hypnosis may be a more promising tool, although its value may be restricted to highly susceptible individuals. We are having some success at the University of Virginia with a procedure involving the use of a progressive relaxation induction and a special audio-visual technique designed to facilitate a sense of detachment from the body. It is too early, though, to evaluate the full potential of this procedure. Perhaps some combination of these approaches will ultimately prove to be the best solution.

While the OOBEE is of considerable interest in its own right, I also see it as relevant both to the facilitation of ESP and to the survival question. ESP is sometimes re-

ported as a concomitant of OOBES, one of the more striking examples of which was Tart's "Miss Z" correctly "perceiving" a five-digit number during an apparent OOBES in his laboratory. A great deal of recent research has illustrated the relationship between psi and altered states of consciousness. Although not all altered states of consciousness are necessarily psi facilitating, the fact that the OOBES is an altered state of consciousness increases the likelihood that it may be such a facilitating vehicle. In a more specific vein, I am exploring a psychological theory of psi which hypothesizes that psi is facilitated to the degree that a person adopts a set of attitudes, expectancies, and cognitions which are consistent with its manifestation. Such a hypothesis is suggested by the "sheep-goat effect" and some of the work demonstrating the facilitative effect of hypnotic suggestions on ESP. If this theory has any validity, the OOBES should be psi-facilitating insofar as it provides the subject with an altered view of his relation to physical reality in which "perception at a distance" is not only possible but natural. This is an example of a hypothesis that can be tested in properly controlled fashion once we learn how reliably to induce subjectively convincing OOBES in the laboratory.

If the OOBES is literally what its name implies, its relevance to the survival question is obvious. While I do not believe that any currently available research technique can prove survival, OOBES research in combination with other types of research may alter the subjective probability one can attach to this hypothesis by providing empirical data which are either consistent or inconsistent with specific theoretical models which assume that certain aspects of the human "psyche" can function independently of the physical body. To me, the most convincing survival evidence obtainable from OOBES research would be for a person having an OOBES simultaneously to communicate through a medium or sensitive unknown to him. As I argued in my paper presented at the 1972 Parapsychological Association Convention, I feel that such a communication would be most impressive as survival evidence if the medium demonstrated not only information acquisition but also skills, personality characteristics, idiosyncrasies, etc., of the communicator. The case for the survival hypothesis would be further strengthened if physical or electrical disturbances in the vicinity of the medium could be demonstrated in conjunction with the psychological effects.

The OOBES paradigm is superior to that commonly

used in mediumship research because of the control the investigator can exert over the "communicator." If the OOBEer can control his experience, the exact time that the communication is to be attempted can be specified in advance, unbeknownst to the medium who need be told only that "someone" may be trying to communicate through him sometime during the session. If psychophysiological indices of the OOBE could be discovered, these could provide convergent indices along with the OOBEer's verbal report that the experience occurred at the time specified. A temporal correspondence between an OOBE during which the OOBEer claimed to have "communicated through" the medium and a convincing personation of the OOBEer by the medium could provide a compelling case for the literal interpretation of at least some OOBEs, and thereby provide support for the survival hypothesis. Additional controls could be introduced by having the OOBEer or someone else attempt to produce the same result through telepathy, the medium again not being told when this was being attempted nor the nature of the attempt.

Of course, the disadvantage of the OOBE paradigm for survival research is that, even if one were convinced that some aspect of the OOBEer left his physical body, the dependence of this aspect on the living physical body could not be ruled out. However, I think that the opportunities for increased control offered by the paradigm outweigh this negative factor. Furthermore, positive results with this paradigm would make the survival interpretation of alleged communications by deceased persons more compelling. While I doubt that we at present have either an OOBEer or a medium sufficiently skilled to demonstrate a full-blown possession with this paradigm, the paradigm is technically feasible and could yield more modest results which nevertheless might strengthen the case for the survival hypothesis. If we stick with this general approach, we may even find that this "ideal" experiment will not seem as "far-out" five or ten years from now as it does at present.

## PERSPECTIVES FOR OUT-OF-BODY RESEARCH

Karlis Osis (American Society for Psychical Research)

The out-of-body experiences (OOBE) looked very different to me before we started our work on them. I had

thought that the main experimental task was just to gather evidence for the out-of-body characteristics. Well, there was much more to it. Out-of-body experience has been best known from introspective descriptions and from spontaneous case collections, which in the last analysis again go back to introspective observations of the experience. From this perspective the material looked rather homogeneous. The spontaneous data indicate clear separation from the body of a part of personality that is capable of perception, decision and even interaction with the physical environment; the feeling of one's identity, of the "I-ness," is displaced into the OOB projection. I am afraid the introspective observations would be experimentally verified only in a small fraction of the cases. In other words, the bulk of the cases seem to be a mirage.

At the ASPR we have by now tested about 100 persons on long distance OOBing. We tested the cream of the claimants who show the introspective characteristics of the genuine OOBE and who have the ability to induce these experiences at will. In spite of careful selection, about 85 percent of those who tried the experiment showed no awareness of the stimulus objects or persons in the target area, although generally they believed they had seen them. In most cases the introspective reports proved to be totally self-deceptive. Corollary: the vast majority of spontaneous cases seem to have nothing to do with bona fide OOBE. A significant but small number still remain where the OOB projection appears to be capable of perceiving. If this is so, the case collections in the literature might have mixed true OOBE characteristics (wheat) with an overwhelming amount of characteristics (chaff) that have no relation to the wheat whatsoever. Even worse, of those individuals in our studies who have shown some signs of OOB perceptual power, we did not find a single one who could see things clearly every time he felt he was out of the body--the perception ranging from fairly good (i. e., clearly distinguishing some objects) to complete failure (i. e., producing very foggy or totally incorrect images). The genuine OOB state seems to be only one of several states of altered consciousness that the subject will experience when attempting a projection. For example, a Canadian OOBer told us that she has to reach a certain level of altered states for real OOBE; otherwise general ESP images or hypnagogic reveries will take over. A psychic from Maine on occasion went into such a deep altered state of consciousness during a laboratory experiment that moments after a response he did not remember

what he had seen or reported--and these were his best trials. The OOB state might be closely interwoven with that many-splendored thing called "altered states of consciousness." Only a small fraction of OOBers have "real" experiences, if we tentatively take the power of perception as a criterion for some realism in OOBE. Not every projection of good OOBers is "real," regardless of whether or not they see themselves separating from the body. For these reasons, spontaneous case collections of OOBE data might be very misleading. There is an urgent need to work out some kind of criterion or syndrome of phenomenological or psychophysiological characteristics by which to separate the "seeing OOBE" from other altered states of consciousness.

In the absence of such criteria the most promising strategy seems to be a multivariate approach. That is, we hypothesize that genuine OOBE will be recordable on several parameters at once. At the ASPR we are developing two multivariate models. One consists of perceptual tests of OOBE. We have developed a test in which the results are determined (a) by complex stimulus displays randomly presented in an optical system; and (b) by positioning of the OOB observer in a limited area in front of the viewing window. A scoring system has been devised that indicates whether the processes of positioning and perceiving are coordinated or not. If the positioning in front of the window is bypassed by some kind of general ESP sweep of the stimuli, the scoring system would pick up this, too. If multivariate aspects of perception and of positioning coalesce in a single holistic percept, we would feel justified in making a tentative inference that the OOBE subject's perceptual powers have been localized outside of his body.

Another model on which we are working involves widely disparate parameters. We simultaneously test perception, physiological variables such as occipital EEG, and physical variables in the viewing area such as electrostatic fields. Ideally the covariation of these parameters might be as follows: If perception at a given time is good, then definite EEG criteria occur and a certain interference with physical fields is registered. So far we do not have enough data to form an opinion as to whether this model works. When we measure physical fields, PK on the instruments is possible as a counter-explanation. We keep the subjects blind as to the physical measurements taken, so that they aren't aware of what could become a PK task. We postulate that the mere presence of the OOB projection would be sufficient



to make the difference in physical fields.

At early stages of methodological developments we need to be open to many possibilities. Our multivariate approaches are just two of the possible methodological developments. We have also tried a single-variable approach that seemed not to work out. We used human observers in the projection area. When the human observer was not especially psychic he seemed to "see" nothing. When an experienced psychic was in the projection area, she did see the projectionist at the approximate time of the projection, and on occasion she did describe the projectionist reasonably well. However, the possibility remains that the psychic might not actually have been scanning the projection area as she was instructed; she might have scanned the projectionist's home hundreds of miles away by means of GESP. Two cases are illustrative: in one, a psychic from Maine felt that when he projected to the place of our stimulus display, he was bent over and floating over the display. His arms and legs were dangling and he could not get down onto the floor. The psychic who observed the area that evening did see someone hovering over the target display who was bent like a jackknife. This seemed a realistic observation of the projection area. However, in another case, the same psychic gave a good description of a Boston writer who was projecting herself to the stimulus area. Then the psychic proceeded to describe a boy on roller skates. No boy was scheduled at that time. On checking with the writer, we found she had a ten-year-old son who fit very well the description given by the psychic. At the time of the experiment the boy had fallen asleep while watching a TV movie showing waitresses on roller skates. It seems to me the psychic not only scanned the projection area but also the Boston home far away from our New York laboratory, where she hit not only the projectionist but also her immediate family surroundings. Apparently such a single-variable approach as seeing an OOB apparition will not separate the OOB hypothesis from others. I personally put my trust in multivariate research models rather than shopping for an old Occam's razor.

## THE USE OF DETECTORS FOR OUT-OF-BODY EXPERIENCES

Robert L. Morris (Psychical Research Foundation)

Out-of-body experiences (OBEs) interest the parapsychologist for two reasons: as a potential psi-favorable state, and as possible evidence of the capacity of some tangible aspect of self to expand beyond the physiological body. To serve as evidence for the latter, the OBE must be more than just a psi-favorable state; it must involve a demonstrably real departure of something from that body, something that can be shown to be somewhere. If an aspect of self can expand beyond the body to a specific location, it may be detectable by human, animal, plant, and/or physical detection systems. One line of systematic research, then, is to have an OBEer project to a specific location where there are a variety of detectors. If certain of them respond consistently, in such a way as to indicate that they are responding to something spatially and temporally discrete, we can follow up with studies aimed at finding out what properties of the apparent displaced consciousness are being detected. Such studies can include use of additional detectors specific to certain properties, modification of detectors, examination of parameters of the detection responses, and so on. If inconsistent results are obtained, followup studies can examine the extent to which OBEs may therefore be "only" psi-facilitating states. Complete failure to obtain positive results with detectors will at least provide evidence against certain frequently offered hypotheses.

For the past eight months we have been investigating OBEs at the Psychical Research Foundation using a variety of approaches including human, animal, and physical detectors. The basic procedure is as follows. The OBEer is located either in a nearby psychophysiology laboratory or in Building A of the Psychical Research Foundation. The target rooms to which he is to project are in Building B of the P. R. F. The OBEer's body is generally monitored for left and right occipital EEG, eye movements, chin EMG, respiration rate, digital plethysmograph including tachygraph, and skin resistance. His task is to "visit" the target rooms, notice some aspect of the detection room such as detector position or a target object, and affect the detectors somehow. Two alternative procedures have been used to insure the blindness of the human detectors and those monitoring the animal and physical (energetic) detectors. (1) The detectors

are monitored for a forty-minute period, during which they will be "visited" by the OOBEer twice, each time for about two minutes. (2) The detectors are monitored on four separate two-minute occasions, two of which are occurring during an OOBE (experimental) and two of which are not (control). In both of these procedures, neither the human detectors nor those monitoring the animal and physical detectors know when an OOBE will occur. Our human detectors include P. R. F. staff members, personal friends of the OOBEer, partial acquaintances of the OOBEer, and others who have had OOBES. Occasionally they have attempted to induce an altered state in themselves, to facilitate the detection task. Our animal detectors include small rodents in activity cages, snakes in observation cages, and kittens in an "open field," a large enclosure whose floor is marked off in a series of ten-inch squares to allow a measure of activity to be taken. Our physical detectors include devices for measuring changes in electromagnetic field strength and permeability, thermistors for registering heat changes, photo-multipliers for registering energy levels in the infrared, visual, and ultraviolet spectra, and other instruments.

At present we have learned the following about the OOBES of Mr. Blue Harary, the individual with whom we have done most of our work. Mr. Harary undergoes a preliminary "cooling down" period during which he uses progressive relaxation and imaging to get his body functioning at a low level of activation, with very stable heart and respiration rates and low skin resistance. Once he is sufficiently cooled down, he can "leave" at will, and sometimes finds it hard to stay in. The more he cools down, the stronger his experience. Mr. Harary's success at describing the detection room is mixed. He seems to do better on colors than shapes, and has had some success in describing the location of human detectors in the detection rooms. The detection results are also somewhat varied. Humans seem to do better during periods when they are occupied with a mildly distracting task and not paying a great deal of attention to being detectors. The physical devices have not provided any results suggesting detection of energy changes in the areas "visited" during an OOBE. Hamsters and gerbils have not responded to the OOBEer by changing their gross activity level, as measured electronically, or by noticeably altering their behavior patterns. Too little work with snakes has been done so far to justify any generalizations. We have, however, found a kitten which responds during OOBE experimental periods by becoming very inactive, never me-

owing and rarely crossing from one square to another in the open field apparatus. During control periods, on the other hand, the kitten is quite active. This difference in behavior is statistically significant ( $P < .01$ ), and has been occasionally very striking.

Such a result can at present be taken only as indicating that a communication channel exists between cat and OOBEEr. We have some evidence that the cat does not respond to "pretend OOBEEs," in which Mr. Harary relaxes and generates a vivid set of visual images of visiting the cat which are similar to those he would experience during a "true OOBEE." We are presently designing studies to explore the spatiotemporal discreteness of this apparent detection response and its relationship to qualitative aspects of Mr. Harary's experience. We are also testing other animal species. From such followup studies will come an assessment of the OOBEE as a two-way psi communication system and also, perhaps, an assessment of whether or not such two-way communication is mediated by a "real" expansion of the self into the environment.

#### SOME METHODOLOGICAL PROBLEMS IN OOBEE RESEARCH: Comments on the Symposium

Charles T. Tart (University of California, Davis, and Institute for the Study of Human Consciousness, San Francisco)

It's been very gratifying to hear these excellent papers on out-of-the-body experience. I have no quarrels with them, so what I would like to do in my comments is to emphasize that it's more complicated than we think. The term "out-of-the-body experience" is too broad: I think it probably covers several different types of experiences, some of which are quite discrete and some of which run along various kinds of continua. It's going to be very difficult at this stage of the game to try and apply the term more precisely. The phenomena that have come to us in spontaneous reports come from people; and people are often very poor observers of internal experiences. Also, our language is not very well suited to describe experiential data in much precision to begin with.

So we start off with a body of spontaneous data that gives us a rather vague concept that probably includes two or three or even half a dozen subsets of concepts within it. First, let's talk about a subtype which I'm tempted to call the classical out-of-the-body experience, or d-OOBE--the "discrete out-of-the-body experience." This is the experience where the subject perceives himself as experientially located at some other location than where he knows his physical body to be. In addition, he generally feels that he's in his ordinary state of consciousness, so that the concepts space, time, and location make sense to him. Further, there is a feeling of no contact with the physical body, a feeling of temporary semi-total disconnection from it. This d-OOBE is the kind of thing that is described by Fox, Muldoon, and Monroe. A d-OOBE may seem to be oriented to an earthly location, in which case our parapsychological interest is aroused. If the subject thinks he's at location X we can ask him what's going on there, or we can put detectors there, and come up with very interesting results. A d-OOBE can also go to some place that's not recognizably part of this earth, and that makes it rather difficult to deal with parapsychologically at present; if the subject says he visited a certain part of heaven, we ordinarily don't have any channels by which we can find out if he was detected by the appropriate instruments there! Even this kind of case is ultimately researchable, I think, if you can produce the phenomena reliably enough to send several independent observers to the same place, and find out whether you have a consistent experiential reality, regardless of whether there is any physical basis for it or not. We would have a state-specific science for d-OOBes then.

With regard to the d-OOBE, there are two quite different approaches you can take in theorizing about its underlying basis. One is the rather standard approach: it's just what it seems to be. Consciousness is somehow "at" a different space-time location than the physical body. That, of course, is our parapsychologically interesting case. Alternatively, you can imagine a d-OOBE as located "within" the brain, but with a hallucinatory reality having been created, as in a dream, and, simultaneously, a kind of disconnection from the body: you can't move, you can't perceive with the ordinary sense channels, etc. As to the second type, you can get what you might call pseudo-classical-out-of-the-body experiences, and confuse them with d-OOBes, by the way you ask people questions. If you're absorbed in watching a good movie, and you forget where you're sitting, and the

fact that your foot's itching, and you're asked only for a momentary report on your experience, you might say, "Well, I was in this old castle in Europe watching these people do such-and-such." But that's not really what we mean by a d-OOBE, because if you ask the person to scan all of his experience, then he will say, "Well, if I turn my attention to it, I'm also aware of sitting here in this seat in the movie theater." In the d-OOBE, on the other hand, the person will not report anything from the body, and indeed will fairly uniformly report that in order to bring back bodily sensations he must effect a transition he calls "returning" to the body, which makes the out-of-the-body location disappear. This is why I call this a discrete out-of-the-body experience; there seems to be a very clear transition in such classical cases.

If one postulates a continuum loosely labelled body awareness, then the d-OOBE represents a definite break in the continuum rather than merely its lower end. The experience may be reached through a gradual movement along the continuum from higher to lower body awareness, but at some point a transition phase occurs. Although movement along the body awareness continuum frequently takes place in d-OOBE induction, it does not seem to be universally the case. Often the experience just occurs, as suddenly as getting hit on the head. There is as much variability in the state itself as there is in the induction process. The d-OOBE sometimes follows a kind of step function in which it turns into something else. It may be transformed into a discrete altered state of consciousness, such as a mystical or psychedelic type of experience. It may change from one discrete state to another while "in" the out-of-the-body location. You can't assume that it's always going to stay within a certain kind of experiential cluster, or remain a classical d-OOBE for its duration.

Another somewhat tricky source of variability is that you may develop procedures which will decrease a person's body awareness, but not all of them may lead to the transition into a d-OOBE. We have a lot of evidence that relaxed states are favorable to the operation of psi. Many of these continua of reduced body awareness may therefore give you enhanced psi results without ever getting the subject into the d-OOBE. If you have a subject who is not very good at discriminating his internal experiences, he may tell you he's having an out-of-the-body experience when really he's just at a low point on the body awareness continuum. In addition,

all sorts of experimenter biasing effects can come in. The subject, although not having a classical d-OOBE, will join you in the game and give you the kind of experiential data you like--experiential data is at least as subject to biasing effects as behavioral data, and possibly more so. I might mention two other possible outcomes of attempting to induce OOBEs. First, you might have a procedure that will reduce body awareness to the point that it in turn induces a discrete altered state of consciousness, which then leads into a classical d-OOBE. Second, you might have a procedure which produces a (psi-favorable?) altered state, but does not ultimately lead into a d-OOBE. The problem is that we simply don't know enough yet to be able to say that we have the criteria, or the questions to ask the subject, which will enable us to distinguish among these possibilities.

I am very much in favor of something that was implicit in all the papers presented at this symposium: the ending of "colonialism" in experimentation, in which the almighty knowledgeable experimenter manipulates the warm body that we politely term the subject for the sake of us educated people who understand the universe. This experimenter-subject model creates all sorts of artificial games in the situation. I would really like to see us talk instead about co-experimenters who cooperate to find out something that's mutually valuable to both of them.

And now a few comments about detectors. Most of the attempts at instrumental, animal, or human detection so far have as their background the idea of the classical d-OOBE. We're obviously not trying to detect somebody on one of the lowered body awareness continua using ordinary kinds of psi, but rather somebody in the classical out-of-the-body state in which we'd like to think "something" leaves the body and thus can be detected at the distant location. The mechanical detectors are a fine idea, but I think we should be prepared for a lot of discouragement. This research is essentially going to be a matter of begging, borrowing, and stealing every possible kind of instrument you can on every occasion, and hoping that something will show the right kind of blip when your out-of-the-body experiencer is there. The animal detectors are probably a better way to approach the problem; we're dealing with a life phenomenon, and will be more likely to detect it with the responses of living things than with the responses of machines. Machines are too simplified, I think, in some sense. But let's not assume that it's simply a matter of finding the right

animal and putting him where the out-of-the-body person tries to go. You may have to train the animal to show the right kind of response. With a combination of training techniques, including probably some kind of operant technique to reinforce the animal for making some sort of unique response at the time the out-of-the-body experience is occurring, we may be able to develop fairly reliable animal detectors. Finally, I think the emotional link between the animal and the out-of-the-body person is important. It's Blue's pet kitten that they're getting results with at the Physical Research Foundation, not hamster number 1437 taken out of the animal colony.

The last thing I want to say is that I think we're going to have to work with drugs in trying to induce out-of-the-body experiences. There are so many rather old but good cases that came from anesthetic drugs. Nitrous oxide would be a good drug to work with, in my opinion, especially since it is relatively safe to use under medical supervision. I would probably combine it with something like hypnosis to manipulate motivation, put in proper post-hypnotic suggestions which then might be effective in the anesthetic state, and so forth. On the other hand, I'm very leery of using hypnosis alone here: I've done one study in which hypnosis produced experientially wonderful out-of-the-body experiences in trained subjects. They showed classical phenomena that they'd never heard about, but their description of what was in the next room was absolutely inaccurate. You can investigate out-of-the-body experiences psychologically, and they're exceptionally important from this point of view whether they have any parapsychological aspects or not. After all, most people who have these experiences come back saying they no longer believe in survival of death, they know it's true because they've been there, and this is extremely important psychologically. But as parapsychologists we're interested in the paranormal aspects of out-of-the-body experiences primarily, so I would caution against using hypnosis alone to induce them. It's too easy to get a good experience without also getting the extrasensory aspects.



SYMPOSIUM:\*  
PSYCHOKINESIS ON STABLE SYSTEMS:  
WORK IN PROGRESS

PK IN THE SOVIET UNION

Montague Ullman (Maimonides Medical Center)

Reports of interesting psychokinetic effects taking place in the Soviet Union have reached an American audience in a number of ways. There have been popular sources such as a book by Ostrander and Schroeder and one by Ebon; English translations of scientific or popular reports appearing in the Journal of Paraphysics; attendance by Western observers at conferences in Moscow and Prague; and finally, reports based on direct personal observation. I think it is important for American parapsychologists to take note of these developments and become as knowledgeable as possible concerning the phenomena reported, the technology involved, and the theoretical concepts that have been developed to account for the observations. This paper offers a comparison of the alleged psychokinetic effects produced by two Soviet subjects, Nina Kulagina and Alla Vinogradova. The theoretical explanations are summarized along with brief mention of some of the ancillary issues involved. Nina Kulagina is a 47-year-old housewife and grandmother with a retiring and self-effacing personality. Alla Vinogradova is in her late thirties, married, and is a child psychologist. Her personality is more forward, assertive, and commanding than Kulagina's. Kulagina is capable of producing a wider variety of effects than Vinogradova. Both of them have achieved movement of objects on a flat surface, and both practice healing, but Kulagina in addition has produced levitation of objects and has influenced magnetic needles and photographic material in the absence of light, radiation, and chemical or

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\*Chairman: Charles Honorton, Maimonides Medical Center.

mechanical forces. Photographic effects produced by Kulagina include evocation of crystal luminescence of a lumino-pher, production of "cold light" on a photographic emulsion, and a change in the spectrum of visible light absorbed in liquid crystal. Finally, she has provided dramatic demonstrations of influence upon biological systems at a distance without any known means of action. Kulagina's ESP abilities in general are clairvoyant and telepathic, whereas Vinogradova claims precognitive ability.

The two subjects differ as well in the amount and kind of physiological change they undergo when they perform. Kulagina experiences great stress, with pulse increases to a rate of 150-240, increased respiration, and pain in her upper spine and the back of her neck. She shows weight losses of 0.7 to 2 kilograms within one hour after her PK attempts, and a raised blood sugar level. At the onset of this activated state, she often has a taste of iron or copper in her mouth, and feels thirsty. During the activated state she experiences occasional vertigo and vomiting. Vinogradova exhibits fewer physiological changes. She performs under far less stress than Kulagina, although she shows some pulse increase. She claims to feel a sensation in her solar plexus and forehead when she performs. The success of both subjects is influenced by the emotional milieu in which they perform. Vinogradova is sensitive to her audience; Kulagina's phenomena depend both on her own mood and on the mood of the spectators. She experiences less stress when she is alone, and responds best in a friendly atmosphere of mutual trust. In a less supportive atmosphere, she requires more energy to produce her effects.

