RESEARCH IN PARAPSYCHOLOGY 1979

Abstracts and Papers from the
Twenty-second Annual Convention of the
Parapsychological Association, 1979

WILLIAM G. ROLL

Editor

The Scarecrow Press, Inc.
Metuchen, N.J. & London
1980

ISBN 0-8108-1327-0
Manufactured in the United States of America
Library of Congress Catalog Card No. 66-28580

CONTENTS

PREFACE:
THE TWENTY-SECOND ANNUAL CONVENTION

The twenty-second Annual Convention of the Parapsychological Association was held at St. Mary's College, Moraga, California, on August 15-18, 1979. The Convention was sponsored by John F. Kennedy University, which has the first accredited graduate degree program in parapsychology in the United States. Approximately 260 people attended the Convention. John Palmer chaired the Program Committee, which also included Arthur Hastings and Joseph Rush. Dr. Palmer also chaired the Arrangements Committee, assisted by graduate parapsychology students at JFKU.

The program included 17 research papers, 22 research briefs, three symposia and three roundtables presented on the Convention floor. In addition, 20 papers and one symposium were presented by title only in "poster sessions." Authors of poster papers could arrange to meet with interested persons in classrooms off the Convention floor to discuss their reports. Another innovation this year was combining full papers and research briefs in thematically integrated sessions on the program. Abstracts of the reports discussed in this paragraph are included in this volume of Research in Parapsychology.

Karl H. Pribram of Stanford University gave the invited dinner address. This annual address has been named the J. B. Rhine Lecture to honor the founder of the Association. Pribram's talk was entitled "Holeness, Wholeness, and No-Thingness." John Palmer of John F. Kennedy University gave the Presidential Address, entitled "Parapsychology as a Probabilistic Science: Facing the Implications."

In addition to the printed parts of the program, there were three workshops: "Computer Applications," organized by Dick Bierman; "Psychics in the Laboratory," organized by Stanley Krippner; and "Applications of Psi," organized by Alan Vaughn.

A special tribute was paid to Gardner Murphy, who passed away last March 18, by several senior parapsychologists whose lives he touched. On behalf of Council, John Palmer presented a plaque to Eileen Coly of the Parapsychology Foundation as an expression of the Association's appreciation of the Foundation's many contributions to parapsychology.

If anything distinguished this year's convention, it was the spirit of restlessness that surfaced in many of the papers. There was an impatience with the traditional methods of parapsychology combined with a groping for more creative applications of the scientific method toward greater understanding of psi. One sensed that beneath the California sun were being sown the seeds of a transformation that would revolutionize the field of parapsychology.

John Palmer
Chairperson
Program Committee

Part 1: Symposia

PSI AND SCIENTIFIC METHOD: VIEWS FROM DIVERSE PERSPECTIVES*

ON THE GENESIS OF RESEARCH HYPOTHESES IN PARAPSYCHOLOGY

Rhea A. White (Reference Department, East Meadow Public Library)

In order to learn about psi, we must first ensure that it is actually operating in our experiments. I think we must consider the possibility that whether or not psi occurs in a given experimental design may depend in part on how the question the experiment was designed to test was generated. I propose that it is important for the experimenter to be personally interested in the answer or answers his or her experiment is designed to reveal. By avoiding this approach, we may actually be shutting psi out of our experiments! Second, I think we need to be more creative in asking our questions. We must be personally enthusiastic about our own investigations. Our (often justified) conservative and cautious approach may be working at cross-purposes with our creativity--with that personal, enthusiastic attitude most favorable to the occurrence of psi. Parapsychologists overemphasize facts and logic; it is enthusiasm, passion, instinct, intuition and desire that will enable us to establish footholds on the new land we are all seeking. I propose that the answers we seek lie more in our emotions and feelings than in our heads. The experiments we design should be outgrowths or developments of those experiences or feelings or fantasies that brought us into the field in the first place. We have to feel out and sense the nature of a reality that can explain psi--a reality that necessarily lies beyond the limits of the senses and of our rational faculties and logical abilities, yet encompasses them as well. We are seeking for hard facts and hypotheses capable of verification, but to succeed we must initiate our search by a leap of faith--by a belief that the answers are there, and that we will find them by faithfully following our feelings.

This is not an unscientific approach. It is the essence of scientific creativity. We must ask why, then, is it missing in the formulation of most hypotheses in parapsychology? I suggest that one of the greatest factors holding us back--preventing us from

*Organized and chaired by Elmar Gruber, Institut für Grenzgebiete der Psychologie und Psychohygiene, Freiburg.

asking the really important questions--is our response to our critics. Our defensiveness is a hindrance to us even in those areas where we are trying to be constructive--in our educational efforts and in our research itself. Of course we need to be cautious and circumspect, but we must be aware of the fact that this attitude, which is so prevalent in our field, is not only discouraging prospective parapsychologists but in the name of science may be preventing us from progressing as a science! There is little to be gained and much to be lost by dealing with our critics on their own terms. I do not feel that either the positive findings of parapsychology or the arguments of the skeptics are based on rational foundations. The "reasons," both for and against, are secondary. We are dealing in fact with the irrational. Yet we let the rational, after-the-fact arguments of our detractors push us into a defensive corner by ourselves placing reason first, and so we are not in a position to rest on that tide of faith, confidence and conviction that, it seems, is a prerequisite for results in our field. We must stop being defensive and self-defeating about the questions we ask, the ways in which we conduct our research, and in proposing wild hypotheses about the nature of psi. If we are not accepted as a science, the fault may lie partly with ourselves. We behave as underlings. We defend ourselves as if indeed we are secretly guilty. If in fact we are a frontier science--and, surely, if we are not, no one is--then let us behave as frontier scientists, and let the devil take the skeptics!

SCIENCE AND PARAPSYCHOLOGY: AN IDEOLOGICAL REVOLUTION

Michael Winkelman (University of California, Irvine)

If parapsychologists are to apply Kuhn's notions of paradigm, paradigm shift or scientific revolution, these notions must be assessed critically. Kuhn's presentation of the historical developments and contemporary state of science seems valid and valuable. The prescriptive aspects, which suggest that science should function as "normal science," conflict with his descriptions of how science must function in order to progress. There seems to be an implicit and uncritical acceptance of Kuhn's prescriptive notions within parapsychology. Analysis of the activity of science, the nature of parapsychology and the relationship between the two reveals that this attitude is misleading and detrimental to the progress of parapsychology. Acceptance of "normal science" as a methodological approach will not lead parapsychology to the changes necessary to end stagnation of the discipline, nor will a scientific revolution or paradigm shift as conceived of by Kuhn lead to an integration of parapsychology into science.

In his Postscript-1969, Kuhn suggested terminological clarifications. Paradigm is used in two major senses: a global one, relabeled disciplinary matrix, which includes paradigms and other aspects of scientific activity; and a particular one, relabeled exem-

plars, which are aspects of the disciplinary matrix. The disciplinary matrix includes: 1) symbolic generalizations--the formalized components, such as laws and theories, which serve as "laws of nature" and definitions of the symbols they employ; 2) metaphysical paradigms, which are shared commitments to particular models, metaphors and analogies; 3) values, widely shared aspects concerned with the characteristics of prediction and theory testing; and 4) exemplars, concrete problem solutions that serve as the basic models, providing a tool or gestalt that is used to solve other problems by seeing them as similar. The activity of normal science can thus be described as an exhaustive application of the elements of the symbolic generalizations through the models provided by the exemplars to the problems in the domain specified by the metaphysic.

Applying Kuhn's schema, we can see that the disciplinary matrix of parapsychology has been notably lacking in symbolic generalizations, laws and theories that would organize the field under a single set of interrelated principles. This lack is tied to the nature of parapsychology's metaphysic, which has two aspects; a negative one, opposed to materialism/mechanism, and an implicit one, borrowing the models and analogies of the physical and psychological sciences.

The negative metaphysic, directly and explicitly opposed to materialism, is the point at which parapsychology has differed most radically from the other aspects of science. Rhine made it explicit that he was using experimental methods to disprove materialist theory (metaphysic) by the demonstration of operations that would defy physical description. Because of this negative metaphysic, parapsychology has lacked the theories that play an integral role in generating progressive or constructive problem-solutions within the research tradition. However, the formulation of investigations and the models of explanation in parapsychology implicitly contained the main influences and assumptions of the materialist/mechanist tradition in the models, analogies and experimental methodology it borrowed from the dominant scientific tradition.

The values of parapsychology have generally been consonant with those of the wider scientific community, and as embodied in the methodology and emphasis upon quantified data, at the positivist extreme. The failure to meet these values derives from problems of repeatability and the failure to generate predictions and theories.

The exemplar that extrasensory perception provided for the field was quickly shown to be inadequate, particularly by psychokinesis. Although this conceptualization has long been a block to the discipline, there has been a general failure among parapsychologists to question this psychobiological model. The unexplicated anomalies generated by these conceptualizations have brought parapsychology to a yet-to-be resolved paradigm crisis.

Although parapsychology can be analyzed in terms of the elements of the disciplinary matrix, the applications of Kuhn's notions

about science to an analysis of parapsychology reveal an inconsistency in the notion of parapsychology arising (or ever arising) as a new paradigm within science. The anomalous findings of parapsychology did not arise as accidental by-products of normal science within established paradigms, but were generated by Rhine and others who directed their investigations toward demonstration of operations that would support a metaphysic directly opposed to the dominant materialistic metaphysic of science. This demonstrates major differences between the development of parapsychology and the activities of normal and revolutionary science. In a paradigm shift or revolutionary science, the metaphysic remains constant, with the symbolic generalizations reinterpreted in light of a new exemplar. Parapsychology cannot be conceived of as a paradigm with normal science as long as it has a different metaphysic, nor could a scientific revolution integrate parapsychology into science without a change in parapsychology's metaphysic to a materialist/mechanist one. If parapsychology were to be integrated into science as a new paradigm, it would utilize the same symbolic generalizations (laws, theories, elements, concepts) presently utilized. Parapsychology doesn't even conflict with the laws and theories of science as would be the case if parapsychology was generating anomalous phenomena within a paradigm of science; the clash is at the level of metaphysical assumptions.

Science's metaphysic provides mechanical and mathematical metaphors/models; it is unlikely that phenomena construed as nonmechanical and nonphysical can be explicated with them. Parapsychologists have been disinclined to construct radically novel metaphors or even borrow existing ones. This avoidance lies in a lack of awareness that science must build from untested and untestable assumptions. Although parapsychology was founded upon a metaphysic opposed to materialism/mechanism, it has borrowed mechanistic models rather than constructing new ones.

Parapsychology's adherence to a mechanistic metaphysic is strongly exemplified in its methodology. Experimental mechanisms assume mechanistic cause-and-effect relations, and establish a context within which an object of investigation is construed in a manner amenable to prediction and control. However, the knowledge acquired is not of the object itself, but of the relationship to the object, mediated by the experimental apparatus and forced to conform to the structure of the investigation. Although psi is not bound by space and time, the knowledge we acquire through experiments is only that which conforms to the assumptions of universal mechanism and the assumptions of linear causality, or "causality that are..." which is embedded in the structure of the experiment. Parapsychologists, in constituting their objects of investigation, have assumed higher-order concepts consistent with materialism and mechanism; consequently, their knowledge primarily tends to that which conforms to those assumptions.

The differences between parapsychology and mainstream science suggest a strong resemblance to the Copernican Revolution, which was an ideological revolution within science. An ideological

revolution involves a basic change in the assumptions as to the nature of science, humankind and the universe; it involves a whole new set of observations and evidence, an entirely new world containing a new view of the human being and our capacities for knowing, new observational language, new means of observation, new frames of reference and new conceptions of frame of reference.

Rhine has argued that it is necessary to go beyond mere physical control of targets, personnel or other experimental conditions, and that a nonphysical methodology appears logically to be the very kind needed to solve the problems of psi indeterminacy. Parapsychology is concerned with a study of the mind, yet lacks a methodology appropriate for studying such an entity. That parapsychology should focus upon mind or consciousness is hardly a novel suggestion. Tart has even laid out extensive procedural guidelines for such studies. However, parapsychology has yet to act upon such assumptions. The realization of the ideological revolution necessary for the continued advance of parapsychology has been stymied by an obsessive adherence to methodologies of investigation and models of explanation developed within a metaphysic with which parapsychology assumes direct conflict.

Parapsychology argues for modes of knowledge and action denied by the dominant metaphysic; on this basis parapsychology must reconstruct itself. Parapsychology needs an epistemology, methodology and metaphysic consistent with the assumptions and knowledge about psi. Parapsychology will not make considerable further progress until it constitutes an approach consonant with the phenomena under study.

USING THE SCIENTIFIC METHOD TO PROBE THE LIMITS OF SCIENCE

John Beloff (University of Edinburgh)

Beliefs may be divided into the categorical and the conditional. The former constitute that sector of our belief system that we are not prepared to modify: our religious or ideological dogmas, our moral axioms, our values and so on. The latter comprise all the propositions of science, which, inasmuch as they purport to rest on argument and evidence, are necessarily conjectural and conditional. Beliefs about the paranormal may be matters of faith, in which case they belong to the former category, but those that derive from parapsychology must, as must all scientifically justified claims, be open to reasoned criticism.

But science represents not just the empirical approach to truth, as implied by the phrase "the scientific method"; it also represents an explanatory framework for the understanding of natural phenomena. The question arises as to whether this explanatory

framework, which has proved itself in the domain of physical phenomena, has universal validity or whether there are phenomena that lie outside its scope. It is the thesis of this paper that psi phenomena transcend the limits of scientific explanation and thus present a challenge to the concept of a unified science with its reductionist implications.

Many parapsychologists, however, still fiercely resist this conclusion and seek to preserve the fiction that eventually, in the long run, the unity of science will be vindicated by the development of a more comprehensive physics that will embrace the paranormal as a special case. In particular, they often draw comfort from those aspects of modern quantum theory that seem to legitimize effects that are space-time independent.

Nevertheless, that this is, indeed, a fiction can be appreciated when we analyze conceptually what is involved in a typical psi phenomenon. For what, in the end, is critical in ESP or PK is that thoughts and ideas are exchanged and intentions are made concrete without any discernible mechanism or system for coding the information. And it is not just that we have not yet succeeded in finding this missing mechanism. The point, as Stephen Braude has been at pains to stress, is that no such mechanism is conceivable--it could not make sense. For such a mechanism would imply, in the first instance, that some thought or intention could be represented unambiguously by some definite brain state. And there are cogent reasons for supposing that no such one-to-one relationship is possible. Furthermore, the recent attempts by the so-called "observational theorists" to overcome the difficulties inherent in the communicational model of psi, by focusing on the act of observation itself as the critical psi effect, fail to meet these logical objections while introducing fresh paradoxes and absurdities of their own.

The conclusion is drawn that, by adhering strictly to the scientific method, parapsychology enables us to demonstrate what conventional psychology by itself cannot do, namely the reality of a realm that transcends the scope of scientific inquiry as hitherto understood.

PSI, METHODOLOGY AND THE SOCIAL CONTEXT

Elmar R. Gruber (Institut für Grenzgebiete der Psychologie und Psychohygiene)

Any discussion about methods and their respective applications to a specific scientific field must take into consideration the special social conditions in which the researchers are involved. This is particularly important for disciplines not belonging to mainstream science.

By a historiographical analysis, we find a macro method of one kind or another and an almost innumerable number of micro methods leading the business of science. In this context, Feyerabend's attack against certain features of mainstream science is not an attack against scientists and their work but more against science as an ideology of macro method. Scientists--and parapsychologists too--believe that something like a macro method exists, and that such a method is necessary in order to gain acceptable knowledge. In everyday scientific practice, scientists feel rather free in applying different micro methods which of course should not be in contrast to the macro method. At some times in the process of scientific endeavor this macro method consists of only very few vital assumptions, categories necessary to secure communicable knowledge.

The question is why do parapsychologists suppress micro methods of research that do not fit their own background macro method, even when their own epistemological basis suggests different methodologies? The only answer can be that they think they found the right way into the realm of knowledge, a way compatible with the commonly accepted macro method. All mechanisms of acceptance or repression of scientific work have to be seen against this background. And parapsychologists would surely have good reasons and rational arguments to reject a scientific work not based on their methodological convictions.

An anarchistic (or, say, dadaistic) point of view would now challenge this view by arguing that everything based on an exclusive method is nothing but ideology and any other way of gaining knowledge may be as successful as the scientific. Indeed there might be ways that would fit the epistemological background better than the accepted methodology of science. Naturally the question is left: how does the argument that everything in science is possible help scientific progress? Someone can turn the argument around and accuse the principle of everything goes (Feyerabend) of being based on an ideology of plurality. But it is exactly this style of argumentation that shows us the relativity of such concepts in science, and no one can tell in advance if it is better to stick to a certain method or to change the method in regard to a certain scientific question. But this relativism also implies that rival ideas should have equal right to be brought into the scientific community and equal right to have access to gateways scientists who based their work on the scientific method have.

We cannot decide in advance whether it is reasonable to change our methodology, say, in poltergeist research, because of certain features of the cases (gap-phenomena, imp-like behavior, etc.) that seem to escape a methodology based on the exclusive scientific method (and in fact hitherto did escape) or to stick to the way of scientific methodology (and have some minor changes only in regard to micro methods). An optimistic dadaistic researcher would joyfully propose a number of different ways to deal with this not-always sympathetic imp, while an equally optimistic rationalistic

scientist would see the special poltergeist features not as something catapulting us off the scientific method but rather as something significant that can be the starting point for a scientific investigation. Who can dare to decide, as so-called competent professionalists often dare to?

But of course there is a major problem in being tolerant of sciences not belonging to the accepted mainstream of sciences. Even if being bold, extravagant, dadaistic and anarchic could be something very fruitful for all sciences, it is hard to accomplish for parapsychology because of its low degree of acceptance in the scientific community and hence its unfavorable social context to dare being different. Only a careful reconsideration of the role of social structure in science can lead to resolution of the problem of ideological incarceration in the name of rationality and lead to a pluralistic free society that could give parapsychology the background for having the chance of being extravagant; no matter if parapsychologists want to be so or not.

METHODOLOGICAL PERSPECTIVES
ON PSI RESEARCH*

PARANORMAL GROUP DYNAMICS

Michael Grosso (Jersey City State College)

Several investigators (Murphy, Myers, Roll, Warcollier) have already underscored the idea that psi may be the property of certain kinds of interpersonal relations. In addition, the crucial role of the experimenter is under current scrutiny. But so far there has been little exploration of the variables and mechanisms of paranormal group dynamics.

Among possible psi-conducive groups, three types can be distinguished: spontaneous, quasiexperimental and nonquantitative experimental. In the spontaneous type there is no conscious aim to produce psi, but psi-related incidents seem to occur with greater frequency than outside the group. Some examples would be the mother-child relationship, families, twins, nonfamily intimate relationships, patient and therapist. Also I would include here the interpersonal structure associated with poltergeist phenomena.

The second type of psi-conducive community is quasiexperimental in two senses: a) certain standard procedures are employed, and b) the procedures involve a specific aim directly or indirectly concerned with something paranormal. A wide range of examples are available for study, e.g., the Eleusinian Mysteries of ancient Greece, primitive initiation rites, the Shakers and Perfectionists of nineteenth-century America, Senoi Indian dream practices, current charismatic prayer groups and so forth. The field is as vast as it is unexplored.

A third source of material it might be interesting to reexamine includes the impressive seances of the early period of psychical research with an eye to better understanding their interpersonal dimension. The table-tilting experiments of Batcheldor and the group PK studies of the Toronto workers may provide some clues to the relevant psychological skills in paranormal group dynamics. Under this heading belong the healing workshops of Lawrence LeShan and the dream workshops of Montague Ullman.

---

*Organized and chaired by William G. Roll, Psychical Research Foundation.

There are at least three ways the concept of a psi-conducive community could be tested. We could select members of apparent psi-conducive groups from their natural setting and match their psi prowess against groups of ordinary, isolated individuals. A second approach would be to manipulate group-oriented variables--chanting, fasting, controlled repression and so on--in other words, to "initiate" experimental groups, including perhaps experimenters, data-gatherers, judges and other possibly influential observers, and to measure their psi performance against comparable but "uninitiated" groups. A third possibility would be for research centers to revise and modify their own group dynamics in terms of possible models of psi-conducive communites.

There are numerous, testable group variables that may be psi conducive. To cite just one example that continually crops up, there are the effects of shared imagery. If I may use an analogy from music; I cannot play a duet with you unless our instruments are tuned to each other. No matter how well we play as individuals, unless the pitch of my A matches yours, the result will not be a "hit" in the world of music.

In sum, I want to make two remarks about parapsychological methodology. First, there is the natural assumption that some distinct person or entity is the causal agent producing the psi--it might be subject, experimenter, potential observer. But suppose the psi is the product of a certain kind of relation, in which the autonomy of the individual is drastically minimized. Unfortunately, the model, Subject-Hits-Target, has a tendency to reinforce the dualistic split between subject and target. Instead of a hit-the-target-I-am-a-star approach, it might help to explore the holistic potential of the experimental group. In order to allow psi to manifest more readily, participants in an experiment might think less in terms of getting--whether it be a high score or evidence in support of a hypothesis--but more in terms of giving, investing, losing the self in a larger superpersonal structure. We observe just this in the classic mysteries and rituals: the "subjects" fast, deprive themselves of sleep, strip themselves of personal identity--in a word, undergo a kind of death and dismemberment.

All this must sound very forbidding and uncomfortably demanding. Why would anyone choose to engage in such an undertaking?

This brings me to my second point. A model of hitting the target is linked to a model of research as problem solving. I think it possible to shift the basic orientation here without sacrificing skills in problem solving. The new dimension might be to think in terms of a model of experimentation as need-subservient and self-developmental. Parapsychology might very well advance if it allied itself to transpersonal psychology, for the good reason that psi may be a transpersonal effect. The model would see psi as a by-product of a more fundamental process of self-transformation. Several studies already seem to be moving in this direction. Roll, Solfvin

and Krieger for instance, performed an experiment with group targets as part of a meditation procedure. This is a very economical approach: if you miss out on the psi, you might at least profit from the meditation. Further, if psi is indeed goal oriented, it may be wise to try to mobilize the deepest needs and goals of the organism as part of the experimental process. At any rate, I think it a misguided methodology that would coax persons to perform a miracle by tempting them with trivia and superficial rewards.

USING ALTERED STATES OF CONSCIOUSNESS TO FACILITATE OR STUDY PSI: SOME METHODOLOGICAL SUGGESTIONS

Charles T. Tart (University of California, Davis, and SRI International)

As psi generally functions poorly in our ordinary state of consciousness, there has always been interest in using discrete altered states of consciousness (d-ASCs) to facilitate or study psi. Empirical results to date have been somewhat positive, but erratic and far short of the potential promise. We do not have well-developed scientific knowledge of various d-ASCs to draw upon straightforwardly, but even with the knowledge we have, parapsychologists usually have done studies that show poor state-of-the-art knowledge of d-ASC research.

We have better scientific knowledge about hypnosis than most other d-ASCs. This paper will sketch how a hypnosis and psi experiment should be done to be state-of-the-art with respect to hypnosis, and thus allow much better understanding of the relationship between the hypnotic d-ASC and psi functioning.

First, general changes in procedure from the "colonial" paradigm so typical in experiments (Tart, JASPR, 1977, pp. 81-102) should be made so participants in experiments are comfortable, feel something to learn or gain from the study, and trust the experimenter.

Next, we must be able to specify the hypnotic susceptibility of the participants and/or screen for specific hypnotic capabilities. The most effective route starts with group screening using the Harvard Group Scale of Hypnotic Susceptibility (HGSHS). Further measurement, as well as adaptation to the laboratory situation and to being in the d-ASC of hypnosis, should be obtained by individual administration of the Stanford Hypnotic Susceptibility Scales (SHSS). Forms A and B of the SHSS focus mainly on motoric suggestibility, and supplement and extend the HGSHS measures. Form C of the SHSS measures many cognitive and perceptual hypnotic abilities, such as dreaming, hallucinated tastes, negative hallucinations, age regression and posthypnotic effects. Use of the HGSHS, SHSS and Forms A, B, and C allows quite adequate assessment of general hypnotic susceptibility.

Hypnotic susceptibility is a long-term performance average. Hypnotic depth refers to a construct that can vary quite rapidly during experimentation. Participants should be taught some method, such as the North Carolina Scale, of reporting depth, so psi tests can be carried out at uniform depths.

When deeper hypnotic abilities are to be investigated, assessment by the Stanford Profile Scales of Hypnotic Susceptibility (SPSHS) is a necessity. Further training sessions to develop or stabilize special hypnotic abilities may be added at this stage.

With participants who are well adapted to the laboratory and whose hypnotic capabilities are well known, we may now add psi components to the experimental sessions. Self-reporting of depth should be used to be sure the d-ASC of hypnosis is actually present and that it is within the desired depth range during the actual psi testing. These procedures should allow much more accurate specification of hypnosis and psi measures by minimizing uncontrolled sources of error variance.

At the conclusion of an experiment, the experimenter should not only debrief the participant, the participant should debrief the experimenter, i. e., the experimenter should treat the participant as a co-experimenter who was in a unique position to make important observations.

Hypnosis is a powerful technique. The experimenter should have appropriate training in psychology and hypnosis to screen out potentially unstable participants and handle occasional unpleasant consequences of hypnosis. The training and ethical standards of the Society for Clinical and Experimental Hypnosis and the American Society for Clinical Hypnosis are highly relevant for any parapsychologists contemplating hypnosis research.

IMAGERY, RESONANCE AND PSYCHIC HEALING

Ralph G. Locke (University of North Carolina, Chapel Hill, and Psychical Research Foundation)

In recent years, imagery studies have regained their lost salience in psychology and psychiatry. This revitalization has been authored mainly by workers in the areas of psychotherapy and cognitive-processes analysis. There are strong trends in the evidence available that therapeutic movement can be produced in cases of psychogenic and physical illness by utilization and development of patients' various imagery modalities. Moreover, these clinical and laboratory findings are augmented by cross-cultural studies of healing that focus on the effectiveness of symbols--shared mythical-cultural systems in relatively homogeneous societies. The value of imagery in healing, following this research, is in the recognition

that imagery is a complex, adaptive system that links the "internal" human environment with the "external" environment. It is, in other words, a system of encoded information distributed in a way that its operation is only detectable by recognizing the level of its reference and its structural arrangements--not its content, primarily. It is an epistemic system.

Most healing takes place in face-to-face situations so that the healer and patient are engaged in reciprocal communication and activation of imagery in several modalities. Recent research highlights the extent to which imagery matching is central to effective healing. Several areas have contributed to the development of hypotheses concerned with imagery matching.

1) Semiology: Studies in human communication emphasizing relations of signification and pars pro toto logic suggest that healing rotates about the establishment of resonance (imagery matching) between patient and healer. Establishing resonance involves the adoption of a particular, passive-receptive attitude, which has parallels in esoteric healing practices, including psychic healing and meditation healing. This attitude allows healers to discern and use the patient's language and imagery code to effect insight and physical behavioral change. Healing may involve a range of imagery modalities concurrently and may also involve mode switching (synaesthesia and guided change). Most important, healing is conceived of as a set of transformations between epistemic systems (imagery-encoded information distributions) of a noncausal type.

2) Interpersonal psychology: Resonance has been the subject of clinical psychotherapy studies concerned with identifying the essential ingredients of effective treatment. In brief, effectiveness is related to adoption of a specific attitude by the therapist (a phenomenological attitude), which allows resonance but not complete identification with the patient. Psychological components of resonance include accurate empathy, unconditional acceptance and genuineness, which amount to a nontechnical, nonreductive perspective, as well as empathic communication.

3) Neuropsychological: Semiological analysis of part-whole (metaphor-metonymy) relations involved in healing transformations is augmented by recent work on models of consciousness put forward by Levenson, Pribram and Prigogine. Parts (say, digital information) and wholes (analogue information) are united by way of structural transformations at any level of organization. So, in the first place, any change occurring in healing (say, insight) might be considered as an expansion or reorganization of "configurational" (imagery) systems. The logic of change in the Levenson-Pribram approach is that of structural identity and transformation modeled on hologram physics. This allows for explanation of spatiotemporal effects emanating from aspatial, atemporal epistemic systems (memory, imagery). Moreover, expansion of the model to include notions of self-organization, structure induction and nonequilibrium in neurological and psychological systems has been suggested as the basis of a

theory connecting resonance with psi (and psychic healing, consequently). Recent work by Prigogine and others has suggested an increase in sophistication of the hologram model, particularly in terms of describing conditions of change in systems, action at a distance and connections between levels of organization of physical and biological systems.

4) Phenomenology: Contemporary and classical studies of embodied consciousness underscore the need to consider imagery systems in terms of part-whole relations, and, further, to cast them as kinds of knowledge. In these terms, healing can be understood as a process of boundary change and reorganization in imagery, which are essential to the integrated operation of the body at varying levels of organization. Evidence supporting this view derives from diverse sources, but the principal areas are studies of the ecology of consciousness and schizophrenia, changes in the integration of consciousness in psychological and physical trauma, and the analysis of changes of consciousness in becoming a medium or shaman and practicing healing.

From these sources and others concerned with the relation between imagery and psi under more limited conditions, resonance can be considered in the following terms. (A minimal definition is covered by parts (a), (b), (d), while the remaining two parts, (c) and (e) are hypotheses concerned with structure-induction--i. e., change in a certain direction.)

A definition of resonance: Resonance is a relational process

a) Where there is an identity between epistemic structures in one subject and another, at the same level (unconscious, conscious, volitional, nonvolitional, etc.);
b) where each level is a unity with the field of embodied consciousness, comprised of physiological and psychological-semiological elements (a psychosomatic relation expressed in digital-analogue terms);
c) where the relation is initiated and sustained by a type of phenomenological attitude that alters the constitution (perceptual-communicative boundaries) of routine, focal awareness;
d) where the epistemic structures that are in relation are Imagery (or representational) systems, which are the wholes contexting the parts of communication exchanges; and
e) where change in epistemic structures occurs as a function of identity and of novelty only when identity is established first, and particularly when the elements of an epistemic structure of one subject are communicated in an inverted form to that subject from the other.

ARE WE SHAMANS OR SCIENTISTS?

Rex G. Stanford (Center for Parapsychological Research)

The claim is heard nowadays that some parapsychologists, particularly the most successful ones, unintentionally manipulate the results of their experiments through their own psi capacities. Indeed, every experimental parapsychologist known to me who has a record of regularly obtaining significant outcomes also has been personally quite successful as a subject in psi studies, usually in both ESP and PK paradigms. This, however, is only circumstantial evidence.

Another interpretation of that observation is that experimenters who have personally experienced psi events with some regularity may for that reason have uniquely useful insights about how to arrange experimental settings, instructions and interpersonal interaction to facilitate psi in their subjects. It never hurts to have the view from the inside! Such insight may also lead to more appropriate ways of testing a given hypothesis.

Among experimenters with actual psi experiences, the sense of the reality of psi and expectations about its occurrence may be greater. These may be communicated to subjects and may result in better psi performance. Taddonio (JP, 1976, pp. 107-14) reported that induced experimenter expectations influenced ESP performance in the direction of the expectation.

Another possibility for explaining the success of certain experimenters is that they may have personalities different from unsuccessful experimenters and that these personality differences may lead to differences in social interaction that influence ESP performance. My personal observations of investigators suggest that this may be a factor, and Carl Sargent (personal communication) has recently gathered data that indicate that psi-favorable experimenters are higher on sociability and are more happy-go-lucky than psi-inhibitory experimenters. Both factors relate to greater extraversion.

All these possibilities should be considered and investigated. Also, when we argue that significant results are due to experimenter psi, we should acknowledge an implicit, untested assumption behind that reasoning. Even granted that we dismiss the alternatives above and further assume that the experimenter's psi is powerful enough to account for the data, we must also assume that the desire for a specific outcome is greater than for learning the truth, or that the desired outcome is more rewarding or efficacious.

My own feeling is that while experimenter-psi influences upon subjects' thoughts and behavior are likely, the same internal states within the subject that would allow such an influence would also allow an influence because of that subject's own dispositions or needs.

There is no known way to rule out experimenter-psi influence upon subjects, but let us consider psi-mediated experimenter influences that do not act through the subject. These possible experimenter effects include: 1) PK influence on the random event generator (REG) used to study PK, 2) PK influence on the REG used to generate ESP targets and 3) ESP or PK influence upon arbitrary choices made in planning, conducting or evaluating the study (including such influences on procedures as shuffling, cutting or coin flipping.

It seems plausible that we could significantly reduce experimenter influence of the second and third types discussed above. To do so, we must reduce as far as possible the number and salience of decisions made in such a way that psi might reasonably influence them and thus bias the outcomes of the study. It may make little or no difference whether these decisions are made by an REG or by a human brain.

As concerns use of an REG to generate ESP targets, a better way to get targets for many process-oriented studies is through the use of standard lists of random numbers. Even the entry point into such a list need not be determined by an REG, but by some fixed rule.

Since by implementing such suggestions, the number of psi-mediated experimenter effects should be reduced, we may thereby enhance cross-experimenter replicability and our significant findings may be more meaningful with respect to the hypotheses tested.

In some studies, there may be little or no importance in knowing with respect to whom the psi is occurring. Nonetheless, in most circumstances we wish to make inferences about what is happening with and to a particular person. In such circumstances, the role of scientist requires that we do whatever we can to minimize psi-related ambiguities in our research. To do otherwise is to place the role of significance getter or magic maker above the role of scientist.

ARE PSI OCCURRENCES RANDOM?

T. N. E. Greville (University of Wisconsin, Madison)

Do psi events occur randomly? If not, are we justified in using statistical techniques to explore what conditions favor or inhibit psi occurrences? Do not these techniques assume random behavior?

The question does not arise when the sole objective is to demonstrate that psi exists, for then the "null hypothesis" is chance, and by definition chance behaves randomly. There is a frustrating

dilemma here. We need statistics to tell us more about how psi works, but reliance on the statistics is questionable until we understand more about how psi works.

A CASE OF HAUNTING

William G. Roll (Psychical Research Foundation)

A few years ago, a death occurred that some of us still seem to be unaware of. You may have to go to the obituaries by Rhea White or Jim Kennedy and Judy Taddonio in the ASPR Journal (1976, pp. 133-66; pp. 333-69) or the Journal of Parapsychology (1976, pp. 1-33) to convince yourself that the Experimenter in parapsychology has really died. No near-death experiences were recorded, though some reports suggest that the independent experimenter and observer we knew so well, or thought we knew, at times returns. I suspect that we have not fully faced our loss and that it is this denial that sometimes creates a ghostly presence.

What the Experimenter seems to haunt is of course the Psi Laboratory. I suggest the evidence for the existence of this is also weak and that in fact the Laboratory disappeared at the time the Experimenter did. Most of us realize that the Experimenter cannot be brought back, but some of us believe that the old Laboratory is still there, and we conduct our experiments as if nothing has happened. Then we are surprised when psi effects appear and disappear mysteriously in our tests.

I think the erratic character of our findings may have something to do with our belief in the Laboratory.

As we struggle to understand the nature of the places within whose walls psi experiments go on, we recall the words of William James:

> Let us take outer perception, the direct sensations which, for example, the walls of these rooms give us. Can we say that the psychical and the physical are absolutely heterogenous? On the contrary, they are so little heterogenous that if we adopt the common-sense point of view, if we disregard all explanatory inventions--molecules and ether waves, for example, which at bottom are metaphysical entities--if, in short, we take reality naively, as it is given, an immediate; then this sensible reality on which our vital interests rest and from which all our actions proceed, this sensible reality and the sensation which we have of it are absolutely identical one with the other at the time the sensation occurs. Reality is apperception itself. . . . In this instance, the content of the physical is none other than the psychical. Subject and object confuse, as it were.

This relationship, or fusing of subject and object, is confusing only if we are unaware of it. We have stubbornly refused to recognize the common-sense point of view James called to our attention. And common sense is here supported by extra sense--for instance, cases of the experimenter effect that apparently were communicated via physical objects, such as the Fisk-West studies, where the subjects reacted differently to the ESP cards that had been handled by the two experimenters and the linkage work Rhea White refers to in her survey (JASPR, 1976, pp. 337-38).

If no clear distinction can be made between organism and environment, should we not expect that the dispositions, memories and other characteristics of the experimenter may be found also in his or her physical surroundings, including the laboratory where psi tests are conducted?

Just as there is no experimenter apart from the subject, so the laboratory itself--walls, tables, equipment--may psychically reflect the people who occupy it and their changing needs and attitudes. According to this picture, the particular hypothesis to be tested would be only one strand in the fabric; other threads might be woven by the unstated purposes experimenter and subject have in performing a psi test. These purposes in turn connect with the images and goals of the society in which the experiment is embedded.

If the experimenter and his or her laboratory do not exist as distinct entities and if the hypothesis that seems to guide an experiment is intertwined with all sorts of other goals, it can be no surprise that results of laboratory experiments rarely bear out the stated hypotheses. If physical structures are also psychical, the notion of a laboratory as independent of the people who use it should be laid to rest. Taking account of a laboratory effect along with the experimenter effect might bring some consistency into the results of our tests.

SYNCHRONICITY AND PSI: CAUSAL VERSUS ACAUSAL PARADIGMS*

SPONTANEOUS WAKING-STATE PSI AS INTERHEMISPHERIC VERBAL COMMUNICATION: IS THERE ANOTHER SYSTEM?

Barbara Honegger (Washington Research Center)

Synchronicity, or perceived meaningful coincidence, is not an explanatory principle, as often claimed. Like ESP or PK, it is a label for a class of phenomena that requires explanation and, as such, cannot be used to explain more well-defined classes of psi any more than they can be used to explain it. Consensual agreement about the improbability of many such coincidences, their often exquisite fit with ongoing affect and mentation, and their timing in relation to them, however, qualify synchronicities as subject matter worthy of serious investigation by parapsychologists.

Because the probability of individual coincidences is impossible to calculate precisely, and because the contribution of unconsciously guided actions cannot be ruled out with impunity, it is difficult to argue conclusively that any one synchronicity entails a psi component. It is possible, however, to allow subjective criteria to define the data--to deal with perceived meaningful coincidences--and then to look for possible structural relationships amongst elements and their associations in sequences of such events. When selection criteria are defined in this way and one moves beyond individual anecdotes to series of perceived synchronistic events, structural relationships do emerge that relate sequences constructed in this way to the manifest content of dreams, the sine qua non of evidence for unconscious mentation. I have cases in my collection where sequences of synchronistic events, including some striking individual cases, are structurally isomorphic to the manifest content of dreams, serve the same purposes--primarily to fulfill or simulate the fulfillment of wishes/needs--and are mediated by the same primary process logic.

If one provisionally accepts that spontaneous waking-state paranormal events, including synchronicities, are structurally isomorphic to dreams, it can be argued that the neurological substrate associated with dreaming is also essential for the production of

*Organized and chaired by Barbara Honegger, Washington Research Center.

waking-state psi-mediated events, and that the relationship between dreams and other better-understood cognitive processes should also hold for spontaneous paranormal occurrences.

Perhaps surprisingly, a fruitful line of investigation has proven to be the relationship of dreams and spontaneous waking-state paranormal events to natural language. My own work interpreting both phenomena using the classical free-associational method led to the generalization that the deep structure of each is a proposition in the natural language of the dreamer/percipient. For dreams, this view has received strong support from the recent work of David Foulkes, who states categorically that "words are the form in which dream images originate, and verbal/propositional structures mold the way in which [dream] images are expressed." I agree fully, and extend this generalization to verbal "daythoughts," which mold the way in which waking-state spontaneous psi-mediated events are selected and organized.

Relating spontaneous waking-state paranormal events to dreams, and both to verbal processing, enables us to make some very specific predictions about the neurological substrate(s) that should be shown necessary for psi processing. Consistent evidence from a number of sources, especially experiments with and reports of split-brain and lesion patients, suggests that the right-hemisphere inferior parietal lobule (IPL) is essential for dreaming. The location of this substrate--opposite the language-comprehension (Wernicke's) area of the left hemisphere--suggests that the right-hemisphere IPL is an independent verbal-processing system heretofore unrecognized because the verbal deep-structure of its productions must be reconstructed from the primarily visual manifest content of dreams and the primarily visual experience of psi-mediated events in the waking state. Evidence from the split-brain studies also makes it likely that this independent verbal-processing system, if real, and when active, is associated with an independent consciousness/personality. Communication between the consciousnesses is held to be the purpose of psi.

As the right-hemisphere IPL has also been identified as the neurological substrate associated with manipulospatial abilities--the ability to express the appreciation of spatial relations via the musculature--it is likely that psi (deep-structure verbal) processing and manipulospatiality compete during Normal-Everyday-Awake (NEA) for the same processing capacity. If this is so, we should expect that paradoxical sleep, during which the musculature is actively inhibited by the central nervous system, and muscular relaxation should free processing capacity for psi. Both, in fact, have been shown to be psi-conducive.

There is evidence that direct, neurologically mediated communication of right-IPL-initiated verbal information to the left IPL is normally blocked during NEA.

The general position of this paper is that rebus-like dreams

and spontaneous paranormal events, their waking equivalents, are ways that a postulated right-hemisphere language system (right IPL) communicates during NEA to the left-hemisphere IPL. In encoding verbal propositions into the "language" of select, probabilistically anomalous portions of NEA experience, it confronts all the difficulties attendant on translation from any language or modality into another; and, because translations are never perfect, its messages often appear camouflaged to the left. Likewise, the left's verbal decodings may often appear forced upon the data to those unfamiliar with their efficacy in making sense of a broad range of cases. Only be means of free association, as with the manifest content of most dreams, can the ego (left IPL) decode the messages implicit in such experiences. What we call Psi-Mediated Instrumental Response is the right-hemisphere initiated/left-hemisphere rationalized motor behavior necessary to frame such events for the primary sensory systems.

I have reviewed and reinterpreted the experimental literature on lateralization effects in psi processing, which generally links psi to verbal processing. I have also related the model to OBEs, RSPK, and GESP, and offered a second, more conservative version of the hypothesis in light of experimental findings.

SYNCHRONICITY, CAUSALITY AND CONSCIOUSNESS AS CREATOR

Alan Vaughan (New Ways of Consciousness Foundation)

Carl Jung's definition of synchronicity as "acausal meaningful coincidence" included psi phenomena, since psi has no physical explanation in classical physics. As Eisenbud points out, psi was synchronicity for Jung and therefore cannot explain synchronicity. Beloff's argument (JSPR, 1977, pp. 573-82) that psi is superior to synchronicity as an explanatory hypothesis does not take into account that the how and why of psi are unknown.

Pratt (JASPR, 1974, pp. 133-55) suggests that psi may in the future become causal when lawful principles are understood even though they may not be reduced to traditional science. But what if what we know about science can be reduced to psi principles?

In a popular study of synchronistic experience (Incredible Coincidence, Lippincott, 1979), I classify coincidence cases with tentative psi interpretations, such as unconscious foreknowledge. Cases of "chance encounters of the third kind"--in which one finds entirely by chance the very thing one is searching for--often involve many people, achieving a "psychic corps de ballet effect."

Cases in which unlikely parallels manifest between two strangers--such as the same three names, same day and year of birth, same year and make of car, having children at the same

hospital, living and working in the same places and so on--may be expressions of the same archetype or distinctive pattern of consciousness that tend to repeat.

Both meaningful coincidence and psi indicated to Jung that space and time are relative to consciousness. I suggest that space and time are relative to consciousness because they are created by consciousness; matter is added to the constructs of consciousness, since it cannot exist separately. This hypothesis suggests that cause and effect are always illusory--not just when we notice something strange. Consciousness is viewed as creating the cause-and-effect illusion of reality by creating space, time and matter.

This may be the principle by which consciousness created the universe in a big bang; note that John Wheeler speculates that our consciousness participated in the creation of the universe. David Bohm has developed a related theory, in which consciousness creates space, time and matter and is the ultimate substrate of energy and matter.

Viewing psi as a direct measurement of consciousness, I suggest that consciousness is "the hidden variable" of the subquantum level. Although all phenomena of nature eventually can be derived from subquantum levels as constructs of consciousness, only psi can be measured as direct evidence of consciousness in action. All ESP and PK phenomena, which can be interpreted as alterations in space and time, may be indicative of the fundamental structure of reality.

Several experimental implications can be listed: 1) psi should be independent of space and time, since psi is a measurement of consciousness, which creates space and time; 2) units of consciousness (people) should show distinctive "mindprints" in psi tests; 3) subliminal consciousness is activated by high intensity of attention, suggesting that high-interest experiments will have better psi results than low-interest experiments (interest being determined by questionnaires); 4) if consciousness creates by manipulating chance variables, the greater the number of chance variables, the greater the likelihood that psi will manifest in both ESP and PK tests; 5) if consciousness changes something when it observes something, ESP and PK are opposite sides of the coin of consciousness. ESP tests with random number generators should show deviations from chance (by PK) when ESP scores are high; 6) if consciousness creates space and time, a core of consciousness must exist outside of space and time. Spatial and temporal distances should not be a variable in psi impressions about the long dead, the recent dead and the living; 7) psychokinesis may be the result of consciousness altering space and time. Metal bending, instant healing, poltergeist effects and teleportation may be interpreted as "special effects" of consciousness in creating space and time; and 8) only psychological variables should affect psi results.

In conclusion, if consciousness creates space, time and matter on the fundamental subquantum level, we may find that psy-

chology and physics spring from the same root of consciousness. Will consciousness research fulfill Pratt's prediction (JASPR, 1974, p. 152) that theorizing that psi occurs at a different level of reality may enormously increase the productivity of parapsychological research?

TRANSCULTURAL UNIFORMITY OF POLTERGEIST PATTERNS AS SUGGESTIVE OF AN "ARCHETYPAL" ARRANGEMENT

Hans Bender (Institut für Grenzgebiete der Psychologie und Psychohygiene)

The word "poltergeist," of German origin but now more commonly used in English and French, means a "noisy, racketing type of demon." Differing from the neutral term "RSPK," it describes a personal entity as originator of the normally unexplainable events that show a transcultural uniformity. The French police officer Emile Tizané demonstrated this uniformity. He analyzed phenomenologically many cases of poltergeist events investigated by the French police between 1925 and 1950. Among the patterns that Tizané found in comparing the independent reports are the following, in a rank order of oddity: 1) often a house becomes the object of a hail of projectiles; 2) bangs against the doors, the walls or the furniture are heard; 3) objects are skillfully dislocated or thrown; fragile objects are often unbroken, while solid ones are sometimes completely destroyed; if the objects hit persons, they mostly do not injure them; 4) displaced objects sometimes show an unusual trajectory; they behave as if they had been transported; 5) in some instances foreign objects penetrate into a closed space; when handled by observers, the objects may sometimes be warm; and 6) objects seem to form themselves in the air.

"Oddity" in this context means incompatibility with familiar energetic processes and normal physical laws. This trait has an extremely important bearing on the problem of how we can understand the poltergeist and come up with workable theories. Tizané speaks of "an invisible intelligent power" and designates the role of the focal person as "intermediary." The Flemish author M. Maeterlinck introduced the term L'Hôte inconnu, "the Unknown Guest." Modern investigators of RSPK cases, such as Roll and Artley, Cox, Eisler, Larcher, Palmer, Pratt and others (the author included), describe the events in question as if there were no agency other than the focal person or focal persons, the "mediums." This "animistic" interpretation differs from the spiritualistic one, which in many cases regards the agency as a nonmaterial intelligent being existing independently of the medium. There is no doubt that many traits of the "poltergeist syndrome" seem to support our preferred hypothesis--dependence of the events on the presence of the focal person or Roll's and Artley's "attenuation effect": the decrease of the number of incidents in proportion to the distance of the focal person. Many

other traits could be quoted. They include a comparative diagnosis of the personality structure of RSPK mediums, as, among others, J. Mischo undertook it. But there is a drawback that indicates a problem: if we suppose that the subconscious mind of the medium is inventor and instigator of the nearly always aggressive, destructive and evasive phenomena, we have to answer the question of how an individual mind may know about the uniform patterns that seem to be autonomous and can mostly not be reduced to mechanical processes.

Let us consider the throwing of stones, a dominant transcultural trait of the poltergeist with the characteristic behavior: "The missiles seem to be aimed at someone, and to move with great speed, but they usually hit lightly or veer off" (Roll). This is obviously an intelligent behavior, but what does it mean? As the most plausible interpretation, I would say it is a threatening gesture, a somewhat infantile teasing, part of the general chaffing character of poltergeist occurrences. The eventual warmth of penetrating objects, stones mostly, seems to have a symbolic function and indicates an affective form of aggression, the uniform manifestations of which cannot be known to the instrumental person. Thus the poltergeist seems to have some degree of independent existence. This conception is not at all new: it is known as the "psychic-crystallisation theory," as it was formulated by William Barrett in his article--"Poltergeist, Old and New" (PSPR, 1911, pp. 377-412). In his understanding, the poltergeist is a psychic reality. It is not coterminous with the personality of the medium and has a temporary life of its own (A. R. G. Owen). This theory corresponds very closely to the conceptions of Carl Gustav Jung, especially to his notion of an "archetypal arrangement" as part of his hypothesis of synchronicity. Jung describes archetypes as dispositions of a "collective unconscious" in which the earliest experiences of humankind are stored. This collective unconscious is identical with nature in its entirety, the transcendent background of which seems to be neither physical nor psychical but a third structure, the "mundus unus" of the alchemists. Archetypes may be regarded as "messengers" of this transcendent reality, in which "mind" and "matter" are one and the same. Their message is mostly a symbolic one, as can be seen in archetypal dreams. In the inner structure of personality, archetypes as instruments of the coincidence of the opposites combine mind and body (i. e., instincts), but their arranging power transcends the limits of personality as an organism: it may comprise the outer world, so that the physical aspect of an archetype may be represented by an inanimate object, which then behaves as if it were an animated being. In Jung's view, the majority of paranormal phenomena happen in archetypal situations characterized by a high emotional tension, such as venture, danger, fatal circumstances and so on. Poltergeist phenomena, in Jungian language, are the actualization of an archetype of aggression that arranges the intelligent behavior of quasianimated objects.

"Archetypal arrangement" is a descriptive term and not a concept of substance comparable to "waves" and "particles" in quantum theory. It may inspire tests such as the search for regular

correlations within the symbolical structure of poltergeist events, to mention but one possibility. The methodology of poltergeist research has to take into account a complex system that cannot be reduced to a mere level of physics (Kornwachs and v. Lucadou).

SYNCHRONICITY, PSYCHODYNAMICS AND PSI

Jule Eisenbud (Denver)

Jung's concept of an acausal principle, which he terms "synchronicity," lacks logical coherence on several grounds, not the least of which is its failure to take into account the indispensable role of an observer in structuring or interpreting the events held to be "synchronistic." Secondly, the concept is psychologically naive in neglecting the needs of the observer (or interpreter). It is parapsychologically naive, finally, in neglecting the considerable range of conceivable latent psi powers of the observer. These could bring about the very events that may appear to him or her to be of external (and acausal) origin.

It has long been known to psychiatrists that thoughts and feelings that are denied access to consciousness in their original form tend to be projected into external events, where they sometimes come back at us giving the feeling that we are the center of something mysterious going on in the very bowels of the cosmos. This can be seen in purest form in the paranoid ideation of schizophrenic psychoses, where, however, anything like consensual agreement on the significance of the strange coincidences that seem to peep out of every corner of existence would be totally lacking. Where, on the other hand, coincidences are such as to induce widespread intuitive agreement on the long odds involved, it is impossible to eliminate the observer (some observer) as a conceivable instrumental agent in bringing about precisely what appears to us so uncanny. This is the case not on the basis of what can be inferred from laboratory data on the presumptive limits of psi, but from our entire cumulative experience with the subject. This points to the possibility that every individual may be latently capable of the wide-ranging cognitive psi exhibited in the best mediumistic performances and cross-correspondences, as well as of the kind of PK manifested by the best physical psychics. If we add to this the possibility of widespread psi-mediated influence of one person on the decisions and behavior of others, then there is nothing in the domain of so-called synchronistic occurrences that does not lend itself to heuristic interpretation along active lines. Such an approach, however much it stretches our conventional notions of psi functioning, has at least some empirical support and leaves us still with some shred of today's somewhat torn fabric of causality. It also encourages us to explore what can be explored in the human mind rather than to throw up our hands at the unfathomability of the universe.

In many instances, however, it is not required, theoretically, that an observer-agent produce all the elements in a multiple coincidence. Starting with one or two that could conceivably be the result of chance, and adding to these the effects of our own psi-mediated intervention with persons and things, as well as the effects of our own unconsciously guided actions, we can sometimes bring about what would certainly appear to be an extraordinary series of coincidences.

In such cases, the observer-agent can be construed as elaborating the coincidences involved as one would elaborate a dream out of bits and pieces of situations already in the works. The observer-agent can be imagined as maneuvering him or herself in relation to these elements much as a bird or a glider pilot adroitly makes use of air currents already there. At the most, he or she would be doing unconsciously, and with no manifest effort at all, what a movie director goes to great pains to do on a conscious level, that is, deploying props, natural surroundings, and events that are there anyway, as well as the wide capabilities of "central casting," to achieve any desired effect. Moving people or things about and into position for this or that "shot" would be no more difficult than simply ordering main characters, extras and prop persons to do thus and so, with special effects as needed. Such an approach, with all the indeterminacies involved, is preferable to a nonmotivational one, which sees coincidences (and presumably all other events) as arising out of an unknowable matrix of cosmic heavings and settlings, with human individuals at best predetermined, script-abiding players in an archetypal drama.

The question arises as to whether the real issue behind the obscure concept of synchronicity is not either causality or the limits of psi but rather the nature of human needs and desires. It is this that has been pushed into the background ever since primitive men and women had to disavow their destructive impulses and developed their first projective scapegoats, beginning with mana and its equivalents and proceeding, in an ever-widening array of progressively autonomous spirits and demons, up to an increasingly abstract idea of causality from which every trace of the human will has been eliminated. The lopsided scientific world view to which this has led could make sense only to a computer--or to a dedicated adherent of the strange concept of synchronicity.

THE SYNCHRONICITY CONFUSION

Stephen E. Braude (University of Maryland)

Proponents of synchronistic explanations of events have elevated to the status of a theory an idea I believe to be deep nonsense. Roughly put, this is the idea that some events occur together, not due to any causal connection between them, or between the group of

events and some causal ancestor, but simply in virtue of similarities in the content or meaning of the events. When proponents of synchronicity describe an event as a meaningful coincidence, they do not really mean that the event was a chance occurrence that simply happened to occasion great surprise or some other intense feeling. Meaningful coincidences, from this point of view, are not chance occurrences at all. Nor are they supposed to be occurrences upon which we merely impose an interpretation or point of view that renders them meaningful for us. The meaningfulness of such coincidences is instead supposed to transcend any such limited human perspective. This meaning is supposed to exist objectively and independently of any human psyche. Meaningful coincidences are thus supposed to exemplify some sort of noncausal natural principle that links events in terms of their intrinsic meaningfulness.

