# RESEARCH IN PARAPSYCHOLOGY 1980

Abstracts and Papers from the
Twenty-third Annual Convention of the
Parapsychological Association, 1980

WILLIAM G. ROLL
and
JOHN BELOFF
Editors
JOHN McALLISTER
Assistant Editor

The Scarecrow Press, Inc.
Metuchen, N.J., & London
1981

ISBN 0-8108-1425-0
Manufactured in the United States of America
Library of Congress Catalog Card No. 66-28580

CONTENTS

## PREFACE:
## THE TWENTY-THIRD ANNUAL CONVENTION

It has been the custom of the Parapsychological Association to hold every fourth annual convention in Europe (coinciding, as it happens, with the leap years). This summer we met in Reykjavik, Iceland, where our host was Professor Erlendur Haraldsson of the University of Iceland. The remoteness of the venue meant, unfortunately, a smaller attendance than usual but there was no lack of acceptable contributions. Indeed, it was necessary to run the Friday sessions in parallel in order to accommodate them all. Naturally, there were more participants from Europe, including a strong contingent from the Netherlands, but there were also speakers from as far afield as Australia and Japan.

The program followed much the same lines as in recent years, with many of the standard topics coming up again for reconsideration. There was, however, only one official symposium, the one on apparitions organized by Karlis Osis, plus three roundtables on diverse topics. Many of the independent papers and briefs could be grouped together to yield sessions on a particular theme. This is illustrated by the "Remote Viewing" session which opened the proceedings, at which we heard a report on a successful transcontinental remote viewing experiment and an important paper from the new Princeton team under Robert Jahn. Similarly, the three papers on paranormal metal-bending showed that this phenomenon is still engaging the attention of parapsychologists in different parts of the world and we heard some striking reports from California, from Japan and from Australia. In general, however, it must be said that there was an unusually large number of papers reporting only a failure to confirm the initial hypotheses or to replicate some previous finding. This is, perhaps, inevitable given the precarious state of the art but it does not help to generate an air of optimism among the assembled researchers.

The sad absence on this occasion of Gaither Pratt, a founding member of the Association who rarely, if ever, missed an annual convention, was felt by everybody, but a warm tribute was duly paid by his close friend and associate Ian Stevenson, our president for this year. A tribute was also paid to J. B. Rhine, who also died in the course of this year. The speaker, appropriately, was K. Ramakrishna Rao, Rhine's successor as director of the Institute for Parapsychology, FRNM. The annual J. B. Rhine lecture was given this time by the British writer and historian, Brian Inglis. Further

tributes for long meritorious service, in the form of an engraved metal plaque, were presented in absentia to Laura Dale and Dorothy Pope, editors, respectively, of the Journal of the American Society for Psychical Research and of the Journal of Parapsychology.

John Beloff
Chairperson
Program Committee

Part 1: Symposium

# APPARITIONS: RESEARCH APPROACHES PAST AND PRESENT*

## THE CONTRIBUTION OF APPARITIONS TO THE EVIDENCE FOR SURVIVAL

Ian Stevenson (University of Virginia)

The first scientific study of apparitions, conducted by several founders of the SPR in the 1880s, led to the description of two theories of apparitions that have, with various modifications, dominated discussion of the subject ever since. Gurney proposed that apparitions are essentially constructed by the percipient, who may, however, use veridical information obtained telepathically from the appearer. Myers assigned a more active role to the appearer, who could be regarded as being--at least in some cases--a real agent, although not necessarily a conscious one.

Most parapsychologists have preferred Gurney's interpretation. It fails, however, to account adequately for a number of features of apparitions. These are: evidence of stronger motivation on the part of agents compared with percipients; collective percipiences; apparitions of someone unknown to the percipient; some similarities between apparitions of the dead and those of the living; quasi-physical features of apparitions; and (rare) apparitions of the concerned previous personality in cases of the reincarnation type.

Further study of apparitions may increase their contribution to the evidence for survival after death.

## APPARITIONS: A NEW MODEL

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

Apparitions are often regarded and, consequently, treated as

*Chairperson: Karlis Osis, American Society for Psychical Research

one basic phenomenological category. However, the actual data on apparitions are not monomorphic but suggest qualitative differences and several very different sources. For example, some apparitions act primarily in the present, which suggests here-and-now causes; others act in the surroundings and circumstances of the past and may be its products. Separate analyses of both types of apparitions could clarify their characteristics, while lumping them together might only blur the picture.

The way an apparition is observed also serves to distinguish it: cases where it is perceived by only one individual among several who are in a position to observe may suggest subjective causes; e. g., incoming psi information projected outward in a visual hallucination. Cases of collective perception suggest the existence of some kind of presence. Theories which try to fit both types of apparitions under one roof always leave some part of the data poorly covered. It is, of course, possible that these theories are so divergent simply because each is fashioned primarily after one type of apparitional phenomenon and so fits one subgroup well, while furnishing poor accommodations for the others, which is why we tried to incorporate all of them in our model.

Most theories can be divided into two major classes: those describing apparitional experiences which suggest the presence of the appearer at the location where he is seen (theories 1-3 below), and those which contend that the apparition is constructed by the percipient from incoming clairvoyant or telepathic information received about or from the appearer regarding his present or past conditions (theories 4-6 below). The six types of theories we considered in our model are:

1. Phantasmogenic Center Theory: Myers (Human Personality and Its Survival of Bodily Death, 1903) assumed that the appearer's consciousness invades the percipient's surroundings as an invisible presence. Presumably the soul of the dead individual is located where the apparition is seen ("phantasmogenic center") but is not itself perceivable by the senses. Rather, he generates an image of himself in the mind of the percipient who, in turn, "sees" a phantom.

2. Etheric Body Theory: Hart (PSPR, 1956, pp. 153-239) assumed that the "etheric" or "astral body" of the appearer is present and "seen" as an apparition. The intensity of these images varies from entirely mental (accessible by ESP) to "materializations" perceivable with the normal senses.

3. ESP-PK Theory: The appearer (apparition) is present, his self-image is perceivable via ESP, and he is capable of psychically (via psychokinesis) interacting with physical reality so that his image can be seen through the normal sensory channel. It is a slightly modernized version of the etheric body theory, which has been hinted at but not explicitly stated in the literature.

4. Telepathy Theory: The appearer is not present in any sense, but cooperates telepathically with the percipient in creating an apparition, which exists only in the percipient's mind (Gurney et al., Phantasms of the Living, 1918). Tyrrell (Apparitions, 1953) assumed that the creative layers of both unconscious minds "stage" the apparition as one would a theatrical production.

5. Clairvoyance Theory: L. E. Rhine (JP, 1957, pp. 13-46) proposes that the apparition is solely created by the percipient, utilizing his or her own clairvoyant impressions of the appearer and his or her situation; e. g., death.

6. Retrocognition Theory: It is often suggested in the literature that certain apparitions involve neither here-and-now presences nor ESP of anything contemporary. They consist of percipients' ESP of past events--"psychometric" readings of a room, house or other locality (Murphy and Klemme, JASPR, 1966, pp. 306-320).

Our intention is not to argue that any of these theories are true or false, but to propose a model which would help in the collection and evaluation of data which, in turn, would enable us more clearly to structure the empirical ground of apparitional phenomena. The model applies only to situations where more than one percipient had the opportunity to observe the apparition. It is divided into two broad categories--singularly observed and collectively observed, each of which is subdivided into whether the apparition is oriented to the present or to the past. It describes predictions about various characteristics of apparitional experiences for each of the four subgroups. Predictions are, of course, tentative, subject to empirical verification or rejection.

## APPARITIONS OF THE DEAD: A REPRESENTATIVE SURVEY IN ICELAND

Erlendur Haraldsson (University of Iceland)

In the mid-1970s a nationwide survey of psychical experiences was conducted in Iceland, using a randomly selected, representative sample of 1132 adults. Of the 902 persons who responded, 31 per cent replied affirmatively to the question: "Have you ever experienced the presence of a deceased person?" This was the second most commonly reported psychic experience.

This high percentage of reports of having had contact with the dead is considerably higher than in the old Census of Hallucinations, in West's more recent replication of that survey and in the survey by Palmer and Dennis in Charlottesville. Our results are closer to those obtained by McCready and Greeley in their nation-

wide U. S. interview survey, in which 27 per cent answered "yes" to the question: "Have you ever felt that you were really in touch with someone after he had died?"

Detailed interviews were conducted with as many respondents as was feasible (127) on the nature and content of the experience and the psychological and environmental circumstances attending it. Only first-hand, waking-state, non-mediumistic experiences were analyzed, thus reducing the number of cases to 100, reported by 35 men and 65 women.

Sensory modality. In 84 cases the experience was of an apparitional nature; in 16, only a vivid feeling of some imperceptible presence. Visual apparitional experiences were most common, being reported by 59 respondents. They comprise 70 per cent of the total number of apparitional experiences. Auditory experiences, many non-vocal, were claimed by 24 per cent. Tactile experiences were reported by seven and olfactory ones, such as experiencing a favorite perfume of the deceased, by five, while one reported a feeling of cold along with a visual experience. One sensory modality was involved in 62 cases, and 22 cases involved two or more, again with visual experiences dominating.

Psychological and environmental conditions. According to popular belief, most apparitions are experienced in darkness or twilight. Our cases did not conform to that view; 44 respondents reported experiencing apparitions in daylight or in full electric light, 20 in semidarkness, and only nine in darkness. According to Gurney's old hypothesis, apparitions tend to occur when the mind is in a restful state and not actively engaged. One-third of our respondents had been physically active in some way, working or going about their daily activities. Another one-third was at rest, sitting or lying in bed. The rest were just awakening or on the point of falling asleep. In thanatological literature, apparitional experiences have frequently been associated with bereavement and grief. In only 11 of the 73 cases where the appearer had been identified, were feelings of grief reported at the time or just prior to the experience.

The fact that most of these reported experiences of the dead occurred under normal lighting conditions, and only rarely when the respondent was in a state of grief, gives some evidence of their being relatively independent of the state of the percipient, and supports the more agent-oriented theories of Myers and Hart. However, one-third of our cases occur near the time of sleep, suggesting some importance of a psi-conducive state of the percipient. These apparently contrasting results beg for a larger sample which one might divide to find out whether the appearer characteristics of the more percipient-dependent and more agent-dependent cases do perhaps differ in a significant and meaningful way.

Characteristics of the appearer. Of the appearers, 63 were identified as men and 19 as women, and two-thirds as relatives or friends. Five cases of crisis-apparitions were reported. A dispro-

portionately high number of the appearers (23 per cent) had suffered violent deaths. In 43 cases more than one person had been present, and apparitions were collectively perceived in one-third of them, which compares well with the Census of Hallucinations.

This high percentage of collective apparitions, the crisis-apparitions and the high number of violent deaths among the appearers lend some support to theories of apparitions which argue for the importance of the appearer.

## A MEMORY THEORY FOR APPARITIONS

W. G. Roll (Psychical Research Foundation, Durham)

Nearly 100 years ago Edmund Gurney (Gurney and Myers, PSPR, 1888-89, pp. 403-485) found recognized apparitions of the dead to "... fall under two heads, which we may distinguish as the personal and the local. That is to say, this experience either befalls some person who has been linked with the deceased by close ties, or it befalls someone in a place in which the deceased, when alive, was strongly interested. Sometimes ... the two characteristics are combined" (p. 408).

The observation that apparitions are connected with inanimate or animate physical systems (though not necessarily reinforced by close emotional ties) is supported by Eleanor Sidgwick's collection (PSPR, 1885, pp. 69-150), "Phantasms of the Living" (PSPR, 1923, 23, 23-429), and Louisa Rhine's (JP, 1957, pp. 31-46) analysis of 49 visual apparitions. Among the latter 15 were local or "haunting" type occurrences in which the percipient did not know the appearer; 16 were personal, nine local and personal, and eight were personal of the "bystander" type where the percipient did not know the deceased, but saw the apparition near a third who had known the deceased.

Rhine notes that the bystander cases "are suggestive of the haunting cases, the main difference ... being that in these the link is a person rather than a geographical location" (p. 39).

If the personal or local connection of apparitions is not a sampling or reporting artefact, what can we make of this connection between psi images and physical space?

Among the four theories for apparitions proposed by Eleanor Sidgwick (PSPR, 1885, pp. 69-150), the second "... is one I can hardly expect to appear plausible and which, therefore, I only introduce because I think that it corresponds best to a certain part of the evidence.... It is that there is something in the actual building itself--some subtle physical influence--which produces in the brain

that effect which, in its turn, becomes the cause of a hallucination" (p. 148).

Though Gurney (Gurney and Myers, PSPR, 1888-89, pp. 403-485) preferred a telepathic hypothesis for apparitions, some cases suggested to him "... the survival of a mere image, impressed, we cannot guess how, on we cannot guess what, by [the deceased] ... person's physical organism, and perceptible at times to those endowed with some cognate form of sensitiveness" (pp. 418-419). Similarly, H. H. Price (PSPR, 1939, pp. 307-343) suggests that images are "Persistent and dynamic entities, which when once formed may have a kind of independent life of their own, and may escape more or less completely from the control of their author.... An image or a group of images might get itself localized in a particular region of Physical Space.... Once localized there, they might continue to be so localized for a considerable period, retaining the telepathic charge which they had at first, though this might gradually diminish in intensity" (pp. 325-326).

Such localized images in a manner of speaking are the "memories" of an area or object in physical space. The localized memories, we might say, are "remembered" by the percipient of the apparition, much as he or she remembers events associated with an object which is always near, the person's own brain.

William James (PSPR, 1909, pp. 2-121) sketched a memory theory along the same lines, not for apparitions, but in his study of the Piper mediumship. James suggested that after death memory traces may exist "psychometrically" in the physical objects associated with the person while he or she was alive. This "system of physical traces" may be activated during a mediumistic session--or, we might add, during an apparitional sighting--amounting to a "spirit redivivus" of the deceased and of "recollection and willing in a certain momentary way" (p. 120).

The picture which begins to form is that of a psi structure or organization which encompasses two or more individuals and the environmental situation where they find themselves. An apparition of a deceased person would not be an independent entity but the product of a relationship which includes the deceased, the living percipient or percipients and the situation in time and space where the apparition is seen. This network of relationships may be the same as Rosalind Heywood has in mind when she speaks of localized holons. As with holons, psi structures may be parts of larger and more complex structures.

If apparitions are mnemonic structures, we would expect them to show the decline characteristics of memory processes (Gurney and Myers, PSPR, 1888-89, pp. 403-485). Myers observed that "... the recognised apparitions decrease rapidly in the few days after death, then more slowly; and after about a year's time they become so sporadic that we can no longer include them in a steadily descending line" (p. 427).

We should also expect apparitions to be associated with psychological states which tend to give memory images the reality of hallucinations. The hypnagogic condition is such a state and apparitions are often seen at night as the person wakes up or before going to sleep. Near death conditions are also hallucinogenic and apparition-conducive, as Osis and Haraldsson have shown.

## Part 2: Roundtables

# SOCIAL AND ETHICAL ISSUES IN PARAPSYCHOLOGY*

## SOCIAL ISSUES IN THE INVESTIGATION OF FOLK HEALING

Stanley Krippner (Humanistic Psychology Institute, San Francisco)

The idea that one person can heal another person is widespread, perhaps universal. It is an anthropological commonplace that shamanism, not prostitution, is the world's oldest profession. Moreover, many anthropological studies have documented the effectiveness of a range of medical systems of tribal and peasant origin.

The idea of internal medicine and the notion that one can heal with drugs (usually ingested) is also very common; a recent compilation of botanical pharmaceuticals used by 48 native American cultures uncovered some 1,305 species in 534 genera from 119 families to be used medicinally in 4,869 ways (Moerman, D. E. American Medical Ethnobotany: A Reference Dictionary. New York: Garland, 1977).

Most analyses of native American healing have emphasized these more specific, "rational" techniques; the standard exercise has been to show that the drugs used in traditional primitive practice do in fact have significant pharmacological action. It is clear, however, that neither native practitioners nor their patients see drugs as a more vital portion of the healing process than song, dance and ritual (e.g., laying on of hands, prayer, exorcism).

The World Health Organization has recently called for cooperation between Western medical practitioners and traditional healers. Some pilot projects along these lines have been surprisingly effective. There is resistance to this move by physicians who admit that some native herbs may be beneficial (or at least harmless placebos) but who recoil at "superstitious" practices that appear to invoke magical thinking.

In this area, parapsychology may help to develop an understanding between the physician and the shaman. Parapsychologists

*Chairperson: Stanley Krippner, Humanistic Psychology Institute

are aware of the "experimenter effect," the importance of belief, and the critical nature of interpersonal interaction in both psi and non-psi effects. Further, parapsychological data might convince some Western practitioners that they should keep an open mind when it comes to the effectiveness of rituals oriented to elicit paranormal phenomena.

Rather than censure traditional healers for being "superstitious," Western physicians should carefully inspect the herbs and rituals used to determine which, if any, are actually physically and psychologically harmful to patients. These could be the topic of dialog with the traditional healer while the others could be accepted as possible potentiators of more "rational" aspects of the treatment. It is hardly inevitable that the ritual will make the herb or medicine more potent through psi. But if this possibility is arbitrarily ruled out there is much to lose and little to gain.

## SOCIAL ISSUES ROUNDTABLE REMARKS

Mark G. Shafer (University of California, Irvine)

These remarks focus on moral and ethical issues which arose from my research on PK metal bending, or which have arisen in discussions with other parapsychologists about this kind of research.