As in other areas, more is known about the mechanical aspects of the witnessed psychokinetic effects in the case of Kulagina. The size and shape of the objects she moves are more important than the nature of their substance. She can displace objects from a few grams to 380 grams in weight. Vinogradova, on the other hand, can slide objects weighing up to 30 grams and can roll objects of up to 100 grams. Kulagina can slide a vertical cylinder more easily than she can roll a horizontal cylinder, indicating that a sliding motion is easier for her to produce. When she moves soft objects they do not change in shape. The direction an object moves depends on Kulagina's will (initially objects tended to move away from her; in the more recent period they move toward her). Rotational and vertical movements also occur, and one object out of many can be made

to move. Kulagina's best working distance is half a meter from the object. Her distance limit is reached when she is sitting one meter from the edge of the table with the object one meter away from the table's edge. Vinogradova can also determine the direction of object movement at will. She works up to a distance of two feet from the object, and claims that her performance is enhanced by autogenic (systematic relaxation) training.

A number of electrical effects have appeared during Vinogradova's sessions. These are considered to be electrostatic, but she can still move objects even though she herself is grounded. Objects that she influences become charged, and there is evidence that the ability to move these objects can be transferred to others. When Kulagina's performances were studied, it was found that an electrical field was generated in the vicinity of the objects she influenced. She has been unable to influence objects in a vacuum. Unlike the claimed electrostatic nature of Vinogradova's effects, in Kulagina's case electrostatic screening has no effect. She cannot influence an electroscope, and her effects are better when the object is under a dielectric cover. The optimal temperature for her success is 18 degrees Centigrade. During Vinogradova's sessions, sparks occasionally emerge from her fingertips up to a distance of 2 centimeters. No such sparks have been observed with Kulagina. Both subjects prefer clear weather when they work: Kulagina claims that electricity in the atmosphere has an adverse effect on her performance. She is unsuccessful during storms. Vinogradova prefers working on starry nights with a full moon, but can perform during thunderstorms. Finally, Kirlian effects [see paper by Burton and Joines, page 15] have often been observed around Vinogradova when she is performing. The flares become more concentrated and scattered. In addition, red blotches sometimes occur during PK successes, and there are shifts in the electrical activity of acupuncture points.

The July-August 1973 issue of the Parapsychological Review carries a report by Benson Herbert which represents the first series of controlled studies of Mrs. Kulagina by a Western observer. He satisfied himself that the effects were genuine and that they could not be accounted for by other physical explanations. He brought with him apparatus both to test the PK effect and to measure any electrostatic influence. He described two new effects, one electrical (prolonged ionization of a saline solution used in a hydrome-

ter) and the other physiological (the production of intense heat and an area of redness and swelling of the skin after his arm had been held by Mrs. Kulagina). Herbert was also able to arrive at a measurement of the force involved in the movement of the objects he employed.

Of the scientists investigating Kulagina and Vinogradova, Drs. Adamenko, Inyushin, and Sergeyev appear to be the leading theoreticians in response to the psychokinetic effects under study. Adamenko is pursuing experiments to ascertain the role of electrostatic effects charging the surface of an object and causing movement. The resultant movement correlates with hand motion as well as the dynamics of the skin's electrical resistance. The Vinogradova phenomena cannot be explained completely on an electrostatic basis since the effect is better when she is grounded. Both Inyushin and Sergeyev have been developing theories based on the existence of a new form of energy, a form of biological energy referred to as bioplasma. They consider PK as analogous to a surface that is accidentally charged by lightning with the movement occurring as a result of the interaction of the object's electrostatic charge and electromagnetic field with the human operator's field. This biological energy is under conscious direction. The movement of the object can be started, stopped, and changed in direction, and the object can be made to rotate. Sergeyev has recorded 10,000 volts/cm. in the vicinity of the object with no indication of an electrical field in the space between Mrs. Kulagina and the table. He has also developed instrumentation which registers changes in the bioplasmic field up to a distance of three meters. According to Sergeyev bioplasmic energy is maximally concentrated in the head region. He attributes PK to a polarization of the bioplasma in a laser-like fashion, referring to this as a biolaser effect. The biolaser acts as a material force upon the object. Sergeyev also claims that microbioenergetic emissions (bioplasma) can act as carriers of information.

The Russian investigations are moving in several directions in an effort to develop material explanations for the psychokinetic effects observed. These involve concentrated studies of the characteristics of the electrical field between object and person, the characteristics of electrical fields about the body, the study of bioplasma through detection devices, electroencephalography, and Kirlian photography. The observers are by no means in accord concerning the nature of the forces at play, although all are in

agreement that a physical energy is at work. The Soviet work points up the possible relevance of Kirlian photography for parapsychological research. The work further suggests that gross PK effects are not as rare as formerly believed, and that qualitatively different kinds of forces may be operative with different subjects. At any rate, at this point in time more information is being distilled out of the fact that two subjects are under study simultaneously. Their different personalities and life styles may have a bearing upon the different circumstances and kinds of phenomena that each is associated with. A fringe benefit has been the development of cordial professional relationships with Soviet scientists interested in parapsychology.

#### PK EXPERIMENTS WITH URI GELLER AND INGO SWANN

Harold Puthoff<sup>†</sup> and Russell Targ (Stanford Research Institute)

Earlier in this convention, we presented several ESP experiments with Uri Geller [see page 57]. In this paper we would like to describe some PK work with Geller and also with Ingo Swann, a gifted subject who is in addition a painter. First, we would like to show the second half of the film we made of the preliminary work with Geller, which describes the PK experiments.

I. Uri Geller (Summary of Film). Two experiments to measure physical perturbation of laboratory apparatus were carried out. One of these involved Geller apparently exerting a force on a laboratory balance, and the other was the generation of an apparent magnetic field recorded by a magnetometer. Both of these experiments were performed several times and the results improved with repetition, showing apparent evidence of learning taking place. (A) Balance Experiment: a precision laboratory balance measuring weights from one milligram to 50 grams was placed under a bell jar. This balance (made by Scientech Corporation of Boulder, Colorado) generates an electrical output voltage in proportion to the force applied to it. The balance had a one-gram mass placed on its pan before it was covered with the bell jar. A chart recorder then continuously monitored the force applied to the pan of the balance. On several occasions Geller caused the balance to respond as though a

force were applied to the pan. This was evidenced by a corresponding displacement shown by the chart recorder. The displacements represented forces from one to one and one-half grams. These displacements were ten to a hundred times larger than could be produced by striking the bell jar or the table or jumping on the floor. It should also be noted that in some instances the displacements were in a direction opposite to the gravitational force on the balance. (B) Magnetometer Experiment: a Bell gaussmeter was used to determine whether Geller could perturb an instrument sensitive to magnetic fields. The instrument was set to a full scale sensitivity of 0.3 gauss. Geller would move his empty hands near the instrument in an effort to cause a deflection of the chart recorder monitoring the magnetometer output. In carefully filmed experiments Geller was able to perturb the magnetometer with an apparent field of up to 0.3 gauss. He did not touch the measuring head of the instrument, and the deflections of the meter were not in general correlated with his hand motions.

II. Ingo Swann. In our work with Geller and, to an even greater extent, in our work with Swann, we have used rather complex equipment. This presents problems when one tries to decide what the locus of PK influence is (the device itself, the recording apparatus, or the transmission lines between the two), but there are several advantages to this approach. One is that the equipment is sensitive, so that if the PK effect is small, you can still detect it. Second, we are interested not just in finding out whether these effects occur, but also in actually discovering what the mechanisms are. With complex equipment such as gravitational detectors and electromagnetic detectors we can get some understanding of the actual mechanisms involved. A third advantage is that these various pieces of complex equipment are more or less standard throughout a number of laboratories, so that other scientists are more likely to take notice of what we find and try to follow it up in their own laboratories. A fourth reason for using complex equipment is that, with special subjects, if we present what would seem like an easy psychokinetic task, it is hard to get them interested. On the other hand, if we challenge them with a seemingly difficult task, the result is a psychological state in which they produce surprisingly good results. This was, in fact, the approach we used with Ingo Swann.

The piece of equipment that challenged Swann in this

manner and from which the good results were derived was a magnetometer, a magnetic field measuring instrument which was buried in a vault below the floor in a laboratory. The device is very insensitive to outside perturbations. It has mu metal shielding which shields out magnetic fields, and it has in addition what we call a superconducting shield, which is an excellent shield for electric and magnetic fields. A characteristic of this device is that if you roll up a large electromagnet and plug it into the wall, it in no way affects the magnetic measurements inside the device; so one nice aspect is that you do not have to worry about a subject having a magnet in his pocket, because it would not help him any. Inside the magnetometer a magnetic field is set up which slowly decays with time. As it decays, you get something which looks like periodic output with a frequency of one cycle per 30 to 40 seconds (a very slow-moving wave). If for any reason you should cause the decay to either change or stop, the periodic frequency of the output will change or stop also.

We took Swann into the laboratory, told him where the machine was, and asked him to see if he could affect it. He was able to see whether or not he was affecting it by looking at the output on a chart recorder which was recording the wave in real time. Swann then, by his explanation, looked down through the floor into the apparatus; and when he did that the frequency doubled, or the period halved, for a period of about 30 seconds. What that would correspond to is a doubling of the rate of the magnetic field decay. The graduate assistant who had built the apparatus then asked Swann to try and stop the field from decaying altogether. Swann attempted to do this, and the output then showed a plateau, which would indicate that the field had stopped decaying. After about 45 seconds Swann said he could not hold it any longer, and then the wave went into its regular motion. When we asked him how it was that he was doing what he was doing, he started to describe the inside of the apparatus as he claimed to be able to see it, including the fact that he liked certain parts which looked as if they were made of gold. They happened to be made of a gold titanium alloy. And as he drew a relatively accurate description of the inside of the device, the effect that he had created before on the output occurred again.

This illustrates a principle that I would like to pursue in future research, which is that a gifted subject can cause effects simply by focusing his attention on a sensitive appa-

ratus without really trying to do anything. Further evidence of this followed. When we took Swann to the other side of the room and engaged him in conversation on other subjects, the output went back to normal. But then, as we started to discuss the apparatus and the experiment again, the chart recorder began to register perturbations. So we decided to find out whether this was an artifact or not. We took Swann out to lunch and told him not to think about the apparatus. During this period we did a control run which verified that in fact the apparatus was working correctly and free of noise or artifact. We returned the following day and did similar kinds of things with similar kinds of results. In summary, we were encouraged by our early exploratory work with Swann, and will be continuing to work with him in the coming months. We felt that our complex equipment approach was justified by the effects we had obtained, and that the additional information it gives us will enable us to explore the PK process more thoroughly than ever before. Geller is stimulated by complex equipment in much the same way that Swann is, and we hope to be able to pursue the same lines of research with him in the future.

#### APPARENT PSYCHOKINESIS ON STATIC OBJECTS BY A "GIFTED" SUBJECT

Charles Honorton (Maimonides Medical Center)

This and the following report concern a series of observations of apparent macro-psychokinetic effects produced by an individual subject, Felicia Parise. Parise is a research technician in the Division of Hematology, Maimonides Medical Center. She is highly regarded in her field and has coauthored a number of technical papers. Parise first visited our laboratory in the summer of 1969, when she participated as a subject in one of my clairvoyant hypnotic dream studies. Her hypnotic dream imagery corresponded to a striking degree with the clairvoyant targets. In her two pilot sessions as a nocturnal dream subject, Parise's dream reports corresponded not with the target pictures our agent was looking at, but with contemporaneous events surrounding persons close to her. We then asked her to participate in an eight-night experimental dream study in which she was more successful in incorporating our target materi-



al. Her blind judging of the dream-target pairs was highly significant ( $P < .0005$ ).

When Montague Ullman of the Maimonides laboratory returned from the Soviet Union with a film of Nina Kulagina, we invited Parise to a small showing. She was obviously deeply impressed, and thereafter set about quietly and on her own to see if she herself could produce movement in small objects. She tried getting into a relaxed, meditative-type state, or a state of anxiety. Neither worked. During this period, Parise was undergoing a great deal of stress because her grandmother was slowly dying in the hospital. The first object movement occurred one night after she had come home from the hospital. She had decided to try to move a small clear-plastic bottle and was just setting it up when the phone rang, with a message to return to the hospital. As she reached for the bottle to put it away, the bottle suddenly moved away from her. She did not mention her success to anyone until about a month later, after she had repeated it a number of times.

Late in the summer of 1971, while in Durham, North Carolina, I received a letter from Parise in which she reported success in displacing a small alcohol bottle, presumably by PK. When I returned to New York I was invited to her home for a demonstration. The alcohol bottle was actually a small clear plastic medicine bottle (59 mm. high and 34 mm. in diameter), filled approximately one-fourth with denatured alcohol. Parise used this bottle to preserve her cosmetic eyelashes. As we arrived in the kitchen, she placed the bottle on the formica countertop, approximately one foot back from the edge of the counter. She placed her hands on the edge of the counter, then silently looked at the bottle for two to three minutes. At that point she exclaimed that the bottle had moved. I did not see any movement. Parise was silent for another minute or two. Then the bottle moved one and one-half to two inches to my right and away from her. I then picked up the bottle and examined it carefully to be sure there was no moisture present and nothing attached to it, then replaced it on the counter to see if it would slide by itself. The bottle did not move. Later, Parise again placed her fingers on the edge of the counter. This time the bottle began slowly to move forward and to my right, in a curved trajectory. It stopped and started again three times and finally (after reaching a distance of approximately four and one-half inches from its starting position) reversed direction, returning toward me,

and then stopped.

The stopping-starting-reversing behavior of the bottle did not fit well with my sliding hypothesis. I spent more than half an hour examining the counter, drawers, etc., and became pretty well acquainted with Parise's kitchen. I attempted to repeat the movements through a variety of normal means. I tried pressing gently and firmly against the sides, top, and underside of the counter; I forcibly jarred the counter top; I moistened the counter and the bottle by spilling some of the alcohol solution. I was completely unsuccessful at getting the bottle to move. Later, I took a carpenter's level to her counter and found that it was in fact not perfectly level; it inclined slightly to the right. The bottle had apparently been moving into the incline. During the following months I had occasion to repeat these observations, under the same conditions, a number of times. Parise became successful at deflecting the needle of a small pocket compass. In working with the compass, she would frequently place her hands, cupped slightly, à la Kulagina, around the periphery of the compass, about six inches over its surface. Often I would unexpectedly pass her hands directly over the face of the compass to insure against concealed metallic shavings, etc. In no case did similar movements occur when I did this.

PK attempts in the laboratory were always a strain for Parise, and she was not as confident there. While she never succeeded in moving the bottle in the laboratory, she did succeed on a number of occasions in producing good compass deflections. On one occasion, while she was in my office, I tried to coax her to "zap" my own compass. She was in a hurry to leave, and did not want to get "worked up" for PK. However, I prodded her, and as she stood about 18 inches from the desk on which the compass lay, she waved her arm in the direction of the compass and jokingly said, "Abracadabra!" The compass needle immediately deflected 90 degrees. We were both surprised by this, since it did not involve the usual Kulaginesque agitation; the incident remains as a solitary case in which it appears that the stressful state is not a necessary condition for PK. I should add that I have never had the slightest reason to suspect Parise's honesty or good faith. She has always cooperated to the best of her ability, realizes the need for controls, and is probably her own most severe critic.

Parise says that everything moves away from her.

This has been the case on every occasion in which I have observed movement. She prefers certain objects--especially nonmagnetic objects--to others. This is because she was sensitized to some of the criticisms leveled against Kulagina and other producers of physical phenomena. She describes herself during attempted PK as trying to develop rapport with the target object. Before she begins, she says she focuses her attention on the object until "that's the only thing there." She usually picks a spot on the object and fixates on it until everything else--including the counter--seems to disappear. She speaks of "pitching," that is, working up excitement to the point where, in her own words, "I want to make it move more than anything else." After a successful session, Parise experiences difficulty speaking for a few moments. She says that she understands what is being said to her and knows what she wants to say, but that "it doesn't come out right." She perspires freely during the session, which is unusual for her. Other physical after-effects include running eyes and nose and trembling. Her only unusual physical characteristics we are aware of are chronic phlebitis in the left leg (which, coincidentally, has also been reported in Kulagina) and hyperacute vision.

How long does it take for movement to occur? She says the quickest movement occurred here in Charlottesville in the summer of 1972 when we stopped in on our way to Durham. Gaither Pratt showed us his films of Kulagina, and Parise became very excited. She obtained some movement with Gaither's compass almost immediately. The longest period before success was later that week with Graham Watkins in the FRNM laboratory, when the compass needle deflected only after a lengthy (and very hot) two-hour session, described in the following paper. Parise feels she was overly worked up on this occasion and "overshot the level necessary." Her success came when she was on the verge of giving up; when, as she says, she began to "come down." The last time Felicia Parise attempted PK with observers present was in October 1972, back again in the kitchen. I was there, and the bottle moved almost immediately, covering a total distance of six inches. Parise has since decided to discontinue PK work because, as she says, "It took all of my spare time. PK is something you have to do every day. It's more than just putting it on your schedule. I did it," she continues, "to satisfy myself. I have done that, and I am not the kind of person who can withstand constant criticism."

APPARENT PSYCHOKINESIS ON STATIC OBJECTS  
BY A "GIFTED" SUBJECT: A Laboratory Demonstration

Graham K. Watkins<sup>†</sup> and Anita M. Watkins (Duke University)

There has been a recent surge of interest on the part of experimentalists in the ability of certain persons to move static objects without known physical mediation, long considered the province of the analyst of spontaneous case reports. This interest has resulted primarily from the phenomena reportedly produced by Nina Kulagina in the U.S.S.R. Since then, several other persons apparently able to demonstrate this sort of effect have come to the attention of researchers. We had the opportunity to work briefly with one of these gifted individuals, Felicia Parise, when she was visiting the FRNM in the summer of 1972. We were interested not only in the phenomenon itself, but also in how it compared with the effects we had seen in our mouse-ether experiments, which we believe resemble this type of phenomenon more than they do the standard dice-electronic machine type of PK. In these studies, reported at two previous Parapsychological Association Conventions, selected subjects were consistently successful at reviving by PK one anesthetized mouse (target) before another (control).

In her first attempt to demonstrate her ability for us, Felicia Parise was located outside the room that contained the experimental setup; she attempted to move a small bottle which she could see through a one-way glass. The bottle was located in the field coil of a metal detector, and packets of film in opaque envelopes were located under the metal detector and at approximately one, two, and three meters distance from it. The film used was Royal-X Pan black and white, ASA 1000 (four by five inch). Parise was connected to a polygraph that monitored occipital EEG, EKG (Lead II), finger plethysmograph, and respiration. Both movie and still cameras were set up to record the phenomenon, should it occur. When no movement was obtained with the bottle, a compass was substituted as the target object, but again with no success. After a short rest period, Parise requested that the glass barrier be removed. The setup, including one pack of film and the metal detector, was then brought out and placed on a chair immediately in front of her. The other three packs of film were moved correspondingly. The compass was tested for proper operation at several points around the room; it indicated approxi-

mate north, and was easily affected by either a small bar magnet or a steel knife blade.

As Parise concentrated on the compass, her movements became so violent that the head electrodes were ripped off, thus rendering the EEG measurements useless; only portions of the other physiological recordings were usable. We were able to note increases in heart rate (about 15 percent above baseline) and decreases in pulse amplitude (down to about 75 percent of baseline) which were comparable to those seen in the subjects who were successful in the mouse-ether experiments. Neither EKG t-wave nor respiration could be accurately determined because of her activity. When the movement of the compass needle was finally accomplished, it was accompanied by a change in the sound frequency produced by the metal detector, whereupon a total shatter of the tone occurred; this could be artificially simulated only by placing a very large metal mass (a two-pound roll of solder) in the field coil. The compass needle moved westward approximately 15 degrees over a period of about two minutes, with a slow, smooth motion, and remained there. Unfortunately, the movies we took of this were later discovered to be useless, because Parise's head had come between the camera lens and the compass.

About five minutes after the first indication of compass needle movement, Parise was disconnected from the polygraph. She walked to a far corner of the room. The compass needle, however, remained 15 degrees off north, and was found to be totally unresponsive to either the knife blade or the bar magnet. We thought that perhaps the needle was jammed. To test this, the compass was moved to a position about four feet away from the point of concentration, and during the movement the needle gradually returned to north. In this position it was easily affected by the knife blade. The compass was then returned to the original spot on the chair, and again the needle moved 15 degrees off north, and was incapable of being influenced by the metal blade. This procedure was repeated several times with the same results. The needle gradually returned to north over a period of about 25 minutes, and also gradually became more responsive to the knife blade.

It is of great interest that the "recovery period" for the compass needle was very similar to the period required for the dissipation of the "linger effect" in the mouse-ether experiments. We had found that if we ran eight trials with

the mice, then had the subject leave the room, and while the subject was gone continued to run eight more trials with two new experimenters who did not know what the target side had been, the mouse on the side the subject had been concentrating on would continue to awaken first, just as if the subject were still there concentrating. We had also found empirically that it took about 30 minutes for this "linger effect" to dissipate. A similar effect has been reported for the key-bending phenomenon apparently produced by Uri Geller (i. e., the key continues to bend after Geller has stopped concentrating on it). It would be interesting to know what Geller's physiological state is during these performances. A final effect we noted in this session with Felicia Parise was that the film immediately under the compass was almost totally exposed, and the other three packs of film were partially exposed, the amount of exposure decreasing in proportion to the distance between the film and the compass. We found the same phenomenon later in the mouse-ether experiments during periods of PK success, although it was less striking than with Parise.

#### PK ON STABLE SYSTEMS: SOME COMMENTS

R. A. McConnell (University of Pittsburgh)

In the course of this symposium the chairman asked me whether I would like to add a comment at the end. As a physicist I find it difficult to grasp fully what we have just seen and heard about gross psychokinetic phenomena. I have some of the same feeling that I think must have come to those who witnessed the first atomic explosion--although, of course, the analogy is a loose one. Instead of trying to evaluate the present situation, I shall give you the perspective in which I see it. I entered parapsychology in 1947 and since then have managed to live peacefully at the University by staying in the laboratory and avoiding publicity. Some years ago a highly regarded member of our Physics Department, with whom I have always had friendly relations, let it be known to the administration that it was disgraceful that research in parapsychology should be allowed to go on at the University of Pittsburgh. Fortunately for me, those officers who had to respond to this opinion did not see it that way. Much later, after word of the incident had reached me, I

happened one day to overtake this faculty member walking on the street. In the course of our conversation I asked him whether he had read any of my papers. No, he had not. What was his feeling toward the possibility of psychokinesis? He answered: "If such an effect were established, it would be, for physics, the greatest discovery of this century--or any other."

The question that many of us are asking is this: "Are the gross PK effects that we have heard discussed and seen on film here today real or fraudulent?" This is the same question that other scientists have been asking for 40 years about ESP. The answer depends not upon the evidence in the journals, but upon the honesty and judgment of those who gathered that evidence. These latter factors can be assessed only by getting to know the experimenters intimately. In a preparadigmatic field of science every newcomer must repeat for himself the basic experiments because there is no generally accepted body of belief--nothing he can take on faith. Soon after I entered parapsychology, I guided an experiment in wishing with dice that was not very dramatic but that did yield results that I could not reasonably explain by ordinary means. Also, I had studied successful experiments done by people whom I later came to know and trust. Consequently, for the last 20 years I have been fully convinced of the reality of the psychokinesis of falling objects. Although this is a weak kind of PK that might be explained in terms of controlling existing physical energies, it has forced me to remain open-minded toward the more dramatic physical effects that were common in psychical research in past generations and that now in recent years seem to have reappeared.

When the Kulagina film became available, I examined it in detail in its total social context as far as that was known. As a result, last winter when I taught a course in parapsychology for the first time, I told my students it was my belief that we must assign a probability rather near to certainty that Kulagina's effects were genuine. On the basis of what we have seen here today, I think we now must prepare to accept the fact that there is no reasonable ground for denying the ability of some people to change a stable physical system, even when that system is isolated against such change as far as present knowledge in physics can determine.

The work from Stanford Research Institute is impres-

sive. Although I have had no opportunity to become personally acquainted with Russell Targ and Harold Puthoff, I think we must all agree that the quality of their presentation was excellent. For me, Charles Honorton's work is especially convincing. Its strength lies in the personal relationships established among experimenters and subject, where the senior experimenter is widely known for his conservative experimental publications in parapsychology. The establishment of the psychokinesis of stable systems will have serious implications and will raise new questions. It will destroy our world view and the world views of scientists generally. We are, it appears, at the beginning of new relationships between the Parapsychological Association and the rest of science.