Synchronistic explanations of apparently meaningful coincidences are supposed to have several crucial features. First, in specifying how events are meaningfully related, such explanations are supposed to be specifying relations or kinds of connections between events that are somehow intrinsic to their joint occurrence. Only in this way can proponents of synchronicity explain how meaningfulness between events is not arbitrary, context-relative, or imposed on the events by an observer, and thus how this meaningfulness is a function of fundamental operations of nature. Second, synchronistic explanations are supposed to explain why the events in question occurred as they did--that is, why the events happened at times and in a way that made them meaningful with respect to one another, rather than at times or in a way that would not make them jointly meaningful. Thus, in the famous incident of the exploding sounds in Freud's bookcase during a dispute between Jung and Freud on the subject of ESP, it must be explained why the sounds occurred when and where they did, and not, say, a year later when no one was in the room, or perhaps in a bookcase elsewhere in Vienna. And third, explanations of why the events in a meaningful coincidence form a meaningful group are not supposed to be causal explanations, and therefore not subject to the problems that have traditionally plagued standard mechanistic explanations in parapsychology. These last two demands, however, cannot both be satisfied, and the first demand is incoherent.

The reason the first demand makes no sense is that it requires that history have an intrinsic structure, independent of anyone's point of view. But there are indefinitely many ways to parse any slice of history into events or elements, and no one of these is intrinsically preferable to any other. What makes a parsing of history appropriate is some sort of perspective or point of view that we bring to our consideration of that portion of history. And since meaningful relations between events presuppose some parsing of history into events (the events meaningfully related), meaningful connections between events must also be perspective-relative or interest-relative.

Moreover, since points of view cannot exist independently of

an interpreter who (as it were) owns the point of view, it thus appears that explanations of the meaningfulness of certain event-clusters must be given in terms of some interpreter(s) of the events who presumably is (are) responsible for the events occurring as and when they did. But then synchronistic explanations of meaningful coincidences turn out to be a subset of the set of causal explanations, despite protestations to the contrary by Jung and his disciples. Such explanations are causal in the same way that an explanation of why a person but not a parrot or phonograph can insult me by producing the string "Braude, you're a buffoon" is a causal explanation. Granted, the theory of synchronicity demands that we do not explain why events occurred meaningfully by linking them to some common causal ancestor. But this demand cannot be met if we are to satisfy the requirement that we explain why the events occurred in a meaningful rather than nonmeaningful way (and that the explanation not be that the meaningfulness is fortuitous). And if we refuse to locate the cause of the coincidence within natural history, we must, it seems, posit some sort of causal (if not crudely anthropomorphic) cosmology. I conclude, then, that the theory of synchronicity is unintelligible and riddled with inconsistencies, and that people who think that an approach like Jung's has promise literally do not know what they are talking about.

There may still be some way of formulating a theory of meaningful coincidences. But so long as we must explain why the events occurred nonfortuitously in a meaningful rather than nonmeaningful way, our explanations will be causal explanations. In rejecting the theory of synchronicity, I am thus not rejecting the viability of some causal theory of meaningful coincidences.

Part 2: Roundtables

IN MEMORIAM: A TRIBUTE TO GARDNER MURPHY (1895-1979)*

GARDNER MURPHY

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

When Gardner Murphy died last March, many of us were saddened by our personal loss, but surely also every parapsychologist must have felt the loss to parapsychology. At 16, he had decided to dedicate himself to psychical research, and he held to this dedication even while he became outstanding in conventional psychology. Only William James was comparable in professional stature and the wisdom of his writings.

It is tempting to reminisce, with anecdotes to show his kindness and his humor, how helpful and modest and busy and brilliant he was, the qualities and combinations of qualities that made him unique. But instead it seems more appropriate here to sketch briefly three of his major and interlocking types of contribution to parapsychology.

One, of course, was his lecturing and writing, which conveyed (in his own words) the challenge of psychical research. His style was to raise a question so deep as to concern us all; to set forth diverse facts on it (from laboratory research in many sciences, from spontaneous experience, from history, anthropology and other fields) to interweave these with persuasive arguments for one approach--and then to show the frustrating gaps in that approach or those arguments. He presented problems in their full complexity, yet with the factual core that showed how it was possible to help resolve them. His critical, sophisticated surveys were slanted to stimulate productive efforts to go beyond what is known.

And he not only stimulated research, he implemented it. Seasoned workers and students just dipping their toes into the water benefited from his suggestions for methodological rigor and for procedures to help test their ideas, from relevant references they had not known of, from practical or statistical guidance. He gave warm encouragement, finding promise even beyond its originator in some approach that was hesitantly proposed; and he gave quiet discourage-

*Organized and chaired by William G. Roll, Psychical Research Foundation.

ment where appropriate. But more: again and again he succeeded in finding grants for their research (even large grants, to found laboratories) or facilities for conducting the work, or publication outlets, or job opportunities. When necessary, he would even take over the actual writing of some project. And he would praise lavishly whatever good work was done.

Part of his ability to be so helpful to others came from his own eminence. He wrote a history of psychology that became a standard reference; he made a reputation as a historian of psychology. He wrote so much on social psychology, including coauthoring a basic, classic text, that he became a leading social psychologist. He wrote a magnificent book on personality; he is an outstanding personality theorist. He was President of the American Psychological Association and received its Gold Medal Award, and he headed other prestigious psychological organizations, just as in our field he was President of the (London) Society for Psychical Research and for many years of the American Society for Psychical Research. Not only did his knowledge of psychology contribute to his wisdom in parapsychology, but surely also the respect in which psychologists held him has contributed to parapsychology's growing acceptance.

GARDNER MURPHY'S CONTRIBUTION TO PARAPSYCHOLOGY

Montague Ullman (American Society for Psychical Research)

Gardner Murphy has played a unique and significant role in the history of psychical research. In his person, he embodied the spirit of the old and the thrust of the new. On the one hand, he was the natural successor to such geniuses as F. W. H. Myers and William James, and, on the other, he served as an early-warning system, calling to the attention of young investigators those areas along the advancing edge of psychology that were apt to prove useful in psychical research. Possessed of a rich, expansive panoramic vision, tempered by a sound scientific and philosophic outlook (a combination so characteristic of some of the early giants in the history of psychical research), he brought to the modern era of parapsychology a concern with its own history and the richness of its heritage. At the same time, his humanistic outlook and his scientific mastery of the whole range of psychological sciences enabled him to pinpoint quite early those trends that were apt to bring psychical research and psychology closer together.

Among the important insights that Gardner Murphy called to our attention were the role played by motivation and need in both normal and extrasensory perception; the importance of feedback in the maintenance of normal bodily function and his very early suggestion that such mechanisms might have relevance for parapsychological research (he exerted a seminal influence in biofeedback research); the role of altered states of consciousness as favoring

psi effects; his orientation to psi as a field effect; and, finally, his regard for the untapped resources in every human being (much of his work was the point of departure for the Human Potential Movement).

What is quite amazing about Gardner Murphy was his ability to pursue successfully two full-time careers simultaneously. He not only fed into the emerging science of parapsychology all the psychological insights at his command, but he also defended in a courageous and outspoken way the legitimacy of parapsychological research to his colleagues. He struggled against closed-mindedness and closed systems wherever he encountered them. His early and continuing encounter with psychical research set his sights on a more extended frontier than orthodox science was willing to recognize. He took on opponents like Boring and Girden in his insistent demand for the recognition of psychic phenomena.

Any assessment of Gardner Murphy's contribution to parapsychology must include a recognition of his personal influence on a whole generation of researchers. His generous spirit responded to all who sought his advice and counsel. He encouraged an interest in psi among students who were unaware of its existence, and he fanned the interest of those who were already knowledgeable. His personality and accomplishments served as a role model for those who sought to include parapsychology in their career choice. In my own case, he was instrumental in the founding of the Dream Laboratory at the Maimonides Medical Center. I am grateful to him not only because of his enthusiasm and support for the work itself but also because of the material help he provided to equip the laboratory.

Those of us who knew Gardner Murphy have suffered a deep personal loss. We are somewhat comforted by the rich heritage of his writings. In these writings, he has addressed every major issue confronting parapsychology today.

GARDNER MURPHY AND THE ASTROLOGY PROBE

Stanley Krippner (Humanistic Psychology Institute)

I met Gardner Murphy in 1959, when he lectured at Northwestern University, where I was working on my doctoral dissertation. He accepted my invitation to meet with me and a few other graduate students who were interested in parapsychology. We were struck by his warmth and his keen appreciation of our work and ideas.

During the 1960s, when I was director of the Maimonides Dream Laboratory, Murphy was a frequent visitor. His visits were especially popular with our student assistants because of the personal interest he displayed toward each of them. Despite his influence on

students and their work, he never took credit for anything he inspired or guided. Apparently, he did not want a "Murphian" school of psychology to develop. He used contributions from various points of view, however, without robbing them of their vitality.

Murphy was even willing to give astrology a chance to demonstrate its validity. In 1972, he was approached by an astrologer who wanted to take part in a scientific evaluation of his craft. I obtained 16 volunteers who gave me their birth information. The astrologer constructed horoscopes of each subject. However, I had taken care to ensure that half the subjects were almost completely naive about astrology while the other half knew their "sun sign," "moon sign," and the characteristics of their astrological "houses."

We gave the members of this latter group eight personality descriptions written out by the astrologer (with any reference to specific "signs" or other astrological terminology deleted). We gave the members of the naive group the eight personality descriptions corresponding to them. Each group rated (on a 100-point scale) the similarity of each personality description to themselves. A statistician evaluated the results.

Neither group obtained significant data. One member of the naive group correctly identified his personality description (giving it the highest rating of the eight) but this is what one would expect from chance alone. Two members of the sophisticated group made correct identifications, and the others tended to give higher scores to the correct descriptions than did members of the naive group.

The average score given to their horoscopes by members of the naive group was 48; they gave the other horoscopes an average rating of 44. The sophisticated group did somewhat better, giving average ratings of 68 and 56. These results were not statistically significant, however. Further, those in the sophisticated group might have zeroed in on key words they knew to be associated with their signs (such as "headstrong" and "determined" in the case of Aries), accounting for the more favorable ratings given their own horoscopes. Murphy and I did not claim to have conducted a definitive study of astrology; however, our probe involved a design that would be useful in future investigations.

Murphy's openness to new ideas was coupled with an insistence on rigorous investigation. This combination is rare among scientists but indispensable if parapsychologists are to make important breakthroughs in the 1980s.

THE INTERPERSONAL FIELD

William G. Roll (Psychical Research Foundation)

Anyone who has attended recent PA conventions will have noticed a change in the way many parapsychologists look at psi.

We may still speak about people hitting or missing ESP and PK targets as if a psi test were some sort of shooting practice, but this is now only a language of convenience. An increasing number of psychical researchers look on psi occurrences not as special personal talents but rather as effects of processes occurring within structures that encompass the individual.

The shift of paradigm, as we now call such a change, can be traced back nearly 35 years to Gardner Murphy's (JASPR, 1945, pp. 181-209) theory of the interpersonal field. This concept showed psi phenomena in an entirely new light, revealing features of the landscape that we have only recently come to appreciate. For instance, by borrowing the concept of field from electromagnetic field theory, Murphy was one of the first to point to concepts originating in physics as aiding rather than contradicting theories for psi.

Murphy made three main points about the interpersonal field. 1) The field is more than the sum of its parts, 2) it can best be explored through altered states of consciousness and 3) survival after death, if it occurs, is a continuation of the interpersonal field rather than of individual existence.

1) Murphy observed that psi occurrences are typically concentrated within groups. Two or more persons "somehow, while remaining themselves, yet become a single, psychical entity." This theme, you recall, was explored by Michael Grosso at this Convention in his discussion of "psi-conducive groups." In psi experiments, the psychic structure includes the experimenter. The theory hereby accounts for the experimenter effect, and Murphy was one of the first to call attention to this type of psi interaction.

Perhaps his most important point, and one that has not yet penetrated the awareness of psi researchers, is that "complex organized wholes," such as interpersonal fields, "cannot be understood in terms of ingredient parts." Murphy developed this theme in his presidential address to the (British) Society for Psychical Research (PSPR), 1945-52, pp. 1-15): "... the world of interpersonal phenomena is a world which must be faced on its own terms... its laws made clear and recognized to be essentially different from those laws which apply to individuals." The possibility that the dispositions of a psi group are different from those of its members might help explain why it is so difficult for us to predict psi behavior only on the basis of our knowledge of the latter.

2) If psi is a group effect, we might expect that a weakening of the concept of individual selves by its members would lead to increased psi interactions between them. Psi conducive states are, therefore, those in which "one's awareness of self [is] pushed away from the center to the fringe of consciousness or almost totally extirpated." Murphy and Laura Dale (JASPR, 1943, pp. 2-15) had already then called attention to psi-conducive states, but it took nearly three decades before we explored such states systematically.

3) Murphy's ideas about a possible life after death combine the first two points. If there is survival, it will be a continuation of the interpersonal field, and it will be characterized by altered states of awareness, since the biological organisms with their competitive tendencies are no longer there.

If the interpersonal structure of which a person is part continues after the death of that person, what would count as evidence for this? According to Murphy, the most important type of evidence would consist of "post-mortem interaction of two or more communicators. It is specifically a field expression; not expression of a solitary individuality, but of an enduring relationship."

Murphy only hinted at the nature of this relationship. The most productive approach may be to explore it while the people are still living and then look for a continuation of it after one or more of the members of the group have died. Forming such groups within our research organizations would be a promising approach.

Just as Murphy did not recognize any sharp border between personality and its social environment, the interpersonal field has no clear-cut boundaries. Just as the field of a magnet interacts with the earth's magnetic field, interpersonal fields may join in a universal configuration. Nevertheless, it may be helpful to describe a limited or concentrated field both for the physical space surrounding the magnet and for the psychic space associated with a group of persons. Thus, Murphy can speak of a "psychical entity" composed of a few individuals (JASPR, 1945, p. 186) and of "the One" of Plotinus (JASPR, 1973, p. 199), which includes all such structures.

Murphy's concept of the interpersonal field is one of the first comprehensive pictures of human personality that relates personality to the wider psychic space of which it seems to be part. Recent studies of ESP interactions among the living, of ESP and altered states and of the evidence suggestive of survival suggest that Murphy's theory points to real features in the landscape he set out to describe. His map is of course only a rough approximation. We must now fill in the details.

IMPROVING COMMUNICATIONS BETWEEN PARAPSYCHOLOGISTS AND OTHER SCIENTISTS*

INTRODUCTORY REMARKS

Ian Stevenson (University of Virginia)

Although parapsychologists have made considerable progress in presenting the results of their research to colleagues in other branches of science, they have obviously much to do to enlarge the number of scientists outside the field who accept the validity of the data of their research. Parapsychologists still tend to publish their research reports in specialty journals read only by other parapsychologists. This means that other scientists are usually unfamiliar with the details of actual research in parapsychology; and they may even obtain misleading impressions about it through reading popular and sometimes sensationalized accounts of the research in news media. Other scientists' lack of sound information about parapsychological research probably contributes to the difficulty of funding research in parapsychology; and the low level of funding keeps down productivity in the field and, circularly, restricts the development of data that might influence other scientists to take more interest in the field.

Parapsychologists sometimes complain, not without some justification, that they have not always been treated fairly by reviewers of articles submitted to general scientific journals and by reviewers of research-grant applications. Some of the fault for this situation, however, may lie with parapsychologists themselves.

They need to improve their communications with other scientists. The present roundtable was organized to explore ways in which they can do this.

*Organized and chaired by Ian Stevenson, University of Virginia.

SOME PROBLEMS AND POSSIBILITIES

Irvin L. Child (Yale University)

The most important step toward improved communication with other scientists might be increased publication of high-quality parapsychological research in journals not devoted primarily to parapsychology. I am impressed here by my own experience. Until I became actively interested in parapsychology just a few years ago, parapsychological research articles published in the journals of the American Psychological Association, and in some other psychological journals, were almost the sole source of my interest in or knowledge of parapsychological research, for I had never seen a parapsychological journal. Such publication seems somewhat rarer in the last few years. Is it because those doing parapsychological research now have a sizable group of researchers who share their interest, and are less motivated to try to reach a wider scientific audience?

Especially desirable for publication in outside journals (and especially likely to be accepted by them, I hope) would be papers of high quality that, while conducted in such a way as to render non-psi explanations unlikely, are primarily oriented toward understanding the psi phenomena with which they are concerned--toward studying the way they depend upon experimental variables, for example. Reading these might persuade outsiders that parapsychology is, in the structure of its activities, similar to other sciences, rather than being bogged down in preliminary effort to establish the presence of a mystery.

Another possibility is increased representation of parapsychology in the conventions of the national associations in psychology, psychiatry, science and so on. But here we may already be doing well. The presentations I know of in recent years have, I think, been quite successful, and I don't know that increased volume would be useful. A small number of excellent programs specifically oriented toward outsiders may get much more attention than a larger number of programs of narrower appeal.

We might also improve potential communication with other scientists through articles published in the parapsychological journals. At least a few outside readers may see each new issue, and the bound volumes are accumulating in the libraries for future perusal by persons not yet interested.

What steps could be taken here? It seems to me some articles have unnecessarily insular aspects. Technical terms are often used without explanation of their meaning, and to understand what is done with them often requires more specific background than should be needed. Statistical terminology is sometimes outmoded or idiosyncratic (as in the continued use of the term CR, or critical ratio, for what elsewhere is more often these days called z; or in the peculiar way of distinguishing between z and t, based on the way

they happen to have been most often used in parapsychology rather than on the nature of the concepts). Constant awareness of addressing a larger potential audience might help the parapsychological author. Perhaps it would help, too, if the editors of parapsychology journals could at times obtain the cooperation of scientists quite outside parapsychology in reviewing manuscripts, or could interest them in contributing comments in the form of letters to the editor.

THE PERSPECTIVE OF A GOVERNMENT OFFICIAL

Richard T. Louttit (National Science Foundation)

The National Science Foundation does not receive many applications for the funding of research in parapsychology. Those we do receive are likely to come into the Division of Behavioral and Neural Sciences, of which I am the Director.

It is important to emphasize first that some of the difficulties that parapsychologists experience in obtaining funds are common to all branches of science at this time. The Division of Behavioral and Neural Sciences of the National Science Foundation can only support 20 percent of the proposals submitted to it each year, although at least two or three times as many other proposals submitted demonstrate good-quality science and would deserve support if we had the funds.

In order to win support from colleagues, parapsychologists, like other scientists competing for funds, must show that they have good ideas that are both new and important. Writers of proposals should always ask themselves if what they wish to do will add significantly to the understanding of a problem. If I may say so, I think that parapsychologists show some signs of preoccupation with refinements of statistical analyses of data that are not basically new, but instead rather repetitious of old data. One of the speakers yesterday, Rhea White, made the important point that your field needs new ideas and new ways of looking at old problems.

Again like other investigators, parapsychologists looking for funding must show in their proposals that they are competent to carry out the research they propose to do. Applicants for funds will be judged by such matters as: the clarity and simplicity of their hypotheses, their qualifications for analyzing the data they expect to obtain, their knowledge of what other investigators have already accomplished in the same area of investigation and their design of a project that is feasible within the time and budget proposed for it. Most proposals are much improved when the applicants obtain advice from colleagues concerning the proposals. It is particularly helpful to ask colleagues outside one's own field of research to read a proposal in order to test it for clarity and comprehensibility by a competent scientist of some other discipline.

I have the impression that many parapsychologists have not come from the behavioral sciences. They should understand, however, that behavioral scientists, such as psychologists, are likely to be reviewing proposals that parapsychologists submit to the National Science Foundation. They should keep this in mind when preparing proposals. They should avoid jargon of their speciality that may not be familiar to persons in other fields; they should show knowledge of the relevant scientific literature outside parapsychology and its relation to what they are proposing to do; and they should be prepared to adopt standards for measurement and control that have become accepted in behavioral science.

Parapsychologists understandably accept the reality of at least some of the phenomena they study. They should remember, however, that not everybody agrees with that. Other scientists reviewing a proposal in the field of parapsychology will expect the planned research to provide satisfactory evidence of the phenomena hypothesized, if it occurs during the project. Some parapsychologists appear to ignore the importance of demonstrability of the phenomena. It is part of the same problem that some of them are insufficiently aware of alternative (normal) explanations for the results of their investigations.

The main influence in making decisions about funding projects submitted to the National Science Foundation comes from outside reviewers to whom the proposals are sent for expert opinions. The staff at the Foundation makes the final decisions, to be sure, but they nearly always base these decisions on the outside reviewers' judgments. Scientists submitting a proposal may suggest to the Foundation the names of persons they believe qualified to evaluate their proposal. If parapsychologists believe their proposals are not being fairly reviewed, they should avail themselves of this feature of the review system. This does not mean that only the proposers' nominees will review their proposal. Obviously, it will be judged by other reviewers as well; but at least you can be sure that some persons whom you believe to be informed about your field of investigation and your qualifications for it will assist in evaluating the proposal.

PUBLICATION IN ESTABLISHED JOURNALS

Robert L. Morris (University of California, Irvine)

Parapsychologists may fail to publish in established journals for a variety of reasons, among them the likelihood that many journal editors do not select competent referees. This situation can be improved in two respects. First, effective guidelines for referee selection and their rationales can be developed and disseminated to major journal editors, via the PA. Second, the authors of parapsychological papers may be able to express themselves more clearly to

referees by emphasizing terms and concepts germane to the journal in question rather than those that have arisen in the relative isolation of institutionalized parapsychology.

CULTURAL ROOTS OF THE COMMUNICATION PROBLEM

Charles T. Tart (University of California, Davis, and SRI International)

Mainstream science is a massive, socially approved activity that is paradigmatic, in Kuhn's sense: within a basic conceptual framework, intellectually satisfying, generally unsurprising work is carried out that constitutes progress. Parapsychology, by contrast, is not only a very small-scale activity, it has neither a satisfactory conceptual paradigm for its phenomena per se (much less one that integrates psi phenomena with mainstream science), nor can it even demonstrate its phenomena on demand. Psi phenomena are replicated often enough to be statistically significant, but usually the magnitude of the effect is very small.

Mainstream science is generally uninterested in parapsychology. Parapsychology isn't needed: mainstream fields are making satisfactory progress with their current paradigms, and are kept busy by them. Further, psi phenomena seem contrary and nonsensical to these currently satisfactory paradigms, so parapsychology is easily ignored.

On a deeper level, quite unscientific and hostile criticism is frequently leveled against parapsychology, further cutting off communication. Why? I believe that the process of enculturation in our society, whereby each of us becomes a "normal" citizen, involves active, unconscious dynamics that repress psi functioning and create irrational defenses against parapsychology.

Scientists are members of their culture as well as scientists. The relationship between infant and parents is the cradle of repression of psi. Parents have been socialized to think of themselves as good and loving, caring only for the child's best interest, but because they are human beings, they often feel angry toward the demanding infant or child. Infants are probably born with a fair amount of innate psi contact with their parents. Infants must learn to accept and mirror their parents' world view (consensus consciousness) in order to survive and become "normal," so they are under great pressure to accept the loving image of their parents. Psi impressions undermine that image by revealing anger that is otherwise concealed. To repress the conflict that results, psi functioning is inhibited. We all grow up with this very deep inhibition against psi functioning. A common consequence for mature Western scientists is that they either ignore psi phenomena or attack the idea--thus they avoid dealing with deeply repressed material. Since scientists possess high verbal skills, rationalization of their attitude is easy.

Parapsychologists themselves show manifestations of this unconscious conflict in what I have called the "religion of the .05 level." This is ready acceptance of psi studies that show statistically significant results at the .05 level or so, which means statistical significance but practical triviality in most cases. Yet intense attacks are launched by parapsychologists on psi studies that show very strong manifestations of psi. Trivial manifestations of psi allow a kind of counterphobic control, but strong manifestations threaten to lift repressive defenses and activate the old conflict. Communications between parapsychologists and mainstream scientists are not likely to improve until these underlying dynamics are dealt with.

EXPLORING CLINICAL APPROACHES TO UNUSUAL EXPERIENCES*

PRACTICAL APPROACHES TO COPING WITH UNUSUAL EXPERIENCES

Stuart Blue Harary (Human Freedom Center)

Persons who have had experiences that seem to be of a "psychic" or unusual nature often undergo great stress in relation to those experiences. Experients often develop identity confusion and a deep sense of social alienation because the greater social milieu in which they have had these "psychic" experiences typically responds to them either as evidence of psychological pathology or of special "spiritual gifts." Many experients feel that they must make one of two choices: be open about the extent of their internal experiences and thereby be branded as social deviants, or deny their experiences and thereby be alienated from a significant aspect of their own lives.

Mental-health and research professionals frequently respond to clients' reports of unusual or "psychic" experiences by either considering them as symptoms of narcissistic or other personality disorders or as opportunities for immediate scientific experimentation with the client. Such responses disregard the possibility that these experiences may not, in themselves, be of a primarily pathological nature and may lead to client discomfort or dysfunction because of the nonsupportive and exploitative social atmosphere.

In an attempt to participate in what they hope will be a more supportive milieu where they may achieve a sense of identity and belonging and where they may obtain readily available interpretations of unusual experiences, many experients join nontraditional religious groups or cults, or they develop "followings" of their own. This response often increases the alienation of experients from the rest of society and may inhibit their personality development as well. Experients who choose this route may have further difficulties directly related to the dynamics of cult identification and involvement; these additional problems may compound the crises that may have led them to join or develop a group in the first place. The resulting societal response to experients as social deviants may help to create

*Organized and chaired by Stuart B. Harary, Human Freedom Center.

a cyclical pattern of societal rejection and deviant responses to this rejection by experients.

As parapsychologists and mental-health professionals, it is important for us to view these reported "psychic" experiences in the context of the experients' lives. Are the experients able to cope effectively with their everyday lives while having these experiences or are these experiences disruptive to their daily rhythms? Are they having problems that are directly related to coping with these unusual experiences, or are these "psychic" experiences in fact manifestations of other ongoing psychological problems? Rather than overemphasizing the unusual experiences themselves or immediately trying to determine their authenticity, it is important for us to focus upon helping clients achieve a balanced relationship between internal realities and the existing demands of social living. We can do this by providing them with an alternative to what they seek from cults: we can offer them an opportunity to learn how to cope with psychic experiences without alienating themselves from society or feeling the necessity literally to depart from the society at large. While working with these clients, we may ultimately learn a great deal from their experiences that may later be applied to scientific research.

THE DETECTION AND PREVENTION OF PSYCHIC EXPLOITATION OF RETIRED PERSONS

Robert L. Morris (University of California, Irvine)

Psychic phenomena present those who take them seriously with a potentially traumatic approach/avoidance conflict. Being psychic is highly attractive because it offers the possibility of greatly enhanced communication with all facets of one's environment. Being psychic is highly unattractive because we know very little about it, we have little control over it, our society has great ambivalence about it, it could be misused considerably and frauds can easily exploit our acceptance of it. Such ambivalence about psi undoubtedly contributes considerably to psychological conflicts both in the interested layperson and in the experimental participant, who succeeds psychically in Session A and now must confront Session B.

Parapsychology has addressed itself to such ambiguities primarily by trying to increase our knowledge about psychic functioning through scientific research, by and large postponing assessment of the social functioning of psi until we have more information. Psychic exploitation through fraud (pseudopsychic exploitation) is an area of increasing public concern, and it is appropriate for parapsychology to expand its activities to include providing the public with useful information in this area also.

Pseudopsychic exploitation of laypeople occurs in at least three major ways by individuals and organizations: 1) they claim to

have psychic powers that they will make available to the layperson at some cost, 2) they claim to be able to teach laypeople psychic powers at some cost and 3) they offer psychic powers as an enticement for the layperson to maintain affiliation with a religious or clandestine organization. The simulation and exaggeration of psychic talent is crucial to such exploitation schemes. The author proposes a project aimed at surveying the existing pseudopsychic exploitation of retired persons, including the special needs of such persons that facilitate exploitation. The results of such a survey could then be considered with respect to what is presently known about the technology of simulating and exaggerating psychic powers, such as to develop a set of guidelines for everyday-life detection and avoidance of psychic-exploitation schemes, tailored to retired persons but generalizable to the entire public. If parapsychology is to integrate with mainstream America, surely it must assume a role of leadership in the reduction of psychic exploitation. Otherwise, it will continue to be identified with such exploitation and, in fact, be an unwitting contributor to it.

WHERE ARE THE PARAPSYCHOLOGISTS?

David Price Rogers (University of North Carolina, Chapel Hill, and John Umstead Hospital, Butner)

As most parapsychologists are aware, psychic experiences did not begin in a laboratory. They occurred in real life. They were experienced in ways that people related to one another, in their attempts to help one another, and in their search to understand things that seemed beyond their own powers of comprehension. With the advent of scientific method, scientists withdrew to laboratories to study particular chunks of the world. As they matured and felt that they had some "facts," members from the cloisters began cautiously to re-emerge into the sunlight of daily living and tell people what they had discovered. They provided notions that helped elucidate other fields of inquiry, and they were able to offer suggestions on how to help people in their daily lives.

It is a sign of security and maturity, then, for a science to feel that it has something pragmatic to offer. What is the place of parapsychology? Is it time for experimental parapsychology to come out more openly from its laboratories? Do we have anything to offer? Should we wait any longer?

When I began as an experimental parapsychologist, I clearly felt that we had little to offer to the outside world. Parapsychology was a fragile child belonging in the lab and it was dangerous to carry it outside those doors. While I was a researcher, I felt an unusually strong sense of belonging to the parapsychological community. When I became a practicing clinical psychologist and a university teacher and eventually stopped publishing research, I began

to feel left out--as if I no longer belonged. However, in overcoming this feeling, I began to realize that I and many others were using our parapsychological knowledge all the time but that we just didn't belong to the research club. I taught parapsychology, consulted with individuals who were having emotional difficulties about "psychic disturbances" and was continually involved in psychotherapy where the paranormal wove its way in and out. I realized that I was as much of a parapsychologist as ever.

So I would like to suggest that the scientific researchers of parapsychology might begin to look outside less defensively. The cloistered activities of scientists in the lab need not be respected more than the activities of persons who carry the findings, in a variety of ways, out into the world. There is a distinct need for scientific parapsychologists who choose to teach, help others and integrate their knowledge with other areas of inquiry. In many ways, this has been going on for a long time, but with little support and guidance from the experimental parapsychological community.

However, experimental parapsychologists must be aware that, in many ways, they are hothouse plants who are only used to discussing psi as it appears in certain restricted conditions. Too often, strict scientists look down on the concerns of "the everyday world." On the other hand, people in all fields of study and human service continually encounter and deal with, willingly and unwillingly, psi and pseudo-psi occurrences. It is here that the experimental parapsychologist can and, in my opinion, ought to provide a needed service by sharing research findings and insights. With the guidelines of candor and restraint, important bridges can be built.

LIFE HISTORY AND PSI EVENTS: SIGNIFICANCE FOR COUNSELING

Montague Ullman (American Society for Psychical Research)

Those who work in the general field of psychical research encounter people from time to time who do not appear to be psychotic and who claim to have had psychic experiences that occur sporadically throughout the course of their lives. It has been possible for me to follow a number of such individuals over a period of several years and to gain some firsthand impressions of their way of life, the circumstances under which psi events occur, their significance to the individual and the variety of encounters they have had with professionals in the course of their efforts to assess and understand these experiences.

These "psychic recidivists" encompass a variety of personality types ranging from those with borderline structures to those who would fall within the categories of hysterical or obsessional variants of normal character structure. Those who have been upset or disturbed by their psychic experiences and who have sought pro-

fessional psychiatric help have often encountered frustrating responses. What they have reported honestly and out of conviction is rarely accepted as such. More often it is either discounted or worse yet, labeled as a psychotic symptom. What the therapist fails to realize is that the law of parsimony does not prevail here. A person may have a lifelong history of severe mental disorder and, at the time, experience some bona fide psi effects from time to time. In fact, there are times when the two seem intimately connected. Patients often turn away from the treatment they need because of the negative, skeptical or even hostile response of the therapist. Conversely, patients who don't need treatment may be misled into therapy because of the therapist's ignorance about the reality of psi.

When our advice is sought in a counseling or treatment situation, our prior knowledge of the various ways psi experiences can become part of the life history of the client can help us give the client not only the reassurance, validation and support he or she might need but also orient us to any patterning that may be present, the nature of the circumstances predisposing to their occurrence and be of help in evaluating the coping mechanisms that have been resorted to in the past.

Since so little is known about psi profiles at this time, every encounter with a psi-disposed individual is a potential case-study research endeavor. We have much to learn about the relationship of psi to life stresses, crises, illnesses, accidents, fluctuations in mood, the impact upon the life of the person, the coping mechanisms evoked, changes with age, ways of distinguishing between the veridical and the spurious and the nature of the relationships involved in the psi communication. Every client becomes in a sense a co-investigator in the project. This counteracts earlier adverse experiences with therapists and transforms attitudes of fear, dread, puzzlement and confusion into that of interest, knowledgeability and realistic expectation of what the future might hold.

PSI COUNSELING: THE SITUATION AT RESEARCH LABORATORIES

Debra H. Weiner (Institute for Parapsychology, FRNM)

Anyone associated with the field of parapsychology is confronted by persons seeking advice about ostensible psi experiences. At the Institute for Parapsychology, we receive per week about 10 letters, telephone calls and visitors specifically concerning spontaneous psi. Information from some other parapsychological organizations in the United States shows variation in this figure, though few exceed this average. Although there may be duplication as persons contact more than one group, it is more likely that this number underestimates the need within the general population, since many persons 1) may not interpret an experience as being parapsychological in nature, or 2) may not know how to contact the para-

psychological community. Thus there seems to be a moderate but continuous demand for help, and it is within our responsibility as parapsychologists trying to "normalize the paranormal" to provide a public service in this matter.

At present, counseling at research laboratories tends to be on an informal basis. The most common approach is to provide the contactees with tools for evaluating their own experiences by discussing with them possible alternative explanations, other spontaneous cases and the research literature, sending them self-testing materials and so on. This approach is effective for certain categories of questions (e. g., expert opinion regarding the paranormality of a particular experience, a request for the testing or "certification" of psi ability, advice on psi development) and when the contactee appears psychologically stable. This "on the spot" counseling can alleviate momentary anxiety, such as the fear of psi or concern about being unusual. However, in reviewing a log of our calls for a three-month period, I found that for at least half the callers asking about psi experiences, there was evidence of some sort of past or present psychological disturbance. Not surprisingly, many of these persons described major complaints of multiple or extended experiences. These calls and contacts like them constitute a great source of difficulty for the research parapsychologist who, untrained in psychotherapy, cannot be sure whether the disturbance is the cause or the consequence of the reported psi experience or, indeed, whether they are related at all. Much time and energy is spent in frustrating encounters that do little to help the person solve long-standing problems.

In such cases, most parapsychologists suggest that the contactee seek professional help. Unfortunately, therapists knowledgeable about spontaneous psi are few and far between, and it is often impossible to refer the contactee to a specific therapist living in his or her area. Referrals are also made difficult by the problem of assessing a therapist's parapsychological expertise if he or she is not well known in the field, though expertise for this purpose might be obtained by completion of one of the intensive courses now being offered by a number of parapsychological research laboratories in the United States and Europe, or of special workshops on psi counseling by qualified therapists. In addition, consultation services by parapsychologists for therapists should be publicized in the appropriate places.

ETHICAL AND SOCIAL ISSUES IN PARAPSYCHOLOGY*

COMMERCIALIZATION OF PSI

Arthur Hastings (John F. Kennedy University)

Currently, there are many courses, classes, training programs and techniques purporting to train persons in psi abilities--e. g., Silva Mind Control, est and Rosicrucians. Some of these are serious in their claims and have been tested for results by parapsychologists. To date, these evaluations have not provided significant results, and claims for these programs rest on anecdotal accounts.

Some problems resulting from such programs may be these: 1) facile claims for easy psi learning obscure the difficulties of testing, investigating and understanding psi; 2) the claims create public expectations that orthodox psi researchers find it hard to fulfill; 3) to the extent that the training does produce psi, these abilities may occur in the service of ego and personality needs or motives that are negative or destructive; 4) to the extent that the training does not produce psi, but the illusion of psi, the trainees are being deceived and misperceive their reality status--what they can and cannot do; and 5) since methods of discrimination and validation are apparently not taught, there is the potential of serious misperception of interpersonal reactions, which could involve projection, paranoia, feelings of omnipotence and mental-health problems.

Some positive consequences of these programs may be these: 1) they increase public interest in psi and the willingness to investigate its potentials; 2) inasmuch as the techniques are reported to produce psi, they deserve investigation by serious researchers; 3) the graduates of such programs can provide a pool of subjects for parapsychology research; 4) because many of the courses are focused on practical use of psi in everyday life, they may stimulate concern for the social impact of potential psychic abilities; and 5) the interest in these programs may motivate psi researchers to explore training and teaching techniques for enhancing psi under conditions of intrinsic motivation.

---

*Organized by Gerald Solfvin, Psychical Research Foundation, and chaired by Ralph Locke, University of North Carolina and Psychical Research Foundation.

ENCOURAGING AND CONTAINING GOVERNMENT INVOLVEMENT IN PSI RESEARCH

Barbara Honegger (Washington Research Center)

A state-of-the-art assessment of U. S. Government involvement in parapsychology research at the state and federal levels suggests a growing interest amongst individual elected and appointed officials, increased interest in and funding of related research under the rubrics of "holistic health" and "personal transformation," but comparatively little formal interest or involvement in parapsychology per se. Representative Charles Rose's recently publicized support for the field, however, may rapidly change this assessment. Chairman of the evaluation subcommittee of the House Permanent Committee on Intelligence, Rose is attempting to call a congressional hearing to investigate claims that the CIA and the Pentagon have already demonstrated the validity of psi and its applicability to intelligence operations. The results of Tart's recent survey of full PA members regarding negative uses of, government interest in and funding of psi research, however, render significant clandestine investigation or use of psi unlikely at this time.

The most significant contribution to government ambivalence with regard to psi results from unreasonable fears of bureaucrats who perceive decisions to fund research in controversial and uncertain areas as a serious risk to employment or advancement. Unconscious fears that may also play a role include fears: 1) of interference with the Constitutional division of Church and State, 2) of interference with the concept of individual free will upon which our political system is based and 3) of radical changes in ideas about personal responsibility, which may lead to equally radical changes in our judicial system. To overcome such apprehensions, the PA might consider funding part-time lobbying at the federal level.

Above all, parapsychologists should emphasize the dangers of any law outlawing abusive psi-mediated actions before psi is causally understood, without which assignment of personal legal responsibility cannot be rational or meaningful. Free public flow of information about all psi-related research and its results should be actively encouraged by the Parapsychological Association.

PSI-RELATED HEALING: CRITICAL QUESTIONS AND TENTATIVE ANSWERS

Stanley Krippner (Humanistic Psychology Institute)

Psi-related healing is one of the most discussed and least researched topics in parapsychology. This situation creates practical problems for those parapsychologists who are asked to give their

opinion of the practice or to recommend specific "healers." Only a handful of prominent "healers" have seen the inside of a research laboratory. In those cases where statistically significant results have been obtained, there are interpretive problems. If a "healer" can hasten the repair of an animal's wound, does this mean that working with humans will be equally effective? If a "healer's" flask of water is associated with a higher percentage of germinated seeds than a control flask, does this mean he or she will be able to assist a patient to recover from a disease? Further, is the effect due to PK, exerting direct action on a living system? Or is it due to ESP, representing a telepathic suggestion for the indisposed individual to regain one's health?

In view of these problems, I have adopted a standard procedure when queried about this topic. I note that there are some laboratory data that suggest that the phenomenon of psi-related healing may be real. If so, however, we do not know if it involves ESP, PK or both. I also point out that sleight of hand is typically involved in dramatic instances of so-called "psychic healing," and that the list of "healers" who have participated in controlled experiments--or even controlled observations--is a short one.

When asked to recommend a specific "healer," I respond that I would prefer to suggest some alternatives. Among the choices I present are a number of "healers" who have attended the workshops in which Lawrence LeShan (a member of the Parapsychological Association) attempts to "train" individuals to maximize their "healing" potential. I also include names of other "healers" known personally to me--individuals who are sincere, whose services are donated (or kept at a minimal cost) and whose clients (or "healees") have given me positive feedback. I might underscore the importance of selecting a "healer" whose belief system matches that of the prospective "healee" because this might facilitate rapport. In addition, I feel that "healers" who appear to be more interested in power, publicity and financial gain should be avoided, especially if they are the central figure in a "cult" or aggrandizing organization. Finally, I warn the person making a request that a "healer" can be used as an adjunct to a physician but not as a substitute.

Science is beginning to understand the role that brain endorphins play in the placebo effect. It is discovering that emotions and attitudes influence the body's immune system, that people can often exert control over their autonomic nervous systems and that a caring, compassionate "healer" can sometimes activitate those self-healing mechanisms. I suspect that even the most powerful "healer" consciously or unconsciously depends more upon the "healee's" ability to heal him- or herself than upon psi. Even so, the careful utilization of a "healer" can do no harm and may possibly be of benefit to the highly motivated "healee."

ASCERTAINING AND INFLUENCING PUBLIC OPINION ON THE POSSIBILITIES AND RESPONSIBILITIES OF PSI IN EVERYDAY LIFE

Lendell Braud Williams (Psychophysical Research Laboratories, Princeton Forrestal Center)

Considerable research must be completed before we understand the nature of psi. However, I do not believe that we can wait for the completion of this goal before we, as PA members and as an association, begin to clarify our position on ethical and social issues in parapsychology. No other branch of science is as qualified to make statements concerning the pros, cons and implications of psychic phenomena as the PA.

We are a small group dealing daily with issues that are of tremendous potential importance to all of humankind at the level of everyday life. Some of the issues at stake are psychic healing, military uses, popular courses in psychic development, spontaneous psi experiences, uses and misuses of "psychic powers" and allocation of public monies for parapsychological research. We need to determine public opinion on these issues. We need to ask people from all walks of life (socioeconomic class, race, religion and so on) for their opinions and "gut-level" reactions to these issues. Such information would be valuable to individual parapsychologists as well as to government agencies and the PA for planning public-education activities. It is time we get laypeople, as well as our fellow scientists, straight on what parapsychologists study and what we do not study. We are the only ones who can correct the misconceptions, and we should use the popular press to do so if necessary. We should be prepared to respond to the public with meaningful answers to its questions. If we could provide information with which the general public can get a clearer, more realistic picture of psi in everyday life, we would be providing a valuable service that will go a long way towards improving our public image.

As PA members, we are in an unusual position in the scientific community. Our research bears on the nature of humankind and is related to its spiritual and consciousness aspects. If unity is a reality, then the moral implications that arise from this concept are immense. We need to stress the positive aspects of psi and state how these are related to such concepts as unity, "psychic energy," and the like. If we truly are "all one," then we can only hurt ourselves by hurting others. Many other social issues, such as pollution, can be seen in this light as well as the interdependence framework. We can make people aware of the negative aspects of going against this unity law but also the positive effects of maximizing it. I agree with Roll, Tart and others who have expressed the desire to emphasize these aspects and their positive forms.

PSI AND COGNITION*

THE EFFECTS OF COMPLEX TASKS ON GESP ABILITY IN YOUNG CHILDREN

Ernesto Spinelli (Hampshire, England)

It was hypothesized that the introduction of an unrelated cognitive task to be carried out simultaneously with a GESP task would impede the subject's ability to employ GESP successfully. As a corollary to this, it was hypothesized that the more complex the cognitive task being presented, the greater would be the GESP impedence.

Sixty subjects (30 males, 30 females) aged between 5.0 and 6.1 years and within a similar IQ range (98 to 102) were tested employing a telepathy paradigm. They were divided into three groups for experimentation purposes. Group M (consisting of 10 dyads) was the Control Group and subjects were tested only for GESP ability using a suitable telepathy-testing technique. Group N (consisting of 10 dyads) was the Simple Task Group. In this case, the sender in each dyad was given a simple paired-associates task at the same time as he or she was presented with the prerandomized target. Group P (consisting of 10 dyads) was the Complex Task Group. Once again, the sender in each dyad was presented with a paired-associate at the same time as being presented with the target. The Group P paired-associates were more difficult than those presented to Group N.

The results obtained showed that whereas Group M obtained highly significant GESP scores, Group N obtained only marginally significant GESP scores, and Group P obtained nonsignificant scores. Most subjects in Group N obtained perfect paired-associate task scores, but in Group P it was noted that individual success in one task (either GESP or paired-associate) tended to correlate with failure on the other.

*Chaired by Arthur Hastings, John F. Kennedy University.

PSI WITHIN A TEST OF MEMORY: A PARTIAL REPLICATION

Debra H. Weiner † and JoMarie Haight (Institute for Parapsychology, FRNM)

Naum Kreiman reported the results of a novel test of the relationship between memory and precognition. The data supported his hypothesis that when a subject freely recalled words from a specific list (from which certain words would be designated as targets) he or she would tend to psi-miss in the early part of the recall, presumably when memory is strong, and psi-hit in the later part, when "searching his mind" for additional words. He tested this by comparing the number of hits in the first and second halves of the lists of recalled words. He also conducted other analyses that speak to a psi-memory relationship. This brief is an attempted replication of his work.

Series 1. Series 1 was carried out with two classes (n = 19, n = 12) of an adult parapsychology course. It was planned to pool the data of the two groups in order to obtain a moderate sample size and to explore, on a secondary basis, trends within each class. The procedure was identical for both groups.

Just before the test was administered to the class, J. M. H. gave a preliminary talk designed to accustom the subjects to the idea that psi could be used, consciously or unconsciously, in everyday life. Subjects were then given identical copies of a list of 51 words arranged in an arbitrary order. Unlike the pseudorandomly obtained words of Kreiman's list, these were selected from available norms to be high in frequency, moderate in meaningfulness, but relatively low in imagery, in order to make the words similar to one another in memorizability.

Subjects studied the list for five minutes and had 15 minutes to write down as many words as they could recall in any order. Although they knew that 20 words would be selected for them as precognition targets, they were not aware of the experimental hypothesis until debriefed after the recall period. Subjects were given their scores about two weeks after the test.

Target sets were created for each subject with the use of a random number generator, and were assigned in alphabetical order according to subjects' surnames. Before the recall sheets were scored, words written more than once and "false memory words (FMWs)," words that did not conform (other than because of spelling errors) to those on the memory list, were eliminated. Scoring was done jointly by the authors and was independently double-checked by

† Dagger denotes speaker.

assistants. The number of hits in each half was determined; a hit on the middle word of the list contributed half a hit to each half.

As a measure of ESP, the ratio of each subject's hits over total number of correctly remembered words (i. e., trials) was transformed into a Z-score to compensate for varying numbers of trials. The combined scores were not significant, though overall missing was found in the first class ($t = -2.53$, 18 df, $p \leq .03$, two-tailed). The main hypothesis, tested by a Wilcoxon T test between the numbers of hits in the first and second halves of subjects' lists, was not confirmed.

Kreiman had carried out a post hoc investigation of the psi-memory relationship by calculating Spearman's correlations of precognition success (percentage of hits in the second half of subjects' lists) with 1) the number of correctly remembered words and with 2) the ratio of FMWs over the number of correctly remembered words. (He interpreted this ratio to mean that writing many false memory words shows poor control over cognitive functioning; thus a high "false memory ratio" indicates a less efficient memory.) In his study the first correlation was significantly negative and the second significantly positive. These correlations, which are not statistically independent of each other, support his contention that good memory is associated with poor psi ability. In Series 1 neither correlation was significant.

Series 2. A second series was carried out following a lecture given by J. M. H. at a community college. The lecture ended with the same comments as had been given in Series 1. Students were invited to participate in the experiment; 31 subjects volunteered.

This group showed hitting ($t = 1.86$, 30 df, $p =$ n. s.). The comparison of hits in the two halves of the recall lists was not significant ($Z = .52$). There was a significant relationship between overall psi and the number of correctly remembered words ($r_S = -.48$, 29 df, $p < .01$, two-tailed), which is in the same direction as Kreiman's result. The correlation of the psi scores to the false memory ratio was also in the same direction as the previous study ($r_S = .33$, 29 df) and would have been significant had a one-tailed test been planned. Further analyses, however, showed that the results of the second correlation in both Kreiman's and our data were due to its statistical relationship to the first correlation.

This study has failed to confirm Kreiman's primary hypothesis but has shown support for his negative psi-memory relationship. Unfortunately, the present task does not allow for a clear separation between primary and secondary memory processes, which are thought to play a role in the direction of psi-memory relationships. The task is unusual in that it involves the use of psi within memory; i. e., that the presence of target material can affect the process of word-selection in recall. A review of experiments that employed this paradigm yielded no consistent trends regarding the direction of the psi-memory relationship in such a situation, although there is evidence that psi can operate this way.

DOES ESP INFLUENCE THE RECALL OF PARTIALLY LEARNED WORDS?

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

Kreiman (1978) suggests that when subjects try to learn material and then recall it, an ESP response to that same material depends on how well it was learned. For the part that was well learned, subjects will depend on memory and ESP is not facilitated. However, when material is difficult to recall subjects are more open to ESP as an aid in recall and are more motivated to use ESP. Thus for material that is difficult to recall, recall is facilitated by ESP.

Kreiman designed an ingenious experiment to test these ideas. He presented his subjects with a list of 50 words to learn in five minutes. Subjects were instructed to reread the list repeatedly, then (for 15 minutes) to write the words they recalled, in the order of recall. Subjects knew that each would have a separate ESP target list, composed of some words from the learning list. They hoped for both high ESP and high recall scores.

Kreiman stated three hypotheses: 1) psi-missing for words readily recalled (this seems not to follow from his theory, which could predict null ESP results rather than psi-missing for well-learned material); 2) psi-hitting for words difficult to recall; and 3) a significant difference in ESP score between words easy and hard to recall.

He took the first half of each subject's recall list as the words recalled easily, and the second half as the words recalled with difficulty. Results showed significant psi-missing on the first half, significant psi-hitting on the second half, and a highly significant difference between halves. All hypotheses were confirmed.

Two parts of his method need special mention: subject population and target selection. Subjects were the 16 students in his parapsychology course. He devoted the two class periods preceding the experiment to precognition, and arranged his research as a precognition experiment. His class had a high level of enthusiastic interest in the test and perhaps also were gifted.

For target selection, Kreiman used a randomizing device. This opens the possibility that his own (clairvoyant) PK might have influenced the correspondence between targets and recall lists.

In an initial pilot study, I replicated Kreiman's method as closely as possible for 42 subjects in three college classes and obtained null results. The fact that target lists did not exist during recall made the ESP testing seem remote to these subjects, and I decided on a clairvoyance-type procedure for a follow-up test.

Three further procedural decisions were made. One was to use a supplementary questionnaire to try to select out the subjects who were uninterested in ESP or ambivalent about it. The remaining subjects would be the primary group to test Kreiman's hypotheses. The second was that, since Kreiman's data came from 16 subjects, testing would continue until 16 subjects were in the primary group. The third was that quartiles as well as halves of the recall list would be evaluated. This followed my observing that most subjects wrote steadily at first, but added very few words after the initial spate. In addition, two exploratory questions about "luck" were included in the questionnaire.

There were 32 subjects, tested individually or in small groups. A coexperimenter, Florence Ginsburg, tested eight in the primary group and eight others; I tested the rest. (Since there seemed no difference in scores between her subjects and mine, all subjects were pooled.)

Kreiman's method for obtaining a learning list of 50 familiar words was closely followed. ESP targets were selected according to a random number table, then wrapped in aluminum foil, inserted into small envelopes, and the envelopes sealed and shuffled.

The questionnaire asked two questions about luck, one question for a choice among goat, sheep and supersheep attitudes; one about whether they cared about making a high ESP score here; and one to tap fear, dislike or positive affect about ESP.

After only brief introductory comments about ESP, the experimental procedure was described. Subjects were told the learning period would be so short that they could not memorize the entire list, and were asked to read through the whole list as often as they reasonably could. After five minutes, the learning list was taken away and they were given the ESP target envelope, the recall blank and the questionnaire. They were told to write the words they remembered in a single column, in any order. After 15 minutes, the recall period ended.

To determine whether other subjects should be pooled with the primary ones for further analysis, intergroup comparisons were made. Responses to the questionnaire items about motivation and affect did not differentiate among sheep, and all sheep were therefore pooled. The five goats, however, showed significantly lower ESP scores than the 27 sheep for the second half of their recall lists ($p < .01$). Goats were therefore segregated.

The sheep had somewhat higher ESP deviations on the fourth quartile of their recall lists than on the other quartiles, but their fourth-quartile means were insignificantly better than chance ($t = 1.54$, 26 df) and insignificantly higher than their mean scores on the first three quartiles ($t = 1.08$, 26 df).

For the question that asked if in general they were lucky, 20

sheep answered affirmatively. Although their ESP scores on the first three quartiles of their recall lists were near chance, scores on the fourth quartile were suggestively high ($t = 2.00 > 19df$, $p < .10$). (The second question about luck may be disregarded here, since many subjects found it unclear. However, the 10 subjects who answered both questions affirmatively had suggestively high fourth-quartile ESP scores ($t = 2.16$, 9 df, $p < .10$).)

These results give no support to the hypothesis that words that are readily recalled will show psi-missing.

The hypothesis that hard-to-recall words show ESP facilitation was not directly confirmed but (if restated) is supported by two exploratory measures. The first is a comparison of sheep and goats. Although their ESP scores were similar for the first half of their recall lists, goats scored significantly lower than sheep on the latter half. This is consistent with the thesis that ESP influenced recall of words hard to remember so as to produce psi-missing for goats and some psi-hitting for sheep. It is better described as ESP influence or ESP effect than as ESP facilitation.

Some indirect support also comes from the suggestively high scores of sheep who considered themselves lucky, on those fourth quartiles, in spite of their near-chance scores earlier. Whether answers to the questions about luck reflect an acquiescent attitude, or a happily optimistic mood, or an actual past record of good luck, the fact that scores were suggestively high only for the words recalled last, and thus presumably recalled with most difficulty, tends to support Kreiman's hypothesis.

My tentative explanation for the difference between these data and Kreiman's is that his subjects were highly and appropriately motivated, while mine seemed to care more about recall than about ESP scoring, and also to feel embarrassed at putting incorrect words on their recall lists.