The first issue concerns subject selection. Exploratory macro-PK research is conveniently done in small groups. Several times I refused to admit new volunteers into our group who I thought might disrupt social cohesion. The issue is: what responsibility do we have to current subjects to select new subjects who can be expected to preserve positive social interaction? In creating a social experience for subjects I feel we have a social responsibility, either to preserve social harmony as best we can, or to inform potential subjects that personal idiosyncrasies may sometimes conflict in a group situation.

A second area is the influence our research can have on subjects' self-concept. Participation in our research may cause an inflation of self-concept beyond an appropriate level. As a way of helping to preclude this, a consent form developed by Robert Morris that we use at the University of California, Irvine, says this: "I understand that I am participating in a study of possible new (human abilities), whose existence is still debated. Success in this particular study ... does (not) necessarily indicate that I would be successful in other similar studies or in daily life. Likewise, a lack of success would not necessarily indicate that I would be unable to succeed in another such study or that no such (human abilities) exist."

A third area of concern centers on the reporting of results.

After research is done, most subjects want to know if their performance was good, and what the general findings of the study were, even if we decide not to write up the results. Though it may mean a small bit of extra work, some form of written feedback to subjects satisfies their desire to see an evaluation or product from their participation.

Subjects' anonymity is again an issue in the reporting of results once research is through. Some subjects wish to be personally identified in published reports, but not if the results are negative. This comes up mostly in research with special subjects. It poses the problem that if we agree only to use a subject's name if the results are positive, we become involved in the business of psychic endorsement. This raises such issues as: should our journals serve as a forum for this? What should constitute a psychic endorsement? One way to avoid these problems is to make an agreement with subjects about use of their name before an experiment is done.

The last issue is use of the experimenter's name by subjects. We do not know how our names will be used. The responsibility for preventing problems in this area seems to lie first with ourselves, where it is most easily prevented. Making explicit agreements with subjects and making copies of research reports available to them for reference have been useful procedures for me in this regard.

## ETHICAL RESPONSIBILITY IN PARAPSYCHOLOGICAL RESEARCH

Michael Winkelman (University of California, Irvine)

The issues of ethical responsibility in parapsychological research will be greatly magnified as some of the implications from anthropological research within magical traditions begin to impinge more upon mainstream parapsychology. Many of the types of phenomena which are an integral part of the traditional approach to the paranormal suggest a nature to the paranormal which is quite different from the types of characteristics of the paranormal derived from laboratory study. One of the central characteristics of the paranormal noted among traditional beliefs is the malevolent use of supernatural power. Although parapsychologists have speculated upon the possibility of malevolent use of psi, the lack of research on such topics has made the issue a purely academic pursuit.

It becomes evident through anthropological research that the belief in the destructive use of psi, and perhaps its prevalence, is at least as frequent as its beneficial use. Accounts outlining the procedures for the development of negative paranormal powers, which include elements experimentally demonstrated to be effective in psi elicitation, leave little comfort in the belief that psi abilities are so

difficult to develop that it would be unlikely that one could/would develop them for negative purposes. It would appear that a more complete understanding of the paranormal requires that we deal with a far wider range of phenomena, many of which entail activities and manipulations which we consider to be unjust, immoral or unethical. The manner in which such investigations can be ethically realized, as well as parapsychologists' ethical responsibilities in releasing such findings, may require serious consideration in the near future.

Apart from the possibility of malevolent use of psi, there are other issues, presently a part of mainstream parapsychology, which anthropological tradition bears upon and which involve unexplored ethical ramifications. Altered states of consciousness (ASCs), implicated as central to psi processes within parapsychology, appear within traditional paranormal practices as essential adjuncts to the development and exercise of paranormal abilities. However, the ASCs utilized within the magical traditions are far stronger and more prolonged than the states explored in experimental situations. The implication is that we should magnify the strength of these states if we are to obtain more outstanding psi performance in our experiments.

However, we have yet to deal with the ethical ramifications of the presently utilized ASCs. Although the techniques used for inducing ASCs in most experiments induce only slight changes in most participants, these techniques have the potential for inducing strongly altered states, particularly in more labile individuals. The impact that exposure to ASCs can have upon certain individuals has been forcefully brought to my attention recently. It appears that remote viewing and similar procedures encourage a projection of self in a way which could lead to subsequent uncontrollable out-of-body experiences in some individuals. The immediate dangers, as well as the long-term psychological consequences of such training and exposure, must be assessed and the potential detrimental consequences to our participants avoided.

## MORAL AND ETHICAL ISSUES OF PARAPSYCHOLOGICAL RESEARCH INTO NON-WESTERN MAGICAL TRADITIONS

Gail C. Kawanami-Allen (University of California, Irvine)

Two perspectives of moral and ethical issues emerge as we become aware of the implications that non-Western magical traditions have for parapsychology: 1. Moral and ethical problems confronted when doing field research in the non-Western cultural setting; 2. Ethical responsibilities involved when integrating traditional beliefs and practices into experimental psi research. Both perspectives are equally deserving of attention.

One obvious ethical concern arising out of anthropological studies of magical traditions is that the religious and social beliefs and cultural norms of the natives must not be violated by field researchers. Traditional beliefs must be recognized and valued apart from any Western cultural biases. Many non-Western cultures maintain magical traditions based on deeply embedded religious and social beliefs, which continue to have a controlling influence over the daily life of the individual as well as his or her community. Thus, it is imperative that we recognize that these different moral and social value systems are an integral part of phenomena and certain types of behaviors arising out of beliefs in the supernatural. We must refrain from biases such as the Western-based notion that all supernatural beliefs and related illnesses should be labeled delusional.

In investigating the realm of paranormal phenomena we may be forced to examine concepts such as taboo. For example, in those Pacific cultures where magic is still extensively practiced, magic serves to promote social control through the principles of taboo; taboo signifying that which is psychically dangerous due to either divine or corrupt properties. It is widely accepted that a transgression of taboo can result in harmful consequences inflicted by displeased spirit(s) or by the threatened individual through acts of sorcery. We must respect these beliefs and their associated practices, including culturally defined responses such as ritual cleansings to remove the effects of taboo.

The role of spirits and ritualistic ceremonies to urge or help the spirits fulfill their functions seems to be of considerable psychological importance to many non-Western peoples in their attempts to bring about paranormal effects. It is evident that certain beliefs and practices seem to be conducive to bringing about paranormal phenomena in the non-Western cultural setting. Similar principles may be effective in facilitating experimental psi. However, we should consider the dangers of tapping supernatural powers when implementing magic for destructive purposes. This has serious implications for experimental psi research, as it is not inconceivable that psi abilities could be developed to serve negative purposes. Such moral and ethical concerns must not be overlooked in undertaking studies integrating implications of magical traditions for parapsychology.

As an alternative conception of the way paranormal phenomena occur, spirits may be conceived of or used as mediating agents for the elicitation of psi. However, if we are to integrate the notion of spirits within experimental designs, the psychological effects should be carefully assessed in order that any potential detrimental consequences to the individual interacting with spirits be at least minimized if not completely avoided.

Observation of shamanistic practices as well as laboratory research has clearly indicated that belief in psi abilities significantly enhances the manifestation of psi phenomena. The shaman often at-

tempts to create an emotionally conducive atmosphere by employing various techniques including fraudulent sleight-of-hand measures. If psi-conducive factors such as belief are enhanced by sleight-of-hand measures in the shamanistic setting, are we ethically justified in using experimental strategies for reinforcing belief such as deception (i. e., false feedback)?

## "EXTERNAL MYSTICAL POWERS"

William G. Roll (Psychical Research Foundation)

Tribal or Third World societies have always seemed a potential goldmine for the psychical researcher. Conversely, scientific validation of the beliefs and practices of these groups could improve their self-image and perhaps their living conditions as well.

In a recent address to the Society for Cross-Cultural Exploration, Alice and Irvin Child note that in addition to the "personal mystical" or ("supernatural") powers attributed to certain persons, many groups believe in external powers and entities connected with objects and locations which we regard as inanimate. These forces are believed to influence the course of natural events as well as human activities, and must be dealt with if successful outcomes are to ensue. Personal mystical powers have become naturalized by parapsychology through such terms as extrasensory perception and psychokinesis. We are also beginning to realize that tribal practices to develop these abilities, such as ASC induction procedures, could aid the laboratory researcher.

It is now time to explore the belief in external powers, that is the belief in the psychical properties of the physical environment. This belief takes at least two forms. Firstly, it is believed by many groups that there is no sharp distinction between a person and places or objects connected with that person. Because physical objects are extensions of the person, they can function as channels for psychic interaction with that person. For instance, it may be thought possible to influence someone's health by means of objects the person has handled.

Secondly, it is believed that things may be imbued with powers or personality characteristics of their own which exist independently of living persons. Examples are ancestors and "bush spirits" who are supposedly connected with certain areas or objects and who must be evoked or placated by appropriate actions if human undertakings are to succeed.

Parapsychology as an outgrowth of Cartesian dualism has looked to the mind or brain of the living organism rather than to its physical environment for clues to an understanding of psi. New insights may be gained by exploring practices based on less restrictive metaphysics.

## RELIABILITY AND OTHER IGNORED ISSUES IN PARAPSYCHOLOGY*

Susan J. Blackmore (University of Utrecht)

There are several important issues which seem to have been ignored in parapsychology. The problem of repeatability has been tackled, but that of reliability has not. The term does not even appear in the index of the Handbook of Parapsychology, nor in those of the major journals over the last ten years. In psychology, reliability of scores is usually measured in two ways, by test-retest reliability, correlating scores from two tests, and by internal consistency, correlating scores from two halves of the same test. These measures are rarely reported for psi scores, but when they are the scores are typically unreliable. If scores are unreliable, can we say that they measure anything?

It is often argued that the validity of a measure cannot be higher than its reliability. We have no external criterion for validity of psi scores and apparently no reliability. Can psi scores then be considered valid, and can the correlations between them and other measures be meaningful? Examples from my research on ESP and memory illustrate this point. We tend to assume that there is some kind of "psi process," but if we define psi negatively, and have no reliable or valid measure of it, is this assumption tenable?

Some other ramifications of the definition of psi should be considered. For example, we seem to limit ourselves to studying an ever-diminishing subject area. Should such things as OBEs, divination or death-bed experiences turn out to be "normal" on our definition, then they would be lost to parapsychology.

## NEGATIVE RELIABILITY: THE IGNORED RULE

Dick J. Bierman (Research Institute for Psi Phenomena and Physics, Amsterdam)

*Chairperson: Susan J. Blackmore, University of Utrecht

It is argued that where experimental data are available which have been randomly split into two parts, it is not uncommon for the data to show negative reliability. For example, the psi scores of a subject show inconsistency over runs, or the relationship between a personality variable and psi scores in one part of the data differs significantly from that found in the other part. The use of direction-independent hypotheses and two-tailed testing, even when a scoring direction can be predicted (e. g., from a theory or from a pilot experiment), should not be allowed to obscure this (psi) phenomenon.

The analyzer effect, or checker effect, as it was first labeled by Feather and Brier, and also the famous Fisk-West experiments, which should be interpreted as checker-effect experiments, suggest that these inconsistencies are caused by psi entering at another level than that of the subjects. This view is completely in line with the Observational Theories. In fact negative reliability implies that the implicit assumption that there is a distribution of "psi ability" among (unselected) subjects is probably incorrect.

Ganzfeld studies and remote viewing experiments are expected to show more stable results because averaging of the psi input of the (selected) experimenter takes place over a number of trials.

Martin Johnson (University of Utrecht)

Another less technical, although important aspect of the concept of reliability is the trustworthiness of the researcher and the accuracy by which research is carried out and reported.

Motivational errors as a bias are involved in most human activities. In research they are of two main types: unintentional and intentional. Against the first type precautions can be taken, whereas cheating is harder to control. It is stressed that we can still do a lot to control our experiments better. It is also stressed that the European Journal of Parapsychology has been a pioneer in trying to control selective reporting and it is suggested that other journals should follow the same policy. To adhere to the policy does not preclude cheating but it alleviates for the researcher the pressures to obtain "positive" results in order to get a piece of research into the journals.

# THE DISTRIBUTION OF PSI*

Martin Johnson (University of Utrecht)

No one really claims to know the distribution of psi. We are not even sure about which manifestations in life and lab really qualify as psi.

Sometimes we talk about psi as an ability. It is however, far from sure that the concept of ability applies to psi. It is still an open question whether psi can be learned. Can psi-performance be improved by practice? Probably it can not be improved by ordinary associationistic laboratory principles.

Rather than considering psi as an ability, it may be fruitful to follow other ways of conceptualizing the phenomenon. H. H. Price once stated that psi may be more similar to a state of "infection" than "knowledge."

Gertrude R. Schmeidler (City College of the City University of New York)

What is the distribution of psi? I will try to defend the position that psi ability is common, and that if our tests give null results, our first interpretation should be that the tests were inappropriate. Laboratory conditions, for example, may make certain subjects feel so constrained that they inhibit their psi while giving formal compliance to their formal instructions. Others may react differently, some with enthusiasm and some with distaste. Their pooled results could be null although individually examined data would be meaningful.

Let me illustrate with an anecdote from last winter. My graduate class in experimental psychology required students, near its end, to design and perform a short experiment following the

*Chairperson: Jürgen Keil, University of Tasmania

Skinnerian model: first establishing the subject's baseline for responses, then introducing a probe (a changed condition) to see if responses changed. One bright student with some mild curiosity about ESP used a rainy afternoon, while he was entertaining his children by teaching them twenty-one, for an ESP experiment. Both youngsters took preliminary (baseline) ESP tests; both scored near chance. They then played their exciting new game against each other, and called a set of ESP cards after each deal. The boy, a stable child, continued to score near chance. His younger sister, more labile and competitive, made significantly higher ESP scores after the games she won. Here a little experiment, done in perhaps two hours, yielded a significant ESP score for one subject and also seemed meaningful in showing stable scores for the other.

Surely we could all tell similar anecdotes. I propose that we take the anecdotes seriously. They suggest that attending to our subjects' special interests and response styles might reveal that many, perhaps all, show psi in predictable but idiosyncratic ways. Our basis for formal predictions about psi should, I think, be each person's change in behavior over a range of situations, moods, and physiological states. Predictor tasks might be psi tests, but might also be any other sensitive measures, such as level of aspiration. And our prediction for subjects like the older brother in my student's mini-experiment might be scores so stable that their variance is significantly low. For others the prediction might be that scores will rise after the challenge of failure or novelty; and for others that scores will fall after failure or with a difficult, novel task--but will rise after the gratification of success, security, or justified praise.

There are good precedents for individualized predictions. Body reactions to arousal need pretest, for example, because the appropriate measure for some subjects is heart rate or breathing, for others their galvanic skin response, or gastrointestinal activity. Threat makes some predictably more vigilant; it makes others withdraw and become less observant. If we learn from this, we will vary both psi tasks and predictions according to subjects' needs, cognitive style, mood, and body patterns. Until we utilize the individualized techniques already validated in psychology or physiology, we have not properly tested the hypothesis that psi is a common ability.

Part 3: Papers and Research Briefs

# REMOTE VIEWING*

## TRANSCONTINENTAL REMOTE VIEWING

Marilyn Schlitz† (Institute for Parapsychology, FRNM, Durham) and Elmar R. Gruber (Institut für Grenzegebiete der Psychologie und Psychohygiene, Freiburg)

Experimental parapsychology basically utilizes two forms of ESP testing: forced-choice, in which the range of targets is restricted, and free-response, which allows for a vast scope of target possibilities. Although there are several free-response procedures in use today, the present study was designed as an attempted replication of the remote viewing work. Within this controlled, laboratory design, subjects are asked to describe the whereabouts of an outside experimenter whose exact location at the time is unknown to the participant. It should be noted as an aside that, although the term "remote viewing" is a fairly new one in the literature, it does not necessarily indicate a radically different approach from the former GESP studies which utilized a free-response methodology.

In exploring the remote viewing design, it was decided to attempt a formal replication of the long distance work. In conducting this experiment, carried out in November 1979, the percipient, $E_2$ (M. S.), remained in Detroit, Michigan, while the agent, $E_1$ (E. R. G.), visited the target sites in Rome, Italy. For this study, the experimenters acted as subject and agent in order to provide us the opportunity of observing a remote viewing experiment firsthand. It was felt that this might lead to greater insights which could be of some help in the design of future studies of the remote viewing type. Our idea is reflected in the statement by White (JASPR, 1964, p. 52):

> If we wish to investigate, perhaps we should first participate; otherwise our position is like that of the astronomer who turns his back upon the stars.

*Chairperson: John Palmer, John F. Kennedy University
†Dagger denotes speaker.

## Target Pool and Target Selection

$E_1$, together with a colleague in Italy, selected 40 target sites in Rome. The target pool was carefully constructed to contain several targets of given types. The target for the day was randomly selected from the pool by means of a random number generator.

## Outbound Experimenter Behavior

$E_1$ arrived at the target location by 5:00 PM (Central European Time, CET), 11:00 AM (Detroit time). At the target site, $E_1$ was free to walk around or sit, observing the surroundings. He carried a tape recorder with him and recorded thoughts, impressions of the scene, or specific street scenes and situations at the site. This was done for a period of 15 minutes. Following the experimental period, $E_1$ sent the final target order, as well as transcripts of his impressions to two colleagues, both of whom were blind to the nature of the experiment.

## Inbound Experimenter Behavior

At 11:00 AM on each of ten consecutive days, $E_2$ sat in a dimly lit room and attempted to describe the whereabouts of the agent. Although she was in a calm state throughout the series, no formal relaxation procedure was utilized. Instead, she used intense concentration in attempting to focus on the distant experimenter. The impressions which followed were then recorded on paper, with both sketches and thoughts being written out as the protocol for a given trial.

Following completion of the ten trials, $E_2$ prepared two photocopies of the protocols. One set was sent to E.R.G. for judging preparation and the other to Hans Bender in Germany. No trial-by-trial feedback was given in this study, and, in fact, no feedback was available for several months following the series.