## CONCEPT AND PSI\*

Rex G. Stanford

I attended my first Parapsychological Association Convention eleven or twelve years ago, and from that experience I could not have guessed what parapsychology would look like today. Even the casual observer of the last decade of parapsychology will recognize that our methodological progress during that period has been great. There have been many changes in the tools we employ, and somehow there is an atmosphere of much greater promise in what we are doing. An appreciable part of the greater promise derives from our increased methodological sophistication and diversity which have affected both how we do our studies and the topics we are studying. Increases in anpsi and in PK work using very efficient methodology are just examples.

However, the conceptual advances of parapsychology have lagged far behind the methodological ones, and to such a degree that this has decidedly held back our progress. This is true even though some of the methodological changes are fostering new conceptualization. Let me explain this further and then propose solutions and directions in which we can move to extricate ourselves from the conceptual void, or occasional chaos, in which we have sometimes found ourselves.

One of the basic difficulties with which parapsychology has historically been burdened is that we have had few if any explicit hypothetical constructs and have had all too many implicit and often unrecognized conceptual biases. This claim is well demonstrated in the most common terms of our field: ESP, telepathy, clairvoyance, psychokinesis, position effect, salience effect, etc. Let us examine some of these terms as a starter. If we attempt an operational defi-

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\*This is the Presidential Address, given September 6, 1973.

nition of telepathy which will differentiate it from clairvoyance, we find that while we can create such a definition, it has a restriction on it which we would not want in practice--that telepathy could not, in terms of its definition, occur in the presence of objects or events which could serve as targets in a clairvoyance paradigm. What is the problem here?

The problem is that both the operational definitions of telepathy and clairvoyance have an unsatisfactory negative aspect. We cannot be sure it is "telepathy" unless we exclude the kinds of stimuli generally associated with the "clairvoyance" paradigm and vice versa. And even if this did not, for some reason, bother us, there would still remain the problem that even with no supposed clairvoyance objects around, "telepathy" might be simply clairvoyance of brain states.

It happens there is a much deeper problem. The attempt experimentally to separate "telepathy" and "clairvoyance" is not meaningful unless these terms are hypothetical constructs, which as they have been traditionally used they are not. We note that person x has experienced a certain internal state and that person y (perhaps a close friend) has simultaneously and inexplicably also experienced the same internal state. We label this telepathy. The term is a summary term. It means that we have several classes of observable facts and no explanation for them is available. If the term telepathy were a hypothetical construct rather than simply a summary term, it would indicate other events which should be observable when the state-report correspondences of persons x and y occur and which would not be expected to occur if the term "clairvoyance" were applicable. In short, our term "telepathy" is not a hypothetical construct, but is a simple summary term for certain observed events.

It should follow that we can never hope experimentally to differentiate telepathy and clairvoyance until the terms themselves are clarified and turned into hypothetical constructs. Then the relevant question will be, "we are observing certain antecedent events and consequences; are there certain additional events intrinsically associated with this situation which we can observe which will tell us whether what we are observing best fits the construct telepathy or the construct clairvoyance?" The attempt to do pure telepathy or pure clairvoyance experiments in the hope of isolating these supposed functions is meaningless as the terms now stand. Yet this kind of effort derives from the inadequacies

of how the terms are currently used. With further thought and experimentation we may even decide that our constructs should not separate the supposed classes of events involved in "telepathy" and "clairvoyance" or that some of the events now labelled "active-agent telepathy" may be better conceptualized along with PK events than with ESP. I specifically suggest that we will find remarkable parallels between mental states optimal for "active-agent telepathy" and those optimal for PK.

The term extrasensory perception, or ESP, is a conceptual curiosity. If taken literally, it sounds like it might be a hypothetical construct, and in any event the term has a definite conceptual bias. It suggests that the events we have labelled with the "ESP" term are primarily perceptual in nature. Even from the most common kinds of spontaneous cases, however, we know that ESP sometimes takes a purely cognitive form, the hunch or intuition. Then we have the motor automatisms (automatic writing, pendulum, planchette) as occasional expressions of so-called ESP. I would suggest there are many other ways in which psi-mediated response to external events gets expressed. (I discuss these at considerable length in papers on psi-mediated instrumental response to be published in 1974 in the Journal of the American Society for Psychical Research [Stanford, 1974a; 1974b].) Thus the term extrasensory perception is curious, indeed. Once we recognize that this psi-mediated response mode does not necessarily involve perception, we can eliminate the term "extrasensory" and perhaps substitute the word "psi," as in the term I have proposed in the papers just cited, psi-mediated instrumental response (or PMIR).

The term psi does not, in itself, so overtly stress the negative side of the phenomenon (or phenomena) in question. The juxtaposition of the terms "extrasensory" ("not of the senses") and "perception" is ironic, and has troubled some parapsychologists--for example, W. G. Roll, who denies that ESP is directly analogous to perception (Roll, 1966). He makes a case for its finding its cognitive expression through the elicitation of memories based upon sensory events. I feel that the whole concept of what we have been terming ESP would be more palatable, particularly to behavioral scientists, if it did not directly make the claim that perception occurs other than through sensory means. Also, the term "extrasensory" seems to claim that the basic nature of ESP is one of obtaining knowledge, a cognitive function. I am not entirely sure this is the case, and

neither are at least a few other parapsychologists with whom I have spoken.

There are parapsychologists who seem to have taken extrasensory perception somewhat seriously as a hypothetical construct. Louisa Rhine, for instance, suggests in her writing that forms of extrasensory expression other than perceptual ones (visionary or dream ones) are manifestations of blocked extrasensory information (Rhine, 1965). Other parapsychologists have felt a similar inclination. Thus it is strange that no one has, until quite recently, done work which directly asks whether the so-called perceptual form of ESP shows the same characteristics as, for example, visual perception. As you will hear ... [see p. 90] Ed Kelly is doing just this, a most laudable endeavor. We may soon learn whether the concept "extrasensory perception" is sometimes a proper one for the kinds of tasks we often use in laboratory studies. It seems to be, in certain spontaneous cases. With respect to the construct "extrasensory perception," then, I am suggesting it may have limited applicability that needs much further exploration.

As for the term "psychokinesis," there is not time to comment on it at proper length. Suffice it to say that the term as traditionally used and defined does not seem properly to describe the functions of such events as we have studied them. The conscious, willing aspect has been over-emphasized in the definitions, whereas the strongest manifestations of such events, as in poltergeist cases, seem not to involve conscious effort at all. Further, the term has unnecessary dualistic biases. (I will consider these matters at more length in my published account of the psi-mediated instrumental response model.) Terms like "position effect," "salience effect," or "variance effect," like a number of other "effect" terms in parapsychology, give themselves away as summary terms wholly or almost wholly devoid of any hypothetical import. Many such terms have existed for years and yet have seldom been upgraded into explicit hypothetical constructs. J. B. Rhine has long felt that some of the position effects within the standard ESP run are related to subjects' cognition of the structure of the run (Rhine, 1969), but such effects have all too seldom been studied in the light of particular hypotheses. Had they been so studied, we might now have new names for some of the effects, and we might have been able to regard them simply as particular manifestations of broader forms of psychological lawfulness in forced-choice ESP performance. Instead, there has

been a tendency to treat them as purely "effects," merely additional kinds of evidence for the occurrence of one or another psi phenomenon.

Let me summarize what I have said and illustrated to this point. Many of the terms of parapsychology and much of the published research reflect a lack of conceptualization or sometimes a prevalence of implicit conceptions or preconceptions which are often not sufficiently recognized that they are subjected to experimental scrutiny. We have already considered some of these preconceptions and will consider more later. We have few hypothetical constructs, few conceptual models, and no real theories. There is a tendency for the dogged pursuit of small probability-values and the almost endless reporting of the anomalous observations called "effects." These we have instead of the conceptualization which could result in incisive experimentation involving the manipulation of independent variables. Nor can the application of electronic and computer technology alone rescue us from the retarding effects of such conceptual lapses. There are simply no substitutes for carefully planned experiments built around clearly-defined hypotheses and the development of programmatic research in particular, well-defined areas. We are seeing considerably more of these today, but still too little.

One of the most basic causes of our conceptual shortcomings may be that there has been too much concern with gathering supposed proof of the reality of psi phenomena and too little concern with studying the processes involved. Most of you are well aware of the historical reasons for this situation, even if you may not agree with me that the attempt to arrange foolproof demonstrations of psi occurrence is foolhardy. The attempt to give a "conclusive" demonstration has held back this field as much as anything else. It has held it back not only because such efforts have been substituted for process-oriented research, but because such efforts are logically ill-conceived and actually play into the hands of the visceral variety of critic of parapsychology. The thought of trying scientifically to validate a proposition like, for example, "There exists extrasensory communication of thoughts" or "There exists extrasensory communication of the faces of cards" boggles the mind. This kind of proposition is essentially an existential proposition--"There exists ... (such and such)." It says nothing of the functional characteristics of the phenomenon supposed to exist. It only tells us we should observe something which could be

recognized or construed as something which is itself negatively defined. It tells us we should be able to observe a certain class of events which, if observed, is to be construed in a particular preordained way, but in a way which is only negatively defined (as not being mediated through the senses, for example). Such a proposition reveals nothing about the conditions under which we should or should not be able to observe the class of event in question or about what positive characteristics the class of event should have. Thus it tells us nothing about which kinds of functional relations should affect the quality or magnitude of the event in question. Therefore it is a mind-boggler of the first order, and it is little wonder that E. G. Boring saw this as the fatal flaw in parapsychological thinking.

It all hinges on this: how can you empirically test a proposition which itself gives you no clues as to when the crucial observation will or will not occur? Such a proposition is not capable of experimental demonstration because it is incapable of experimental falsification. We can observe anomalies, but such observation only proves we can observe anomalies, which no one questions anyway! Parapsychology and psychical research got off on the wrong foot in trying to demonstrate ESP as an isolated event. One can test the validity only of constructs regarding the observations we label psi phenomena. In short, we should treat "ESP" as a specific, delineated construct and thus deliberately develop it and study it as such--and change the construct as necessary.

No amount of historical justification can extricate us from our conceptual crisis. Nor are we saved from this crisis by the oft-heard argument that we were bound to have to offer "proof and more proof" of our phenomena because the phenomena "contradict the most fundamental laws of physics." Laws are never "contradicted," nor can they be violated like one of the Ten Commandments--God forbid! What research can do is to explicate further the boundary conditions which hold for certain particular lawful relationships. It can also establish new classes of interactions. Further, in the present stage it is doubtful whether parapsychological findings could modify physics in the proper sense just indicated. Parapsychological findings are not precise enough at the moment to do so, and our phenomena are not really studied, in most current instances, in a framework in which the findings even match the units of physical equations. However, there seems little doubt that

findings regarding psi phenomena will one day have something to say to physics, and vice versa.

There are a number of secondary reasons why parapsychology lacks suitable conceptual development. One is that much of the research, sometimes termed process-oriented, seems to have been aimed at either simply producing a marked or strong scoring trend of some kind or simply successfully predicting a difference of some kind between two groups of subjects or test conditions. Seldom has research been carried beyond this point. Seldom have researchers moved beyond finding an initial effect to ask how or under what specific conditions the effect comes about. Seldom have researchers looked at functional relations other than on a two-point basis, such that the shape of functions almost always remains indeterminate. The explanation for these failures may at least in part be that researchers have not made a clear-cut decision for a full program of process-oriented research. It may often have seemed sufficient to produce more evidence for the occurrence of ESP, provided that evidence seemed to make some suggestion regarding process. Also, there have been too few workers and too little funding.

Correlations of psi performance with such things as attitude or personality variables have been some of our major findings, but the valuable leads from these areas have not been followed up to enable us to make inferences about causal factors. This level of analysis in our experimental work is much better than nothing, but it is not the kind of thing which will make neutral or hostile scientists from other fields take notice of us. This level of analysis must be transformed into a much more rigorous level of analysis if we are to move parapsychology in the direction of a nomothetic science. But more on that later. Another retarding influence has been that we seem too often to have sought a kind of security, in our process-oriented work, in falling back on certain simple, but "tried and true" (yet sometimes none too reliable) psychological measures designed, initially, for purposes quite different from our own. We should acknowledge that we are concerned with areas of psychology in which we shall have to break some new ground and that we probably will have to make some solid contributions to emergent areas of psychology such as states of consciousness, perhaps to psychophysics and a number of other areas.

Certainly the interface between psychology and parapsychology is great, and thus there is much we can learn from psychology and also contribute to it. Indeed, we have not learned enough, for we have only made use of a small portion of psychological knowledge and theory in structuring our research. We have been unnecessarily selective. We have depended too exclusively on personality-clinical tools. There are at present signs that this is rapidly changing. We will find that to solve many of our own problems we shall have to venture out in developing new practical and conceptual tools of a specifically psychological sort. If we do this, we will not only advance parapsychology, we will contribute to psychology very directly, and it should do much for our scientific credit, not to mention our credibility.

Another retarding influence is that some parapsychologists have been very shy about publicly attempting to conceptualize psi phenomena. To put into print a conceptual model or a low-level theory would appear to be perceived as risky business. Just being in parapsychology is risky enough, some would say, and if we engage in such flights of imagination, we are apt to be "shot down." To that I would reply, "When you ain't got nothin', you got nothin' to lose." I would also add that those fly highest who are not afraid of the trip back down. It is this absence of conceptualization for which many scientists have criticized parapsychology. I have said enough of our problem--a lack of conceptual development--and its presumed causes. To be constructive, I must now show some solutions to the problem. I am happy to note that I can already see marked progress in some of the directions I will suggest.

## SOLUTIONS FOR THE PROBLEM

### More Process-Oriented Studies

The most obvious immediate solution is for us to be sure that each study we do has at least some aspect which potentially could tell us something about process. This is happening more and more nowadays because researchers are becoming reluctant to waste time doing or writing up a study which would merely provide more evidence of a significant deviation from mean chance expectation. A study of the latter kind would provide no increment in knowledge.



### Consider Carefully Our Level of Analysis

Much of the psychologically-oriented ESP research has involved a level of analysis too far divorced from what is actually happening inside the subject as a function of his involvement in the ESP test. This probably explains, at least in part, the rather low replicability of the personality-ESP work and the none too great, though appreciable, replicability of much of the work on psychological or psychophysiological correlates of ESP performance.

Probably most parapsychologists would today agree that it is naive to think of certain personality traits being directly or intrinsically correlated with ESP performance level. I have not done a poll, but it is difficult to see how anyone even moderately familiar with the literature could feel otherwise. Most of us would agree that individual difference measures interact with such factors as type of ESP task, the task-set the experimenter gives the subject, and the personality or demeanor of the experimenter. There may even be third or fourth-order interactions of importance in such connections. To make it worse, there has been little systematic study of the role of the experimenter in ESP work (Honorton, Ramsey, & Cabibbo, 1973). Someday someone may untangle this rope of many strands and show us what it all means. But most of you may agree that the situation just described is unsatisfactory. The road toward a nomothetic parapsychology cannot be paved with third- or fourth-order interactions. But what shall we do about this?

First, we should develop well-thought-out models to describe the relevant functional parameters for specific, well-delineated aspects of ESP-test situations. We can make informed guesses about these parameters on the basis of our knowledge of the literature and our own feelings for what happens in ESP-test situations of this kind. Then we can do experiments to test and refine such models. Sometimes we may be able to borrow or adapt models from specific areas of the behavioral sciences. We might be able to benefit, as I have been doing in recent research, from ideas in modern psychophysics such as certain signal detection models; or perhaps from perceptual or cognitive psychology. Whatever we do, the emphasis should be on making our level of analysis more specific, simpler, more molecular, if you will. We need to develop highly specific parametric models for specific well-defined problem areas relative to a particular kind of ESP testing.

The second point concerns the things such models should be concerned with. We must analyze carefully and study incisively what is actually happening or could realistically be expected to happen when a subject actually takes an ESP test (or a PK test, for that matter). This is not a call to bring in so-called individual differences or personality traits. Our need is to know what kinds of thinking, choosing, and experiencing each and every subject will experience in the task at hand even though there may be individual differences in these respects. Let us develop manipulations and measures of the specific aspects of the processes which our models suggest are relevant to the end-product of ESP testing. In so doing, it is important to recognize that ESP does not make its appearance on an open stage or tabula rasa. It interacts with ongoing or readily available mental processes or content. We shall have to try to understand these interactive processes in specific situations and learn how these interactions influence choices, perceptions, behaviors, or attentional factors in which we are crucially interested. Thus several of my research projects in the past few years have studied the interaction of extrasensory influences with and upon specific memories, associative connections, etc. These are the contexts in which ESP makes its appearance in life situations.

This proposal sounds complicated, but in essence it is simple. Let us study what actually happens when individuals are put in a situation in which they are asked to use, or implicitly have to use, their ESP. It really comes down to studying and conceptualizing what is going on in the heads of subjects as they make use of ESP in particular contexts. To the degree that our manipulations can control or our tests measure what actually happens then, at that specific time--whether in general or on a trial-by-trial basis--we will be at a much more sophisticated and powerful level of analysis than if we are concerned with personality traits or even such things as attitudes.

This level of analysis is conceptual, relatively molecular, and is aimed at processes closely related to the ESP test-taking itself. It is not content with relationships such as that attitudes or beliefs of a given kind or personality factors of a given kind (perhaps in interaction with other factors) are correlated with ESP performance. It goes on to ask why, for instance, belief in the possibility of ESP occurring in the task at hand should have a positive influence on performance. It seeks to discover possible causal

relationships which will elucidate how the observed correlation comes about. The approach assumes that if we can find how effects reflected in correlational or low-level experimental studies are mediated, we shall find ourselves with a much greater degree of replicability and perhaps strength of effect than we have heretofore observed. It attempts to analyze any second-order effects to reduce them to first-order effects. If, for instance, attitudinal effects are mediated through the effects of differing response strategies, we should study such response differences and see whether they do not yield a higher level of replicability than do attitude measures.

This approach aims at a level of analysis very close in time to or concurrent with the psi-mediated response. Ultimately the individual ESP response should be the centering point for our analysis. The approach assumes that, psychologically speaking, psi phenomena are lawful, that a nomothetic science is possible, that we can discover reliable, powerful regularities in psi function which cut across individual and attitudinal differences--provided we aim at a proper level of analysis.

I feel a careful survey would show that the experiments which most closely approximate the recommended level of analysis are the most replicable experiments we have in parapsychology. In the areas in which considerable process work has been done, I would subjectively rank the degree of replicability as follows from lowest to highest: personality work; attitude work; state-of-consciousness studies; and work on response factors. This same ordering would seem to obtain if we consider the degree to which the variables studied approximate the suggested level of analysis. It may also be of interest to note that in psychology in general we note the same correlation between level of replicability and the degree to which the level of analysis approaches that suggested. This suggested level of analysis is an ideal, but we must sometimes begin at a less appropriate level in certain areas of study.

Recordings of such things as spontaneous EEG rhythms during and/or immediately before ESP testing do not represent quite as great an approximation to this recommended level of analysis as might at first seem the case. EEG work will more nearly approximate this level of analysis when we are studying EEG response to specific aspects of the test-taking endeavor, when these responses are

measured against the individual's own baseline, when the EEG measures are used with respect to relatively small units of ESP measurement, and when EEG indices are used as functional indicators of attentional factors or as indicators of other information-processing features of brain function. By "functional indicators" I mean using EEG indices as measures of how the brain is responding to or failing to respond to, for example, extraneous sensory inputs during the ESP testing. Evoked potential may be one approach, blocking or shifting of spontaneous rhythms, another.

Within the current EEG-ESP literature there is evidence of greater replicability in those studies which have involved specific measures of EEG response to ESP test-taking (e.g., alpha frequency shifts) than in the work merely involving spontaneous rhythms during the test. My guess is that by approximating the level of analysis suggested, our findings will have both greater replicability and optimal generalizability across a variety of different experiments.

#### More Boldness in Constructs, Models, and Theories

It has often been claimed that we are at a "preparadigmatic" stage of our science. Acceptance of this proposition could conceivably reduce our sense of responsibility for trying to integrate the findings we already have. A major value in well-articulated constructs, models, and testable theories is in inspiring, inciting, and guiding new, imaginative, incisive, and systematic research. They also make us think about and make explicit--or perhaps even question--what was implicit or perhaps vague and poorly articulated. Those persons who are shy about putting forth new constructs and theoretical ideas might benefit by examining the applicability to parapsychology of ideas from other sciences such as psychology. In fact, all of us might benefit from that and we might at the same time increase our acceptability among psychologists or other scientists whose ideas we apply. In recent years a considerable portion of my work has been built around ideas derived from modern psychophysics, specifically from signal detection theory. Signal detection ideas can very likely subsume most of the findings from the so-called random behavior trials in ampsi research. So far these findings have not been conceptualized by those reporting them. The impact of this seemingly highly replicable work on nonparapsychologists would be even greater if it were conceptualized and particularly if the findings could be subsumed under current psychophysical constructs.

Use of Construct Validation and Conceptual Replication

Let us consider construct validation via a specific example. The term "extrasensory perception" seems to be a half-baked hypothetical construct which suggests that extrasensory knowledge always manifests, or at least seeks manifestation, in perceptual ways analogous to sensory information. One can design and perform experiments to see whether this construct is valid.

Among the things required would be evidence that extrasensory information manifests characteristics analogous to sensory perception--that, for instance, visually representable material would show the same characteristics when received extrasensorially as when received through ordinary vision, perhaps tachistoscopically. There might be similar confusions, illusory effects, contrast effects, figure-ground shifts, etc. Additionally, this construct, if it is intended to subsume the whole range of effects called extrasensory, seems to imply that the organism cannot make effective use of extrasensory information except to the degree that such material attains a perceptual form of expression. If evidence could be obtained that such information can function effectively in the organism without there being a perceptual representation of the extrasensory material, the construct would have to be questioned. In my opinion, there already exists abundant evidence that the construct "extrasensory perception" is not valid if it is construed in the broad sense indicated above. Indeed, the unexamined belief that extrasensory function is basically perceptual in nature has probably impaired the advance of our science as much as any single factor. However, a perception-like mediating vehicle for psi may very well have validity as a subconstruct under some broader hypothetical construct such as psi-mediated instrumental response. Other constructs or quasi-constructs in parapsychology should be similarly examined in the light of present knowledge or new experimentation.

The issue of replicability in parapsychology is more often seen in the context of providing further proof for psi than in the context of understanding psi. It is in this latter context that I wish briefly to consider replicability. Specifically, I will consider conceptual replication. Hypotheses typically tested in parapsychological experiments can be regarded as concepts concerning the function of a given psi ability within certain boundary conditions. As such, the hypotheses should have some generality beyond the specific

experiment being done. One does not truly have a hypothesis, in this sense, when one's preexperimental expectation is merely an intuitive (or even an empirically-based) expectation that in the experiment at hand a particular effect will occur. A true hypothesis has more generality than this because it specifies the particular aspects of an experimental situation which are regarded as influencing the dependent variable. If the hypothesis is complete it also stipulates something about the domain of conditions under which such a functional relationship (of independent and dependent variables) should and should not obtain. A full-blown hypothesis can thus be tested both with respect to its validity and with respect to the accuracy of the specification of the boundary conditions. In practice, we most often state a hypothesis with vague or only partially specified ideas about its boundary conditions, and we hope to explore those in further research. If the hypothesis is part of a network of assumptions, as in a model, or especially in a theory, it should have the expected boundary conditions more clearly stated.

A hypothesis of the kind being discussed can and should be tested under a variety of experimental conditions. Experimental confirmation of the hypothesis under a considerable variety of conditions--that is, in a number of somewhat diversified experiments--gives a strong indication that the hypothesis is accurate, that the antecedent conditions presumed to affect the dependent variable are in fact the conditions which do just that. This strong confirmation derives not merely from an accumulation of a number of instances in which the hypothesis is confirmed. That would provide relatively weak confirmation, mere confirmation of the existence of "an effect," the kind of confirmation which is obtained through an exact replication of the study. Rather, the strong confirmation derives in large order from a convergence of evidence obtained under diversified experimental conditions. Many things vary in the sequence of different experiments, but what remains constant is the manipulation through a number of different means, of the specified independent variable and an observed, stable effect upon a measure of the dependent variable. Thus we can become relatively sure that what is producing the effect upon the dependent measure is the specific independent variable being studied.