If the experiment is replicated, there should, I think, be three changes in method: 1) subjects should be strongly encouraged to write, during recall, any word that they think might possibly have been on the learning list, i. e., to include words where they feel uncertain; 2) questionnaire items to separate groups of subjects should be improved; and 3) words that are readily recalled should be distinguished more accurately from words that are recalled with difficulty.

Two easy techniques can provide this last desideratum. In individual administration, the experimenter should note when the subject pauses. Words before the pause should be considered readily remembered. In group administration, subjects should draw a horizontal line on the recall list whenever they have to stop to think about what another word could be.

In sum, the data give enough support to the hypothesis that

ESP influences recall of partially learned material to justify further investigation.

THE INFLUENCE OF IMAGERY AND FEEDBACK ON PK EFFECTS

Ariel Levi (Yale University)

Most previous research in PK has not explicitly dealt with the strategies by which observers attempt mentally to influence physical systems. Morris, Nanko and Phillips (RIP 1978, pp. 146-50), however, found psychokinetic influence to be a function of the type of visualization strategy used by the subject. Morris et al., using a binary random generating device as the PK target, found that subjects instructed to use a "goal-oriented" imagery strategy (vividly visualizing the desired event) had a hit rate significantly greater than subjects instructed to use a "process-oriented" strategy (visualizing "energy" flowing from their body to the device so as to produce the desired event). The experiment reported here represents an independent replication of the results of Morris et al.

The primary purpose of the experiment was to test hypotheses about the influence of imagery on subjective probability of success in chance situations. Since a Schmidt machine (RIP 1973, pp. 46-48) was used as the chance device, it was possible to determine whether actual scoring differed across groups using different types of imagery. The output of the Schmidt random number generator (RNG), a number from one on up, on a three-column digital display is determined by a rapidly reversing electrical current and the radioactive decay of strontium 90 atoms as picked up by a Geiger counter inside the machine. The RNG was set so that the theoretical mean chance expectation of the output was 16.

Subjects were 51 Yale University psychology students who signed up for an experiment on "Chance Events." Subjects were randomly assigned to one of three imagery conditions, and were run individually. All subjects were given background material that defined Jung's concept of synchronicity and stated the purpose of the experiment as "determining the conditions under which synchronistic sequences of hits occur in a chance situation." A simple description and schematic diagram of how the RNG worked were included in the instructions. The experimenter explained that the object of the experimental task was to get as many "hits" (numbers greater than the mean of 16) as possible out of 24 trials on the RNG. The experimenter demonstrated how to operate the machine and gave instructions concerning imagery. One group of subjects was told simply to imagine high numbers before each trial (Outcome Group). A second group was told to imagine how the internal workings of the machine could lead to high numbers (Causal Group). A third group listened to a tape containing information about chance and chance events, to prevent spontaneous imagery about the outcomes (Control Group).

Each subject attempted, for 24 trials, to get as many high numbers as possible. For 12 of these trials, the digital display was visible to the subject, enabling immediate feedback of results; for the other 12 trials, the machine was shifted to prevent feedback. The order of the feedback and no-feedback trial blocks was counterbalanced.

A three (Imagery Group) by two (Feedback) analysis of variance of the mean scores indicated no main effect for either imagery or feedback, but a highly significant interaction between these two factors, $F = 8.13$, 2 and 48 df, $p < .001$. Subjects in the Outcome Group scored higher with feedback than without (mean scores 18.79 and 13.90, respectively), while subjects in the Causal Group showed the reverse pattern (14.75 and 17.43). Subjects in the Control Group were apparently not affected by feedback (mean scores 16.09 and 16.53). Post hoc analysis showed that the mean difference between the feedback and no-feedback trials was significant for the Outcome Group ($p < .02$) but not for the Causal Group. Considering just the feedback trials, there was a significant difference between the mean scores of the Outcome and Causal Groups, $t = 4.73$, 32 df, $p < .001$, two-tailed. Neither of these groups differed significantly from the Control Group. Considering just the no-feedback trials, there was a significant difference between the mean scores of the Outcome and the Causal Groups, $t = 2.63$, 32 df, $p < .02$, two-tailed.

Each of the six cell means was tested for significance against the mean chance expectation of 16. Two of these means--Outcome feedback (18.79) and Outcome no-feedback (13.90) were significantly and independently different from 16, $Z = 2.05$, $p < .04$ and $Z = -2.39$, $p < .02$, respectively.

The results support the conclusion of Morris et al. that visualization strategies can affect PK influence. A goal-oriented strategy appeared to result in greater PK influence on a random electrical system than did a process-oriented strategy. This result held only when feedback was provided; without feedback the process-oriented imagery tended to become more effective than the goal-oriented imagery.

AUTONOMIC "RECOGNITION" OF ESP TARGETS

Gary D. Davis † and William Braud (Mind Science Foundation)

In the present study, skin-conductance changes of subjects were recorded while they viewed a series of relatively neutral target pictures, one of which was the actual target being continuously viewed by a remote agent. It was assumed that when the subject viewed the actual target it would be more arousing or meaningful than the other stimuli, thereby eliciting a galvanic skin response

(GSR) of greater magnitude than those elicited by the other, control, stimuli. Thus, targets were used as cues to elicit autonomic recognition of psi information that may have already "entered" the percipient's system.

This approach is congruent with the idea that the detection of psi information and the detection of subliminally presented information operate in an analogous fashion. It has been demonstrated that skin-resistance fluctuations can accurately respond to stimuli presented at intensities too low to elicit awareness. This suggests that the autonomic nervous system (ANS) may be a more sensitive indicator than is a percipient's verbal response.

Experiment 1. It was decided that 15 volunteers would be used in the pilot study. These were all volunteers who satisfied a criterion of producing a measurable response to at least 10 of the 25 stimuli presented to them; persons not meeting this criterion were replaced.

The purpose and procedures of the experiment were discussed with the volunteer during the initial visit to the lab. This usually required approximately 30 minutes, during which time G. D. attempted to establish a relaxed rapport with the participant.

Participants were told that the experiment was designed to test the idea that ESP information may be received by a person without conscious awareness. When G. D. was satisfied that the participant understood the purpose and procedures of the experiment, the volunteer was led to the test room, located approximately 60 feet from the agent's room. The volunteer was seated in a recliner chair and stainless steel GSR electrodes were attached to the pads of the index and ring fingers of the left hand. The electrodes were attached via shielded cable to a GSR amplifier and a chart recorder located in the agent's room. Located behind the volunteer was a carousel slide projector that was remotely controlled from the agent's room. The slide carousel contained 29 35mm slides. The first four slides were neutral slides having no relationship to the remaining 25 slides, which comprised the target pool. The four neutral slides were included to aid the volunteer in becoming accustomed to the projector changing slides, hopefully eliminating novelty-induced orienting responses. The remaining 25 slides consisted of five pictures of each of the five standard ESP symbols, thus constituting a standard ESP card deck. The 25 slides were arranged in blocks of five slides, each block containing one picture of each different ESP symbol. The exact order of slides within each block was randomly determined by a card shuffle, and prepared by someone not otherwise connected with the experiment. The sequence of slides was not known to G. D. until all responses had been scored at the end of the session.

G. D. served as the agent. After leaving the volunteer alone in the test room, G. D. returned to the agent's room and opened a previously prepared envelope containing information as to which of the five symbols would be the target for that session. As with the slide

sequence, the envelopes were prepared by someone not directly connected with the experiment. The sensitivity of the equipment was adjusted so that a large response would not move the recorder pen off scale. The amplifier was set in the AC mode, meaning that only rapid decreases in skin resistance would result in a pen deflection, thus filtering out slow basal resistance shifts.

After 10 minutes had elapsed, G. D. projected the target slide onto a screen in his room and attended to it. At this point, G. D. also activated the slide projector in the volunteer's room and began presenting the target pool to the volunteer, changing slides approximately every 20 seconds and noting on the chart recording when each slide was presented. This procedure was followed until all slides had been viewed by the volunteer. G. D. then reentered the volunteer's room and asked for a verbal guess of the target for that session.

Scoring of the GSR responses was done by G. D. on a blind basis. A response was defined as a deflection of the recorder pen of at least two mm. The deflection of the pen must occur, or begin to occur, within one-half to seven seconds after presentation of the slide. After the amplitude of all responses had been scored, the sequence of target slides viewed by the volunteer was made known to G. D., who then noted on the chart record which responses were associated with which target symbols.

For each volunteer, the amplitudes of responses to each of the five different symbols were summed. The symbol with the largest summated response amplitude was designated as that volunteer's "ANS Guess" of the target. In this manner, each volunteer produced a "verbal guess" and an "ANS guess."

Experiment 2. The second experiment was intended as a confirmatory experiment. The procedures used in the pilot study were followed, with the exception that a microcomputer was used as an aid. The computer randomly chose the target slides, controlled the volunteer's slide projector, and scored the GSR responses. This freed the experimenter from many tasks and provided more opportunity to attend to the target picture.

There were seven correct ANS responses and four correct verbal responses in Experiment 1. The ANS hit rate exceeded chance expectation ($p < .05$, binomial test, two-tailed), but the verbal hit rate did not. Experiment 2 yielded four correct ANS responses (n. s.) and three correct verbal responses (n. s.). Combining data from both experiments, there were 11 correct ANS responses ($x^2 = 4.22$, 1 df, $p < .05$) and seven correct verbal responses (n. s.).

The data at least suggest that GSR responses can serve as indicators of psi, perhaps being more accurate than verbal responses. Additional data will be needed to determine if the ANS is reliably more accurate.

MICRO-PK STUDIES*

THE POTENTIAL OBSERVER EFFECT, OR THE MYSTERY OF IRREPRODUCEABILITY

Dick J. Bierman † and Joop M. Houtkooper (Research Institute for Psi Phenomena and Physics, and University of Amsterdam)

This experiment tested an extension of the Observational Theories proposed by Millar and Hartwell in their "Dealing with Divergence" in RIP 1978. In this extension, all people contribute to the total psi strength. However, their contribution is weighted with the probability to become an observer of this outcome. The theory is not explicit on how to determine this probability. For our purpose, however, two definitions are satisfactory. The probability might be determined at the moment of target generation or at the moment of first conscious observation (generally by the subject).

The potential-observer model indicates that there is a possibility to screen off future observer influence. It was decided in our PK experiment to destroy randomly a fixed percentage of the trials after the first conscious observation by our subjects. For each trial, the potentiality to stay in the final analysis is therefore reduced, and hence observers of the experimental results are bound to have less influence on the experimental results.

As a first check on the success of this procedure, the data left over were split into two parts to be analyzed by different analyzers ("observers"). Any differential effect between analyzers could be interpreted as a failure of the procedure because the screening would not have worked. Moreover, the decisions as to which parts of the data were to be analyzed by whom were made randomly, and the potential-observer model would predict that these analyzers would each contribute to 50 per cent of each data set, thus excluding the possibility of an analyzer effect.

On the other hand, we introduced two conditions yielding two different numbers of potential observers. On the basis of a non-random decision, the results of half of the runs were hand copied and given to the subject. These data could therefore potentially be observed by more people. Direct PK, random PK within as well as

*Chaired by John Beloff, University of Edinburgh.

over runs and burst scores were analyzed. No condition effects were found. However, a significant analyzer effect was found for random PK within the run ($t = 2.307$, $df = 8$, $p < .05$).

The combined score of the analyzer effects in the four planned analyses was appreciably larger than the comparable combined scores for the condition effects, but no statistical test could be applied to this measure because of nonindependence.

From pilot to confirmation, it was decided to increase the destruction rate, for the obvious reason that analyzer effects still seemed to be present. To keep about the same amount of data after destruction, we had to raise the number of subjects from 10 to 20. It was therefore decided that the two experimenters, who in the pilot had handled each subject together, would work alone in the confirmation. This enabled us to test for psychological experimenter effects. Furthermore, because a significant post hoc effect was found in the pilot, it was planned to include this analysis in the confirmation, which raised the number of planned analyses to five.

The results in the confirmation showed no condition effect whatsoever; also, the analyzer effects were reduced, except for the one that was found post hoc in the pilot. There turned out to be more experimenter effects this time. A combined score over the five planned analyses shows that only the psychological experimenter effects are marginally significant. The only finding that replicated from pilot to confirmation was a scoring difference in the first 32 trials of the runs that was related to the analyzer. This applies to the direction of this difference as well as to the magnitude. For the pilot, $t\ diff = 3.163$, $df = 8$, $p = .0133$, and for the confirmation, $t\ diff = 2.759$, $df = 18$, $p = .0135$.

A way to interpret this finding is to assume an interaction between the subjects' and the analyzers' psi. In the "Checkers" experiment of Feather and Brier (JP, 1968, pp. 167-75), the main checker effect was found in the first run, which, to some degree, coincides with our finding. The overall conclusion of the experiment is that no substantial support for the potential-observer theory was found, but that decrease of analyzer effects from pilot to confirmation justifies another experiment to test this model.

AN INVESTIGATION INTO THE USE OF AVERSION THERAPY TECHNIQUES FOR THE OPERANT CONTROL OF PK PRODUCTION IN HUMANS

Richard S. Broughton, † Brian Millar and Martin Johnson (University of Utrecht, Netherlands)

One possible way of overcoming the problems of reliability continuing to trouble parapsychology would be to find evidence of psi

clearly linked to identifiable activities on the part of the subject. An attractive candidate for a source of such evidence is the possibility that psi phenomena can be learned, developed or in some way brought under the control of the individual.

With regard to ESP, this idea has received intermittent consideration, and some years ago Tart made certain theoretical advances in this area. Recent experimental work by the same investigator has yielded evidence that must be considered equivocal due to strong criticisms by Stanford and Gatlin.

Attempts to demonstrate the learning of PK, in contrast to ESP, would largely bypass many of the problems inherent in the use of ESP, particularly those highlighted by Gatlin. Very little work expressly designed to test this idea has been reported, and PK work for other purposes, though often fulfilling Tart's criteria for learning, have provided no evidence to suggest that learning does occur.

Following a long-standing idea of one of the authors, M. J., it was decided to employ one of the most powerful conditioners of behavior available, operant conditioning, in this case in its negative form as aversion therapy, to see if it would provide the necessary conditions, whether related to learning, motivation or both, to enable subjects to gain control of or to learn to use PK.

Punishment is widely recognized as a potent conditioner of human and animal behavior and is in wide use in behavior-therapy programs. Clear guidelines for the effective use of punishment are available.

One of the simplest means of administering punishment contingent upon an unacceptable behavior is by painful electric shock, a means commonly employed in human behavior modification. This investigation sought to determine if a painful electric shock contingent upon a predefined "unacceptable" level of performance in a PK task is sufficient to cause subjects to avoid such responses to a degree that would indicate PK activity.

The PK task was performed on a game-like device used in previous work and called "Kop van Jut" (EJP, 1979, pp. 337-57). It consists of 32 lights in a column that respond to a computer-controlled random event generator. Lights come on only for the hits in a run of 50 binary trials and are timed to simulate a weight rising and falling as in a fairground amusement device. Subjects initiate each run by hitting a button.

In this investigation, for each run score falling below lamp 22, the subject was given a very unpleasant shock via a commercial behavior-therapy device connected to the computer.

Following behavior therapy research, an ABA design was employed, the A condition being an initial baseline measurement on

the dependent variable, the B condition being the introduction of the manipulation (in this case shocks), and the second A condition representing a return to the baseline situation. With the small number of subjects used, the three authors and one volunteer, the investigation was essentially a case study.

To allow for a thorough examination of each individual's performance while preserving statistical rigor, it was decided at the outset that each participant would do two sets of ABA conditions, a pilot and a confirmation. Robust effects were thought necessary to justify such extreme techniques.

The experiment ran as follows: each button-press initiated a run of 50 binary trials. A session consisted of 24 such runs (about eight to 10 minutes). The baseline consisted of eight sessions with no punishment. The experimental condition consisted of eight sessions during which each run with a score of less than 22 was punished by a shock. By chance, there would be about four per session. For the return to baseline, there was a third set of eight sessions but without the shock. Subjects ran their sessions ad lib with an average of one per day being the recommended rate.

The principal dependent measure to test the effectiveness of the treatment was the number of run-scores falling below 22. Other possible effects would be looked for, including a release-of-effort (ROE) effect, for which this experiment seemed particularly suited.

The pilot results are reported first. It is in these data that effects are sought, the second ABA set serving only to confirm effects found in the pilot.

The results were mixed. For two subjects, R. B. and S. S., there was no evidence of any psi effects. The below-22 scores for R. B. were 28, 30 and 32, and the 30 shocks were not significantly different from the expected total of 31. The below-22 scores for S. S. were 26, 31 and 32.

Subject B. M. produced more interesting data with scores of 43, 24 and 32. With 24 shocks, he achieved the lowest total but it was not significantly different from chance. The shock condition was significantly lower ($p = .01$, two-tailed) than the preceding baseline condition, but this was due to the significantly large ($p < .02$, two-tailed) number of "would-be" shocks in the baseline.

Subject M. J. turned in chance scores of 26, 34 and 26, but in his data was a strong suggestion of a ROE effect. During the shock condition, his effort trials (all those up to and including the 22nd hit) yielded a CR = 1.36, while the ROE trials (those occurring after the 22nd hit) yielded a suggestive CR = 1.77 ($p < .08$, two-tailed). Of special interest was Session 3, prior to which the subject entered into the record a complaint that the shock was particularly strong in the presession test. For that session, his effort trials produced a CR = -0.67, but the ROE trials produced a CR = 2.66 ($p = .008$, two-tailed).

From the results of the pilot study, it was clear that the main aim of the experiment was not achieved. Punishment by electric shock did not reduce the number of low runs in the PK task. The subsidiary effects noted in the data of B. M. and M. J. were to be looked for in the confirmation data, and the suggestive evidence for a release-of-effort effect seemed particularly promising.

The confirmation results for subjects B. M. and M. J. only are reported here since only in their data were there effects to be confirmed.

The confirmation results for subject B. M. did not replicate those of the pilot. His baseline score of 25 was the lowest rather than the highest of the set and the number of shocks received was close to chance at 28.

In the confirmation data of subject M. J. the ROE effect also failed to replicate. In the shock session, effort trials yielded a CR = -0. 81 and the ROE trials CR = -0. 34. In the experiment, the senior author managed to reproduce the "Session 3" situation at one point in the shock series. (Subject M. J. was unaware of the pilot results.) However, this session produced the lowest rather than the highest ROE rate in the series.

This experiment can serve parapsychologists as a reminder of the need to be extremely cautious in the interpretation of effects found in situations where ad lib statistical searching is employed, as in our pilot. One must always insist that post hoc effects are rigorously confirmed before releasing them as findings.

The main conclusion of this experiment must be a negative one. Aversion-therapy techniques, punishment for low scores, completely failed to alter scoring patterns in four representative subjects in a PK task. The two interpretations that may be offered are either that the shock was not an adequate conditioner for PK or that PK is inherently unconditionable.

In support of the first interpretation, it should be noted that all subjects reported a degree of adaptation to the shocks. However, this occurs in normal behavior modification, which gets results nonetheless.

The second interpretation suffers from the same problem as trying to "prove" ESP, i. e., the difficulty in demonstrating a universal negative, but it remains the case that there is virtually no unequivocal evidence for conditioning effects in psi behavior. It cannot be ignored that the technique that failed to get results in this experiment is one of the most potent conditioners available, and its failure may provide clues to the nature of psi behavior.

While no firm conclusions can come from an experiment with essentially negative findings, it is hoped that this work may guide future researchers in this area.

A PROGRAM FOR CHANNELING PSI DATA INTO THE LABORATORY AND ONTO THE CRITIC'S DESK

Helmut Schmidt (Mind Science Foundation)

In an attempt to gather psi data for research or applications more efficiently, a large-scale project has been started that includes special attempts to eliminate all possible sources of human unreliability and to channel psi data to seriously interested critics. These attempts are particularly aimed at the fundamental question of whether psi effects are, by their very nature, bound to disappear under the direct scrutiny of sufficiently aggressive critics.

If psi is based on a noncausal mechanism whereby the future can affect the past, a hostile critic who inspects the evidence after the experiments might be expected to exert a negative influence. The issue then is whether a negative effect from the critic can be outweighed by the efforts of enthusiastic field-workers and subjects.

The following are main features of the project:

1) The work of collecting data is shared between a "central investigator," with scientific training, and a number of "field-workers," selected for their particular skills in motivating test subjects. The field-workers may include parapsychologists, teachers of classes in meditation and psychic development, "psychics" who use themselves as subjects and even salesmen who are paid according to their subjects' success.

2) The field-workers are provided with self-recording electronic PK testers, which minimize the possibility of errors or fraud.

3) Some of the PK tests are done with prerecorded random events. These tests can be designed with "absolute" safeguards against unreliability of people and machines. PK effects observed under these conditions may provide a critic with primary psi evidence not dependent on the reliability of people or test machines.

A typical PK test with prerecorded random events proceeds in three steps. First, a random sequence of "heads" and "tails" is automatically generated and recorded, and several copies of the sequence are made; however, the outcome is not displayed, and is seen by no one. Next, the sequence is presented step by step to a subject who makes a PK effort to have, say, more heads than tails displayed. Finally, the sequence is checked to see whether, indeed, it contains more heads than tails.

Previous experiments suggest the following:

a) PK with prerecorded random events may operate as well as direct PK, provided the subject is not aware of the prerecording and does not, consequently, build emotional barriers against success.

b) Copies of the prerecorded events made before the PK tests still agree with the original data after the PK test. Therefore, we may imagine that PK does not alter the data in the memory chips after they have been recorded, but rather that PK efforts act backward in time to affect the random target generation during recording.

c) For the experiment to succeed, the test session must actually occur. If the experimenter-subject team does not make the later PK effort, the retroactive mechanism cannot operate, and we may expect only chance results.

Operating the Test Machines in Different Modes. Two test machines have been developed for the project: the "Spiral Tester" performs the function of a "16-sided electronic die"; i. e., it randomly selects one of the numbers 1, 2, ..., 16. After a number s is selected, it is displayed by a sequence of s downgoing tones accompanied by the motion of a light moving inward in a spiral formed by 16 lamps. The subject's PK goal is to obtain a high number s; i. e., to hear a long string of tones and have the light move as far as possible toward the center of the spiral.

The other test machine is similar to the "Electronic Coinflipper" used in earlier experiments. A sequence of binary decisions, "Heads" or "Tails" is displayed by clockwise or counterclockwise jumps of a light in a circle formed by 16 lamps. These jumps are accompanied by upgoing or downgoing tones respectively. Thus, the subject's goal may be the generation of many heads; i. e., predominantly clockwise light motion coupled with predominantly upgoing tones.

For brevity, I shall restrict the following discussion to experiments with the "Electronic Coinflipper." One run consists of 64 binary decisions.

In the Play Mode, the binary decisions are made within about one second, and the generated sequence is displayed to the subject at a selectable speed through the described tone and light movements. At the end of the run, the numbers of generated Heads and Tails are displayed for manual recording.

In the Direct PK Test Mode, scores are furthermore automatically recorded prior to the subject's display of the sequence. The data are stored in an INTEL2758 EPROM Memory Chip. These data can be read back at any time, and the memory chip can be removed for computer evaluation.

In the Prerecorded PK Test Mode, the score (numbers of Heads and Tails in a run) is not decided during the test session but is obtained from a memory chip that already has been prerecorded. To prerecord the data for one test run, a sequence of 64 binary random events is generated. To conserve memory space, only the numbers of Heads and Tails are recorded--not the entire sequence.

During the test session, the test machine shuffles the specified number of H Heads and T Tails into a random pattern so that the subject experiences a random sequence of events.

Some Basic Test Arrangements. Let me focus here on the case where we want to use the test machines to provide psi evidence to critical outsiders. I shall discuss a few test arrangements where the critical outsider does not have to trust the reliability of the test machine or the central investigator who provides the machine and arranges the experiment.

The option of a Play Mode was built into the machine mainly for exploratory runs to help select subjects, to find the optimal conditions for a subject or to check if a promising subject is momentarily in a promising mental state. This mode, however, is already sufficient if the critical outsider is willing to run his or her own experiments and trust his or her own recording of data.

Provided that the target directions (clockwise or counterclockwise motion) are randomly alternated from run to run, one does not even require a bias-free operation of the machine to draw conclusions about the presence or absence of psi.

If the automatic recording is also used (Direct PK Test Mode), the critic may entrust the gathering of data to some field-worker who is selected for the ability to motivate and communicate with people. This field-worker would have to be reliable to the extent that he or she would not commit cleverly premeditated fraud by opening the test machine and changing the electronics or the data stored in the memory chip.

As safeguard against a possible machine bias, the critic could specify the target direction for each run, and the field-worker would instruct the subjects accordingly. When the memory chip is filled, the critic can operate the test machine to read out the recorded data and check the extent to which the scoring directions agree with the target direction.

For the critic who does not want to invest much time and who does not want to rely on the dependability of any other person, the Prerecorded PK Test Mode may provide "absolutely reliable" evidence for the presence of psi in the following arrangement:

As first step, the main experimenter generates many 64-bit random sequences and has the numbers of Heads and Tails automatically recorded in a memory chip (one chip can hold the scores for 1,024 runs). A printout of these scores is sent in a sealed package to the critic, who must not break the seal before the experiment is completed. Next, the critic prepares, with the help of a random number table, a list specifying the target direction (Heads or Tails) for each of the 1,024 runs.

The test machine with the memory chip and the critic's target

list are then sent to the field-worker, who does the test runs with the specified target directions. After the 1,024 test runs are completed, the critic opens the sealed computer printout and compares it with his or her list.

If the subjects in the tests scored predominantly in the specified directions, this is apparent by a correspondence between the scoring directions from the sealed package and the target direction that the critic prepared independently. Because the critic received the package before handing out the list of target directions, the critic can rest assured that this correspondence is not fraudulently produced, and that only chance fluctuations or psi could have produced it.

At the present stage of the project, several test machines of each type mentioned are available, and more machines are under construction. Thus, the program is starting gradually.

The main question to be studied is whether psi effects can be produced under the direct supervision of skeptical critics--or whether psi effects tend to evaporate under such conditions.

We cannot expect this to be settled overnight, and we plan to approach the question gradually. In the first phase of the project, the emphasis will be on the selection of promising field-workers: we seek field-workers who appear to "get results" from the subjects. The tests will often be done in a two-experimenter design so that two persons can assess the outcome independently. If, at this stage, PK effects have appeared, we will enter the second phase by arranging tests with the most successful field-workers under the scrutiny of critical outsiders. Again, we would proceed gradually, inviting first the open-minded critics who are not emotionally committed to one fixed viewpoint.

The success of the project will depend upon how energetically it is carried out. Considering the small size of the effects, and the efforts required to involve many promising field-workers, it will by no means be an easy task to present hard psi evidence to critics. If, on the other hand, the scrutiny of hostile critics should turn out to stifle psi effects, we want to be sure that we have really made the best possible effort to overcome such negative effects by selecting good field-workers.

DIRECTIONAL PK EFFECTS WITH A COMPUTER-BASED RANDOM GENERATOR SYSTEM: A PRELIMINARY STUDY

Lawrence Tremmel † and Charles Honorton (Maimonides Medical Center)

Over 40 observers, individually or in small groups, attempted to demonstrate significant directional PK effects using a totally auto-

mated, computer-based random generator system. We expected High-Aim runs in which observer feedback was contingent upon positive deviations from chance to show significantly higher random generator scores than Low-Aim runs in which observer feedback was contingent upon negative deviations from chance. Simple on/off graphic feedback was calculated from runs of 100 binary samples. The number of runs per session varied from 10 to 100 with 67 per cent of the total sessions (159/239) consisting of 50 runs. The required magnitude of directional random generator scores necessary to trigger observer feedback displays was varied from $\pm$ 0.2 standard deviations to $\pm$ 4.0 standard deviations to measure the value of stimulus hunger as a psi motivator. Since observers sat in an otherwise dark room and the frequency of displays decreased with the increases in display threshold criteria, we would expect to see stronger PK effects with higher threshold criteria if stimulus hunger is a psi motivator.

Observer feedback consisted of animated, colorful abstract graphic designs. Unless the directional criterion was met on a given run, the observer saw only a dull and boring display (e.g., the word "RELAX" or a white dot in the center of the screen). When the run criterion was met, the observer saw a graphic display develop. Since there were a large number of such displays, 60 initially and later 120, and the colors, patterns and duration of the displays varied randomly, novelty and sustained interest could be maintained. Observers operated on instructions to sit back, relax and allow themselves to be entertained.

All data were collected, recorded and analyzed via a Cromemco System Three microcomputer. The Z80-based system includes 64K (bytes) RAM, dual disk drives, CRT and color TV monitors and color-graphics capabilities.

Random numbers were obtained from one of three sources each session. Source-1 (Texas Instruments MD20 diode) and Source-2 (Micronetics Diode NK-1) are electronic noise diodes and both are indeterminate systems. Source-1 operates via a quantum mechanical tunneling process and has a noise output approximately three orders of magnitude greater than Source-2, a pure avalanche device. Source-3 is a pseudorandom algorithm residing in Cromemco's 16K Extended Basic and is deterministic after initialization. If psi serves to organize the noise in a system, we would expect stronger PK effects on Source-1 than on Source-2. If PK influences indeterminate systems more easily than deterministic processes, we would expect stronger PK effects on the two electronic generators (Source-1 and 2) than on Source-3.

Overall, 239 High and Low aim sessions have been completed with this PK program, comprising 10,510 runs (1,051,000 binary samples). Neither High nor Low aim conditions were independently significant, though both were in the predicted direction. The difference between conditions was significant by t-test ($t + 2.08$, 237 df, $p = 0.038$, two-tailed), but not quite by Z analysis based on the trial as a unit ($Z = 1.75$).

The results from the two electronic generators (Sources-1 and -2) show that the High-Aim condition is independently significant by both t and Z analyses ($t = 2.40$, 95 df, $p = 0.018$; $Z = 2.47$, $p = 0.018$), as is the difference between High and Low aims. This effect was contributed almost entirely by Source-2, with directional but clearly nonsignificant results for Source-1. The results from Source-3, the pseudorandom algorithm, were totally nonsignificant.

The effect of stimulus hunger as a psi motivator was assessed by correlating the Z-threshold value of each session with the overall session PK score. For all sessions, the Pearson r was significant ($r = 0.169$, 237 df, $p = 0.0095$). The r was also significant ($r = 0.183$, 177 df, $p = 0.015$) for sessions using only electronic random generators (Source-1 and -2).

The experimental results were simulated in 60,000 control runs, 1 million binary samples per source, without observers, with means and standard deviations that closely approximate chance results.

The primary goal of this preliminary work, a demonstration of significant directional PK effects using a computer-based random generator system with programmed motivational elements, was achieved to a modest degree in sessions using electronic random sources. While only suggestive, the absence of directional effects in sessions with the pseudorandom source supports the hypothesis that PK operates most effectively with indeterminate systems.

The isolation of significant effects to Source-2 in this study, the less noisy diode, does not support the hypothesis that noisier systems are more sensitive psi detectors. While tantalizing, the result is uninterpretable at present because this result reverses that obtained in another study reported, in which the significant effects were obtained only on Source-1. This discrepancy must be resolved through further work with both diodes.

The small but significant correlation between PK score and observer feedback threshold provides tentative support for the hypothesis that stimulus hunger can be used as a psi motivator.

SPONTANEOUS CASE/FREE-RESPONSE METHODOLOGY*

A PARTLY INDEPENDENT REPLICATION OF INVESTIGATIONS OF CASES OF THE REINCARNATION TYPE

Satwant Pasricha † and Ian Stevenson (University of Virginia)

Since 1961, one of us (I. S.) has investigated in India and elsewhere cases of persons (usually children) who claim that they lived before and can remember details of the claimed previous life. In an attempt to replicate I. S.'s investigations, S. P. studied a series of 45 cases of this type in India independently of I. S. We then compared her results with data from 50 Indian cases investigated earlier by I. S. The present report is mainly concerned with the assessment of authenticity of the cases rather than with their evidence for paranormal processes, for which we refer readers to other published reports of these cases.

Interviewing informants connected with the subjects as well as with the concerned deceased persons elicited data on 86 variables. For nine of these variables, the data were insufficient or judged unreliable and therefore were not included in the final analysis. Of the remaining 77 variables, 16 concerned demographic features, 57 concerned the main features, and four concerned aspects of the investigation of the cases. All these variables were originally analyzed separately, but later, on the basis of similarity in content of certain features, some variables concerning main features of the cases were combined, and this reduced their total from 57 to 36; variables with significant differences, however, were not combined in any way. No data concerning the demographic features or the investigation of the cases were combined. Compared by means of appropriate statistical analyses (chi-square test or median test), 12 variables showed statistically significant differences. Of these, three were demographic features, six were main features, and three concerned features of the investigations in the two series.

In considering the differences, we were aware that significant differences might derive from several factors, including differences in the samples, dissimilarities among informants in observing and reporting features or inconsistencies between the investigators in collecting data. Of these factors, two seemed particularly important.

*Chaired by Robert L. Morris, University of California, Irvine.

First, newspaper reports furnished first information about many of I. S. 's cases, whereas S. P. generally learned about her cases from informants for other cases. Since cases in cities and towns are more likely to come to the attention of journalists than are those in villages, the different sources probably yielded samples that differed in demographic and other features. Second, since I. S. began investigating cases 13 years before S. P. did, both the period during which his cases developed and the length of time he has followed them differ importantly from these features in her investigations. These factors may account also for some of the differences found among the main features of the cases and in aspects of the investigation of the cases.

There were, however, other differences between the two series that do not seem explicable by sample differences deriving from these two factors. Some (such as the frequency of reports of ESP in the subjects) may derive from differences in the investigators' methods; others, however, do not lend themselves even to this third explanation. For example, there was a significant difference between the two series in the geographical distance between the subject's birthplace and the site of the concerned previous person's death; and yet we consider data of this type objective, and therefore differences between the series due to recording errors seem improbable.

The resemblances between the two series were more numerous and, in our opinion, among the more important variables compared. Although I. S. trained S. P. and controlled funds for much of her research, we do not believe the similarities derive from this association. S. P. had over five years of previous research experience. More importantly, many of the variables involved objective data that virtually preclude even unconscious recording biases by different investigators. Finally, I. S. 's publications contained only brief allusions to a few recurrent features in cases of this type; the present analysis was, in fact, the first systematic analysis of a large series of Indian cases and therefore the first comprehensive information either of us had about many recurrent characteristics in them. S. P. could not therefore have known about all the features of a typical Indian case, to which she could then (consciously or unconsciously) have made her data conform.

We cannot exclude the possibility that I. S. communicated to S. P. more than he realized about his own emerging ideas about the characteristics of Indian cases of this type. He might have done this when they worked together on other, jointly investigated cases. We think it unlikely, however, that such communications of still undeveloped impressions about the cases could account for all the correspondences found in the comparisons of these two series of cases.

We have also considered the possibility that some oral tradition in India about features of a typical case of the reincarnation type has influenced the data given by informants to us. Although some informants had rudimentary ideas about such features, most

had no clear concept of the features that have emerged from these investigations. Furthermore, informants rarely know details of another case, and, even with the increased publication of reports of cases of the reincarnation type, little information about recurrent features has reached informants in India.

The uniformities between these two groups of cases seem to indicate that the informants' reports are accurate (on the whole, if not in all details) and that the events described relate to some genuine phenomenon. The interpretation of the phenomenon becomes relevant, however, only when we have established the authenticity of the reports. Although S. P. 's investigation provided a replication that was only partly independent of the earlier study, we believe the results of the present analysis constitute a step toward ascertaining such authenticity.

QUALITIES OF FREE-RESPONSE TARGETS AND THEIR RELATIONSHIP TO PSI PERFORMANCE

Lendell Braud Williams † (Psychophysical Research Laboratories) and Darrel Max Duke (California Institute of Professional Psychology)

A variety of targets have been used in ESP experiments, including ESP symbols, art prints, binary targets, animal pictures, erotic pictures, words, geographical locations, sounds, music and a variety of other auditory and visual stimuli. However, no systematic attempt has been made to evaluate the quality of targets.

One of the issues concerning what makes a good target centers around individual differences. Are there really "objectively good" targets, or is this totally a subjective matter? In order to address this question, two types of analyses were performed. One involved the subject's preference for the target, or how well the subject liked the target. The other involved assessment and evaluation of specific characteristics of targets and how these characteristics related to psi performance.

All 174 subjects were run as part of other experiments designed to investigate defensiveness and altered states of consciousness. The authors felt that this sample size was large enough to provide an adequate analysis of which targets were best. However, we felt that a target had to be used at least three times before minimal data could be collected on the quality of that picture as a target. This criterion reduced our subject sample to 91 subjects.

The experimental procedure was the same as used in our previous studies (see RIP 1976, p. 163). All subjects were run in a classroom-type environment at a local school or church and were run in groups after undergoing a 45-minute psi-conducive tape. Each subject had his or her own target concealed in two opaque envelopes.

The Houston lab has 152 targets. Each picture was rated on 39 separate items on a 1 to 7 scale. The 39 items were designed to measure general qualities, altered states qualities, emotional qualities and form and color. The Target Evaluation Rating (TER) provided a measure of the presence, absence and degree of certain target qualities.

Both authors rated each item independently and averaged these ratings to yield a single score for each of the 39 items on the evaluation form. The items that composed each of the dimensions were averaged to yield a mean for each dimension and a total mean for the target. The TER yielded eight means for each target.

We decided not to include the form and color dimension in the total TER because we were unable to develop an adequate evaluation procedure for this dimension. The emotional impact score was not added into the total since it was only one item. It was placed on the original evaluation form as a control to see if a one item rating of emotionality could discriminate as well as the 12 items on the total emotional scale.

Each of the 152 targets were evaluated in this way. After evaluation, we calculated the number of times each target had been randomly chosen in the ESP experiments. We had previously decided that a target needed to be chosen at least three times before we could collect adequate data on its effectiveness as a target. This criterion provided 22 targets that had been randomly selected three or more times, and these 22 targets included ESP data from 91 subjects. Individual psi scores for each of these targets were averaged to provide a composite psi score for each target.

The composite psi scores ranged from 1.00 to 3.25. The data were split into two groups: good psi targets (1.00--2.33) and poor psi targets (2.40--3.25). This division yielded 12 high-psi-scoring targets and 10 low-psi-scoring targets. The Target Evaluation Ratings (TER) for the 12 high-psi targets were compared to the TERs for the 10 low-psi targets using Mann-Whitney U tests.

The psi results for the 91 subjects were analyzed for hits and direct hits. A ranking of 1 for the target picture was a direct hit, while a ranking of 4 was a direct miss. Rankings of 1 and 2 were combined to yield the number of hits, while rankings of 3 and 4 were combined to yield the number of misses. There was 55 hits for the 91 subjects and these results produced significant psi-hitting ($Z = 1.00$, $p \leq .047$, two-tailed) by a binomial test.

The subject's evaluation of the targets reflected subject preference, or how well the subject liked the target as a picture. Each subject ranked the picture on a 1-to-4 scale on how well the pictures corresponded to his or her ESP impressions. In addition, the subject also ranked the picture on the degree he or she liked the picture as a picture. Mann-Whitney U tests were calculated to compare liking for the hit and the missed targets. The data from

the first sample (N = 101) yielded a Z = 2.12 and a $p \leq .035$ (two-tailed) while the data from the second sample (N = 80) yielded a Z = 2.90 and a $p \leq .0038$ (two-tailed). In both samples, subjects who liked the target did significantly better at ESP performance than subjects who did not like the target. These results indicate that the target is an important variable in ESP performance.

We compared good psi targets to poor psi targets after these targets had been evaluated on all the dimensions of the TER. In general, targets that were rated as better targets on the TER were associated with better psi performance (hitting). Mann-Whitney U tests were calculated for each of the TER dimensions and the total TER. The total evaluation was significant with a $p \leq .02$. The general dimension was also significant ($p < .02$).

The emotional dimension was evaluated in three ways: positive emotion, negative emotion and total emotion. Targets that contained a positive emotion were associated with better psi performances ($p \leq .02$). Targets that contained negative emotion were associated with poorer psi performance ($p \leq .047$). The total emotional score was a mean of the positive and negative emotional ratings. Targets that contained a stronger emotional content were significantly better targets ($p \leq .001$) than nonemotional targets. We wanted to make sure that the three emotional scores (positive, negative and total) were actually measuring specific emotions and that it was necessary to specify and evaluate these specific emotions. If this were not the case, then the three scores might each be measuring the degree of emotional impact alone, without regard to the specific type of emotion. This would mean that each of the three emotional scores was nothing more than a global rating of emotional impact. Therefore, we did a separate one item rating of the degree of emotional impact of each target on a 1-to-7 scale. This was not a post hoc analysis since it was rated on the original TER form but it was not included in the total. We analyzed this one item to see if it discriminated psi-hitting targets from psi-missing targets as well as our three emotional scores. This analysis was not significant ($p \leq .28$). It appears that the specific emotional items on the TER are specific and are not merely an index of general emotional impact.

The altered-states-of-consciousness dimension was not significant, but this dimension was not truly tested, since the 22 targets did not include any altered-states targets.

The form-and-color dimension was added to see if this factor could be evaluated. We have not yet developed a proper way to evaluate this dimension. This dimension did not yield significant results. Our new two-item dimension appeared to be better.

After completing the statistical evaluation, we looked at which specific targets had been associated with psi-hitting and which had been associated with psi-missing. It appeared that most of the psi-hitting targets were natural, while the missing targets were material objects--mental, concrete, artificial and mechanical. There-

fore, we did one post hoc analysis on this one item. This analysis was significant ($p < .02$), indicating that natural targets did better than unnatural targets.

The response bias or artifact issue is an important one and must be addressed systematically. This issue is particularly important when considering the good targets. A target could be receiving spuriously good psi scores for several reasons: 1) the target matched specific content in the psi-induction tape, 2) the tape produced a state that induced some general meditation imagery that matched the target and 3) the subjects just liked these targets and therefore chose them as the target. However, if this is true, the target should also be chosen as the target when it is serving as a control. Therefore, if the good targets are ranked better when they are serving as targets than when they are controls, this would falsify the response-bias hypothesis. This analysis was run. Eleven of the 12 good targets were ranked better when they were serving as target rather than control (binomial $p < .0029$). In addition, the picture's mean-control rank was subtracted from its mean-target rank ($\bar{X}T - \bar{X}C = d$) to yield a difference score for each of the 12 good targets. A t-test was computed on these difference scores. We found that the good targets were ranked significantly better when they were serving as target rather than control ($t = 4.56$, 11 df, $p \leq .00082$, two-tailed). In this analysis, the good target is compared against itself under two conditions (target vs. control).

However, another analysis could be done to address the response bias hypothesis that compares good vs. poor targets under the same (control) condition. If the good targets are systematically biased or just liked better, then subjects should in general give them good rankings even when they are serving as controls, while poor targets should not be given good rankings when they are serving as controls. An analysis was done comparing the control rankings of good and poor targets, and this analysis was not significant.

There is another theoretical possibility that should be considered other than tape-content bias or other forms of response bias. If ESP is a more "primitive" or basic communication system, then it should be associated with subcortical brain functioning or perhaps "right hemispheric" rather than "left hemispheric" content, such as language. There would appear to be no need to have two separate communication systems both designed to duplicate verbal information. The left hemisphere is well designed to communicate information through language. In addition, there would have needed to be other communication systems before language developed in humans or to serve this function in preverbal, brain-damaged or mentally retarded children who are poor in verbal skills, or in infrahuman animals. If ESP is associated with this type of communication, then perhaps targets that reflect basic "subcortical content," such as emotion, sex, survival, nature, food and other basic concerns, might be psychically perceived better than other types of targets.

THE USE OF JUDGES' RATINGS TO TEST HYPOTHESES ABOUT PSI PROCESSES

Irvin L. Child † and Ariel Levi (Yale University)

We previously reported (Child and Levi, RIP 1978, pp. 67-68; JASPR, 1979, pp. 273-89) data suggestive of psi-missing in a free-response situation. As a class exercise, students in a parapsychology course were required to take part in Ganzfeld telepathy sessions. One student, as agent, concentrated on a picture (one of the Maimonides slides prepared for this purpose--see Honorton, JASPR, 1975, pp. 353-59) and tried to communicate it telepathically. Another student, with presumably relaxing stimulation by monotonous light and sound, attempted to receive the picture and described aloud the imagery he or she experienced during the 30-minute session. This student, termed the percipient, afterward rated the resemblance of the imagery experienced to each of the five pictures in the pool of potential targets from which the actual target had been selected at random. In 14 sessions, percipients gave a significantly lower average rating to the targets than to the nontargets.

At each session, the percipient's imagery descriptions were written down by an experimenter. These descriptions were subsequently typewritten (following detailed rules to ensure objectivity of transcription). Similarity between these protocols and relevant slides was later rated by outside judges. The outcome of those ratings will be reported here, and the use made of them follows, in general intent though not in details of procedure, the study by Palmer, Bogart, Jones and Tart (JASPR, 1977, pp. 121-45).

The rating was done in individual sessions by eight judges paid for their participation and not otherwise connected with the project, who signed up in response to a notice posted on a bulletin board in the Psychology Department. No mention was made of psi in the notice or in subsequent communication with the judges, and apparently none of the judges guessed that the research was parapsychological. In other respects, the judges were accurately informed about the task. They were to rate on a 100-point scale the similarity between imagery descriptions and pictures that may have influenced the imagery. The first rating session began with the judge's reading detailed instructions about the various kinds of similarity that might be relevant (adapted from instructions previously prepared or used by Arthur Hastings, Robert Morris and John Palmer, who all generously made their instructions and suggestions available to us). The instructions remained available to the judge during this and the subsequent rating sessions.

For each Ganzfeld session, a judge began by reading the imagery description, which averaged more than a page of double-spaced typing. He or she then had to rate 10 slides for similarity to the imagery, giving each of the 10 a different rating (in order that rankings would be available if needed). The judge controlled the

projector and was encouraged to move back and forth among the slides in any way that would be useful. (All slides used were colored, because when the Ganzfeld sessions were done we had only the colored half of the Maimonides slides.)

Each set of 10 slides rated in relation to a particular imagery description was made up as follows: one was the target the agent had tried to transmit; four were the other slides in the target pool, which--along with the target--the percipient had seen and rated at the end of the Ganzfeld session; the other five were additional slides from the Maimonides set. To increase the chances of substantial variety, each of the latter was opposite to a different one in the target pool on the nine dimensions other than color used in constructing the Maimonides set. These nonpool slides had never been associated in any way with the imagery description until the materials were assembled for the judges' rating sessions, several months after the Ganzfeld sessions. The 10 slides of a set were arranged in a separate random order for each judge, determined by consecutive entries in a table of random numbers. The 14 sessions were also arranged in a separate random order for presentation to each judge.

The judges went at their own pace, while the experimenter sat elsewhere in the room, reading or tabulating data. On the average, a judge spent about 20 minutes in rating each set of 10 slides.

The ratings by outside judges confirmed the indication of psi-missing, though with a significance level of .01 instead of the .004 that a parallel analysis yields for the percipients' own ratings. This provides a sound basis for arguing that if there is a stable difference between similarity ratings of targets and of nontargets it does not depend entirely on the percipients' psi at the time of rating, nor on aspects of the percipients' experience not mentioned in the protocols. The difference in our data depends at least partly on genuine similarities and dissimilarities between slides and imagery descriptions.

Percipients' ratings, as commonly used in free-response psi experiments, permit a comparison between outcomes for targets and nontargets in the pool. In the data we are considering, the imagery appears more similar to the nontargets than to the targets. If this is a dependable finding, to what processes does it point? Is the imagery dissimilar to the target because the percipient is actively avoiding the target, suppressing even random similarities to it, and is the imagery more similar to the nontargets only because the percipient is not suppressing the random similarities? Or is the imagery positively similar to the nontargets in the pool because the percipient has displaced psi functions onto them as substitute targets, while its lower similarity to the proper target is perhaps just at a level attributable solely to chance?

Similarity assessments by outside judges make possible an answer to these questions, while not guaranteeing that the answer

will be neat and decisive. The similarity of the percipient's imagery to the target and to nontargets in the pool can now be compared with its similarity to a control set of pictures not directly associated with the session. The mean rating of the control pictures, 31.0, was lower than the 36.5 mean for nontargets; with a two-tailed test, this difference is suggestive but not significant ($F = 3.38$, 1 and 13 df, $p = .09$). The control (nonpool) slides and the target slides were closer together; while the controls averaged higher than the targets (31.0 against 27.7), the difference was not significant ($F = 1.54$, 1 and 13df, $p = 0.24$).

The outcome here from our data alone is only suggestive in indicating the possibility that the similarity of imagery to the nontargets of the pool is increased, and similarity of imagery to the target may not be diminished, in comparison with control slides. The suggestion is that psi-missing in this situation may be brought about not by active avoidance of the target but by displacement to the nontarget pictures in the pool. The suggestion gains strength from the fact that Palmer et al. (JASPR, 1977, pp. 121-45), with procedures very different in detail, also found evidence that psi-missing in Ganzfeld sessions was accompanied by displaced hitting on other pictures in the pool. Their evidence came only from ratings by a judge who had had Ganzfeld experience herself and not from ratings by another less-experienced judge. Our confirmation is based on ratings by judges not only inexperienced but ignorant of the fact that psi was the object of study. This fact points toward the possibility that this displacement effect may, when tested by procedures maximizing both sensitivity and reliability, prove to be real and substantial. A more adequate test might be made by selecting judges in ways suggested by the qualitative findings of Palmer et al., and pooling the ratings of several such judges.

Perhaps, then, psi-missing might be due in considerable part to displaced psi-hitting? If it is, could a similar process be involved in psi-hitting? Resemblance of imagery to the target might, for example, in some psi-hitting, be only of a degree compatible with a chance origin, whereas resemblance of imagery to the nontargets might be depressed well below the chance level by an active avoidance of the nontargets. Or, perhaps more plausibly, both processes might be occurring, as Tart (RIP 1977, pp. 190-249) has suggested in connection with his concept of transtemporal inhibition (which he applies in a situation where the elements comparable with our nontargets are the preceding and following targets in a temporal series). To investigate these possibilities, we had as many judges as possible (six out of the original eight) rate another body of data suggestive of psi-hitting, after they had finished rating the psi-missing data.

The outcome of this application to psi-hitting data of rating by outside judges was entirely negative. There was no indication that the similarity ratings for nontargets in the pool were depressed below the chance level indicated by the controls.

Our use of judges' ratings instead of percipients' ratings alone has led to the suggestion that a process of displacement plays a part in psi-missing but does not play a part in psi-hitting. Comparing this suggestion with the outcome of other recent research, we find some confirmation of it for psi-missing, some challenge to it for psi-hitting.

THE IMPLICATIONS OF SUBJECT FAMILIARITY WITH FREE-RESPONSE TARGET POOLS

Carol Plumlee Irwin (John F. Kennedy University)

The issue under consideration here is the relationship between psi and memory within the context of the free-response methodology. It is hypothesized that when a subject has knowledge of, or familiarity with, the free-response target pool and receives a psi impression of some aspect of the actual target, psi and memory may be engaged together to create a response that reflects closer correspondence to the target than would occur given only the psi impression and no relevant memory trace. This hypothesis is a derivative of William G. Roll's memory theory, which suggests that ESP responses are revived memory traces. When either an atomistic method of analysis or judging is used to determine the statistical significance of the degree of correspondence between response and target, it may be incorrect to attribute such significance solely to psi.

In an atomistic method of analysis, such as the 10-digit binary coding system used with the Maimonides slide pool, it has been assumed that each target trial constitutes 10 independent trials; to get all 10 content categories correct, the subject must receive psi impressions corresponding to each of them. The number of correct correspondences has been considered a measure of the amount of psi manifesting in the trial or as the psi information rate.

I suggest that this assumption is valid only if all other methods of information acquisition can be ruled out. One such alternate method is memory of the target slide. Since each slide is a unique combination of the 10 content categories, it is possible for a subject who is familiar with the slides to determine cognitively the exact target given only partial target relevant information.

Similarly, in a free-response test in which responses are judged by either rating or ranking, the combination of psi and memory traces of the target may produce a response that reflects greater correspondence with the target than would have occurred without target-relevant memory traces. This increased correspondence will be reflected in the ratings and may be reflected in the rankings of the judges.

Let us assume that a response may have two components: a

"psi component" consisting of those elements attributable to psi, and a "memory component" consisting of those elements attributable to memory recall. In the 10-digit binary coding system, a score of nine correct target-response correspondences may be the result of three psi component and six memory component bits of information. In a rating system with a scale of 0 to 30, the psi component may be responsible for 10 points and the memory component for 15 points of a target rating of 25. The situation with ranks differs in that the effect of the memory component will be inflationary only when it produces sufficient additional correspondence to cause the assignment of a higher rank than would have been assigned based on the psi component alone.

To the extent that psi is displacing to nontarget members of the judging pool for which memory associations are correct, subject familiarity with the pool may cause psi-missing, that is, greater correspondence between response and nontarget members of the pool. The overall effect would be an increase in variance.

The implications of the suggested hypothesis extend beyond the specific situation in which the subject has foreknowledge of the actual members of the target pool to one in which the subject has general familiarity with the target material. When the targets are famous art prints, a student of art history may be more likely to have psi impressions stimulate target-relevant memory traces than would a subject who had never seen a famous art print. This represents a type of response bias.

In the free-response methodology adequate consideration may not be given to the effect of matching target and response biases. While in forced-choice tests the probability of a response matching the target is determinate, in a free-response situation it is indeterminate. The use of human judges or an atomistic method of analysis is an attempt to quantify free-response results so that statistical analysis can be performed. In effect, a forced-choice task is superimposed onto the results of the free-response task. A statistically significant result indicates that something other than chance is operating in the evaluation of target-response correspondence. In the case of judging, this may be psi on the part of the judge, but more likely it is the judge's perceptual ability to recognize some definite correspondence between response and target. The score may be considered a measurement of the amount of agreement between response and target, but not necessarily a measurement of the amount of psi involved in the creation of the free response.

One might counter the foregoing with the view that as long as the response is psi mediated it does not matter how much psi and how much memory is involved in the process. From the point of view of proof research, this may be true. In process research, however, where comparisons are made between subjects based on a quantification of their relative amounts of psi, false conclusions can be reached. A difference in free-response scores may be attributable not to different levels of psi but to differences in target relevant memory traces and psi-mediated cognitive processing.

Until these factors are incorporated into the free-response methodology, we should endeavor to control the apparent source of bias that subject familiarity with target pools presents.