## Judging

After receiving the transcripts from $E_2$, E.R.G. and another person translated the transcripts into Italian. The translators then checked the transcripts for phrases from which one might infer temporal order of the transcript/target sequence, which is a criticism made by Marks and Kammann and discussed by Puthoff, et al. The lack of trial-by-trial feedback also served to control for such a criticism.

As a follow-up of a previous work (RIP 1979, pp. 124-127), it was decided to use several judges. For this study, each of five judges scored all protocols against all target sites visited during the experimental period. In this way, the free-response procedure

adopted a forced-choice judging process, where all the target possibilities were known to the judges. In so doing, judges were asked to rank each transcript to each target site on a scale of 1 to 10. In addition, judges rated the degree of correspondence between protocol/site by making a slash along a line with one end designating 0 correspondence and the other end representing total correspondence. Judges visited the target locations independently and in the order of their choice. For each target site, judges were also provided with the impressions $E_1$ had recorded while visiting the target sites during the control period.

## Quantitative Assessment

After receiving the judges' responses, $E_2$ prepared the ratings and rankings for analysis. To do this she first measured the lines for ratings and then summed the ratings for all judges for each protocol/target. The same procedure of summing the judges' responses was used for rankings. Following this, $E_2$ arranged the scores into two 10 x 10 matrices, one for ratings and one for rankings with all of the five judges' responses added together to represent one score in the matrix.

In deriving the appropriate statistical evaluation for this closed-deck series, we assumed non-independence of target/protocols (JASPR, 1979, pp. 1-16). We then utilized the direct-count-of-permutations methods to assess the statistical significance of the given matrices. This statistic computed an exact p by scoring and counting all possible permutations of targets while keeping the response matrix fixed. The permutations method yielded a p of $5.8 \times 10^{-6}$ for rankings, and $4.7 \times 10^{-6}$ for ratings. (While the permutations of rankings and ratings were the planned analysis, we also looked at the number of direct matches on the diagonal. It is interesting to note that this method was, as expected, less sensitive than the permutations method, although it was still significant with six direct hits out of ten, yielding a p of $6 \times 10^{-4}$.)

In addition to the combined judging, we also looked at each judge's scoring separately. This was done in an attempt to observe the degree of consistency within judges. Since four out of five judges showed significant scoring based on the permutations method for both rankings and ratings, we must conclude that there appears to be a general reliability among judges.

## Discussion

In view of the highly successful results of the present study, we might again stress the value of remote viewing as a psi-conducive procedure, which is seemingly unaffected by distance. However, since both experimenters have obtained significant results in previous psi experiments (EJP, 1979, pp. 36-50; RIP 1979, pp. 124-127), it may well be that the results are not necessarily due to a psi-conducive

procedure but to the subject/experimenters themselves, who, moreover, are the most highly motivated persons to want a positive outcome from the experiment. This is in line with the observations made by Puthoff et al., of the a priori necessity and relevance for obtaining information (i. e., seriousness of purpose) as one of the factors serving to enhance success in remote viewing.

An area in which the authors encourage further investigation is a greater concern within experimental reporting for a "method of response." This would be in line with much of the earlier work, in which we are able to observe such an interest. A possible means of implementing this approach into current methodological designs would be the development of what might be noted as a phenomenological inventory which could be administered following each experimental session. The inventory would be aimed at eliciting information which would allow the organization of subjects' experiences to be mapped. That is, it should be capable of providing a basis for describing the basic structures of consciousness involved in the remote viewing experience. In addition, it should throw light on contextual features of the elicitation process, i. e., subject-experimenter interaction within specific settings.

In conclusion, it should be reiterated that remote viewing appears to offer many avenues for exploration. As such, we feel that it is a worthwhile area for psi research and are looking forward to a follow-up of the present ideas.

## ANALYTICAL JUDGING PROCEDURE FOR REMOTE PERCEPTION EXPERIMENTS

R. G. Jahn,† B. J. Dunne and E. G. Jahn (Princeton University)

Remote perception experiments, although relatively simple to conduct and comparatively high in yield, have been somewhat problematic to evaluate quantitatively. The conventional techniques of rank ordering by independent human judges have proven vulnerable to vagaries in the judges' capability, to their subjective biases regarding the individual experiments and the field as a whole, and to possible psychic input of their own. Beyond this have remained some unresolved questions of proper statistical treatment.

In an effort to circumvent much of this uncertainty, we have developed a more analytical judging method, based on a code alphabet of thirty binary descriptors--ranging from quite factual discriminations, e. g., "are there trees present?" to much more impressionistic aspects, e. g., "is the scene oppressively confined?"--in terms of which all targets and perceptions in a given experiment may be characterized. Given input data in this form, a computa-

tional program then carries out the following sequence of exercises: (1) The a priori probabilities of each descriptor appearing in affirmative/negative form in the given target pool are calculated to provide the basis for various weighted scoring techniques to follow; (2) using several alternative scoring recipes, the maximum (perfect) scores obtainable on each target of the pool are calculated to provide normalizations for the actual perception scores to follow; (3) for each scoring recipe, various "chance" scores are defined and calculated, also to provide normalizations for the actual perception data; (4) the effectiveness of each descriptor in relating the perceptions to their proper targets in the given pool is calculated for each scoring recipe as an aid to subsequent further refinement of the descriptor lists; (5) the actual perception scores are calculated by each of the scoring recipes; (6) the perception scores are normalized by the various maximum scores and chance scores calculated in (2) and (3); (7) in addition to its proper target, every perception is scored against every other target in the pool, using all of the same scoring and normalization recipes; (8) for each scoring/normalization recipe, all data are arrayed in full perception-target matrices, first in the original ordinal correlations, and then in order of decreasing scores, i. e., in rank-order matrices, and the perception ranks against the proper target are displayed; (9) appropriate statistical parameters of these perception/target matrices are then calculated, e. g., mean rank/chance mean rank, Z and chi-square treatments, and the corresponding probabilities against chance.

Application of this judging method to a body of remote perception data obtained previously in the Chicago area yields significant statistical scores in the $p < .0001$ range which are rather insensitive to the particular scoring/normalization recipes employed. For example, five different scoring methods yield mean ranks from 5. 79 to 7. 13 (chance value 12. 5); 19 to 21 perceptions ranked below chance mean (chance value 12); Z scores from -3. 36 to -4. 75, and chi-square values from 27. 2 to 57. 2. When the analytical ranks are compared with rankings previously assigned by human judges using the conventional procedures, it appears that in the majority of cases the analytical and impressionistic evaluations concur in their estimate of the quality of the perceptions, especially in the low rank assignments.

To obviate the possibility that the particular list of descriptors employed somehow may process even random inputs to apparently significant scores, artificial perception data matrices may be constructed from the output of a random event generator, and the computational schemes applied to various combinations of these with each other, and with true data. In all cases the results fall well within chance.

In using data from earlier experiments, most of the rendering of the perceptions into binary descriptor form must be extracted ex post facto from free-response transcripts, without benefit of comment from the percipients. To guard against possible bias of those performing this rendition, and also to explore the sensitivity of the

descriptors to subjective interpretation, an arbitrary group of eight perceptions was assigned to five independent judges for rendition into the binary descriptor form. Reassuringly, the preponderance of the responses agreed well with the original assignments. Specifically, in less than 13 per cent of the 240 choices did more than one of the judges dissent from the majority, and in only 10 per cent did the majority disagree with the original assignment. Even those discrepancies were localized in only eight of the 30 descriptors, all of which were subsequently reworded in an effort to reduce further any ambiguities.

Clearly, these uncertainties should be further reduced when the method is applied to new remote perception experiments, where the percipients can be queried for the descriptor choices immediately after completing their free-response reports. Our first attempts at a new series of precognitive remote perception experiments in the Princeton area, to which the analytical judging procedure could be applied ab initio, produced evidently insignificant results, and the analytical method indeed scored the series well below significance. Further experimental efforts are now in progress.

In the development of an analytical method such as this for the quantitative assessment of an intrinsically broad-spectrum and subtle source of information, one is inevitably forced to balance the degree of information extraction against the complexity of the formulation and calculation. If the assessment mesh is too gross, major features of the information content will slip through; if the mesh is too fine, the process becomes unwieldy. Clearly the version presented here has tended to the coarse-grained side, and the extent of its positive yield would seem to bode well for subsequent refinement.

Beyond increasing the number and effectiveness of the descriptors, we are exploring the advantage of employing ternary rather than binary responses to convey more shaded information about the aspects queried. A given feature could thereby be specified as (a) present and dominant; (b) present but secondary; or (c) absent. Or alternatively, the feature could be described as (a) definitely present; (b) ambiguous or unspecified; or (c) definitely absent. While these approaches clearly provide more specific target and perception data, the scoring thereof becomes more complex, especially in the definition of certain of the normalization denominators, e. g., the proper "chance" score for a given ternary descriptor target. We are also examining an alternative approach to similar refinement of the scoring process, wherein any aspect which is emphasized, reiterated or specified in very explicit detail by the percipient in his free-response transcript, or which on query he acknowledges to be a clear and dominant element in the scene, is given more weight, both in success and failure, than those aspects specified more casually.

## REMOTE VIEWING: EXAMINATION OF THE MARKS AND KAMMANN CUEING ARTIFACT HYPOTHESIS

Charles T. Tart† (University of California, Davis), Harold Puthoff and Russell Targ (SRI International, Menlo Park)

Targ and Puthoff reported a nine-trial remote viewing experiment with subject Pat Price that seemed to illustrate that ESP could sometimes function at very high levels of information transfer and accuracy (Nature, 1974, 252, No. 5476, p. 602). The results of the experiment were significant at $p = 10^{-4}$, one-tailed. Because of the important implications of high-level functioning of ESP, these results brought about considerable discussion in the scientific community. Here we examine an alternate hypothesis to ESP that has been put forward by Marks and Kammann, who hypothesized that the judging procedure was defective in that extraneous transcript cues (such as whether a trial was early or late in the series) had allowed the judges to match transcripts and targets significantly on an artifactual basis.

H. P. suggested to C. T. T. that Marks and Kammann did not actually test the hypothesis they claimed to be testing. They, in effect, claimed that there were two kinds of material in the transcripts, descriptions (D) intended to fit the designated target by ostensible use of ESP, and remarks which provided extraneous cues (C). They hypothesized that the latter kind of C material alone was sufficient to account for the results reported by Targ and Puthoff. When Marks judged his subset of five, however, he apparently worked with both D and C material, viz., the complete transcripts. Marks' successful judging results, then, might be due either to the adequacy of the (ESP-inspired) descriptions of the targets, the extraneous cues, or a mixture of both. As a result, their methodology suffers from the same shortcoming they criticized as existing in the original study. Therefore we decided to examine the Marks and Kammann hypothesis adequately, carrying out the experiment they failed to.

To assess the actual importance of the extraneous cues, C. T. T. went over the Price transcripts to extract all such potential cues. Three transcripts had no extraneous cues at all in their texts, five of the others had one or two indications of which target the transcript was not intended to match, and one had three indications of which target it was not intended to match.

In the best case for cues the probability of a correct match can probably be increased from one-ninth to one-third; in the next best case from one-ninth to one-seventh; and in two other cases from one-ninth to one-eighth. The remaining five cases gain no direct advantage from cues; just indirect advantage from not using as possibilities the ones already used in the more advantageous cases.

When one takes into account the resulting constraints due to cues, the number of possible target/transcript matchings is reduced from 9! = 362,880 to 68,760 combinations. Assuming that the cues are used to maximum advantage, we find that the significance level associated with obtaining at least seven matches (as was done) in a forced-choice, nonindependent assignment of transcripts to target sites (the most conservative statistic) is only reduced from $p = 10^{-4}$ to $p = 3.9 \times 10^{-4}$, still a quite significant result.

If the original judging and re-done judging (described below) of the edited Price material had been only marginally significant, the argument concerning possible confounding of the results due to potential cues might have carried some weight. As we see, however, the change in significance resulting from use of the extraneous cues is negligible considering the magnitude (seven first place matches) of the actual results. This supports our original assessment that the cueing artifacts were probably not of major importance in the original analyses. Our present position in retrospect, however, is that we (H. P. and R. T.) should have elected to edit the Price transcripts before giving them to our blind judges (as has been standard procedure in later experiments), rather than having set a policy to use unedited transcripts to avoid possible criticisms of selective editing.

One of us (C. T. T.), who had not been involved in the original Price series, decided to re-analyze the data independently, after editing out all possibly useful extraneous cues. The details have been reported elsewhere (Nature, 1980, 284, No. 5752, p. 191). Briefly, C. T. T. edited out all potential statements from the transcripts that could possibly cue a judge as to which target a particular transcript was not intended for. Since the judging task depends on a judge's ability to extract signal from noise for correct matching, C. T. T. began to search for a talented judge to re-judge the series, testing potential candidates (who knew nothing of remote viewing research) on material from a different remote viewing study. The second potential judge tested did well, and was thus selected for this task. She then judged the edited Price series transcripts and, as our previous skilled judge had done, successfully matched seven of the nine, reproducing the $p = 10^{-4}$ result. A complete analysis of the judge's rating matrix by a direct-count-of-permutations factorial method, in which the judge's rating matrix is permuted through all possible target/transcript assignments, yields a result significant at $p = 2.2 \times 10^{-5}$, one-tailed. The subset of five unpublished transcripts that Marks and Kammann's judges could not match was also significant at $p = .025$, one-tailed. Thus extrasensory perception remains the best hypothesis to explain the Price series result.

## STUDIES WITH CHILDREN*

### THE EFFECTS OF SCHOOLING AND FORMAL EDUCATION UPON EXTRASENSORY ABILITIES

Michael Winkelman (University of California, Irvine)

This is a second replication of research findings which suggest that school and formal education influences are detrimental to ESP abilities. Consistent with previous findings, we hypothesized: clairvoyance measures would correlate negatively with educational measures independent of age, and show no correlation with age independent of educational measures; psychokinesis shows no correlation with age or educational measures; clairvoyance and psychokinesis correlate negatively.

#### Method

Participants in this experiment were 40 children living in Marquelia, Guerrero, Mexico. They were evenly divided between sexes and evenly dispersed between the ages of seven and 14. Although children with widely varying participation in school were included, some degree of schooling was necessary since the cognitive tests required school-related skills. The participants were those children with whom the experimenter established easy contact. This undoubtedly biased the population in the direction of the more socially assured children, although this variable was not assessed. Children were offered small monetary payments and candy rewards for participation.

Testing began with the cognitive tests, completed in three sessions in the order below. Psi tests began with the clairvoyance with gum and clairvoyance with marbles, counterbalanced across participants. The second psi test session included the psychokinesis with marbles and psychokinesis with dice test in order. A second series of clairvoyance tests were carried out at a later date; a repeat of the clairvoyance with gum and a clairvoyance-instrumental response in the same session.

*Chairperson: Gertrude Schmeidler, City University of New York

Educational Assessments.

Age and years of participation in school were elicited from participants in initial interaction.

Formal Mathematical Test (math) involved 18 mathematical problems assessing basic formal abilities in addition, subtraction, multiplication and division.

Informal Mathematical Test utilized the addition and subtraction problems from the Formal Mathematics Test, but in a manner which placed the mathematical manipulation in the context of daily experience. Children counted cocoa beans; only two failed to achieve a perfect score.

Children's Embedded Figures Test is designed to assess the field independence/dependence variable; this was a straightforward application as suggested in the manual.

Reading test--formal language assessment developed on site. The test consisted of five short paragraphs ordered in increasing complexity. Children were asked to read the paragraphs and report on their contents.

Memory tests involved the children reporting on the contents of a folder of fixed photographs which they studied for about two minutes. The 22 color photographs were of the following categories; animals, food, vehicles, clothing and tools. The test provided two measures: total recall and the presence of category structuring in recall.

A combined index of education was determined by a summation of all above measures.

Psi tests.

Each psi test involved 20 trials.

Clairvoyance with Gum (CV-Gum)--Target selection involved the removal of one colored box of gum by the experimenter's gloved hand from within an opaque bag containing 60 boxes of gum of equal distribution of three colors. Children would make their choices following target selection but prior to its removal from the bag by marking an answer sheet with a crayon corresponding to the color of their choice. Following the display of the target, children would mark their answer sheets with a crayon corresponding to the actual color of the target. Participants were rewarded on correct trials with a gum; sampling was with replacement. Tabulation of response totals utilized a summation of matching response/target markings made by the participants and a tally of the actual rewards given by the experimenter; discrepancies were resolved by recording the lower score.

Clairvoyance with Marbles (CV-Marbles) utilized a wooden box with a selection lever and three different colored marbles. Previous testing of the box indicated no significant deviations from randomicity (1740 trials, $x^2 = 1.54$, 2df, $p < .4$). Testing involved the experimenter manipulating the box to cause a marble to fall into the lever, the participants recording their responses with a same color crayon, the experimenter displaying the marble, and the child recording the appropriate target color. Rewards, tabulation and control for accuracy followed the procedure of the CV-Gum test.

Clairvoyance-Instrumental Response (CV-Instrumental) followed the basic procedure of the CV-Gum test, with the child making the response by blindly selecting by hand a crayon from a bag.

Psychokinesis with Marbles (PK-Marbles) utilized the box described in the CV-Marbles test, with the children manipulating the box without removal of the selection lever until they thought that the color requested by the experimenter was in the selection lever. The box was then handed to the experimenter who determined and recorded whether the response was correct. Rewards and control for accuracy followed the procedures specified above. Target order was systematically varied for each participant and counterbalanced across participants.