One value in this approach derives from the fact that, especially in psychological experimentation, and perhaps in parapsychological experimentation in particular, the simple,

ideal model of the controlled experiment is seldom fully realized. The ideal model is to change a single variable in one experimental condition and leave it unchanged in the other, with all other factors constant between the two conditions. We can try to do that, but in psychology it is very difficult to be sure that we manipulate only one variable at a time. When we deliberately manipulate a variable, we may be inadvertently manipulating others as well. Thus a major value of a program of conceptual replication is that the conclusion ultimately transcends the vicissitudes of the individual experimental designs. When, in many areas of psychology, we use only a single experimental design to test a hypothesis, however many times we successfully replicate the study, the confirmation of the hypothesis may remain weak. This confirmation is strengthened through conceptual replication in which evidence from diverse experimental designs converges on a conclusion. For the purposes of a conceptually sophisticated and process-oriented parapsychology, conceptual replication is of the utmost importance. Simple replication alone is of minimal value.

Conceptual replication has advantages beyond confirmation of a functional relationship. It establishes something about the generalizability of one's finding and it encourages exploration of and awareness concerning the boundary conditions for one's hypothesis. It allows and encourages testing the full ramifications of a hypothesis. We are beginning to see effective use of conceptual replication in parapsychology. Its importance and potential are nowhere more clear than in the work of Charles Honorton on states of consciousness and ESP performance (Honorton, Davidson, & Bindler, 1971; Honorton, 1972; Honorton, Drucker, & Hermon, 1973). Thus Honorton's work will serve as a good illustration of the power of the method in a difficult research area.

The considerable variety of specific manipulations of state of consciousness which have been studied by Honorton and which have yielded the same effect on ESP performance helps to assure that it is what the manipulations produce in common that is responsible for the effect observed in each. What they seem to produce in common is the inward-directed, non-distracted, calmly observant state which Honorton posits as psi-conductive. Thus, instead of repeating a single experiment again and again, Honorton has adopted the much more informative and conclusive method of using conceptual replication to validate his constructs regarding psi-conductive states. In this process Honorton has included convergent

measures of internal state which include state reports from subjects--a healthy break from the confines of behaviorism. As a consequence of this work we now have some rather firmly-grounded generalizations about ESP and states of consciousness.

We can now say with some confidence that when a certain internal state exists in an individual attempting to use his ESP, his ESP performance is apt to be boosted more or less without regard to the specific set of conditions which brought about that internal state. Note that in his studies of psi-conducive states, Honorton has at different times used free-response and forced-choice ESP tasks, in addition to various methods for inducing the psi-conducive state and measuring its magnitude. Thus the generality of his findings is increased.

#### Consider the Generalizability of Hypotheses

If hypotheses are stated in the way I suggested earlier and if they aim at a suitable level of analysis as suggested earlier, they would seem to hold more promise of generalizing across experiments and perhaps across species. It is encouraging that results deriving from response factors in animals seem to have some cross-species generality. There has been no formal review of this topic, but my information suggests this is the case.

#### More Direct Experimental Manipulation of Independent Variables and Determination of Functional Relationships

These admonitions pretty well speak for themselves, for we all know they form the basis of genuine experimentation. We will probably see more experimental manipulation and clear delineation of functions when researchers more closely approximate the level of analysis suggested earlier. By examining a number of levels of the independent variable, we can better derive realistic information about the shape of the functional relationship between the independent and dependent variables. Such functions are often not as simple as is sometimes presupposed and simple, linear relationships may not be too common. If well-established breaks in functions are apparent, such discontinuities are often important clues as to process and thus demand further investigation and reconceptualization.



### Do More Experiments Directly Testing Alternative Interpretations of Findings

There are few studies in our literature in which an experiment is specifically designed to help decide between two or more alternative interpretations of an observation. The lack of such studies provides further evidence of how little conceptualization has gone on in parapsychology. Seldom are alternative interpretations of experiments even discussed in the literature, by a given author or between authors.

### Reconsider the Bases on Which We Follow Up Results

One important consideration regarding whether we follow up a result from any particular study should be whether or not the result seems conceptually interesting and to what degree this is true. Thus the mere probability-value, as statistically computed, associated with a particular outcome should not necessarily be the deciding factor in whether the finding in question is followed up. The strength of a finding might reasonably be a consideration regarding a follow-up, but we should remember that *p*-values do not necessarily indicate the strength of a finding. Further, conceptually interesting weak findings should be followed up. I had far rather follow up a conceptually interesting finding made at the .05 significance level than follow up a conceptually uninteresting finding made at the .005 level. Too often, though, parapsychological research seems to follow *p*-values rather than a clear-cut rationale.

### Increase Sample Size and Permit a Larger Alpha Error

If the effect studied is a real one, an increase in sample size should increase the power of the statistical test to detect the effect. However, the critical-ratio statistics traditionally used by parapsychologists do not consider the number of subjects in the statistical analysis. Probably because of the frequency of misuse of the typical parapsychological critical-ratio statistic or the critical ratio of the difference to make inferences about groups of subjects (Stanford & Palmer, 1972; 1973), some parapsychologists seem to have lost sight of the importance of sample size in experimentation. I suspect that with the increasing use of statistics more appropriate for process-oriented research we will see an increase in sample size.

In many instances conceptually important effects may not be strong ones, particularly with the relatively unrefined conditions under which they may first be studied or observed. This is why the use of fairly sizable samples of subjects is important in process-oriented work. We cannot afford to fail to detect a conceptually important effect, and some such effects may not be detectable with a tiny sample of subjects. Similarly, we should adopt a reasonable level of alpha error (such as .05) for process-oriented research. A Type II statistical error in process-oriented work can be a serious error and would likely have more serious long-term consequences than a Type I error.

### Use Statistical Tests That Allow Proper Inference

When very small sample sizes are used and typical parapsychological critical-ratio statistics are applied in the hope of making inferences about subject populations, the result can be very misleading, especially if, as has often happened, large numbers of trials are given each subject in the very small sample. This can lead to false starts in which one tries to follow up a lead derived from such a fallacious analysis. When, in process-oriented work with groups of subjects, statistics are used which treat subject scores as the basis of the analysis, and when this is combined with adequate sample sizes, some of the difficulties in replicating studies may disappear.

### Question Myths Regarding Psi Testing and Adopt Tests Suitable for Specific Purposes

Myths concerning ESP testing are quite prevalent. Fortunately, they are not believed by most parapsychologists. One very common such myth is that to have a useful measure of ESP from one's subjects, each subject must be given at the minimum a rather large number of trials. A popular figure in the U.S.A. is 100 trials, but one often hears the figure of 400 or more coming from other quarters. Curiously, such statements are typically made without specification of the intrinsic nonpsi hit probability and without regard for the kind of ESP test being given or the conditions of its administration. Another such myth is that work with free-response measures of ESP is highly inefficient and is a waste of time. My experience and the work on within-session (or within-run) variance declines (Carpenter, 1966; Rogers & Carpenter, 1966; Carpenter & Carpenter, 1967; Carpenter, 1968) suggest that making an ESP test very long

changes what one is measuring because it influences psychological factors which affect ESP performance. It thus might work against reliability of measurement. By and large I have used relatively small numbers of trials per subject in my research and have had the impression that this is a benefit rather than a liability.

Rather than arbitrarily adopting a common, standard form or length of ESP test, we should ask ourselves what problem we wish to study and what kind and length of test will, at least on the face of things, best measure this aspect of extrasensory function. We should also consider how a given kind of test can be expected to function in the context in which we wish to study ESP. We should then adopt or design an appropriate test and use it and forget about the mythology concerning trials and what kinds of tests are supposed to be inefficient. We know that a one-trial test can supply meaningful ESP data with a high level of measurement. There are a number of situations in which a single trial, if it is used to generate a fairly high level of measurement, may constitute the most useful and valid measure of ESP that we can obtain in a single session. This is certainly the case in much PMIR research in which we are really trying to model the function of psi in unique events out in the real world.

As concerns the claim that free-response ESP work is inefficient, the altered states work reported at this Convention should put that rumor to rest. The level of success in such studies with unselected subjects is clearly outstanding in the history of the field. If we compute the Schmidt (1970) psi-efficiency quotient for the data of the Honorton Ganzfeld study presented ... [see p. 52], the PQ is 148; for the Stanford-Mayer replication of the Braud work, 298. The advocates of forced-choice psi efficiency will have trouble beating that. For comparative purposes, the psi-quotient for the Pearce-Pratt work was 64.

#### Increase Inputs into Parapsychology

There are a number of ways to increase the amount and variety of raw information input to parapsychology and thus to broaden perspectives and perhaps create some new and better constructs.

- (a) Derive more experimental hypotheses from casual, everyday events which may be psi-mediated.

Experimental parapsychologists seem to test remarkably few hypotheses based upon spontaneous cases. Though this may seem puzzling, the reasons are not far to seek. Such cases have seemed irrelevant to what is studied in the parapsychology laboratory. Our typical experiments involve events very remote from those encountered in typical spontaneous cases. It is often said that it would be impossible to do experiments having the emotional or motivational impact of the events involved in many spontaneous cases.

How, then, can experimental work benefit from hypotheses generated from spontaneous cases? First let us recognize that the traditional spontaneous case reports are of a highly selected nature and may very well be nonrepresentative of the bulk of spontaneous psi. Such cases normally pass through at least three selection processes before they are reported in the literature. The events in question must be cognized as psi by the experiencing person. They must be thought evidential enough to impel him to report them. Finally, they must be regarded by the investigator as evidential or interesting enough to publish. Thus the bulk of spontaneous case reports are perceptual-cognitive cases, usually of a highly evidential and often dramatic sort. For these reasons such cases may not adequately represent the bulk of spontaneous psi. Many, if not most, psi events may occur in contexts which provide the poorest evidence of their psi nature. Further, such events may occur in contexts in which it is very unlikely that they are recognized as such.

If, then, the objective is not to try to prove the occurrence of psi in the spontaneous cases themselves, but rather to derive experimental hypotheses, more careful consideration should be given the nondramatic, possible psi events which occur many times in the lives of every one of us and which certainly occur in many noncrisis situations. These are the odd, titillating events which would never be reported as evidence of ESP but which can raise the question of whether psi is occurring and can suggest something about process. The number of such events is multiplied when we consider psi as a possible unconscious, directive or guiding influence in everyday affairs. I have found that these odd events which one would never report as good evidence of ESP provide a veritable garden of ideas for experimentation, and it does not cost much in time or effort to experimentally test some of them. If an experiment confirms a particular hypothesis, the nonevidentiality of the

original events becomes unimportant.

A major portion of my current research program involves work derived from such casual observations from life experience. If you will read my forthcoming papers on the psi-mediated instrumental response (PMIR) model, you will see that such events can be a fruitful source of experiments which may lead to major changes in our ways of conceptualizing psi (Stanford, 1974a; 1974b). Hypotheses derived from such casual, everyday observations can often best be tested in experimental designs which model specific real-life situations. A key characteristic of many such designs is that they give the subject a chance to use nonintentional psi in the service of his personal needs, in analogy to spontaneous cases.

(b) Give careful consideration to interesting casual observations during experiments.

These are often very unexpected and very striking. I will give you two examples. Robert Morris, then a graduate student, was testing subjects on a complex PK machine built by W. E. Cox, one of the famous "Cox clocks" machines. The procedure was rather difficult for the subject to follow, and junior high-school subjects were being tested. One girl made a very outstandingly high score and Morris explained to her how well she had done and asked what she had done to try to influence the machine, whether she had used any special technique, etc. She replied that she really had not done anything because she had not understood the instructions and had not asked for clarification. She had just punched the clock-actuating button whenever the experimenter had asked her to do so. She had no idea what she had been asked to try to do so she pushed the button each time and "just hoped everything would turn out all right." This anecdote dovetails well with other anecdotal material and with material from certain religious sources regarding the psychology of petitionary prayer. It may contain some very important clues to the psychology of PK.

While I was testing subjects in the word-association-ESP study recently reported in the Journal of the A. S. P. R. (Stanford, 1973), I quite frequently noticed that when the subject whom I had just tested gave a very unusual response to the stimulus word, the following subject gave the same response, especially when I did not think of the previous subject's response prior to its being given by the current

subject. Often these two subjects were the only ones to give the response in question. This experience I have very tentatively interpreted as providing an important clue concerning the mechanisms of so-called active-agent telepathy. This clue dovetails with other material, and a researcher at the Maimonides center recently reported to me a similar observation without my having mentioned my experience.

(c) Experimenters should endeavor personally to experience as many psi phenomena as they can. They may also profit by being subjects in experiments of their own or similar to their own.

Imagine the plight of the sex researcher who is a virgin or the LSD researcher who has never had a trip. A strong perceptual-cognitive ESP experience can be a compelling experience, and several of them can be an education for a parapsychologist. If you do not know how to get yourself to have such an experience, talk with a good psychic. Such a person should be able to give you some guidance. He may also give you encouragement and a sense of reality about these things which will greatly help you. You might also consult the literature on psi-conducive states. Being in experiments of one's own or in similar ones is helpful in various respects, but is optimally helpful after you have successfully followed the first advice and have had a few ESP experiences of your own. There is no adequate substitute for direct experience. The phenomenological reports of psychics are often very valuable, but they are best understood by someone who has, at least occasionally, "been the route" himself.

(d) Study the practices and beliefs of magic, religion, and mysticism of diverse cultures and times as they relate to possible psi phenomena.

The intention of this suggestion is not that we should naively adopt the beliefs in these areas for direct translation into hypotheses. This may be possible or desirable in a very few instances, but in fact many of the beliefs are definitely not testable as hypotheses since they are not stated in testable terms--for example, they may involve assertions about a particular deity, intangible metaphysical realities, etc. Besides, most of them will not appear plausible as hypotheses. Careful consideration of these areas, including an assessment of the recurrence of certain similar or analogous beliefs or practices, may indirectly or

directly yield important clues about psi processes, at the psychological level and perhaps at more basic levels. Through such efforts experimentation and, ultimately, theorization might be significantly advanced. Tapping such scientifically unencumbered sources might be valuable in suggesting insights which would otherwise not occur to persons with our typical background and conditioning.

I must confine myself to a few indications of some places where we might start in this endeavor. There are many other equally interesting possibilities. Some parapsychologists are becoming interested in studying traditions such as yoga, tantra, and Tibetan Buddhism, which have considerable to say about things psychic. I need not dwell on that except to say I think it very worthwhile to the extent that testable ideas can be derived from such traditions. One specific example will help to clarify the level of analysis I feel should be applied in these areas. There is a considerable tradition among yogis and among many psychic healers that a healer (or a yogi in "taking on the karma" of a disciple) may experience many of the pains, symptoms, or other kinds of distress of the person on behalf of whom he is acting. Most persons would think that this vicarious experiencing of difficulties in which one person can seemingly experience them for another would be the exclusive province of highly developed psychics or advanced yogis, assuming the phenomenon to be real.

But I would remind you that as we have studied psi phenomena it has become increasingly evident that the abilities possessed by so-called outstanding psychics are also possessed by persons in general, even if they may not be exercised so consciously or perhaps so often or so remarkably in most of us. My suggestion is that this vicarious experiencing by which one person can stand in the place of another may well be common to all of us and may in fact be rather commonplace. I am thinking of the great proclivity almost all of us have to empathize with someone whom we see in pain--to experience pain upon seeing him in pain--especially if this is a person for whom we have some affection or special sympathy. Might our intuitive response of empathizing with a person in pain actually be an adaptive response by which, at least under the right conditions, the level of pain that person is experiencing is actually reduced? I have worked out a suitable experimental design to study this problem, and I hope to do the study one day. The literature of religion, yoga, and psychic healing may give

us some clues as to the optimal conditions for this psychic burden-sharing--if it is an actuality, which of course remains to be determined. If such a phenomenon occurs, I doubt we can parsimoniously explain it in terms of our current parapsychological constructs. If it is real, careful consideration of it might give some important new insights into the nature of our phenomena and, more generally, into the problem of consciousness.

There are many other areas in which careful and cautious, but imaginative, investigation of such popular religious or mystical beliefs could, potentially, yield great benefit for us. To put it into one sentence, what we shall have to do is to examine these traditions and think about them in a sophisticated manner such that we can get behind the surface mythology and discover potentially important principles which might be operative. Often this is best done by cross-cultural comparison. This can help give us a picture of what is sheer myth in the beliefs and what may be a core truth or implicit principle of relevance to parapsychology. It is at any rate a good exercise in concept formation.

### CONCLUSION

Let me conclude with some summary impressions of our field as it exists today. We are in the best situation in our history to produce significant advances in understanding and even in controlling psi phenomena. We are now taking advantage of the technological adjuncts which can permit--even if they cannot insure--rapid progress. Further, we have recently been developing the methodological diversity which can unshackle not only our experiments, but our thinking. It is quite clear that we now have at hand the prerequisites for the conceptual development advocated in this address.

There are some indications it may be only a matter of years before parapsychology is a widely accepted and valued part of the scientific enterprise. We can all see the change coming, and there is even some talk that the Zeitgeist is working in our behalf. Exactly how long it will take for this change in status to occur is, of course, uncertain. Certain it seems, though, that the change which has already occurred derives in large part from a growing feeling that it is, after all, possible to gain some positive knowledge of psi phenomena--that the events we study are



not, in the end, intrinsically capricious and unpredictable, even if a very few parapsychologists have sometimes lent their names to this escapist explanation for some of our failures. Certain it also seems that if a valued "place in the sun" is ultimately to be the lot of parapsychology, we shall, like Orpheus, have to "sing the sun up." For in the realm of science, it is positive knowledge, not a Zeitgeist, which makes the world go round.

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On the Nature of  
ALTERED STATES OF CONSCIOUSNESS  
with Special Reference to Parapsychological Phenomena\*

Charles T. Tart

It is very appropriate that I should be talking to you about altered states of consciousness this evening, because I have made an interesting observation while listening to these banquet addresses at these Conventions in past years. My mind was always keenly interested in what the various speakers had to say. My body, on the other hand, had been active listening to presentations all day, was sending a lot of tired blood to my brain, remembered that it had a little too much to drink at the cocktail hour before the banquet, and was busy anyway digesting a full meal, and its only desire was to alter my state of consciousness and go to sleep. In the ensuing struggle, the mind clearly won during the first half of the speech. During the second half, the tired body almost predominated. I have never been able to go to sleep while sitting up straight and trying to keep my eyes open, so an unsatisfactory state resulted where I couldn't pay as much attention as I wanted to the speaker. I hope that you don't have the same problem, but since my observations of others in the past have suggested that a fair number of you do indeed have this problem, I shall try not to talk too long. I should also warn you that, at some point in my talk, I may produce some stimulation which is not at all disruptive to maintaining one's ordinary state of consciousness, but is very difficult to deal with during a sleep state.

Altered states of consciousness are an increasingly important topic to parapsychologists, primarily because of the many spontaneous cases that suggest psi may operate

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\*This is the Invited Dinner Address, given September 7, 1973. Copyright 1973 by Charles T. Tart.

quite strongly in some altered states of consciousness, and also because we now have some experimental work which further supports this. Unfortunately, if we want to use altered states of consciousness to gain more control and understanding of psi, it too easily becomes a case of the blind leading the blind. Our scientific knowledge of both psi and altered states of consciousness is very similar: we know some important things happen, but our actual understanding and control of them is very poor indeed. We have no overall theoretical understanding of them, only large assortments of miscellaneous and isolated data.

What I'm going to do this evening is give you the general outlines of a theory about the nature of altered states of consciousness that I have been working on for several years (Tart, 1970a). This is a relatively low-level kind of theorizing in that it sticks fairly close to the experimental data rather than becoming very abstracted from them. On the other hand, it has the advantages of having numerous immediate applications for guiding research. It can guide research on both altered states and parapsychology. Although time is limited, I shall point out some direct applications of the theory to parapsychology.

I shall emphasize a psychological approach to altered states of consciousness. I am defining myself as a conservative experimental psychologist these days, sticking with the age-old definition of psychology as the study of the mind, and being interested in how experimental procedures will help us in this study. The theoretical framework I give you can be translated quite easily into behavioral or neurophysiological terms, and those who feel more comfortable with such terminology have free license so to translate. But my primary interest is psychology, and I no longer accept the all too popular belief that if psychology is ultimately to be science it must rest on physiological or behavioral grounds.

### Terminological Problems

Let me begin by pointing out that our first problem in studying this area is terminological. The terms, "states of consciousness" and "altered states of consciousness," have now become very popular. And I share a certain amount of guilt in having helped to popularize them by editing a book, Altered States of Consciousness (Tart, 1969). As a consequence of becoming popular, however, they frequently tend to be used in such a loose fashion as to mean

almost nothing in particular. Many people now use the term, "state of consciousness," for example, simply to mean whatever is on their mind. So if I pick up a water tumbler and look at it, I am in water tumbler state of consciousness, and if I now touch the microphone, I am in microphone state of consciousness, and if I now touch the top of my head, I'm in top of my head state of consciousness, etc. Then an altered state of consciousness simply means that what you are thinking about or experiencing now is different or altered from what it was a moment ago. I would prefer to stick with the older terms of the experience of the moment, or what's on your mind right now, and try to rescue the terms, "state of consciousness" and "altered state of consciousness," for more precise usage. I shall introduce the terms "discrete state of consciousness" and "discrete altered state of consciousness" to do so, but rather than define them now, I want to talk about the background concepts that lead up to them, and give a tight definition later.

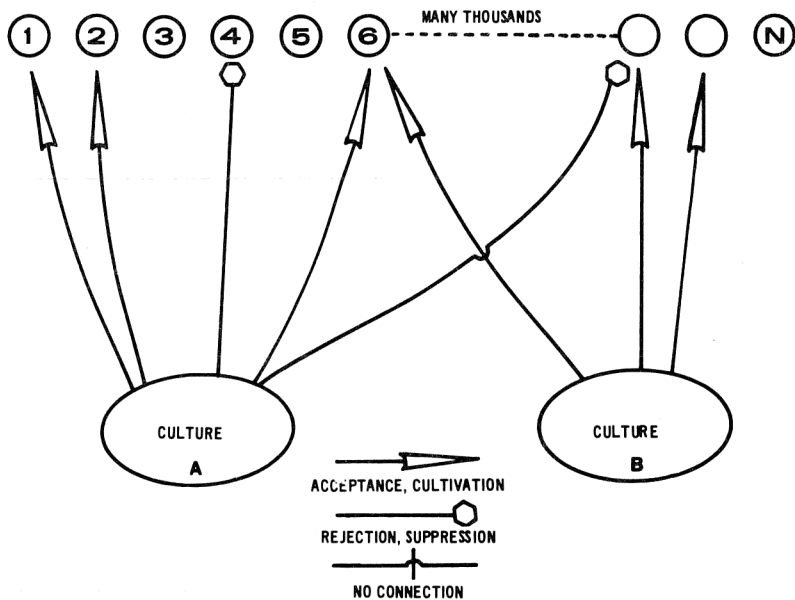
### The Spectrum of Human Potentialities

A basic concept that I want to start building from is the idea of the spectrum of human potentialities. Figure 1 diagrams this idea. By virtue of being born a human being, having a certain kind of body and nervous system, and functioning in the general environment of the spaceship Earth, a very wide variety of potential behaviors and experiences are potentially available to you. These may include running a four-minute mile, being able to learn mathematics, various kinds of esthetic appreciations and, of particular interest to us, learning to use certain kinds of psi abilities.

But, any individual human being will only develop a small fraction of his total human potentialities because he is born into a particular culture at a particular place and time, has certain parents, relatives, peers, and teachers, and has various "random" events happen to him. A culture can be looked at as a group of individuals who have (implicitly) agreed that certain human potentialities are beneficial and should be developed, and have set up a society, a system of interlocking relationships, to select these potentialities out of the total spectrum of human potentialities and to develop them to various degrees. These selections are shown by the arrows in Figure 1. A given culture also knows about certain other human potentialities, but considers these as undesirable or bad, and actively blocks their development. How

Figure 1

## SPECTRUM OF HUMAN POTENTIALITIES



many of you, for example, were given basic instruction in the first grade of school on how to go into a trance state so your mind could be possessed by a friendly spirit that might teach you interesting songs and dances? Not many, I warrant! In fact, if you had started developing that trait on your own and were foolish enough to talk to your teacher or your parents about it, I suspect you were very actively discouraged from any further development! Yet in many other cultures, this kind of human potential is highly valued.