SEQUENTIAL EFFECTS*

"CHANCE": A PROGRAM SYSTEM FOR ANALYZING RANDOM SERIES

Wilfried Kugel (Technische Universität Berlin)

The mathematical method of evaluating psi experiments consists of a basic model that compares measurements with random series. The author developed a Fortran software package containing three parts: a) "Chance," consisting of random analysis of a string (random series); b) "Coincidence," consisting of pattern analysis of a coincidence string (hits); and c) "Strategy," consisting of pattern analysis of a string (response series). The first part (Chance) is completed and will be presented here.

General Purpose of the Program "Chance." It is possible to analyze strings made of an alphabet of two to 10 alternating symbols. The program works in the case of equal probabilities as well as unequal probabilities. It is even possible to adjust the program to detect relative frequencies and then use them as estimation for probabilities in all further analyses. Each string analyzed can be specified with starting point and ending point.

The program is equipped with a facility for generating random numbers with the help of the computer pseudo random number generator. This can be useful for test purposes. As a rule, every pair of theoretical and empirical frequency is compared by the program with the help of the two-tailed sum over the binomial distribution.

As many probabilities are calculated, for every set of binomial probabilities a comparison probability r is given, which serves as a kind of significance level for the case of m probabilities calculated. The formula used is:

$$r = 0.95 \exp(1/m).$$

For all the chi-square tests used the corresponding probability is given by the program.

*Chaired by J. G. Pratt, University of Virginia.

Statistical Methods Used: Frequency Analysis: In the program, theoretical and empirical frequencies of the symbols are generated and printed together with the corresponding (two-tailed binomial) probability. The distribution is tested by the chi-square test. Sequence Analysis: This analysis looks for the distribution of pure sequences of symbols and nonsequences of a symbol. For instance, the string ... 1223231... contains one 2-sequence and one 1-sequence of symbol 2; one 5 nonsequence of the symbol 1; and so on. Since it might be useful to detect patterns of higher order, a sequence analysis was developed. The theoretical frequencies of sequences and nonsequences are calculated by the following formulas:

$$E_n = p^n$$

$$E_i = (n-i+1)p^i - \sum_{j=i+1}^{n} [(j-i+1)E_j]$$

where n is the length of the string, i, j is the length of the sequence, p is the probability for the appearance of the symbol and E is expectancy. For nonsequences, p has to be replaced by (1-p). The second term of the formula for E_i takes into account all smaller sequences contained in a larger one (for instance, a sequence 111 contains two sequences of 11). With this formula, sequences of different length are independent. Theoretical and empirical frequencies are compared by the chi-square test. Transition Probabilities: This analysis generates the transition matrix and compares it with the theoretical matrix by means of the chi-square test. The analysis can be made to the follow-up symbol (distance 1) up to an optional distance (for instance, to the tenth symbol following).

Examples. Tart Data, First Training Study (JASPR, 1979, pp. 29-66): All tests described were applied; the transition matrix was calculated up to the length of 10. The total analysis shows that not one single test ranged within its random limits. Only one of the single evaluations shows randomness (P32). Additionally, there is a tendency of the single deviations to accumulate, as can be seen from the increase of nonrandomness in the calculation of the total analysis. Usually, there should be a tendency of single deviations to cancel each other out, but the contrary is the case here. This evaluation shows, as previously reported by Lila Gatlin, that the random number generator of the first training study in C. T. Tart's experiments did not work well enough to allow further evaluations. Fps Biokommunikation, Series H: Series H consisted of 40 sessions, each with 250 trials. The test device AASW4, a five-choice machine was used. All tests described were applied, the transition matrix was calculated up to the length of five. The total analysis shows that not one single test deviated significantly from random expectancy. Twelve of the single tests are significant at the five-per-cent level, two at the one-per-cent level. This of course lies within the random fluctuations. The single deviations cancel each other out, as can be seen from the total analysis. The evaluation shows that randomness of the target series can be assumed.

TRYING TO PROFIT FROM NONRANDOMICITY IN ESP TARGET SEQUENCES: INITIAL EXPLORATIONS WITH THE PROBABILISTIC PREDICTOR PROGRAM

Charles T. Tart and Eugene Dronek (University of California, Davis and Berkeley)

With the increasing use of immediate feedback of target identity in parapsychological research, the question of consistent departures from randomicity (equiprobability and serial independence) in target generators becomes increasingly important, as it is possible that some percipients might identify such departures and develop some sort of mathematical inference strategy for predicting future targets, thus artifactually inflating their scores. The key question is not lack of randomicity per se, since some pattern can always be retrospectively fitted to any data, but the degree of predictability of the target generator, given knowledge of earlier outputs. C. T. T. has shown (JASPR, 1979, pp. 44-60) that standard chi-square tests of randomicity are poor measures of predictability in short to moderate-length experiments. Here we report the working principles and some initial results with the Probabilistic Predictor Program (PPP), a direct approach to assessing predictability in biased sequences that is expected to be more powerful than known human inference abilities and so can set upper bounds on the degree to which particular results might be due to a mathematical inference strategy.

Target numbers (digits 0-9, to date) are presented to the PPP one at a time. The PPP makes a call, then receives feedback on target identity. The general principle of operation is to create and constantly update files on various aspects of the target distribution, compute the maximum deviation to date of the distribution from a model of equiprobability and serial independence, and choose a call based on that maximum deviation.

For example, suppose that in six trials to date the PPP notes that 1's have been the most frequent singlet to date, viz., two 1's in six trials. If this is all the information available, obviously the best strategy is to infer that the target generator may be biased toward 1's and call a 1. The resulting feedback will update the singlet (first order) memory register, etc.

To date, we have kept registers up to the sextuplet level, which is all we can afford on the Data Corporation model 6400 computer at the Berkeley campus of the University of California. This use of higher order registers makes the PPP sensitive to higher order sequential biases. Suppose that on the start of trial seven, for example, the PPP consults its singlet register and notes that targets 1 and 2 are tied for highest frequency of singlet occurrence. The exact binomial probability of either one of them having occurred twice in six trials, given an assumed equiprobability model ($p = .10$), is .11, one-tailed. The PPP then notes that a 2 was the target on the previous trial, consults the doublet register, and sees

that a 2 has been followed by a 3 once to date. The exact binomial probability, assuming serial independence, of any particular doublet occurring one or more times in six trials, is .06, one-tailed. There is also a relevant triplet event to date, namely that a 1 and a 2 have just happened in the previous two trials, and a 1, followed by a 3, has happened before. The probability of any particular triplet happening in six trials to date is .01, one-tailed. There are no other relevant higher-level registers for this example.

Since the p value of .01 represents the greatest departure from the equiprobability and serial independence model, the PPP chooses to respond from the triplet level, calling a 3, since a 3 followed the previous 1-2. In the example (worked out more fully in the paper) where the target generator is highly biased to produce the sequence 1, 2, 3, 4, 1, 2, 3, 4, 1, 2, 3, 4, 1 ... etc., the PPP would hit on its seventh call and hit on every single trial thereafter.

We expect the PPP to be considerably more powerful than any human mathematical inference strategy that can be expected, for the PPP: a) never forgets or gets confused about what the targets to date have been, up to the sextuplet level, and for any number of trials; b) has a precise and accurate way (exact binomial probabilities) of assessing the maximum departure from the equiprobability and serial independence model to date; c) never gets tired and so prone to error and d) does not become emotionally or cognitively attached to some particular guessing strategy, but accurately updates itself on every trial. Until someone specifies and empirically demonstrates a more powerful mathematical estimation strategy, we offer the PPP as a benchmark for estimating the upper limits of performance of a mathematical inference strategy.

Application to Date. The PPP (with higher registers to the sextuplet level) has been applied to both the target sequences and the response sequences of the 10 percipients completing C. T. T.'s first Training Study (Tart, Learning to Use Extrasensory Perception, University of Chicago Press, 1976). In each case, targets were the digits 0 to 9, and each percipient made about 500 responses to 500 targets. Two of the target sequences showed significant (in this paper "significant" always means $p < .05$, one-tailed) singlet bias by chi-square test. One of these was significantly called by the PPP, for an average hit rate of 3.32 per run of 25, compared to chance expectancy of 2.50 per run. The second and two other target sequences also contained significant doublet biases, a lack of target repeats due to experimenter error in operating the random generator, but the PPP averaged only a trivial 2.55 hits per run on the second sequence and 2.78 and 2.95 hits per run on the other two, both nonsignificant. Two other target sequences, not significantly biased by chi-square test, were significantly predicted at mean hits rates per run of 3.05 and 3.30, respectively.

We expect human percipients to be much more biased sources of numbers than electronic generators, and this was the case here. Nine of the ten response sequences were significantly biased at the

singlet level, seven at the doublet level. The PPP significantly called six of the 10 response sequences, with mean hits per run ranging from 3.28 to 4.90, as opposed to the chance expectation of 2.50.

Both target and response sequences were run through the PPP at various levels of memory register operation, from singlet only through sextuplet. In general, almost all of the predictive power of the PPP for these sequences came from the singlet level, with very little predictability added by higher-order deviations from the serial independence model.

The possibility that successful percipients in the first Training Study inflated their scores by using a mathematical-inference strategy in addition to or instead of ESP was examined in several ways. In terms of hits on the target sequences, the PPP made 577 hits in 5,136 trials, for $Z = 2.95$, but the percipients made 722 hits in 5,000 trials, $Z = 10.46$. (The PPP had more opportunity to score than the percipients, as we let it have feedback and make calls on 136 trials where the percipients passed and did not receive feedback.) The best individual sequence performance by the PPP yielded $Z = 2.50$, but five of the percipients had $Z > 4.17$, with the highest $Z = 11.03$. The performance of the PPP and the percipients on calling the target sequences, in terms of Z-scores, was uncorrelated ($r = .08$).

The temporal displacement pattern of the PPP output was compared with the patterning of the percipients. C. T. T. reported very significant psi-missing by the percipients on the target one ahead into the future (RIP 1977, 197-249), a missing that was significantly correlated with degree of hitting on the real time target: the greater the real-time hitting, the greater the +1 precognitive missing. The overall pattern of temporal displacement for the PPP is strikingly different from that of the percipients, and the correlation between real-time and +1 hitting is opposite in sign to that shown by the percipients. Because of the enormous difference in magnitude between the results of the actual percipients and the PPP in terms of hits, the lack of correlation between percipient and PPP performance and the differences in temporal effects, we conclude that it is doubtful that effective mathematical inference strategies, of the type described here, played any important role in the results of the first Training Study.

A PROBABILITY MODEL WITH PSI HAVING TWO COMPONENTS: USAGE AND ACCURACY

Edward W. Leyhe (Towson State University)

A probability model of psi performance is proposed in which a distinction is made between the usage of psi capabilities and the

accuracy of that usage. It is proposed that not only must we learn to use our psychic capabilities but we must also learn to use them accurately. This psi model makes four assumptions: a) psi exists; b) psi-directed responding and chance responding are mutually exclusive; c) psi accuracy is independent of psi usage and d) both psi usage and psi accuracy are initially undeveloped but are capable of being trained to a higher level.

The model takes the form

$$P(S) = \left(\frac{1}{1 + r}\right)P_c(S) + \left(\frac{r}{1 + r}\right)P_p(S)$$

where P(S) is the probability of overall success; $P_c(S)$ is chance, the probability of success when making a chance response; $P_p(S)$ is psi accuracy, the probability of success when making a psi directed response; and r is the ratio of psi usage to chance responding.

There are two immediate and obvious results predicted from this equation: a) if psi does not in fact exist, overall success equals chance, and b) if psi does exist and can be trained to perfection as to when it will occur and as to its success when it does occur, then failure can no longer occur. These results are, of course, trivial.

However, when psi accuracy equals chance, the model predicts that overall success will remain at chance level regardless of the level of psi usage! Furthermore, the model predicts that the level of overall performance depends upon the relationship of psi accuracy to chance. If psi accuracy is greater than chance, overall success is predicted to be greater than chance as long as psi usage is other than zero. Overall success is, as expected, predicted to increase as either psi usage or psi accuracy or both increases. However, if psi accuracy is less than chance, the model predicts that overall success is always below chance regardless of the amount of psi usage. In fact, if psi usage increases while psi accuracy remains below chance, overall performance is predicted to decrease, not increase. In everyday terms, this means that if you have a choice between two strategies and you choose to concentrate on the less successful strategy, your overall success will decrease.

The model also predicts that the degree of any statistically significant deviation of overall performance from chance is dependent upon the relationship between psi accuracy and chance. Thus, the larger psi accuracy as compared with chance, the greater the predicted significance that overall performance is above chance. Similarly, the smaller psi accuracy when compared with chance, the greater the predicted significance that overall performance is below chance.

In general, the level of overall performance and the level of its significance are dependent upon complex interactions amongst the levels of, and the rates of change in, psi usage, psi accuracy and chance.

The implications of this model's predictions are often both unexpected and opposite to expectation. These implications will compel a much greater attention to experimental procedures and to the analysis of results than have heretofore been applied.

The most striking implication of the present model is that a nonsignificant experimental result could occur even though a considerable amount of psi usage and psi accuracy or both could have been applied. Since a nonsignificant result could occur for a wide range of combinations of psi usage with psi accuracy, psi could have gone undetected in numerous experiments. However, the model does provide a clue as to how to improve the probability of detecting any psi occurrence.

The model predicts that significant results would be more likely to occur the more psi is used and the greater the difference between psi accuracy and chance. Since neither psi usage nor psi accuracy is under the direct control of the experimenter, the difference between psi accuracy and chance could be maximized by choosing extreme values of chance. For example, in free-response experiments, where chance is very low, psi accuracy might be expected to be greater than chance and above-chance performance might be expected. On the other hand, high-chance-expectancy experiments, such as with the Zener cards, would be predicted to lead to below-chance performance. This prediction leads to the next striking implication. Psi-missing may simply be an artifact of the general use of research conditions in which chance would be expected to be greater than psi accuracy, as, for example, during an unskilled psi performance by a "beginner," whose psychic talents are undeveloped.

Yet another striking implication of the model is that a performance decrease is often an expected consequence of repeated testing. This decrease can occur in a number of ways. It can occur most often during follow-up testing, or during the latter runs of successive blocks of trials, when unskilled performers or a high-chance expectancy are used for the experiment, i. e., whenever psi usage or psi accuracy is increasing with practice but psi accuracy remains smaller than chance. The decrease in this example is not a true psi decline, however, as it progresses from a less-than-chance performance to a still-lesser-than-chance performance. Thus, performance will decrease but significance will increase.

A performance decrease can also occur, according to the model, when both psi usage and psi accuracy remain constant but the experimental condition changes the chance expectancy from a value smaller than psi accuracy to a value larger than psi accuracy. Thus, if performance under the smaller-chance expectancy was significantly above chance, the performance under the larger-chance expectancy would be either at chance level or significantly below chance level. In either event, overall performance would increase but decline would appear to occur in terms of comparison with chance performance.

Again, performance decrease is predicted by the model whenever psi accuracy decreases from a value initially larger than chance to a value smaller than chance. This type of performance decrease might occur during the course of a long run or a number of runs in which fatigue or boredom might interfere with and reduce both psi usage and psi accuracy. If psi accuracy reduces to a point where it becomes smaller than chance, a position effect would be produced.

Still another way in which a performance decrease could occur, according to the model, is to reinforce one of the two psi components while extinguishing the other. This prediction points to another consequence of the model. The assumption of two psi components means that there would be two responses requiring feedback, not just one. The proposed model implies that the current research practice of providing only a single feedback for a successful response may be inadequate and may lead to erroneous conclusions. The necessity of providing feedback for two psi components compounds the existing problem of providing feedback. Currently, when feedback is presented following a successful response, it is unclear as to whether the feedback is reinforcing a successful psi behavior or a successful chance behavior. Still, it was believed that when psi did occur it would be reinforced as it was always 100 per cent accurate. With the present model, this is no longer true. A psi usage may result in a miss and not receive feedback.

Additionally, even if the researcher acknowledged both psi components and could reinforce each, there is still no assurance that overall performance would increase. For example, if both components are reinforced when psi accuracy is smaller than chance accuracy, and if psi usage increases more rapidly than psi accuracy, performance will decrease, exactly the opposite of the intended purpose.

During an experiment, the researcher has control of only two of the variables mentioned, the level of chance used during the experiment and whether or not feedback is presented. This limited control does not permit the researcher unambiguously to interpret the experimental results. There are a number of experimental conditions, however, that could reduce these ambiguities and permit testing the validity of the proposed model. If the model is validated, these same experimental conditions would define procedures for obtaining consistent psi performance for future studies.

The conditions that appear to offer the greatest chances to validate the model make use of the model's predictions that performance is significantly below chance when psi accuracy is less than chance and significantly above chance when psi accuracy is greater than chance. Thus, the percipient might be expected to perform significantly above chance when chance expectancy is selected to be very low, whereas the percipient might be expected to perform significantly below chance when chance expectancy is selected to be very high. Furthermore, if we assume that for short runs, feedback would increase psi accuracy but not affect psi usage, then, if

psi accuracy is initially less than chance, feedback should increase performance but decrease significance even though performance remains less than chance. Yet, if psi accuracy is initially greater than chance, feedback should again increase performance but this time significance should increase.

An examination of the model should stimulate other possibilities to test the model's validity.

A CLARIFICATION, AND POSSIBLE EXTENSION OF DR. GATLIN'S STOCHASTIC INFERENCE MODEL TO NONFEEDBACK CASES

Barbara Honegger (John F. Kennedy University)

> The subject is not actually calling individual targets, but is responding instead to the run or sequence of targets as a whole.
> --Gaither Pratt (JASPR, 1974, pp. 133-55)

I would like to comment on the stochastic inference model presented by Dr. Gatlin at the 1978 convention because I believe her work can potentially open whole new vistas of interpretation for parapsychology. First, I will make explicit some points left out of her initial presentation, and then extend the theory.

Gatlin has emphasized that we bias the interpretations of our experimental results in favor of paranormal explanations if we assume that any finite sequence of targets generated from a random source is, or can be, itself random. All finite sequences contain more or less structure, and therefore more or less information potentially detectable by subjects provided feedback. Gatlin's novel contribution is based on simple sampling theory applied to the run: she has pointed out that in those rare cases in which the target pattern in the initial portion ("estimation window") of target sequences is representative of the target pattern in the sequence as a whole, subjects receiving feedback may use information about the initial targets to generate informed strategies to guide their guessing of the remainder.

The point of Gatlin's model that needs clarification is that, given a verified random source, the number of target sequences found post hoc to have fixed-length "estimation windows" whose structure happens to encode information about the structure of the sequence as a whole should, on the null hypothesis, follow the normal distribution. Only a small number of sequences--those in the far right tail of the distribution--should therefore evidence part-whole isomorphisms that even enable a subject, given that he or she also applies a strategy such as Dr. Gatlin proposes, to score significantly above chance. Alternatively, those sequences in the

far-left tail of the distribution should mislead a subject, given that he or she applies the strategy, to "psi-miss." Whether a subject receives a positively significant Z-score under feedback conditions will thus depend on two factors: whether the subject receives target sequences having high part-whole isomorphism measures (r_{net} in Gatlin's system) and whether he or she is both neurologically equipped to and does take advantage, by employing the kind of stochastic inference strategy Gatlin proposes, of the real information that may be presented to the subject in this way by chance. (An argument is presented that Gatlin's model is capable of identifying instances of psi and distinguishing classes of psi for high-scoring subjects.)

Gatlin's model is directly testable using prestructured target sequences representing the full range of r_{net} values. Subjects who are led to believe, for the duration of their testing, that a generator is producing random sequences in real time could be presented with target sequences incorporating controlled degrees of part-whole isomorphism in randomized order. I will be performing such a test with Gatlin in the near future.

There is unfortunately no reliable way of determining whether chance structure/strategy or psi has been used by high-scoring subjects in experiments with feedback. This is not the case, however, for experiments without feedback where sensory cues are eliminated.

One of the most replicable results in parapsychology is the decline effect for guess sequences in tests without feedback. One way of thinking about the decline effect is that psi is accompanied by least noise for targets in the initial portions of runs. Given that this is true, if a subject's target sequence also happens to evidence significant part-whole isomorphism (high r_{net}), he or she may be receiving just the psi-mediated information necessary to apply Gatlin's strategy successfully. Under nonfeedback conditions, then, subjects may combine psi-mediated information about the initial portions of runs with guessing based on inference for the remainder of runs. (An argument is presented that Gatlin's model is as valuable for identifying instances of psi and for distinguishing classes of psi in high-scoring subjects in nonfeedback as in feedback conditions.)

Finally, I would like to point out something that may appear obvious to some--that a subject who receives extra-chance shares of target sequences having high r_{net} values exists, for whatever reason, in an environment more information-rich than most subjects. If we recall that some subjects continue to receive more than their fair share of sequences with strong part-whole isomorphisms, and that life itself presents individuals with similar series of chance events, we can extend the kind of argument we have been making here to understand the ability of rare individuals to predict their own and others' life experiences successfully. For those "lucky ones," in and out of the laboratory, the world holds a mirror to itself and contains far more structure, and therefore information, than we would normally like to imagine.

(Tables of preferred interpretations of results of forced-choice GESP experiments as a function of target sequence part-whole isomorphism measure, Z-score, evidence for application of inference strategy, and evidence for decline effect in the guess sequence for both feedback and nonfeedback conditions are provided.)

R_{NET} ANALYSIS

Lila Gatlin (University of California, Berkeley)

Let us first review the "Optimal Estimation Window" calculation summarized in Table 2 of Gatlin (1979). In the data of the Tart Training Study (1976), the single letters, doublets and triplets were simply counted for the first part of each of the 10 target sequences at estimation lengths varying from 25 to 250 trials in increments of 25. Then these counts were correlated with similar counts for the entire sequence and these correlation coefficients were called r_{net} values where n = 1, 2, 3 is the n-tuple length, e the estimation length of which there are ten and t the target sequence.

For fixed n and e, the 10 r_{net} values were correlated with the 10 Z-scores. There are 30 such correlations, 15 of which are significant at the $p < .05$ level. Furthermore, these significant correlations of r_{net} versus Z form a pattern of optimal estimation ranges or "windows." If the subject happens to estimate within these windows using only the information from sensory feedback at that point, there is a significant correlation between scoring and r_{net} value. This simple parameter thus appears to be a good predictor of score. Tart's (1979) criticism, with its heavy emphasis on the question of whether or not the subjects are actually predicting, completely ignores this r_{net} analysis.

Barbara Honegger has pointed out that, under the null hypothesis, r_{net} values should be randomly distributed across subjects and across runs for a given subject for a sufficiently large sample from a generator of very low bias. The observation of a significant clustering of high (or low) r_{net} values in a single subject or unexpectedly small group of subjects would be evidence of the "paranormal" in the sense that it would demonstrate the need for science to extend basic concepts. This could be either PK, the direct causal influence of mind over matter, or synchronicity of at least a conservatively paranormal character in the sense that some individuals tend to organize their environment such that the random null hypothesis of statistics does not apply to them and their local environment. r_{net} analysis might appear to be just another test for PK, but it is fundamentally different in that it does not require that the overall statistics of the source depart from random expectation. It examines only the distribution of whole-part isomorphisms of experimentally parsed sequences from a source.

I have made a diagrammatic representation of the correlation between r_{net} and Z-score. In region A, we have high Z-low r_{net} relative to the regression line and in region B, high r_{net}-low Z. Region A could be interpreted under a conservative paradigm as good strategy-poor sequence structure or under the psi paradigm as high ESP-low PK, whereas region B is the region of good structure-poor strategy or high PK-low ESP. In experiments with feedback, there is no way to decide between the psi paradigm and the strategy-structure paradigm.

Honegger, however, suggests that we look for significant correlations of r_{net} versus Z in experiments without feedback. Subjects could be fed prestructured sequences with a range of high to low r_{net} values to facilitate the possible correlations. If higher scores were significantly correlated with higher r_{net} values, this would imply that the subject had obtained a structured sequence and found precisely the strategy to exploit it without sensory feedback. This would be a very interesting observation.

The most intriguing feature of this approach is that it offers new experimental designs with the possibility of demonstrating the basic claim of parapsychology without the necessity of working with gifted subjects and without the necessity of worrying about the statistical randomness of the source; because the correlations, if they exist, may be readily demonstrable with nonsignificant Z-scores. The search would address only the underlying structure of r_{net} versus Z. This approach would also seem to carry promise of a degree of reproducibility that is lacking in the classical psi experiment, which always rests ultimately on obtaining a statistically significant number of hits, the "white crows" of R. Laurence Moore. r_{net} analysis can be carried out with black crows, which are not so difficult to obtain. This is a truly new experimental design for parapsychology.

PSI AND PERSONALITY*

OPENNESS VERSUS CLOSEDNESS AND THEIR RELATIONSHIP TO PSI

Lendell Braud Williams† (Psychophysical Research Laboratories, Princeton Forrestal Center) and Darrel Max Duke (California Institute of Professional Psychology)

The present study is a continuation of a series of studies reported at the 1975 and 1976 Parapsychological Association conventions (see RIP 1975, pp. 155-59, and RIP 1976, pp. 162-65). The purpose of these studies is to develop and norm an instrument designed to measure openness versus closedness and their relationship to psi. The term "openness" refers to both the opening up and taking in of experiences (input) and the opening up and letting out or self-disclosure of aspects of ourselves (output). An 88-item questionnaire was developed consisting of a number of scales and subscales related to these concepts. Briefly, there were three major scales: Openness to unusual experiences and altered states of consciousness (Scale I); Openness and tolerance of different attitudes, cultures, races, religions, nationalities, political and economic views, behavior and change and flexibility (Scale II); and Openness to aspects of the self, nondefensiveness and a willingness to disclose aspects of the self with relevant others (Scale III). A fourth scale, Introversion versus Extroversion, was added as a "control" scale. It was thought that extroversion was related to openness but that these were not identical concepts. A person could be quite extroverted and yet not be disclosing his or her true self nor revealing certain relevant material to others in an open, nondefensive manner.

The previous analysis included data from 101 black college students. The present study includes data on an additional 112 black college students and 31 white high school students.

The experimental procedure was the same as in our previous studies (see RIP 1976, p. 163). One week before the experiment, each subject was given the 88-item openness questionnaire and returned this questionnaire the day of the experiment. All subjects were run in a group classroom situation. Each subject had his or her own target concealed in two opaque envelopes. Therefore, the

*Chaired by Martin Johnson, University of Utrecht.

psi task was a clairvoyance task. Care was taken to make sure that there were no marks or cues made on any of the envelopes, and three to five experimenters monitored the room to make sure no subject opened his or her target pack. The lights were dimmed and a 45-minute psi-conducive tape was played. The tape included relaxation exercises, autogenic exercises, music, natural sound effects, meditation, imagery, mind-blanking and other "right hemisphere" activities.

The lights were turned up and the subjects recorded their impressions. After a brief time, the target pack was returned to the subject containing the target and three control pictures for ranking. Each subject ranked the pictures (1 to 4) on their correspondence to his or her impressions. The subjects also ranked each picture on the degree they liked the picture as a picture.

Significant psi-hitting was demonstrated on the clairvoyance test in the original study ($N = 101$, $p \leq .007$) and in the total sample ($N = 244$, $p \leq .04$). Mann-Whitney U scores were calculated for each scale and subscale comparing psi-hitters with psi-missers and direct hitters with direct missers. For the total black sample ($N = 211$), all of the probabilities are in the predicted direction (psi-hitters and direct hitters being "more open"). However, none of the scales for hitters versus missers were significant. The results for direct hitters versus direct missers were better. Several analyses, such as the total questionnaire ($p < .10$), Scale I (altered states, $p < .05$) and Scale III (openness to aspects of self and willingness to disclose aspects of self to others, $p < .09$) were all promising. The data from the white high school sample were very disappointing. The only interesting finding was the fact that seven out of eight questions regarding openness to sexual attitudes, sexual behavior and discussion of sexual issues and problems discriminated good psi subjects from poor psi subjects. Sexual issues should be of particular concern to high school students.

The items were analyzed to determine which items discriminated best. The means for psi-hitters and psi-missers were calculated for each item and the difference obtained. The means for direct hitters and direct missers were calculated for each item and the difference obtained. These two differences were combined to yield a composite mean difference. These composite mean differences were ranked to determine the best 40 to 50 items on the questionnaire. The data for the total black sample ($N = 211$) indicated that 63 per cent of the items discriminated psi-hitters from psi-missers in the predicted direction, while 64 per cent of the items discriminated direct hitters from direct missers. The difference scores were combined to yield a composite mean difference and in this calculation, 65 per cent of the items discriminated good from poor psi subjects. The best items for the original black sample ($N = 100$) were compared with the best items for second black sample ($N = 111$). Sixty-four per cent of the original 50 best items were also found in the top 50 items for the second sample.

Therefore, there appears to be consistency in specific ques-

tions that continue to discriminate good versus poor psi performance in the black sample. The questionnaire needs to be normed on at least 100 white college students before we can determine which questions are best for white subjects and if these are consistent with our black sample.

One of the most important findings is that the best items are well distributed among all the scales. Among the best 40 items, 23 per cent were altered states, 20 per cent were from the tolerance scale, 25 per cent from the openness (III-A), 28 per cent from the Jourard scale (III-B) and only 5 per cent from the introversion-extroversion control scale. Therefore, good items appear on all the major scales. This indicates that our original decision to define openness in a broad way was perhaps wise.

OPENNESS, CLOSEDNESS AND PSI

Jeffrey Bellis and Robert Morris (University of California, Irvine)

This study was done in an attempt to gain further information regarding the apparent relationship between openness and psi. The instrument used for determining openness was an 88-item questionnaire. Three sheep-goat questions were added to the questionnaire on an additional page. The three questions dealt with general belief in ESP, personal psychic experience and prediction of performance on the ESP test given in this study.

The 23 volunteer subjects were all undergraduates enrolled in R. M. 's Introduction to Parapsychology course at the University of California, Irvine. The targets in the psi test were selected from a set of 100 thematically simple, full-page color photographs from National Geographic. The photos were numbered and ordered from one to 100 in such a way that the primary content of each of the photos was different in every set of four consecutively numbered photos. All subjects were assigned a separate, nonoverlapping target pool of four thematically distinct photos.

R. M. prepared the target pools of four photos and randomly selected one of the four photos in each pool to be the actual target. The selection of target was done at least several hours before each session and made use of an order of random digits generated several weeks in advance of the study. The target and target pools were left for J. B. in a cabinet. The photos were in folders numbered from one to four for each pool, with target photo folder on top. Upon arrival at the Social Science Laboratory, each subject was escorted by J. B. to a small lab room (Room 1) and seated at a table. J. B. gave a brief explanation of the study and the subject was given an envelope containing the questionnaire. J. B. then left the room; when the questionnaire had been completed, the subject was then escorted to another room (Room 3), shown a small, low

table, and told that the target photo would be placed face down on this table. The subject was then escorted back to Room 1 and seated in a comfortable chair. J. B. left the subject alone with instructions to relax as much as possible, for a four-minute imagery period.

J. B. returned to Room 3, removed the folder containing the target photo from the top of the stack and slid the photo from the folder down onto the table. When four minutes had passed, J. B. slid all four photos in consecutive order into a single folder marked "judging." J. B. neither saw nor touched any of the photos. A rating sheet was included in the folder, which was then slid under the door of Room 1 to the subject. Subjects rated relative correspondence between their imagery and each photo, the relative attractiveness of each photo, and their coherency of imagery. When this was completed, J. B. entered, collected the subject's materials and notified the subject of the number of the target photo. This concluded the session.

The overall results of the ESP test were insignificantly below chance. Scale I (openness to altered states of consciousness and unusual experiences) appeared to have a nonsignificant positive relationship to psi-hitting.

Neither Scale II nor any of its subscales (openness to, and tolerance of, the following (each a subscale): behavior, sexual practices, economic and political views, human nature, race and nationality and change and flexibility) had any relationships to psi-scoring.

Psi-hitting appeared to be related to nondefensiveness ($r = .3041$, $p = .079$). When the psi-scoring variable was broken down into four categories (low to high) and an analysis of variance was performed between this variable and the nondefensiveness subscale, the correlation was significant (eta = .6348, $p = .0182$). There was a positive but nonsignificant correlation between openness to personal disclosure and psi-scoring. When the two subscales were combined to form Scale III, the correlation was not significant, but still was rather suggestive of a relationship ($r = .2897$, $p = .09$).

The fourth scale, introversion versus extroversion, proved to be the scale most strongly correlated with psi-scoring, with extroverts scoring higher ($r = .4666$, $p = .012$). When an analysis was done between the broken-down psi-scoring variable, correlation was strong (eta = .4391, $p = .0361$).

The highest single item correlation with psi-scoring was the item "While in trains, buses, etc., I often talk to strangers." This comes from the introversion versus extroversion scale. Out of all 88 items, 59 (67 per cent) showed correlations in the predicted direction ($Z = 3.09$, $p < .005$, two-tailed). Sheep-goat questions appeared not strongly related to psi.

The data in this study seem to indicate that there is a weak but positive relationship between psi and openness.

PERSONALITY CHARACTERISTICS OF SHEEP AND GOATS

Michael A. Thalbourne (University of Edinburgh) and Erlendur Haraldsson (University of Iceland)

A great deal of parapsychological research has been carried out to investigate the relationship between ESP and the so-called "sheep-goat" variable, that is, belief in the existence of ESP, either in the abstract or with respect to one's own psychic ability. The general consensus of opinion seems to be that there is indeed evidence for the supposed positive relationship between belief in ESP and ESP test performance. However, very little research has been directed toward the question of possible personality differences between sheep and goats.

Such research would be valuable for a number of reasons. First, it may shed light on the purely psychological factors involved in being a sheep or a goat. Second, and more importantly, if an individual's belief in ESP is related to his or her personality, then this may imply a serious confounding of variables in experimental situations, and may necessitate a re-evaluation of studies of the sheep-goat effect. But third, and more optimistically, findings of personality differences may provide a crossroad for the two relatively independent strands of research represented on the one hand by personality and on the other hand by the sheep-goat variable. For example, there is evidence that ESP performance is related to extroversion. Given that a) extroverts tend to score higher than introverts on tests of ESP, and that b) sheep tend to score higher than goats, one would therefore expect that sheep are more extroverted than goats. If such were found to be the case, then a promising experiment would be a comparison of the ESP performance of extroverted sheep and introverted goats, with the prediction of a more marked and stable difference between the two groups. Furthermore, knowledge of the personality correlates of belief in ESP would prove valuable as a guide to explicating and theorizing about the "dynamics" of the sheep-goat variable and its effect on ESP, and would thereby bring us closer to the actual psychological dimensions relevant to ESP performance. This paper brings together the work of two parapsychologists as a contribution to this research. The data consist of four experiments--two carried out by M. T. (Section I), and two by E. H. (Section II).

Section I. The instruments used by M. T. and by E. H. to classify subjects as sheep or goats are each examples of the "scalar" approach. The scale used by M. T. was formed from the responses to 10 multiple-choice items covering a range of belief issues (belief in the existence of ESP, in life after death, contact with spirits, and belief that one is psychic), and a number of personal-experience items (belief that one has experienced a veridical hunch or premonition, a precognitive dream, a paranormal vision, telepathy with another person and general experience of ESP). This scale has

shown some promise as a predictor of GESP performance in a free-response experiment, where correlations as high as +0.50 (n = 31) have been found (unpublished data). The individual items are presented in the form of statements to be rated by the subject as being true, uncertain or false of him- or herself. They are scored such that "true" is given two points, "uncertain" one, and "false" zero points; scores are summed over the 10 items to give a total ranging from 0 to 20.

Six personality scales taken from three established questionnaire tests were administered. From the Cattell 16PF were taken Factor A (reserved versus outgoing), Factor H (shy versus venturesome), and Factor Q_2 (group-dependent versus self-sufficient). Form A of the 1967/68 revision was used, together with the college-student norms for subjects 25 years of age or less, the adult norms for older subjects. From the California Test of Personality were taken two scales, viz., Freedom from Withdrawing Tendencies, and Social Skills. Finally, Drake's Social Introversion Scale was used, which is derived from the MMPI.

In the first experiment, a total of 62 persons (28 males, 34 females) took part, most of them students at the University of Adelaide, South Australia, the median age being 20. It was planned, both in this experiment and in the other three studies to be described below, to designate the bottom 33 1/3 per cent of Sheep-Goat Scale scorers as goats, and the upper 33 1/3 per cent as sheep, eliminating from analysis the middle 33 1/3 per cent (whose sheep-goat status is rather indeterminate). However, a perfect "three-way split" was not always possible, owing either to too small an initial sample or to a restricted range of possible Sheep-Goat Scale scores.

In this first experiment, 23 subjects scored below eight on the Scale and were designated goats, and the 28 subjects scoring 10 or more constituted the sheep. T-tests with 49 df and two-tailed alpha at five per cent revealed that compared with sheep, the goats tended to be more reserved (Factor A: mean sten-score for sheep = 5.29, for goats 4.26, $t = 2.01$, $p = .049$) and more socially introverted (mean T-score for sheep = 53.64, for goats 59.96, $t = 2.14$, $p = .038$). Rao's stepwise discriminant analysis indicated that the most important of the six variables for discriminating between sheep and goats were, in order, Social Introversion, Freedom from Withdrawing Tendencies and Factor A. The canonical correlation for the discriminant function was 0.504, and successfully reclassified 71 per cent of the 51 subjects as either sheep or goats.

The second experiment contained 235 subjects (86 males, 148 females), all of them First-Year Psychology students at Adelaide University, median age being 18. A total of 78 subjects scored six or below on the Sheep-Goat Scale, and were designated goats. Those 85 subjects scoring 12 or above were allocated to the sheep group. The results of t-tests with 161 df showed that compared with sheep, the goats were more shy (Factor H: mean sten-score for sheep = 6.00, for goats 4.99, $t = 3.30$, $p = .001$), more introverted (sheep

mean = 52.85, goat mean = 56.99, t = 2.72, p = .007) and more lacking in social skills (mean percentile-rank for sheep = 45.22, for goats 34.42, t = 2.84, p = .005). Five of the six variables entered Rao's stepwise discriminant analysis, the order being Factor H, Freedom from Withdrawing Tendencies, Factor A, Social Introversion and Social Skills. The canonical correlation of 0.331 was fair, and the constructed discriminant function correctly reclassified 62 per cent of the 163 subjects.

The values of omega-squared--a statistic that reflects the amount of association between the dependent and independent variables --were in both experiments typically low (that is, in the range of four to seven per cent), indicating rather weak relationships between the sheep-goat and personality variables. This leads one to expect difficulty in predicting accurately an individual's sheep-goat status simply on the basis of his or her personality scores. Nevertheless, prediction from the initial sample of 51 subjects to the later sample of 163 was attempted using classification equations yielded by discriminant analysis on the former sample. However, the predictive power of the equations was very slight, correctly classifying as sheep or goats only 56 per cent of the sample (corrected chi-square = 1.51, 1 df, p = .22).

Section II. The Sheep-Goat Scale used by E. H. was formed from the responses to three multiple-choice items, concerning the reading of literature on psychic phenomena, as well as belief in telepathy, clairvoyance and precognition. There are four possible responses to each item: the reply least favoring the paranormal hypothesis is given a score of one, successively increasing to four points for the most favorable reply. The Scale thus ranges from three to 12.

The personality test instrument was the Cattell 16PF, Form C, translated into Icelandic and standardized for the native population. The test is a set of 16 factor-analytically derived scales, each of which is said to measure a different "primary source trait."

In the first experiment, a total of 75 subjects took part, all of them male first-year students at the University of Iceland, Reykjavik. Mean age was approximately 22 years. Twenty-six subjects whose Sheep-Goat Scale scores were less than eight became the goat group, and the sheep group comprised the 31 subjects scoring above eight. T-tests with 55 df revealed that, compared with sheep, goats were more experimenting (Factor Q_1: mean sten-score for sheep = 5.23, for goats 7.15, t = 3.67, p = .001), more sober-minded (Factor F: sheep mean = 6.58, goat mean = 5.15, t = 2.62, p = .011) and more reserved (Factor A: sheep mean = 6.48, goat mean = 5.00, t = 2.51, p = .015). Rao's stepwise discriminant analysis indicated that the most important sheep-goat discriminators were, in order, Factors Q_1, F, Q_2, Q_3, H and A. A large degree of discriminating power exists in these six variables, as shown by the high canonical correlation of 0.62 and the successful reclassification of 79 per cent of the subjects. The values of omega-squared indicate

that the sheep-goat variable was associated with about nine per cent of the variance in Factors F and A, and a mildly impressive 18 per cent of that in Factor Q_1.

The second experiment contained 180 subjects, the sample characteristics being very similar to those of the first sample except that 56 subjects were females. Sixty-four subjects scoring above eight on the Sheep-Goat Scale were grouped as sheep, 78 below eight being designated goats. T-tests with 140 df showed that, compared with sheep, goats were more reserved (Factor A: sheep mean = 6.36, goat mean = 5.15, t = 3.45, p = .001), more self-assured (Factor 0: sheep mean = 6.03, goat mean = 5.17, t = 2.45, p = .015), more trusting (Factor L: sheep averaging 5.31, goats 4.56, t = 2.22, p = .028), more emotionally stable (Factor C: sheep averaging 4.91, goats 5.17, t = 2.04, p = .043) and more experimenting (Factor Q_1: the sheep mean being 5.72, the goat mean 6.49, t = 2.00, p = .047). However, the values of omega-squared are quite low (ranging from two to seven per cent), indicating that not very much of the variance in the personality variables was related to the sheep-goat variable. Seven of the 16 factors entered Rao's stepwise discriminant analysis, the order being Factors A, C, G, L, O. F and E. The canonical correlation of 0.451 was modest, and the discriminant function correctly reclassified 68 per cent of the 142 subjects.

Prediction from the first sample to the second was attempted using the classification functions derived from the former. The predictive power of the equations was not much better than chance, correctly predicting the group-membership of 54 per cent of the subjects but incorrectly classifying 46 per cent (corrected chi-square = .391, 1 df, p = .532).

Each of the two sets of studies seems to have demonstrated the existence of certain relationships between personality and the sheep-goat variable. In M. T.'s work, the Social Introversion Scale has given consistently significant sheep-goat differences, as have Factors A and Q_1 in E. H.'s research. It is also promising that even though the authors have used such very different sheep-goat scales (the Icelandic one emphasizing belief, the Australian one experience), Factor A has yielded sheep-goat differences on three of the four occasions of its administration.

At the same time, it must be conceded that the magnitude of these effects is by no means large. The sheep-goat variable typically was related to a mere seven per cent of the variance in the personality measures. This is one reason why, in both studies, prediction of sheep-goat status in a new sample proved relatively unsuccessful. Another reason is that the variables entering the discriminant functions sometimes differed quite markedly between the initial and follow-up study (particularly in the case of E. H.'s work), which suggests some disparity regarding the personality-constellations designated as being relevant to the sheep-goat variable. Personality thus is not a very strong predictor of whether a person is a sheep or a goat.

Nevertheless, it would still be true to say that if such personality-differences had been found in the context of an actual sheep-goat ESP experiment, they would to some extent have represented confounding variables. Sheep-goat researchers (or at least those using similar scalar measures of this variable) might therefore be advised to take these personality characteristics into account when planning their experiments and when interpreting their findings. It would also be very valuable to investigate the personality-correlates of sheep and goats as defined by other classification schemes.

Finally, is it possible to derive from our own research any general conclusion as to what are the personality correlates of the sheep-goat variable? Our opinion is that one would not go very far wrong with the suggestion that goats tend to be slightly more introverted than sheep, on average. There is also evidence that goats tend toward free-thinking intellectual skepticism and sheep toward religious and political conservatism (as shown by the consistent differences on Factor Q_1 in E. H. 's data). However, this latter effect may be a consequence of the difference on extroversion, since Eysenck (JSPR, 1967, p. 63) has claimed that "introverts tend to be characterized by scientific, cautious and doubting tendencies which would seem to predispose them to become 'goats,' while extroverts are more characterized by artistic tendencies and to be less resistant to such group pressures as often exist (or may be imagined to exist) in ESP experimental circles." Thus, extroversion may be the principal dimension underlying the sheep-goat differentiation. Intuitively, it seems plausible to suppose that people who are socially withdrawn and have no great desire for interpersonal intimacy, would also feel uncomfortable and threatened by the notion of paranormal acquisition and communication of information, which might represent a potential intrusion into the privacy of their lives.

A MASS ESP CONTEST: DEMOGRAPHIC DATA AND STIMULUS-RESPONSE BIAS

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

The editors of Games, a popular magazine, decided to publish an ESP contest. They telephoned various individuals who might advise them, and eventually me. I made a few suggestions about what not to do, and soon found myself acting as an unpaid consultant. In return, they agreed to include with their contest some of the questions I thought might yield interesting data, and to let me examine the contest results.

The contest was to consist of 25 ESP calls on a closed pack of ESP cards. A large response was expected. This therefore seemed an excellent opportunity to study birth order. Psychological research has repeatedly found that with a large sample, there are small but highly significant advantages for firstborns over later-borns

in intelligence-test scores, school grades, rate of college graduation, rate of volunteering as research subjects and so on. The classical explanation is that in the typical family, firstborns during their early formative years are rewarded for conforming to the rules laid down by their authorities, their parents. They therefore learn to accept authoritative restrictions and find satisfaction in complying. Later-borns in these typical families find the firstborn entrenched as the one who already knows the rules and can best follow them. Later-borns' attempts to comply are less likely to be praised and rewarded; later-borns are therefore more likely to gain satisfaction by being independent or rebellious. Therefore, restrictive situations, like school, standard intelligence tests, or the Games ESP test, should elicit more acceptance from firstborns and more resentment from later-borns. Since many families are atypical, only large samples are likely to show this tendency. One hypothesis to be tested was therefore that firstborns would have higher ESP scores than later-borns; and a question on birth order was included.

A second hypothesis was the familiar one that sheep would score higher than goats, and questions on attitude were included.

The contest also offered an opportunity to make various exploratory analyses: for age, for sex, for complete versus incomplete responses and for position effects with an unusual target layout of the cards. No formal hypothesis was stated for these, but my expectation was that age and sex would not relate to ESP score, that subjects who left out some responses would score higher than those who punctiliously answered every question, and that the visually distinctive positions of the cards would show higher scores.

Examination for position effects yielded an unexpected result. ESP scores were much lower for the star and waves than for the circle, plus and square (chi-square = 35.02, 4 df, $p < .000001$). Since the five-pointed star and three vertical wavy lines shown in the magazine were much harder to draw than the other symbols, I stated a third hypothesis: subjects who used a word to designate star and wave would have a less pronounced difference between scores on these and the other targets than would other subjects.

Of the 4,543 responses to the contest, 4,493 made 25 ESP calls and were fully analyzed. Of these, 1,992 reported they were males, and 2,414 reported they were females. Ages ranged from preschool to the 70s.

Games employed an accountant to randomize the targets. He shuffled a pack of ESP cards repeatedly, cut it repeatedly and recorded the card order.

Games offered an unspecified prize to all who had ten or more responses correct, and stated in the instructions that those who did not believe in their own ability were expected to score lower than those who believed in it.

Greville's formula corrected for the stacking effect. To test the third hypothesis, records that designated waves with two words or with all symbols, or with mixed words and symbols, or with typed responses were all classed as "other."

The hypothesis about birth order was significantly confirmed: ESP scores of firstborns were higher than the scores of later-borns ($Z = 2.12$; $p < .05$).

The data were inconsistent with the sheep-goat hypothesis. Sheep ESP scores were (insignificantly) lower than chance expectation; goat scores were (insignificantly) higher; the difference between them was not significant. Perhaps the first question to be raised in discussing this was why there were any goat responses under the test conditions. The money value of Games's prizes is small; the likelihood of chance success is very low; utility considerations should have guided those who thought there was no possibility of ESP success not to spend their time, or even the money for a postage stamp, on submitting entries. Nevertheless, 226 goats entered the contest. Might they have been provoked by the article's statement that their scores were expected to be low? Did they hope to prove this statement wrong, and therefore hope for high scores? It would be interesting to check out the possibility of such a double negative effect by research where a random sample of half the goats were told that their making low scores would support the thesis that ESP occurs, and the other half were given the usual noncommittal instructions that imply that high scores support the ESP thesis.

Of the vaguer expectations, not stated as formal hypotheses, four were supported by the data; one was not. As anticipated, neither age nor sex showed a significant relation to ESP scores. However, there was a strong trend in the expected direction for incomplete responding: records with omissions in the questionnaire were suggestively higher in their ESP scores than records with completed questionnaires ($Z = 1.91$, $p < .06$). Against expectation, no relation appeared between conspicuous positions in the layout of the ESP cards and ESP scores.

The third hypothesis examined the possibility that low ESP scores on the star and wave, compared with the other responses, were due to the extra nuisance of drawing the elaborate responses of a five-pointed star and three wavy vertical lines. Its statement was: subjects who used a word to designate star and wave would have a less pronounced difference between scores on these and other symbols than would other subjects. To test it, my first step was to separate out the responses that consisted of words.

Difficulties arose. For example, some responses were mixed words and symbols. Perhaps improperly, I left those with the "other subjects." Many with all words used two words for waves (e.g., "wavy lines"); these were segregated but for test purposes were also classed with "other subjects." For these records, it was of course expected that the two-word response would have a relatively

low hit rate, and that the difference in success between waves and other targets would be more pronounced than for subjects who used one word for waves.

Subjects who wrote words and used one word for waves showed a slight trend in the hypothesized direction. Their 315 records showed even lower scores for plus than for star or waves, and there were no significant differences in success among their responses. The difference in success between star and wave, and the three other responses, however, was not significantly different from the corresponding difference for all other subjects. Thus, the hypothesis was not confirmed. (I wish now that I had proposed a cleaner test: comparing the difference for these subjects with the difference for subjects who drew the five targets as they were shown.)

The expectation about the 375 subjects with two-word response to waves was well supported. Scores on wave responses were significantly lower than their other scores (chi-square = 4.56, 1 df, $p < .05$). The difference between scores on the other four targets and on waves was suggestively greater than the corresponding difference for the subjects who used one word for waves (CR = 1.94, $p < .06$).

Meanwhile, another oddity appeared in the data. Uncorrected scores of the subjects with two words for waves showed a rather high positive deviation (+75 for 9,360 calls); subjects with one word for waves had a rather low negative deviation (-72.8 for 7,874 calls) and the difference is significant (chi-square = 7.96, 1 df, $p < .01$) as is the difference between overall uncorrected scores for subjects with two words for waves and all other subjects (chi-square = 6.84, 1 df, $p < .01$). I do not have even a tentative interpretation for why those scores should be higher than the others.

PSI AND SOCIAL INTERACTIONS*

THE POLTERGEIST AND FAMILY DYNAMICS: A REPORT ON A RECENT INVESTIGATION

D. Scott Rogo (Northridge, California)

In the past, most poltergeist investigators have tried to locate specific "poltergeist agents" in the families and homes they have studied. In most cases, it appears as though poltergeist agents are adolescents who are harboring deeply embedded hostility and aggression, which are being repressed from conscious expression. It therefore looks as though RSPK outbreaks might be a psi-mediated method used by these agents to vent their pent-up feelings. Few parapsychologists, though, have ever looked at the families of these agents to see what kind of personality dysfunctions might also be affecting persons who have meaningful relationships with the presumed agent. As I have suggested elsewhere, (JSPR, 1974, pp. 433-47) a poltergeist might be a family problem more than an individual one. In this report, an evaluation was made of a poltergeist case, reported from Los Angeles in 1978-79, in which the author was able to make a detailed psychological analysis of all the family members involved. It was hypothesized in advance that if each family member were examined, all of them would show personality profiles that we normally associate only with specific poltergeist agents.

The subjects of this investigation consisted of Mr. and Mrs. Harry Dell (pseudonym) and Mrs. Dell's 16-year-old daughter by a previous marriage, and an unwed mother. The poltergeist had begun some two years before, after one of Mrs. Dell's sons (who was no longer living at home) had suffered an acute psychotic episode. The family had witnessed footsteps in the house, lights flicking on and off, ghost-like shadows, movements of objects and so on. At least one neighbor also witnessed some of these events. The PK, which often resembled that which occurs in more conventional "haunted houses" than in typical poltergeist cases, most commonly activated at times of interpersonal stress within the family.

Each family member was given the Rosenzweig Picture-Frustration Test, a version of the House-Tree-Person Test, and Rotter's

*Chaired by Rhea A. White, Reference Department, East Meadow Public Library.

Incomplete Sentence Blank. These were then sent to Gertrude Schmeidler for evaluation. Schmeidler knew, of course, that she was dealing with a poltergeist case, but did not know who the "agent" was supposed to be, nor that a specific hypothesis about poltergeist dynamics was being tested. As predicted, various personality characteristics that we normally associate with poltergeist "agents" showed up in the evaluation for each of the three participants. According to Schmeidler, all showed an "astonishingly large number of similar themes," including "agression directed against others... as opposed to the self-punishing or the mute, minimized kind." The Dells' 16-year-old daughter was described as showing an inability to handle even minor frustration coupled with an incapability to enter into normal relationships, while Mrs. Dell was described as a woman inwardly discontent with her marital and personal relationships and vainly trying to repress those problems. Schmeidler also described her as having "closed up the vent to her problems, she tries not to let any of it come out." Similarly, Mr. Dell seemed to possess an extraordinarily high level of aggression, stemming from feelings of personal insecurity. Basically possessing an extrapunitive personality, he harbored, Schmeidler felt, a potential for physical violence, which had been kept in check by his reliance on repression.

As can be seen, this entire family, if looked upon as a gestalt, seems to possess a group dynamic similar to those dynamics seen within the personalities of poltergeist foci. It is perhaps significant, in this regard, that the poltergeist came to an end when Mr. and Mrs. Dell decided to separate.

This case tends to support the view that poltergeists are sometimes reactions to a total family situation and that it might be unwise to conceptualize a RSPK outbreak as necessarily a psychic eruption revolving around a central agent. Just as W. G. Roll, Hans Bender and others have begun to isolate those personality factors typical of poltergeist agents, it might also be possible to isolate those factors typical of "poltergeist families." This does not mean that there is no such thing as a poltergeist agent. But it does indicate that some poltergeists might be a joint product of an entire family, or that personalities of various family members may have a role just as crucial in the ontogenesis of a poltergeist outbreak as that of the presumed agent.

It is also interesting to note that the phenomena reported in this case tend to resemble a haunting more than a classic poltergeist. It is therefore possible that family poltergeists might have paranormal dynamics different from the classic "adolescent poltergeist." Phrased from a different standpoint, perhaps in the future it would be wise if researchers be aware of the possibility that some hauntings, like poltergeists, might be psychic products generated by the witnesses themselves.