Psychokinesis with Dice (PK-Dice) utilized a cardboard box with a baffled chute and a cup of twelve pipped dice. Participants dumped the cup of dice into the chute. A counter-balanced design was employed, with participants randomly assigned to an initial condition of ten trials of 1's or 6's, with target direction balanced for the second ten trials. Results were tabulated by having the participant and/or bystanders first report the number of dice with the assigned target, and the experimenter verifying his own simultaneous count with this information. Discrepancies were resolved through recount until social consensus was achieved.

Total Clairvoyance was determined by a summation of all clairvoyance tests.

Total PK was determined by a summation of PK-Marbles and 1/6 of PK-Dice, which adjusted the later score to the same probability range as the former.

## Results

Clairvoyance tests showed no individual or overall significance. PK-Marbles was positively non-significant. PK-Dice was positively significant at the $p < .05$ level. CV-Gum and CV-Marbles showed slightly negative or no correlation to educational assessments. CV-Gum retest showed negative correlations with all education assessments, significantly with school, math, reading and combined measures. CV-Instrumental correlated generally negatively to education,

but reached significance only with math. CV-Total correlated significantly and negatively with all education except total recall. Age also exhibited negative correlations to clairvoyance variables, reaching significance with CV-Total. The PK to age and education correlations showed a slight negative tendency, principally on PK-Marbles. PK/clairvoyance correlations were negative or neutral; CV-Total and PK-Total showed near significant negative correlation ($p=.07$).

Partialling age from the generally negative clairvoyance/education correlations has the effect of reducing the correlations. Only the CV-Total/schooling correlation is maintained at significant levels; the other CV-Total/education correlations are reduced to an average of .00. Partialling out all of the education assessments from the age/clairvoyance correlations, we find that the generally negative correlations shift to slight positive correlations. Age and the education assessments predict very well the correlations of one another to clairvoyance. This mutual predictability is reflected in the high intercorrelation of age with education (average $r = .66$).

Partialling schooling (but no other single or combined education assessment) from the age/clairvoyance correlations brings about the same positive shift in the age/clairvoyance correlation as partialling all education assessments simultaneously. This implies that the predictive value of education in the age/clairvoyance correlations is largely the result of schooling. This is borne out by partialling all education assessments simultaneously except for schooling and the combined measure. Although this brings about a positive shift in the age/clairvoyance correlation (average shift of $r = .1$), it is not of the same magnitude as partialling schooling (average shift of $r = .35$). Schooling assesses an important variable in relation to clairvoyance which is significantly different from that assessed by the other educational assessments.

Age and schooling show correlations of comparable magnitude to clairvoyance. However, partialling one from the other in relation to clairvoyance has significantly different effects. Partialling schooling from the age/clairvoyance correlation brings about a large shift in the correlation to a weak positive one. However, partialling age from the schooling/clairvoyance correlations has a smaller effect, and does not eliminate significance with CV-Total. Since partialling schooling from the age/clairvoyance correlations eliminates the negative correlation while partialling age from the schooling/clairvoyance correlation exercises little effect, the negative age/clairvoyance correlation is a reflection of the correlation of age with schooling, while the effect of schooling upon clairvoyance is independent of age. Since schooling, instead of individual cognitive measures, correlates independently significant with clairvoyance, this suggests that the effects are not particular cognitive ones, but general ones related to the general socialization experiences associated with schooling.

## PSYCHOKINESIS EXPERIMENTS WITH INFANTS AND YOUNG CHILDREN

William Braud (Mind Science Foundation, San Antonio)

In Experiment 1, the output of a radioactive decay-based binary random generator was arranged so that each hit resulted in five seconds of movement of a remote-controlled toy truck; misses resulted in no movement. An experimental session consisted of two runs of 64 trials each, generated at a rate of one trial every five seconds. The two 5-1/3-minute runs were separated by a four-minute rest period. The subjects were four male and five female children, 4, 5, and 6 years of age, recruited by the experimenter, Mary Ann Howe, from among the children of friends and relatives. The children were tested individually in a large, brightly illuminated, open area within the premises of the Foundation. Each child was simply shown the toy vehicle and was requested to watch it move and to wish for it to move as much as possible. At the end of each run, scores were recorded directly from the random generator's hit and miss counters by the experimenter. A permanent record of all trial outcomes (on punched paper tape) was also produced. These records were examined by W. B. at the conclusion of the experiment and were used to verify the experimenter's records. Single mean t tests indicated that hit scores did not differ significantly from MCE for either the first or the second run, nor did the scores for the two runs differ significantly from one another. An equivalent number of randomness test trials, conducted after the sessions, indicated no significant departure from chance expectation.

A number of "improvements" were made in Experiment 2: (a) the sample size was doubled, (b) slightly older children (who seemed more interested in the activity of the toy truck) participated, and (c) the sessions were conducted in a more familiar environment. Otherwise, the protocol was identical to that of Experiment 1. Ten male and eight female children participated. Three children were seven years old; the remainder were six years of age. The test environment was a comfortable, quiet waiting room containing furniture which functioned as an "obstacle course" for the truck. The children typically stood and ran about the room during the sessions, some of them shouting commands at the truck. As before, punched paper tape records were kept of all trials. This time, the results were much more encouraging. Significant hitting occurred in the first run ($\overline{X} = 33.61$, $t = 2.24$, $p = .039$, two-tailed), nonsignificant missing in the second run, and a significant difference (decline effect) between the two runs ($t = 2.52$, $p = .022$, two-tailed). An equivalent number of randomness test trials conducted after the sessions indicated no significant departures from chance expectation.

Experiment 3 involved time-displaced psychokinesis. Infants between the ages of six weeks and 12 months served as subjects, and the playback of a tape recording of the mother's voice provided rewarding feedback. In Phase 1 of the experiment, individual recordings

were made of the voices of fifteen mothers as they deliberately interacted with their respective infants. In Phase 2, these fifteen recordings were gated through a random event generator and onto fifteen new audio tapes. The output of the random generator was arranged so that each hit resulted in the recording of 15 seconds of mother's voice onto the new tape; each miss resulted in the recording of 15 seconds of silence. This electronic gating of the mother's voice recordings was accomplished silently and remained unobserved by Gary Davis, who supervised the mechanics of the process. Punched paper tape records were made of all trial outcomes, but these were not observed until the experiment had been completed. In Phase 3 of the experiment, each infant was separated from its mother by the experimenter, Debbie Syring, and left alone in one of the rooms of the parents' home. It was expected that this isolation by a stranger would distress the infant and would therefore effectively motivate the production of mother's reassuring voice via a time-displaced PK influence upon the random process which originally controlled the recordings. In Phase 4, the punched paper tapes were observed by G. D. and D. S. and the data were subsequently analyzed by W. B. It should be pointed out that the experimenter herself (D. S.) was unaware of the time-displaced nature of the experiment until the latter had been completed. Statistical analysis of the results indicated above chance hitting which approached but did not quite reach significance for the first run ($\overline{X}$ = 10.87, t = 1.53, p = .075, one-tailed). Scoring for the second run was not significant, nor was there a significant difference between the scores of the two runs. A post hoc analysis of whether the infants were hitters (> 20 hits overall, N = 10) or missers (< 20 hits overall, N = 3) yielded a binomial p = .046, one-tailed (two infants were neither hitters nor missers, scoring exactly 20 hits).

## METAL-BENDING*

# PK METAL BENDING IN A SEMI-FORMAL SMALL GROUP

Mark G. Shafer (University of California, Irvine)

This paper summarizes two years' exploratory research on PK metal bending in a small group setting, with selected persons from Southern California. A total of 13 people participated in the research, from 1976 to 1978, joining the ongoing program at different times and for varying lengths of time. Participation was voluntary and by the invitation of the experimenter (the author) only.

Participants ranged in age from 12 through 40, with five being under 20. All claimed macro-PK ability, and were invited to participate in the research either after making this claim at one of the author's public lectures, or after being referred to the author by an acquaintance familiar with his research.

All participants claimed to be able to bend stainless steel spoons and/or other cutlery psychokinetically by holding the item in one hand and rubbing the neck continuously between bowl and stem until through PK the metal softened there. At this point a bend or twist would be attempted using the other hand for physical manipulation. Participants "knew" when a spoon was softened either through periodic testing with the other hand "to see if it was ready to go," or through feeling it become slippery beneath the grasp, "as if suddenly coated with mercury." The psychokinetically softened condition was reported to last only a fraction of a second.

## Initial Research Sessions

Research sessions were held approximately three times each month, for 1 1/2 hours in the evening in a video lab room of a social sciences building at the University of California, Irvine. The atmosphere of meetings was kept congenial yet purposeful. Participants sat around the room on chairs or couches, forming a rough circle about 15' in diameter. The first PK tasks always consisted

*Chairperson: John Beloff, University of Edinburgh

of each person's attempting to bend rather lightweight cutlery via PK with touching. No effort was made to determine whether any PK was used to effect these bends, in a deliberate attempt to establish a psi-conducive social context. After successful bends were being produced in this fashion by most participants, cutlery of a heavier weight and brass keys were introduced for similar attempts, as were other more well-controlled PK tasks.

In about half the sessions a video recording system was used. Participants were taught how to operate the video camera, and were given opportunities to record live research footage and make suggestions to the others about how to improve the strength of evidence being gathered. By inviting participants to become co-experimenters as well as subjects, a greater cooperation, honesty, and interest seemed to be engendered.

Participants could sometimes bend an item almost immediately after having received it, and at other times not until late in the session. Mental strategies used for bending included visualizing the item bending or bent, mentally commanding the item to bend, and most prevalent, conceiving the idea or image of bending, then ignoring it while rubbing the item and conversing with others.

Very heavy-gauge cutlery was bent and twisted by participants, some adolescents, in ways that the experimenter could not duplicate using the greatest of his physical strength. Generally the physically stronger participants produced the more difficult and complex bends, with occasional exceptions, suggesting that physical force was used by most participants for accomplishing most bends. One result strongly suggestive of PK was that the PK-bent cutlery rarely showed the signs of physical stress present in cutlery bent similarly but with physical force only: roughened surface texture, microcracks, and ruptures in the area of bending.

## More Controlled Findings

Tasks were eventually introduced where either normal physical contact with the target or target area of a system was prohibited, such as by having participants attempt to bend a spoon without using either hand to physically make it bend; or targets whose properties seemed to exclude the possibility of bending without fracture under ordinary, short-term conditions were used, such as by using items of brittle metal as targets. Because of the rarity of success and frequent malfunctioning of the videotape system, only one of the results reported below, the causing of movement in a pendulum system from a distance, was recorded on videotape.

Two instances of "droop-overs" occurred, where a spoon held at the base was softened at the neck by PK, causing the bowl to droop slowly over of its own weight. These were observed by watching participants but not by the experimenter.

A spoon clamped to a table with a weight hanging from the protruding bowl was seemingly softened at the neck by PK from an adjacent room, causing the spoon to bend downward at a 90° angle toward the floor. The experimenter was conversing with the participant concentrating on the setup at the time, when the participant suddenly grimaced and felt he had influenced the spoon. Upon checking, the bend was discovered, but because the target had been left uncontrolled for a period with the other subjects the significance of the event was considered to be marginal.

An ordinary brittle hacksaw blade was successfully bent into a perfect U shape 1" wide by a participant on one occasion, a feat hypothetically impossible without fracturing the blade. The result was accomplished in about three minutes, in three or four discrete bends-with-touching which lasted about a second each. Non-participants attempting to duplicate this feat broke the blades in every case without instilling any permanent bends.

A small aluminum bar was coated with small fragile spheres about the size of salt grains, and used as a bending-without-touching target. One participant concentrated on this bar for about 20 minutes, at which point it spontaneously bent in the middle 15°, according to subsequent measurement. Another participant was observing at the time and did not notice any contact made with the target. Though it was both the experimenter's and a guest experimenter's feeling that the bend was not fraudulently produced, because neither observed the actual bend and some spheres were broken, the evidence of this case was considered to be diminished.

A pendulum arrangement was constructed containing two equal length 5" swinging pendulums in a closed plexiglas cube. One pendulum was selected as target and participants asked to make it swing without touching either the cube or what it was on, which was either a heavy table, a heavy trunk, a three-legged plexiglas stand, or a heavy television stand with wheels. The second pendulum served as a control against use of external physical force to make the target swing, so long as the target did not swing perpendicular to a line between the two pendulums, which would imply application of a rotational force co-linear with the control pendulum. Two participants were on several occasions able to induce up to a $\frac{1}{2}$"-swing in the target while the control remained stationary, on videotape. Successes were obtained in about six of 30 ten-minute trials with bobs of plastic or glass faceted spheres about $\frac{1}{2}$" in diameter. No successes were obtained after approximately ten 10-minute trials each with bobs of wood spheres $\frac{1}{2}$" in diameter, 3/8" metal bolts, or irregularly shaped 3/4" chunks of rose quartz stone.

Notable failures to obtain PK occurred when participants were asked to bend-with-touching 1/8" and 1/4" thick plexiglas strips, to bend metal objects sealed in glass flasks, to concentrate at a distance on an ordinary liquid thermometer bulb to heat it, or to concentrate on a sheet of liquid crystal sensitive to temperature to heat it.

## Discussion

The aim of the research was to determine whether macro-PK influences were being exerted on target systems. Results, while not conclusive, indicate that on some occasions they may have been. The strongest evidence was judged to obtain from: the effortlessness with which some physically impossible bends (for the experimenter) were observed to occur, some produced by young adolescents; the noted absence of fractures and ridges on PK-bent items, as compared to items bent physically over approximately similar time intervals; the successful bending of a brittle hacksaw blade in a U; the bending of a small aluminum bar without touching or producing effects designed to signal use of physical force; and the production of swinging action in one only of two pendulums sealed in a glass case. However, it is the feeling of the experimenter that when and if PK was exhibited it was only infrequently. A conservative estimate is that only six of the 13 participants showed results suggestive of macro-PK. Under the moderately open conditions of the research, none of the participants was ever caught cheating or detected in a deliberate lie or act of fraud, by the experimenter, other participants, or occasional guest observers.

Many results were produced with a seeming lack of conscious technique and amidst casual conversation. This finding is strongly suggestive of the use of passive volition for PK found increasingly in the psi literature. Provision of a period at the beginning of each session when easy bending tasks were assigned that were not assessed for evidence of PK seemed to build morale and enhance subsequent PK attempts. Maintenance of the light-hearted though serious, casual (yet formal when necessary) atmosphere also seemed to help elicit PK. These conditions are similar to those reported elsewhere in the psi literature for successful PK groups.

A future consideration for research of this type is the development of a macro-PK target that can be touched by subjects, and to which some ordinary physical force can be applied without harm during PK attempts. If such a target can be constructed that does not present a task so psychologically formidable that it is discouraging, then investigation of the presence of weak macro-PK ability in the wider population would be facilitated.

# RECENT CASES IN AUSTRALIA SUGGESTIVE OF DIRECTLY OBSERVABLE PK

Jürgen Keil† (University of Tasmania) and Charles Osborne (Caulfield Institute of Technology, Melbourne)

In 1968, reports about directly observable PK created a good deal of interest because at that time, apart from investigations of

poltergeist cases and the work with Ted Serios, little research involving large-scale PK was attempted. Recent reports of numerous mini-Geller cases (e. g., see ZP, 1975, pp. 219-240; 1976, pp. 1-20) and the realization that stage magicians may succeed in creating illusions which are difficult to detect and that children particularly are more often prepared to use deception than might be expected, have led to what might be called a reluctance among some parapsychologists to pursue such PK research opportunities. We do not wish to suggest that psi investigations should be restricted to macro PK but rather that directly observable PK ought not to be neglected and that more opportunity for work in this area may exist than is perhaps generally realized.

A joint investigation of the cases presented here has not been possible so far. However, the authors have had a number of opportunities to discuss relevant matters and have coordinated their work as far as possible. Nevertheless, it seems appropriate to discuss the cases investigated by C. O. and J. K. separately.

J. K. is situated in Hobart, the capital city of an island state with a total population of about 400,000. The population in the greater Hobart area is approximately 130,000. Geller visited Hobart on only one occasion, in 1977. Only two cases were reported to the local television station which had televised an interview with Geller, during which Geller had suggested that PK phenomena might occur among viewers. One case was followed up by J. K. and strongly suggests that paranormal metal bending occurred. It also suggests --in agreement with some others in Europe (EJP, 1979, pp. 21-35)-- that other factors than those which may be said to be associated with psi ability, play an important role. Although only two persons responded to the invitation by the television station to report relevant events, numerous psi phenomena probably occurred in the Hobart area when Geller acted as a catalyst.

In this relatively small, stable and somewhat conservative community, persons who experienced phenomena were generally concerned with keeping such experiences as guarded secrets. J. K. has private reports of four further cases. Two of these could not be followed up because the persons concerned were not prepared to reveal their identity and to discuss details with J. K. Among the other two cases metal-bending apparently occurred only once in connection with Geller's visit to Hobart. No further deliberate attempts had been made and no further paranormal phenomena were noticed.

In the cases discussed so far, J. K. had only become aware of them after the event. With no further occurrences, they can only suggest PK without any evidence that might be taken seriously by a skeptic.

J. K. became aware of one further case through a professional acquaintance in Hobart who had observed metal-bending produced by a close relative (V. P.) who lives in Melbourne. J. K. was able to visit V. P. on three occasions and to observe deformations of spoons

on two occasions. V. P. is a middle-aged male in a responsible work position. The phenomena are only known to a few close relatives and friends. After seeing a televised program about Geller, V. P. felt he could do something like that, tried and succeeded. He has bent metal objects apparently by paranormal means on numerous occasions over a period of about three years. In March 1980, V. P. bent a spoon completely wrapped in plastic foil (secured with tape) which was purchased for the attempt by J. K. This spoon, together with similar ones, was cleaned with petrol, wrapped in foil and has not been directly touched. It is hoped that it will be possible to make use of these arrangements for investigations of the microstructure of the metal.