Figure 1 shows two cultures making different selections from the spectrum of human potentialities. Sometimes the potentials cultivated overlap between cultures. All cultures, for instance, teach people something about how to eat. Yet what is good and cultivated by culture A may not be at all cultivated by culture B, or may even be actively

rejected as an undesirable trait. Note further that any given culture not only selectively develops some potentials and actively inhibits other potentials, but in addition it simply doesn't know about a very large variety of potentials. Thus they are undeveloped through ignorance and unuse, rather than inhibition. Eskimos, for example, discriminate and have separate words for seven different kinds of snow. We do not have such fine discriminations in our culture, as it does not occur to us that we need to or could make such fine discriminations.

The human potentials which a given individual develops then are in many ways severely limited by the culture he is born into. Within the cultural framework there will be further limitations depending on particular circumstances, and there are probably also genetic differences in each individual's inheritance, but the cultural limitations and selections seem much more obvious than the genetic ones at this point.

### Spectrum of Experiential Potentialities

This same concept of the spectrum of human potentialities can be extended to a spectrum of consciousness potentialities, or experiential potentialities. Look again at Figure 1, but change the label "culture" to "state of consciousness." In his ordinary state of consciousness, represented by the circle at the lower left, a given individual is able to have certain kinds of experiences, use certain kinds of mental skills. Some other kinds of mental skills are actively inhibited in his ordinary state of consciousness, and many other experiential potentials that the individual was born with were simply never developed and so are not available in his ordinary state of consciousness. Some of these may still be latent, waiting for the right stimulus to bring them into flower, others may no longer be developable due to disuse.

Now, using the idea of states of consciousness rather loosely as simply implying an overall organization of consciousness, an overall patterning, it may be possible for this same individual to change into an altered state of consciousness, a new organizational patterning of his consciousness, and in this second state he may be able to tap and use certain of the potentialities that are unavailable in his ordinary state of consciousness. This is represented by the lower

right circle in Figure 1. A psychic, for example, may be able (at least intermittently) to use certain psychic faculties in an altered state of consciousness which are not available in his or her ordinary state of consciousness.

Just as two cultures may develop some common human potentialities, most of the altered states of consciousness that we know much about do share some experiential potentialities with our ordinary state of consciousness. The psychic, for example, continues to speak his native language either in his ordinary state of consciousness or in an altered state of consciousness. The availability of new human potentials, new modes of functioning, in altered states of consciousness is a reason for our interest in them, especially if one does not feel that one's ordinary state of consciousness allows optimal functioning and thinks that the potentials available in an altered state may be of value. The promise of greatly enhanced understanding and control of psychic abilities in altered states is what interests us as parapsychologists.

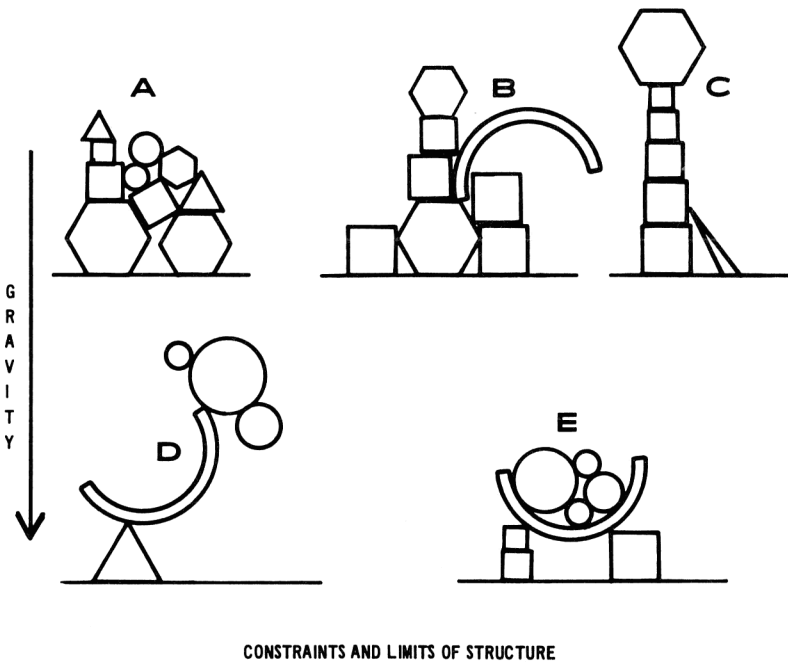
#### Limits of Structure

What we know of altered states of consciousness to date, however, suggests that you cannot make just any selection you want from the spectrum of human potentialities or the spectrum of consciousness potentialities. Different potentialities have certain kinds of characteristics which may or may not mesh harmoniously with other potentialities, so there are only certain ways that you can combine things. We are far from knowing the ultimate limits of these, but there do seem to be limits. Figure 2 illustrates this idea, using the analogy of different shaped blocks piled together to form structures in the gravitational field. The particular shape and size of different building blocks represent the characteristics of particular human potentialities, the gravitational field represents the reality structure of the human body and earthly environment in which these potentialities have to operate in harmony with each other, and the overall shape of the structure built by the blocks represents the overall pattern of the state of consciousness.

Structure A, for example, represents a relatively stable configuration combining a fairly wide variety of human potentialities. It is stable in the gravitational field, and relatively resistant to sideways pushes, environmental stresses, or psychological stresses. Structure B also rep-



Figure 2



resents a state of consciousness which is relatively stable in the gravitational field, but the particular potentiality represented by the semi-circular arc is held in place by the weight of the central pile. While this is stable in the ordinary gravitational field, a sideways push on the free end of the semicircular arc would apply a lot of leverage and tumble the central pile. You might think of this as analogous to a culture or state of consciousness which is relatively stable but has a particular weak spot. The tall structure C gains height at the expense of width, and while it is stable in the ordinary gravitational field and relatively resistant to sideways pushes, if it ever received a sufficient push, the whole thing would collapse at once. This is analogous to a culture or state of consciousness which is quite specialized to achieve certain things, but if it ever is broken down, it is very hard to put together again.

Suppose the various-sized circles represent some particularly valuable human potentialities. Of our first three structures, only A managed to work any circles into the system, and those only in a small way. Figure E represents a culture or a state of consciousness that has managed to tap into many of the circles through ingenious use of structure. It is relatively stable both in terms of a constant gravitational field and in terms of sideways stresses. Finally, structure D is a fanciful one that obviously won't work. It can't hold together in the ordinary gravitational field. It could represent a momentary "schizophrenic" state that someone dreamed up as a good way to structure consciousness, but it's very impractical. Or it could represent the idealistic commune dedicated to love and brotherhood that almost immediately comes apart over such questions as who does the dishes and who takes out the garbage.

### Feedback Control

Figure 2 is somewhat misleading in suggesting a relatively static configuration of consciousness potentialities making up a state of consciousness or a culture. In point of fact each block is, as it were, varying its size and perhaps altering its shape slightly all the time, but each part is connected to others by control and feedback mechanisms that keep the variation inside each part within limits tolerable to maintaining the structure as a whole. Your automobile has many moving parts, but they only move within certain limits and maintain the structure's overall identity as an automobile. Our culture has all sorts of variations and dissent, but maintains a certain quality we call American. Similarly, your ordinary or any other state of consciousness shows a lot of variation in the particular contents at any given time, or the particular level of functioning of various psychological functions, but maintains a certain quality that allows us to call it a state of consciousness. Earlier, when I looked at the water tumbler or the microphone or touched the top of my head, I changed the particular content of my psychological experience, and perhaps altered the level of functioning of, say, my muscular activation, but through it all I stayed in what was to me my ordinary state of consciousness: the overall shape of the structure, the overall pattern of my consciousness remained the same.

With modern psychological research we have begun to learn something about the nature of the psychological func-

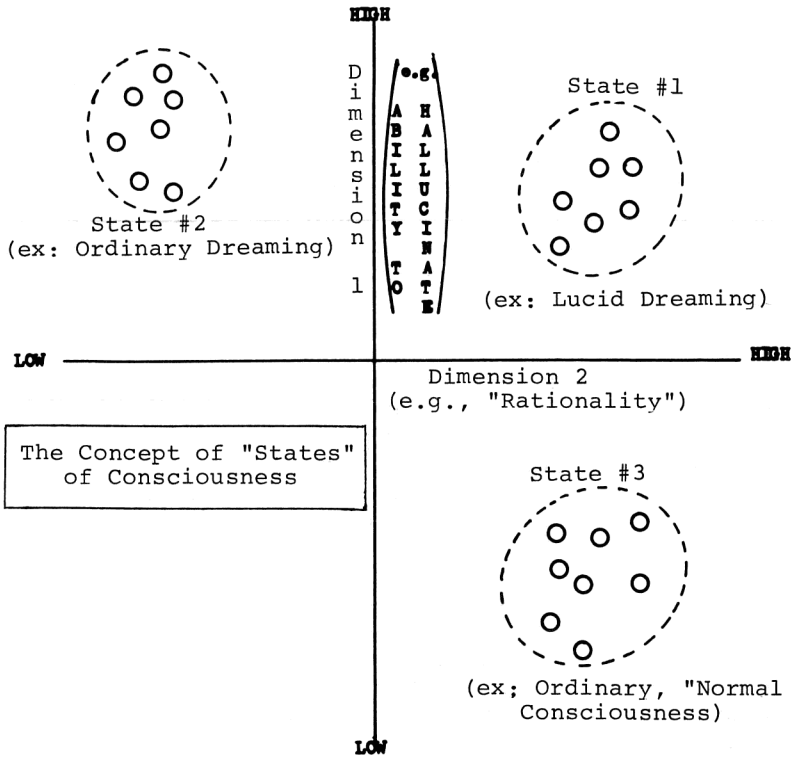
tions that comprise our state of consciousness, and the nature of the feedback control processes among them that stabilize the overall structure into a recognizable state of consciousness. We do not have time to go into these mechanisms tonight, but they include such things as classical and instrumental conditioning of various basic cognitive, emotional, and bodily responses, all sorts of perceptual learning so that we recognize certain things as being "important" with very little effort, all sorts of selective perception mechanisms so that we see and sense things that are congruent to maintaining our state of consciousness; a whole series of complexes and processes we refer to as the dynamic unconscious, and last, but hardly least, innumerable processes of social correction and reinforcement that maintain our perception of what we can call "consensus reality." That is, our culture forms a psychological system which not only shapes our selection of human potentialities into a "normal" state of consciousness for that culture in our development, but constantly reinforces that state in each one of us. Those in whom the process is not adequately reinforced are liable to be dubbed deviant or mentally ill, and be forcefully readjusted to or eliminated from a particular culture. Let us now move toward a more precise definition of a state of consciousness.

### States of Consciousness

Suppose, as psychologists, students of the mind, we believe that an individual's experience (and/or behavior) can be adequately described at any given moment if we knew all the important dimensions along which behavior varies, and could assess the exact point along each dimension that an individual occupied at a given moment. That is, we presume we have a multi-dimensional map of psychological space, and by knowing exactly where the individual is in that psychological space, we have adequately described his experiential reality. This is a generally accepted theoretical idea but, of course, it is very difficult to apply in practice because there may be many psychological dimensions important for an individual's experience at any given moment. We may be able to assess only a small number of them, and an individual's position on some of these dimensions may change even as we are busy assessing the value of others. Nevertheless, it is an ideal to be worked toward, and we shall assume that we can do this with some adequacy.

Further to simplify, let us assume that what is im-

Figure 3



portant about an individual's experiences can be mapped along only two dimensions, so we can draw a nice graph. Figure 3 represents such a mapping. Ignore the illustrative labels on the two dimensions for a moment, and simply consider them two dimensions of psychological experience. Each small circle represents an observation at a single point in time of where a particular individual is in this two-dimensional psychological space. In this particular example, we have taken a total of 22 measures at various times.

The first thing that strikes us about this individual is

that his experiences seem to fall in three distinct clusters, and that there are large gaps between these three clusters. Within each cluster, this individual shows a certain amount of variability, but he has not had an experience at points outside the defined clusters. This kind of plot of an individual's location at various times in experiential space is precisely what I mean by a discrete state of consciousness. To put it another way, it means that you can be in a certain region of experiential space and show some degree of variation within that space, but then to transit out of that space you have to cross a "forbidden zone" where you either cannot function or cannot have experiences (or cannot be conscious of having experiences), and then you find yourself in a discretely different experiential space. It is the quantum principle of physics applied to psychology: you can be either here or there, but there is no way of being in between. By analogy, a woman can be pregnant or not pregnant, but there is no in-between point!

If I ask you whether you are in your ordinary, awake state of consciousness right now or having a dream about this particular event right now, I expect that all of you would be able to tell me unequivocally this is your ordinary, waking state of consciousness (unless some of you have fallen asleep already). Yes, I know some of you might want to get into abstract, philosophical arguments about the nature of reality, but I doubt very much that any of you would actually believe your argument; you know for certain that right now you are ordinarily awake and not in a dreaming state. If I asked the same question of an audience in California, I might get even more equivocation but, from having asked this question seriously of many audiences, even in California, I seldom find more than one in several hundred persons who is ready to quarrel seriously about whether he is awake or dreaming at the moment!

There are transitional states between some discrete states of consciousness which may confuse the issue in particular cases, but we will deal with them in more detail later. For now, being in a discrete state of consciousness means that you are in one or another of these three distinct regions of psychological space shown in Figure 3. Now let's concretize this example and refer to the labels I have put on the two axes of this figure. Let's call dimension 1 your ability to image or hallucinate. This varies from a low at one extreme of literally just "imagining" that you see something outside yourself but not having anything corres-

ponding to a sensory perception at all, to a high at the other extreme where what you image has all the qualities of "reality," of an actual sensory perception. Let's call dimension 2 your ability to be "rational," to think in accordance with the rules of some logic or other. We won't concern ourselves with the cultural arbitrariness of "logic" for the moment, but simply take it as a given set of rules. Then obviously you could vary from a low of making many mistakes in the application of this logic, as on days when you feel rather stupid and have a hard time following what people say, to a high of following the rules of the logic perfectly, feeling sharp as a tack as they say, with your mind working like a precision computer.

Now we can give names of states of consciousness to the three clusters of data points on the graph. Ordinary consciousness (I am avoiding the term "normal" consciousness because of its implicit value judgments and cultural relativity) is shown in the lower right corner. It is characterized by a high (but generally far from perfect) degree of rationality, and a relatively low degree of ability to hallucinate. We can think without making very many mistakes in logic, and most of us get some mild sensory qualities when we image, but far less intense than in sensory perception. Notice again that there is variability within the state we call ordinary consciousness. My logic may be more or less accurate, my ability to image may vary somewhat, but this all stays in the ordinary, habitual range.

At the opposite extreme, if I have been severely poisoned and am in a state rather loosely called toxic psychosis (which will probably turn out to be a whole series of discrete states of consciousness when we really understand it rather than just one), then my rationality may be very low indeed, while my ability to hallucinate may be quite high. To the severely intoxicated man, such as one going through alcohol withdrawal symptoms, the classic pink elephants (undoubtedly shaped by cultural expectations) will look very real indeed, while great liberties will be taken with logic.\* Ordinary dreaming would also fall somewhere in this upper left-hand region of the graph, because we create the entire dream world, hallucinate it. It seems real, yet we often take considerable liberties with rationality.

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\*One can be "more or less" poisoned, so there may be varying degrees of toxic psychosis.

The third cluster of data points defines a particularly interesting discrete state of consciousness called lucid dreaming. This is the special kind of dream, first described by the Dutch physician Frederick Van Eeden (1913), in which you feel as if you "wake up" in terms of mental functioning within the dream world--i. e., you feel as rational and in control of your mental state as in your ordinary state of consciousness, but you're still experientially located within the dream world. Here we have a range of rationality at a very high level and a range of ability to hallucinate at a very high level too. A discrete state of consciousness refers to a particular region of experiential space, as we have shown here, and adding the adjective "altered" simply means that with respect to some state of consciousness (usually our ordinary state) that we use as a baseline, we have made the quantum jump to another region of experiential space, another discrete state of consciousness.

Now let's go into more detail. I shall define a discrete state of consciousness for a given individual as a unique configuration or system of psychological subsystems. The subsystems show some variation in the way in which they process information, or cope, or have experiences within one or more varying environments.\* The subsystems operative within a discrete state of consciousness comprise a system where the parts, the psychological subsystems, interact with each other and stabilize each other's functioning by means of feedback control, so that the system, the discrete state of consciousness, maintains its overall pattern of functioning within a varying environment. That is, the parts of the system that comprise a state of consciousness may vary individually, but the overall, general configuration, the overall pattern of the system, remains recognizably the same. The discrete state of consciousness, the overall pattern of the mind, must adapt reasonably well to the environment, because lack of adaptation may not allow the individual to survive in that environment.

Our way of understanding a discrete state of consciousness, then, depends both on understanding the nature of the parts, the psychological functions or subsystems that

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\*One may also consider particular environments or classes of environments as a subsystem acting as part of the overall system, controlling and stabilizing it, as will become clearer when we discuss induction techniques.

comprise it, and on taking into account the Gestalt or pattern properties that arise from the overall system and which are not an obvious result of the functioning of the parts.\*

### Discreteness of States of Consciousness

Let me make a few further comments about the discreteness of different states of consciousness and on the quantum leap between them. First, it should be realized that the concept of discrete states of consciousness, in its common-sense form, did not come from the kind of precise mapping along psychological dimensions that I have sketched in Figure 3. Rather, the immediate experiential basis of it is the feeling that "this condition of my mind feels radically different from some other condition, rather than just being an extension of it." The experiential mapping is a more precise way of saying this.

Second, for most of the discrete states of consciousness that we know something about, there has been very little or no precise mapping of the transition from the baseline state of consciousness to the altered state. Little has been done, for example, in examining the process by which one passes from an ordinary state of consciousness into the hypnotic state,† although for most subjects the distinction between the well-developed hypnotic state and their ordinary state is very marked. Similarly, when one begins to smoke marijuana, there is a period of time during which one is in an ordinary state of consciousness and smoking marijuana, and then later one is clearly "stoned," i. e., in a discrete altered state of consciousness we call marijuana intoxication. The only study of this is a preliminary survey that Joseph Fridgen and I carried out, asking experienced marijuana users about the transition from the one state to the other. We found that they had, essentially, never bothered to look

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\*A practical limitation in our understanding of discrete states of consciousness is that they must have some reasonable stability over time: we could imagine discrete states of consciousness that would only hold that particular pattern for a fraction of a second, but this would be too short for us to make any useful kinds of observations of their properties. The ones we are beginning to have some knowledge about can last from minutes, to hours, to lifetimes.

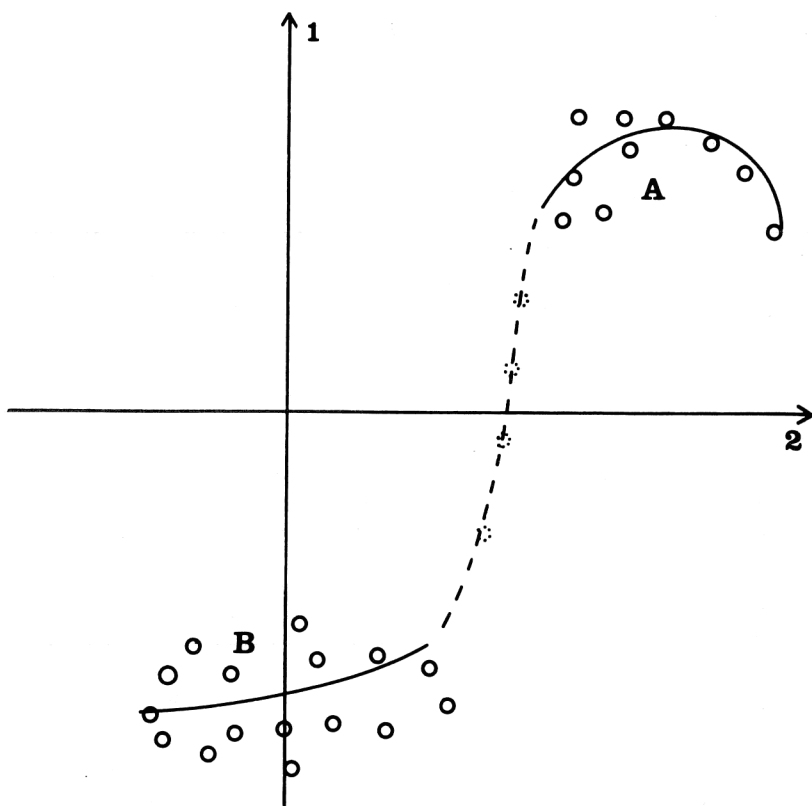
†Some preliminary psychoanalytic investigations are of interest here (Gill & Brenman, 1959).



at the transition, either being in a hurry to get into the intoxicated state or being in social situations that didn't encourage them to observe what was going on in their minds.

So, for many discrete altered states we don't really know what the size and exact nature of the quantum jump is or, indeed, whether it might be possible to effect a continuous transition between two regions, thus making them extremes of one state of consciousness rather than two discrete states. For example, Figure 4 shows what appear to be two discrete states of consciousness in an individual, clusters A and B, mapped along two psychological dimensions. When functioning in state A is pushed to its lower limit along dimensions 1 and 2, a transition occurs to state B. But I have sketched in some hypothetical points, even though they've never actually been observed, to illustrate that with other factors intervening it might be possible for a person to function in the experiential space between states A and B. But, given the nature of the individual, unless such special forces intervene, he might never stay within this intermediate space long enough to observe it, and so always seems to make a discrete jump from state A to state B. By analogy, if I sway from side to side as I talk, the height of my head above the floor and the angle of my body will change, variables we can plot in two dimensions, but I would still be in the state called standing. If I go beyond certain limits, I shall rather suddenly fall over and end up in a different state called "lying on the floor and feeling foolish!" But, with the aid of some unusual mechanical supports, my body could be made to remain at any angle in between vertical and horizontal. So while many of the states of consciousness we know something about seem to show very discrete quantum jumps, it is possible that with extra forces applied, a person could be made to function in the intermediate, forbidden zones. Or it may be that there is a temporary function in an intermediate state, but the person's powers of observation are so interfered with that he is not aware of it or has only a confused recollection of it. This is a problem for future empirical investigation, but in view of current evidence, such states as the fully developed hypnotic state, ordinary stage-1 dreaming, lucid dreaming, non-stage-1 sleep thinking, marijuana intoxication, and ordinary consciousness certainly seem discrete. (This is not an exhaustive listing.)

A third important point to note is that both Figures 3 and 4 imply a kind of underlying continuity in terms of the

Figure 4

psychological dimensions we are mapping, even though there are portions of these dimensions where an individual's structure does not allow him to function. This implies that discrete states of consciousness are basically quantitatively different, with certain portions of the continuum missing. Recall, however, that I talked of latent human potentials which are undeveloped in certain states of consciousness becoming available in others. Thus we may have qualitative differences as a new kind of psychological function appears or a habitual kind of psychological function disappears,

rather than simply old psychological functions operating more or less intensely. Indeed, in most of the altered states of consciousness we know about, experiencers' reports suggest that there are indeed striking qualitative differences; and although this cannot be plotted very well on two-dimensional graphs, it further underscores the point that discrete states of consciousness are different from each other in a quantum way. Again, a great deal of empirical research is needed to fill in the specifics of this statement.

### Individual Differences

Now we must turn our attention to a methodological pitfall that has seriously slowed psychological research in general, as well as research on states of consciousness. This is the lack of recognition of individual differences. Now lip service is paid to individual differences all the time in psychology courses, but the reality is quite different. Individuals are things that are relegated to the clinical psychologists' domain, and everyone "knows" clinical psychologists practice more of an art than a science. As "scientists," who have been caught up in the all too human struggle for prestige, we ape the physical sciences where individual differences are not of great significance, and the search is for general, "fundamental" laws. I believe it was exactly this lack of real recognition of individual differences that was the rock on which psychology's early attempts to establish itself as an introspective discipline foundered. Following the lead of their more successful physicist and chemist colleagues, the early psychologists immediately began to look for the general laws of the Mind, and then when they found their data were not agreeing, they took to quarreling and wasted their energies.