PSI AND VOLITIONAL ACTIVITY: AN EXPERIMENTAL STUDY OF PSI IN A COMPETITIVE SITUATION

K. Ramakrishna Rao, † H. Kanthamani and Shanti R. Krishna (Institute for Parapsychology, FRNM)

The research that is reported here is an attempt to understand the resultant psi effect in the face of opposing volitional activity. A number of examples of the latter can be drawn from life situations, such as gambling tables and horse races, in which the participants are interested in different outcomes. If two people have opposite stakes in the outcome of a psi task, does their involvement in that task cancel each other's effort or enhance the effect? In a competitive situation, in which the participants are working for opposite outcomes--for example, each attempting to obtain more hits than the other in an ESP test--they could realize their intention by registering a high score on their own part or a low score on the part of the opponents. There may be circumstances that promote one or another of the outcomes. The present study consists of three series of experiments designed to identify the modus operandi of psi in a test where two individuals actively engage in a competitive role of guessing concealed targets under conditions in which their goal-relevant targets are sometimes the same and at other times different.

In each of the three series, two subjects, one of them being the experimenter (K. R. R.), attempted to guess ESP symbols recorded by and kept with a staff member of the Institute for Parapsychology (M. M.). Before the beginning of each of the series, M. M. generated targets for the whole series, utilizing a standard computer program available at the Institute for Parapsychology. The program generated five runs of 25 trials (125 trials total) per sheet, printed the target sheet and also stored the targets for later computer analysis.

The number of sessions was preset at 20 in each series with five runs by each of the participants in every session. In the first and third series, this goal was reached. In the second series, however, the subject K. K. had to leave Durham somewhat unexpectedly after completing only nine sessions.

The experiment was carried out in an atmosphere of competition that was sometimes rather intense. Since K. R. R. felt strongly that the psychological set of competitiveness was essential for the success of this experiment, he attempted, on the one hand, to motivate the subject by challenging her to defeat him and told himself, on the other hand, that he should come out as the winner.

K. R. R. gave a numbered record sheet to the other participant and took one himself for recording the guesses. As soon as both the participants completed the recording of their guesses, K. R. R. (or sometimes S. K. or H. K.) took both the call sheets,

made Xerox copies of them, gave the copies to M. M. and collected from M. M. target sheets marked separately for the two participants. Then K. R. R. and the participating subject checked their calls with the appropriate target sheet and circled the hits. After the completion of the experiment, the subjects' responses were typed into the computer and compared with the target file stored in the computer. The computer printed out the scores of the subjects for each session, and these were then compared with the results of hand-scoring by the subjects so as to correct any discrepancies found between these scores.

The main focus of the study was to observe the manner in which psi effects manifest when two persons involved in an ESP test actively pursue two opposing goals and when the targets of their pursuit are sometimes the same and sometimes different. It was expected from the beginning that whatever psi effects might be present in the data would be more likely to appear in the same-target condition than in the different-target condition.

Since no consistency of results over 20 sessions was expected, it was planned to analyze the data by obtaining the critical ratios for the difference in the scores of the two participants in each session and to square them to obtain chi-squares. The chi-squares for all the sessions in each condition were added to obtain the probability value, separately for each session. Since the subjects in the same-target condition guessed the same targets, it would be possible for them to obtain artifactually similar or different scores to the extent that their response patterns were similar or different. To avoid such a possibility, the calls of the two participants in each session were compared and the necessary corrections in computing the CR's were made to eliminate the effect of call patterns.

In the first series, the combined chi-square for all the 10 sessions where both the subjects guessed the same targets is 26.345. With 10 degrees of freedom, it has an associated p value of $< .005$. The combined chi-square for the different-target condition is 7.750, 10 df, and is insignificant.

The second series consisted of only nine sessions. Five of these were in the same-target condition and four in the different-target condition. The combined chi-square for the same-target condition is 11.566, 5 df, $p < .05$; and for the different-target condition, it is 4.925, 4 df, which is insignificant.

In the final series, the combined chi-square for the differences in the subjects' scores in the same-target condition is 10.653, 10 df. It is 4.075, 10 df, in the different-target condition. Neither of these chi-squares is significant, though the former is considerably larger than the latter.

If we combine the chi-squares of all three series, the sum of the chi-squares for the same-target condition is 48.564, 25 df, $p < .005$. For the different-target condition, however, we obtain an insignificant chi-square of 16.75, 24 df.

An F ratio between the mean chi-square of the same- and different-target conditions was computed to compare the results in both conditions. The mean chi-square per session in the same- and different-target conditions is 1.943 and .698, respectively. The F ratio is 2.784, $p < .01$, which clearly suggests that the subjects did differently in the same- and different-target conditions.

ESP AND THE DEFENSE MECHANISM TEST (DMT): A CASE OF EXPERIMENTER EFFECT?

Erlendur Haraldsson† (University of Iceland) and Martin Johnson (University of Utrecht)

In the present-day concept of psi, there is an inherent assumption of a psychological mechanism of distortion or defense of some kind at work in psi-missing. For theoretical reasons, research into the relationship between psi and the Defense Mechanism Test (DMT) has therefore been of considerable importance. So far, results of six experiments have been published and all of them show a significant relationship between ESP and the DMT. These results reveal a rather high degree of repeatability. In this case, we know with certainty of all studies that have been conducted, since only a handful of psychologists know how to evaluate the DMT.

The DMT is a projective test. TAT-like pictures are presented tachistoscopically at exposure times varying from 10 to 2,000 milliseconds. The stimuli are utilized to measure what has sometimes been referred to as "perceptual defense," in Kragh's terminology, "preconscious defensive organization."

Forty-one male students participated in the experiment. This consisted of two sessions, one for DMT in which about six persons were tested at a time, and one for ESP. Eight students of psychology served as experimenters in the ESP part of the experiment.

The ESP test consisted of 80 trials ($p = .25$), 40 of clairvoyance (using an electronic testing machine) and 40 of precognition (computer-generated targets).

Most of the planning and preparation of the experiment was done by E. H. and M. J. carried out the DMT testing. Many of the subjects wrote their protocols in English. Protocols in Icelandic were verbally translated by E. H. after the DMT sessions and during M. J.'s stay in Iceland.

Rating and final evaluation of the DMT were carried out by M. J. in Utrecht. The ESP testing took place after he had left Iceland.

On a predetermined date after all ESP sessions were over,

E. H. mailed the ESP data to Utrecht, and M. J. mailed the DMT data. At the same time, Sybo Schouten generated and mailed the precognition targets to Reykjavik. Thus, an independent treatment of the ESP and DMT data was ensured.

When the total ESP scores (for clairvoyance as well as precognition) were correlated with the DMT ratings, we obtained a nonsignificant correlation coefficient of $r_s = .02$. For clairvoyance, the correlation was .07, and for precognition, .00.

Looking at the ESP scores, we find for clairvoyance a mean of 9.10 (MCE = 10.00), which is significantly below chance (CR = 2.11), whereas the mean for precognition is 9.98, or close to mean chance expectation.

At the University of Iceland, we have conducted three experiments testing the ESP-DMT relationship. These experiments have by and large followed the same design, with only slight changes between experiments. In the first experiment, we obtained a correlation of $r_s = .44$, in the second experiment an insignificant correlation of .17 and we are now down to .02.

The history of ESP research with the DMT is interesting. The first experiment yielded a high correlation of .79, but then decreased with every experiment until it reached .26 in the fifth experiment. In the first Icelandic experiment, the correlation went up again to .46, to drop in this third experiment to the lowest so far, .02.

It is of interest that a negative correlation has not been obtained in any of the eight experiments conducted so far, and only two have not been significant. This indicates a striking degree of repeatability in parapsychology. But the decline of correlation is impressive in its regularity and raises several questions. Are these results only fluctuations of a small but genuine positive correlation? Or are the significant positive correlations solely caused by the experimenters, presumably psi mediated, but revealing nothing about a relationship between ESP and DMT? Or are both these factors involved?

M. J. initiated the first five experiments and was the driving force behind them. His results show a noticeable decline. The three experiments conducted in Iceland were initiated by E. H. These also show a steep decline. This suggests an experimenter effect or an initial effect of some kind. The question remains whether the DMT-ESP relationship holds to some extent in further experiments or whether it will sink into oblivion, as have most other promising predictors of ESP performance.

A NOTE ON PERSONALITY DIFFERENCES BETWEEN PSI-CONDUCIVE AND PSI-INHIBITORY GANZFELD EXPERIMENTERS

Carl L. Sargent (Cambridge University)

Many anecdotal reports suggest that the personality of the experimenter is an important factor in psi elicitation. In a post hoc survey, Parker (RIP 1976, pp. 107-09) found that psi-conducive experimenters differed significantly from psi-inhibitory experimenters on only two of the 15 personality factors from the 16PF inventory; the conducives were less dominant and less gullible than the psi inhibitories (E-, N+). These effects were almost certainly spurious on over-analysis. Further, Parker's data did not eliminate sex, age, race, nationality and other potential contaminants. Parker also did not consider the possibility that different personalities may function best in different environments. It might take a quite different personality to elicit psi in the Ganzfeld to that required to elicit psi working with a Schmidt PK machine, for example.

Data for eight experimenters who have conducted free-response Ganzfeld ESP experiments were taken from Parker's data, and profiles for three others added, making a total of five psi-conducive and six psi-inhibitory Ganzfeld experimenters. The two groups did not differ in terms of age, race, sex or nationality, and the 16PF data were collected before they had conducted their Ganzfeld research. Parker's original hypotheses were then tested against the data.

Taking the 15 primary factors first, one of Parker's three predictions was verified at the .005 level of significance (one-tailed). Psi-conducive experimenters were higher on Factor A than psi-inhibitories. An A+ person is stated by the originators of the 16PF to be warm, sociable, easygoing, while the A- person is cold, aloof, reserved, detached. The mean A score for the conducives was 5.8 (on a normalized 1 to 10 scale) while for the inhibitories it was 1.67, a phenomenally low level which suggests that, personality-wise, it is the psi inhibitories who are unusual, not the psi conducives.

Examining the two secondary factors of extroversion and anxiety, conducives differed significantly from inhibitories in being more extrovert (means of 6.08 and 2.22). The difference was significant at the .01 level (two-tailed). The anxiety means (4.03 and 5.37) were not significantly different. Conducives were significantly less neurotic than inhibitories (means of 4.05 and 6.90; $p < .05$, two-tailed) but this is not independent of the extroversion difference.

The data, in summary, suggest that, with Ganzfeld experimentation, there is nothing particularly special about psi-conducive experimenters with respect to personality; but there is something unusual about psi-inhibitory experimenters. They are remarkably cold, aloof, reserved, introverted.

Two caveats need to be expressed. The definition of a psi-

conducive experimenter in this study was a researcher who had reported clear significant psi-hitting in Ganzfeld work. No "credit" was given for correlational significance.

A second point is that these results may not be generalized willy-nilly to radically different experimental environments in parapsychology. The extroversion difference noted makes sense in an altered-state, vulnerable-subject setting, but matters might be very different in automated PK testing, for example. Research into experimenter effects should consider task-variable dependencies carefully.

A COVERT TEST OF PSI ABILITIES OF PSI-CONDUCIVE AND PSI-INHIBITORY EXPERIMENTERS

Carl L. Sargent (Cambridge University)

One hypothesis of experimenter differences in psi elicitation is that psi-conducive and psi-inhibitory experimenters differ in their native psi abilities. This study set out to test this hypothesis.

Using a previously established scheme for encoding written material into binary code form (JP, 1977, pp. 275-93), letters written to me by seven psi-conducive and six psi-inhibitory experimenters were encoded into binary form and used as responses in a binary ESP task, binary random digits being used as targets. One letter per experimenter, and 1,200 trials per experimenter, were used.

The seven psi-conducive experimenters scored significantly above mean chance expectation (which is 600 hits) on the ESP task. Their mean score was 611.14, $t = 3.49$, 6 df, $p = .013$, two-tailed. The six psi-inhibitory experimenters had a mean score of 597.16, which does not differ significantly from MCE. The between-groups difference approaches significance ($t = 1.95$, 11 df, $p = .077$, two-tailed). Of the six psi-inhibitory experimenters, two scored exactly at MCE; but one of the other four had the highest individual score of any of the 13 experimenters (627 hits).

The results lend some support to the experimenter-psi hypothesis of experimenter effects. Adrian Parker (RIP 1976, pp. 107-09) noted similar results from a different covert psi test with psi-conducive and psi-inhibitory experimenters; a significant psi effect for the psi conducives and chance data for the psi inhibitories. Results presented here are thus a conceptual replication of Parker's study.

An important methodological note is that the two groups of experimenters did not differ significantly in terms of age, sex or nationality. American experimenters tended to score higher than

Europeans, but the difference did not reach significance even at the .1 level; with a small sample, however, this suggests an effect strong enough to make nationality an important potential contaminant variable in further research of this type with larger samples.

FREE-RESPONSE STUDIES*

PRECOGNITIVE REMOTE PERCEPTION: A CRITICAL OVERVIEW OF THE EXPERIMENTAL PROGRAM

Brenda Dunne† (Princeton University) and John P. Bisaha (Midwest Parapsychological Research Institute)

Over a three-year period, 1976-79, an experimental program in precognitive remote perception (PRP) has been carried out under the auspices of Mundelein College, the University of Chicago, the Midwest Parapsychological Research Institute and Princeton University. This paper is a review of this program.

In PRP the percipients try to describe, by free-response verbal or written narrative or drawing, a randomly selected remote, unknown target location where an agent will be situated at a future time, with no sensory means of communication and no means of deducing the target by logical process. The target is unknown to anyone, including the agent or the experimenter remaining with the percipient, until after the percipient has completed the description. Quantification of the results is achieved by having independent judges match descriptions to targets by a blind rank ordering of the targets in each series.

To date, we have conducted a total of 40 formal trials in seven experimental series, employing 19 percipients with no claims of psychic abilities, and resulting in 82 transcripts, 80 of which have been evaluated at least once by independent judges in accordance with Solfvin, Kelly and Burdick's (JASPR, 1978, pp. 93-111) method of analyzing preferentially ranked data (RIP 1976, pp. 84-86; RIP 1977, pp. 146-51; RIP 1978, pp. 68-70; JP, 1979, pp. 17-30). Of a total of 157 transcript judgings of these trials, 84 of these, or 53.5 per cent, have been correctly assigned ranks of 1. In addition to these formal trials, nearly 40 informal trials have provided anecdotal evidence.

We hypothesize that the success of this design might be due to the fact that it comes close to simulating spontaneous psi in that: 1) it utilizes real experiences with real targets in a natural environ-

*Chaired by Charles Honorton (Princeton University).

ment; 2) the agent, percipient and experimenter are all actively participating in a shared experience within a shared, if temporary, belief system and 3) the precognitive aspect of the design emphasizes a totally "paranormal" mode of communication. Nevertheless, such an hypothesis is difficult to test and provides no parameters for the quantitative aspects of information transfer.

Earlier criticisms of remote-perception experiments have concerned the randomness of that target selection or the possibility of sensory cues that might aid the judges in assigning their ranks (Kennedy, JASPR 1979, pp. 1-15). The present experiments were carefully screened for possible sensory cues; and the authors have concluded that as long as the target cannot be deduced by logical process and is not chosen until after the percipient completes his or her description, the method of selection is relatively unimportant.

Of more concern is whether this design is actually measuring that which it purports to investigate. We are dealing with a series of communications, from target to agent to percipient to transcript to judge, and every link in that network is vulnerable to distortion, bias and experimenter influence. When quantification of the fidelity of the information transfer finally is attempted at the end of the process, the abstract and impressionistic component may be overlooked.

Perhaps the most serious flaw is the inadequacy of our quantitative techniques. The evaluation involves post facto comparisons by individuals who have not participated actively in the experimental test, and the distinction between a hit and a miss is often blurred by the judges' subjective bias. In an effort to gauge the extent of this bias, the 27 transcripts of Protocols 1, 2 and 3 (JP, 1979, pp. 17-30) were subjected to a minimum of five independent judgings each. Of these 27 transcripts, only three were consistently ranked as 1 by all judges, and three others were never ranked higher than 2. In all, 13 of the 27 transcripts received a mean rank score of 2 or less. The remaining 14 transcripts received a wide range of ranks, demonstrating a broad diversity in the judges' opinions. In our overall assessment, approximately 50 per cent of the trials were clear hits, while the rest were of varying ambiguity. This subjective variability in judges' ranks is especially insidious when applied to the least-correlated transcripts. Often, once a judge has selected his or her first, and perhaps second, choice, he or she will tend to be less precise in ordering the remaining targets, and the choice of whether to assign, say, a 4 or a 7 might be arbitrary. Yet those extra few points can make the difference between a "significant" sum of ranks or a "nonsignificant" score. When the outcome of an entire series is this sensitive to a single rank, the assignment of these ranks should not be so vulnerable to the subjective opinion of a single individual.

There is a further problem. A rank of 1 may be assigned because of outstanding fidelity of transcript to target, or because of a vague symbolic resemblance that catches the judge's attention. In

either case, the rank is the same from a statistical standpoint. We include a measure of confidence on the judging form with several of our later judgings but had too little data to draw a conclusion.

Another difficulty stems from similarities in targets, an unavoidable by-product of a random target-selection process, which complicate the judges' task. Such similarities affect the probability of the assignment of a first-place rank even when the transcripts are accurate.

Finally, there is the possibility of psi influences in the judging process itself. An argument might be made to regard the procedure up to the judging process as an elaborate technique for selecting targets for an ESP matching test where the judges are the subjects.

If we are interested in the probability that the percipient is obtaining actual information about a specific target, a standardized hierarchy of binary choices, or bits, could be applied to both target and transcript to ascertain and compare the amount of actual information contained in both. By determining beforehand the actual probabilities of any bit appearing in a given target selected at random from a target pool, it may be possible to measure the information transfer directly, eliminating much of the subjective judicial bias, chance target similarities and lucky guesses that influence results. In addition, such a procedure would permit each trial to stand or fall on its own merit, instead of representing a single component in the statistical outcome of a total series. We are now exploring the feasibility of such an approach (Jahn, Dunne & Jahn, J P , Sept. 1980).

We have observed that regardless how strict the adherence to the experimental protocol, the degree of success in any given PRP trial is still unpredictable. In spite of the multitude of studies that have attempted to identify the personality characteristics of successful psi subjects or experimenters, we are as far from the magic formula for replicability as ever.

It may be that the positive effects demonstrated in PRP research are not simply evidence of an individual percipient's, or experimenter's, "paranormal" ability, but are by-products of the interaction between the two. If this is the case, such phenomena cannot be predicted or evaluated simply on the basis of the personality parameters of either (or any) of the participants, or even on the basis of an additive process, such as Personality A + Personality B = psi. The process, or interaction, is complicated by factor E: the physical, intellectual and emotional environment within which A and B interact. Within the context of the experimental protocol, A and B together may be capable of behavior of which neither is capable independently. Those personality factors that have been identified as being representative of good subjects may be no more than characteristics that define the type of individuals best capable of entering into, contributing to and functioning within the type of bonded interaction that is conducive to psi effects. We have observed,

informally, that the more comfortable, intimate and warm the relationship between the participants, the more likelihood there was of a positive outcome.

The shortcomings of the design that have just been described are not intended to discourage research in this area, but to encourage the development of a more fastidious refinement of the technique, enabling us to learn more about the phenomenon we are observing. We are past the point of needing to prove the existence of psi. The time has come to determine whether or not we can fit this fact into our existing models of reality. If we cannot, then perhaps we must conclude that these models themselves are in need of revision.

RESOLUTION IN REMOTE-VIEWING STUDIES: MINI-TARGETS

H. E. Puthoff, R. Targ and C. T. Tart (SRI International)

To obtain an estimate of the resolution capability of the remote-viewing process, an experimental series was carried out in which a subject was asked to render descriptions of objects hidden in small light-tight metal containers (35mm film cans).

In preparation for the series, a target selector, not known to the subject and not otherwise associated with this study, was asked to select 10 small objects and to place each in a separate 35mm light-tight film can. The target pool was constructed to contain objects not particularly distinct from one another, so as to circumvent subject strategies based on knowledge of previous targets (by feedback) as the series progressed. The film cans were then turned over to a second experimenter, who, without opening them, numbered them 1 to 10 and secured them in a safe. At this point, all information as to film can contents and target-can number was lost.

At the beginning of each trial, the subject was closeted with an experimenter in an isolated windowless room. A second experimenter then left the laboratory, generated a random number by the use of the random number function on a hand calculator, obtained the associated can from the target pool in the safe and took it to a convenient location (not told to the subject) in a park about one eighth of a mile from SRI. The target can was thus not seen by the subject before or during the experiment; the location of the target film can was known to the subject only as being on the person of an experimenter, known to her, outbound to an unknown site. The outbound experimenter then remained at the remote location for a 10-minute target period, with the film can still unopened so that he remained ignorant of the target.

During the target period, the subject was asked to locate the outbound experimenter and to describe the contents of the film can in his possession. Since the investigator with the subject was igno-

rant of both the particular target and the contents of the target pool, he was free to question the subject about her perceptions without fear of cueing. The entire interaction in the laboratory was tape-recorded, and the subject was encouraged to make drawings to accompany her verbal description of the film can's contents.

Following the target period, the outbound experimenter returned to the laboratory, at which time all concerned (subject and experimenters) learned for the first time the contents of the target film can by opening it. A sequence of ten trials was carried out. During the series the target cans were used without replacement until the ten possibilities were exhausted. To facilitate analysis, it was decided in advance of the experimental series that the ten trials would be broken down into two subgroups of five trials each. Thus, in the blind rank-order procedure used (described below), a judge was asked to compare each target with five transcripts--one generated during the target period of interest and four generated during the other target periods of the subgroups.

In preparation for the judging of each subgroup, the subject's tapes were transcribed. The resulting transcripts were then edited only to the extent of deleting information that might act as artifactual cues to a judge, such as references to other targets, or phrases that might indicate the temporal order of the transcripts.

The transcripts and film-can targets, each in their own random order different from the order of target usage, were then turned over to an independent judge not otherwise associated with the experimental series. The judge was instructed to blind rank order, on a scale of 1 to 5 (best to worst match), each of the five transcripts against each of the five film can targets in each subgroup, generating five-by-five matrices. In the two series of five each, four were directly matched in the first series, two in the second.

Since the rank orderings by a single judge are not independent, the resulting matrices were analyzed using Scott's exact calculation method involving a direct count of permutations (JSPR, 1972, pp. 79-90). For the two matrices, respectively, we obtained by direct count $P_1 = 2/5! = .017$, $p_2 = 35/5! = .292$, one-tailed. Combining the results of the two subgroups using the conservative method devised by Eddington, we obtain an overall probability of $p = (\Sigma p)^N/N! = (p_1 + p_2)\, 2/2 = .047$, one-tailed. Thus the experimental series as a whole reached significance, with the bulk of the significance being generated by the first subgroup that was independently significant.

As examples of the quality of descriptions obtained, the results generated in the first subgroup of five contain quotes from the subject's first paragraph of each description as follows: for a spool and a pin, "It's definitely something thin and long ... with like a nail head at the end;" for a curled-up leaf: "a nautilus shape with a tail"; for a leather belt key ring: "The strongest image I get is like a belt"; for a can of sand: "like a miniature tower ... light

beige"; for a grey and white quill: "Grey and black and white ... it's organic and has been alive ... pointed or slightly rounded off at the top ... open or pointed at the bottom."

We thus obtained evidence that small objects can be discriminated by psi processes, and that the channel functions down to at least the order of a few millimeters spatial resolution. Furthermore, given that all experimenters, including the outbound experimenter with the target, were blind as to both the particular target and the contents of the target pool, we are led to conclude that either 1) the successful use of light-tight film cans indicates that the light level required to illuminate the target can be vanishingly small, or 2) the primary information channel is precognition of the feedback. Further work will be required to differentiate between the two.

INVESTIGATIONS OF TARGET ACQUISITION

R. Targ, H. E. Puthoff,† B. S. Humphrey and C. T. Tart (SRI International)

We have carried out a brief series of trials in an effort to shed some light on the question: by what means does the subject acquire the target in an ESP experiment?

In our remote-viewing experiments over the past seven years, we have asked subjects to describe remote geographical sites demarcated as a target by 1) the presence of a person known to the subject at the target site, to act as a beacon for the subject, and 2) geographical coordinates (latitude and longitude) of the target site. Furthermore, in the literature we have examples of the use of token objects (psychometry) as an abstract coordinate seemingly further removed from rational connection.

As part of an effort to determine what is necessary for target acquisition, we carried out an experiment in which the target location (one of 16 cells of a four-by-four matrix of target boxes) was designated by an arbitrary row-column (A, B, C, D; 1, 2, 3, 4) address. The purpose of this experiment was to determine whether an experienced remote-viewing subject could identify the contents of a randomly selected box, given only its coordinates, say A-4.

First, a team of three experimenters selected 16 objects. The objects were chosen to be easily recognizable, but not particularly distinct from one another (e. g., circular dial clock, circular dial compass; a number of objects of all-metal construction; etc.). This was to circumvent subject strategies based on feedback knowledge of previous targets as the series progressed.

The objects were then turned over to another experimenter, not otherwise associated with this work, who distributed them at

random, one per cell, in the four-by-four matrix located in a closed and locked laboratory he maintained in a separate wing of the Radio Physics Laboratory.

A final experimenter, different from any of the above and who was kept ignorant of the target pool, was closeted with the subject at the beginning of each trial, and acted as interviewer during each of the experimental sessions. At the beginning of the session, he would generate the target coordinate pair, say B-3, by means of a random generator function on a hand calculator. He then asked the subject to describe the contents of the target cell. The six targets generated by this process were D-2 (eyeglasses), A-1 (compass), B-3 (book), B-2 (plant), D-3 (doll) and D-1 (trumpet).

The entire interaction was tape-recorded, and the subject was encouraged to make drawings to accompany her verbal description of the target-cell contents. At the end of a 10-minute targeting period, the experimenter with the subject would telephone the experimenter with the target objects, stating simply the target-cell coordinates. The target control experimenter would then bring the target object to the laboratory in which the subject and interviewer were waiting for feedback. Six trials of this type were carried out, without replacement. The subject's descriptions were tape-recorded and transcribed.

In preparation for judging, the transcripts were edited by an independent editor only to the extent of deleting information (e. g., mention of previous days' targets, temporal order) that might act as an artifactual cue to a judge. The transcripts, together with their associated drawings, were assembled into six packets, which were then randomly numbered. These six packets were given to an SRI research analyst, not otherwise associated with this experimental series, to series, to serve as a judge. The judge's task was to blind rank order, on a scale 1 to 6 (best to worst match), each of the six transcripts against each target, generating a six-by-six matrix. Four out of six were first-place matched. Since the rank orderings by a single judge are not independent, the resulting six-by-six matrix was analyzed using Scott's exact calculation method involving a direct count of permutations (JSPR, 1972, pp. 79-90). The result is significant at $p = 20/6! = .028$, one-tailed.

With regard to the quality of descriptions obtained, in the first trial the subject described and drew what she summarized with the statement, "It's like eyeglasses. Two things that are round, which belong together," and the target was a pair of eyeglasses; for a book, she described "flat features, like sheets of metal connected somehow," and made a drawing resembling pages of an open book standing on end; for a Raggedy Ann doll, the subject described an object "velvety, feely, ... " having "mid-calf boots on ... pliable and floppy," and correctly drew a boot with a striped sock above it; for a brass trumpet, the subject described "a gold bell-shaped object, brass." The other two objects, chosen but not matched, were a compass and a plant.

With regard to the interpretation of an experiment with feedback such as this, one cannot differentiate between 1) real-time targeting on the object as it resides in its numbered cell, guided by the abstract coordinate, or 2) precognitive targeting on the object soon to be seen in the feedback phase. It can be concluded, however, that target acquisition by arbitrary abstract coordinate can take place under conditions such as those described.

REMOTE VIEWING: A CONCEPTUAL REPLICATION OF TARG AND PUTHOFF

Marilyn Schlitz† and Stephanie Deacon (University of California, Irvine)

Puthoff and Targ have put into experimental form a type of paranormal experience that they term "remote viewing." Their study involves the testing of subjects' ability to describe the whereabouts of a distant target team without the aid of the known senses. In attempting to replicate their findings, we followed their procedures as closely as possible while also taking into account procedures used by others in repeating the remote viewing work. Our intention was to test the hypothesis that a group of unselected subjects would be able to show some degree of remote-viewing capabilities under a few special, but easily obtainable, conditions--what Targ and Puthoff call the "recipe."

In carrying out this study, the two experimenters selected 12 target sites within a 20-minute driving radius of the UC Irvine campus. The targets were then given to an outside assistant who was instructed to arrange them into three groups of four each, such that each target within a group was distinct from the other three. The outside assistant next determined the target for each of 24 trials by use of a random number generator.

Twenty-four subjects were obtained for a study in ESP from the UC Irvine Social Sciences subject pool and from the surrounding community. A distinction in this study from the original design laid out by Targ and Puthoff was the factor of one trial per subject, a procedure used to avoid the possible carry-over effects from repeated testing of individual subjects. Trials were conducted several times per week, at approximately the same time of day, over a six-week period.

Following arrival at the Social Sciences Laboratory, the subject was taken to a small, quiet, semidark room. The two experimenters and the subject were given a short period in which to develop rapport and to discuss the experimental procedure. The subject was given a positive set by a discussion of the successful results achieved by Targ and Puthoff and by looking at examples of previous remote-viewing sketches as presented in the Proceedings of

the IEEE. The subjects were asked to rate their acceptance of ESP on a scale of no belief to total belief. They were then asked to accept remote viewing as possible and agree that it would be "okay" for them to have a psychic experience. The subject was then left alone to listen to a 17-minute relaxation tape. During this time, the outbound experimenter received her target instructions and proceeded to the target site. Following the tape, the inhouse experimenter returned and informed the subject that the critical period had begun (that is, the outbound experimenter had reached her destination). The critical period lasted 10 minutes, after which the subject was allowed to expand on the imagery generated during the critical period.

Feedback for the present study was supplied by a verbal description of the site by the outbound experimenter after her return, as well as any sketches she had made while there. The subjects were, however, encouraged to visit the site following the session.

After running all subjects, the two experimenters transcribed the recordings of the subject's descriptions, including any sketches that had been made. Each response was assigned a random number and then organized into one of three groups, according to which packet the target belonged to. In this way, each transcript would be compared with only the four target sites included in the packet.

Two blind judges were selected to visit independently each target site and to rank the degree of correlation between the site and each subject response made when the target pool included that site. To indicate their decisions, each judge was instructed to make a slash along a line anywhere between the end designating zero correlation and the end representing total correlation to reflect correspondence. This was to be done while at the target site. The lengths of the lines were later measured in therms of mm's.

For each subject, a psi score was computed by comparing the judges' average rating for the correct target with the mean of their ratings for the incorrect targets within that target packet. A positive difference would indicate psi-hitting; a negative difference, psi-missing. The mean of these differences scores was +12.52 mm; this mean was significantly greater than zero, indicating that overall psi-hitting had taken place ($t = +2.41$, 23 df, $p < .05$, two-tailed). Another analysis took overall hits into consideration. In this instance, Judge 1 had 12 direct hits and Judge 2 had nine, where six would be expected by chance. The resulting Z scores were 2.59 for Judge 1 ($p < .01$) and 1.18 for Judge 2 (p = ns).

Subjects' rating of their acceptance of ESP turned out to be uniformly neutral to positive, so no analysis with this measure was undertaken.

In view of the significant results of the judged responses, we feel that the null hypothesis may be rejected, and that we may accept the hypothesis that a group of unselected subjects can indeed exhibit

remote-viewing capabilities, under a few special conditions. We must conclude that there is some form of information transfer taking place that is not explainable in terms of the known or accepted senses.

TRAIT AND STATE FACTORS INFLUENCING ESP PERFORMANCE IN THE GANZFELD

Trevor A. Harley† and Carl L. Sargent (Cambridge University)

Two series of Ganzfeld GESP studies using pictorial targets were conducted. In each session, a receiver was initiated into Ganzfeld using a mattress for relaxation, and uniform auditory and visual fields created by white-noise and red-light fields. The receiver remained in Ganzfeld for 35 minutes, during which time his spoken mentation report was taken down verbatim by an experimenter.

A sender randomly selected a set of pictures from many diverse sets for use in the session after the subject had been placed in Ganzfeld and then a random decision specified the target picture from the set. Senders were located in a soundproofed room in a building other than the one in which the receiver and experimenter were located. The target was viewed for a 15-minute period in the middle of the receiver's exposure to Ganzfeld isolation.

After the 35 minutes had elapsed, the subject ranked the pictures from a duplicate set of pictures to the one employed by the sender 1, 2, 3 or 4, according to their correspondence with his mentation.

Personality data were available for subjects before testing from the 16PF inventory and data from a postsession questionnaire relating to Ganzfeld experiences, modified from one used by John Palmer and colleagues, were also used in a factor analysis.

The first series of testing employed 26 subjects. Of these, eight placed the correct picture first. The direct-hit rate of 31 per cent is not significantly above mean chance expectation. The sum of ranks was 64, a tiny deviation of -1 from MCE. However, while the first seven sessions yielded six direct misses and no direct hits, the subsequent scoring rate was 42 per cent direct hits, which was an encouraging improvement.

The 12 subjects who placed the correct picture first or second ("binary hitters") were significantly more extroverted than the 14 "binary missers" (mean extroversion scores of 6.43 and 5.04 on a 1 to 10 normalized scale). The difference is significant at the .02 level, two-tailed ($t = 2.59$, 24 df). The binary hitters also gave questionnaire replies indicating that the procedure was more success-

ful in altering their state of consciousness than the binary missers (Mann-Whitney U' = 43.5, $p < .05$, two-tailed). Extroversion and this state-shift item correlated significantly also ($r_S = +0.53$, 24 df, $p < .01$, two-tailed).

In the second study, nine of the 20 subjects tested placed the correct picture first in their judging, a significant (exact $p = .041$) direct-hit rate of 45 per cent. The sum of ranks was 37, a deviation of -13 from MCE indicative of psi-hitting (exact $p = .006$). We were able to replicate the extroversion effect; the 15 binary hitters had a mean extroversion score of 6.05, the mean for the binary missers being 4.30, and the difference again being significant at the .02 level ($t = 2.43$, 18 df). The state-shift/ESP relationship was also replicable, with larger state-shift being reported by the binary hitters (Mann-Whitney U' = 16.5, $p < .05$). However, the extroversion/state-shift correlation was not significant ($r_S = +.30$, $p > .05$). Thus, the parapsychological findings were found to be replicable while the psychological finding was not.

In the second study (but not in the first), pre- and postsession expectancy of success correlated positively with ESP performance ($p \leq .02$ by Mann-Whitney test). We regard this as spurious significance on over-analysis.

The findings provide support for the contention that Ganzfeld is psi conducive, and suggest that extroversion and state-shift in their interrelation and their relation to ESP performance may be important correlations in suggesting further work more specifically orientated at examining physiological factors in state-shift.

CONFORMANCE STUDIES*

"LABILITY" AND "INERTIA" IN CONFORMANCE BEHAVIOR

William Braud (Mind Science Foundation)

A derivation from the "noise reorganization" theory of Mattuck (RIP 1976, pp. 191-95) and the "conformance behavior" theory of Stanford (JASPR, 1978, pp. 197-214) is that the magnitude of a psychokinetic (PK) influence upon a target system should be proportional to the "lability" ("free variability," ready capability for change) of that system and inversely related to the degree of constraint or structure imposed upon the system. PK effects would not be as likely in "inert" systems (systems characterized by constraint, limited freedom to change). In general, indeterministic systems are more labile than are deterministic ones. The lability of a system may be measured, independently of any PK attempts, by determining the transition probabilities of the system or by measuring the reaction of the system to an imposed nonpsychic influence.

We have recently been searching for useful labile and inert physical systems to use in PK research and are especially interested in the magnitudes of PK influence upon the respective systems. We have explored three general types of labile systems. The first is the "inherently random" system, which is already quite familiar and which, in the form of bounding dice and quantum mechanical random number generators, has been well exploited by PK researchers. A second type of labile system is a biological one. Living-target systems may be especially useful for PK research because of their complexity (number of possible alternative states), flexibility and plasticity. Last year (RIP 1978, pp. 111-15), we reported the successful results of 12 PK experiments involving biological-target systems. A third type of labile system is a physical system upon which variability is nonsystematically imposed. We have conducted two experiments with such a system.

In the first experiment, the labile target consisted of a photocell--resistance bridge--amplifier system driven by a flickering candle flame located in a metal box in another room. Forty volunteers attempted to influence the activity of this system during 10

*Chaired by Hoyt Edge, Rollins College.

30-second duration Influence periods randomly interspersed among 10 30-second duration Control periods. The dependent variable was the electronically integrated voltages generated by the system for each period. Significant PK hitting occurred both when 20 volunteers and the experimenter received immediate analog feedback ($\bar{x}$ = 53.38 per cent, t = 3.35, 19 df, p = .003, two-tailed) and when another 20 volunteers and the experimenter received grosser, delayed feedback ($\bar{x}$ = 52.17 per cent, t = 3.66, 19 df, p = .0017, two-tailed). Scores for the two conditions did not differ significantly from each other. The analyses were done on the percentage of integrated activity occurring during Influence versus Control periods, with 50 per cent being mean chance expectation.

In a second experiment, the labile system was compared with an inert system, consisting of the same system just described, but driven by a more constant light source: a lamp powered by a well-regulated DC power supply. All volunteers received immediate analog feedback. Significant PK hitting occurred for the 20 volunteers attempting to influence the labile system ($\bar{x}$ = 52.03 per cent, t = 2.60, 19 df, p < .02, two-tailed), but not for the 20 volunteers attempting to influence the inert system ($\bar{x}$ = 49.83 per cent, t = 0.03, 19 df, p = ns); there was no significant difference between the two groups.

Receptive psi (telepathy, clairvoyance and precognition) may be viewed as the conformance of the percipient's brain processes to a more structured target event. Conformance should be most likely to occur when the brain is momentarily labile, freed from external and internal structuring influences, i.e., approximating a random event generator. A more inert brain--one that is heavily structured or constrained by some specific information-processing routine--would be less likely to demonstrate conformance behavior. Three experiments were conducted in which we attempted to influence the degree of lability or inertia of the brain processes of our percipients. We used auditory stimuli that were either random or predictable. Our hypothesis was that the more random sounds would suggest a more variable and fluid form of mentation, while the predictable sounds would suggest a more constant and constrained form of mentation. We deliberately avoided the use of strong stimuli that, through a kind of "driving" effect, would induce structure of a different sort.

In the first of these three receptive psi experiments, 10 volunteers (in a within-subjects design) listened to 11 minutes of progressive relaxation instructions followed (on different occasions) by 11 minutes of exposure to either relatively unpredictable or quite predictable sounds (order was counterbalanced). Following the sounds was a five-minute impression period during which the volunteer attempted to become aware of the contents of a slide viewed by an agent in another room. The slide was randomly selected from the 1,024 slides of the Maimonides binary target pool (JASPR, 1975, pp. 353-59); the binary coding system was used to evaluate degree of correspondence between target content and mentation content. A matched t-test indicated that the binary target scores for the more labile condition (following exposure to the unpredictable sounds) were

marginally superior ($\bar{x}_L$ = 5.6, $\bar{x}_I$ = 4.1, t = 2.18, 9 df, p = .057, two-tailed) to those for the more inert condition (following exposure to the predictable sounds). Analysis of self-report questionnaire items indicated that the two sound tapes were indeed judged to have different degrees of predictability, but did not differ in pleasantness or other possibly confounding variables.

The next experiment was a systematic replication of the study just described, with the following changes: a) a between-subjects design was used, with $n_1 = n_2 = 20$; b) the progressive relaxation exercise was eliminated; c) slightly different sound stimuli were used; d) the tone stimulation period was 15 minutes and e) rather than W. B., a research assistant (D. M.) conducted the experiment and functioned as agent. The pattern of results was similar to that of the above experiment, but this time the difference between psi scores for the two groups was not significant ($\bar{x}_L$ = 4.87, $\bar{x}_I$ = 4.20, t = 1.34, 38 df, p = .19, two-tailed).

In the final experiment, a semiconscious motoric response was substituted for the more conscious, perceptual/cognitive psi task of the previous experiments. The task was a modification of one reported by Rush (RIP 1978, pp. 88-89). While listening to eight and one-third minutes of either random or predictable tones, the volunteer absentmindedly turned a knob attached to a device that generated a changing DC voltage. Every five seconds, a microprocessor in a distant room randomly generated a target voltage; 157 distinct voltages were possible. Two measures were examined: a) the Pearson correlation coefficient between the 100 volunteer-generated voltages and the 100 target voltages for each volunteer, and b) the number of "direct hits" (perfect matches of target and response voltage, with p = 1/157). The correlations did not differ from chance expectation for either the 20 volunteers in the Labile condition or the 20 volunteers in the Inert condition, nor did the correlations for the two groups differ from each other. For the direct hits, significant psi-hitting occurred for the Labile condition ($\bar{x}$ = 1.25, t = 2.84, 19 df, p = .011, two-tailed), but chance performance occurred for the Inert condition ($\bar{x}$ = 0.55, t = 0.57, 19 df, p = .58, two-tailed); the difference between these two sets of scores was significant (t = 2.64, 38 df, p = .012, two-tailed).

The overall results of the present five experiments suggest that more labile systems are more susceptible to psi interactions ("conformance behavior") than are more inert systems. If this conclusion is supported by further research, an important question is whether this lability effect is physical or psychological. On the one hand, physically labile systems may simply be more susceptible to conformance effects. On the other hand, perceived lability may be the important factor. Observing a changing system may eliminate the pessimism, frustration and conflict that occur when an unchanging (inert) system does not immediately respond to our PK intentions. Another issue to be researched is how generalized (diffuse) or localized (focused) must structuring or destructuring be in order to inhibit or facilitate, respectively, the likelihood of conformance be-

havior; i. e., how closely associated must the effect of some structuring agent be to the particular response processes involved in the psi task?

TYPE OF MUSIC, TARGET-PROGRAMMING RATE, AND PERFORMANCE IN A MOTOR ESP TASK

Rex G. Stanford† and Gail Lafosse (Center for Parapsychological Research)

This experiment was intended, partly, to explore the relationship of spontaneity and ESP performance. Finger movement was the response mode in the ESP task. We sought to manipulate response characteristics, including spontaneity, by means of music. Measurements of finger movement could aid in understanding any difference in ESP performance associated with musical selections and might themselves relate to ESP performance.

We used two musical pieces of equal length (as edited). One was selected for its strong, driving beat; the other, for its slower, drifting, "spacey" quality. The first would tend to set the finger into motion but could impose heavy constraints on movement due to its strong beat. That might block ESP. The slower music would tend to produce less movement, but it might allow more spontaneous response because of having fewer repetitive, identifiable, patterns. It might favor ESP.

In the ESP task, subjects attempted to use finger movements to match random targets programmed throughout the music. With more time between targets, ESP might more easily modify movement. Therefore, we also studied target-programming rate.

Because this study involved measurable changes in trial-to-trial target position, we could examine Pratt's holistic hypothesis (JASPR, 1974, pp. 133-55) that "the subject is not actually calling the individual targets but is responding instead to the run (or sequence of targets) as a whole" (p. 149).

Subjects were 80 unpaid volunteers of college age or older with sexes represented about equally.

The experimental design was a two-by-two factorial with two kinds of music and two target-programming rates. Twenty subjects were assigned to each condition through a quasirandom process based upon the Rand table of random digits. For every subject, the experimenter was blind concerning number of targets and type of music.

Gail Lafosse tested the subjects individually. They were shown the chart recorder used to record finger movement and were

told that, using another device in the experimental room, its pen movements could be controlled by movement of the index finger of their preferred hand. The objective was for their finger movements to cause the pen position to match random-target positions programmed throughout the session.

To create the relaxed, unselfconscious circumstances that favor ESP, they would do the ESP task while listening to music and relaxing in a recliner chair in the experimental room.

Upon entering that room, they examined a small finger-movement box and were shown how the knob affixed to the lever extending from the box could be swung freely in a wide horizontal arc (about 120 degrees). Subjects held the box in the lap and rested the index finger on the knob. Swinging the knob-lever arrangement controlled a potentiometer that was interfaced with a Sanborn 7708A chart recording system.

Subjects sat with eyes closed in the recliner in whichever of the three positions (upright, middle or far back) felt most comfortable. The experimenter exited, and via cassette tape they heard instructions and the ESP-task music.

Both musical selections were electronic. The fast, rhythmical piece was part of "Neon Lights," by Kraftwerk, from the album "The Man Machine" (Capitol SW11728). The slower selection was an editing-extended recording of "Reflection of a String," by John Pfeiffer, from the album "Electronomusic 9 Images" (RCA VICS-1371). The music was heard in stereo through headphones.

Unknown to the subjects, length of target programming during the music was three minutes. The musical selection began 15 seconds before and ended 15 seconds after target programming.

A target was considered to exist each time appropriately spaced "horizontal" chart-paper lines moved past the recorder pen. When this happened, one of the ten blocks ($p = 1/10$) along the pen-swing path was considered the target according to the random designation of a target list derived from the Rand table. An assistant had prepared such a list in advance for each subject and sealed it in an opaque container. Half the subjects had a total of 180 targets spaced at one-second intervals. The other half had 60 targets spaced at three-second intervals. No subject knew how many targets she or he had.

Everyone in contact with subjects was blind concerning the contents of target containers. Subjects never had access to their lists.

Following ESP testing, subjects filled out a questionnaire. By putting slashes through lines, they rated how much they enjoyed the task as a whole, how much they enjoyed the music, and how favorable they felt this situation was for the occurrence of ESP.

They also indicated which recliner position they had assumed during ESP testing. Gail Lafosse, who scored response charts, was blind concerning targets.

To derive a subject's ESP score, we first computed the product-moment correlation (r) between the recorded pen position (range: 0 to 9) at the time a target was programmed and the target position for the pen (range: 0 to 9). Then, by dividing r by the reciprocal of the square root of the number of trials, we computed the corresponding Z-score (ESP score).

We developed two response measures for each subject. Mean change is the average change in pen position over the 360 half-second intervals during which targets were programmed. Standard deviation of changes is the standard deviation of these changes produced by a subject during target programming. Two comparable measures were computed for the target sequences of each subject. Target-based measures were used in evaluating Pratt's holistic hypothesis.

The faster music evoked greater mean change than the slower ($F = 7.91$; 1, 76 df, $p < .007$). It also produced a significantly greater standard deviation of changes. Musical selection did not influence answers to any questionnaire items.

The experimental plan called for examining the effects of our experimental variables upon the Z-scores and, also, for comparing the variances of those scores for our experimental groups.

Analysis of variance showed that neither music nor target-programming rate influenced Z-score means. Nor did they interact to influence Z-scores.

Music did not influence the variance of Z-scores, but target-programming rate did. There was greater Z-score variance for subjects with the faster target-programming rate ($F = 1.97$, 39. 39, df, $p < .02$). The effect of target-programming rate on variance was quite uniform regardless of the music played.

To help interpret this ESP finding, we examined possible differences between persons in the two target-programming conditions. Finger-movement response measures, sex of subjects, chair position selected, and enjoyment of the task as a whole did not reliably differ for the two target-programming groups. Target-programming rate did influence two questionnaire measures. Subjects with 180 targets judged the situation more favorable for ESP than did those with 60 targets ($F = 4.81$; 1,76, df, $p < .032$). The former group also enjoyed the music more ($F = 5.09$; 1, 76, df, $p < .027$).

No significant relationships emerged between questionnaire items and ESP performance (Z-scores).

For those who heard the fast music, we computed the correlation of Z-scores and a response-based measure intended to reflect the

ability of subjects to be spontaneous and occasionally to break away from the compulsive beat. No significant correlation was found between this measure and either Z-scores or unsigned Z-scores. This analysis was not as conceptually valid as we had hoped, for the two response measures involved in the "spontaneity" measure were much more highly correlated than we had expected.

Odd-even reliability for subjects' ESP performance was very close to zero and nonsignificant.

To study Pratt's holistic ESP hypothesis, we computed separately for our two musical-selection groups, who had shown different movement characteristics, the correlation of mean change in responses and mean change in the target sequence. We also computed the comparable correlation for standard deviation of changes. None of these correlations was significant. Our final planned analysis for this hypothesis showed that target-programming rate did not influence mean change in responses. These analyses offered no support for the holistic hypothesis.

The analysis that showed that those with more numerous targets judged the situation as more favorable for ESP can be interpreted retrospectively as compatible with the holistic hypothesis. The idea is that subjects used unconscious ESP to apprehend target "density" and that those with more targets therefore felt success was more likely. Zenhausern, Stanford and Esposito (RIP 1976, pp. 170-73) found that subjects felt greater confidence when the target situation, unknown to them sensorially, constituted less of a threat of failure. Could psi-mediated optimism about the ESP task possibly have been the factor that caused greater ESP-score variability? Rogers (JP, 1966, pp. 151-59; JP, 1967, pp. 290-96) found that when persons did ESP runs in self-judged favorable as contrasted with unfavorable moods, they showed greater run-score variance. These moods included the confidence factor.

The above speculation should be the basis of new research rather than any conclusions, especially given the unanticipated nature of the findings under discussion here.

CONFORMING OF PRE-RECORDED GROUP BEHAVIOR WITH DISPOSED OBSERVERS

Elmar R. Gruber (Institut für Grenzgebiete der Psychologie und Psychohygiene)

The experiments reported here had the purpose of showing whether conformance behavior occurs in living systems in a special real-life setting. The experiments involved complex social situations and subjects who were not aware of participating in any experiment (and never knew of it). Six experiments were performed with a

total of 21 subjects, each participating in two types of experiments. The first three experiments involved as "biological REG" (random event generator) persons entering a supermarket in Vienna on Saturday mornings. A photoelectric device at the entrance of the supermarket produced a click in an intercom every time a person interrupted it by entering the supermarket, and a second click when the person had passed the light beam. Persons entering the supermarket, blind to the experiment, could not hear the clicks.

The remaining three experiments involved as REG the cars passing through a small and short tunnel in the center of Vienna during rush hour. The tunnel can only be passed by one car at a time, just as only one person could enter the supermarket by the photoelectric device at one time.

Recordings for all six experiments were made by a coexperimenter who did not know the purpose of it. He furthermore was not allowed to be present during the recording in the room with the (already acoustically shielded) clicking device. Because of technical problems, the study was designed as time-displaced conformance behavior experiments. The recordings that were made during several weeks in the winter of 1978-79 were used for feedback one to two and a half months later. Observers were seated in comfortable reclining chairs and asked to listen to the clicks played back to them via headphones. They were not aware that the clicks were prerecorded. They were told to imagine many persons entering the supermarket or many cars passing through the tunnel, respectively, while they were to try to make as many clicks as possible appear in the headphones. This was for the 10 40-second periods called conformance periods, and they were not to think about the REG at all during the 10 40-second control periods, in which no click feedback was provided.

The experimenter sat in a room separated by another room from the room with the observer. He could not hear the clicks and had no feedback whatsoever regarding the prerecorded events. By means of a pack of 20 cards, he decided whether a control or conformance period was to follow, indicating a conformance period by a one-second buzzer sound immediately before its incipience, at the end of the 15-second intervening rest period. During rest periods and the control periods, the volume of the tape-recorder was turned off, so that no feedback was provided to the observer. The coexperimenter, not knowing about the purpose and meaning of the experiment, performed the statistical analysis at the end of the experiments. Mann-Whitney U tests were calculated between the 10 conformance and the 10 control periods for each session. The probability values thus derived were combined across subjects by the chi-square method recommended by Guilford. One-tailed tests were chosen, since similar experiments yielded positive results in the predicted direction.

Results for the "supermarket-setting": Experiment 1: one subject contributing 10 sessions: chi-square of combined probabilities

chi-square 23, 21, 20 df, p = .30, one-tailed. Experiment 2: 10 unselected volunteers each contributing one session: chi-square 33.94, 10 df, p = .05, one-tailed. Experiment 3: 10 additional observers each contributing one session: chi-square 23.67, 20 df, p = .30, one-tailed. The chi-square of combined probabilities across all 30 sessions of Experiments 1 through 3 was chi-square 80.82, 60 df, p = .05, one-tailed. Thus, the behavior of a group of people in a real-life situation conformed significantly with the intentions of motivated distant observers. Pooling was therefore made within and between subjects, the unit of analysis being the single session.

Results for the "car-setting": Experiment 4: one subject contributing 10 sessions: chi-square 39.35, 20 df, p = .01, one-tailed. Experiment 5: subjects as in Experiment 2: chi-square 20.49, 20 df, p = .50, one-tailed. Experiment 6: subjects as in Experiment 3: chi-square 33.91, 20 df, p = .05, one-tailed. The chi-square of combined probabilities across all 30 sessions of Experiments 4 through 6 was chi-square 93.75, 60 df, p = .005, one-tailed.

Subjects did not score consistently lower or higher in the conformance periods than in the control periods. Statistical analysis of this feature was not performed because hypotheses and purpose of the study was to show if overall conformance behavior occurs. Results were not uniformly distributed across subjects. Nonetheless, the behavior of a group of people driving cars in a real-life situation conformed significantly to the intentions of motivated distant observers.

The results extend positive findings about the conformance behavior concept as well as about prerecorded target studies. An attempt to eliminate experimenter psi was made by a number of precautions, such as blind analysis of the data and the deprivation of any sort of feedback to the experimenter during the experiment.

MACRO-PK STUDIES*

THE EFFECT OF THE LAYING ON OF HANDS ON AN ENZYME: AN ATTEMPTED REPLICATION

Hoyt Edge (Rollins College)

Of all the purported paranormal abilities, perhaps the one that would have the most direct usefulness is paranormal healing. A practical problem with this kind of research is a methodological difficulty in working with humans. The one experiment that stands out as not being affected by these complex psychological problems is the one performed by Justa Smith using the enzyme trypsin, a pancreatic enzyme assisting in the digestion of protein. The use of enzymes in healing research is further justified by the fact that "it has become evident to most biochemists that all metabolic reactions of each cell are catalyzed by specific enzymes ... It would follow, then, that any disease or illness proceeds from a lack of or from the malfunction of an enzyme."

The original experiment done by Smith with the healer Estebany in the summer of 1967 was successful, but a replication in the fall of 1967 failed, presumably due to certain psychological factors that were different from the original experiment. Tests with other enzymes resulted in their being affected in the appropriate way that would create a healing effect. Further tests with the effect of other local healers on trypsin failed to give significant results, although at times some of the healers affected the enzyme in the expected direction. An experiment using nonhealers showed a consistent noneffect on the enzyme. This contrasts decisively with the original experiment with Estebany, when there was no time when there was no effect and there was never any deactivation of the trypsin.