V. P. 's metal-bending was observed by J. K. under conditions which make it unlikely that the distortions could have occurred by normal means. The spoons were lightly held by one hand and were in clear view by J. K. However, to date no cine or video recordings could be arranged and J. K. does not wish to claim that a professional stage magician could not have produced similar results using the tricks of his trade. The strongest evidence in favor of psi was the continuation of a distortion of the spoon wrapped in plastic after V. P. had placed it on a table and did not touch it at all. The spoon continued to bend by approximately 30 degrees. Without a video record perceptual errors cannot be ruled out with absolute certainty, but appear rather unlikely. There were no distractions during the observations.

V. P. 's wife and relatives had observed similar phenomena on various occasions. One relative had also been able to bend a spoon which she was holding with one hand while she grasped V. P. 's hand with her other hand. Numerous background factors known to J. K. suggest that the observed phenomena associated with this case can be regarded with considerable confidence. V. P. has so far mainly produced these phenomena in order to satisfy his own interest and curiosity, as well as for his own amusement and that of a few others. Under these conditions he has been successful in a high percentage of his PK attempts. V. P. believes he recognized a sensation which indicates to him that metal-bending is likely to occur. During the two occasions when J. K. had an opportunity to observe these phenomena, they started within two or three minutes after V. P. had picked up a spoon. V. P. found that he could more easily start the process if he did not himself observe the object which he tried to bend. Otherwise there were no particular features associated with this case.

In general, a relaxed and quiet atmosphere prevailed. V. P. had not been under pressure to demonstrate the phenomena on demand. It is hoped that investigations can be continued, although V. P. is understandably reluctant to engage in any public or semi-public activities which could have detrimental repercussions in association with his responsibilities in his work position.

C. O. lives in Melbourne, a city of approximately 2. 5 million

inhabitants. Geller had visited Melbourne on three occasions. C. O. had obtained a copy of video recordings of Geller which had been televised by one of the commercial stations. C. O. played the recording to six groups of students (approximately 20 students in each group) but no phenomena occurred.

C. O. then approached persons who had claimed success and had contacted television stations after C. O. had explained his interests in a televised interview. C. O. received more than 1000 replies, and by testing about 800 has so far found about 40 persons, mainly children, who were able to bend metal under controlled conditions. Movements of mobiles could also be observed in connection with a few of these cases.

Since C. O. was able to examine the claims of a large number of persons, detailed background factors could only be considered in some of the cases. However, it was possible to carry out tests under laboratory conditions and in particular the following controls were introduced. (1) Initial testing procedure for metal-bending. All spoons are standard household spoons which are blackened both front and back of the bowl. All spoons must be bent with one hand holding the extreme end opposite the bowl and one finger gently stroking the middle section of the spoon. This is observed via eye and with TV monitors. The bend must exceed 5°, there must be no blackening of the hands and the whole operation must be viewable on the TV screen throughout the bending. The film is shown to at least two independent judges and if they say they think the protocols have been satisfied, then we pass to stage 2. (2) Subjects who succeeded were then asked to bend a flat strip of 5mm. thick aluminum (20mm. wide and approximately 150mm. long) which is presented in a cradle made from rigid perspex. Under these conditions a subject cannot apply any normal force to the metal which would result in bending the aluminum. Although the metal strip can bend out of the cradle it cannot be readily removed while it is flat. Subjects were allowed to touch the metal. Forty subjects passed this test as well. (3) Attempts were made to register smaller metal distortions with the aid of a strain gage while the subject does not touch the metal to which the strain gage is attached at all. C. O. registered variations but is not satisfied that these variations cannot be explained by uncontrolled normal variables. At any rate, this is a problem when the changes registered by the gauge are relatively small. Further improvements are presently incorporated in the design of the setup. As a psychological aid to subjects, a device providing a sound signal which co-varies with the strain gage changes is also under construction. Possible paranormal temperature variations are also under investigation.

With three subjects (two 15-year-old males and one 17-year-old female) C. O. has obtained turning movements of dumbbells suspended in a perspex cover under a Faraday cage made from copper fly wire. The bar (approximately 75 mm. long) with dumbbells is centrally suspended, in such a way that it can rotate in a horizontal plane. An ordinary cotton thread fixed to the clear perspex cover

is used for the suspension. Rotations of 360 degrees were observed. Subjects were able approximately to control the angle through which they moved the dumbbells, and they were able to keep the device in a new position for approximately five seconds before the dumbbells returned to their original position. In contrast to a report in 1978 by Balanovski and Taylor, subjects continued to produce these phenomena after antistatic spray had been used. Dumbbells made from different materials such as wood, brass, glass, aluminum and iron were moved successfully in a similar way. The maximum weight of the suspended device can be about 50 grams. Subjects are able to produce a movement from approximately 25 cm. They have greater difficulties if the distance is increased.

## A STUDY ON PK ABILITY OF A GIFTED SUBJECT IN JAPAN

Toshio Kasahara, Nobushige Kohri, Yasuhito Ro, Shigeyuki Imai and Soji Otani† (The Japanese Society for Parapsychology)

The present paper is concerned with the claimed PK abilities of a gifted subject, Masuaki Kiyota, a 17-year-old boy. In this report some preliminary findings derived from our PK sessions with him will be presented.

Three preliminary experiments with Masuaki were conducted in which he was allowed to perform PK freely, and was observed and interviewed. Two preliminary experiments in spoon-bending were carried out successfully in the subject's and S. O. 's houses on September 9 and 23, 1979, respectively. Experiments in thoughtography were carried out also in S. O. 's house on September 23 and October 7, 1979. These preliminary experiments showed: that it took some time for the subject to change his psychological state to favor performing paranormal phenomena, that we experimenters and observers needed to cooperate with him to facilitate this and that it was difficult for us to predict if and when the conditions were actually realized. Consequently, in our main experiments we intended to monitor his behavior during sessions with a video camera.

The main experiment consisted of three series which were conducted on November 17, 1979, and January 26 and February 23, 1980 at the laboratory of the hospital attached to the National Defense Medical College.

When the subject arrived at the laboratory, we first engaged him in relaxing conversation for about 30 minutes and then guided him to the Faraday cage, in which he sat in a rattan reclining-chair. One of the experimenters attached electrodes on his head, arm and abdomen for physiological measurements. In the adjoining room were the measuring apparatus and eight to ten observers, including experimenters and technicians. The door between the two rooms was

kept open during the experiments and observers could also see the subject through a window.

The main purposes of the first series were to help the subject become accustomed to exhibiting his ability in a new situation, and to measure physiological conditions in his normal state. A thoughtography experiment was conducted first. One of the experimenters, T. K., handed a model 355 Polaroid camera to the subject. He held the camera in both hands with the lens directed toward his face. There were seven trials with intervals of several minutes, and the results were unsuccessful except for the fifth trial in which there was a pattern just like the surface of a blanket over the whole photo.

At 7:30 p. m. the spoon-bending experiments started. Metal bars, prepared by Y. R., and an ordinary coffee spoon were handed to him. After he had stroked the spoon for a few minutes the bowl of the spoon began to bend upward. The spoon was twisted about 180 degrees at its neck.

Due to successful results of the first series, we designed a second, more strictly controlled experiment. A video recorder was used to record the whole course of the experiment. We asked the subject to break a spoon by PK in order to inspect the fracture with an electron microscope. In addition to the physiological measurements, two other measurements were introduced. These were: a continuous measurement of the subject's body temperature, and a biochemical inspection of his blood before, during and after the subject's PK performance.

In the second spoon-bending experiment five spoons were used, four prepared by T. K. and one by Y. R. The subject began to stroke a spoon at 7:07 p. m. and at about 7:18 p. m. EEG measurement was stopped at his request. At 8:18 p. m. he asked resumption of the measurements. Several seconds later he gave a signal. The observers noticed the spoon had twisted 180 degrees at the neck, but unfortunately none had observed the course of bending. At 8:55 p. m. the subject said that the spoon had broken. Unfortunately, again none of us saw the breaking process.

At 8:57 p. m. the thoughtography experiment started. The Polaroid film was put into a Polaroid developing apparatus made by Mamiya Optical Inc. which has no lens or shutter. In the second trial a figure something like an orange-colored flame was observed. At 10:03 p. m. he started the third trial. At 10:06 p. m. he gave a signal and said a blue color might have appeared. After developing, a blue color had appeared on the film in an area from the center toward the edge of the photo. At this point it was decided to terminate the experiment.

Masuaki expressed his intention to try a thoughtography experiment again, along with video recording. Masuaki said he would try to expose all the film's eight sheets in the camera at the same

time. He began to concentrate at 10:36 p. m. At 10:39 p. m. he relaxed and T. K. received the camera from him and developed the first five photos in succession. Exposed phenomena with blue color were observed in all five photos. (The remaining three photos were also found to show exposed phenomena.)

The aim of the third series was to get more reliable, objective data on spoon-bending and thoughtography and a recording of all the processes of the experiment from beginning to end. However, the subject seemed to be in low spirits that day. There were eleven unsuccessful trials. At 9:05 p. m. the subject came out of the Faraday cage. After a short conversation, he re-entered the Faraday cage and lay down on the bed. He held several spoons in his hands and threw them toward N. K., who was standing by the bed. N. K. noticed one of the spoons was twisted about 180 degrees.

The fractured surface of the spoon broken in the second series was inspected by one of the experimenters, Y. R., a metal engineer, using a scanning electron microscope. The fracture surface of the spoon is a typical ductile one. It could be presumed that the fracture was initiated at the front side position, where the glide plane decohension or elongated dimples may be observed. Consequently, the ductile crack would have propagated from the front side to the back side. No clear striation, while would be a verification of cyclic stressing, could be observed on the fracture surface.

The records of ECG, respiration, EMG, GSR and eye movement of Masuaki did not show any significant deviation from the normal standards. The skin temperature increased from 33.3° C to 35.0° C during the experiment. There was no distinct difference between the right hemisphere and the left one. No abnormal wave appeared when photic and sound stimulus were applied to the subject. From the visual inspection of EEG records it may be said that there is a slight decrease in the frequency five or seven seconds before the bending of the spoon. In biochemical investigation of his blood, there were no significant differences in ACTH, 11-OHCS, TSH, triiodothyronine, GH or DOPA from normal standard values. However, one interesting finding was that the insulin density increased in the course of the experiment, and the C-AMP density decreased.

It is said that Masuaki has produced a very clear photo by his PK ability, but in our experiments he produced only partial exposure phenomena. The photos produced by Masuaki in our experiments were probably caused by some process other than normal exposure.

There was no trace on the spoon which showed that some tool was used to bend the spoon. The result of the EMG record did not show that the subject moved the muscles of his hands. The data we have obtained did not show any proof of abnormal breaking, although the experimental situation seems to support supernormal breaking. The EEG pattern during the experiment suggests that the

subject had been in a rather relaxed mental state when he engaged in spoon-bending and thoughtography. This is in accord with the fact already demonstrated in other psi experiments, that a relaxed mental condition is favorable to psi performance.

# PROBLEMS OF RANDOMNESS*

## ARE PREPARED RANDOM SEQUENCES AND REAL TIME RANDOM GENERATORS INTERCHANGEABLE?

Charles T. Tart (University of California, Davis)

Electronic random number generators (RNGs), which generate a random target number on demand for each trial, became widely accepted in parapsychological research following their use by myself (JASPR, 1966, pp. 256-269), Schmidt (JP, 1970, pp. 219-224), and Targ and Puthoff. Numerous studies have now shown, however, that electronic RNGs are susceptible to PK effects. Thus, when an electronic RNG is used as a real time generator in any ESP experiment, the possibility exists that some unspecifiable proportion of significant results might be due to PK action rather than ESP.

One result of this ambiguity has been a call by several colleagues to use prepared random number tables, stored targets, as standard operating procedures. If prepared, stored random numbers and real time RNGs are indeed experimentally equivalent, this suggestion has great merit. The data presented here suggest, however, that they are not experimentally equivalent.

In two reported studies using a real time electronic RNG, I found a strong and statistically significant negative correlation between positive scoring on a designated real time target and +1 precognitive scoring on the upcoming but not yet determined next target. I proposed a theory of transtemporal inhibition to account for this (RIP 1977, pp. 197-250), hypothesizing that the part of the mind which uses psi is less "localized" in space and time than our ordinary brain-mediated consciousness. Simply activating this psi part of the mind per se might produce ESP-mediated information not only about the desired, real time target, but also about immediately temporally adjacent targets as well. On those trials on which ESP is activated, some non-conscious part of the mind discriminates the immediately future and real time targets, and then creates a psychological inhibition against calling the immediately future target, thus enhancing (by contrast) the chances that the real time target will be

*Chairperson: Tom Greville, University of Wisconsin, Madison

called correctly. Insofar as trans-temporal inhibition theory is valid, it suggests that an already prepared, stored sequence of targets is not experimentally equivalent to a real time RNG, where a new target is not determined until after a previous response. In the already prepared target case, at a time of any given response all of the targets exist and some sort of "spatial" discrimination, as well as possible temporal discrimination, may be required by the percipient. Thus different kinds of performance, in terms of hitting or missing on immediately future targets, might be expected when comparing stored random target sequences with target sequences generated by real time RNGs.

In the course of a third study on the effects of immediate feedback on ESP performance, a modification of equipment was made which allowed a test of whether percipients responded differently to stored targets than to targets generated on line. The third training study was similar in procedure to that of the two previous training studies (JASPR, 1979, pp. 151-165). The main test device used was the Apple-ADEPT ten-choice training device (A/ADEPT), a more sophisticated version of the original ten-choice trainer (TCT), which was also used. An Apple II microcomputer controlled the functioning of the display and response consoles, as well as electronically recording data. The basic task on each was to push the button beside one of the ten unlit lamps that corresponded to the current target.

While an on-line, real time electronic RNG was always used with the TCT, the A/ADEPT was modified so that, unbeknown to the experimenters or percipients, it randomly chose to use either an on-line, real time RNG or to use a sequence from a stored table of RNGs on particular runs.

As a group, the 15 percipients did not show any evidence for ESP functioning in terms of hitting on the intended, real time target. They made 609 hits when 591.3 were expected by chance, corresponding to an insignificant Z-score of +.77. One individual percipient scored significantly above chance (53 hits when 40 were expected by chance, $p \cong .03$, 2-tailed), but this is probably not significant when 15 individual significance tests were conducted. There were no suggestions of significant differences (by t-test) between the TCT and A/ADEPT devices, or between the real time RNG and stored target conditions of the A/ADEPT for real time hitting on the designated target.

The hypothesis of a scoring difference on +1 future displacement scoring between real time and stored targets was tested in two ways. First, the real time and stored target conditions on the A/ADEPT were compared by a t-test between the standard Z-scores of each percipient on +1 scoring. The mean Z-score for real time RNG targets was -.13, while for stored targets it was +.78, $t = 2.11$, $p \cong .06$, 2-tailed. Second, the RNG conditions on the TCT and A/ADEPT are essentially equivalent (both on-line, real time), so those data were combined for a more sensitive test of +1 differences with a larger data base. The mean Z-score for scoring on +1 dis-

placements with the real time RNG changes trivially to -.06, and for the comparison, $t = 2.34$, $p < .05$, 2-tailed.

With little or no apparent ESP manifesting on the intended real time targets, the effects of trans-temporal inhibition on the hypothesized non-equivalence of real time RNG and stored target conditions might be expected to be minimal or non-existent. Nevertheless, a significant difference was found on +1 precognitive scoring, and the study suggests that stored random number targets and targets generated by real time RNG are not equivalent experimental conditions in terms of the way psi works. (See Erratum, p. 152.)

## PSI SCORES WITH RANDOM AND PSEUDO-RANDOM TARGETS

Gertrude R. Schmeidler† and Randall Borchardt (City College of the City University of New York)

Donald and Martin (EJP, 1976, pp. 17-36) set forth a theory for the physics of psi, based on time-symmetric thermodynamics. Their thermodynamic equations are solved for both positive and negative values of time, thus showing future events influencing past events.

The theory makes many predictions about psi. Some are strongly supported; some untested; none disconfirmed. One untested prediction is that "results of psi tests using truly random events will be more significant than those using pseudo-random events" (p. 33). We performed an experiment to examine this.

Methodological decisions were: (a) to control individual differences in psi ability by using a within-subject design; (b) to determine order of conditions randomly; (c) to control conscious preference effects by keeping both experimenter and subject blind to the target condition (though obviously extrasensory knowledge could not be shielded); (d) because our random events generator (REG) lends itself readily only to precognition or PK and because a fully pseudo-random series permits clairvoyance, to initiate our pseudo-random sequences from random choices; (e) to analyze hits by subject preference and run order as well as by target type.

An REG designed and constructed by Edwin May (and generously donated by Charles Honorton) produced random binary sequences. The subject throws a switch; the instrument displays the outcome.

A table of random numbers provided the odd and even digits for pseudo-random sequences. Each subject drew slips of paper from concealing envelopes to designate page, column, and row for entering the table.

Subjects were 50 volunteers, chiefly college students: 48 sheep and two supersheep. Thomas Krusz tested seven and G. R. S. tested 12 of the first 19 subjects; R. B. tested the last 31. Experimenters' scores showed no significant differences and were pooled.

The experimenter introduced the procedure by saying that it was like guessing how a coin would fall, and tossed a coin to illustrate. Without observing its fall, the experimenter pushed it beneath some papers. It remained concealed until all calls were completed. Its toss determined whether the first two runs were for random or pseudo-random sequences. The subject checked a supersheep-sheep-goat questionnaire. Sheep or supersheep then made 50 binary calls, rested quietly until they declared themselves refreshed, and made 50 more calls.