All too often today we do the same thing, albeit in a more sophisticated form. We take a couple of measures on a group of subjects, and tempted by the "scientificness" of a nearby computer, we dump the data into a prepackaged analysis program and get an output like that in the lower right corner of Figure 5, showing a nice straight line fitted to the data and a highly significant (therefore publishable) correlation coefficient between variables X and Y. If we distrust that much abstraction of the data, we might ask our computer to give us a scatter plot of the raw data, and then we might see something like the plot in the lower left corner of Figure 5, which gives us great confidence in our fitted curve and correlation coefficient, in that it shows the data

Figure 5

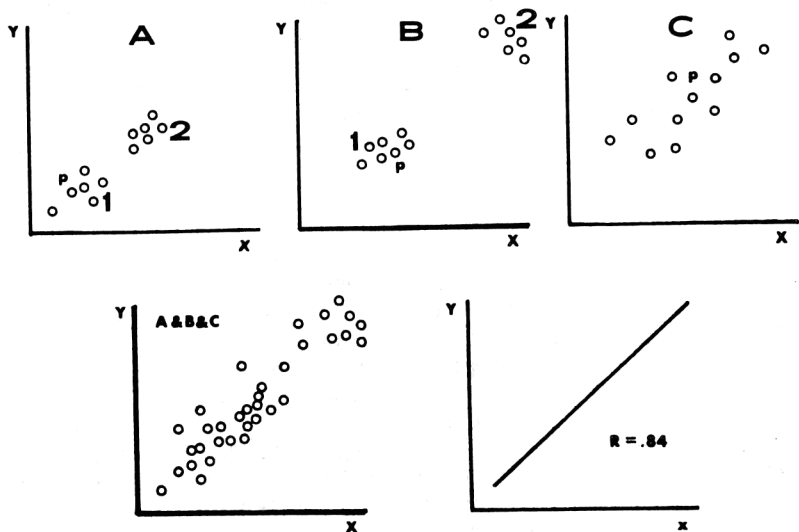
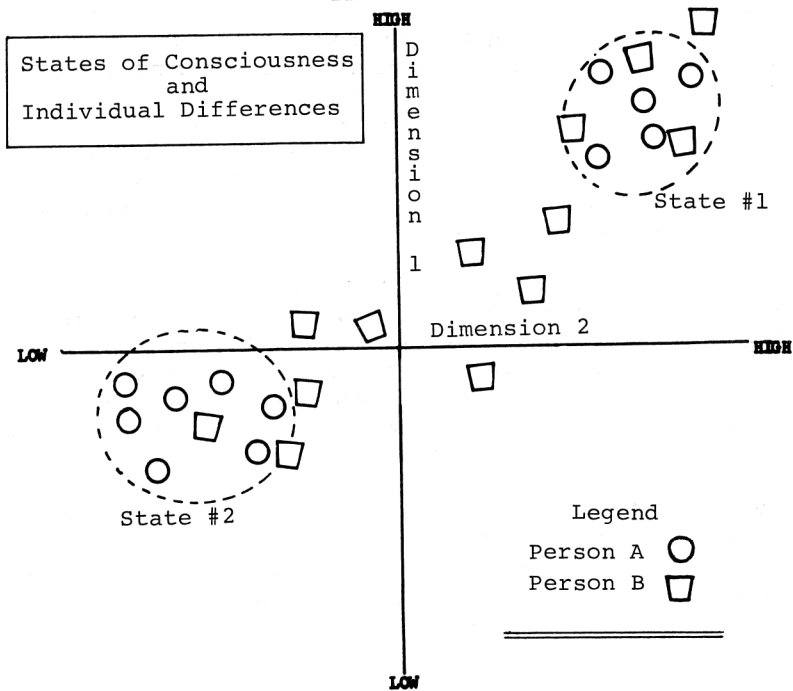


Figure 6



to be rather tightly clustered around the fitted line.

Suppose these data came from three subjects, A, B, and C, and in a more cautious way we actually consider each subject to be a single experiment and look at the data for each. Suppose we have three plots as in the upper half of Figure 5. Lo and behold, instead of the single cluster that our summary scatter plot gave, we see that while subject C shows variation over most of the range of variables X and Y, both subjects A and B show the kind of discrete clustering that I earlier used as a definition of discrete states of consciousness! If we want to deal adequately with our data now, we must say that the experimental manipulations of variables X and Y apparently produce an altered state of consciousness in subjects A and B, but that subject C seems to function in one state of consciousness throughout the experiment. To combine these quite different data into a group result is very misleading.

Figure 6 illustrates the problem more succinctly. Suppose we have again done an experiential mapping of the experiences of two individuals. Person A's experiences are mapped by the circles, and person B's experiences are mapped by the trapezoids. We then see that the concept of two discrete states of consciousness is appropriately descriptive for person A, but person B has experiences both in the areas that seem to be two discrete states of consciousness for person A and in the area which is the forbidden zone for person A. Thus the concept of discrete states of consciousness does not make any sense when applied to B.

I think it is hard to realize the full impact of these individual differences because of the deep (emotional) ingrainedness of the assumption that we all share a common state of consciousness to begin with, our ordinary or so-called "normal" state of consciousness. The more I have really gotten to know other individuals and started to get a feeling for the way their minds work, the more I have become convinced that the label "ordinary state of consciousness" conceals enormous individual differences. If I clearly saw the way your mind works (perhaps by telepathy?) in its ordinary state and vice versa, we would both be amazed, and perhaps horrified at the same time (a good reason to suppress one's telepathic faculties). Yet because we speak a common language that stresses external events rather than internal ones, we are seldom aware of the differences.

Psychologically, we each tend to assume that our own mind is an example of a "normal" mind, and we then project our own experiences onto other people without being aware of how much projecting we are doing. This can have interesting results scientifically. For example, there is a raging controversy in the hypnosis literature now over whether the concept of a discrete altered state of consciousness is necessary to explain hypnosis, or whether the hypnotic state is in fact continuous with the ordinary state, consisting simply of certain psychological functions, such as suggestibility and role-playing involvement, pushed to somewhat higher levels of activity than they are under ordinary conditions. One of the chief proponents of this latter view, Theodore X. Barber, is someone who can produce most of the classical hypnotic phenomena in himself without doing anything "special," i. e., he can sit down and anesthetize his hand or produce mild hallucinations and the like without experiencing a breakdown of his ordinary consciousness, a transitional state, or anything else "special" (Barber, 1972). That is, the phenomena included in his ordinary state of consciousness encompass a range that, for another person, must be attained by unusual means. To go back to Figure 6, where as person A might have one state of consciousness that we call his ordinary state and a second called the hypnotic state, person B's ordinary range of consciousness includes both of these regions. I am sure the truth is that what has been called hypnosis, to stick with this example, is indeed "merely" an extension of the ordinary range of functioning for some people, but for other people it is a discrete altered state of consciousness.

I cannot emphasize too strongly that the mapping of experience and the use of the concept of discrete states of consciousness must first be done on an individual basis, and only after that, if you find regions of great similarity among individuals, does it become legitimate to coin common names that apply across individuals. Naturally, this is an idealistic statement, and does not reflect the way it is really done! The very fact that we have names like dreaming state or hypnotic state indicates that there appears to be a fair degree of commonality among a fair number of individuals, but it is hard to say how far this goes with any precision at the present stage of our knowledge.

#### Psi Functions and Discrete States of Consciousness

Let's apply this kind of reasoning to ESP research.

Returning to Figure 5, let the little Ps interspersed among the data circles indicate that you have some kind of psi results in your laboratory situation where you were manipulating variables X and Y. Thus we see that subject A showed psi functioning in discrete state of consciousness number one, and subject B also showed psi functioning in discrete state of consciousness number one,\* but subject C showed psi functioning throughout the entire range of variables X and Y, in the single state of consciousness he was in. If you combine all this into group data, as we did in the lower half of Figure 5, what you'll apparently "find" is that psi has manifested throughout the entire range of variables X and Y across the three subjects, so you conclude that variables X and Y are unrelated to psi operation. If you accept this conclusion, which can easily come from working with summary data, you will probably stop investigating X and Y since they don't seem to be related to psi. But if you take the states of consciousness approach, the moral of the story is quite different: for subject C and other subjects like him you needn't bother to manipulate variables X and Y, while for subjects like A and B you need to manipulate variables X and Y in such a manner that each subject gets in the appropriate discrete state of consciousness. And I emphasize that you need to manipulate the variables not just to some fixed values, but to values that turn out to be effective in putting the subject in the right state of consciousness for obtaining psi. If, for example, X is relaxation and Y is a particular drug, you don't just tell all subjects to get very relaxed and give them a standard dose of the drug; you find the right level of relaxation and drug for each individual on each occasion that actually produces the transition to the psi-favorable discrete state of consciousness.

#### INDUCTION OF AN ALTERED STATE

General Principles. Now, having seen that the important thing about your experimental variables is not that

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\*The reader will note I am assuming a high enough degree of similarity between the experiences of individuals A and B in the lower left region of this two-dimensional psychological space to call them both "discrete state of consciousness number one."

they have a certain relationship to each other but that they are effective at inducing in the appropriate subjects a psi-favorable discrete state of consciousness, let us look at the process of inducing a discrete altered state of consciousness. Remember our starting point, the baseline state of consciousness, usually our ordinary state of consciousness, from which we begin. The baseline state of consciousness is a stable, overall patterning of psychological functions which, via multiple feedback stabilization relationships among the parts making it up, maintains its identity in spite of environmental changes. I emphasize multiple feedback stabilization for, as in any well-engineered, complex system, there are many processes maintaining a state of consciousness: it would be too vulnerable to unadaptive disruption if there were only one or two.

For example, right now in your ordinary baseline state of consciousness, which I presume you are all still in, a number of things act as feedback stabilization processes. First, the stable physical world around you, the invariant relationships in it, give you a pattern of input which constantly stimulates you in expected patterns that you are used to. If you push your hand against the chair you're sitting in, it feels solid, just as it always has felt. If you push it again it will still feel solid, and so on. You can depend on the spectrum of experience we call "physical reality." But, if the next time you pushed on the chair, your hand started passing through the material, I expect most of you would be rather surprised or alarmed and immediately begin to suspect that this is not your ordinary state of consciousness!

Second, your body (including your internalized body image) is another source of stabilization. Every morning when you wake up you have one head, two arms, and two legs. Although the exact relationship of the parts of your body to one another change, as do internal feelings within it, it is all within a well-learned range. If you suddenly felt half of your body starting to disappear, this would again make you wonder whether you were in your ordinary state of consciousness. Third, if you move your body, it has a certain feel to it; the kinesthetic feedback information about the relation of parts of your body and muscle tensions as you move them are also within an anticipated range. If your arm suddenly felt three times as heavy as normal when you lifted it, this might disrupt your ordinary state of consciousness.



Fourth, we have a constant internal thinking process going on, constant internal chatter, which runs through familiar and habitual associative pathways and keeps us within our ordinary state of consciousness. We think the kinds of things that please us, we feel clever as a result of thinking them, feeling clever makes us relax, feeling relaxed makes us feel good, feeling good reminds us that we are clever, and so on. This constant thinking, thinking, thinking, thinking, thinking is very important in maintaining our ordinary baseline state of consciousness, as well as the numerous interactions between various psychological functions. Although there is no time to enumerate all the sources of stabilization for your ordinary state of consciousness, I should mention the social reinforcement for maintaining it. If you doubt that the world is real or that you're normal, ask the person next to you, and he will probably reassure you that it is real and that you're all right, and then, as part of the social pact, you can reassure him that he's normal and he's all right. Thus we keep up a consensus reality which helps structure and stabilize our consciousness within the ordinary range.

Given this starting point, making the transition to a discrete altered state of consciousness is a three-step process, based on two psychological (and/or physiological) operations. I will describe the operations sequentially, but realize that in reality they are often simultaneous and combined. The first induction operation is to begin to disrupt the feedback stabilization of your baseline state of consciousness, to interfere with the patterning and feedback mechanisms that keep all your psychological functions operating within their ordinary range. For example, I will clap my hands loudly right now.... Since you've been listening to me drone on for a while, that is somewhat startling, certainly increases your level of activation, and may even have made you jump. I doubt very much, however, that anyone has now entered into an altered state of consciousness, even though your level of activation peaked up rather high momentarily. By throwing a totally unexpected and intense stimulus into your mind, I caused a momentary shift within your ordinary state of consciousness but not a shift to an altered state. Indeed, if you were a little drowsy, I might have totally disrupted one feedback stabilization loop for a moment, but since we have multiple feedback stabilization processes going on, this was not sufficient to alter your state of consciousness.

So the first operation in inducing an altered state of

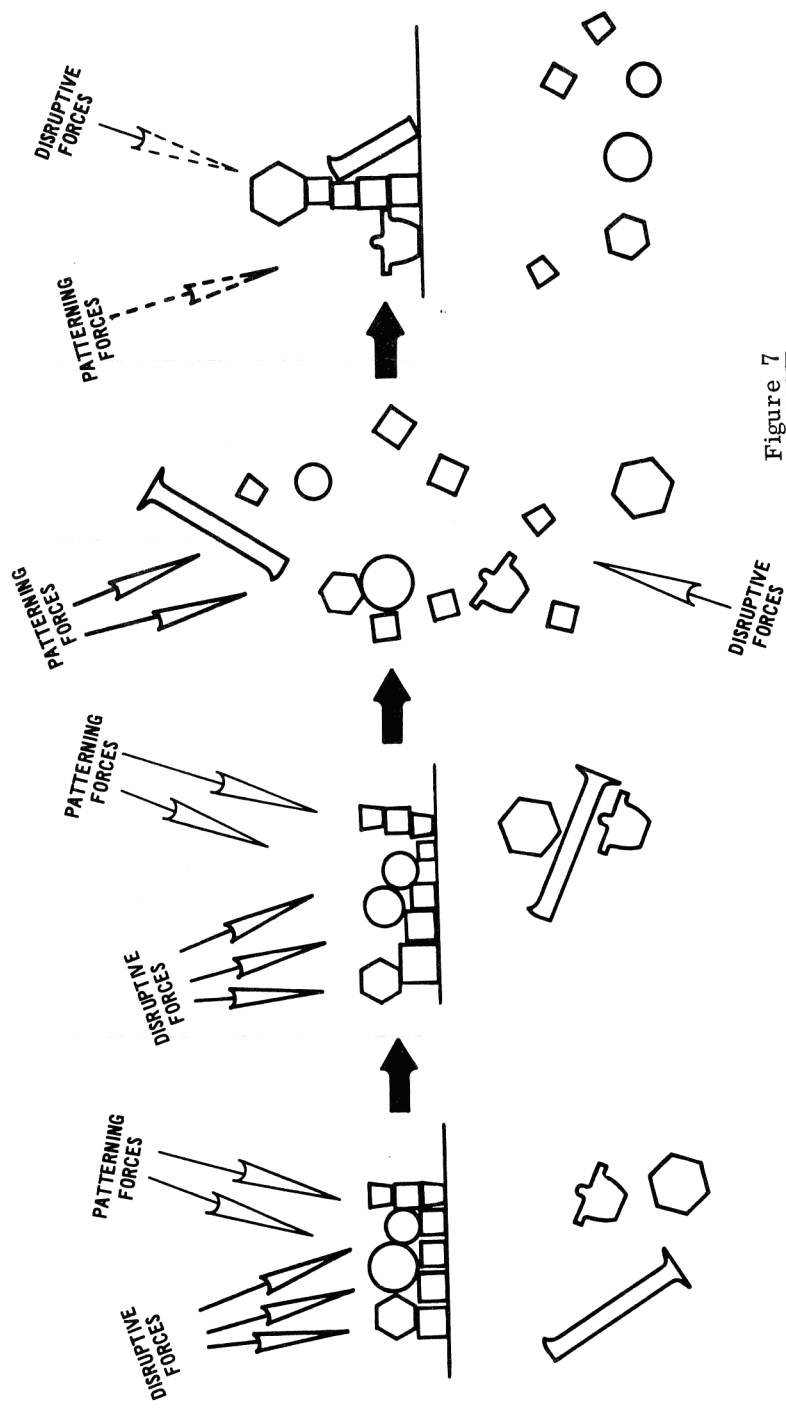


Figure 7

consciousness is to disrupt enough of the multiple feedback stabilization processes to a large enough extent that the baseline pattern of consciousness can no longer hold together. One may try to disrupt feedback stabilization processes directly or indirectly by pushing psychological functions to and beyond their limits of functioning. One may disrupt particular subsystems by overloading them with stimuli, depriving them of stimuli, or giving them anomalous stimuli which can't be processed in habitual ways. The second induction operation is to apply what I call patterning stimuli, stimuli that then push disrupted psychological functioning toward the new pattern of the discrete altered state of consciousness that you want to get into. These patterning stimuli may also serve to disrupt the ordinary functioning of the baseline state of consciousness insofar as they are incongruent with the ordinary state of consciousness. Thus the same stimuli may serve as both disruptive and patterning stimuli.

Figure 7 sketches the steps of the induction process, using our analogy of discrete states of consciousness with various shaped and sized blocks (representing particular psychological functions) forming a structure (the state of consciousness) in a gravitational field (the environment). At the extreme left, we see a number of consciousness potentialities assembled into a stable structure, our baseline state of consciousness. The detached figures below the structure represent some potentialities that are not available in the baseline state of consciousness. The heavy arrows at the left represent the disrupting forces, which are most important at the initial phase of induction, and the lighter arrows represent the patterning forces, which are not as important at this stage.

The left-most figure is our starting point, a stable state of consciousness to which we begin applying disrupting and patterning forces. The second figure represents quantitative change within the baseline state of consciousness. That is, the disruptive and patterning forces are being applied, and while the overall structure remains the same, some of the relationships within it have changed. Quantitative change has about reached its limit: at the right and left end of the structure, for example, things are about to fall apart. Particular psychological functions or subsystems have varied as far as they can while still maintaining the overall system. Below the heavy horizontal lines in Figure 7, I have drawn in the latent functions outside of consciousness. These functions change their relationship to con-

sciousness, something we must postulate from this theory and our knowledge of the dynamic unconscious, but about which we have very little empirical data at present. This is the first step of the induction process.

If the disrupting and patterning forces are successful in finally breaking down the organization of the baseline state of consciousness the second step of the induction process occurs, the structure comes apart, and a transitional state occurs. In the third figure, this is illustrated by having the parts of the structure scattered about, with no clearcut relationships to one another, or perhaps momentary relationships forming, as with the small square, the circle, and the hexagon on the left side. The disrupting forces are now represented by the single light arrow underneath the figure, because they are not as important now that the disruption has actually occurred, while the patterning forces are represented by the two heavy arrows above. The patterning stimuli must now push the isolated psychological functions into a new structure, the third step of the process, where a new, self-stabilized structure--the discrete altered state of consciousness--forms. Some of the psychological functions present in the original state of consciousness, such as those represented by the squares, trapezoids, circles, and the small hexagon, may not be available in this new state of consciousness, while other psychological functions not available in the baseline state of consciousness have now become available. I have also indicated that the patterning forces and disrupting forces may have to continue to be present, although in attenuated form, in order for this state to be stable; it may not have enough internal stabilization at first to hold up against environmental change, and so you may need to continue artificial props. A person, for example, may at first have to be hypnotized in a very quiet, supportive environment in order to make the transition into hypnosis but, after he has been hypnotized a few times, the discrete altered state is stable enough so that he can be hypnotized under very noisy, chaotic conditions.

In following this example, you probably thought of going from your ordinary state to some more exotic discrete state of consciousness, but this theoretical sequence applies to transition from any discrete state of consciousness to any other discrete state of consciousness. Indeed, the example also shows what we might call the deinduction process, the process of going from a discrete altered state of consciousness back to the baseline state of consciousness. You apply

forces to disrupt the altered state, and patterning forces to reinstate the baseline state, you get a transitional state, and the baseline state reforms. Since it is generally much easier to get back into our ordinary state, because it is so over-learned, than to get into an altered state, we usually pay little attention to the deinduction process, although it is just as complex as the induction process. The transitional state may or may not be observable, depending on the particular person and the particular states he is transiting between. In addition, the transitional state during induction may be different from that during deinduction.

Indeed, it may be that some discrete states of consciousness cannot be reached directly from other particular discrete states of consciousness, but some intermediary discrete state of consciousness has to be gone through. It is like crossing a stream that is too wide to leap over directly, so you have to leap onto one or more stepping stones in sequence to get to the other side. Each stepping stone is a stable place in itself, but they are transitional with respect to the beginning and end points of the process. This kind of stable transitional state should not be confused with the inherently unstable transitional states we have been talking about before.

Techniques. What about the actual techniques for inducing altered states, the actual operations to produce disruptive and patterning forces? If you begin looking at the incredible variety of procedures that have been used in various cultures throughout history to induce discrete altered states of consciousness, you are liable to come away thinking that almost anything you can do may induce an altered state in some people. In spite of this vast variety, however, they all incorporate the disruptive and patterning operations, and you can make three basic classifications of techniques.

First, some induction techniques work in a very direct, usually physiological, kind of way and are not too affected by cultural factors. The old hypnotist's trick, for example, of pressing on the carotid artery in the throat of the subject, thus cutting off the blood supply of the brain, while telling him he will become sleepy, produces the very direct effect of disrupting ordinary consciousness. One is tempted to include drugs in this very direct acting category but, as will become clear later, the action of the kind of drugs we think of as facilitating discrete altered states of consciousness other than sleep is very much affected by

psychological factors. Second, there is an immense number of techniques for inducing altered states that depend on belonging to a particular culture in that certain human potentialities, certain psychological functionings must be operative in the person, and only then will the techniques become effective. To use a simple example, if you do not speak Greek, and I begin going through a hypnotic procedure with you speaking Greek, and you do not know the technique is supposed to hypnotize you, it is very doubtful that you will become hypnotized.

Notice that I added the qualification, "and you do not know the technique is supposed to hypnotize you": if the belief system inculcated in you says that technique X will produce an altered state of consciousness in you, it may very well do so. I always think of the rather amusing example given by Estabrooks (1943) where he described trying to find out whether you could induce hypnosis if the whole procedure was put on a phonograph record. He asked for volunteers from a class for this experiment, sat them down in a quiet room, and put on the record. Much to his embarrassment, it turned out he had brought the wrong record from his office: this was one of Swiss yodeling. Without saying anything, he left the record on to entertain the subjects while he went back to his office to get the right one, and when he came back he found one subject was in a deep hypnotic state. The professor had said that this procedure would hypnotize him, and he was well acculturated and believed in professors (that was in the good old days), so he became hypnotized! Third, there are undoubtedly "noise" elements in any traditional techniques for inducing discrete altered states of consciousness, things that might have been effective once or that accidentally got mixed in with the induction procedure but do not have any particular effect.

### Methodological Hazards of Operationalism

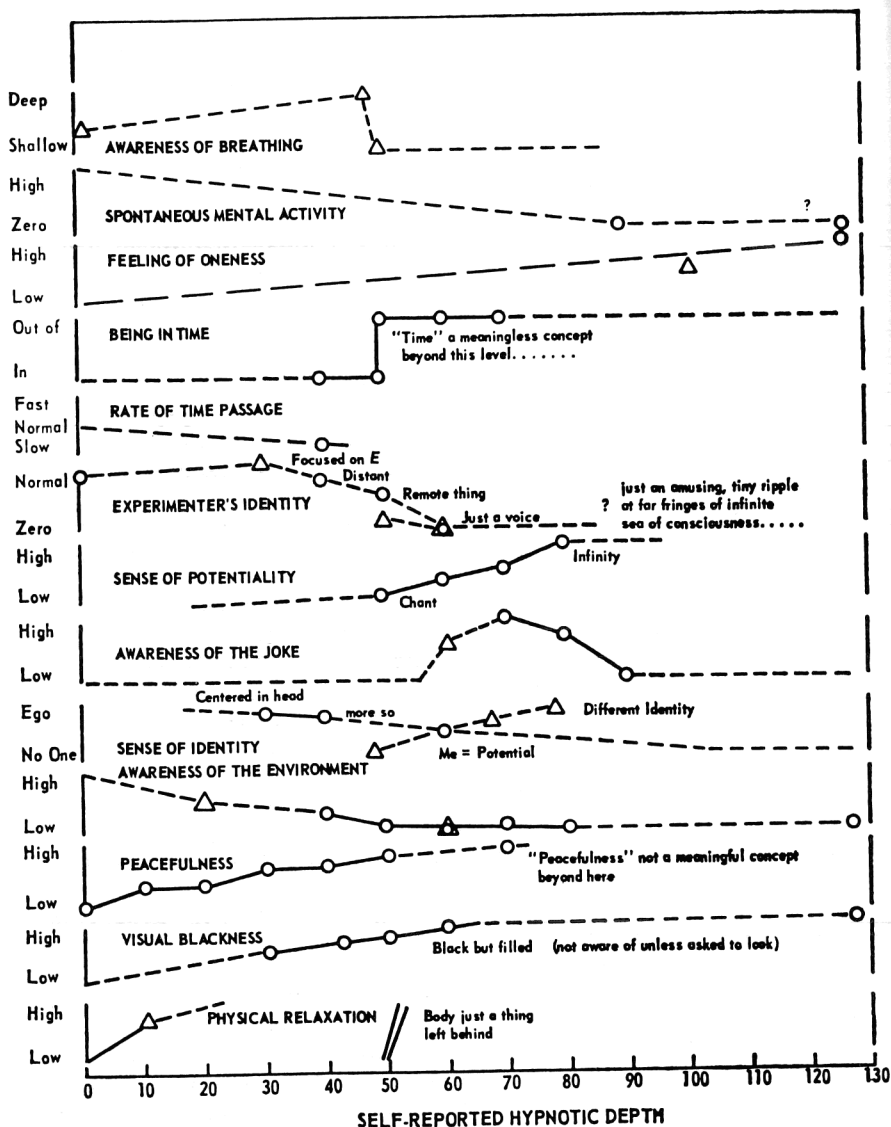
Now in our consideration of induction we come to another major methodological pitfall that has greatly plagued psychological research on altered states and is beginning to show up in parapsychological research. This is operationalism carried out rigorously, to the point of absurdity. That is, in our search for "objectivity," we start putting our emphasis on things that can be physically measured, often to the point of making our experiments useless.