Anne Gehman, a medium well known in Orlando, Florida, for her healing and mediumistic abilities, acted as the healer. We usually worked at night, after Gehman had put in a day's work, once or twice a week when possible in the main biology lab at Rollins College. The experiment took three months because of scheduling problems. We kept the door locked whenever possible, but there were occasional interruptions, and once a class was in progress during the experiment.

---

*Chaired by Stanley Krippner, Humanistic Psychology Institute.

In the original experiment, to ensure the same temperature of the sample being healed and the control, a sensitive thermister was placed between Estebany's hands; the water bath, in which the control was put, was kept at the same temperature. This was necessary because the activity of the trypsin is sensitive to temperature changes. In our experiment, we modified the temperature bath so that the water from the bath came down a tube and circulated around the aliquot in which the trypsin was contained and continued back to the bath; thus, the water in the bath was constantly being circulated around the sample being healed as it was around the control. We felt this assured a greater equality and consistency of temperature. The water bath was kept at 27° C. This modification was tested and found to keep the temperature the same. T8253 Trypsin Type III: 2x Crystallized (Bovine Pancreas) from Sigma Chemical Company was mixed in the chromogenic substrate A-N-benzoyl-DL-arginine-p-nitroanilide HCL in phosphate buffer, pH 8. 6, at room temperature. When measurements were taken, the cuvettes were directly placed in a Beckman Model DB Prism Spectrophotometer set at 410mu, and I recorded the readings every 30 seconds for a total of six minutes. After the completion of all the experiments, the results were entered into a computer, which performed the necessary calculations.

After Gehman was comfortable, she was given a large test tube. Inside the test tube was an aliquot of the trypsin to be healed, around which water from the water bath circulated. We began by pipeting the control solution. Our spectrophotometer was such that measurements for the control sample had to be made first and then measurements for the experimental (healed) sample immediately afterward. For each measurement in both control and experimental, two cuvettes were used. One was the reference, which contained . 3 mill of substrate, plus water, HCL and buffer. The second cuvette had the same solution plus trypsin. For the control, the assistant took the trypsin that was being temperature controlled from the water bath. It took about six minutes to measure solutions for both cuvettes, and they were brought to me in a small room off the lab. The cuvettes were put into the spectrophotometer, and I made recordings every 30 seconds for six minutes. During this time, the assistant was mixing new solutions for two other cuvettes, which would be used to measure the activity level of the healed trypsin. He put in trypsin from the sample being healed by Gehman, and the cuvettes were brought to me for recording. This sample was designated as the experimental. I again made recordings every 30 seconds for six minutes, while the next control sample was being drawn. This procedure was continued normally until we had five measurements of the healed trypsin (experimental) and their corresponding controls.

Because of our equipment, the three experimental conditions could not be run simultaneously, as it was in the original experiment. We first ran five experiments under Condition One, which used undamaged native trypsin as the solution to be healed. Condition Two followed, which used trypsin damaged in ultraviolet light as the solution to be healed and as the control. Condition Three, which

used native trypsin in a magnetic field of 1,300 gauss was run on different nights but while the second half of Condition Two was being performed. Condition Three did not require the presence of the healer and was fit in at the convenience of the lab assistant.

The difference between the first and last measurement made on each test was calculated for the control and experimental samples, which eliminated any extraneous differences that might have been introduced to cause different initial readings on the spectrophotometer. A student's t (paired samples) was derived for each daily run as well as for all of the experiments in each of the three conditions. In Condition One (native trypsin), only one daily experiment was significant at the .05 level, and that was the very first one. However, when all of the data from the five experiments under Condition One are totaled, a significance level of $p < .01$ is reached, thus indicating that some kind of paranormal effect on the activity level of the trypsin has been found. The effect seems to be slight, however, and generally not great enough to be seen in individual experiments. Not even this conservative conclusion can be made about Condition Two (damaged trypsin), as no effect is found either in any of the individual experiments or when the experiments are totaled. In Condition Three (trypsin in a magnetic field), the results are somewhat better, as three of the six experiments reach a level of significance.

My results fail to confirm Smith's initial experiments, in which activation was found in each of the individual experiments in each of the conditions. However, my results lend support to her hypothesis that both the laying on of hands and a magnetic field affect the activity level of trypsin. Let me first discuss Condition Three. Considering that I allowed the trypsin to remain in the magnetic field for only about 75 minutes rather than three hours (as in Smith's experiment), fairly strong support can be derived from my data that a strong magnetic field has an activating effect on trypsin.

In considering Conditions One and Two, the results are more disappointing. In only one experiment was there any significance, and that at only a marginal level. It is interesting to note that exactly the same factors mentioned by Smith to explain why she did not have success in the second series of experiments with Estebany were present in our situation.

Another interesting factor is the amount of deactivation of the trypsin rather than activation. In Smith's original experiment, no experiment with deactivation was found. However, I am not particularly surprised to see this deactivation, and I see no reason why the psychokinetic effect of one person on an enzyme or on another person has to be beneficial.

EFFECTS ON MOTILITY BEHAVIOR AND GROWTH RATE OF SALMONELLA TYPHIMURIUM IN THE PRESENCE OF A PSYCHIC SUBJECT

Elizabeth A. Rauscher† and Beverley A. Rubik (University of California, Berkeley)

We examined the apparent interaction of a psychic subject with the motility behavior and growth rate of Salmonella typhimurium. At the University of California, Berkeley, on February 20, 1979, we had the fortune to work with Dr. Olga Worrall. There have been examinations of her purported abilities under laboratory conditions (e. g., Grad, JASPR, 1965, pp. 95-129).

Our experimental design involved a number of considerations. First, the subject, Olga Worrall, works as a spiritual faith healer and, therefore, her attention is applied to biological organisms. The target substance is biological. Second, there is a six-year baseline that has been established for the system. Third, motility of the organism can be directly observed by two sets of microscope oculars so that the subject and one experimenter can observe motility changes in real time. Fourth, motility and growth rates represent rapidly changing parameters in Salmonella and are effects that can be quantified. Fifth, these effects can be permanently recorded by a stroboscopic--Polaroid film recorder on control and target samples.

Lucio Gatto, a biologist interested in healing research, was observer.

Strains ST 171, a tumby mutant, and ST 1, the wild type of Salmonella typhimurium were grown in Vogel-Bonner citrate with a one-per-cent glycerol medium--"food supply"--and used directly for experimentation. The subject performed what she described as healings for up to several minutes, during which she held her hands cupped around the glass surface containing aliquots of bacterial cultures with no physical contact with the glass containers. These were either in the form of three μl on a microscope slide under a coverslip or in a stoppered test tube containing about 15 ml of solution. Phenol was added to the slides to a final concentration of 15 mM, sufficient to cause loss of all motility in a control, in approximately one minute after application. After treatment in part one of this experiment, slides were observed and photographed directly at 600-times magnification using a xenon stroboscopic source and recording film to record motility.

In part two of this experiment, Olga Worrall treated a test tube containing 15 ml of ST 171 at a concentration of 3×10^8 bacteria per ml to which was added 100 μg/ml of antibiotic chloramphenicol, and a test tube of ST 1 under normal growth conditions. Control test tubes of identical cultures were removed from the room before treatment of experimental cultures. All experiments and cultures were incubated in an electromagnetically (10^5 to 10^7 MHz)

shielded 30° C constant-temperature room, in which cultures were aerated by mechanical rotator at 20 rpm. The possibility of culture contamination is remote, as sterile procedures were followed and the growth medium is designed to be specific to Salmonella typhimurium and in general unfavorable to other bacterial species.

Inhibition of Toxicity of Phenol on Salmonella Typhimurium Motility. Applications of 15 ml phenol completely stops all motility of ST 171 in a control experiment within one to two minutes. In over 30 experiments conducted prior to February 20, 1979, no motile bacteria were observed after two minutes of the addition of the phenol "de-energizing" agent. In our experiment, the subject held her cupped hands near the test sample microscope slide for approximately one minute. Worrall appeared to inhibit this effect by the application of her treatment to the Salmonella typhimurium ST 171 strain with the effect that about 5 per cent of the bacteria continued to be motile after more than 12 minutes' exposure to phenol.

Inhibition of Antibiotic Effect of Chloramphenicol. The application of 100 μg/ml of chloramphenicol completely inhibits growth of ST 171 after about four hours of constant exposure. Worrall appeared to inhibit the growth inhibition to a small but significant degree above random error of measurement. In previous determination of the growth rate in over 10 cases, under the effect of chloramphenicol and under similar conditions to our control for this experiment, the standard deviation is $\sigma = .22$, for the largest variance from the mean. The standard deviation of the sample exposed to the subject, in which she cupped her hands about the sealed test tube, without physical contact, containing the test culture is σ . 39.

Twenty-four hours after incubation, it appeared that the treated culture was beginning to regain growth, and that perhaps maximum differences between experimental and control cultures were not yet fully realized. Depletion of culture for measurement terminated the experiment at this point. Motility in the treated culture at 24 hours was about 15 per cent greater than control.

Lack of Effect on Bacterial Culture Under Ideal Growth Conditions. The ST 1 in Vogel-Bonner Citrate--one per cent glycerol from a refrigerated inoculation--was treated during its initial growth lag phase. No difference from a control in growth up to 24 hours was seen, during which time both cultures were still in lag phase.

We designed and conducted a pilot study as a design experiment to examine the possible short-term effects of a psychic healer on a biological test specimen with controls, direct feedback and a permanent recording system. Our results indicate a 5 per-cent inhibition of the toxicity of phenol in its effect on bacterial motility after 12 minutes so that 5 per cent remain motile compared to none motile after one minute in the control. Also previous results of over 30 experiments have indicated complete immotility of the strain ST 171 upon addition of 15 mM phenol after one- to two-minute ex-

posure. We also showed an inhibition of the antibiotic effect of chloramphenicol after 24 hours' incubation with greater motility in the treated culture. The positive results have encouraged us to plan further experiments. Replication of this work by other laboratories would also be most desirable. We feel that we have demonstrated that there are suitable systems within which to study healing or subject intervention interactions.

KINETIC EFFECTS AT THE OSTENSIBLE LOCATION OF AN OB PROJECTION DURING PERCEPTUAL TESTING

Karlis Osis† and Donna McCormick (American Society for Psychical Research)

Psi has been conceptualized as having two capabilities: cognitive information gain and kinetic action, which Rhine hypothesized always go together: that there is an ESP component during PK action, and PK action during ESP (The Reach of the Mind, 1947). The first proposition has already been supported through testing (Osis, JP, 1953, pp. 298-309). The second has gone untested and is often explained away using acausal theories of psi, though it is suggested by spontaneous out-of-body experiences (OBE), where target persons report bodily sensations and the displacement of physical objects.

Since the fundamental nature of most OBE processes are unknown, they are subject to wide variations in theoretical interpretations as expressed, for instance, by Hart (JASPR, 1954, pp. 121-46) Palmer and Tart. The two main hypotheses are: a) extrasomatic hypothesis: during a true OBE, consciousness is externalized, existing apart from the physical body; and b) intrasomatic hypothesis: nothing leaves the body; the personality remains a psychophysiological unit in an altered state. Verifiable perceptions are attributed to ordinary ESP.

Both hypotheses describe some aspect of the OBE but, taken singularly, are too rigid to accommodate all the available information. Extensive observations suggest that rather than being an all-or-none phenomenon, the OBE is an ever-changing process that might oscillate between these two extremes. We hypothesize that the OB projection is an autonomous unit, ever-changing in its perceiving-acting capacity and able to localize itself in a physical environment. Such a unit would use ESP for "seeing" and PK for "action." The primary difference between the externalized mode of the OB state and ordinary psi processes would then be the matter of localization.

We predict that registrable PK effects at the ostensible location of the OB projection will be greater when subjects are more fully externalized and consequently hit on visual targets, than when they are less externalized and score misses on the perceptual task.

This hypothesis is in concordance with subjects' introspective reports: more hits are achieved when they report being more fully externalized.

The Optical Image Device (OID) is an apparatus for the display of randomized visual stimuli. It is partially composed of a round disk divided into four quadrants, each of which is a different color. The final composite picture consists of a line drawing of one of five images (or symbols) randomly projected onto one of the four quadrants. The stimulus thus consists of three aspects: the identity of the symbol, the quadrant on which it appears and the color of that quadrant. The final target is not located in its entirety in any part of the apparatus, but appears as an optical illusion, visible only from a location directly in front of the viewing window. The activation of a random number generator by a push-button in the polygraph room generates a new composite target picture and controls a marker pen on the polygraph that indicates the exact moment and duration of target generation.

The Shielded Chamber (SC) is an electromagnetically shielded, 18-inch cube, suspended from the ceiling by rubber strips to reduce environmental vibration. Within the cubicle are two nine-by-eight-inch metal sensor plates placed eight inches apart. Extremely sensitive strain-gauges (Baldwin-Lima-Hamilton Electronics type SPB3-35-500) are connected to the plates so that very small movements or vibrations will generate electrical impulses in the strain-gauges, which are AC coupled and have a time constant of three seconds, permitting high-gain recording without baseline drifts. The electrical output is preamplified 100 times at the SC and sent to the Beckman type RM Dynograph for further amplification and rectilinear recording at settings of .5/x .1. Recording speed: 10mm per second. Because of a technical breakdown, only one strain-gauge produced usable data for this experiment.

Scoring Device. This electronic device scores and retains in its memory the color and quadrant aspects of the target; the line-drawing aspect is scored manually after the session. As these two aspects of the subject's response are punched into the scoring device, immediate feedback is given. Therefore, the true identity of the target is not known to the experimenters until after the subject's response is recorded. Furthermore, the target display does not occur until after the critical part of the polygraph recordings have been obtained.

Alex Tanous (A. T.) teaches at the University of Maine. He shows enthusiasm for laboratory experiments, and we have worked with him sporadically on OBE and ESP studies since 1973. He was selected for this study on the basis of pretest results.

The SC is suspended in front of the OID approximately one hour before the experiment to ensure kinetic quiescence of the sensors. At the same time, the OID is also made ready. The room containing this equipment is then closed up, as are the rooms directly above it.

During the experiment, the subject is lying in a dark, 40-decibel sound-attenuating room, six doors away from the room containing the OID and SC, which is adjacent to the room where the experimenters are. Verbal contact is maintained with A. T. via intercom (direction determined by a control switch operated by K. O.). Tanous experiences divided or dual consciousness when OB and is therefore able to communicate his responses without interrupting his projection.

K. O. first activates the target generator at the OID by pushing the control button. He then announces the trial number to A. T., whose responses are tape-recorded, registered on a record sheet and punched into the scoring device by K. O., while D. M. records the response, and when it occurs, on the polygraph chart. Ten trials are completed in this way for each session. After the session, OID scores are obtained and communicated to A. T. Tanous is not, however, informed of the kinetic effects during these trials.

To minimize the likelihood of A. T. intentionally effecting the sensor plates, we tried to create the impression that only the perceptual results were important during the formal experiment, and that kinetic effects would be attended to only in the subsequent, exploratory part of the session during which A. T. engages in OB actions in the SC.

A "hit" was defined as the correct identification of any of the three target aspects: color, quadrant or symbol. Over 20 sessions, we obtained 197 usable trials resulting in 114 hits and 83 misses.

After the completion of the 20 sessions, the deflections on the strain-gauge channel of the polygraph record were blindly measured (the person did not know which of the OID trials resulted in hits and which in misses). Each deflection above the assumed noise level (five mm), obtained during inspection of pretest data, was measured in millimeters for eight consecutive sampling periods. Each period represented two seconds of recording time. Four periods (20 mm each) were sampled immediately prior to target generation and four after. The periods after target generation were of three types: when A. T. was reportedly "looking" at the target; when he claimed he was "assimilating" the information; and the latter two periods were when he was giving a verbal response. If the response occurred in less than four seconds, but more than two seconds, the second of the response periods was eliminated; this occurred in 96 trials. If the response was completed in less than two seconds--which happened in eleven trials--both periods were eliminated.

Mean strain-gauge activation levels for each sampling period were obtained by individually averaging the deflection amplitudes over those trials resulting in hits and those in misses.

As predicted, the mean strain-gauge activation level for the period immediately following target generation, during trials scored

as hits, was significantly higher than when misses occurred during that period: $t = 2.16$, 19 df, $p < .05$, two-tailed.

Over all eight periods sampled, the mean activation level for hits was significantly greater than for misses: $t = 3.64$, 19 df, $p < .002$, two-tailed. There was a significant difference in activation levels between hits and misses for periods prior to target generation ($t = 2.92$, 19 df, $p < .009$, two-tailed) and periods after target generation ($t = 2.57$, 19 df, $p < .017$, two-tailed). This general spread of activation is a post hoc finding.

Rhine hypothesized that psi should be regarded as a composite of ESP and PK action and that a PK effect exists at the surface of stimuli. However, the results of our original hypothesis: ostensibly unintentional kinetic effects can occur as by-products of narrowly localized OB vision, which is somewhat different from the effects that would follow from Rhine's hypothesis, since the kinetic action was not registered at the OID target, but at the sensor plates within the cubicle. We found a significant difference in the strain-gauge activation levels of hit and of miss trials when the target was supposedly being perceived (within two seconds of target generation). This finding may initially appear to be in the direction of Rhine's hypothesis. However, strain-gauge differential effects were also found in the eight-second period prior to target generation, and were spread over the entire 16-second period. This extends beyond the subject's verbal response to the perceptual stimuli and is, therefore, difficult to attribute to the hypothesis of unified ESP and PK action.

This prolonged strain-gauge activation conforms more to the hypothesis that the extent of OB externalization is a fluctuating process. Introspective reports concerning trials during which the subject felt himself to be more externalized also corresponded with a better rate of hitting. We assume that at times of greater externalization more hits are achieved. According to this tentative assumption, the strain-gauge activation level would rise not only because the subject scores a hit, but also because increased externalization facilitates both kinetic and perceptual effects. This conjecture is, of course, still highly speculative. We present it in the hope that it would stimulate further experimentation on OBE processes.

DISTORTIONS IN THE PHOTOGRAPHS OF TED SERIOS

Jule Eisenbud (Denver)

The camera pictures of Ted Serios are anomalistic insofar as the circumstances of their production do not lend themselves to explanation in terms of normal optical processes. In some of these, in addition, certain distortions or alterations in the objects pictured would not be easily explicable even if normal optical processes (e. g., microtransparencies placed between a light source and the camera

lens) were operating. While a number of such distortions have been reproduced in various publications, these have not received much attention in the continuing controversy over the subject. A recent discovery by Ian Stevenson and J. G. Pratt of apparent distortions in two pictures obtained during sessions witnessed by them in 1967 prompts a new look. I will therefore present here three examples of such anomalistic features of Serios's anomalistic pictures from my own collection and one other source. This will be followed by Pratt's presentation of the two instances discovered by him and Stevenson. In the case of each picture presented, Serios's manner of working with the Polaroid camera, and the precautions taken against trickery, were more or less as described elsewhere in various publications. The reason why this need not be gone into here in any detail will become apparent in what follows.

The print in the first example was obtained on October 26, 1962, in the presence of Curt and Mary Fuller, editors of Fate, and two other witnesses. It was identified through the Office of the Commissioner of the Royal Canadian Mounted Police as, except for the departures from the original to be noted, that of the RCMP Air Division hangar at Rockcliffe, Ontario.

Besides the spelling of Canadian as something like "Cainadain," there are other features of Ted's version that differ from the original. First, the upper windows in Ted's picture are not cleanly rectangular. Secondly, the lower floor of the building is dark, contrasting with the upper floor, which, like both floors of the original, is light. The most intriguing distortion of the original is in the placement of the windows and doors of the two floors relative to each other. In Ted's version the right-hand door and the two windows to the left of it (in the original) are pictured as if they were moved about 12 feet to the left. In the original, a lower-floor window is squarely centered under the large rectangular window on the upper floor directly under the letters "IAN MO." In Ted's version, instead of a window there is the blank wall space between the right-hand door and the window to the left of it. None of these distortions could result simply from the interposition of a transparency of the original between a light source and the lens of the camera. (In Ted's version, incidentally, the shadows in the door on the right side of the lower story show something like a 12-hour difference from those of the original.)

The second example was obtained on January 30, 1965. In response to my expressed but unpremeditated, desire to see the property shown in the lower portion, Ted came up with the picture shown in the upper portion. The house in Ted's version does not show the shutters, which can be seen in the original shown in the lower portion of the Figure (photograph taken on the day following that on which Ted's version was obtained). The house had been without shutters some years before, however (and well before Ted ever saw it), as can be seen from a photo taken in 1956. But some years before Ted came on the scene, the upper one of the two Dutch doors seen in the lower-left portion of the barn seen in this picture had been removed, so that Ted had never seen the door

when both of its parts, upper and lower, were present (or, of course, closed). At no time, in any case, could the doors when closed have photographed totally white, as in Ted's version, as they would always have shown thick darkly painted rims (one-by-four-inch raised boards) at the sides and top and bottom, and transversely across the middle (where an eight-inch-wide dark strip would have shown). At no time during the period when the house was unshuttered, finally, was the lower portion of the barn in dark contrast to its upper portion, as in both Ted's version and in the picture taken the following day. All ordinary photographs of the house and barn, incidentally, were kept in albums on the second floor of a town residence to which at no time did Ted (who, for various reasons, was always strictly monitored on this premise) have access. Even if Ted had had any existing photograph of the barn in his possession, however, he would have had to alter it to produce a transparency that would have shown a totally white barn door as it appears in his version.

The third example was obtained on March 16, 1965. One of the six targets written down at this session, but not shown to Ted (or me), was "staggerwing airplane," one of which is shown in the lower portion of the picture (JASPR, 1967, pp. 241-53). Although in Ted's version a faint shadow of the left-hand bar of the inverted V made by the strut between the wing and the fuselage shown in the original can be seen, what shows up more clearly is a V rightside up. There is no way this could have been obtained through a transparency made from an unaltered picture of the original staggerwing plane.

The general significance of distortions such as these will be discussed by Dr. Pratt.

FURTHER EXAMPLES OF DISTORTIONS IN PHOTOGRAPHS PRODUCED BY TED SERIOS

J. G. Pratt (University of Virginia)

I will present two instances of distortions obtained by Dr. Stevenson and myself in April 1967 (JASPR, 1968, pp. 103-29).

The first recognizable instance of distortion occurred in our third session, in which the abbreviation "Va." appeared in a reversed ("mirror") image. It was superimposed upon a background design that we later recognized as parts of the boundaries of Albemarle County. In our 1968 report, we pointed out that the boundaries of the county were not exact, though they were sufficiently accurate to leave no doubt regarding what they represented. The fuzziness of the county lines in Ted's picture makes it difficult to make an exact comparison with a map of the county, but there is one unmistakable distortion in Ted's production.

A casual examination of the county map may leave one with the general impression that the eastern boundary stretching from the northeast corner to the James River in the south is a straight line. More careful inspection shows that there is a bend in this boundary about a fifth of the way down, and from that point the line turns slightly to the west and continues straight until it joins the river. Ted's production shows a bend in the boundary at approximately the right place, but his picture shows the boundary turning slightly to the east, a direction that is a reversal of the reality.

The second example of distortion occurred in the session in which Ted produced structured photographs showing the dome of Jefferson's home, Monticello. Before that session, Ted had at different times shown each of the investigators a picture of Monticello and had said he would like to get it in one of his sessions. The two pictures were views of the building as seen from the garden (west) side but taken from different angles. Neither investigator was aware of the fact that Ted had shown another picture of Monticello to the other, and when pictures of the familiar dome showed up on some of the trials of our next meeting with Ted, each of us assumed initially that the picture was based upon the photograph of the building that Ted had pointed out to him as a likely target for his psychic photography. But his representation seemed to resemble the photograph taken from an angle showing the side of the dome more closely than the one taken from nearly directly in front of the building, and we said in our report that the former picture was the one Ted had reproduced. The features that did not seem to conform with this assumption were not pointed out in our report--we had not become aware of them by that time.

Looking again at the results with a particular interest in distortions, Ian Stevenson and I discovered that Ted's picture of Monticello really combined features from the two pictures he had pointed out to us before the session. His production, though it showed an unmistakable likeness of Monticello, contains distortions that cannot be accounted for in either of the two target photographs taken alone. Ted apparently produced an inaccurate photograph of Monticello, combining in one production aspects of the building that cannot all be seen from the viewpoint taken in either of the two photographs. The distortions involve mainly the positions and shapes of the windows in the sides of the dome and the course of the balustrade that passes near the base of the dome.

I visited Monticello with a professional photographer to see whether all of the features of Ted's psychic photograph could be duplicated in a picture taken at an angle different from the two used in the target pictures. We were not able to find any position in the garden from which the features of Ted's representation of the building can be duplicated in a single picture. This observation is important in relation to the apparent distortions, because Ted had visited Monticello several times. It is therefore conceivable that the reality back of his production was the building as he had observed it directly rather than in the photographs he had pointed out to the investigators.

Distortions in Ted's psychic photographs may be relevant to the interpretation of his performance in several ways, but there is time to mention only one here. They strengthen the evidence of the paranormality of his pictures. Ted's critics have attacked his claims --without any direct evidence and thus on a purely speculative, "What if ... ?" basis--by saying that he must have held a microfilm in front of the Polaroid lens to project structured images onto the film. This hypothesis assumes that Ted's pictures were derived indirectly from normal photographs. The existence of distortions shows that this assumption will not stand up as an explanation of all of his successes, since some of them contained features unlike any picture that could have been taken. This is not to say that the faking of a distorted photograph is impossible, but this task would require far more expertise than the critics have attributed to Ted or anyone else in their ad hoc microfilm hypothesis as offered heretofore.

Part 4: Poster Sessions

A FOUR-SUBJECT STUDY OF PSI IN THE GANZFELD

Hugh T. Ashton, Peter R. Dear, Trevor A. Harley and Carl L. Sargent (Cambridge University)

Four subjects, the authors, each completed eight Ganzfeld ESP test sessions, using a standard GESP procedure and an MCE value of .25 for success on each trial. Senders and receivers were located in different and nonadjoining buildings.

After a subject had been placed in Ganzfeld in a session, the sender randomly selected one of 25 sets of pictures for use in the session, and then randomly determined which of the four pictures in that set would be the target for the session. The receiver started a mentation report when he wished to do so and terminated the session when he wished to do so. 16PF personality data were available for all participants, and pre- and postsession questionnaires derived from those used by John Palmer and colleagues were also utilized. Following Ganzfeld termination, subjects judged correspondence of mentation with a duplicate set of pictures so that sensory cueing from a target was eliminated as a possibility. Ranking and rating on a 0 to 99 scale were utilized in the judging.

There were 14 direct hits in the 32 sessions, a scoring rate of 44 per cent, which is significantly above MCE ($p = .012$). The overall sum of ranks was 65, a deviation of -15 from MCE that indicates significant psi-hitting ($p = .009$). The mean Z-score, derived from rating data, was significantly positive (mean $Z = +.471$, $t = 3.42$, 3 df, $p < .02$). Using the individual as the unit of analysis, the mean rank sum per subject was significantly below MCE ($t = 3.38$, 3 df, $p < .02$). Thus, the data show significant psi-hitting.

As predicted, extroversion correlated significantly with scoring level assessed using the rank sum per subject (Spearman $r_S = +1.00$, exact $p = .042$).

Finally, for all four subjects individually there was a positive correlation between the ESP Z-score and session duration. The mean r_S value of +.358 was significantly above chance ($t = 5.61$, 3 df, $p < .006$). Analysis of presession questionnaire data shows that this duration effect is not contaminated by presession relaxation, mood, motivation or expectancy effects. The duration effect is predicted by Honorton's model of Ganzfeld ESP-optimization.

The results of this study suggest that Ganzfeld ESP-optimization is reasonably replicable and that "orthodox" forced-choice psychological correlates (e.g., extroversion) correlate with Ganzfeld

GESP performance. The duration effect also provides some support for Honorton's theoretical model of Ganzfeld ESP-optimization.

AN INVESTIGATION INTO THE TAROT

Susan Blackmore (University of Surrey, England)

People who consult Tarot readers often claim that they were given accurate information about themselves. Is it possible that this ancient system might incorporate methods of divining information unknown to parapsychology? The first step to answering this must be to test its effectiveness. Three such experiments are reported here.

Traditionally, in using the Tarot for divination, a reader asks the querier to shuffle and cut a pack of 78 cards, some of which are laid out in a predetermined pattern. The position and orientation of the cards ostensibly determine the reading, but in fact correct information may be produced in many ways: 1) it may be selectively recalled from more information given; 2) it may be judged correct when it is simply extremely general; 3) the reader may consciously or unconsciously use cues from the querier; 4) the reader may search for feedback from the querier; 5) the pattern of the cards may reflect information about the querier; and 6) either the reader or querier may use psi. The parapsychologist is interested in the last two possibilities (which are arguably distinct) and to investigate these the others must be ruled out. The following experiments were designed to do this.

Subjects were 10 students, five male and five female, from a parapsychology class. The experimenter acted as Tarot reader and another student as assistant. Each subject had both an ordinary reading, face to face with the experimenter, and a test reading. Five subjects took each type first. Each subject rated the reading on a 1 to 7 scale according to how well it applied to him or her. Test readings were conducted blind. The assistant asked the subjects to shuffle and cut the cards as usual and for each recorded the order of the top 10 cards. These orders were given to the experimenter with code letters but no names. The experimenter assigned each a new letter and wrote a reading for each. The subjects then rated and ranked these. Finally, the codes were exchanged and it was determined whether the subjects had rated their own reading higher than others' or ranked it higher than would be expected by chance. The results for both types of reading were compared.

For ordinary readings, the mean rating was 5.7. In test readings, the subjects gave their own reading 4.1 and all others 3.2. Clearly, the ordinary readings were highly successful and rated better than test readings ($t = 3.54$, 18 df, $p < .005$, one-tailed) but also ratings given to own test readings (4.1) are significantly higher than to others (3.2; $t = 2.13$, 18 df, $p < .05$, one-

tailed), which implies some residual effect of the cards when all other sources of information have been removed. This is confirmed by the sum of ranks, which is 38 when MCE is 55 ($p = .035$, one-tailed). We can conclude that although blind readings are not judged as successful as face-to-face readings, there is some residual effect of the cards when all other cues have been removed.

A second, confirmatory, experiment used an identical procedure, but subjects and an assistant were recruited by advertisement. The mean rating given to ordinary readings was 5.4, to own test readings 3.3 and to others 3.2. Again, the ordinary reading was judged as more successful ($t = 3.71$, 18 df, $p < .005$, one-tailed), but there is no significant difference between the ratings of own and others' test readings ($t = 0.21$, 18 df) although the small difference is in the expected direction. The sum of ranks is 52 (MCE = 55).

It has been suggested that subjects unknown to the experimenter but interested in the Tarot would be preferable. Therefore, in the third experiment the assistant and subjects were all parapsychologists from the U.S. This was made possible by JoMarie Haight, who kindly acted as assistant. Only test readings were conducted, using the same method as before. Nine subjects completed the task, giving a mean rating of 3.4 to own readings and 3.3 to others. Again the difference is in the expected direction but is not significant ($t = 0.47$, 16 df). The sum of ranks is 45, not significantly lower than MCE of 49.5.

The evidence from three experiments indicates that when all other cues are removed from the Tarot reader and only the card order is given, the ability to provide accurate information is drastically reduced and is very little, if at all, above chance expectation. In only one of three experiments was there a slight tendency for subjects to select their own reading from among nine others under these conditions, although face-to-face readings were judged as very successful.

It could be argued that the method used to conduct blind readings may disrupt the special conditions necessary for the Tarot. We may therefore only draw conclusions for the conditions used here; further studies might improve on this method. These experiments do not throw any light on the nature of the process, if any, involved, but the results of the first experiment give encouragement that this may be worth pursuing further.

A STUDY OF MEMORY AND GESP IN YOUNG CHILDREN

Susan Blackmore (University of Surrey, England)

Previous experiments with adult subjects investigated whether errors made in ESP resemble those made in memory or perception

tasks and whether ESP ability correlates with memory test scores. In none of these experiments was there independent evidence of ESP occurring, although there was some indication that form of targets is more important for ESP than their meaning. In an attempt to obtain better ESP scores, similar experiments used young children as subjects. Spinelli (RIP 1976, pp. 122-24) found that children aged three to eight years scored positively in a GESP test, with the youngest groups scoring highest. The conditions of Spinelli's experiments were repeated here as far as possible.

Subjects were 48 children aged three to five years, tested at three playgroups in pairs. Each child acted as either sender or receiver for 10 trials and then changed roles for a further 10 trials. The children were taken from the group in pairs when they were not otherwise occupied and as far as possible with a friend of their own choosing. No child who was not keen to "play the game" was tested. Many children wanted a second turn, but only five were allowed this, these extra results being included in the overall scores but not in the other analyses.

In most tests, the sender occupied one room with an assistant who showed the sender the target pictures, asked the sender to think about them and, when ready, to press a switch operating a light in the adjoining room. Here, the receiver sat with the experimenter, who presented him or her with a pool of five pictures to choose from on each trial. When the light came on, the child made a choice, the experimenter recorded it and then informed the assistant. If the guess was correct, all participants received a piece of candy. As in Spinelli's study, the children wore thinking caps if they wanted to, and two stuffed owls helped them play the game.

Targets were pictures. On each trial, the receiver chose from five pictures taken from a pool of ten. There was always one of each of five colors, and each picture appeared exactly once in each position. In this way, color and position preferences were controlled and there was a different pool on each trial. In addition, the positions of all choices and targets were recorded and childrens' picture and color preferences were elicited either before or after the ESP test.

On each trial, the sender looked at one of 30 possible pictures. These included 10 key pictures identical to those seen by the receiver, 10 of the same object but that looked entirely different and 10 of a different object but perceptually similar. The pictures were developed by testing with similar age children until all were readily named and their relationships easily perceived. Target orders were prepared by computer and lists for each run sealed in opaque numbered envelopes by an assistant who took no further part in the experiment. ESP scores for each of the three types of target were compared.

In addition, 46 of the children were given a memory test either before the ESP test (N = 25) or afterward (N = 21). They played with the 10 key picture cards for about four minutes, then the cards were turned

face down and they were asked to recall them. A score was assigned according to how many they recalled.

The following hypotheses were tested: 1) ESP scores were expected to vary with the three different target types; 2) a correlation between ESP and memory scores was expected, although its direction was not specified; and 3) a negative correlation was expected between receiver's age and ESP score, on the basis of Spinelli's findings.

The mean ESP score was 2.02, which is not significantly different from the MCE of 2.0 ($t = .21$, 52 df, $p = .83$). The number of hits on each of the types of target was compared. There were 36 direct hits, 34 hits for dissimilar pictures of the same object and 37 for visually similar pictures representing different objects. There is no significant effect of target type (chi-square .13, 2 df, $p = .94$). There were also no significant correlations between memory and ESP scores either as sender ($N = 45$, $r = .05$) or as receiver ($N = 46$, $r = .23$). A negative correlation between age and receiver's ESP score was found ($N = 45$, $r = -.18$), but it is not significant ($Z = 1.19$, $p = .23$).

None of the hypotheses was confirmed. There appeared to be no significant overall ESP scores, no effect of different types of target and no correlations between either ESP scores and memory test scores or ESP scores and age. No conclusion, however tentative, can be drawn from these results.

SPLIT ANALYSIS TECHNIQUES FOR ROBUST EFFECTS

Richard S. Broughton and Brian Millar (University of Utrecht)

Along with psychology, parapsychology is already in the midst of a second computer revolution, as the advent of microcomputers puts powerful tools of experimental analysis and control in the hands of researchers at a relatively modest cost.

The growing accessibility of computers will certainly improve experimental control, but the concomitant new freedom to carry out many different statistical tests virtually at the touch of a button brings its share of responsibilities.

Periodically, parapsychologists have been cautioned regarding the dangers in multiple analyses of data and in endless post hoc searching of data for "significance." Recently, Kennedy (JASPR, 1979, pp. 1-15) has again drawn attention to these problems, so one might expect that most parapsychologists will strive to avoid such pitfalls.

It may, however, be argued that in our present state of

ignorance concerning psi there is always a need for post hoc combing of data to ensure that no potentially important findings are missed. No one would quarrel with this position if such effects are treated accordingly, as suggestive findings requiring further confirmation. All too often, in the course of the report such suggestions are magically transformed into effects with little or no consideration of the fact that their true origin may be what Barber has termed the "Investigator Data Analysis Effect."

What we shall do here is suggest a simple compromise that is available to those whose experiments collect blindly, that is, without the experimenter seeing it, and this could include the majority of present and soon-to-be computer users. It is an adaptation of the familiar pilot-confirmation design, which we have found useful for allowing maximum statistical freedom while ensuring that the effects found are not analysis artifacts.

The technique is this: after the data are collected but before any examination takes place, the data are split, according to some suitable criterion, into a predictor part and a confirmation part. The confirmation part is set aside unseen, but the predictor part is examined for any effects it is thought might be present. The investigator can happily embark upon a "no holds barred" analysis of the predictor data (assuming the tests are appropriate) until he or she is satisfied that the potential of that batch is exhausted. Then, using the findings of the predictor, the investigator formulates a few specific hypotheses regarding the effects thought to be present. Finally, the confirmation part of the data is tested to see if these hypotheses are confirmed. The predictor data are used only to generate specific hypotheses for the confirmation set and are not used in the confirmation analysis.

This strategy combines the best of both worlds, the freedom to look for any effects that could be present and the statistical rigor of strict hypothesis testing. It is a simple matter to incorporate this strategy into an experiment, with the only principal requirements being that the data are collected in such a way that the experimenter is blind to the results and that sufficient data are collected to enable meaningful analysis of the two parts. This strategy is ideally suited to computerized experiments, where the first requirement can be met as a matter of course and the second can be readily accomplished due to the ease with which experiments can be run.

A recent example of this technique in action is described by Broughton (EJP, May 1979, pp. 337-57) in a report of an experiment in which the data from each of 48 subjects were split into predictor and confirmation halves in an ABBA pattern.

It should be clear that split-analysis techniques can easily be built into a large proportion of semi- and fully automated psi experiments. With the little extra effort required to gather enough data to support the split analysis, the investigator can have the statistical power of a pilot-confirmation design with little more than the price of a single experiment.

One caution must be issued. Once an investigator decides to use this technique, it is not possible to combine the split parts under any pretext, or the purpose of the exercise will be defeated. Still less should any claim be based upon significance in the predictor data alone.

Split-analysis techniques are not new to parapsychology, but there are compelling reasons why parapsychologists should use them more frequently. Not only will the appropriate use of such procedures leave investigators with greater confidence in newly discovered effects, leading to more widespread follow-ups, but creative investigators can make use of such techniques to attack some of the more intractable problems in our field. It is our hope that the computer explosion and the experimental innovation that these devices facilitate will enable parapsychologists to find new and exciting psi effects; but let them be robust effects. We simply cannot afford to waste precious time and scarce resources chasing chimeras.

FURTHER STUDIES OF PRECOGNITION IN MICE

David B. Clemens (University of California, Santa Barbara) and
David T. Phillips (The Information Connection, Santa Barbara)

The mouse precognition experiment reported under the pseudonyms Duval and Montredon (JP, 1968, pp. 153-66) was repeated with modifications. A cage was constructed with two independent shock grids separated by a barrier that could easily be crossed by the mouse. On each trial one side was selected to receive a shock by a Zener diode type random number generator. A hit was recorded if the mouse avoided the shock. A Helium-Neon laser was used to determine the position of the mouse. A PET microcomputer controlled the experiment and recorded the data. Ten male Swiss Webster white mice were used as subjects.

Overall results were not significant: 888 hits in 1,728 trials ($Z = 1.15$), and 84 hits in 171 "random behavior trials" ($Z = .23$). Random behavior trials were recorded when the mouse moved across the barrier when it had not received a shock during the past five seconds. Each mouse was used in two sessions, and an apparent decline effect was observed, from $Z = 2.03$ in the first session, to $Z = .18$ in the second. Other small-animal experiments using aversive stimuli (JP, 1975, pp. 153-66; RIP 1975, pp. 11-14) have also failed to reproduce the strong positive results reported by Duval and Montredon.

A TEST OF RUNNER'S EUPHORIA AS A PSI-CONDUCIVE STATE

Hoyt Edge and Wendell Wright (Rollins College)

All distance runners at one time have experienced the state of euphoria caused by the extreme exertion of a long run. Runners may lose conscious contact with their physical exertion, and consciousness may produce hallucinatory visions or a state of euphoria. At any rate, there seems to be a state of disassociation. The euphoria achieved in this state has been compared with the meditative state of mystics, who also achieve a euphoric trance state. The meditative state has been found to be productive of psi (Braud, JP, 1976, pp. 6-16) at least in one experiment, and a whole array of experimental literature attests to altered states of consciousness, in general, being psi conducive. In addition to meditative states, successful ESP experiments have been done in dreams, Ganzfeld, extreme relaxation and hypnosis. It was the object of this experiment to see if the state of euphoria achieved by the distance runner might be psi conducive.

At a meeting of the Daytona Beach Track Club the purpose of the experiment was explained by a fellow runner; 10 of the members volunteered to participate. The names and telephone numbers of the volunteers were taken down, and each of them was given an addressed envelope and an answer sheet with instructions. On a prearranged day (each runner having a separate day), a picture, which had been placed by a third party in an envelope with the runner's name on it, was put in a target area. During that 24-hour period, the runner was to make his usual run and to note his thoughts, particularly during the state of well-being achieved by a distance runner. Upon the completion of the run, the runner was to write down his thoughts and images on the answer sheet and mail it to one of the experimenters (W. W.). At the completion of the test, the unopened envelopes with the answer sheets and the envelopes with the target pictures were brought to experimenter H. E., who had an uninvolved person transfer the target pictures into an envelope and make out a key, marking the target pictures with a letter. This key was put in a sealed envelope and locked up by H. E. Then H. E. sent the targets and the answer sheets to JoMarie Haight at FRNM, who had 10 people, each seeing only one protocol, rate the target pictures on a scale of 0 to 30. These were sent back to H. E., who transformed the rating scale to a ranking one and decoded the targets.

One of the judges passed on rating the targets based on her protocol, so we only had a target pool of nine. Only one of the judges ranked the correct target as first, thus yielding only one direct hit. Five of the nine ranked the appropriate target in the top five, which would be expected by chance. The rankings were also well within chance.

No psi was found in this experiment. It is difficult to know the cause of this, whether the altered state of consciousness pro-

duced by running is not psi conducive, or whether runners do not have the types of personality that produce psi, or whether something about the experiment led to the negative finding. Several points are worthy of consideration. Very little time was spent "psyching up" the runners to believe in the success of this experiment. Also, looking at the protocols, one sees that they are extremely short. This gave the judges difficulty, and none felt good about ranking the targets. One even refused. The experimenters may have failed to emphasize enough the necessity of the full reporting of their thoughts, or perhaps this state is not conducive to a good deal of imagery. Further, runners in general are reserved, shy and tend toward introversion, and most of the personality studies have shown that extroversion is more psi conducive. Although we did not give personality tests to our subjects, it may be that our subject population was biased through personality against giving ESP responses.

THE EFFECT OF FEEDBACK AND AWARENESS ON A PK TASK

Hoyt Edge and Kevin Burke (Rollins College)

In a recent survey of 17 of his PK experiments, Braud (EJP, 1978, pp. 137-62) found that neither feedback nor awareness of the task seemed to be a factor in psi success. Because of recent modeling of psi by Stanford (JASPR, 1978, pp. 197-214), Schmidt, (JASPR, 1975, pp. 267-92), Tart, and Walker, the twin questions of feedback and awareness have assumed importance in experimentation. This experiment is an attempt to investigate further the roles of feedback and awareness of the psi task by seeking conceptually to replicate the tentative conclusions reached by other experimenters that neither feedback nor awareness have a significant effect on psi performance.

A Paratronics ESP-2 random event generator (REG) modified for a PK task was used in conjunction with Paratronic's stimuli Module and XY Module to distribute hit/miss impulses and provide 120-volt power to external units. An 18-volt transformer was connected to the Stimuli Module to transfer power to the coil of a 120-volt relay, which in turn controlled the two lights of a remote light unit (RLU).

Thirty-two volunteer unpaid participants ranging in age from eight to 60 years were tested over a period of 24 days. Participants were divided into two equal groups: those who received direct visual feedback of their PK efforts and those who received no feedback either during or after the tests. Both groups were also tested two ways. Part I was a test for nonintentional PK during a relaxation sequence on the tape between the initial instructions and the actual commencement of the PK task. During this time, the RLU was disconnected and only the REG and counters were turned on in an adjacent room where the experimenter was during each session. Part

II was a direct test of intentional PK with all elements operating. To prevent the participants of the nonfeedback group from prying for scores or other information, they were led to believe the important purpose of the test was to measure brainwave activity between the right and left hemispheres. The headband with simulated EEG electrodes was worn by members of this group to enhance this story. Also, the group that did receive feedback was not aware of the test for nonintentional PK until the completion of their test. In summary, then, there were four conditions: 1) intentional PK with immediate feedback, 2) nonintentional PK with gross feedback after the run, 3) intentional PK with no feedback and 4) nonintentional PK with no feedback.

Initially, a cassette tape was prepared by H. E. Both sides of the tape were identical except for the instructions of the specific task, i. e., the right or left light on the RLU to be lit more often. The tape was recorded with one minute and 25 seconds of preliminary instructions. These instructions were followed by seven minutes and 20 seconds of a relaxation exercise. It was during this period that the REG was turned on without the RLU being connected to test nonintentional PK. A final minute and one-half of instructions followed before the commencement of white noise, which continued to the end of the test.

The experimenter running the test instructed participants to enter the quiet room and be seated in the recliner chair making themselves as comfortable as possible. In the case of the feedback group, the RLU was left in full view. In the nonfeedback group, the RLU was encased in an opaque plastic box, and the participants were told to "visualize" the RLU within the plastic box in front of them. The experimenter retired to the adjoining lab and operated the REG and RLU in accordance with the proper timing of the tape.

Generally speaking, the subjects did not display psi. When the individual runs are examined, only three instances of significant PK are found. When one looks at the various conditions as they are divided by which side of the tape they heard (which told them that a hit was either the red or the amber light), only the second side of the tape in the intentional but nonfeedback group reached significance ($p = .026$, two-tailed). However, when the overall scores are examined, no significant levels are found in any of the four conditions, nor when conditions are totaled in various ways, nor is there a significant difference between conditions.

PSITREK: A PRELIMINARY EFFORT TOWARD DEVELOPMENT OF PSI-CONDUCIVE COMPUTER SOFTWARE

Charles Honorton and Lawrence Tremmel (Maimonides Medical Center)

If interactive computer software with self-contained motivational elements could be developed, perhaps the persistent problem

of replication failures, generally attributed to poor experimenter-participant interactions, could be circumvented or reduced. Computer-participant interaction would reduce the traditional reliance upon the experimenter's social skills for inspiring participant interest and motivation.

PSITREK was developed to test the feasibility of portable, computer-participant interactive programming. PSITREK resembles the popular computer space-fantasy games. Success or failure is totally dependent upon the participant's psi abilities. We report the results from all 443 sessions run since the program's inception, a period of almost one year. Overall significant positive scoring was our only hypothesis, although the program was designed to provide simple comparative analysis of 1) ESP and PK modes, 2) clairvoyance and precognitive ESP modes and 3) two fundamentally different random sources.

Ninety-three volunteers contributed a total of 443 sessions (games) trials. Participants sat in a lounge chair opposite the CRT terminal or a 19-inch color TV monitor, depending on the version of PSITREK selected. After explaining the operation of the game to the participant, the experimenter generally left the experimental room and pursued his business. The experimenter returned after the session to read and discuss the results with the participant.

Data were collected, recorded and analyzed automatically on a Cromemco System Three microcomputer. The Z80-based system included 64K (bytes) of RAM, CRT terminal, disk drives. The logic output of the ASI 1700 Feedback Myograph was interfaced to the computer. Random numbers were obtained from two electronic random noise sources characterized by fundamentally different processes. The Texas Instruments MD20 diode (Source-1) operates via a quantum mechanical tunneling process and is approximately three orders of magnitude greater than the Micronetics Diode NK-1 (Source-2), a pure avalanche device. Target numbers (1-4) were generated by one or the other of these diodes in the ESP modes. In the PK modes, both sources were used together such that one generated a target bit and the other a matching bit. A randomly determined complementing/decoding process was used to control for a quartering bias.

Psi tasks were embedded in a computer video game similar to popular space-fantasy games. Participants were instructed to use their "intuition" to locate "hidden" Klingon spaceships, thus preventing a Klingon invasion of the participant's territory. Winning the game, "repulsing the invading spaceships," required above-chance scoring. Chance or negative scoring resulted in a successful Klingon invasion and from the participant's perspective, a lost game. Statistically significant positive scoring ($Z \geq 2.0$) was rewarded by a "Jackpot" display at the end of the session, consisting of congratulatory messages and a variety of unusual sound effects.

All games consisted of 48 independent trials with a hit probability of .25. Each successful match ("hit") triggered one of 28

randomly determined, animated audiovisual graphic subroutines, displayed on the CRT terminal or on the color TV monitor. Every third hit eliminated one of the invading Klingons. Certain randomly selected misses, identified by a "RED ALERT" display, allowed the Klingon spaceships to fire upon the participant's imaginary spaceship. Each successive Klingon hit inflicted progressively worse damage upon the participant's spaceship, culminating in the destruction of the latter if the overall score was at or below chance.

Data were collected on six different versions of PSITREK. There were two ESP versions and four PK versions. These different versions provided variety in the mode of participant response and helped to maintain novelty.

The overall results for the 443 sessions completed are significant by Z-score ($Z = 2.34$, $p = .019$, two-tailed) and single-mean t-test ($t = 2.32$, 442 df, $p = .021$, two-tailed) with essentially identical scoring rates for the ESP and PK modes of PSITREK. The ESP effect was contributed almost entirely by the precognitive sessions ($Z = 1.94$) while the clairvoyant ESP sessions yielded chance results. The ESP effect was contributed entirely by Source-1 ($Z = 2.82$), with slightly negative scoring on Source-2 ($Z = -.43$). The paired output of both random noise sources in the PK versions produced positive but not independently significant results ($Z = 1.70$).

The experimental results were simulated in 8,000 control sessions without participants with results that closely approximate chance values.

The nearly identical scoring rates in the ESP and PK versions of PSITREK support the hypothesis that similar processes underlie ESP and PK.

The isolation of the psi effects to Source-1 in the ESP modes of PSITREK is uninterpretable at the present time because it reverses the results obtained in our PK graphics study also reported. This discrepancy can only be resolved through further work comparing the two diodes.

PERCEPTUAL CORRESPONDENCE IN A HOUSE WITH RECURRING PHENOMENA

Joan Krieger (Psychical Research Foundation), Donna McCormick (American Society for Psychical Research) and Michael Luthman (Ohio State University)

In August 1978, J. K. received a telephone call from two women who wanted an explanation of some unusual occurrences in their newly acquired, three-story brownstone in New York City. Among the phenomena were the disappearance (sometimes later re-

appearance) of objects, hearing music when none was playing, the sensation of a furry animal brushing against their legs, the inexplicable sound of glass breaking, electric lights on two stairway landings flashing on and off. The owners once saw a "rush of white"--a line curved in a 75° arc--moving across one of the rooms in the house. A painter reported being pushed down the stairs by unseen hands. Two weeks later, another painter reported the same thing, though he had not been told about the events. A friend of the owners had also witnessed several occurrences.

The authors decided to attempt a replication of the earlier studies of Schmeidler (JASPR, 1966, pp. 139-49), Moss and Schmeidler (JASPR, 1968, pp. 399-410), Maher and Schmeidler (JASPR, 1975, pp. 341-52) and Solfvin and Roll (JP, 1978, pp. 63-64) to determine if the responses of a group of sensitives would show any similarity to the experiences reported by witnesses or to the responses of a control group.

Ten persons (five controls, five "sensitives"), naive to the reported activity, walked through the house separately and recorded their impressions on questionnaires. To minimize influencing subjects' impressions, D. M., who knew nothing about the case at that time, coordinated the visits through the house at times when the residents were absent.

The questionnaire packet given to the subjects and witnesses (the two owners and their friend) included scale diagrams of each of the three floors of the house. Respondents were to mark the areas where they believed something paranormal had occurred. A "blind" assistant later divided blank copies of each floor plan into 40 boxes of equal size, prepared transparent overlays for each diagram then scored subjects' and witnesses' responses. Scores were entered into two-by-two tables to measure the correspondence between subjects' and witnesses' reports. Other forms in the packet included a list of 79 terms describing possible physical characteristics of the "ghost," a checklist of 40 action words for possible activities of the "ghost," and a 62-item sensory-impression checklist. Respondents were instructed to circle appropriate items and crossout nonapplicable ones. As with the floor plans, each subject's responses were marked against the witnesses' combined report by placing them in two-by-two tables.

Floor plans. Each subject's responses were compared with witnesses' reports for each floor plan. One psychic yielded significance agreeing that six areas were "active" and 26 were not ($N = 40$): chi-square = 6.58, $p < .01$. Subjects' data were pooled for each floor but showed no significance (neither did the data of all sensitives or all controls taken together). No correspondence was found when responses of controls were compared with those of sensitives for each floor.

Subjects' versus witnesses' responses for all three floors pooled together ($N = 120$) produced no significance. Tests showed

no correspondence between sensitives and controls for all three floors combined.

Checklists. Only the responses of one psychic (no controls) produced significance when compared with witnesses' reports on any of the three checklists: chi-square = 6.498, $p < .011$, agreeing that three of the physical characteristics were applicable and 61 were not (N = 79). No significance was obtained when we looked at subjects' data pooled together in any of the three possible ways. Tests for correspondence between responses of controls and sensitives for each checklist: chi-square = 19.9, $p < .00001$ (agreeing that 28 physical characteristics applied, 31 did not), chi-square = 5.0, $p < .025$ (agreeing 10 activities applied, 18 did not; N = 40), chi-square = 10.0, $p < .002$ (agreeing 16 sensory impressions applied, 28 did not; N = 62).

Pooled responses to all three checklists (N = 181) for each individual subject: one psychic did not concur with witnesses on any of the applicable items but agreed on 130 of the nonapplicable ones (chi-square = 3.52, $p < .05$). The rest were all nonsignificant, as were subjects pooled and data of controls and sensitives as individual groups. A comparison of responses of controls with those of sensitives for all three checklists (N = 181) showed they agreed on the applicability of 54 items and that 77 were not applicable (chi-square = 33.48, $p < .00001$).

Data from checklists and floor plans were pooled (N = 301) for each subject individually. Two psychics and one control showed correspondences with witnesses' reports ($p < .03$, 01 and .06). Responses from the other seven subjects were not significant, nor were they when we pooled the data for all subjects, all controls and all sensitives. However, a test between controls and sensitives for all forms taken together (N = 301) demonstrated agreement that 69 items were appropriate and 115 were not (chi-square = 17.8, $p < .00003$).