The experimenter next examined the coin. Targets were determined first for the first two runs. For pseudo-random targets, the experimenter explained how the table showed odds or evens, the subject selected slips of paper to enter the table, and 50 successive digits specified the targets. For the random sequence, the experimenter took the subject to another room and demonstrated the REG. The subject pressed its switch to determine 50 targets, which the experimenter recorded on a separate page. Forty-six subjects were asked to express preference as to method. All subjects saw their scores before the session ended.

There was insignificant psi-missing for both random (deviation = -45) and pseudo-random (deviation = -40) sequences. Total scores were significantly low ($t = 2.72$, 49 df, $p < .01$, two-tailed).

Only two subsets of scores were significant, both for the subjects where pseudo-random sequences came first. The eleven subjects who preferred random sequences psi-missed for random sequences on Run 3 ($t = 3.50$, 10 df, $p < .01$, two-tailed). The ten subjects who preferred pseudo-random sequences psi-hit for pseudo-random sequences on Run 1 ($t = 2.57$, 9 df, $p < .05$, two-tailed).

To find if subjects were self-consistent, a routine (but not a formally planned) analysis was performed: correlating scores within each pair of runs. Runs with random sequences showed a low but significant positive correlation ($r = +.28$, 48 df, $p < .05$, two-tailed). The correlation for pseudo-random sequences was $r = +.02$. The difference between correlations was not significant. The correlation between totals for random and pseudo-random sequences was significantly negative ($r = -.29$, 48 df, $p < .05$, two-tailed), but since the correlation between first and second halves was -.27, this may be only an order effect.

When we evaluate the data, both subjects' typical reactions and the psi-missing for total scores permit one trivial conclusion: the task was dull.

For comparison of random with pseudo-random sequences,

preplanned analyses showed no major differences. Each type had one significant score (when not corrected for selection). Neither showed overall significance. Random sequences perhaps showed a faint, nonsignificant trend toward more extreme scores. It is striking, however, that random sequences showed a significant positive correlation between successive runs, while pseudo-random sequences did not. This gives Donald and Martin's theory some support and deserves further testing.

For subsequent research on random versus pseudo-random targets, we recommend continued attempts to control experimenter as well as subject preference, and order effects, but a change to a more interesting procedure.

## PK TESTS WITH PRE-RECORDED AND PRE-INSPECTED SEED NUMBERS

Helmut Schmidt (Mind Science Foundation)

Psychokinesis was first observed in the laboratory as a mental effect on ongoing random processes that occur in the fall of dice or in electronic random number generators. Later experiments indicated that PK is basically different from the known physical forces. Specifically, it appeared to operate independently of the complexity of the random process to be affected and to operate even when the subject made his conscious PK effort after the random process had taken place. Thus one might see PK, tentatively, as a goal-oriented principle that cuts through the conventional limitations of space, time and complexity of the task.

In order to study this feature of the PK mechanism further, the following experiment uses as targets for the PK efforts a sequence of quasi-random numbers which are completely determined by one randomly selected "seed number," as follows. We have a microcomputer programmed such that it can generate a sequence of quasi-random numbers (in the range 1, 2,..., 16) such that the sequence repeats only after about half a million steps. This cyclical sequence or "ring" of about half a million numbers appears to the casual observer as random, but still the numbers are completely determined by the computer program. For the PK experiment we select with the help of a truly random process an entry point into this number ring and then we take the approximately 500 subsequent numbers as targets for the PK effort in a test run. The random number which determines the entry point into the ring is called the "seed number." Then the score of a run will depend on the section of the ring selected, i.e., on the seed number. And if PK operates in the goal-oriented manner indicated, then it might focus the subject's effort during the test run backward in time towards the selection of a seed number which leads to a good score. Before the test

sessions were begun the seed numbers for all runs to be made were generated (with radioactive decays as source of randomness) and stored on a memory chip in the test machine.

In a test run the random numbers (1, 2,..., 16), equivalent to the outcomes of throws with a "16-sided die," are retrieved at a typical rate of eight per second. These numbers serve to generate two types of random time intervals, the ON-intervals alternating with OFF-intervals. During the ON-intervals the subject sees a light rotate clockwise (one step for each generated number) in a lamp circle and hears a gong tone. During the OFF-intervals the light stops and no tone is present. The run starts with an ON-interval which lasts until a "3" has been obtained on the "16-sided die." Then follows an OFF-interval which continues until a "12" appears. The subsequent ON-interval lasts again until the next "3", and so forth, and the run ends after 16 ON-OFF pairs have been completed. The subject's goal is to extend the duration of the ON-intervals (above the expectation length of 16 steps) and to shorten the OFF-intervals. The success is measured in terms of total numbers H' and M' of steps in the ON- and OFF-intervals respectively.

In exploratory experiments subjects were encouraged to try a variety of mental approaches to the PK tasks, such as:

a. Try to keep the light moving effortlessly by visualizing a lively mountain stream which goes on and on.

b. Try to keep the light moving while listening to light music, feeling your body following the music and the light motion.

c. Remember, while working on the light, a situation in your life where you were successful and had an exuberant feeling of winning.

d. Relax and feel easy going while the light is moving but make a tense, determined effort when the light stops.

e. Listen to the gong tone and try to "hang on" to the tone as long as possible. In the quiet periods expect the tone to start any second and be prepared to hang on to the next tone.

f. Try in an ego-less manner to meditate on each tone and to experience the tone to its fullest.

At this exploratory stage, one formal test series of prespecified length was done, with the experimenter as only subject. Ten sessions of five runs each were held on different days. In five sessions a meditative, relaxed approach was used. In the other five sessions the approach was forceful and dynamic, frequently accompanied by lively music. These two types of sessions were al-

ternated. A total of H' = 13577 hit events and M' = 12263 miss events were obtained. The difference--(H' - M') = 1314--corresponds to a Z = 2.12, significant at the .02 level (one-tailed). The values obtained for H' - M' under the relaxed and forceful conditions were similar (733 and 581 respectively).

Apart from testing the existence of PK under the given conditions, the main experiment had one further objective. Previous experimental and theoretical work on PK with pre-recorded targets had raised the question whether it would make a difference whether or not the pre-recorded targets were inspected by the experimenter before the test sessions. Since no such pre-inspection had been done in previous work, in the present study half of the seed numbers were printed out by a computer and inspected by the experimenter in advance. Note however that the seed numbers gave the experimenter no clues about the resulting scores because these scores were calculated with a highly complex computer program.

For the main experiment it was decided to perform a total of 100 test runs with predominantly unselected volunteers, and to complete another 50 test runs with a few particularly promising, selected subjects. It was decided to evaluate these two groups separately. Prior to the test sessions, two blocks of 100 and 50 random seed numbers (blocks U and S respectively) were generated and recorded in different sections of a memory chip.

A selector switch on the test machine allowed experimentation under three conditions labeled P (play), S (selected subjects), and U (unselected subjects). In the play mode (P), the seed number for the test run was obtained by activating a true random number generator in the machine; whereas, in the setting S and U, the seed number was taken from the corresponding memory block. The seed numbers in the memory were accessed in the sequence in which they were recorded, and each number was automatically checked off so that it could not be used twice.

Most sessions were begun with a number of play runs to acquaint the subject with the different psychological approaches and forms of feedback. These play runs were not systematically recorded.

After the play runs a flexible number (average: eight) of test runs were done with seed numbers from block S for the most promising subjects, and from block U for the others. The decision when to terminate a session was made rather subjectively, depending on the scores and the subjects's and experimenter's mood and confidence. In a few cases where the subject appeared uncomfortable and prone to psi-missing under all conditions, the session was terminated after the play runs. A total of 11 subjects contributed to the U group. Subjects could participate in more than one session. Four subjects were selected for the S group. Three of these were known to the experimenter from previous work. The fourth was a newcomer who impressed the experimenter with his confidence and his

proficiency in martial arts. He was the only one in this group to obtain a negative score.

The result shows that the total score of the S group is significant with Z = 3.42 at the .0005 level (one-tailed) and that the half of the runs for which the seed numbers were pre-inspected is still significant with Z = 2.68 at the .005 level. For the U group of unselected subjects, the scores are lower: The total significance for the U group is with Z = 2.19 at the .05 level and the contribution of the runs with pre-inspected seed numbers provides a Z = 1.45.

Thus the results indicate that PK effects under the given conditions can be observed, even in cases where the seed numbers which determine the outcome of the test runs are pre-inspected by the experimenter. These findings are of practical usefulness for experimenters who want to use computers for PK experiments. Then it seems permissible to use the available quasi-random computer programs for generating the PK target sequence provided that only the underlying seed number has been determined by some truly random process.

## PHILOSOPHY OF PARAPSYCHOLOGY*

### CAUSALITY AND SYNCHRONICITY: STEPS TOWARD CLARIFICATION

Charles T. Tart (University of California, Davis)

Carl Jung's concept of meaningful but acausal events, synchronistic events, has been of great interest to parapsychologists and also a source of much confusion. I believe some of the confusion stems from Jung himself, as well as from later commentators: e. g., Jung gave examples of what I would call paranormal causation as examples of synchronicity. This paper is a preliminary attempt at refining and clarifying concepts of causality, paranormality, and synchronicity.

In the development of ordinary (consensus) consciousness a major step is the creation of internal representations, symbols, for external events. We can then sometimes observe regular proximities and orderings of events, correlation. When we attribute the responsibility for this correlation to an external mechanism, we have created the concept of causality. We may verify the usefulness of this attributed causality on the basis of the strength of the observed correlation, and/or on making predictions about new situations, and/or on creating a mental model or explanation of presumed hidden causal mechanisms. Now let us define various types of causality and synchronicity.

Physical causality: We observe a proximity/ordering between physical world events and can construct a model of the causal process by drawing on already known mental models of the physical world. I release a ball from my hand, e. g., watch it fall (perception), and say that the "force of gravity" (a mental model based on previous experience) "caused" it to fall.

Presumed physical causality: Again we observe a proximity/ordering between two perceived physical events. Although we cannot make a clear mental model of the physical cause in terms of models we already have, we assume that further understanding (modeling)

*Chairperson: Martin Johnson, University of Utrecht

of the physical world will eventually lead to an explanation. This kind of faith is useful in stimulating inquiry: carried to an extreme, it is a pathological limitation of cognition.

Psychological causality: We observe proximity/ordering between people, persons and physical objects, or mental events and model the cause of the proximity/ordering in terms of psychological factors. We might explain the attraction of older women for a young man in terms of an unresolved Oedipal complex, for example.

Presumed psychological causality: Analogous to presumed physical causality, we observe a proximity/ordering between psychological events and although we cannot now create any kind of psychological model of causality, we presume that extension of our psychological knowledge will eventually provide one. As with presumed physical causality, this may be a matter of small extrapolations from current knowledge or a global leap of faith that may be a cognitive pathology.

To those who hold a psychoneural identity position, these forms of psychological causality are not fundamental, as they will presumably be reduced to cases of physical causality as knowledge of brain functioning grows. All four categories also implicitly assume that the human mind, in its ordinary state of consciousness, has the capacity for modeling (explaining) all significant events.

State-specific causality: From the point of view of my systems approach to consciousness (RIP 1973, pp. 163-218), our ability to understand/model in our ordinary state is limited, but can be supplemented by altered styles of perception, consciousness, and action in various altered states of consciousness (ASCs). What may be unrelated happenings in an ordinary state may be clear proximity/orderings, with creatable mental models (postulated causes) in some ASC. Thus we have state-specific causality. Some state-specific causal explanations may lead to predictions that can be verified in ordinary states; others may only be comprehensible to people in that ASC, analogous to the way certain mathematical explanations are only understandable to the changed state resulting from years of study of advanced mathematics.

Paranormal causality: We observe proximity/orderings (A tries to send a telepathic message to B and succeeds a significant percentage of the time), but no models are available in terms of physical world models. Motivation can sometimes lead to effect although the intermediate stages are unknown. We presume there is an intermediate mechanism, however, which will someday be susceptible to useful modeling: thus paranormal events can be causal.

Being-specific synchronicity: Here we recognize the ultimate limits of man's cognitive abilities: we observe significant proximity/orderings but can find no explanation. They are caused (we presume), but our being is such that we will never be able to model/understand the causal mechanism. They are caused at a synchronis-

tic (to us) level. For example, there are things whose causal mechanism is obvious to me, but are synchronistic to my cat. Perhaps there are intelligences for whom obviously causal events are synchronistic to us. It is important to distinguish being-specific synchronicity from state-specific synchronicity by trying to develop state-specific understandings of events that appear synchronistic in our ordinary state of consciousness.

Indirect being-specific synchronistic causality: This illustrates complexities that can arise. A caused action on our parts might sometimes initiate causal (but to us synchronistic) chains on a synchronistic level, leading to inconsistent yet somehow meaningful events.

Absolute synchronicity: We observe proximity/orderings between events, but they are truly not caused at any level. Things "just happen" to be meaningfully related. While we can define this category, it is not clear it will ever be meaningful to us.

Several apparently paranormal and/or synchronistic events happened in conjunction with the writing of this paper that could be interpreted as confirming its usefulness. It is hoped that the above categories will contribute to more meaningful and clear discussions of the paranormal and synchronicity.

## WHAT IS WRONG WITH DISEMBODIED SPIRITS?

Frank B. Dilley (University of Delaware)

Disembodied spirits pose no threat of contradiction to common sense or science. Disembodied spirits are para-scientific but they do not contradict physical science. A scientist may dispense with disembodied spirits on the grounds that proper methodology precludes including them in a scientific world view, but to be properly cautious that scientist should realize that the mere fact that science has no need of such hypotheses does not thereby disprove disembodied spirits. Disembodied spirits have no place in physicalistic versions of science because science allows them no place. Occam's razor renders disembodied spirits para-scientific. It also renders ordinary embodied minds para-scientific, and that is what this paper is all about. Disembodied spirits and ordinary embodied minds are tied together, conceptually.

Once this issue is faced squarely, worries about disembodied minds based on the fact that they are para-scientific (supernatural) should be set aside. Minds themselves are para-scientific, and so are their contents--feelings, thoughts, sense images and the like. Our world as felt is para-psychological. I want to shore up the concept of disembodied minds and their powers by making clear their

conceptual continuity with ordinary embodied minds and their powers. Most of us feel perfectly comfortable with embodied minds and their powers and, unless we are philosophical, might never have noticed their super-natural qualities. Drawing attention to mind's super-natural status may make us more comfortable about disembodied minds.

What I will argue is that standard dualistic accounts of how mind is related to body require that mind be supplied with the very qualities that it would need to be able to act, learn, etc., in a disembodied state. In short, embodied minds have to be endowed with parapsychological nature and powers. Mind is outside of nature, and both clairvoyance and PK are needed to explain the relation of a mind to its body in ordinary so-called embodied cases. Popular understanding often fails to appreciate this because of persistence in thinking of minds as located in skulls, or at least linked to skulls by a set of quasi-mechanical connections; hence we think of mind-brain relations as transactions of an ordinary sort, but there are notorious difficulties with this view.

To make mind-brain interaction work, we must suppose that embodied minds have "parapsychological" powers. What we call parapsychological powers are really quite ordinary powers extended beyond their normal manifestation. It should be noted that these powers are para-psychological and para-physical. They do not operate in accordance with laws of physics. Both parapsychologists and physicists are in general (but not unanimous) agreement that psi powers do not behave by physical rules. This should not be surprising, but often is. Minds neither exist nor operate by normal physical rules. Physical properties are electro-chemical, and mental qualities are not. Physical transmissions involve waves and particles but sequences of thoughts do not. Minds, then, are outside the realm of physics, both as to their nature and as to their internal processes. It is not strange, then, that the processes by which minds are related to brains fall outside the scope of physics as well.

Ordinary mind-brain relations have nothing to say about precognition, but they do provide a basis for concluding that at least two parapsychological powers, PK and clairvoyance, exist. The trouble with a third phenomenon, telepathy, is that it is difficult to see how it could occur. How can one mind causally affect another mind other than indirectly? I join those philosophers who want to reduce telepathy to clairvoyance or PK or disallow it.

If the observations I have made so far are sound, some philosophical objections to disembodied minds on the grounds that such minds could not communicate, or act, or acquire new knowledge, or meet other minds, can be disposed of quickly. Others cannot. Since embodied minds do manifest clairvoyance and PK, disembodied minds can be thought to have those powers also. But if telepathy is not allowed as a possible power which embodied minds could manifest, then disembodied minds could become aware of other disembodied minds only through their use of physical objects. Dis-

embodied minds would have the same limitations as embodied minds, namely that they could become aware of each other only indirectly, through the bodies they use. Disembodied meeting is no more problematical than is embodied meeting of persons. Each occurs only through physical objects, but disembodied meeting could not occur except through bodies.

That disembodied minds in a world of pure spirit could not meet does not mean that they could not do anything, of course. They could meditate, and they could digest knowledge they already have, and they could contemplate the sorts of persons they have been and perhaps repent of past misdeeds. It might not even be important that souls communicate at each stage of their life cycles.

Once we are agreed that disembodied minds could exist, and act, learn and even form communities as long as there are bodies around for them to use, another question arises. How does a disembodied soul establish who it is? It cannot look in a mirror to see who it is. Can it consult its memory? Can it ask its neighbors? Part of the Cartesian view, which is part of the framework of the beliefs of most ordinary persons, is that the soul or mind is the self and that the self is able to transcend bodily death by continuing to exist after the body dies. But how can souls be identified?