The primary example that comes to mind in psycho-

logical research is the equating of the hypnotic state with the performance of the hypnotic induction procedure by the hypnotist. The hypnotic state is a purely psychological construct (or experience if you've ever gone through it), not at all amenable to measurement with a voltmeter or a camera. The hypnotic induction procedure, on the other hand--the "magic" words that the hypnotist says aloud--is eminently susceptible to physical measurement. One can photograph it, videotape it, record it, measure the intensity of the hypnotist's voice, and come up with all sorts of wonderful physical measurements. If you now talk about the responses of the "hypnotized" subjects, when you mean that these are subjects who sat in the same room while the hypnotist read the magic words (an objective measurement), you make a serious mistake, for the fact that the hypnotist goes through the procedure does not mean that the subject will enter the discrete state of consciousness. I stress that the concept of a discrete altered state of consciousness is a psychological, experiential construct, and so the ultimate criterion for whether a subject is in an altered state is a mapping of his experiences to actually determine at a given time whether he is in a region of psychological space we call a discrete altered state of consciousness. Figure 8 is an example of preliminary attempts at mapping a subject's experiences on 13 psychological dimensions as he goes into a hypnotic state and as he then goes into another state of consciousness after reaching the limits of the hypnotic state. Time does not allow us to look at this figure in any detail [for more detail see Tart, 1970b], but I would like you to notice how, toward the middle of the graph (the region roughly between fifty and seventy on the axis at the bottom), a lot of psychological functions seem either to plateau at some maximum or minimum value, or show a fairly sharp change. I believe this is a crude example of the kind of quantum jump that takes place with the transition from one discrete state of consciousness to another, although this level of measurement is still too crude to show this exactly.

So I emphasize very strongly that you must not equate a discrete state of consciousness with the performance of the induction technique. Going through hypnotic induction does not necessarily induce hypnosis, lying down in bed does not necessarily mean you're sleeping or dreaming, performing a meditation exercise does not necessarily mean that you enter into some kind of meditative state of consciousness. Where the induction technique is physiological--as, for example, using drugs--it is very tempting to think that this is

Figure 8



The relationship of the intensity of various experiences to self-reported hypnotic depth.



a complete definition of the altered state, but even that is false. Smoking marijuana does not necessarily mean that someone gets stoned, enters into a discrete altered state. Many people smoke marijuana several times and never experience an altered state, and experienced marijuana smokers sometimes deliberately turn off all the effects of the drug in order to return to their ordinary state of consciousness. Even with high doses of a powerful drug like LSD, some people do not feel any change in their consciousness.

This does not mean that we do not need to describe techniques in detail in reporting experiments, we certainly need to do that, but we also need to have measures of the effectiveness of these techniques in actually altering a subject's state of consciousness for each individual subject. Time does not permit us to go into a discussion of how these assessments can be carried out, but I refer you to an article of mine on self-report scales for measuring the depth of altered states of consciousness (Tart, 1972a) and to another on scaling the level of marijuana intoxication (Tart & Kvetensky, 1973) to show some of the ways we can do this. (We do not have time to treat physiological correlates of discrete states of consciousness, so I shall just say that where we have such correlates [stage 1 REM dreaming, for example], we can use them with the convergent operations strategy advocated by Stoyva and Kamiya [1968]--but remember that state of consciousness is a psychological concept, and psychology does not need a physiological basis to be scientific and useful.)

### Subsystems Comprising States of Consciousness

Shortly we shall apply the principles for inducing a discrete altered state of consciousness to concrete situations of inducing altered states like hypnosis or meditation, but as background for that we must look at the psychological functions that are combined to form the system of a state of consciousness in more detail. The general picture of a particular discrete state of consciousness made up of a certain number of psychological functions or subsystems, each with its own characteristics, interacting with each other in a certain pattern, can be described in detail in many ways. The particular number and kind of subsystems necessary for an adequate description would vary greatly from theorist to theorist, and what might look like a basic psychological function or subsystem to one theorist would be seen as capable of being broken down into simpler ones by another. My

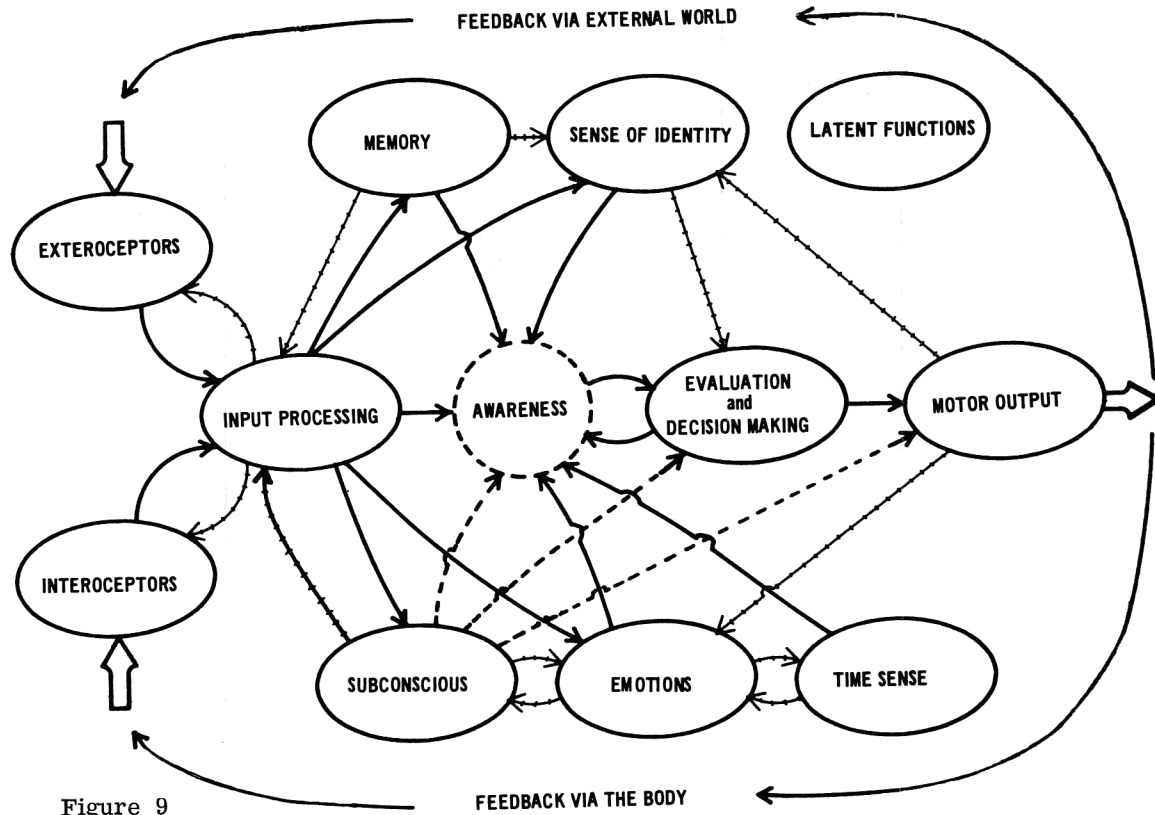


Figure 9

own theorizing at this time calls for ten major subsystems plus latent psychological functions which may come into operation in a discrete altered state of consciousness. My current choice of subsystems or functions is based on what I know about the general kinds of variations one sees in many discrete altered states of consciousness, conceptualized by the kind of psychological knowledge currently available. Each of the subsystems I will talk about is a convenient subsystem in terms of present knowledge, but certainly is susceptible to finer analysis when we have the data to do so. Someday each of these subsystems may be treated as a system itself and analyzed into finer subsystems.

Figure 9 shows ten major subsystems: Exteroceptors, Interoceptors, Input Processing, Memory, Sense of Identity, Evaluation and Decision Making, Motor Output, Subconscious, Emotions, and Time Sense. The heavy solid-line arrows represent major information flow pathways within the system. The extra large hollow arrows represent either input from the external world or body, or output to the external world or musculature. The heavy dashed arrows represent important information flow routes from our Subconscious that are only inferential in our ordinary state of consciousness. The thin-line hatched arrows represent feedback control pathways between various psychological systems, i. e., information flow routes which do not represent consciously experienced information but the influences of one subsystem on another which keep them operating within a range appropriate for maintaining the overall state of consciousness. I have not attempted to show all major information flow routes; to do so would produce much too messy a diagram. Thus Figure 9 shows only some of the major and more obvious information and feedback flow routes. An oval for Latent Functions is shown as not connected to other psychological subsystems, to illustrate the latent possibilities that may come into being as we go to a discrete altered state of consciousness.

The dotted circle labeled Awareness is the "ghost in the machine." I have not counted it as a subsystem, but rather as a psychological function of basic consciousness into which many subsystems feed or, if you like, a function which extends into other subsystems. But, since we do not know what consciousness is, even though we are learning a great deal about how consciousness can be organized, it does not have the same theoretical status as known subsystems. I shall very briefly say something about the nature

of each subsystem and the range of variation it may show in various altered states of consciousness.

Exteroceptors refer to our classical sensory organs for perceiving things in the external world, while Interoceptors refer to those senses for perceiving the conditions of our own body. Deliberately altering input to Exteroceptors and Interoceptors is a device used in almost all techniques for inducing discrete altered states of consciousness. There may be changes in the functioning of Interoceptors or Exteroceptors resulting from the induction of an altered state. We know very little about the functioning of Exteroceptors and Interoceptors in isolation, psychologically speaking, because they each feed their information into one of our most important psychological subsystems, Input Processing. This is the vast collection of perceptual learnings that makes our perception highly selective. It is the process that throws away 99 percent of the sensory data actually reaching us and passes on to Awareness only instantaneous abstractions of what's "important" in the stimuli reaching us at any time. Change in Input Processing, deautomatization (Deikman, 1966) of this abstracting process, occurs in many altered states and accounts for the frequent reports of enhanced vividness and beauty of perception.

All of these psychological subsystems are here being given convenient names for what may be a related collection of functions, and nowhere is this more obvious than in the case of Memory. It is a great oversimplification to speak as if we have one memory. We have a large number of specialized memories for handling different kinds of information. In various altered states, memory function may be grossly deteriorated, may be greatly enhanced, or may shift in style of functioning, as when memories start being recalled as vivid visual images rather than verbal abstractions about stored data.

The psychological subsystem labeled Sense of Identity is that collection of psychological functions that we might call the ego or the sense of my-ness. It is a quality that gets added to other information within our state of consciousness, rather than necessarily being information itself, a quality that, because we value our egos, calls for special handling of information that it is attached to. For example, if I live in New York City and look out the window and see some kids smashing a parked car, I might be relatively blasé and feel minor indignation at the lawlessness of our

times but not be inclined to do anything about it. If with further information coming out of Input Processing and Memory I recognize that it is my car that they are smashing up, the same information that a car is being smashed up now acquires an entirely new priority and emotional tone, to put it mildly. The sense of my-ness or egoness is highly variable in discrete altered states of consciousness, and may go from a low of zero, where everything is perceived simply as information and there is no ego on the scene, to a high where the self becomes extended to include other people, other events, or even the whole universe.

The subsystem labeled Evaluation and Decision Making refers to what we ordinarily think of as our cognitive processes, the various learned (and perhaps innate) rules and procedures we use for analyzing and working with information according to one or more kinds of logics. This subsystem can not only work more or less efficiently in discrete altered states of consciousness, but the particular logics with which it works can be deliberately altered. For example, in hypnosis a subject can be given an axiom that there is no such thing as the number five, that it's a meaningless concept, and he then evolves a new arithmetic taking this axiom into account.

The subsystem labeled Subconscious includes the classical Freudian unconscious but is more comprehensive than that, including many of the things that Myers, for example, would have included within the subliminal self. It refers to all those psychological functions which are not directly available to consciousness in an ordinary state of consciousness but which we hypothesize as being active in order to account for observable conscious behavior and experience. In some discrete altered states of consciousness, processes which were subconscious in the ordinary state, that we only hypothesized to exist, may become directly experienceable. The subsystem labeled Emotions refers to all of our various feelings. In discrete altered states of consciousness a variety of changes may take place, such as stimuli triggering different emotions than they normally do, or emotional intensity being much greater or lesser than it ordinarily is.

The psychological function labeled Time Sense refers to the general feeling we have for the rate of flow of time and the structuring of experience in terms of psychological time. It may be based at least partially on internal biologi-

cal rhythms, as well as on cues from external events. In various discrete altered states of consciousness, Time Sense may speed up, slow down, or seem to stop, or events may even seem to happen totally out of their normal order, with the future preceding the past. I stress that this is a psychological Time Sense we're talking about, an internally generated concept of time, rather than simply some kind of physiological or psychological process that mirrors clock time. Whether we want to consider clock time more "real" is an interesting question. The final subsystem diagrammed in Figure 9 is Motor Output, referring to both our voluntarily controlled skeletal muscles and various internal effects on our body of taking various kinds of actions, such as glandular secretions. Motor Output may be inhibited, unaffected, or enhanced in various altered states.

As a system, then, a discrete state of consciousness is a process that takes in information from both the external world and the internal world of our own body; processes it according to various psychological subsystems or functions which vary within certain limits; contains more or less of this processing in conscious awareness; and may result in some kind of output, either to the external world or simply in terms of internal bodily reactions. Both of these kinds of outputs may affect the external world or our own bodies where they are once again sensed by the Exteroceptors and Interoceptors, constituting another feedback stabilization for the system or state of consciousness as a whole. Now let us look at the induction of hypnosis in terms of this theory.

### The Induction of Hypnosis

The procedures for inducing hypnosis are many and varied, but I shall dwell on certain steps which are common to most of them. The first such step usually involves having the subject sit or lie comfortably, so he does not have to exert any effort to maintain his bodily position, and telling him not to move and to relax his body as much as possible. This has a variety of effects. For one thing, the subject may be somewhat anxious about what is going to happen, but anxiety is intimately related to bodily tension, and if the subject relaxes a good deal, he will not feel much anxiety, and this will make it easier for him to alter his state of consciousness. Too, by putting the body in a relaxed position and lying still, many of the Interoceptors adapt out. That is, many of the receptors for body position and sensations respond to changes in these sensations, and

when there is constant stimulation on them for a period of time, they gradually stop firing. Thus the body as a whole begins to fade out as a conscious experience, this known, patterned stimulation fades, and no longer serves to help stabilize the baseline state of consciousness.

Second, the hypnotist commonly tells the subject to listen only to the hypnotist's voice and, if other thoughts or sensations come into his mind, to ignore them. Ordinarily, via the Exteroceptors and Input Processing, we are constantly scanning the environment to see if important stimuli are present. This constant scanning keeps up a continuous, varied pattern of stimulation which tends to keep other subsystems active. As varied perceptions come in one must make decisions as to whether or not they are important; one must draw on memories from the past in making these decisions; importance will be determined by whether there is a sense of ego attached to them, and so on. By eliminating this scanning of the environment a good deal of psychological energy and activity is withdrawn from a number of these subsystems.

A third common instruction is that the subject should not think about what the hypnotist is saying, but just listen to it passively, just take it in. That is, if the hypnotist says your arm is feeling heavy, the subject is not to think "He says it's feeling heavy, I wonder if it really will get heavy, I remember it got heavy a long time ago but that's because there was a weight on it, well I guess I shouldn't be doubting..." In our ordinary state of consciousness we constantly think about what is being said to us and what is happening to us, and this maintains a great deal of Evaluation and Decision Making activity, as well as again activating other subsystems to provide relevant information. So this step also slows down the constant thinking that helps to maintain our ordinary state of consciousness.

Fourth, the subject is frequently told to focus his attention on some particular thing (as well as the hypnotist's voice). Let us take the example of the subject being asked to look fixedly at some simple object like a candle flame or a bright, shiny disc. This helps to eliminate the scanning of the environment, with the same sort of effects as we mentioned above, but there is a further effect. It is unusual for us in our ordinary state of consciousness to stare fixedly at one thing, and if we do, all sorts of (to most people) unexpected visual effects occur because of fa-

tiguing of the receptors. Colored halos may start to appear around things, shadows will appear and disappear, there may be apparent movement, parts of the object may fade, etc. To the extent that these are not part of a person's usual experience, they constitute a kind of input that Input Processing has not been used to handling, and so they tend to disrupt the normal functioning of Input Processing. Further, because the hypnotist earlier has stated that he has the power to make the subject have unusual experiences, and the subject is now having unusual experiences, this tends to enhance the prestige of the hypnotist, and to give the subject more trust in him. This is a sort of "trick": by using physiological effects that the subject does not realize would come simply from staring at anything without the process of hypnosis being invoked, the hypnotist manages to take credit and so enhances his psychological effectiveness. The importance of this will become even clearer when we talk about Sense of Identity later.

Fifth, the hypnotist commonly suggests to the subject that he is feeling sleepy or drowsy. This acts on Memory functioning to draw up a variety of memory associations which help the induction process. For one thing, going to sleep means that one's baseline state of consciousness breaks down, so this acts as a disruptive force. Further, going to sleep is associated with one's body image fading out, so this enhances the fading of the body image already happening because of the adaptation of the Interoceptors due to the subject's relaxed, still posture. Too, going to sleep is a passive kind of state, you don't grit your teeth and will yourself to go to sleep; so it encourages a sense of passivity on the subject's part and further reinforces the earlier instructions not to think about what the hypnotist is saying, but just to accept it. And the reference to sleep draws up memories of one's identity fading; as you start to fall asleep you're generally not concerned about whether you're a butcher, a baker, or a candlestick maker.

Sixth, as well as suggesting sleep the hypnotist will often further indicate that it's not quite the same as real sleep because the subject will still hear him. The hypnotist may not need to suggest this overtly. Everybody in our culture knows enough about hypnosis to know that the subject can still hear him. This is a specific patterning force. The suggestions telling the subject that what is happening is like sleep primarily disrupt the ordinary state of consciousness, but the hypnotist does not want the subject actually to



go to sleep, so now he adds a patterning force to produce a passive sleep-like state, but one in which the communication with the hypnotist is still effective.

Seventh, if the subject appears very passive and relaxed, most hypnotic procedures then usually go on to simple motor suggestions, such as having the subject hold an arm horizontally out in front of him and telling him it is getting heavy. Usually motor suggestions like this are relatively easy to experience, and as the subject begins to respond to these suggestions the hypnotist's prestige is further enhanced. Now this automatic response to suggestion affects the Sense of Identity subsystem, for ordinarily it is our own voice inside us that tells us to do things and then the things happen. Now it is the hypnotist's voice saying such and such will happen and it happens, so the sense of self now begins to include the hypnotist. The special "modulation" from this subsystem that constitutes the ego sense is added to the stimuli that would ordinarily be perceived as the voice of an outsider. Psychoanalysts have often spoken of this as the transference element of hypnosis, especially when some of the transference involves parental transferences onto the hypnotist. The deliberate or implicit encouragement of identification with the hypnotist's voice is an application of patterning forces. Success with simple motor suggestions also produces a novel kind of stimulation of Interoceptors and Exteroceptors; one feels one's body moving but with different sorts of qualities than ordinarily. One's arm, for instance, feels exceptionally heavy and seems to move by itself. This is a kind of datum that again does not fit the habitual Input Processing patterns, and so tends both to disrupt the stabilization of the ordinary state of consciousness and to help pattern the hypnotic state. As a subject carries out simple motor suggestions, the hypnotist usually goes on to harder and more impressive motor suggestions and various kinds of cognitive suggestions, and continued success leads to increasing inclusion of the hypnotist within the subject's ego sense.

Finally, we should note that an exceptionally important aspect of the hypnotic induction technique is the subject's implicit expectations of what it's like to be hypnotized and how a hypnotized subject behaves. Ronald Shor (1964) did a survey which showed that among college students there is a fairly good general knowledge of what hypnosis is like, even though there are also numerous misconceptions. So if the subject agrees to be hypnotized and believes that the hypno-

tist can do it, he has all sorts of implicit expectations which affect his reactions to the particular things the hypnotist does.

### The Hypnotic State

If the induction is successful and the hypnotic state is developed, we have a discrete altered state of consciousness characterized by a quiet mind (Tart, 1966a). That is, most of the psychological subsystems we have talked about are not actively functioning. Typically if a deeply hypnotized subject is asked what he is thinking about or experiencing, the answer is "nothing." However, this state is also characterized by greatly enhanced suggestibility, so when a particular experience is suggested to the subject, he will usually experience it far more vividly than he could in his ordinary state of consciousness, often to the point of total experiential reality. Thus the hypnotic state shows a very high flexibility of functioning, even though it is relatively quiet between particular functionings. Also, there is a quality called rappport, a functioning of the Sense of Identity subsystem to make the hypnotist part of the subject's own psyche.

Now where is the transitional period, where is the quantum jump between one's ordinary state of consciousness and the hypnotic state? It is easy to see how the various techniques mentioned above operate on various psychological subsystems and push them toward extreme values of functioning, but where is the actual quantum jump? We do not know. Studies of hypnosis have generally paid very little attention to the transition point between hypnosis and waking. Some psychoanalytically oriented case studies (Gill & Brenman, 1959) have reported marked transitional effects, but essentially no studies have looked at the phenomena in a way that tried to pinpoint the exact nature and extent of the quantum jump.

Too, much modern research that has tried to see whether hypnotic suggestibility is indeed greater than waking suggestibility has committed the same methodological error I warned about earlier, namely using group data without looking at individual data, so that unless every individual were to make the transition at exactly the same point on the appropriate measure of psychological subsystem functioning, we would see no transitional point in the group data at all. To put it another way, if there were some one variable on which the jump were made from the normal state

into hypnosis, and one subject jumped from a value of two to six to make his transition, a second subject jumped from three to seven, and a third from four to eight, the group data would show absolute continuity and no evidence for a transitional phase. My theoretical position stresses the importance of examining this particular transitional period of hypnotic phenomena, and thus can act as a guide to future research in this area, either to extend and support or to refute the theory.

### Meditation and Meditative States

We shall further consider the process of inducing discrete altered states of consciousness by considering two types of meditation. I again emphasize that "meditation" refers to a variety of techniques which may or may not induce a discrete altered state of consciousness at a given time. Meditation techniques are incredibly varied, but Naranjo and Ornstein (1971) have broken them down into three basic types: concentrative meditation, opening up meditation, and expressive meditation. We shall consider the former two, and begin by considering a technique common to both of them before further distinguishing between them.

Most meditation techniques involve, as the initial step, sitting absolutely still in a posture that is not only comfortable, but which involves keeping the head, neck, and spine on a straight, vertical line, so that a certain amount of muscular effort is needed to maintain this posture. As was the case in inducing hypnosis, the comfortable posture in meditation allows various Interoceptors to adapt out, so the body image fades. In contrast with hypnosis, the fact that a slight amount of muscular effort is needed to hold the body in this upright position prevents sleep from occurring for most people. Hypnotic induction procedures can allow for the subject to slip in and out of actual sleep, but this is usually quite disruptive in meditative procedures, as the person begins to fall over. Since a lot of our sense of identity comes from our body image, the fading of the body in a comfortable, steady posture also tends to reduce the operation of our Sense of Identity subsystem. Further, the total stillness eliminates all kinesthetic feedback that would ordinarily maintain the body image. The vertical posture for head, neck, and spine is also of theoretical importance in some meditation systems which believe that a latent human potentiality, the Kundalini force, is stored at the base of the spine and may flow upward, activating vari-

ous other postulated latent potentials, the psychic energy centers or Chakras, as it rises.