The tests comparing controls with sensitives demonstrated a high degree of correspondence on the checklists. Such factors as personality, environmental cuing, attitude and psi ability might have had an influence. We would suggest a greater attempt be made in future investigations to obtain a more detailed profile of subjects, perhaps implementing formal selection criteria (e.g., scores on ESP tests).

In conclusion, the experiences in this home were undoubtedly real to the occupants. As to whether or not any of the phenomena were of a paranormal nature, no clear evidence was obtained.

STUDIES OF THE DIFFERENTIAL EFFECT IN LANGUAGE ESP TESTS

Shanti R. Krishna and H. Kanthamani (Foundation for the Research on the Nature of Man)

Rao hypothesized that the differential effect (DE) occurs only when the subject perceives a contrast between two conditions. Two series of tests were carried out to see whether the DE between Telugu and English would manifest only in the dual-target condition (DTC), i. e., when both the sets of targets are presented together, and not in the single-target condition (STC), i. e., when only one set of targets is presented at a time. In both series, the subjects who were student volunteers were individually tested.

In Series I, there were 60 subjects (both male and female) who were randomly assigned to three conditions. In Condition I, the subjects attempted to match Telugu and English target cards against Telugu and English key cards. In Condition II, the subjects attempted to match English target cards with English key cards, while in Condition III, they tried to match Telugu target cards with Telugu key cards. All three conditions were randomly distributed and the subjects were randomly assigned to one of the three conditions through a procedure using the Rand Tables.

Using the standard blind matching technique, the subjects attempted to match a closed deck of target cards (50 in STC and 100 in DTC) against a set of key cards enclosed and sealed in black opaque envelopes by a colleague (J. M.). The words used in all three conditions were BALL, FISH, LOVE, PEACE and TREE and their Telugu equivalents. Appropriate instructions were given to the subject in all the three conditions.

The main hypothesis to be tested was stated as follows: "The DE would occur only in the DTC and not in the STC condition." It was planned to analyze the results in terms of considering the group as a whole and also by separating the group into boys and girls. The overall results indicated no noticeable difference in the two conditions. In the STC, the average per run for Telugu targets was 4. 9 and the average per run for English targets was 5. 13. In the DTC, the averages per run for the Telugu and English targets were 4. 9 and 5. 20, respectively. When the results were analyzed for males and females separately, we found that the male subjects showed greater DE than the female subjects in both STC and DTC, though none of the values were statistically significant.

Sixty volunteer subjects were used in Series II. This time, half the subjects were tested by S. K., who was the experimenter in the first series and the other half by H. K. The testing procedure was exactly the same and J. M. helped to randomize the key cards and keep the duplicate records. The results were analyzed as in the first series and the subjects' scores on the Telugu and English tar-

gets were compared separately for the two conditions. In the STC, the subjects obtained an average of 4. 81 hits on Telugu targets and 4. 92 on English targets. In the DTC they averaged 5. 28 and 4. 71, respectively. The difference between Telugu and English scores for STC gave a t of . 36, 19 df. For the DTC, the t value was 1. 53, 19 df.

The results were also analyzed in terms of sex differences. In the STC, there were no noticeable differences in the scoring rates of boys and girls. In the DTC, however, we found that while the scores of the boys showed no noticeable difference, the girls obtained average run scores of 5. 38 on Telugu targets and 4. 4 on English targets. The difference between these two scoring rates gave a t of 1. 75, 11 df. Though none of these t's are significant, the fact that the t's obtained in the dual-target condition are considerably larger than those obtained in the STC as predicted gives us some encouragement for carrying out further testing.

APPARATUS ERROR IN PK METAL-BENDING EXPERIMENT WITH JEAN-PIERRE GIRARD

R. D. Mattuck and Scott Hill (University of Copenhagen and Danish Society for Psychical Research)

At the 1976 convention of the PA, we reported (RIP 1976, p. 209) psychokinetic stretching of an aluminum bar bent by Jean-Pierre Girard (JPG). Our preliminary conclusion was based on the fact that the strain-gauge attached to the concave side of the bar gave a small positive voltage output indicating a stretching of the gauge (or bar plus gauge) accompanying the bending, instead of the usual large negative signal indicating compression.

An alternative explanation is that the gauge had "slipped" or "crept" relative to the metal bar, so that it did not follow the deformation of the bar. To test this hypothesis, we first did some elastic-bending tests on the specimen that JPG had bent. These tests showed that the gauge behaved normally, being at least able to follow small, nonpermanent deformations. The next logical step would be to see if the gauge could follow a large permanent bending deformation. However, this would have destroyed the specimen with regard to microphotography and strain analysis of the metal surface. Hence, we worked instead with 11 duplicate specimens specially prepared so as to be prone to slip. As described in our paper, mechanical bending of these duplicates produced the expected large negative voltage outputs, in contrast to the JPG specimen. Our preliminary conclusion was that slip could not account for our results.

After the microphotography and stress analysis was completed in 1978 (carried out by Dr. Jean Bouvaist, from the firm Pechiney-Ugine-Kuhlmann, in Voreppe, France), we performed an

additional permanent mechanical bending of the JPG specimen itself. To our surprise, the strain-gauge voltage followed the deformation only up to about two degrees of extra bend, at which point it dropped abruptly, and the final output was slightly positive, just as in the JPG case, and in complete contrast to all the other 11 gauges that we bent mechanically.

The conclusion we must reach is that our results with JPG were due to strain-gauge slip and not to PK.

However, it should be noted that it is a remarkable coincidence that the gauge given to Girard was the only gauge that showed anomalous slip behavior. That may indicate that this highly unusual defect in the gauge bond was brought about by JPG himself.

A FAILURE TO OBTAIN RESULTS WITH GOAL-ORIENTED IMAGERY PK AND A RANDOM EVENT GENERATOR WITH VARYING HIT PROBABILITY

Robert L. Morris and Victoria Reilly (University of California, Irvine)

At last year's convention, Morris, Nanko and Phillips (RIP 1978, pp. 146-50) reported significant positive evidence for PK influence over a noise-diode random event generator (REG) when subjects were asked to employ a goal-oriented imagery strategy to facilitate PK performance.

Subjects were 24 college-age students all drawn on a volunteer basis (no extra credit given) from R. L. M. 's introductory parapsychology class at UCI. The apparatus was the same one employed in the previous study, except that the display to the subject was a single LED that blinked on and off during the course of a run, in accordance with the decisions of a noise-diode-based REG. Each time S pressed a button, the LED would turn off and on with decisions made at the rate of approximately 50 times a second, for a total of 4,096 trials in the run. A run would thus last about eight seconds, during which time the LED would appear to glow with fluctuating brightness. For half of the runs, the subjects were asked to visualize the light glowing brightly (GLOW); for the other half, the subjects were asked to put their thumb over the light and experience darkness (DIM). There were eight runs per session, with a DIM run and GLOW run within each of four conditions. One-fourth of the time, the REG was set to activate the LED 50 per cent of the time by chance; the other three conditions were activated 25 per cent, 12.5 per cent and 6.25 per cent of the time by chance, respectively. Taking the eight runs per session in couplets, there are 24 possible ways of ordering these four conditions within the session. All possible ways were used, each assigned to a separate subject on a random basis (using the REG) before the study was started. Within each

couplet, the order of target conditions, DIM versus GLOW, was counterbalanced across conditions.

All sessions were conducted by V. R. in the Social Science Laboratory. The subjects were greeted in the waiting room and ushered by V. R. to Room 2, the equipment room, where the REG and display was shown to them and explained. The subject was then taken to Room 1, the subject room, and seated in an office straight-back chair in front of the display. V. R. left a sealed envelope with the subject that contained instructions for the session. All instruction had been prepared and randomized by R. L. M. in advance and sealed in envelopes bearing the number (one to 24) of the subject. The subjects were told they could initiate runs whenever they pleased, but to allow V. R. a couple of minutes to return to Room 2 and get set up. V. R. then returned to Room 2, opened her instruction envelope for that session and set the probability of activation level for the first two runs. After each two runs, the level would be changed according to instructions, until the eight runs had been completed. After each run, V. R. recorded the number of activations indicated by the automatic tally board on the equipment console. Since she was unaware of whether the target was DIM or GLOW, motivated scoring errors would be eliminated. At the end of the session, she brought the subject back to the equipment room, recorded the target order for that session and gave the subject rough feedback on results.

The results of our study were insignificantly above chance, and there were no meaningful treatment differences.

Our new procedure for enhancing the results using a goal-oriented imagery strategy therefore failed to meet our expectations and did not generate new information. Our later studies will explore other procedures.

RIGIDITY AND ESP

S. P. Mythili and P. V. Krishna Rao (Andhra University)

In the field of parapsychology, a good amount of research has been directed to studying psi correlated with unselected subjects. A few excellent reviews of research on personality variables have also appeared in recent years. Notwithstanding the low reliabilities and variations in some of the findings, the reviewers found a general affinity between certain personality characteristics akin to general adjustment of individuals and their psi ability. The conclusions of Mangan, Rao and recent observation of Palmer point out the role of adjustment to test situation as crucial for psi manifestation.

In a recent review, Palmer observed that people who are

relatively well adjusted and people who believe in ESP perform better on laboratory tests of ESP since they are more comfortable in the test situation and exercise the relaxed spontaneity necessary for high scoring. The conclusion, however, indicates that this relationship does not hold up in group situations of testing.

Schaie has developed the Test of Behavioral Rigidity (TBR), which measures the ability of the individual to adjust to various types of stress due to constant changes in the environment. A high score on TBR indicates low rigidity and low score high rigidity. The composite rigidity (CR) score of this test gives a general estimate of the individual's rigidity-flexibility. Schaie suggests that an individual's rigidity-flexibility can be described in terms of the following three dimensions: a) Motor-Cognitive Rigidity (MCR) measures the efficiency in dealing with symbolic or semantic type of restraint, b) Personality-Perceptual Rigidity (PPR) measures the efficiency in dealing with interpersonal restraints, and c) Psychomotor Speed Rigidity (PSR) measures the individual's efficiency in coping with restraints from physical objects.

In the present study, an attempt has been made to relate general adjustability of individuals as assessed by TBR and their performance on an ESP test. Although Palmer concludes that there is no relationship between adjustment and psi in group-testing situations, an attempt has been made to have group-testing situations in this study.

The subjects were 61 postgraduate students of Andhra University, Waltair, who were willing to participate at the request of the investigators and their students.

The ESP test was a clairvoyance test prepared by following the Anderson and White technique (JP, 1956, pp. 141-57). The target sheets were prepared by P. V. K. The target sheet for each subject contained five runs of standard ESP symbols in the card column of a standard record sheet. These symbols were written in the following way: the five symbols were assigned numbers 0 to 9, each symbol represented by two numbers. An entry point was determined in the random number tables of Kendall and Smith by throwing dice, and from that point onward the corresponding symbols of the numbers were entered from sheet to sheet. These sheets were numbered and each of these were kept in an opaque folder and enclosed in a brown envelope and stapled. An ESP record sheet was stapled to the outside of this target envelope. These envelopes with record sheets on them were handed over to S. P. M. by P. V. K.

Rigidity of behavior was measured using the Test of Behavioral Rigidity (TBR) devised by Schaie. Schaie defines the rigidity construct as a tendency to perseverate and resist conceptual change, to resist the acquisition of new patterns of behavior and to refuse to relinquish old and established patterns. The TBR has three sub tests. Each of these tests yields two or more scores. These scores are combined to yield three factor scores, viz., a) Motor-

Cognitive Rigidity, b) Personality-Perceptual Rigidity, and c) Psychomotor Speed Rigidity. TBR is suitable for adolescents and adults alike. The manual recommends individual testing for aged persons and people with low educational level. Since the subjects of the present study were postgraduate students with high level of education, group-testing procedure was adopted.

The TBR and the ESP test were administered to the subjects by S. P. M. in small groups consisting of eight to 10 individuals. The order of administration of the two tasks was balanced. For the ESP test, the instructions of Anderson and White were modified and given orally. The subjects were asked to write in the call column of the record sheet attached to the target envelope the symbols that they thought were present in the card column of the target sheet. Both the ESP test and TBR were scored after the end of the series. TBR was scored and checked by S. P. M. and the ESP test by P. V. K. The ESP scoring was double-checked by K. G., a staff-member of the Department of Psychology and Parapsychology with the assistance of V. K., a research scholar.

The following analyses were done: 1) Pearson product moment correlations were worked out for the relation between ESP and the Rigidity components; 2) the ESP scores of above- and below-mean groups on rigidity were compared; 3) the Rigidity scores of above- and below-mean groups on ESP scores were compared; and 4) from the distribution of Rigidity scores the first quartile (representing low-rigid group) and fourth quartile (representing high-rigid group) were compared for the ESP performance.

The correlations were found to be positive for all the rigidity components with ESP but were insignificant. The high and low rigid subjects did not differ significantly in their ESP performance. Psi hitters (who scored above 25) and psi missers (who scored below 25) did not differ significantly in their rigidity scores. High and low quartile groups on ESP scores also did not differ significantly in rigidity.

GANZFELD AND REMOTE VIEWING: A SYSTEMATIC COMPARISON

John Palmer, Thomas Whitson and David N. Bogart (University of California, Davis)

Two free-response ESP testing protocols that have compiled successful track records are the "Ganzfeld" and "remote viewing." While each method has its advocates, no one heretofore has attempted to compare them systematically.

The methods differ on two basic parameters: type of target and induction of an altered state of consciousness (ASC). Targets in most Ganzfeld studies consist of artistic reproductions located

near the subject, while remote-viewing targets are natural or architectural features located some distance away. Subjects in remote-viewing studies customarily are told simply to relax, while the Ganzfeld is an explicit attempt to induce a hypnagogic ASC by means of perceptual deprivation. These parameters were manipulated in a two-by-two factorial design.

We consulted conventional wisdom to define a subject population likely to produce positive results. To the degree possible, we selected persons who were a) friends of T. W. or D. B., b) in creative or artistic professions and c) in our admittedly subjective judgment well adjusted and having positive life experiences. Ten subjects were assigned quasirandomly to each of the four conditions, constrained by the necessity to reconcile their schedules and the days the agent needed to be in Davis (where subjects were tested) or San Francisco.

We employed two target pools: a) 12 distinctive locations (primarily architectural) in San Francisco and b) half of the Maimonides binary target system, consisting of 512 colored slides. A target was selected by J. P. for each subject individually by means of a random number table.

The agent (D. B.) could meet with the subject prior to the session only in the Maimonides slide (M) conditions, a potentially confounding factor in the design. In this condition, D. B. was located in a room two doors down the hall from the subject and looked at the target slide through a viewer off and on during the reception period. In the remote site (R) conditions, J. P. contacted D. B. by phone 45 minutes before the reception period, telling him the target location. D. B. arrived there at approximately the beginning of the reception period, remaining 15 minutes and interacting with the environment as much as possible.

Subject sessions lasted 30 minutes. Half of the subjects were given a standard Ganzfeld induction (halved ping-pong balls placed over the eyes illuminated by white light; white noise channeled in through headphones) while seated in a reclining chair (G), whereas the other half were simply told to relax normally with eyes closed and the chair in an upright position (N). During the first 15 minutes (reception period), they were asked to observe and report imagery of the target. T. W., who was always blind to the target, stayed in the subject's room transcribing his or her verbal report. Immediately following the reception period, subjects were asked to draw pictures of their imagery and to fill out a rating scale describing their experiences and expectancies during the session.

Following the session, J. P. showed the subject the target slide (or a slide of the remote target). In the R Conditions, the subject also talked to D. B. by phone. (T. W. was not present at the debriefings in the R conditions so as to remain blind to this relatively small target pool.)

After testing was completed, an outside person divided the 12 remote sites into three sets of four each. Likewise, three control slides were selected for each Maimonides slide by a standard algorithm. In both cases, the idea was to maximize within-set diversity. A different pair of judges independently blind-rated the M and R transcripts, respectively, against each of the four members of the appropriate judging set on a 0 to 30 scale. Judges in the R conditions visited the sites to make their judgments. Practical constraints prevented us from using the same judges for all trials, but we tried to select judges with comparable skills in evaluating the respective types of target material. The ratings were converted to Z-scores, and the two scores for each trial were averaged to yield the final ESP scores.

The overall mean ESP score was nonsignificantly below chance (Mean = .12). A two-by-two analysis of variance assessing the two experimental manipulations (target type, ASC) yielded no significant effects. Friends of the agent scored no better (-.19) than other subjects (-.02). These results allow no conclusions about the relative efficacy of the Ganzfeld and remote viewing or of the particular elements of these procedures we manipulated.

The rating scale was factor analyzed separately for the G and N conditions, with five orthogonal factors extracted from each. Only the G conditions yielded a factor structure similar to J. P.'s previous Ganzfeld experiments. Prior to examining correlations with ESP scores, one factor was selected from the G matrix that most closely resembled the "ASC scales" of the author's earlier studies. Because the overall ESP mean was below chance, a negative ASC-ESP correlation was predicted. The correlation was negative ($r = .33$). Although nonsignificant, it was the largest of the 10 ASC-ESP correlations computed from the two matrices.

Finally, subjects in the G conditions evidenced significantly higher variance of their ratings of the success of the ASC "induction" than did the N subjects ($F = 2.94$, 17, 16 df, $p < .025$, two-tailed). The means, however, did not differ significantly.

A PET COMPUTER FOR THE PARAPSYCHOLOGY LAB

David T. Phillips (The Information Connection, Santa Barbara)

Microcomputers costing $600 to $2,500 have become important tools for conducting laboratory experiments. The most popular microcomputers include the RadioShack TRS-80, the Apple II, the Commodore PET and the new Texas Instruments TI-99/4. The PET has been used for parapsychology studies at the University of California at Santa Barbara, by Robert Morris at Irvine, by John Palmer, John F. Kennedy University in Orinda, Calif., and by Helmut Schmidt at the Mind Science Foundation in San Antonio.

All of the microcomputers mentioned above, and many others as well, have similar computational power. The color display capability of the Apple and TI-99/4 may be of particular interest for some applications. Special features included with the PET are: real-time clock, IEEE 488 bus control and a user port of eight input/output lines that can easily be used to control experiments and record data. The user can easily utilize machine-language programs on the PET, a feature that makes the PET faster for special jobs than the inherently more powerful TI-99/4. For complete information on PET applications, contact Channel Data Systems, 5960 Mandarin Dr., Goleta, CA 93017.

Our PET has been used for random number generator PK studies with preliminary success both for gifted subjects and in longer experiments with a group of college students. The PET was used to produce random numbers, control shock apparatus and record the position of a mouse in an animal-precognition experiment. Remote keyboard and display circuits have been developed. Interface circuits that can be constructed from low-cost parts are available on request from the author. Listings of sample programs are also available.

PSI SCORING ON INDIVIDUAL AND GROUP TARGETS BEFORE AND AFTER MEDITATION

William G. Roll, Gerald F. Solfvin and Joan Krieger (Psychical Research Foundation) and David Ray and Lee Younts (University of North Carolina, Chapel Hill)

During the past five years, the PRF has explored experiential and psi aspects of meditation to test the theory that individual personalities are joined in psychic fields or structures.

Two types of ESP have emerged, the preferential effect and group psi. In this study, we focused on the latter. We used a dual-target design, one aspect of which was the same for everyone in the session while the other was generated individually.

The experimenters were part of the group of meditators of five men and five women aged 21-60. The group met daily for 10 days for half-an-hour's meditation. Each day, each participant completed a seven-item Presession inventory concerning the person's feelings that day, a 14-item Postsession I list on the quality of that day's meditation and a Postsession II list with the same seven items as the Presession.

Each question was answered by marking one of nine response boxes corresponding to the intensity of the feeling, e.g., Item 4 in the Presession (and in Postsession II) was on a continuum from "I feel very free and at ease" to "I feel very anxious and upset."

The three questionnaires also served as ESP response sheets. For each item, we divided the column of nine blocks into parallel columns (separated by the numbers 1 to 9). One ESP response was made by marking the left or right column, and a second ESP response was made by entering a cross or check mark. E. g., if the participant rated his or her feelings a "3" on a given item, either the right or left box of that item would be marked with a cross or square. The target sheets were copies of the three questionnaires, the targets being indicated by lines drawn through either the left or right column of each item and by a cross or check written above the item. The selection of sides and marks was determined by the Rand table.

Group and individual targets were randomly assigned to the left-right or cross-check aspect for each day so that group targets were right-left and individual targets cross-check five days and vice versa five days. On any given day, each subject would thus have one set of targets corresponding to everyone else's (the group target for an item might have been, for everyone, a check) as well as an individually randomized set of targets (the target for a particular meditator for that item might have been the left column). The target sheets were in opaque sealed envelopes with the response sheets stapled on top.

Another target difference was introduced accidentally. W. G. R. expected each sheet to be in its own envelope to reduce target confusion, but discovered on the first day of testing that the batch prepared for the first five days was not separated in this manner. The three target sheets (Presession, Post I and Post II) for each participant for a given day had been placed in a single envelope. We decided to keep these targets and to compare the results with those for the last five days, where we used separate envelopes for the three target sheets.

To maintain an atmosphere of freshness, the meditation was varied each day and the participants were encouraged to take a playful attitude to the ESP aspect.

We hypothesized 1) that meditation would enhance group psi scoring so that a) there would be more psi-scoring on group targets after meditation than before meditation; and b) group targets would elicit more psi than individual targets. We also hypothesized that 2) there would be less psi-contamination or confusion between the separate target sheets than between contiguous ones so that the last five days would show more evidence of psi than the first five.

We planned separate analyses for group versus individual and for cross-check versus left-right for each of the three sessions. Hypothesis 1a was supported for the cross-check targets by a significant positive deviation in Postsession II ($Z = 2.06$, $p < .05$) a significant difference between this and a slight negative score on the Presession cross-check group targets ($Zdiff = 2.21$, $p < .03$). Hypothesis 2 was supported also for the cross-check targets (indi-

viduals and group) with a significant deviation for the last five days ($Z = 2.35$, $p < .02$). The analyses of the relation between the questionnaire items and ESP have not been completed. We plan to continue to explore meditation as a means to activate psi interactions in group settings.

TWO LONG-DISTANCE ESP DRAWING EXPERIMENTS BETWEEN AUSTRIA AND ICELAND

Michael A. Thalbourne (University of Edinburgh)

On Saturday, October 8, 1977, a long-distance test of ESP was conducted by Erich Mittenecker and Günter Schulter of the University of Graz, in collaboration with Erlendur Haraldsson of the University of Iceland. The experiment had been first suggested to each party by Stefan Donaczi--a 17-year-old high school student who claimed to possess the ability to communicate by telepathy.

Prior to the test, E. M. and G. S. randomly selected 20 colored pictorial targets from a number of children's games, making sure that each picture displayed just one easily recognizable object. The targets were given English labels, and were divided into two sets of 10, one set to be used as the actual targets and the other as controls.

The experiment began at precisely 11 a. m. GMT, and consisted of 10 trials: each target was viewed in Graz by a pair of agents simultaneously (Donaczi, and one Frau Roth) for a period of one minute, and after each viewing there was a rest interval of two minutes. At the same time, seven male subjects in Reykjavik were attempting, by means of line drawings, to reproduce the targets being viewed by the agents, some 3,500 miles away in Austria. These subjects had previously participated in a DMT-ESP experiment and had obtained scores that were above chance (though not significantly so). In the present study, they sat each at a separate table in four or five adjacent rooms, and their instructions were to draw and make a short description of each target.

E. M. and G. S. have carried out a Carington-type analysis, in which blind judges scored each response-drawing against each target as either a hit or a miss. Their conclusion was that there was no evidence for a greater number of hits on actual targets than on controls, either as regards simultaneous hits or displacement.

Independently, however, the author carried out an evaluation of the data by means of a ranking method, in which the pool of 20 targets was ranked for similarity against each of the 70 response-drawings. No evidence was found for overall psi-hitting or missing: the mean rank was 10.6, a nonsignificant negative deviation from MCE of 10.5. A novel method of displacement analysis was employed,

in which the grand mean of the rank-scores awarded to all 10 actual targets was compared with the grand mean rank of all the 10 control targets. The group results were nonsignificantly negative: grand mean rank for actual targets was 10.56, for controls, 10.44. One of the seven subjects did however obtain individually significant results: grand mean rank for actuals was 9.72, for controls, 11.28, paired $t = 2.86$, 9 df, $p = .019$, two-tailed.

Finally, a test for serial-position effect was made, using repeated measures analysis of variance, this being an effect that the author has occasionally observed in his 10-trial picture-guessing experiments (unpublished data). The results of this test were highly significant: the between-trials F-ratio was 3.598, 9, 54 df, $p = .00144$. Tests for trend, using orthogonal polynomial coefficients, indicated that the 10 trial-means conformed significantly to a linear equation ($F = 14.36$, 1, 54 df, $p < .01$), and to a quartic (W-shaped) function ($F = 8.071$, 1, 54 df, $p < .01$). There thus seems to be evidence that the scores of the group fluctuated between positive and negative to a significant extent, thereby producing a cancelation effect.

There are, however, a number of methodological uncertainties regarding this experiment. First is the possibility of a stacking effect, since all subjects simultaneously guessed at the same target-set. This may not in fact be an important problem, since inspection of the actual response-drawings shows that on four of the 10 trials all of the subjects produced drawings of different objects, and one of these trials happens to be the second, which gave the most extreme trial-mean. The second difficulty concerns the fact that the data were rank-evaluated by a sole judge. The independence of the rankings of the pool of targets can be jeopardized if the judge has any tendency to avoid assigning any target a rank of one for more than one response-drawing. However, it must be said that the author, as judge, was keenly aware of the possibility of this sort of bias, and made an especial effort to avoid it: in considering each response-drawing, he reminded himself to rank the pool independently of previous rankings, so that the same target selected as first choice for one response could also be first choice for any other response. The third difficulty is that the author had originally thought that the use of so large a ranking-pool as 20 targets would enhance the sensitivity of the ordinal scale (since it ranges from 1 to 20). However, practical experience of such a ranking procedure casts doubt on this assumption of increased sensitivity. It is quite difficult to see any similarity at all between the response and the last 10 or so of the targets in the pool; the consequent difficulty in discriminating between them will introduce unreliability into the scoring system (other judges may give very different rank-orderings), and also renders dubious the use of parametric statistics.

In view of these potential problems, the data have been reanalyzed in a somewhat less contentious way. Seven ranking-pools were constructed, each single pool consisting of the 10 responses produced by one of the seven subjects. The basic procedure was to

use a number of judges equal to the number of targets (namely, ten). Each judge ranked all the responses in a given pool against just one target, and evaluated each of the seven response-pools against a different target. (Thus, no judge ranked a given response-pool more than once, nor any pool against the same target more than once.) Two additional sets of 10 judges evaluated the data in precisely the same way, with the result that each response-pool was ranked a total of three times against any one target; the rank-scores so awarded were averaged, to yield average ranks, which may range from 1.00 to 10.00 with an expected mean of 5.50. (Kendall's W coefficient of concordance was calculated between the three rank-orderings for each of the 70 responses: in 47 per cent of these cases, W was significantly high, and the group mean W was 0.61.)

The results of this reanalysis seem to accord with the essential conclusions from the single-judge analysis. For instance, the group mean rank of 5.738 departs from chance by a nonsignificant negative amount. Regarding the test for serial-position effect, repeated measures analysis of variance gave a significant between-trials F-ratio of 2.0574, 9, 54 df, $p = .05$. While the linear trend was quite nonsignificant, the quartic trend was comfortably significant ($F = 9.006$, 1, 54 df, $p < .01$). In fact, the rank-order correlation between the 10 trial-means as yielded by the single judge evaluation and by the multiple-judge regime is +0.62 (one-tailed $p < .05$). The correlation between each individual subject's mean rank and his sum-of-ranks is +.92 ($n = 7$, one-tailed $p = .002$). For each individual response-drawing, the correlation between the rank awarded by the single judge and the average rank awarded by the three independent judges, is +0.56 ($n = 70$, one-tailed $p = .001$). So, the conclusion is that both types of evaluation have yielded evidence of a serial-position effect, and the data suggest that this effect conforms most closely to a W-shape.

An exact replication of this long-distance experiment was carried out on Saturday, February 24, 1979. Ten subjects altogether were used, eight males and two females, four of the males having taken part in the first long-distance study. The 100 response-drawings were rank-evaluated by the author under blind conditions, the ranking-pool being the 10 actual and 10 control targets. He was also kept blind as to the order in which each response-drawing had been produced. Additionally, each subject evaluated his or her own drawings by ranking the pool of 20 targets against each response.

The prediction was that the major result of significance would be a serial-position effect. However, no evidence whatsoever was found for such an effect, in either the author's or the subjects' evaluation. Thus, the prediction was not confirmed, and the effect has failed to replicate. However, there were some other significant results, which suggest that ESP may have been operating in the experiment even if not in the predicted fashion. For instance, according to the single-judge evaluation, the group showed a psi-missing effect: the mean rank was 11.68 (MCE = 10.50), $t = 3.077$, 9 df, $p < .02$, two-tailed. In addition, there was evidence for positive

displacement, inasmuch as the response-drawings tended to resemble the 10 targets actually used to a greater degree than the controls. Grand mean rank for actuals was 10.40, for controls 10.60, paired $t = 2.32$, 9 df, $p = .045$, two-tailed. The subject evaluation yielded nonsignificant group results, although the means were in the same direction as those for the single-judge evaluation. Nevertheless, one subject obtained significantly negative results. Her sum-of-ranks was 143 (i.e., a mean rank of 14.3), $Z = 2.057$, $p = .04$, two-tailed. Also, one subject showed significantly negative displacement: grand mean rank for actual targets was 11.41, for controls 9.59, paired $t = 2.43$, 9 df, $p = .038$, two-tailed.

Finally, analyses have been carried out in order to examine the similarity between the rank-scores awarded by the author and by the subjects themselves. The average Spearman correlation for the rank-orderings of the pool of 20 targets was +0.40 ($n = 100$), the correlation being positive in 92 per cent of the cases. For each individual response-drawing, the correlation between the rank awarded by the single judge and by the subject was +0.42 ($n = 100$, $p = .001$, one-tailed) and for the displacement scores was +0.22 ($p = .013$). And the correlation between each individual subject's two sums-of-ranks was +0.18 ($n = 10$, p = n.s.), and between the total displacement scores was -0.21 (p = n.s.). Thus, while the overlap between the two sorts of evaluator was in general positive, it was not sufficiently great as to ensure that significances in the author's evaluation would also appear in the subjects' evaluation, and vice versa.

THE POSSIBLE VALUE OF REPEATED VISITATION IN GROUP TESTING

Debra H. Weiner, JoMarie Haight, Melissa Dru Marion and R. Jeffrey Munson (Institute for Parapsychology, FRNM)

In considering possible factors for the lack of success in a previous series of group tests, we speculated that testing during the first (and only) visit to a group might not adequately allow for the development of a psi-conducive subject-experimenter interaction. This brief does not specifically test this idea but rather represents an example of an alternative procedure.

The study took place during the participation of the first three authors in a "minicourse" program at a local elementary school. Students had previously chosen two minicourses and met with one group in the first hour of the weekly sessions and with the other in the second hour. In our minicourse ("Mind Bogglers"), there were nine fifth- and sixth-grade students (five males and four females) in the first group and 10 fourth and fifth graders (six males and four females) in the second. The first two sessions were spent in discussions and informal test demonstrations in order to create

the desired rapport; formal testing was planned for the final session. All tests, with the exception noted below, were analyzed separately for each group.

Session 1. An informal competition between males and females was set up using the "Keep It Going" machine, a white-noise-diode random number generator (RNG) that "counts" upward from zero on a digital display, with the a priori probability of stopping at each number equal to 1/64. The task was described as one of precognition in which the subject used a switch to connect and disconnect the display from the RNG if he or she felt that the RNG was about to stop the counting process, thus keeping the numbers "going" as high as possible. Although the females of Group 1 tended toward hitting (CR = 1.90) there was no significant difference between their scores and the males' in this group, nor were significant results found in Group 2.

Session 2. This session focused on PK, and a tray of rye seeds was brought in for a discussion of PK on living systems. The subjects were told that the computer would randomly select control seeds, seeds for the boys to influence and seeds for the girls to influence during the subsequent week. Separate target seeds for the two groups were not created; thus, the boys of both groups attempted to influence the same seeds and similarly for the girls. After two weeks of growth, each sprout was measured by R. J. M., who was blind to its classification as control or experimental. The mean length of the boys' seeds was 5.2 mm and of the girls' seeds 4.7 mm as compared with 6.9 mm for the control seeds. These data showed missing (boys versus control, $t = -1.47$, 254 df, p = n.s.; girls versus control, $t = -1.90$, 254 df, p = n.s.), which, when pooled post hoc, was significant ($t = -2.00$, 382 df, $p < .05$, two-tailed).

Session 3. The formal testing of the study consisted of two tasks, both with the sex-competition theme, using a four-choice RNG interfaced to a KIM microprocessor such that complete trial-by-trial data could be recorded on cassette tape for later analysis. First, subjects tried to miss the correct target and stopped when they obtained a hit. Each subject participated in three attempts. Comparing the boys' and girls' hits to misses by a chi-square test did not show significant results for either group.

For the last test, the culminating effort of the study, subjects were asked to predict the targets correctly. The competition between the sexes was emphasized, but, in addition, prizes were offered to the highest scorer of the group. In Group 1 each subject completed 40 trials; this number was reduced to 20 for Group 2 since the attention span of these younger children would be burdened by the longer task. Even with the small sample sizes, significant scoring differences between the sexes were found in both groups (Group 1: $t = 2.54$, 7 df, $p < .05$, two-tailed; Group 2: $t = 3.19$, 8 df, $p < .02$, two-tailed). The boys of Group 1 scored above chance and the girls below, while in Group 2, this trend was significantly reversed ($p < .01$).

These results are encouraging in the light of previous difficulties in obtaining significant results from group testing. They are in line with (but do not necessarily prove) the expectation that repeated visitation can be a successful procedure, particularly when combined with other methods, such as the awarding of prizes, to enhance motivation. Since the direction of the scoring differences between the sexes of the two groups was not consistent, such differences are interpreted as responses to the competitive situation and not as inherent differences in psi ability. The significant reversal of these scoring differences is discussed in terms of clinical impressions of subjects' attitudes and behavior during testing. These results support other successful experimentation with younger subjects.

PARAPSYCHOLOGY AND ITS CRITICS: IMPLICATIONS FOR PHILOSOPHY AND SOCIOLOGY OF SCIENCE*

A REPLY TO EDWARD GIRDEN

Charles Akers (Institute for Parapsychology, FRNM)

Parapsychologists tend to overstate the case for psi. Hence, scholars who seek a balanced introduction to the field must familiarize themselves with critiques of the research. Unfortunately, there are no careful critiques that adequately survey the entire field. C. E. M. Hansel's ESP: A Scientific Evaluation (1966) is often cited in this regard, but this book dealt with only a few experiments and relied almost exclusively on an a priori assumption that psi cannot occur. Edward Girden's 1962 Psychological Bulletin review of PK research is another frequently cited source. Although Girden did raise some legitimate criticisms, he spent most of his attack on minor experiments that no one had ever cited as evidence of psi. In addition, Girden employed polemics that were rather far removed from careful scientific argument.

In a more recent critique (Vol. 10 of the Handbook of Perception), Girden attempts to show why parapsychology has failed to gain scientific acceptance. To Girden, the answer is simple. The parapsychologists are a group of credulous pseudoscientists, whose views are in conflict with all of established science. They are "true believers," who refuse to acknowledge evidence of fraud, even when the evidence amounts to conclusive proof.

Perhaps to correct for this, Girden makes his own accusations of fraud, without, however, providing much supporting evidence. Thus, he asserts that fraud was "unequivocally established" in three classic experimental series: Pearce-Pratt, Pratt-Woodruff and Soal-Goldney. But the critics whom Girden cites never made so strong a statement. Hansel claimed only that the Pearce-Pratt series did not exclude the possibility of fraud (of which there was no evidence). In their analysis of the Pratt-Woodruff experiment, Medhurst and Scott (JP, 1974, pp. 163-84) claimed only that fraud was a probable explanation of the obtained results.

*Organized and chaired by K. Ramakrishna Rao, Institute for Parapsychology, FRNM.

In the case of the Soal-Goldney experiments, Scott and Haskell (PSPR, 1974, pp. 43-111) did claim to have made "rather a strong case" for fraud. The continuing controversy over Soal's research led to Markwick's recent investigation (PSPR, 1978, pp. 250-78), which provided more definite evidence of fraud. If parapsychologists fitted Girden's profile of the "true believer" they should by now have either ignored Markwick's findings, or found some devious way of avoiding her conclusions. But this has not been the case. Even Soal's coexperimenter, K. M. Goldney, has acknowledged the strength of the evidence implicating Soal.

Girden's general contention is that parapsychologists tend to ignore or cover up allegations of fraud. In support of this thesis, Girden somehow manages to construe Rhine's exposé of Levy (JP, 1974, pp. 215-25) as a defense of a fraudulent experimenter. Rhine is portrayed as having defended all of Levy's work, prior to the exposé, as "authentic." But Rhine never made such a foolish statement. The adjective that Rhine used was "unacceptable," and he applied it to all of Levy's work, "published or unpublished, authored by him alone or jointly with others."

In another unsupported allegation, Girden refers to "pertinent sources of error" in a study by Fisk and West. The errors were supposedly suggested by the subject of the study, a Dr. Blundun. But in the source cited by Girden, there is not even a passing reference to the Fisk and West research.

If parapsychologists are inclined to overstate the case for psi, Girden seems determined to totally misrepresent it. Thus, he blandly asserts that Schmidt's findings have never been replicated. He also implies that there was not a single successful replication in the Stepanek research. Yet, the work with Stepanek (PASPR, 1973) represents an impressive series of strict replications, which were often carried out by visiting scientists who brought their own test materials.

REPLY TO PERSI DIACONIS

Edward F. Kelly (Duke University)

Persi Diaconis' recent critical attack on parapsychology research contains numerous major errors and misrepresentations. Referring primarily to anecdotal and non-experimental material rather than the actual experimental literature of the field, he repeatedly characterizes as typical of current research, situations and procedures which are in fact very atypical. He makes no effort to document sweeping and inaccurate generalizations about alleged inadequacies of the research, and he makes numerous incorrect and misleading statements about specific matters of statistical and experimental fact. Undisciplined attacks of this sort can only obstruct the development

of informed and rational discussion of this still controversial subject. Fortunately this and related issues will soon be explored in detail in the forthcoming volume, Science and Psi: The Pro and Con Debate (McFarland) edited by K. R. Rao.

WHENCE THE ENCHANTED BOUNDARY: CULTURAL SOURCES OF INTOLERANCE FOR PARAPSYCHOLOGY

Brian Mackenzie† and S. Lynne Mackenzie (University of Tasmania)

Parapsychologists sometimes claim that critics of the field are harsh and intolerant, carrying skepticism to the point of irrationality. W. F. Prince's The Enchanted Boundary is a well-known collection of instances in which critics, having stepped over the enchanted boundary separating parapsychology from ordinary science, lose their impartiality, fairness and critical acumen. Can such a tendency be explained? Sometimes it is suggested that overweening prejudice, either materialistic or religious, is what makes critics so hostile. The implication is that some disturbing influence robs critics of their usual rationality, and that if not thus disturbed they would discuss parapsychology in a much more balanced way.

We suggest, however, that this critical intolerance for parapsychology has deeper roots, grounded in the assumptions underlying the modern western confidence in rationality itself. Critics are often intolerant of parapsychology because it seems to be undermining these assumptions, undermining the conviction of rationality itself. Furthermore, we suggest, the critics are quite correct; that is just what parapsychology does.

The substantive assumptions central to the modern western confidence in the power of reason are embedded in the beginnings of the seventeenth century scientific revolution. The fundamental one, explicit in the writings of Galileo, Descartes and others, has been called the "reification of mathematics," the conviction that the world can be completely understood through, and only through, mathematics. A corollary is the distinction between primary and secondary qualities, whereby directly measurable and hence "primary" properties of objects (length, size, etc.) are physically real, while not directly measurable and hence "secondary" ones (color, smell, etc.) exist only in the mind of the observer. The distinction is necessary to permit the field of applicability of mathematical knowledge to be coextensive with the real world. As a result the secondary qualities, and by extension any other aspects of experience that could not be assimilated to a mathematico-physical view of nature, came increasingly to be considered separate from the physical world. They lacked physical reality and had, at most, a real status only in the mind. In this way both the methodological and the theoretical basis of the scientific revolution required an a priori conception of the world as a self-contained mathematico-physical

system, in which irreducibly mental qualities had a physically indescribable position, tolerable only if they were confined within individual organisms. This a priori conception was a condition of the intelligibility of nature.

Parapsychological phenomena are a priori impossible because they violate these a priori assumptions necessary to guarantee the intelligibility of nature. They depend on the causal efficacy of putatively irreducible mental or otherwise nonmathematico-physical factors outside the boundaries of individual organisms. It is irrelevant whether these irreducible and causally efficacious elements exist in some way separate from individuals (e. g., Mesmer's universal fluid), or wholly within individuals but with extra-individual causal significance (e. g., many accounts of telepathy). In either case, things that cannot belong in nature are acting in nature. Parapsychology and its forerunners thus constitute an affront, not primarily to particular scientific theories (which are modifiable), but to the common foundation of scientific theories, scientific method, and the enlightened commonsense view of the intelligibility of nature.

Serious more or less empirical involvement with the paranormal on this definition has flourished mainly since the Enlightenment, comprising what we label the "parapsychological tradition." It has largely been a direct and continuing reaction against this exclusion of uniquely mental or otherwise physically irreducible factors from the "real," including the physical, world. The parapsychological tradition has differed significantly from the religious, philosophical and mystical traditions, which have also frequently been incompatible with aspects of the scientific tradition. The first two were able to make gradual accommodation to the claims of science, maintaining their authority in a restricted domain. The third avoided detailed confrontation by basing its opposition largely on a monolithic worldview, incommensurable with the scientific one.

However, the parapsychological tradition has typically insisted that its claims are both methodologically and theoretically relevant to the scientific tradition, and has demanded substantial revisions to the latter. It has thereby been committed to detailed ongoing opposition to parts of the scientific tradition, as well as to the assumptions on which it is based and which have formed in turn the basis for a general cultural confidence in the power of reason. Under the circumstances, it is not surprising that parapsychology and its forerunners have often received apparently harsh treatment at the hands of critics. What may seem more surprising is that, with this background, it should ever have received any sympathetic hearing at all.

On the other hand, the depth of the parapsychological tradition's opposition to the assumptive basis of modern science guarantees that, should it become able to force acceptance of its claims, the achievement would have very great implications for both methodology and theory in the scientific tradition generally, as well as for their cultural offshoots. Unfortunately for the modern proponents of the parapsychological tradition, they have not been able so far to coerce

any such acceptance of their claims; since all attempts to do so face the evaluative resistance stemming from the a priori impossibility of such claims being valid, it is not clear what sort of demonstration would be sufficient for the purpose. But unless (or until) such an achievement is realized, the a priori unacceptability of parapsychology is unlikely to be significantly reduced, and parapsychologists should not expect, despite individual exceptions, to receive much more sympathetic general treatment from their critics than they have received up to now.

NORMAL EXPLANATIONS OF THE PARANORMAL: THE DEMARCATION PROBLEM AND FRAUD IN PARAPSYCHOLOGY

Trevor J. Pinch (University of Bath, England)

This paper examines the problem of demarcating genuine science from "pseudo science." It is shown that it is possible to turn the demarcation arguments which have been used against "pseudo sciences," such as parapsychology, against the fraud hypothesis--which is the principal normal counter-explanation for the parapsychological evidence. It is argued that the fraud hypothesis fails to be scientific on the grounds of replication, metaphysical bias, falsifiability and lack of theory. Since fraud is accepted and parapsychology rejected, the role of demarcation criteria in determining acceptable science is challenged. An alternative account of their role is presented. It is argued that the rejection of parapsychology rests on cultural differences which demarcation criteria serve to legitimate.

ON "THE SCIENTIFIC CREDIBILITY OF ESP"

K. Ramakrishna Rao (Institute for Parapsychology, FRNM)

In a paper entitled "The Scientific Credibility of ESP" Moss and Butler reject the case for ESP on six grounds. 1) The test procedures are so inadequately reported and the experimental designs are so informal that the evidence generated by parapsychological experiments cannot be regarded as establishing the existence of ESP. 2) Replication by a qualified nonsympathetic observer is essential before results should be accepted, and no such replication has been successfully carried out in parapsychology. 3) In order to believe in ESP we must discover at least one lawful relationship involving ESP, and there are no supposed "ESP laws" that cannot be accounted for more parsimoniously by existing psychological laws. 4) ESP is not in harmony with established laws and therefore it must be rejected. 5) We do not encounter ESP in the market place, therefore it must be spurious. 6) There is no need to have an open mind on the question of ESP if the evidence has not yet established it.

It is shown 1) that the reporting style and experimental design of good experiments in parapsychology are just as good as any in the behavioral sciences; 2) that replicability cannot be a primary criterion for demarcating the genuine from the spurious in every controversial area and that, in any case, parapsychological findings have, in a significant sense, been replicated; 3) that failure to find lawful relationships does not logically negate the existence of a phenomenon and that, in any case, there is sufficient evidence in ESP data to suggest such relationships; 4) that lack of perceived harmony with the "established laws" does not warrant the rejection of evidence and that, in any case, it is by no means certain that psi phenomena are outside the scope of all physical laws; 5) that the "marketplace" test is irrelevant to the question of the existence of a phenomenon if it fails; and 6) that the evidence for ESP is strong enough to compel an unbiased observer to take it seriously.

Moss and Butler argue that a) "the discovery of at least one lawful relationship involving ESP which cannot be explained more parsimoniously by an already existing psychological law" (p. 1069), is a precondition for the recognition of ESP and that b) this condition is not satisfied by the available evidence in parapsychology. This argument is fallacious on both logical and factual grounds. First, while the discovery of a lawful relationship involving a phenomenon logically entails the existence of that phenomenon, failure to discover such a relationship does not necessarily negate its existence. Second, Moss and Butler are incorrect in their assertion that there are no lawful relations in parapsychology, if we mean by lawfulness a degree of generalizability of results. A number of known relationships exist. It is recommended that anyone interested in a review of the experimental results bearing on this question read the Handbook of Parapsychology edited by Benjamin B. Wolman (1977).

Moss and Butler conclude that ESP "is nothing more than a thinly disguised form of essentialism, a reversion to a prescientific religio-mystical tradition. It relates, quite clearly, to the primitive practice of assigning causation to mysterious, impalpable, evanescent inner forces whenever the natural web of causation is not immediately apparent ... The deleterious effects of ESP beliefs may be obscure and delayed, but they are real and inevitable. The direction of human affairs based upon misconceptions must, in the long run, produce maladaptive and antisocial effects" (p. 1077). In the same blatantly rhetorical vein, it might equally well be said:

> Rejection of ESP evidence is a cleverly disguised form of naive materialism on which totalitarian systems are founded. It relates to the least defensible but most widely used form of repression of new and challenging ideas, a denial of anything that does not conform to the orthodoxy and the establishment, a tendency to reject a phenomenon whenever a cause-effect chain is not immediately apparent. The deleterious effects of such a philosophy are all too obvious in the history of civilizations. Moreover, it is a philosophy that cuts out the

roots of the values which we in free societies so solemnly cherish.

Of course, such arguments neither establish nor refute ESP; they only arouse sentiments which help further to confuse the issues. In the final analysis, the case for or against ESP rests with the quality of evidence one way or another. But the quality of that evidence can be judged best in an atmosphere in which the effects of prejudice and bias are minimized by strict application of scholarly discipline. The paper by Moss and Butler does not, in my judgment, contribute to informed scientific discussion of parapsychology. As a mixture of rhetoric, dubious philosophy of science, and false and misleading statements about particular matters of experimental fact, it may reinforce a skeptic who already had made up his mind and misinformed someone who is unaware of the state of parapsychology, but it hardly contributes to an objective and scientific settlement of the disagreements concerning psi.

"PSEUDOSCIENCE? OR PSEUDOCRITICISM?"

Theodore Rockwell (Chevy Chase, Maryland)

Legitimate scientific criticism is seldom accorded psi research by other scientists. Most criticism of the field has little to do with the subject. The critics usually describe a variety of folk beliefs (such as Bermuda Triangle and Astrology), cite fraudulent 19th-century spiritualists and more recent stage figures and leave the impression that their remarks apply to current research--which they have not discussed. Critics who do discuss the subject still take an entirely different approach from what they would apply to other areas of research. They assume, a priori, that psi phenomena do not exist, and thus the events described by the investigator must not have occurred. This leads to conclusions of fraud or self-deception, postulation of fantastic scenarios to avoid psi, cries for "tighter controls" imposed by a board of hostile magicians and finally to demands that psi research be "thrown out of the workshop of science" until "battle-tested evidence" is in hand.

Such pseudocriticism serves neither psi research nor science. It reveals a fundamental ignorance of both. The charge that psi research is not a science is semantically invalid; science is a process, not a subject matter. Science is not defined by fields of research but by techniques and approach--the "scientific method." This method may be applied to any postulated or apparent phenomenon, and if it is done competently, the investigation is properly called scientific.

The experiment is only a part of the chain of processes called science. Scientific "truth" is a temporary consensus arrived at by continuous interaction among scientists. This interaction is brought

about by publication. Therefore, a critical part of the scientific process is the procedure by which scientific papers are reviewed for publication. A similar process is used to evaluate research proposals for funding. This process, called "peer review," depends on the judgment of scientists knowledgeable about the field. Mal-operation of this part of the scientific process is even more harmful to science than a poorly run experiment, since it can introduce a continuing bias not subject to the natural correcting forces in the process. It is therefore suggested that all scientists feel and exercise a responsibility to examine and publicly criticize aberrations in the review processes with the same zeal they apply to reports of experiments.

Examples are given of rejection letters and reviewers' comments that deserve condemnation in their own right, regardless of any lack of merit in the papers being reviewed. It is suggested that this situation results from an a priori premise by some editors that peer review will not work in this field, and that special steps must be taken to find reviewers who do not "believe in" psi phenomena. Such a position is without merit and demeans science. If one believes in science, one will trust it to determine the truth concerning psi or any other phenomena in the physical world.

CRITICISMS OF PARAPSYCHOLOGY

Douglas M. Stokes (Avon, Connecticut)

Recent criticisms of parapsychological research by Paul Kurtz and H. B. Gibson have appeared in the Skeptical Inquirer and the Bulletin of the British Psychological Society, respectively. Both critiques cite examples of alleged sloppy experimentation and experimenter fraud in support of their cases. They also point to the lack of a repeatable psi experiment as evidence that the parapsychologists have not established their claims. Both authors remark that phenomena that have not been explained at a particular time (such as the bat's navigational abilities) are often asserted to be inexplicable in principle and hence "paranormal" by some researchers. They provide a psychological analysis of the reasons why parapsychologists may succumb to the temptation to resort to fraudulent practices, including the desire to uphold a religious or spiritualistic view of the world (Kurtz), the desire not to be seen as being taken in by a pseudoscience and the desire to further the scientific and sociological status of parapsychology (Gibson).

Several errors occur in these two critiques. Kurtz asserts that PK necessarily contradicts the law of the conservation of energy, whereas it need not do so under the modern observational theories (such as those of Schmidt and Walker). Kurtz suggests that psi-hitting and psi-missing may well average out to chance, but he neglects to note that a psi effect would still be detectable through a

variance analysis. He criticizes the vagueness of parapsychological theory in not distinguishing between PK and precognition. What this amounts to is a criticism of parapsychologists for their caution and desire not to theorize unduly beyond their data. He also grossly misspells the name of Eusapia Palladino (he calls her Eustasia), suggesting that he is criticizing a field with which he is not entirely familiar.

Gibson's errors are more errors of judgment than of fact. He uses the fact that Ingo Swann linked the SRI research to the Scientology movement (in a way which Gibson does not specify) to discredit these experiments, a guilt-by-hearsay-association tactic. He also states that the SRI research gives the "kiss of death" to parapsychology in that the experimentation was so good that fraud must be inferred! (This directly contradicts Kurtz's use of the SRI research as an example of sloppy experimentation.) He states that the accusations of fraud in parapsychology reflect badly on science in general and that it would have been better had scientists never entered the field. The implicit assumption that, were it not for parapsychology, science would be a fraud-free enterprise is an absurd one for anyone even minimally conversant with the history of science.

Part 5: Presidential Address

PARAPSYCHOLOGY AS A PROBABILISTIC SCIENCE: FACING THE IMPLICATIONS*

Being President of the PA during the past seven or so months has been a strange experience for me. It has not been strange because of anything that has happened to me or because of anything I have been asked to do, but for a more subtle reason. I have been a parapsychologist for nearly 10 years, but never before have I felt so close an identity with the field, with its past, its present and its future. This sense of at-one-ment, if you will, has caused me, almost in spite of myself, to spend many hours contemplating the current status of our field, where it has been and where it is going.

Tonight, I would like to share with you some of these reflections. I do so out of no sense of great wisdom or of having all the answers. I simply come to you with a perspective that has been conditioned by the life experiences that have brought me to this point. For some reason, whatever forces govern the universe have chosen this as the time and place for such a perspective to be articulated.

Last year, Dr. Rao gave us a moving address which I described in a somewhat notorious article in Parapsychology Review (Palmer, 1979), as "bidirectional." I meant by that adjective that the talk had two distinct and emotionally complementary elements. I have chosen to extend that happy precedent for one more year.

As I have reflected upon the current mood of parapsychology, I have noted a growing impatience with the capacity of the scientific method to provide us with answers to the riddles which nature has posed to us in the form of those events we collectively label as psychic. We have seen it erupt overtly in two symposia this year, and it is an undercurrent in much of the research and theorizing that permeates modern parapsychology.

It is this malaise that I want to address tonight, and I want to address it "bidirectionally." I think part of the malaise stems from a misunderstanding, or perhaps it would be more accurate to say a failure to fully appreciate the implications of that aspect of the scientific method which has traditionally guided "hard" parapsychological research. Although, even in this part of the talk, I will say things that might seem blasphemous to the scientific purist, the general tone will be conservative, a ringing defense of the status

*Delivered August 1979.

quo. Some of it will be technical (although I hope not overbearingly so), because those aspects of the scientific method which I think are causing our malaise can only be approached at a somewhat technical level. But I think we can all live through it.