In the space remaining some things will be said about this problem. It should be noted here as well that the question of identity of disembodied persons is closely related to the question of the identity of embodied persons. It should also be noted that neither embodied persons nor disembodied persons can be identified with certainty (can be <u>certain</u> that they are who they and their friends think they are). Cartesian minds can suffer from Cartesian doubts, and the possibility of being mistaken as to who one was is inherent in all Cartesian views of self.

Short of appealing to God, disembodied souls (souls when they are unrelated to bodies) cannot be identified infallibly. Personal continuity cannot be established because no one could possibly witness that personal continuity was true, and memory needs the prior establishment of personal continuity as a criterion to distinguish between true and false memories. Does this situation cause special problems with disembodied souls that mean we should never use the notion that an unrelated soul is the same soul as the one which previously was related to a particular body? My view is that the problem of identification of disembodied souls is of the same character as the problem we have in identifying <u>with certainty</u> an embodied soul.

In both disembodied soul identification and embodied soul identification we have a similar problem with certainty. But if we are Cartesians, we have learned to live with that sort of uncertainty. For Cartesian doubts we counsel reasonability; however, something can be reasonable and still be false. I would argue that there is a strong presumption that any soul-claimant is the soul it "remembers"

itself to be. There are circumstances under which memories could be false, and if it generally turned out to be true that people had false memories it could even become reasonable to think that memories are not a good basis for deciding who one was; but in the absence of known conditions of that sort, the reasonable thing to do is trust the memories.

There is nothing wrong with disembodied spirits. The objections philosophers raise about disembodied spirits can be raised equally well about embodied ones, and the replies that work for embodied souls work for disembodied ones as well (or as badly). One reason people sometimes miss this sameness of condition is that they have not thought enough about the soul-brain relation as it occurs in ordinary persons. Once it becomes clear that embodied souls are para-normal, and that the powers they use to relate to bodies are also para-normal, then any tendency to differentiate the problems of disembodied souls from those of embodied ones disappears. Admittedly, souls need bodies to communicate with other souls (it needn't be their bodies, any bodies will do), and souls perhaps need their bodies if problems of identification arise, but in these respects disembodied souls are in the same situation as embodied ones are. Neither embodied nor disembodied souls deal directly with each other.

Both have the same powers and because of their privacy they have the same difficulties in achieving certainty about who they were in past times. This lack of certainty makes for fine story-telling possibilities for some fantasy writers, but we need not worry about the intelligibility of the concept of disembodied souls or about their identification.

# POLTERGEISTS*

## A POLTERGEIST CASE WITHOUT A POLTERGEIST AGENT

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

Ostensible poltergeist phenomena are reported occurring in a 258-year-old southern New Jersey house which, in 1978, was converted into a gift shop. Such disturbances as the recurrent, inexplicable malfunctioning or operation of electrical equipment, kinetic influence on objects, the sound of footsteps and infrequent apparitions have been reported to occur over a period of approximately ten years and three sets of owners. The phenomena were experienced by 26 individuals with varying degrees of knowledge about prior incidents; some observations were collective.

Attempts to explore alternate explanations revealed no normal physical cause. Moreover, no one living poltergeist agent, or group of agents, was discovered in the analysis. Experients' reports seemed to suggest the presence of a deceased agent. This hypothesis was supported by impressions received by one psychic who visited the house and whose descriptions of the appearance, character and purpose of a deceased former owner were either verified or seemed to fit her personality and circumstances. Information regarding the case also seemed to fit criteria proposed by Stevenson (JASPR, 1972, pp. 233-252), and Gauld and Cornell (Poltergeists, London: Routledge and Kegan Paul, 1979) for the presence of a deceased agent.

## A POLTERGEIST IN DURHAM

Arthur S. Berger, Joyce Berger and William G. Roll† (Psychical Research Foundation, Durham)

For seven months in 1977 and again in 1980, a series of dis-

*Chairperson: Erlendur Haraldsson, University of Iceland

turbances occurred in a single-family dwelling in Durham occupied by a husband, wife, infant child and the husband's brother. Our investigation showed that no similar disturbances had taken place when the house was occupied by former owners.

The disturbances consisted of objects being moved and noises heard. They had their inception in April and May, 1977, when certain volumes of an encyclopedia set were pulled out and pillows were removed from a couch and rearranged on the living room floor. In later months, fragile glass globes were removed from a chandelier and placed on a dining room table without breakage and books were arranged on the floor symmetrically at angles to a room divider. A humming sound was heard in the living room. In October, the night the child was conceived, they ceased. They resumed in February, 1980 when, while the infant child was in the living room, chairs were toppled and pillows from the sofa were wedged in the chandelier.

The wife, who was not present when some disturbances occurred and who seemed anxious to know their cause, was dismissed as their cause. So was the brother, because he did not move into the house until September 1979. Since every disturbance occurred only when the husband was in the house (or in a room in the house he used as an office), the indication is that he could have produced the disturbances. It is possible that he was under stress because of a decision made in May, 1977 to have a child which, prior to that time, the couple had not wanted. But the disturbances do not seem related to emotional conflict or stress. Some, in April and May, had taken place before the decision to have a baby. The controlled arrangement of many objects and the lack of breakage of many more suggest that the disturbances could not have been caused by the unconscious mind of a poltergeist agent, and it is believed that the disturbances in 1977 can be attributed to the husband's deliberate acts for undisclosed reasons. The incident of February 1980 could have been caused by the infant child. In brief, the case seems entirely susceptible to normal explanations.

## REPORT ABOUT A RECENT ALLEGED RSPK CASE IN FRANKFURT

Walter V. Lucadou (University of Freiburg)

The Freiburg Institute was involved in an alleged RSPK case which occurred at the flat of an Italian family in a suburb of Frankfurt in October 1979, and which lasted until February 1980. The family and other witnesses (policemen, relatives and acquaintances of the family) reported stone throws which could not be explained. The family consists of parents (39, 32) and three girls C (14), M (11), G (7) and a boy R (12). The father (39) is a former fisherman and an illiterate. They come from southern Italy (Calabria)

and strongly believe in ghosts. They were completely terrified by the phenomena which occurred both by day and night. We spent a total of four days with the family and witnessed about eight stone throws. We found the following:

1) It turned out that the 14-year-old girl C was totally suppressed by her family and was treated like a "Cinderella." It seems likely that she was the focal person. A welfare officer reported several times that the girl C was mistreated by her mother and father, so it was decided by a civil court to bring her into a psychiatric clinic, even against the wishes of her family. The clinical examination showed that the girl was in good physical health, but she showed somewhat unusual behavior. She gave the impression of being infantile and inhibited but at the same time she acted older than her age, not childlike. Sometimes she reacted in an aggressive way. Her intellectual development seemed to be retarded; nevertheless it was reported that she was able to steal some toys from other children in a rather skillful manner. The Rorschach test supported the impression that the girl is not able to manage her aggressive and self-destructive feelings in a rational manner, and this led to a severe neurosis. The EEG examination showed a dominating alpha-rhythm but no hint of abnormalities. With the Snijders-Oomen test she scored an IQ of 95.

2) It was extremely difficult to get reliable and prompt information from the police, whereas sensational newspapers were obviously able to get such information. In the reports which became available half a year later, two policemen suggested that the girl C had thrown a stone. Another one reported that nothing had occurred, and two others reported independently that they had witnessed stone throwing although no person could have thrown them. There are obvious differences between these reports and the statements of other witnesses. During a civil court case to decide whether the girl could go back to her family, the Italian witnesses refused to say anything about their observations. One of the social workers had the impression that some of the witnesses may have been anxious and/or influenced by another person. One Italian witness in particular, who seemed to be a contact person of the police and had given quite clear statements about his observations at the beginning of this case, withdrew his statements completely.

3) It was very difficult to convince the family that it was possible to help them, and that they should get rid of their prejudices. The family, especially the father, was afraid of medical treatment. So they did not want the girl C to be examined by a physician. Later, we found that the concept of health is very important from the point of view of their socio-cultural structure in south Italy. The family also hoped that the phenomena would cease if they obtained a new home. They believed that ghosts of deceased persons threw the stones. The father said that he had seen several apparitions, but other members of the family never saw them. Especially at the beginning of the phenomena the situation for the family was very dangerous because the father had a hatchet with him day

and night to "defend" the family. Since he did not sleep for several days there was the danger that he might accidentally hurt a member of the family.

4) Two welfare officers from two public health organizations and the director of a psychiatric clinic were also involved and tried to help the family. It was extremely difficult for them to convey clearly the nature of the problem and to get administrative help. It was not possible to get a new home for the family. The administration argued that the stone throws were merely a trick the family was using to obtain better housing conditions. This suspicion was even raised when the family had the opportunity to go on holidays for some weeks in order to stop the phenomena. During this time no stone throws occurred. Still, it was very difficult to convince the family to take holidays because they feared losing their jobs.

5) Some witnesses reported the typical features of RSPK "stone rains"; i. e., that the stones (gravel and bricks) felt warm or cold, appeared in closed rooms from outdoors, and that the stone throws could not be observed directly but only inferred afterwards. The trajectories of the stones showed the typical paranormal behavior of RSPK-cases. The first stone throws which we witnessed occurred on the evening of 7 October 1979. Three stones were thrown into the kitchen. Because we did not have complete control over the situation (several persons were standing around), we cannot rule out the possibility that someone had thrown the stones by normal means. One of these stones clearly felt warm. It was not possible either to see the stones falling in all the other cases which I investigated, only to hear them. In all cases, they could have been thrown by one of the children, although I never detected any manipulation. In some cases it would have been necessary for the child (for instance the girl C) to make very elaborate and skillful tricks to achieve a stone throw. In all cases the stone throws occurred in the neighborhood of the girl C. The noise of some falling stones could be obtained in a continuous recording with a stereo tape recorder.

## PERSONALITY VARIABLES*

### PERSONALITY AND "BELIEF" IN RELATION TO LANGUAGE ESP SCORES

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

Method

A list of the names of the 122 students who participated in language ESP tests at Jordan High School was given to JoMarie Haight (J. M. H. ), who earlier had administered HSPQ and attitude questionnaires to these students and had the data stored in the computer. J. M. H. obtained the relevant data from the computer for 97 of these subjects; the data for the rest were not available.

Personality data were not available for two of these 97 students who did have sheep-goat data. Thus, while in our subject pool we had a total of 97 subjects, there were only 95 for personality-ESP study and an equal number for analyzing sheep-goat and ESP data.

It was felt useful and appropriate to employ the split analysis technique, a technique that involves a split of the data into two groups which may be considered pilot and confirmatory groups for the study of personality-ESP relationships. The odd-numbered subjects (49) constituted the pilot group and the even-numbered ones (46), the confirmatory group; the two subjects for whom there were no personality data fell into the even-numbered category.

The ESP test was one of clairvoyance, in which subjects responded to 50 targets enclosed and sealed in an opaque envelope. The language ESP studies were designed to see whether the differential scoring between Telugu and English target words occurred only when the subjects responded to both language words. Therefore, three different experimental conditions were introduced. In one, the Dual Target Condition (DTC), subjects responded to target words written in Telugu and English. In the other two, Single Target Condi-

*Chairperson: Carl Sargent, University of Cambridge

tions (STC), the subjects responded to Telugu or English targets, i. e., all the 50 targets were either in Telugu or English. The target words in all the conditions were: BALL, FISH, LOVE, PEACE, and TREE, and/or their Telugu equivalents. After a brief talk on parapsychology, the subjects were given the envelopes and asked to make their guesses on a response sheet attached to the envelope with appropriate instructions on them.

## Results

The personality and ESP scores of the subjects were correlated by means of Pearson's product-moment correlation. Factors A, C, and O in the pilot were found to be significantly correlated with overall ESP scores. Factor A represents a warm-hearted, out-going and easy-going individual and this was found to be positively correlated with ESP. The r obtained here is .33, $p < .05$, two-tailed. Factor C, which is a measure of emotional stability, also correlated positively with ESP, the r here being .37, $p < .01$, two-tailed. Factor 0, which indicates the guilt prone and worrying individual, was found to be negatively correlated with ESP, $r = -.41$, $p < .01$, two-tailed. None of these factors was confirmed in the confirmatory series. Instead, factors E and $Q_2$ reached the .05 level of significance. Factor E, which represents individuals who are independent, aggressive and stubborn, was negatively correlated with ESP, the r obtained being -.29, $p = .05$, two-tailed. Factor $Q_2$, which measures self-sufficiency, was found to be positively correlated, $r = .30$, $p < .05$, two-tailed. The results in the confirmatory series thus failed to confirm the findings in the pilot.

Among the secondary factors, factors II, III, and X in the pilot were significantly related to ESP scores. Factor II, which is a measure of anxiety level, was found to be negatively correlated with ESP, $r = -.35$, $p < .02$, two-tailed. Factor III (cortertia), which measures cognitive activity, was positively correlated with ESP performance, $r = .37$, $p < .01$, two-tailed. This indicates that individuals who are alert and active are better psi scorers than those who are frustrated and depressed. Factor X represents leadership potential in individuals and this factor was found to be positively related to ESP, $r = .36$, $p < .02$, two-tailed. In the confirmatory series none of these findings was confirmed; instead a very strong and significantly negative relationship was found between extraversion and ESP. The r obtained here was -.45, $p < .002$, two-tailed.

The results of the pilot group showed that those personality factors found to be significantly correlated with ESP also correlated significantly and in the same direction with the ESP results in the DTC. In the confirmatory series, the same trend was seen but the significances were scattered over the two conditions.

There were 47 sheep and 48 goats. The sheep obtained a total of 485 hits with a deviation of +15, and the goats a total of

456 with a deviation of -24. Neither of these deviations is significant nor are the differences between them. When the ESP data are analyzed in terms of Dual and Single Target Conditions, the sheep obtain a deviation of +30 ($t = 2.309$, 21 df, $p < .05$, two-tailed) and -15 in the DTC and STC, respectively. The goats obtained -7 in the DTC and -17 in the STC. No significant scoring differences were found in the languages for both groups.

## BELIEF AND SEX DIFFERENCE IN A LANGUAGE DIFFERENTIAL TEST*

R. Jeffrey Munson (Institute for Parapsychology, FRNM)

In the present test, target sheets were printed with a 5 x 10 grid, making 50 target calls. A closed-deck random sequence of 50 digits between 0 and 9 was generated by a PDP 11/20 computer at the FRNM for each target sheet. Each number corresponded to one of five words (ball, tree, fish, love, peace) in either English or Telugu. Each target sheet was stamped with a word in every target cell, sandwiched between sheets of construction paper, and sealed in a manila envelope by an assistant. Outside each envelope was an instruction/response sheet with a matching 5 x 10 grid, stapled correspondingly over the target sheet inside. Students in four classes (49 college students, and 17, 79, and 33 high school students, in order of classes tested) were given this envelope clairvoyance test after a half-hour talk on parapsychology by the experimenter (R. J. M.). Prior to the testing, R. J. M. asked the subjects to answer this question (with a "yes" or "no" written somewhere on the response sheet): "Do you think that you can use your clairvoyance to cross the language barrier in this test?" Subjects were divided into sheep and goats on the basis of their responses to this question, with ambiguous answers being considered as negative responses. This type of sheep/goat question was used in order to orient subjects to both the testing procedure and their own confidence.

Scores were analyzed with a three-way analysis of variance with one repeated measure, using sheep/goat and male/female as the independent factor levels, and English/Telugu as the dependent factor levels. In addition, t-tests were done where indicated by the ANOVA and/or relevant cell means. The college class was the pilot test, and the three high school classes were evaluated separately in the subsequent formal series. Lastly, the three groups comprising the formal series were combined in an overall analysis.

In the pilot series, there were no significant interactions in

*Delivered by K. Ramakrishna Rao.

the main analyses. However, it should be noted that goats scored significantly negatively when compared against chance ($t = -2.569$, 21 df, $p < .02$, two-tailed), and the significance was by and large contributed by missing on the Telugu targets by disbelievers ($t = -3.464$, 21 df, $p < .003$, two-tailed). The lack of interaction effects is the result of general missing on the part of nearly all subjects. Only female sheep scored positively on English targets but the difference, when compared against the respective Telugu scores, is still nonsignificant.

In the first test of the formal series, we find a suggestive interaction between the sheep-goat and the male-female variables ($F = 3.73$, 1 df, $p = .07$). Here we see that the male sheep have the lowest mean score of the four contributing groups and the female sheep have the highest. However, none of the groups is independently significant, nor are the differences between them. Similarly, when sheep and goats are broken down into English and Telugu categories, the sheep score lowest on Telugu. The interaction between sheep-goat and English-Telugu, as shown in the ANOVA, is slightly suggestive ($F = 2.14$, 1 df, $p = .16$). The scores of this formal group must be approached with care because of the small sample size ($N = 17$).

In the second test of the formal series, no significant results were revealed through the analysis of variance. However, the interaction between sheep, goats, males and females is suggestive ($F = 3.53$, 1 df, $p = .06$). Although the degree of significance is approximately the same as in the first group, the relevant means appear nearly contrary. In this case, the sheep as a group deviated significantly positive from chance ($t = 2.236$, 26 df, $p < .05$, two-tailed). Most of this positive scoring is given by hits on Telugu targets ($t = 2.226$, 26 df, $p < .05$, two-tailed). When sheep and goats are separated into males and females, the male sheep scored positively against mean chance expectation ($t = 2.274$, 15 df, $p < .02$, two-tailed), and significantly differently from male goats ($t = 2.275$, 37 df, $p < .01$, two-tailed), who were somewhat below chance.

In the final test of the formal series, there are no significant interaction effects in the main analyses. However, there is again a somewhat suggestive effect in the interaction between the sheep/goat and English/Telugu factors ($F = 2.11$, 1 df, $p = .15$). In this case, the sheep score positively on English targets and negatively on Telugu targets, but the difference is nonsignificant.