### Concentrative Meditation

Concentrative meditation techniques basically involve the instruction to put all of one's attention on some particular point. This point can be an external object that is looked at fixedly, or some internal sensation such as the rise and fall of the belly in breathing. Similarly to hypnotic induction, the meditator is told that if his mind wanders away from this point, he is to bring it back to this point, rather than getting caught up in the distractions. Again similarly to hypnosis, this greatly restricts the variety of input to the system, inhibits thinking about various stimuli that come from scanning the environment, and in general takes psychic energy away from and reduces the activity of Input Processing, Evaluation, Memory, Identity, and Time Sense. Since the person is sitting absolutely still, the Motor Output subsystem has little to do beyond postural maintenance. Thus many sources of activity that maintain the ordinary baseline state of consciousness tend to fade out.

The fixing of attention by the Exteroceptors or Interoceptors on one thing can also produce unusual kinds of phenomena due to various kinds of receptor fatigue processes, similar to hypnosis, but in most meditation systems it is stressed that these anomalous perceptual phenomena are not to be taken as signs of success or to be paid any attention to, but should simply be ignored and attention continued on the one concentration point. So while these anomalous perceptual phenomena may act as disruptive forces for Input Processing, they do not have the same amount of attention paid to them as in hypnosis and so may have an overall different pattern of effects. As in any induction technique, the person sitting to meditate has all sorts of explicit and implicit expectations of what will come about.

### State Resulting from Concentrative Meditation

Naranjo and Ornstein describe the meditative state of consciousness that can result from concentrative meditation as being a discrete state described by such words as voidness, blankness, or no-thingness. There seems to be a temporary non-functioning of all of the psychological subsystems that we have been talking about so far except Awareness. In some sense, difficult to deal with verbally, aware-

ness seems to be maintained, but there is no object of awareness. The appearance of this meditative state seems to be very sudden and to represent clearly a quantum leap. The practices of meditation may quiet down the various subsystems, but then there is a rather sudden transition to this pattern of voidness. If you will look again at Figure 8, it may represent a transition from the hypnotic state to this state of void-experience. This meditative state may or may not be valued in and of itself, depending on the particular spiritual discipline and its philosophy. What does generally seem to be valued is the aftereffect of this meditative state, generally described as a great "freshening" of perception or increase in feelings of aliveness. In terms of our theory, one major thing that seems to happen as the aftereffect of the concentrative meditation state is that there is far less Input Processing than there is in our ordinary state of consciousness, much more "raw" sensory data is passed on to Awareness, instead of the highly selected abstractions we usually see, and this produces a great intensification of sensory perception of both the external world and one's own body. This is usually felt as quite joyful. As Wordsworth so vividly put it, in his "Ode: Intimations of Immortality,"

There was a time when meadow, grove, and stream,  
The earth and every common sight,  
To me did seem  
Apparelled in celestial light,  
The glory and the freshness of a dream ...

Going on to contrast this with his ordinary state of consciousness perception, he said,

It is not now as it hath been of yore;  
Turn whereso'er I may,  
By night or day,  
The things which I have seen I now can see no more.

I know nothing of Wordsworth's personal life, but I suspect that if he were alive today he would be quite interested in altered states of consciousness and a spokesman for many who find the life that flows exclusively from an ordinary state of consciousness inadequate.

This is a good point at which to remind you that a state of consciousness generally has many feedback processes stabilizing it. I'm sure many of you have had the experience of sitting down and trying to meditate according to some

prescription or another, and finding that rather than reaching some discrete altered state of consciousness, you only reached a sore back! Sitting still in the correct posture and trying to do the technique may indeed disrupt some of the customary feedback processes that stabilize your baseline state of consciousness, but if others are still active, such as continual thinking, no actual shift in state of consciousness will result. It is probably too late to keep people from using the words in a sloppy fashion, but we might say that a person "tried to meditate" when he attempted to carry out the instructions but didn't think he was very successful at concentrating or holding the posture, etc.; we might say he "did meditate" when he felt he was relatively successful in following the instructions, even though no meditative state developed; and we might say that he reached a certain kind of meditative state with certain described characteristics when he actually felt he had done so.

### Opening Up Meditation

This term refers to a variety of techniques in which the basic aim is to be fully sensitive to and aware of what-ever happens to you, being a conscious observer of what is happening to you without being caught up in it. It is a matter of being aware of what's happening without thinking about what's happening or becoming identified with what's happening. Vipassana is a Buddhist meditation of this sort, the word meaning something like bare attention to sensations, feelings, thoughts, and reactions to these things as they occur. The "simple" rule is to notice anything and everything that happens ("simple" to say, extremely difficult to do!): neither to reject anything as unworthy of attention, nor to welcome anything as worthy of more attention than anything else. Opening up meditation is usually practiced in the same sort of postures as concentrative meditation, so all the effects of posture on disrupting the baseline state of consciousness are similar.

This non-identification with any stimuli that reach one prevents a lot of psychological energy from being caught up in the automatic, habitual processes involved in the subsystems of Input Processing, Memory, Evaluation, Emotion, Identity, and Time Sense. Thus while Awareness is kept very active, other psychological subsystems tend to drift to lower and lower levels of activity. Traditional accounts indicate that after a high level of success is achieved, there is a sudden shift into a meditative state of consciousness

characterized by a great freshening of perception and deautomatization of Input Processing. This is part of the meditative state itself, rather than an aftereffect of it as in concentrative meditation. Almost all psychological energy is present in the Awareness function, and there seems to be far less Input Processing, so things are perceived more directly. The phenomenology is that one experiences things as much more intense and clear and valid; whether this means that one does perceive the external more accurately has not, to my knowledge, been tested.

### USE OF DRUGS TO INDUCE ALTERED STATES

I spoke earlier of a methodological pitfall that comes from equating a technique that might induce a discrete altered state of consciousness with the altered state itself. This error is particularly seductive when one is talking about psychoactive or psychedelic drugs, for we tend to accept the pharmacological paradigm, the essence of which is that the specific chemical nature of the drug interacts with the chemical and physical structure of the nervous system in a lawfully determined way, invariably producing certain kinds of results. Observed variability in human reactions tends to be seen as the perverseness of psychological idiosyncrasies interfering with basic physiological reactions, and is averaged out by treating it as "error variance." While this pharmacological paradigm seems usefully valid for a variety of simple drugs, such as barbiturates inducing drowsiness and sleep, it is quite inadequate and misleading when one deals with psychedelic drugs like marijuana or LSD.

Figure 10 shows a complete model of drug effects that I developed when I was beginning to study marijuana intoxication (Tart, 1970c; 1971). In addition to the physiological stimuli or forces impinging on the subject, shown in the upper right side of the figure, there are a large number of psychological factors which, in many cases, are far more important for determining whether a discrete altered state of consciousness will occur and what the exact nature of that altered state will be than are the physiological effects of the drug. Thus while it is useful to know what drug a subject has taken, the quantity of the drug, and the method of administration, in some ways these are some of the least important things one can know. Without having some knowledge of the psychological factors, it may be very difficult

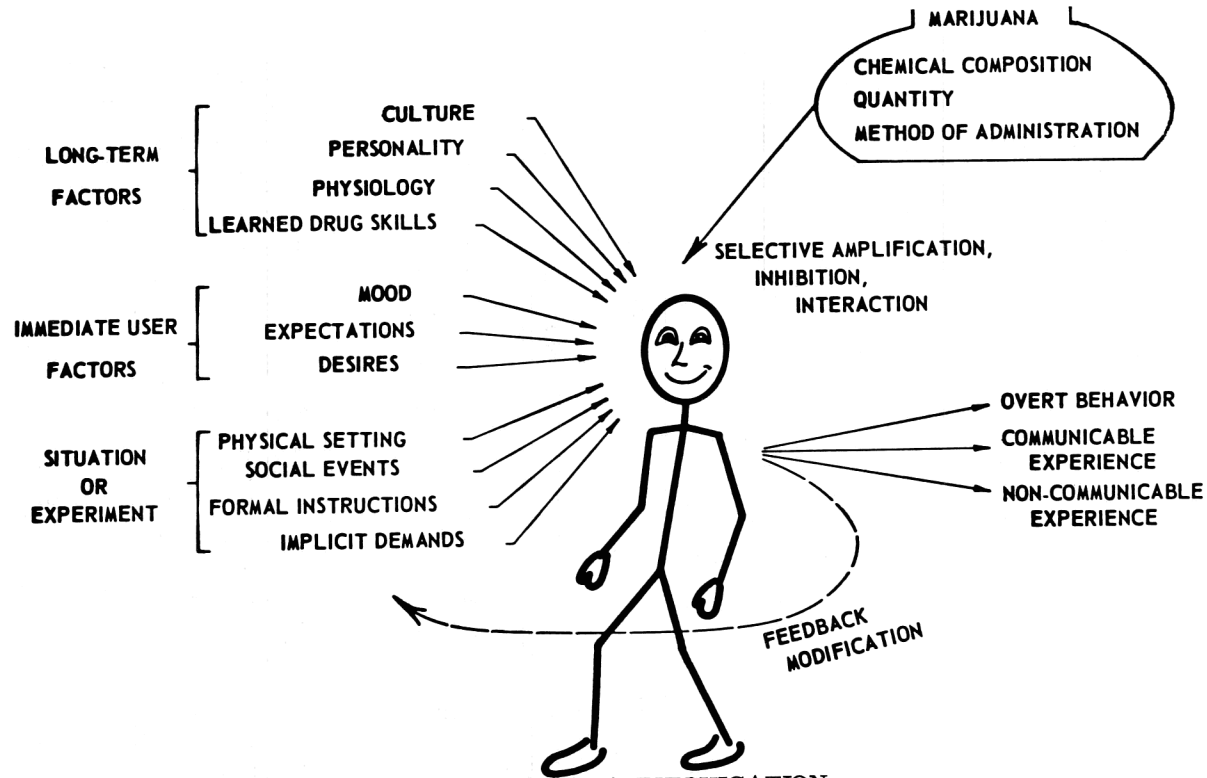


Figure 10. FACTORS AFFECTING MARIJUANA INTOXICATION



to predict with any accuracy at all what the subject's behavioral and experiential reactions will be.

These non-drug factors include the culture a person was raised in and all the effects it has had in terms of structuring his baseline state of consciousness and providing specific expectations about the drug, the particular personality of the subject, possible specific physiological vulnerabilities he might have to the drug and, particularly, his learned drug skills: has he taken this drug many times before and learned to enhance desired reactions and inhibit undesired reactions, or is he naive with respect to this drug, so that most of his energy will go into coping with the (often stressful) effects of novelty? Then we come to a class of more immediate factors. What is the subject's mood when he takes the drug, since this mood may be amplified or inhibited? What does he expect to experience in this situation? Are these expectations the same as what he desires to experience?

Then there are other factors involving the situation or experimental setting in which the drug is taken. What is the physical setting, and how does that affect the subject? And how about the social setting? What kind of people are with the subject and how do they interact with him? A frightened person present, for example, may communicate his fright to the subject sufficiently to make the effect of the drug quite anxiety-provoking. If this is an experiment, what are the formal instructions given to the subject, and how does he react to and interpret them? Perhaps even more important, what are the demand characteristics, the implicit instructions, and how do they affect the subject? If, for example, the experimenter tells the subject that the drug is relatively harmless, but has him sign a comprehensive medical release form, the total message communicated is much more complex than "this is a relatively harmless drug." Further, the subject is not a completely passive victim of all these forces impinging on him, but may selectively facilitate the action of some of them and inhibit others. Time prevents us from going into any detail on these many non-drug factors, but they are vitally important in determining how a drug will affect a person, whether or not a discrete altered state of consciousness will result from the drug's action, and the specific nature of events within that altered state.

Given this cautionary note on the complexity of using

drugs to induce altered states of consciousness, there are nevertheless a few general things we can say about drug-induced states in terms of our theoretical model. Particular drugs may have specific effects on the neurological basis of the various psychological subsystems we have discussed, possibly exciting or activating some of these subsystems, suppressing or slowing the activity of others, or altering or distorting the particular mode of information processing within a subsystem. Psychological processes in relatively unaffected subsystems may compensate for changes in affected subsystems and/or maintain sufficient feedback stabilization so that the baseline state of consciousness does not break down. The drug itself produces physiological effects, but our interest is in the psychological effects that go to make up a state of consciousness. We certainly should remind ourselves that the nature of the interaction between physiological states and mental states is largely unknown.

#### Learning to Use Physiological Effects

Given physiological effects that may affect various subsystems, we must however remember that it is how you interpret a particular physiological effect that determines much of your reaction to it and whether or not a discrete altered state of consciousness may result. To take one of the most common examples, most marijuana smokers have to learn how to achieve the discrete altered state of consciousness we refer to as "being stoned." Typically, the first few times a person smokes marijuana he may feel an occasional isolated effect, but the overall pattern of his consciousness remains that of his ordinary state, and he usually wonders why people make so much fuss about taking a drug which doesn't seem to do much of anything interesting. With the assistance of more experienced drug users, who suggest he focus his attention on certain kinds of happenings or try to have certain specified kinds of experiences, additional psychological factors, patterning forces, are brought to bear to disrupt the ordinary state of consciousness and pattern the discrete altered state of consciousness. Often quite suddenly the transition takes place and the person finds that he is now stoned. This is a good illustration of how the physiological action of the marijuana may disrupt many of the ordinary feedback stabilization processes of our ordinary state of consciousness, but not all of them, so the state of consciousness does not necessarily change.

We should also note that it is a common experience

for marijuana users (Tart, 1971) to say they can "come down" at will. That is, if they find themselves in a situation that they don't feel able to cope with adequately while in the discrete altered state of consciousness of marijuana intoxication, they can deliberately suppress all the effects and temporarily return almost instantly to their ordinary state of consciousness. By psychological methods they can disrupt the altered state and pattern their ordinary state into existence.

A third and quite striking example of the importance of psychological factors in deciding whether a drug produces a discrete altered state of consciousness or not comes from a review by the pharmacologist Solomon H. Snyder (1971) of the attempts to use marijuana in medicine in the nineteenth century. Snyder says, "It is striking that so many of these medical reports fail to mention any intoxicating properties of the drug. Rarely, if ever, is there any indication that patients--hundreds of thousands must have received cannabis in Europe in the nineteenth century--were 'stoned,' or changed their attitudes toward work, love, their fellow men, or their homelands.... When people see their doctor about a specific malady, they expect a specific treatment and do not anticipate being 'turned on.'" Apparently then, unless you have the right kind of expectations and "a little help from your friends," it is unlikely that marijuana will produce a discrete altered state of consciousness. Thus, equating the ingestion of marijuana with the existence of a discrete altered state of consciousness is a very tricky business. One might think that this shows that marijuana is a weak drug, but it is also the case that some people do not respond to large doses of far more powerful drugs.

### Major Psychedelic Drugs

In dealing with very powerful drugs like LSD, mescaline, or psilocybin, we come into an area of extreme variability of outcomes that requires great caution in terms of an altered states model. It is clear that for almost everyone who takes these more powerful psychedelic drugs, their ordinary state of consciousness is disrupted, and in a sense we may say that the primary effect of the powerful psychedelic drugs is to disrupt the stabilization processes of the ordinary state of consciousness, so that the state breaks down.

While there is a great deal of commonality of experi-

ence among marijuana users, at least in our cultural setting (Tart, 1971), so that it is useful to speak of the "marijuana state" as a distinctive state of consciousness across subjects, variability with the powerful psychedelics is so great that I interpret present evidence as showing that there is no particular state of consciousness at all produced by them. Rather, we see a highly unstable state in which you never get more than very transient formations of patterns that constitute discrete altered states of consciousness. The temporary association of scattered functions in the third part of Figure 7 illustrates this. We see continuous transition between various kinds of unstable states.

Now while this is probably true for most of the uses of powerful psychedelics in our culture, it is not necessarily true. Those of you who have read Carlos Castaneda's fascinating accounts of his work with don Juan (Castaneda, 1968; 1971; 1972) will realize that Castaneda's reactions to psychedelic drugs were like this, but don Juan was not interested in having him "tripping." Instead he tried to train him to stabilize the effects of psychedelic drugs so that he could get into a particular discrete state of consciousness suited for a particular kind of task at various times. So with the addition of further psychological patterning forces to the primarily disruptive forces caused by psychedelic drugs, it is possible that stable states of consciousness can be developed that will have particularly interesting properties such as the use or understanding of psi faculties. Meanwhile, we should not use terms like "the LSD state," or believe that the statement, "X took LSD" tells us much of anything about the effects on X's consciousness.

#### ALTERED STATES AND PARAPSYCHOLOGY

I have tried to give you an overview of the psychology of states of consciousness, with some commentaries on the application of this theory to parapsychology. My overview has been all too brief, and I have not had time to go into any degree of detail. I hope eventually to write a small monograph to present the theory and its application to particular states of consciousness in detail, but to finish our evening together, let me make a few more remarks on relationships between parapsychology and discrete altered states of consciousness.

We should remember that you don't have to have a

discrete altered state of consciousness in order to get psi phenomena working. Most of our data for the existence of psi were collected from ordinary subjects in their ordinary states of consciousness and many of our most gifted sensitives claim that they work in their ordinary state of consciousness and simply listen to their hunches. But for most people psi is a very latent function indeed, one that may never appear at all in their lives, or only appear very sporadically and unpredictably. For some of these people, learning to function in some particular discrete altered state of consciousness may provide the key to making latent psi abilities functional. Some models which illustrate this point are those of mediumship and shamanism.

If we want to use discrete altered states of consciousness to tap latent psi abilities, however, I believe we will need a much more sophisticated research approach than we have had in the past. Some of the older parapsychological research with altered states of consciousness and psi seems to be based primarily on an implicit paradigm that psi was far out and mysterious, and hypnosis or LSD or whatever was also mysterious, and therefore if you combined the two, you should get lots of psi. So subjects were hypnotized or whatever and told they had marvelous psi abilities, and were then expected to go to it! As you are aware, results from this procedure have often been non-existent. Sometimes there has been a slight improvement in psi functioning, and very occasionally, possibly through good luck, a relatively significant increase in psi performance. Indeed, I would predict, not banking on luck, that if we're going to learn to use discrete altered states of consciousness to enhance and understand psi functioning in the near future, we are going to have to become investigators of altered states per se, as well as parapsychologists. There isn't a highly developed, highly refined science of discrete altered states of consciousness for us to draw upon. This branch of science is in its infancy, so we have to be concerned with understanding states of consciousness themselves, not just their application to parapsychology. (I might add that this can be a good thing in some ways. Since the study of altered states of consciousness is becoming respectable, it can provide a good source of grant funds for research, with some parapsychological research done as a fringe benefit.)

To adequately research discrete altered states of consciousness and their possible applications to parapsychology, you are going to have to develop a level of sophistica-

tion in research that is a notch above that still generally prevailing in psychology in general. You will for example have to overcome the operational fallacy of equating the application of an induction technique with the development of a discrete altered state of consciousness, as I talked about earlier. You will have to learn how adequately to map experience. You are going to have to deal with the problem of individual differences, which may make the concept of a discrete altered state meaningless for one subject and the key to psi performance in a second subject. You will have not only to get subjects into a discrete altered state of consciousness, but also to help them explore the range of functioning in that state of consciousness, and help them stabilize their functioning in that state of consciousness. This may mean some kind of growth or psychotherapy procedures to make subjects comfortable in the altered state, and it may mean specific psychotherapeutic work dealing with fears of psi phenomena. And let's not kid ourselves that there are no latent fears of paranormal phenomena, fears on the experimenter's part as well as the subject's. This is a fascinating topic but we don't have time to go into it here.

You are also going to have to realize that while psi abilities may be a latent function in one or more discrete altered states of consciousness, they may not be there in fullblown form, but only as a potential. So in addition to a subject getting into the discrete altered state of consciousness and learning to function generally in it, you then have to specifically develop the subject's latent psi potentiality. We have an excellent illustration of how this might be done in Milan Ryzl's early experiments (1962) involving developing the psi faculty through hypnosis. Ryzl's is probably the most sophisticated research that has been done on hypnosis and psi, for he did not make the common error found in older studies of simply assuming that because a subject was in a hypnotic state and was told he had psi faculties, he then had them. Rather, he effectively used three specific properties of the hypnotic state and further applied basic learning theory in a way I argued it should be applied several years ago (Tart, 1966b) to get results.

Specifically, Ryzl used the fact that hypnosis can produce a very quiet state of mind, and so eliminated most ordinary types of distractions for the subject. He used the easily-manipulable motivation in the hypnotic state to motivate subjects to develop psi, and he used the fact that subjects can visualize intensely in hypnosis by asking them to

try to produce images of objects placed on a tray in front of them. He then applied basic learning theory by having subjects open their eyes to compare the images which came to them in the hypnotic state with the actual target material, so over a number of trials they could gradually learn to discriminate the characteristics of veridical from those of non-veridical images. I think this is a good model to suggest how discrete altered states of consciousness may help us develop psi functioning, and I regret that there has never been any adequate replication of Ryzl's technique. We should also realize that psychological functions, in terms of my theory of states of consciousness, do not exist in isolation but interact with other psychological functions, subsystems, to form a total system. Thus I think we should be wary of the idea that some discrete altered state of consciousness will produce psi phenomena at our convenience. We must find out how these phenomena interact with other characteristics of that state, and work within this more complex framework.

I have spoken at length about how discrete altered states of consciousness may be helpful to parapsychology; the reverse is also true. The prevailing physicalistic philosophy in science aims at reducing all of the phenomena experienced in altered states to chemical and electrical interactions. If one experiences contact with a higher entity in an altered state that profoundly affects one emotionally, the experience "simply" demonstrates, from the physicalistic point of view, how arbitrarily we can program any sort of "nonsense" into our brain functioning. The data of parapsychology demonstrate that the physicalistic approach is useful, but certainly incomplete in very important ways. They tell us that it is incorrect to treat the human mind as something exclusively bounded by the brain and body, and so encourage us to take a wider look at some of the more intriguing and important phenomena of altered states experiences. If, for example, a person in a meditative state reports that he became merged with an object and knew it in some profound, intimate sense, perhaps this is just a matter of altered functioning of the Sense of Identity subsystem, but perhaps it would also be appropriate to do some clairvoyance testing. Time prohibits discussion of this here, but I shall go into it in detail elsewhere (Tart, in press).

Another idea that I have no time to develop, but which follows from this theory of states of consciousness, is the proposal that we have to learn to apply scientific

method while we are functioning in certain discrete altered states of consciousness, and so develop state-specific sciences that are quite different from the kind of sciences we have now (Tart, 1972b; 1972c). The development of these state-specific sciences may provide not only a way to increase psi functioning, but also a key to understanding these important human faculties that seem so incomprehensible in our ordinary state of consciousness and so incommensurate with the one state-specific science we do have--ordinary state of consciousness science, and the map of reality that has been built up from it.

I believe the possibilities for enhancing psi functioning and understanding it through the development of scientific knowledge of discrete altered states of consciousness are enormous, but it is going to be a very long and complex struggle. Quite aside from their importance to parapsychology, altered states of consciousness are vital in understanding religion, creative inspiration, and many other aspects of human behavior, and it is a good sign that this area is starting to become a part of science instead of continuing to be isolated in the "taboo" areas of religion and superstition.

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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-THE-BODY EXPERIENCE**
- CALL.** An individual guess to a specific target.
- CLAIRVOYANCE.** ESP of a physical event.
- DECLINE EFFECT.** A decline in scoring during a series of trials.
- DIFFERENTIAL EFFECT.** A differential scoring rate between two procedural conditions within the same experiment.
- DISPLACEMENT.** An ESP response to a target other than the intended one.
- DT [Down Through] PROCEDURE.** The clairvoyance method in which the cards are called down through the pack before they are checked.
- ESP [Extrasensory Perception].** Information obtained by a person about an event without the use of known means of information.
- ESP CARDS.** Cards bearing one of five standard symbols: circle, cross, square, star, and wavy lines.
- FREE VERBAL RESPONSE METHOD (FVR).** Any procedure in which the range of targets is not known to the subject, such that he is free to make any response he wants.
- GESP [General Extrasensory Perception].** Any method designed to test the occurrence of ESP which permits

either telepathy or clairvoyance or both to operate.

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

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

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

**PK** see **PSYCHOKINESIS**

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

**PRECOGNITION.** ESP of a future event.

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

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

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

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

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

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

**RETROCOGNITION.** ESP of a past event.

**RUN.** A group of consecutive trials.

**SENSITIVE.** An individual who purportedly has strong psi ability.

**SPONTANEOUS CASE.** An unplanned natural occurrence apparently involving psi.

**SUBJECT.** The person whose psi ability is being tested.

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

**TELEPATHY.** ESP of a mental event.

**TOKEN OBJECT** see **PSYCHOMETRY**

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



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