During the second part of the talk, I will address that aspect of the malaise which incites us, or perhaps I should say inspires us, to a broader vision of the field of parapsychology. In this part of the talk, I will say things that may seem to some of the more traditional among you as radical, or even heretical. But only in a sense. It is my conviction that the radical and the traditional must coexist in a state of dynamic equilibrium--the yin and the yang, if you will. After all, isn't that what parapsychology has always really been about?

Despite the bidirectionality which I just outlined, my talk does have a unifying theme. This theme is perhaps most clearly represented, or symbolized, by quantum physics. Quantum theory has become something of a fad in parapsychology of late. On the one hand, some writers claim that it provides the long-awaited bridge between parapsychology and "respectable" science, that psi is totally compatible with the world of modern physics (e. g., LeShan, 1974). Others say that such a link is premature at best or naive at worst (e. g., Phillips, 1979), and one theorist on the leading edge of the "new physics," John Wheeler (1979), views us with utter contempt.

I respectfully decline to enter the debate. For one thing, as a psychologist not trained in the subtleties of physics, I don't feel competent to do so. Secondly, I don't think the issue is very important, at least not at the present time. Quantum physics is not of value to contemporary parapsychology because it gives us a real or imagined ally in the battle with our critics. It is not valuable because it as yet explains psi in any satisfactory way. It is valuable because it has inspired us to generate sophisticated models and conceptions of psi that are often radically different from those of the past. In short, its value has been primarily heuristic, and it is from this standpoint that I wish to approach it.

Part I: In Defense of Orthodoxy (More or Less)

Quantum theory has been of interest to parapsychologists primarily in terms of its assumptions about the possible causal efficacy of observation (collapse of the state vector) and possible effects backward in time (e. g., retroactive PK). While these have been important heuristics, I have always been more fascinated by one of the more basic assumptions of quantum theory: namely, that the universe, at least at the microscopic level, is probabilistic. We are told, for example, that an electron does not exist as a distinct particle in space and time, but only as a "probability wave." The universe is pervaded by an underlying indeterminacy. The laws of quantum physics are statistical laws, in contrast to the deterministic laws of Newtonian physics.

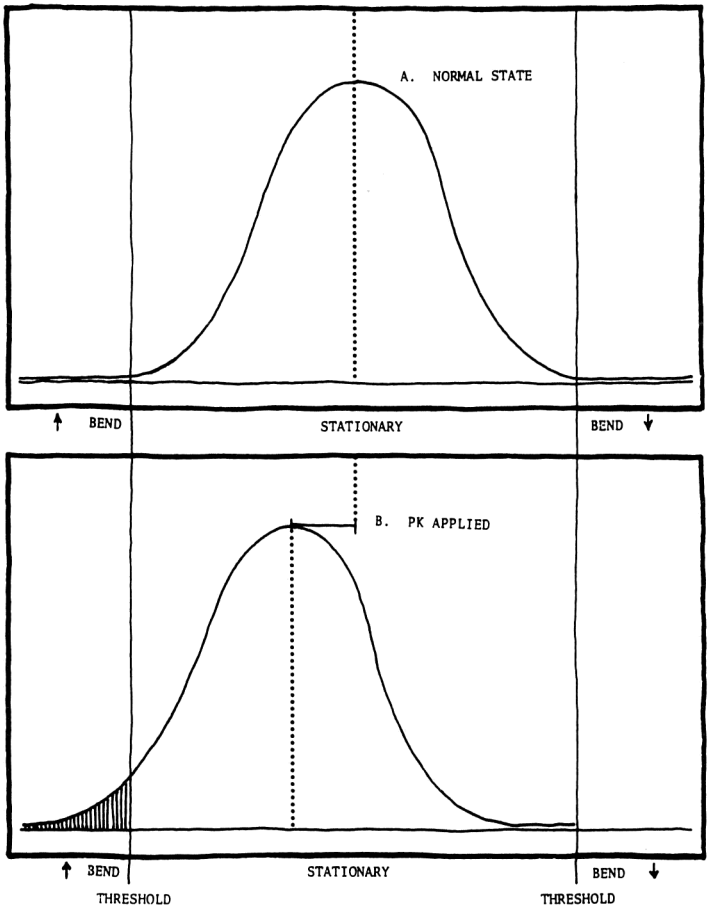

Figure 1. Hypothetical distribution of "quantum states" of atoms at the neck of a metal key under normal conditions (A) and under PK (B).

What implications might this type of thinking have for parapsychology? Again, let me stress that I am not proposing a "quantum theory" of psi, but using quantum theory instead as a heuristic device for the development of models that are appropriate for parapsychological data. However, as I have since learned, my ideas are similar to ones stated more formally by physicist Richard Mattuck (1977) as derived from quantum theory.

Some Probabilistic Models

Let's take macro-PK as an example. More specifically, consider that old classic: key bending. Assume that this normal

curve (see Figure 1) represents a distribution of possible states of the atoms at the neck of a key under "normal" conditions. Each of these states, in turn, represents the composite or average of the states of a large number of individual atoms. Let's assume further that, again under normal conditions, half of the potential states of each atom are such as to create a tendency for the key to bend upward, and half for it to bend downward. On the average, we would expect the number of atoms in each state to be equal, their vectors canceling each other out, leaving the key unbent. These states would be represented at the center of the curve, corresponding to MCE in parapsychological jargon.

Of course, the vectors will not always cancel exactly, but it would be absurd to assume that precise cancelation is necessary to prevent the key from bending. The vertical lines on the far left and right of Figure 1 represent a threshold; only those states outside the lines are unbalanced enough to actually cause the key to bend. If such bending were to occur (and the model assumes that in very rare cases it does occur), it would be classified as an isolated case of RSPK, in principle incapable of being predicted by a deterministic factor.

Next, let us assume that PK is applied to the key. As illustrated in Figure 1b, the effect of this PK, according to the model, is to bias the distribution of possible states in a direction favoring the bending of the key (in this case) upward. Note three things, however. First, although more of the potential states are now outside the threshold, the key does not bend every time. Second, the particular times the key will bend depend on the sampling of the underlying chance distribution of potential states: i. e., they are indeterminate and unpredictable. Third, however, superimposed on this indeterminacy is a determinate and partially lawful cause, PK, that biases the distribution of probabilities. It is this feature of order superimposed on an underlying chaos that I want to stress this evening. If I were responding to Einstein's (1971) famous quote questioning the fundamental nature of the statistical interpretation of quantum theory, I might say "God does play dice with the universe, but the dice are loaded."

Let us now see how a similar model might be applied to ESP. Here, I will draw upon ideas that have often been linked to quantum theory in the conceptualizations of some parapsychologists (e. g., Walker, 1974), namely the neurophysiological speculations of John Eccles (1977). Eccles suggests that ESP might be represented in the brain as a systematic structuring of the spontaneous firing of neurons. When I was at the University of California, Davis, a couple of years ago, a professor there was recording such spontaneous firing from a single neuron by means of a microelectrode implanted in the brain of a cat. As I watched the pattern of firing on the oscilloscope, I couldn't help thinking of a random event generator. The activity of the neuron at least looked like a random process.

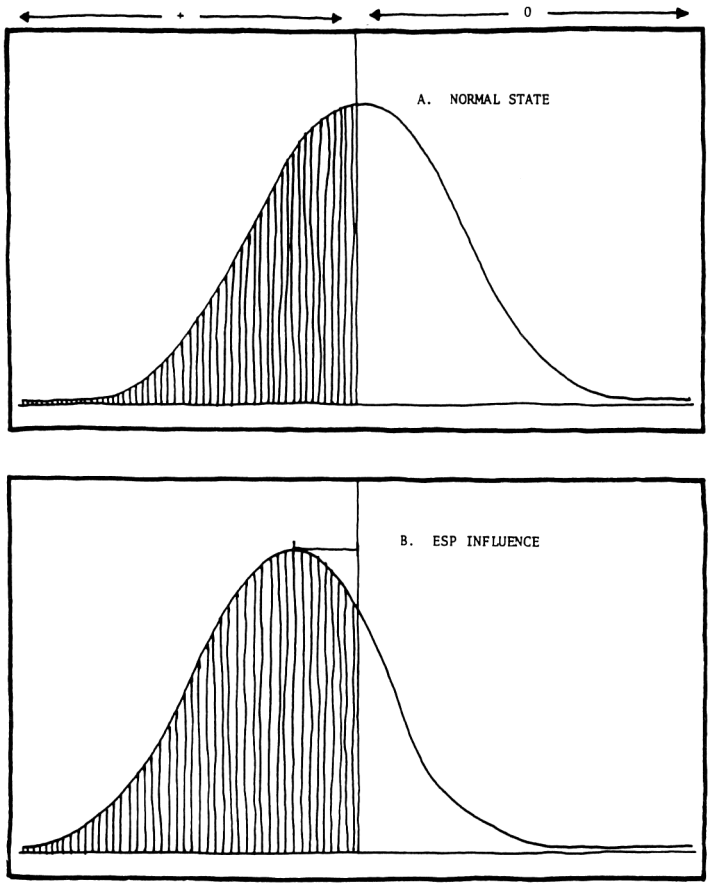

Figure 2. Hypothetical distribution of brain states favoring a "+" or "0" response on a two-choice Zener card test under normal conditions (A) and ESP influence (B).

Consider a subject taking a two-choice ESP test, as illustrated in Figure 2a. Assume a distribution of potential brain states, each consisting of the composite spontaneous firing of innumerable neurons. Assume further that under null conditions for a given trial, half of these brain states favor one response and the other half the other response. Under these circumstances, we would expect a hit rate of 50 per cent--chance.

According to the model, the effect of psi would be to bias the distribution of brain states on a particular trial in a direction favoring the target for that trial. But, again, note from Figure 2b that the correct response will not necessarily be favored, i. e., it is unpredictable whether on a given trial it will or will not be favored.

In practice, of course, the situation is further complicated by the fact that other factors besides psi influence the response. Such factors have been dealt with particularly by Rex Stanford in his applications of response bias and signal-detection theory (Stanford, 1975; Zenhausern, Stanford and Esposito, 1977). I will not dwell on these complications because they are not directly relevant to the point I wish to make, but that is not to deny their importance.

I am not claiming that these models are particularly novel, and I certainly am not claiming that they are necessarily correct. I do think, however, that models of this type are appealing, because they can account for the capriciousness of our phenomena while at the same time affirming their lawfulness. Such probabilistic models are also especially appropriate for our field, because we have traditionally treated parapsychology as a probabilistic science. Our almost total dependence on statistical methods of analysis is ample testimony to this fact. I say that with no apology--I think the capriciousness of our phenomena literally demands a probabilistic approach.

Implications for Evaluation of Evidence

At this point, I want to return to the malaise I mentioned earlier. I am sure that if I asked you point blank whether or not parapsychology is a probabilistic science, most of you would answer "yes." However, I sense that often we do not fully appreciate the implications of this fact in our day-to-day functioning as parapsychological researchers. Specifically, I think that some of the malaise I have been alluding to comes from a misguided application of the standards of nonprobabilistic science to probabilistic data. I will discuss two concrete examples of this: our attitude toward replication and our attitude toward post hoc analyses.

Replication. The traditional view of replication is to do an initial experiment. If there is a significant result, you or someone else repeats the experiment. If the effect is again significant, you claim the hypothesis is confirmed. If the effect is nonsignificant, you conclude that the hypothesis should be rejected, implying that the original result was a statistical fluke or the consequence of some (usually unspecified) experimental error or indiscretion. This process makes perfect sense for a nonprobabilistic science: if an effect is real, it should occur every time; if it doesn't occur every time, it isn't real. The process makes absolutely no sense, however, for a probabilistic science, like parapsychology.

The principal reason, but not the only reason, that the traditional view of replication makes no sense for parapsychology (and to varying degrees for other sciences as well) is, assuming psi is present at all in our data, that it is embedded in a great deal of random noise. Consider a simple Zener card test, for example. Our best subjects average no better than about six hits per run. Assuming an average of five chance hits per run, that means psi is effectively operating on an average of only one trial out of 25--four

percent.[1] A greater problem is the fact that the number of chance hits is not constant; it varies randomly. In a test of 100 trials (four runs), for example, the number of chance hits can vary from 12 to 28, assuming a confidence interval of ± two standard deviations. Assuming one ESP hit per run, the number of total hits can vary from 16 to 32, or from four to eight hits per run. While the yield of psi is considered in some circles to be greater in free-response than in forced-choice tests (e. g., Honorton, 1975), I think the same kinds of problems apply to both.

Let us analyze more closely the sources of random error that can enter into psi experiments. I will distinguish three categories. The first, which I illustrated with the above example, is simple sampling error. It should be apparent from the above example that an experiment can "fail to replicate" simply because the number of chance hits was less in the second experiment than in the first, assuming that the number of ESP hits is constant.

An example of where we sometimes fail to pay sufficient attention to sampling error is when we attribute failures to replicate to some kind of experimenter effect. Such causal inferences are sometimes made without first demonstrating that the two experimental outcomes differ significantly; the fact that one outcome is significant and the other not does not necessarily imply a real difference. If the outcomes do not differ significantly, we can say either that both were due to psi or both were due to chance (and there are rules for deciding between these two options that I won't go into here), but we cannot say that psi occurred in the first experiment and not in the second. The "difference" between the two outcomes is most properly attributed to sampling error, not to the relative psi-conduciveness of the experimenters involved or to some other difference in the experimental conditions.

This first kind of random error in psi data refers to the random variation in the number of chance hits. We also must consider possible random variation in the number of ESP hits. This randomicity is explicitly proposed in the probabilistic models I described earlier. For example, in the model based on Eccles's speculations, random variation in the frequency with which spontaneous brain states are biased toward the correct response over a series of trials, or blocks of trials of undetermined duration, will influence the number of ESP hits obtained. The same principle could also apply to PK data. (In fact, ESP, according to this model, really implies PK.)

Finally, random variations in the strength or effectiveness of the hypothetical psi source could interact with random variation of the brain state to increase further the variability of ESP hits. This assumption seems less compelling to me than the other two at the present time, but it is certainly worth considering as a possibility.

As if these various sources of random error are not enough of a burden for the traditional notions of replicability, we by no

means have been successful in controlling or even assessing the various causal factors that might differ from experiment to experiment. I could easily spend the rest of this address talking about the various possibilities suggested by the parapsychological literature, so I will limit myself to discussing the one I consider the most pernicious: the psychological state of the subject.

I consider this variable, or perhaps it would be more accurate to say set of variables, pernicious because they are so hard to get a handle on. For one thing, they are very difficult to measure. The assessment of people's subjective states is in a very primitive stage of development within psychology, let alone parapsychology. Most methods rely in the final analysis on subjects' verbal or written judgments. Such measures make the dubious assumption that most subjects are skilled enough at introspection to report their own states accurately. Even if they were, the so-called "demand characteristics" of the situation may prevent them from reporting these states with complete candor. Physiological indices and projective techniques help to overcome these problems, but at the price of providing only an indirect or superficial measure of the variables of interest. A composite of the two kinds of approaches is probably the best bet, but still far from ideal.

The other side of the coin is that most experimenters are unskilled in uniformly creating the intended kinds of psychological states in their subjects, despite the best of intentions. The social-stimulus value of experimenters and experimental environments vary widely, leading to many differences in psychological state factors that might be difficult to detect, either subjectively or by the imperfect measuring instruments discussed above. We have plenty of evidence from the research literature that such factors can affect ESP scores. Our relative inability to control these factors either within or between experiments is another source of variability in research outcomes.

The "bottom line" implication of all this discussion is that it is unrealistic to expect genuine psi effects to replicate successfully to anything even approaching the degree required by the assumptions of classical nonprobabilistic science. One should neither be surprised nor depressed if a first replication of a significant outcome is nonsignificant. On the other hand, it is no solution to say simply that replication is unnecessary and that the original finding should be allowed to stand on its own. The general principle of replication is central and indispensible to the scientific method. A failure to replicate in the traditional sense surely doesn't provide support for the original finding any more than it destroys it: it just doesn't tell us much of anything. What we need is an application of the principle of replication that is tailored to fit the needs of probabilistic rather than nonprobabilistic science.

Such an approach must derive from the premise that a series of experimental outcomes will be more or less randomly distributed about a central value. If, and only if, that central value deviates significantly from the null hypothesis can the experimental hypothesis

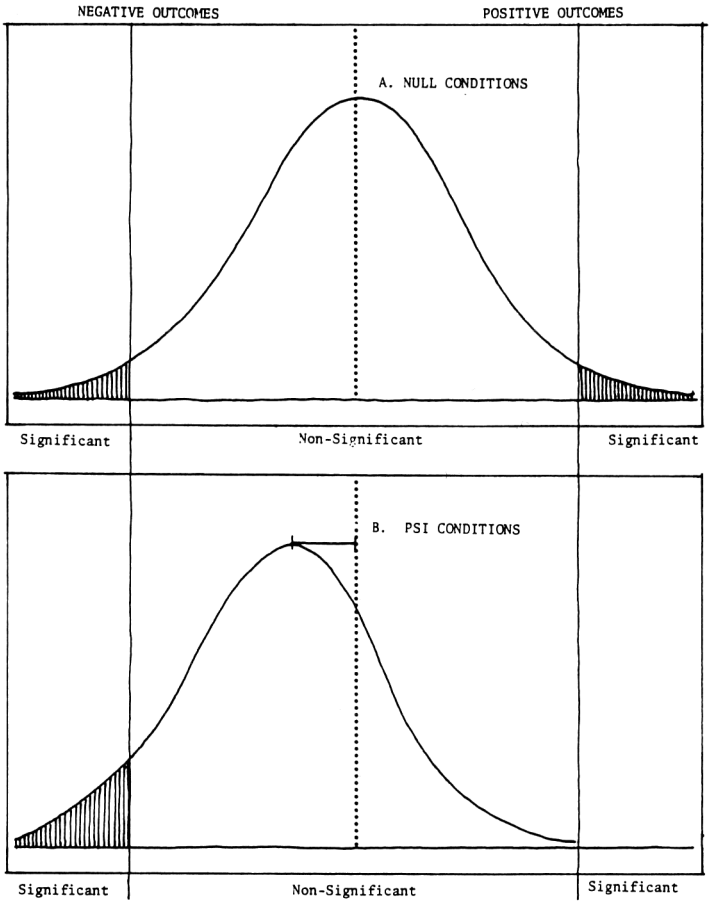

Figure 3. Distribution of experimental outcomes under null and psi conditions.

be considered confirmed. The model further assumes that only a fraction--often a small fraction--of the individual outcomes need to be significant for the hypothesis to be confirmed. This model is illustrated in Figure 3, which you will notice is similar to the representations of the theoretical probabilistic models I discussed earlier. In fact, they are based on the same underlying assumptions.

If this approach to replication seems strange to you, I might point out that its assumptions are identical to those we make in evaluating the results of individual experiments. In Table 1, I have listed hypothetical mean Zener card scores for 10 subjects. I have set it up such that the grand mean is significantly above chance, thus confirming the experimental hypothesis. Note also that only two of

Table 1

Mean Subject Scores per Run on a 100-trial, Five-choice Zener Card Test

	MEAN	CR
S1	7. 50	2. 38
S2	5. 50	----
S3	6. 50	----
S4	4. 25	----
S5	5. 00	----
S6	7. 25	2. 13
S7	5. 00	----
S8	6. 00	----
S9	6. 00	----
S10	5. 25	----
TOTAL	5. 83	$t = 2.54$, $df = 9$, $p < .05$

the 10 individual subject means are significant. I'm sure you will agree that this is not an unusual kind of result in a psi experiment--if anything is unusual, it is that as many as two of the individual subject means reached significance.

Now if you will look at the subject means instead as experimental outcomes from a group of experiments testing a common hypothesis, I think you will see the analogy. Conversely, it may be instructive to go back and look at the individual means as subject means and see what happens if one applies the logic underlying the traditional approach to replication. We would test the first subject and find significant evidence of psi. We would then test the second subject, find a nonsignificant result, claim a failure to replicate and prematurely reject the experimental hypothesis. This illustration is admittedly a bit farfetched, but I hope it will help you appreciate the basic absurdity of applying the nonprobabilistic view of replication to a probabilistic science.

By the way, I recognize that the validity of the method I propose depends heavily on how the experiments to be included in the

sample are selected. This issue of sampling bias will come up momentarily when I discuss post hoc analyses, so I would prefer to postpone that discussion until then.

Before moving on to post hoc analyses, though, I would like to discuss briefly how the above reasoning applies to our evaluation of functional relationships between psi and other psychological or physical variables. Such relationships in science are often called empirical generalizations or laws. In nonprobabilistic science, such laws are deterministic. Given X, Y necessarily follows. However, the laws of probabilistic science are statistical laws. They are of the form, given X, Y will follow Z per cent of the time. What this means in parapsychology, for example, is that if we can demonstrate reliably (within a given error of measurement) that "sheep" score higher than "goats" just 51 per cent of the time, we have as much right to claim the establishment of a statistical scientific law as any other scientists. Can we then say that the sheep-goat effect has been established as a statistical law? No, I don't want to go that far just yet. But it surely becomes more plausible to think of it as a law when we fully appreciate parapsychology as a probabilistic science.

Post Hoc Analyses. One of the favorite targets of methodological critics of parapsychology, both inside and outside the field, is what they see as the excessive stock placed by some parapsychologists in post hoc analysis (e. g., Diaconis, 1978; Wiklund, 1977). I have become one of the principal sinners myself, and I constantly marvel at the restraint you all have shown in not calling me on it, at least not publicly. But I figure that my luck will run out sooner or later so I had best get the jump on it and defend myself.

I am going to propose not only that post hoc analyses provide a fruitful source of ideas for future research (the usual apologia offered in their behalf), but that under certain circumstances we can actually draw conclusions from them. Now before you all rush out to initiate impeachment proceedings, let's examine the issue of post hoc analysis in light of the principles of probabilistic science we have been considering.

We can begin our examination by looking at the concept of prediction, which we might say is the converse of post hoc analysis, i. e., post hoc analyses are analyses made in the absence of prediction. What is the role of prediction in science? I contend that prediction has only one legitimate role in science--as part of the process by which we evaluate theories. A theory is a set of integrated concepts that purport to explain a set of observations or laws of nature. A good theory not only explains the observations or laws from which it was derived, but it also generates new observations and new laws, which can then be explained by the same set of concepts. The latter is accomplished by deducing from the theory hypotheses or predictions, which are then subjected to empirical test. If the predictions, or at least a good percentage of them, are confirmed, then the theory is said to have received support. It is

said to be valid, either in the sense of being useful (the conservative position) or in the sense of somehow reflecting an aspect of reality (the liberal position). Thus, we predict in order to test the relative validity of our theories.

I suspect that an outside scientist approaching our field might wonder how a group of people who do so little theorizing can do so much predicting. It is true that we do some theory testing (and I am happy to say an increasing amount of it), but traditionally we have devoted most of our efforts (aside from trying to demonstrate psi to the skeptics) to finding empirical generalizations, or what I am calling laws.

Don't misunderstand me. This kind of activity has a legitimate place in our field. I bring the matter up because it is precisely in the context of this type of research that post hoc analyses are undertaken--and attacked because they were not predicted in advance. However--and this is the key point--the validity of an empirical generalization is solely a function of the regularity or consistency with which it appears in nature, under a specified set of boundary conditions. It has nothing directly to do with whether the observation or relationship was predicted in any particular case.

Perhaps an example will help to clarify this point. Let's say that I have a set of 10 experiments, each testing the relationship between ESP scores and the length of one's big toe. In the first experiment, a positive relationship is predicted from some theory and the prediction is confirmed. Then an independent replication is undertaken. The relationship is predicted based on the first study and again significantly confirmed. However, in the other eight experiments, in all of which the relationship was not predicted (i. e., it was tested by post hoc analysis), the relationship was not significantly confirmed nor was a consistent directional trend noted. Given only two of 10 successful confirmations and no significant overall trend, I would be reluctant to accept the ESP-toe length relationship as a valid generalization. Even though it was successfully predicted twice, it never failed to occur when predicted and (incidentally) it met the traditional requirement for a successful replication with flying colors. On the other hand, if the relationship had been significantly confirmed in the same direction in all eight of the post hoc analyses, I would be inclined to accept its validity even if it failed confirmation in the two studies where it was predicted. Eight of 10 is not bad--for a probabilistic science.

An example of where I think the concept of prediction is sometimes misapplied in parapsychology is the pilot-confirmatory method popularized in parapsychology by J. B. Rhine (Rhine and Pratt, 1954). The method works as follows. A pilot study is conducted and a series of post hoc analyses are performed. A replication is attempted with the relationships that were significant in the first study serving as the basis for predictions. If any of the predictions are confirmed, the relationship in question is accepted.

Now the method itself is commendable and has served the field well. I would only suggest that a relationship surviving this standard is acceptable not because it was predicted, but because it occurred in each of two experiments. I dare to propose that if somehow the significant relationship in the first experiment had not been found until after the "confirmation" had been noted, that the relationship would still have been accepted. In fact, isn't that exactly what happened in the case of the QD effect (Pratt, 1960)?

Whether or not a relationship is predicted is an especially precarious criterion for accepting or rejecting a relationship, because of the kinds of factors that in practice influence whether or not a prediction is made in a given instance. The ideal of predictions flowing from theories with computer-like precision and objectivity is rarely achieved in orthodox science, and even more rarely in parapsychology. Whether a particular prediction is made can be governed by such things as the investigator's awareness of the previous literature, what appears to him as intuitively reasonable, or simply his conservatism or liberalism in making predictions at all. Are we to seriously suggest that a research finding should be rejected because the author had missed a research report in which the effect had been previously reported, or accepted because the author had an intuitive hunch it would work, even though there was no empirical or theoretical basis for the prediction? In fact, connoisseurs of the more obscure applications of the experimenter-psi hypothesis might wish to ponder the possibility that some experimenters in our field might precognize the outcomes of their own experiments and on that basis unwittingly predict them.

One note of caution. Nothing I have said should be interpreted as a blanket license to use post hoc analysis indiscriminately. In most cases where conclusions are drawn from post hoc analyses in the context of a single experiment where a large number of such analyses were performed, such conclusions are unwarranted. Post hoc analyses are like firecrackers; they must be used cautiously lest they blow up in our faces.

Sampling Bias

At this point, I would like to deal with the question of sampling bias. I have contended that in probabilistic science the validity of an empirical generalization is inferred from the collective outcomes of a series of replications only some of which need to be significant or convincing in and of themselves. Furthermore, it makes no difference whether these sample outcomes were predicted in advance. However, it makes a great deal of difference whether or not the sample of experiments considered in the analyses are representative of a larger real or hypothetical population that ultimately defines the validity of the generalization. This is nothing more than a statement of the most basic rule of statistical inference, but it is amazing how frequently it is overlooked in this particular context.

One obvious rule that must be followed if an unbiased sample is even to be approached is that the experiments included in the sample must be selected independently of their outcomes. In a single ESP experiment such as the one I illustrated earlier, we would never think of reporting only the results of those subjects who scored significantly and throwing out the rest. Yet, whether the results of negative experiments should be published continues to be a source of controversy in parapsychology. I am not saying that we should shout them from the rooftops or allot them 40 pages of journal space, but in cases where the kind of sampling I am talking about is appropriate, they need to be reported, and in sufficient detail to properly explicate the procedure and methods of analysis. Perhaps suppression of negative results has a place in nonprobabilistic science--this is the only way I can rationalize why the practice is condoned by some scientists in more orthodox disciplines (although even here I have trouble buying the argument)--but it clearly has no place in probabilistic science.

One might question the need to report negative outcomes when the empirical generalization at issue is a functional relationship between two variables that can exist in a positive or negative direction. In such cases, inferences can sometimes be drawn on the basis of the ratio of significant positive to significant negative outcomes, discounting nonsignificant outcomes. However, this approach virtually guarantees that the sample of experiments is uncomfortably small, and there is still no guarantee that the sample is unbiased with respect to directionality. I personally think our literature is quite unbiased in this respect, especially as I sense the glee with which we sometimes seem to report significant reversals of relatively well-established directional trends. However, I would have to sympathize with an outsider who does not find this argument convincing.

I might say in defense of our field that we do a relatively good job of reporting negative results--much better than most other sciences, certainly better than psychology when I was more intimately involved with that field. In general, I think our literature is unbiased enough to justify some conclusions based on applications of the probabilistic methods of analysis to carefully selected retrospective samples, but I think we can do better.

How can we do better? Some of you will recall that a couple of years ago I proposed a registry of experiments. The idea was that PA members would submit protocol forms describing their experiments prior to their being conducted and the results known. While the sample might still be somewhat biased if participation was spotty, at least it would not be biased with respect to the significance of experimental outcomes. A few of you warned me at the time that the idea was impractical and would never get off the ground, and I want to publicly confess tonight that you were correct. But I make that statement with regret. If I have succeeded at all tonight in laying out for you the framework and rationale of probabilistic parapsychology, I hope you will see the need for something like the

registry. I might mention that a similar idea has recently been proposed by Ray Hyman (1979), one of our more constructive and fair-minded critics. His idea is to set up in advance a series of replications of a particular study by independent investigators. I suspect that this idea, too, is easier to talk about than to actualize, but it is worth thinking about, and I know many of you are sympathetic to this approach. I would welcome any ideas any of you may have on how to deal with this problem.

However, we must be sensitive to the fact that in the real world any sample of experiments is likely to be specialized in one or more respects that limit the generality of any empirical laws derived therefrom. Cynics often quip that the conclusions of psychology experiments are valid only for the college sophomore and the laboratory rat. While this is obviously an overstatement, it also has a grain of truth in it. We in parapsychology must also be vigilant on this point. For example, most of our experiments are conducted by experimenters who believe in psi. While I strongly disagree with critics who say that this is sufficient grounds for doubting the evidence for psi, it perhaps defines a boundary condition for at least some of our empirical generalizations in the field. Should we be saying, for example, that there is good evidence that altered states of consciousness facilitate ESP, given that our experiments are conducted by experimenters who believe in psi?

Summary

Now it is time for me to summarize the first (and longer) part of my talk. I have proposed a general model which states that psi is lawful, but that this lawfulness is superimposed on a great deal of random error from various sources that cause the particular manifestations of psi to seem capricious and unpredictable. It is the goal of parapsychology to extract or isolate these laws from the random error that obscures them. I have outlined in broad terms methods of analysis that can help us accomplish this objective. These methods have not been fully exploited in the past because of our failure to appreciate fully the probabilistic nature of parapsychology, and our insistance on applying the standards of nonprobabilistic science to probabilistic data.

Acceptance of this model has two major strategic implications--one optimistic and the other sobering. The optimistic implication is that if we persevere with our application of the traditional scientific method, modified as I have suggested tonight, we will eventually reach our goal of cracking the mystery of psi. This is not to say that we will not from time to time modify what Gruber (1979) has called the "micro method," perhaps rather drastically, to deal with new challenges in the field, such as those presented by our realization of experimenter psi. But I must say that I see no reason for a major methodological overhaul. The traditional methods are slowly increasing our knowledge--or at least our educated guesses --about the nature of psi. I have tried to indicate some of these in

my chapter on ESP for Krippner's Advances in Parapsychological Research (Palmer, 1978). Slow as this progress may seem, it is the rate of progress we should expect if the model I have proposed tonight is valid.

This brings me to the sobering implication. The breakthrough in our field is not imminent. It will take a lot of time, a lot of data and a lot of drudgery to reach the mountaintop. There are no simple solutions. We are not going to be saved by a psychic superstar, by a paradigm revolution, or by pretending that the implications of quantum theory allow us to abandon the canons of scientific evidence. If the model is correct, we are dealing with a phenomenon that is governed by a multitude of causes that are subtle, hard to measure, interact with each other and are obscured by randomness the sources of which we are only beginning to comprehend. Teasing all this apart is no small task, and it may be periodically stalled as we await advances in other fields of science. I recognize that this assessment will not excite the media, it will not excite the general public, it will not excite potential funding sources, it will not even excite other scientists. But if it is the truth, we must have the courage to embrace it. History will reward our perseverance.

Of course, as most of you know who have chosen to stay in the field for any length of time, parapsychology has its rewards--the excitement of exploring the frontiers of science, the satisfaction as the pieces of the puzzle are slowly revealed to us, one by one.

Last year, Dr. Rao pointed out that we have much to learn from the wisdom of the East. I agree. There is one attribute of Eastern culture that I think it is particularly important we incorporate into our study of parapsychology, given what I have said so far tonight. That attribute is patience.

Part II: The Impending Revolution

The part of my talk you have already heard could serve as a complete and self-contained address, conforming in length and substance to addresses of previous years. But I would not be satisfied terminating my talk at this point. I have given you but one of my perspectives on the field--the yin. I need to balance things out with the yang.

I have stated my belief that our malaise stems in part from a failure to appreciate fully the methodological implications of a probabilistic parapsychology. But I think our malaise also stems in part from a dissatisfaction with our traditional theories and conceptualizations about psi, a feeling that somehow we are on the wrong track. In this case I am less inclined to defend the status quo, but I think it matters little whether I defend it or not. Changes in how we conceptualize psi are already taking place, and they will continue. Just as I sense that we do not fully appreciate the methodological implications of probabilistic science, I also sense that we do not fully

appreciate the implications of the conceptual changes that are upon us. I would like to explore some of these implications with you, as I see them.

Changing Paradigms

Any self-respecting discussion of this topic must begin with a reference to the concept of "paradigm" as introduced into our vocabulary by Thomas Kuhn (1970). The writings of Kuhn have not, of course, escaped the notice of us parapsychologists, who enjoy seeing ourselves as embattled rebels in the latest paradigm revolution of science. A side issue that often comes up when folks discuss Kuhn in this context is whether or not parapsychology is a paradigmatic science. There seem to be three schools of thought on the subject: 1) parapsychology is a paradigmatic science (e. g., Nilsson, 1975), 2) parapsychology is a preparadigmatic science (e. g., McConnell, 1966) and 3) since parapsychology is not a science at all, the question is meaningless.

I naturally reject the third option, and in confronting the question of whether parapsychology is a paradigmatic or preparadigmatic science, I find myself reacting much as I did to the question of whether quantum physics explains psi. I would be quite content to let Kuhn himself tell us whether or not parapsychology is "really" a paradigmatic science (that is, if he agrees it is a science), as I suspect he is the only person who knows what the term paradigm "really" means anyway.

On the other hand, I personally find the term paradigm useful in discussing parapsychology, insofar as it is taken to mean no more than a set of highly abstract and often implicit assumptions that guide research and theorizing in the field. In this sense, I would say that parapsychology is a paradigmatic science and has been so since its founding a century ago, although the paradigm was not fully articulated until J. B. Rhine in the 1930s. I will call this paradigm the "transmission paradigm." Simply stated, this paradigm assumes that psi involves the transmission of information across some kind of channel from a source to a receiver, at least one of which is a mind (or, according to a few diehards, a brain). The fallout from this assumption is pervasive. We see it most clearly in our terminology: extrasensory perception, clairvoyance, remote viewing. PK is perhaps an exception, but even here I marvel at how PK is sometimes conceptualized as a transmission of information to the environment. (I am sure this is defensible; it has just always seemed strange to me.) The paradigm is reflected in some of our theories of the psi process (Tart's [1977] learning theory is a good example), in how we analyze our data (e. g., focusing on direct hits rather than global displacements), and our methodology (e. g., our selection of target materials). An excellent discussion of its impact on parapsychology and some of the difficulties it has in adequately accounting for our data is presented in Stanford's (1978) seminal paper introducing his conformance model.

For better or worse, I see the transmission paradigm dying out in parapsychology. I see it being replaced by what I will call, for lack of a better term, the "correspondence paradigm." This paradigm is more difficult to define than the transmission paradigm, because it is less fully developed. In fact, it might be fair to say that it is simply a negation of the transmission paradigm. About the best I can do to define it positively is to say that it postulates some principle which causes events in nature to coincide to a greater than chance degree, given certain preconditions.[2]

A criticism that is often leveled against this paradigm, especially when it is represented by the concept of "synchronicity," is that it is untestable. I think this criticism is a category error, resulting from a confusion between theories and paradigms. Theories, or at least good theories, must be testable. Paradigms, by definition, are not directly testable, but are evaluated in part by their capacity to spawn theories which are testable. The correspondence paradigm is quite capable of generating such theories, especially ones which define the conditions under which correspondences will or will not occur. Indeed, there are already examples of this sort of thing in the literature (e. g., Stanford, 1978).

It is true that the correspondence paradigm has been around for a long time (e. g. Kammerer, 1919; Jung, 1955; Koestler, 1972), but only in the last few years has it provided a serious challenge to the still-regnant transmission paradigm. I see signs of its emergence all around me, and it would be impossible for me to articulate these signs adequately. Perhaps the best concrete indication I can cite to you of this emergence are the proposals, all within the last three years, of three conceptual systems representing the correspondence paradigm: the mathematically based synchronicity theory of Lila Gatlin (1977), the semantically based synchronicity theory of Barbara Honegger (1979) and the conformance model of Rex Stanford (1978). It is noteworthy that these systems all evolved from different roots, use different terminologies and state or imply different mediating links with the rest of the natural world. Stanford's is probably the most conservative of the three but is also the most significant--at least sociologically--because of his stature in the field and his roots within the transmission paradigm.

Curiously enough, the one thing besides adherence to the new paradigm that seems to unite these three conceptual systems is the inspiration they have drawn from quantum physics. I am not clear whether the more overtly quantum theoretical theories of psi proposed by Schmidt (1975) and Walker (1974) should be classified under the correspondence paradigm. However, it is clear to me that the major catalyst, or at least a major catalyst, for the emergence of the correspondence paradigm in our field at this time is parapsychologists' belated discovery of quantum physics.

I said at the beginning that quantum physics would be the unifying link of my talk. Quantum theory was relevant to the first part of my talk because it postulates a probabilistic universe at the

microlevel. I think it is likely that the theories and models evolving from the correspondence paradigm will be even more probabilistic, or at least more overtly probabilistic, than those representing the transmission paradigm. Thus the points I raised earlier are likely to become more important to the degree to which the new paradigm prevails.

Implications of the New Paradigm: Redefining the Field

I want to stress that I am not just talking about a new batch of theories that we can throw in at the end of a "theories" chapter in some textbook. We are in the midst of a paradigm revolution unprecedented in the history of our field--a fundamental reconceptualization of psi. I am personally convinced that the correspondence paradigm will sooner or later prevail, and that its implications will be staggering. We are only beginning to comprehend them, let alone experience them.

I want to talk in greater depth about one of these implications, but first let me be clear what I mean by "implications." In moving from the transmission paradigm to what I see as a transition point, we have followed a certain path. I am not going to talk about where we are now, but where I see us being at some future time if we continue down this same path. You might say that I am extrapolating a line. I am not foolish enough to speculate precisely how long that time interval will be, but I think the endpoint will be within the lifetimes of most everyone in this room. Of course, it does not follow that each and every one of you will follow this path. Some of us will be counterrevolutionaries, even, perhaps, some of us who are now revolutionaries.

One of the most important functions of paradigms, according to Kuhn, is to define what topics of inquiry and what facts are relevant to a scientific discipline. Guided by the transmission paradigm, parapsychologists have traditionally defined their subject matter narrowly: ESP and PK, with survival thrown in. (Survival research is really a subspecies of ESP and PK research, because we generally assume that discarnate spirits interact with their earthly environment through ESP and PK, even though we don't always call it that.)[3] What these phenomena have in common that makes them relevant in terms of the transmission paradigm is the active role of a mind or brain in sending or receiving information. If the transmission paradigm goes, so will this constraint. This implies, it seems to me, that the definition of parapsychology must be broadened drastically to include a much wider range of potentially synchronistic events than is presently the case. This implication, in turn, has another implication that will force us to reexamine some long-held attitudes: some of the synchronistic phenomena that will fall under our umbrella are now most commonly classified in that category we disparagingly label "the occult."

Now here, of course, is the point you all have been waiting for when I become heretical. I can take some comfort in knowing

that my trail has been blazed by John Beloff, not one of our more radical members, in an article that recently appeared in the European Journal of Parapsychology (Beloff, 1978), but I hasten to absolve him of responsibility for any heresies I may commit during the remainder of this talk.

The Emergence of Scientific Astrology. In an effort to clarify the implication I just jolted you with, I would like to consider a concrete example: the field of astrology. Since coming to the altered state of California, I naturally have been exposed to some of the more "fringe" phenomena in the general area of the paranormal. Although I have tried not to overdo it while building up my immunity, my slight exposure to the bacterium astrologicus has revealed to me some surprising similarities between the structure of astrology and parapsychology. Now to make this comparison meaningful, I must resort to the popular definition of parapsychology, which of course is broader than the definition endorsed by most members of the PA. I hasten to add that I strongly favor restricting the definition of parapsychology to the scientific study of the paranormal, so I hope you will forgive me for abandoning this definition just this once.

So what are the parallels of which I speak? First of all, both parapsychology and astrology are dominated by persons whose orientation is primarily metaphysical, experiential or clinical. They frequently make knowledge claims unsupported by what we would consider valid scientific evidence.

Likewise, both fields have within their midst a determined minority who choose to examine their subject matter scientifically. Yes, Virginia, there is such a thing as scientific astrology. Its practitioners have their own professional organization, called the National Council for Geocosmic Research--note the absence of "astrology" in the title. I recently came upon a copy of an obscure journal called Cosmecology Bulletin, where I found two research reports that used the same principles of design and analysis we are familiar with in parapsychology (Metzner, 1979; Vidmar, 1979). If the topics were more strictly parapsychological, I could easily see one of them on the program of this year's convention.[4] Recently, scientific research in astrology was reviewed in an exhaustive and quite sophisticated volume entitled Recent Advances in Natal Astrology (Dean, Mather et al., 1977), an effort somewhat comparable to our own Handbook of Parapsychology (Wolman, 1977). The tone of the book is sober and critical, often brutally so, although guarded support is claimed for some of the conclusions of traditional astrology. The book received a generally favorable review from one critic of astrology in an illuminating series of papers recently published in Zetetic Scholar (Abell, 1979). Of course, the astrological research most of you are probably familiar with is the work of Gauquelin (1974), who as far as I can tell has a good reputation as a sober and scientific researcher. Finally, one of our own more distinguished members, Hans Eysenck, has published in the astrological literature (Mayo, White and Eysenck, 1978). I am not claiming that scientific

astrology has matured to the level of scientific parapsychology, and I am not going to try to comment tonight on the validity of its data, but the recent growth of scientific astrology is a development we cannot afford to ignore, paradigm revolution or not.

Parallels also exist between the two fields in the ways they conceptualize their phenomena. Most scientific astrologers take a rather empiricist and atheoretical approach to their subject matter, not unlike the parapsychologists of the 1930s. However, one can still distinguish paradigms in astrology, two of which seem to coincide with the transmission and correspondence paradigms of parapsychology. Prototypical of the transmission paradigm in astrology is the theory that the planets and stars influence human behavior by direct "geocosmic" forces. Such an approach seems analogous to the ELF wave and bioenergy theories of ESP and PK, the most extreme manifestations of the transmission paradigm in parapsychology. The correspondence paradigm in astrology has the same origin as its counterpart in parapsychology--the writings of Carl Jung, who, as you know, treated astrology as a prime example of synchronicity (Jung, 1955). According to this view, cosmic events and events on earth coincide because of an ordering principle inherent in nature and represented as archetypes of the collective unconscious.

It is my sense that scientific astrologers at the present time seem to function from the transmission paradigm more so than from the correspondence paradigm. But that could change. If it does, and if parapsychology continues to move toward the correspondence paradigm, we could find ourselves in a situation where parapsychology and astrology are applying comparable theories to explain classes of phenomena whose boundaries become increasingly indistinct. Even if the transmission paradigm continues to dominate astrology, the relationships they uncover--if they are reliable--are still relevant to and probably explainable by parapsychological theories arising from the correspondence paradigm.

Now I am not proposing a shotgun wedding between parapsychology and astrology. Neither field is ready for that yet. I am simply saying that as the correspondence paradigm becomes more and more dominant in parapsychology, it will become increasingly difficult (for us, at least) to justify intellectually the separation of the two fields. The time may soon be coming when an article by Gauquelin on the birth charts of athletes would be considered appropriate for a parapsychological journal.

Opportunity and Danger

It is not my purpose tonight to propose or advocate any course of action. I personally am not yet prepared to completely abandon the transmission paradigm, although to do so is becoming increasingly tempting. It is my purpose to urge that if we continue on the path I see us traveling, we do so with our eyes open and with some awareness of its likely implications. Of course, I could be wrong;

I haven't yet established a great track record as a prophet. If I am right, however, our path provides us with both opportunity and danger.

The opportunity it provides is for us to broaden our conceptual horizons. As part of this exercise, we might wish to look at a body of nonscientific literature that we so far have been able to ignore. Parapsychology has traditionally drawn inspiration and ideas from the Eastern spiritual traditions. The correspondence paradigm invites us to utilize in similar fashion the writings of Jung and the vast literature based on his ideas. I think it might be particularly fruitful to approach the altered-states work from the theory of archetypes. We also might look more closely at the so-called "magical" traditions, as these seem to be based at least implicitly on a synchronistic world view. We should not turn away from this literature because of the exaggerated claims or unethical practices of some of its adherents.

We also should continue to draw from the more orthodox spiritual traditions. While the analogy between quantum physics and mysticism has perhaps been overdrawn, I think it still contains an important element of truth, even if all we can say is that it is easier to distort quantum physics to meet the mystical world view than to so distort classical physics. As we continue down our path, our conceptualizations of psi will become (or seem to become) more and more mystical--in the positive sense of the word.

The danger of our path is that those who wish to destroy us and those who wish to exploit us will seize upon what they see as a new opportunity to link parapsychology with popular occultism, i. e., the antirational and antiscientific. We must resist this kind of simplistic association, as we always have. But we should also resist the temptation to avoid certain concepts and lines of research because of their political risks. To alter our course in this way would be to make ourselves hostages to the abovementioned parties and to make a mockery of our overriding commitment to the pursuit of truth.

Part III: Synthesis--The Dynamics of Balance

I would like to conclude my talk by drawing the dynamic balance I referred to earlier between the conservative and radical elements of my message. I will do so by referring to Reichenbach's (1949, pp. 433-34) classic distinction between the two elements of scientific method: the context of discovery and the context of justification.

The context of discovery is what we sometimes call hypothesis formulation--the source of our ideas, indeed the source of our knowledge. It is in this aspect of the scientific enterprise that we should be bold, even radical. We also should not be afraid to seek ideas from nonscientific sources. While scientists have undoubtedly made great discoveries, the mind trained in the rigors of logical thought

and exacting observation is not necessarily the mind best equipped to make the creative jumps of insight needed to steer us in the right direction. Dr. Pratt's (1974) "future Einstein" may not be a parapsychologist, or even a scientist. The key that we need to unlock the mystery of psi may already exist in the wisdom of the past, albeit requiring translation into proper scientific terminology. If some of our most common speculations about the nature of psi are valid, the answers we need may be lying dormant in the unconscious minds of us all, awaiting only the person in touch enough with his or her true being to tap them. Here is where the humanistic and the transpersonal can contribute to the scientific enterprise, beyond their more obvious contribution in helping us to keep our ultimate values in perspective.

What is more clearly the province of the scientist per se is Reichenbach's context of justification--scientific verification. Here I would urge conservatism. While the ancient traditions and the surface manifestations of our deeper selves may contain great wisdom, they contain great nonsense as well. Historically, intuition and common sense have been poor guides in distinguishing truth from fiction, especially, it seems, when they hide behind the name of science (thereby giving the latter a bad name). The scientific method, broadly defined and properly applied, is still, despite its faults and its slowness, the best method yet devised to choose amongst competing ideas. We must continue to affirm the basic principles of this method. If we cannot do so, I see no reason for the continued existence of this field or of this Association. It is the very cornerstone of our existence.

The key, again, is patience.

I now have come full circle. I'm sure I have said at least one thing tonight that has offended every one of you in the room, but one of the advantages of being President of the PA is that I don't have to run for re-election. If I have caused you to think about some things, I have met my objective.

If I have been preachy this evening, I assure you that it comes from no lack of respect. Let me pat parapsychology on the back for a moment because you deserve it. You have steadfastly, and at great sacrifice, held to the middle path, resisting both the hyperrationalism on your right and the antirationalism on your left. You have had the courage, when appropriate, to utter the three most underused and unappreciated words in the English language--"I don't know"--followed by three of the most noble words in the English language--"Let's find out." And you will--we will--we all will.

Notes

1. The proportion of hits may be somewhat larger than I have indicated if the hitting/missing cancelation effects suggested by

Leyhe (1979) are operative to any great extent, but the essential problem still remains. Moreover, the ratio of hits to misses also might vary randomly.

2. My use of the word "cause" in this sentence reveals my belief that the correspondence paradigm is causal, despite the fact that synchronicity is often considered to be an "acausal" principle. In my opinion, this causality question is a rather secondary one.

3. Reincarnation theory might be an exception to this rule.

4. The other one, unfortunately, suffered from a stacking-effect problem.

REFERENCES

Abell, G. O. Review of Recent Advances in Natal Astrology: A Critical Review 1900-1976. Zetetic Scholar, 1979, Nos. 3 and 4, 74-83.

Beloff, J. The limits of parapsychology. European Journal of Parapsychology, 1978, 2(3), 291-303.

Dean, G., Mather, A., et al. Recent Advances in Natal Astrology: A Critical Review 1900-1976. Bromley Kent, England: The Astrological Association, 1977.

Diaconis, P. Statistical problems in ESP research. Science, 1978, 201, 131-36.

Eccles, J. C. The human person in its two-way relationship to the brain. In J. D. Morris, W. G. Roll and R. L. Morris (eds.), Research in parapsychology 1976. Metuchen, New Jersey: Scarecrow Press, 1977.

Einstein, A., and Born, M. Born-Einstein letters. London: N. p. 1971.

Gatlin, L. L. Meaningful information creation: An alternative interpretation of the psi phenomenon. Journal of the American Society for Psychical Research, 1977, 71, 1-18.

Gauquelin, M. Cosmic influences on human behavior. London: Garnstone Press, 1974.

Gruber, E. R. Psi, methodology, and social context. Paper presented at the Twenty-second Annual Convention of the Parapsychological Association, Moraga, California, 1979.

Honegger, B. Spontaneous waking-state psi as interhemispheric verbal communication: Is there another system? Paper presented

at the Twenty-second Annual Convention of the Parapsychological Association, Moraga, California, 1979.

Honorton, C. Receiver-optimization and information rate in ESP. Paper presented in the symposium, "The Application and Misapplication of Findings in Parapsychology," at the 141st Annual Meeting of the American Association for the Advancement of Science, New York City, January 27, 1975.

Hyman, R. Avoiding bias. Contemporary Psychology, 1979, 24, 270.

Jung, C. G. Synchronicity: An acausal connecting principle. In C. G. Jung and W. Pauli, The interpretation of nature and the psyche. New York: Patheon Books, 1955.

Kammerer, P. Das Gesetz der Serie. Stuttgart and Berlin: Deutsche-Verlags Anstalt, 1919.

Koestler, A. The roots of coincidence. New York: Random House, 1972.

Kuhn, T. S. The structure of scientific revolutions (second edition). Chicago: University of Chicago Press, 1970.

LeShan, L. The medium, the mystic, and the physicist: Toward a general theory of the paranormal. New York: Viking Press, 1974.

Leyhe, E. W. A probability model with psi having two components: Usage and accuracy. Paper presented at the Twenty-second Annual Convention of the Parapsychological Association, Moraga, California, 1979.

McConnell, R. A. ESP research at three levels of method. Journal of Parapsychology, 1966, 30, 195-207.

Mattuck, R. D. Random fluctuation (thermal noise) theory of psychokinesis. In J. D. Morris, W. G. Roll and R. L. Morris (eds.), Research in Parapsychology 1976. Metuchen, New Jersey: Scarecrow Press, 1977.

Mayo, J., White, O., and Eysenck, H. J. An empirical study of the relation between astrological factors and personality. Journal of Social Psychology, 1978, 105, 229-36.

Metzner, R. An empirical study of the astrological elements and physical appearance. Cosmecology Bulletin, March 1979, 40-45.

Nilsson, I. The paradigm of the Rhinean school. Part I. European Journal of Parapsychology, 1975, 1(1), 45-59.

Palmer, J. Extrasensory perception: Research findings. In S. Krippner (ed.), Advances in parapsychological research, Vol. 2. New York: Plenum, 1978, 59-243.

Palmer, J. 1978 Parapsychological Association Convention. Parapsychology Review, 1979, 10(1), 1-5.

Phillips, P. Some traps in dealing with our critics. Parapsychology Review, 1979, 10(4), 7-8.

Pratt, J. G. The case for psychokinesis. Journal of Parapsychology, 1960, 24, 171-88.

Pratt, J. G. Some notes for the future Einstein for parapsychology. Journal of the American Society for Psychical Research, 1974, 68, 133-55.

Reichenbach, H. The theory of probability. Berkeley: University of California Press, 1949.

Rhine, J. B., and Pratt, J. G. Parapsychology: Frontier science of the mind. Springfield, Illinois: Thomas, 1957.

Schmidt, H. Toward a mathematical theory of psi. Journal of the American Society for Psychical Research, 1975, 69, 301-19.

Stanford, R. G. Response patterns in extrasensory performance. Journal of Communication, 1975, 25, 153-61.

Stanford, R. G. Toward reinterpreting psi events. Journal of the American Society for Psychical Research, 1978, 72, 197-214.

Tart, C. T. Toward conscious control of psi through immediate feedback training: Some considerations of internal process. Journal of the American Society for Psychical Research, 1977, 71, 375-407.

Vidmar, J. E. Astrological discrimination between authentic and spurious birthdates. Cosmecology Bulletin, March 1979, 56-90.

Walker, E. H. Consciousness and quantum theory. In J. White (ed.), Psychic Exploration: A Challenge for Science. New York: Putnam, 1974.

Wheeler, J. A. In "The Role of Consciousness in the Physical World." Proceedings of the AAAS Conference, Houston, Texas, January 3-8, 1979.

Wiklund, N. Parapsychological temptations: A discussion of decline effects, sex effects, and post-hoc effects. In J. D. Morris, W. G. Roll and R. L. Morris (eds.), Research in Parapsychology 1976. Metuchen, New Jersey: Scarecrow Press, 1977, 125-28.

Wolman, B. B. (ed.) Handbook of Parapsychology. New York: Van Nostrand-Reinhold, 1977.

Zenhausern, R., Stanford, R. G., and Esposito, C. The application of signal detection theory to clairvoyance and precognition tasks. In J. D. Morris, W. G. Roll, and R. L. Morris (eds.), Research in parapsychology 1976. Metuchen, New Jersey: Scarecrow Press, 1977, 170-73.

NAME INDEX

* Convention participants are identified by asterisks.

SUBJECT INDEX