When the data of the three formal tests are combined, the results are nonsignificant, and no individual groups show any great deviations. This lack of results occurs because data groups with varying results combine to wash out the separate pockets of significance.

# RELIGION AND BELIEF IN PSYCHICAL PHENOMENA: SOME SIGNIFICANT CORRELATES

Erlendur Haraldsson (University of Iceland)

In a collaborative paper, Michael Thalbourne and the present author described four studies in which a search had been made for relationships between belief in psi phenomena (the "sheep-goat variable") and various personality measures. In the two studies conducted by E.H., the Cattell Sixteen Personality Factor Questionnaire was used, and it was found that Factors A and $Q_1$ were related to the sheep-goat variable to a small but significant degree: sheep (the "believers") tended to be more outgoing and conservative than goats (the "disbelievers"), who were more reserved and radical-thinking.

But though some significant relationships with personality have emerged, the degree of association is typically quite small. The four studies described in this paper are not concerned with personality traits as such. For the most part they explore how belief in the psychic is related to attitudes, experiences and activities in the domain of religion and politics.

Subjects were in all cases administered a three-item "Sheep-Goat Scale." The items comprising the scale were of the multiple-choice type and concerned the subject's belief in the existence of GESP and of precognition, and the reading of articles and books on psychic phenomena.

Study I. In a national survey of psychical experiences, conducted in Iceland during 1974/75, data were also collected on such items as religiosity, religious experiences and dream-recall. The sample consisted of persons aged between 30 and 70, selected at random from the national registry, and was thus highly representative of the Icelandic population as a whole. Approximately 900 persons, or 80 per cent of the original sample-size, returned usable questionnaires.

The survey contained two questions relevant to religion. The first was: "How religious do you consider yourself to be?" and showed a significant correlation of +.30. The second question, "Have you ever had a vivid religious or spiritual experience?" was also positively related to belief in psi phenomena, the correlation being +.26. Two questions in the survey concerned the reading of articles or books on religion: Bible-reading displayed a very slight positive correlation ($r_s$ = +.12), while reading about Eastern religions showed a rather more significant relationship ($r_s$ = +.35). This may indicate that belief in psychic phenomena is related more to liberal or general religious interests than to orthodox or sectarian Christian beliefs.

The Sheep-Goat Scale also showed a slight positive correlation with reported frequency of dream-recall ($r_s$ = +.15), and a

rather stronger correlation with attempts to interpret one's dreams ($r_s$ = +.30) and with number of types of reported psychic experiences ($r_s$ = +.21). The largest correlation was found with belief in survival ($r_s$ = +.42). It may be added that neither age nor education correlated significantly with the Sheep-Goat Scale, but that sex did ($r_s$ = +.22), women showing slightly more belief in the reality of the psychic than did men.

Study II. The second sample consisted of 75 male students at the University of Iceland, each of whom had participated in one or other of two studies conducted to examine the relationship between ESP and the Defense Mechanism Test (DMT). A questionnaire was administered containing items pertaining to dream-recall, politics and religion.

Again it was found that the highest correlation with the Sheep-Goat Scale was with belief in survival ($r_s$ = +.52). Furthermore, all four religious items had a significantly positive correlation: self-reported religiosity ranked highest ($r_s$ = +.49), followed closely by praying ($r_s$ = +.42); considerably behind were attending religious meetings ($r_s$ = +.27) and reading books or articles on religion ($r_s$ = +.26). A "Religious Scale," consisting of only those three religious items which were used across Studies II, III and IV (namely, self-reported religiosity, praying and reading religious literature), here yielded a highly significant correlation of r = +.46.

Unlike Study I, the two dream-recall items ("Did you dream last night?" and "How frequently do you generally dream?") did not correlate significantly with the Sheep-Goat Scale. However, there was again a significant relationship with attempts to interpret one's dreams ($r_s$ = +.33). No significant correlations were found with any of the items concerning politics, or with the DMT.

Study III. The third sample comprised 180 students at the University of Iceland (56 of them females), most in their early twenties. The sample was formed by taking whole classes of students selected from various faculties, and is probably fairly representative of Icelandic university students. Subjects were administered a short questionnaire dealing with political interests and participation, religion and dream-recall.

Once again, significant relationships were found with all three of the religious items: self-reported religiosity ($r_s$ = +.35), reading of books and articles about religion ($r_s$ = +.39), and praying ($r_s$ = +.30). The three-item Religious Scale showed a highly significant correlation of +.43.

None of the political items--individually, or combined as a scale--correlated significantly with the Sheep-Goat Scale. Sex again correlated positively, but non-significantly (r = +.12). However, the significant positive relationship with dream-recall was replicated: "Did you dream last night?" correlated +.22, and "How frequently do you generally dream?" +.23.

Study IV. This sample consisted of 197 Icelandic university students, whose characteristics were very similar to those of the third sample. A rather long questionnaire was administered, containing items dealing with attitudes, activities and experiences relating to politics and religion, as well as an Icelandic version of Rotter's Internal-External Locus of Control Scale.

Belief in survival was again the highest sheep-goat correlation ($r_s$ = +.66). As in the three previous samples, all of eight religion items correlated positively and significantly with the Sheep-Goat Scale: coefficients ranged from .36 for "thinking about religion" to .61 for belief in God; self-reported religiosity correlated .58, praying .55, reading about religion .47, attending religious meetings .46, having felt the presence of God .37, and having experienced divine guidance or consolation .54. The three-item Religious Scale yielded a highly significant correlation of +.61, and a larger scale (comprising all eight religious items) gave a correlation of .64.

Again, no significant correlations were found with political interests and attitudes. Nor was there a significant correlation with Rotter's Internal-External Locus of Control Scale. In this sample, sex correlated significantly (r = +.30), women averaging higher on the Sheep-Goat Scale than did men.

Discussion. In four studies using large numbers of subjects, certain significant correlates were found of belief and interest in psi phenomena as measured by the Sheep-Goat Scale. Particularly striking is the strong relationship with all the religious parameters examined: correlations with self-reported religiosity ranged from .30 to .58, with praying from .36 to .55, and with reading of articles and books on religion .26 to .47. A Religious Scale composed of these three items correlated from .43 to .61 with the Sheep-Goat Scale, indicating that from 18 to 37 per cent of the variance of the two scales had a common source. It should be emphasized, however, that the religious variables used were very limited in scope: there were, for example, no items exploring religious orthodoxy or religious affiliation. The indications are that what is being tapped by the particular items used here is a liberal or unorthodox kind of religious approach rather than a more sectarian or dogmatic one.

In all three samples where there were both males and females, women tended to score higher on the Sheep-Goat Scale than did men, and significantly so on two occasions. The correlations are rather small, however, ranging from .12 to .30, but do replicate similar findings from several other countries.

Dream-recall was found, in two out of three samples, to be slightly but significantly related to belief in psychic phenomena. A somewhat stronger relationship was found with attempts to interpret one's dreams (r = .30, .33). Dream-recall and dream-interpretation may perhaps be looked upon as measures of attention to purely subjective experiences and as efforts to search for meaning in them.

In this connection, it may be of interest that some sociologists of religion interpret religion as being a personal effort to structure reality into a meaningful universe. The labeling of an experience as "psychic" is frequently, if not in most cases, dependent upon an individual's assessment of events as being either pure coincidence or as meaningfully connected events. On this basis, one may wonder whether a varying tendency in individuals to see or judge events or life in general as meaningful, may not be the common source of the small but significant relationship we have found between religion (in a broad sense) and the belief in psychic phenomena. But these findings may be equally open to other interpretations.

## EXTRAVERSION AND THE SHEEP-GOAT VARIABLE: A CONCEPTUAL REPLICATION

Michael A. Thalbourne (University of Edinburgh)

At last year's convention, Erlendur Haraldsson and I presented a joint paper in which we described a number of experiments which we had each conducted in an attempt to investigate the possible relationship between personality and the sheep-goat variable. The major conclusion at which we arrived was that there was a significant though weak association between the sheep-goat variable (as measured by two inventory-type scales), and the dimension of extraversion, the tendency being for goats to be more introverted, on average, than sheep. The instruments used to measure extraversion had been derived from the Minnesota Multiphasic Personality Inventory (MMPI), and from Cattell's Sixteen Personality Factor (16PF) Questionnaire. The present author set out to replicate and thereby strengthen the extraversion/sheep-goat finding, by using a different but conceptually related personality test, namely the extraversion scale from Eysenck's Personality Inventory (EPI). Since the EPI also has scales for measuring neuroticism and social-desirability responding, it was additionally planned to examine these traits in relation to the sheep-goat variable, and also their interaction with sex. Previous research suggested that sheep might be more neurotic than goats, and that this would be particularly so for females.

A total of 161 subjects participated in this study. The sample consisted of the entire class of 1979-80 First-Year Psychology at the University of Edinburgh, Scotland, together with a handful of non-psychology students. Age ranged from 17-41, with a median of 18 years. There were 55 males and 106 females, and the vast majority (96 per cent) were unmarried.

Two different scalar instruments were used to classify subjects as sheep or goats. One, referred to as the "Australian Sheep-Goat Scale," contains ten forced-choice questions pertaining to the respondent's belief in (three items), and experience of (seven items)

various psychic phenomena, including belief in post-mortem survival. The second scale, hereafter referred to as the "Icelandic Sheep-Goat Scale," consists of three questions regarding the subject's reading of literature concerning psi phenomena, and their belief in telepathy (or clairvoyance) and precognition. This is the first experiment in which the two scales have both been administered to the same sample; the Pearson correlation between the two was +.55 ($N = 161$, $p < .001$), indicating 30 per cent variance in common. There were no sex differences on the Australian Scale, but on the Icelandic Scale, females scored significantly higher than did males.

Eysenck's Personality Inventory contains 57 yes/no questions, 24 going to make up the E-scale (high scores indicating extraversion), and 24 contributing to the N-scale (high scores suggesting neuroticism). There are also nine questions comprising a Lie-scale, L, which is intended to detect any tendency to answer questions according to the favorable light which the response would throw on the subject.

The mode of statistical attack adopted was to trichotomize the distribution of Sheep-Goat Scale scores into "high," "medium" and "low" groups; high scorers were designated as sheep, low scorers were categorized as goats, and the medium group was excluded from analysis. The mean personality-scores of these two "extreme" groups were then compared using t-tests for independent samples.

For the Australian Sheep-Goat Scale (theoretical range 0-20), the 53 subjects scoring six or below were designated goats, and the 57 subjects scoring 11 or above were classified as sheep. As predicted, sheep scored significantly higher on extraversion than did goats (mean for sheep being 13.21, for goats 11.57, $t = 2.12$, 108 df, one-tailed $p = .018$, omega-squared = .03). Sheep also scored higher on neuroticism and lower on the Lie-scale, but the differences were not significant. To examine the interaction between sex and sheep-goat personality differences, males and females were considered separately, and the t-tests performed again. Even when this division-by-sex was made, the sheep-goat differences were in the same direction as those in the undifferentiated sample. However, the significance of the relationship between extraversion and the sheep-goat variable disappeared for the females while intensifying for the males: the 19 male sheep averaged 13.79, the 14 male goats 10.00, $t = 2.83$, 31 df, two-tailed $p = .008$, omega-squared = .18. It may also be mentioned that in line with expectation, the 38 female sheep were more neurotic than the 39 female goats to a marginally significant degree: the former averaged 13.55 on the N-scale, the latter 11.85, $t = 1.62$, 75 df, one-tailed $p = .055$.

For the Icelandic Sheep-Goat Scale (theoretical range 3-12), the 48 subjects scoring seven or below were classified as goats, the 34 subjects scoring ten or above being placed in the category of sheep. For the extraversion scale, sheep averaged significantly higher than the goats, once again confirming the prediction: mean for sheep was 13.06, for goats 11.54, $t = 1.75$, 80 df, one-tailed

p = .042, omega-squared = .02. Just as for the Australian Scale, sheep were non-significantly higher on neuroticism and lower on the L-scale. As regards the sheep-goat/personality breakdown by sex, the direction of the sheep-goat difference was the same as for the combined sample in five out of six cases. However, none of the differences is significant, nor do there appear to be any marked sex differences.

We may thus conclude, from the overlapping results obtained using the two Sheep-Goat Scales, that the positive (though weak) relationship between extraversion and the sheep-goat variable has been conceptually replicated. The present finding thus lends confidence to our previous assertion that sheep tend to be extraverts and goats introverts, even though this tendency is admittedly very slight. There was also some evidence from the Australian Scale that this relationship with extraversion was attributable to male rather than female subjects, as well as a faint indication that female sheep might be more emotionally unstable than goats. However, there was no evidence to suggest that the subject's sheep-goat responses were related to the desire to present themselves in a favorable light to the experimenter. Thus, "need for approval" seems not to be a major contaminating variable.

The author's unpublished data suggest that the Australian Sheep-Goat Scale can be predictive of scoring in a GESP free-response test, sheep scoring positively and goats negatively. Independently, there is some (very meager) evidence that the EPI may also have some power to predict ESP-score, extraverts scoring high and introverts low. One might expect, therefore, that greater predictive power could be achieved by combining the sheep-goat scale with the E-scale when selecting subjects for an ESP experiment. This is in fact exactly what is being undertaken at present in the Parapsychology Laboratory at Edinburgh University: it is intended that a group of extraverted sheep will be compared with a group of introverted goats, and it is predicted that, ceteris paribus, the former should score above chance and the latter below. Thus, it is hoped that capital can be made on the independently psi-predictive attributes of the two psychological measures.

PSI-CONDUCIVE STATES*

## TRANSCENDENTAL MEDITATION, CLAIRVOYANT ABILITY AND PSYCHOLOGICAL ADJUSTMENT

Susan E. Harding and Michael A. Thalbourne† (University of Edinburgh)

Three groups of subjects were used: (1) a control group of non-meditators, (2) a group of subjects practicing the technique known as "Transcendental Meditation" (TM), and (3) a group of subjects practicing the so-called TM-Sidhi program. This latter is a set of techniques collectively referred to as "the sidhis," recently developed by the TM organization as an advanced form of meditation. The sidhis are loosely described as "seeds" for cultivating higher states of consciousness, and involve what parapsychologists would refer to as paranormal abilities, the most controversial of which is the sidhi for levitation, or "flying." To the best of our awareness, such claims as have been made for the TM-sidhi program have not yet been tested in a parapsychological context. In the present study, it was proposed to explore the efficacy of one particular sidhi: that for obtaining "knowledge of hidden objects."

The experiment was originally designed as a three-group comparison, with 14 subjects in each group. A research proposal was duly submitted to Mentmore Towers, an academy in Buckinghamshire, England, where the TM-sidhi technique is taught, asking for permission to carry out some of the research there. This, however, was refused, as the organization has an unfavorable attitude towards parapsychology, and subjects were therefore recruited, with some difficulty, from around Edinburgh. The groups ultimately consisted of ten control subjects, nine ordinary TM meditators, and five "sidhas," as they are known. Out of a total of 24 subjects, 14 were male, ten female. The ordinary meditators had been practicing TM for between one month and four years (average being 20 months). The sidhas had been practicing meditation for between $2\frac{1}{2}$ and seven years (the average being four years and nine months), with an average of 14 months being spent practicing the sidhis.

*Chairperson: Irvin Child, Yale University

Two different clairvoyance tasks were administered in counterbalanced order both before and after a 20-minute period of meditation (or relaxation in the case of control subjects). One test consisted of two runs of a standard ESP card-guessing test using the Down Through technique; the other was a free-response test in which subjects had to guess the content of a concealed pictorial stimulus. The free-response test was evaluated by having the subject give ratings to a duplicate set of four pictures (one being the target, three being controls), and these ratings were converted to Z-scores. Just prior to the clairvoyance tests, all subjects completed Shostrom's Personal Orientation Inventory (POI), which is a measure of self-actualization frequently used in previous studies of the effects of TM and as a predictor of ESP. Subjects also completed a sheep-goat scale developed by the junior author, and at the end of the experiment, a mood questionnaire.

The three groups were found to be quite comparable on mood and sheep-goat variables. However, there were striking differences in personality: the 14 meditator subjects (i. e., sidhas and ordinary meditators combined) scored significantly higher than did controls on ten of the 14 POI subscales, indicating superior self-actualization in the subjects practicing TM; (nevertheless, there was no indication that sidhas were at all superior to ordinary meditators in this regard; if anything, they tended to be less self-actualized!)

In contrast, the results of all the ESP tests were uniformly non-significant. In the card tests, the overall mean score (i. e., the average of pre-and post-scores) for the 14 combined meditator subjects was found to be slightly higher (5. 39) than that of the controls (5. 15). For the picture-guessing tests, however, the direction of the difference was reversed: whereas MCE is 0. 0 the mean Z-score for combined meditators was . 107, for controls . 440. Contrary to hypothesis, sidhas obtained the lowest scores of the three groups: for the card test, their mean was 4. 80, while that for ordinary meditators was 5. 72; for the picture test, mean Z-score for sidhas was -. 227, compared with . 292 for meditators.

This experiment gave evidence which clearly supports the hypothesis that the practice of TM is associated with superior psychological adjustment, as measured by the POI. At the same time, no evidence was found for the assertion that meditation per se facilitates the occurrence of ESP in an experimental situation. It could perhaps be argued that it is not simply the experience of meditation that is psi-conducive but rather the quality of that experience; no attempt was made to measure the individual's subjective feelings, or degree of alteration in consciousness undergone while meditating; it could be that these are the variables which are related to psi-scoring--a suggestion which comes out of some of the Ganzfeld research.

No support was obtained for the claim that the sidha subjects possessed a special technique for gaining "knowledge of hidden objects." It may have been the case that the sample size was rather

too small to demonstrate any significant ESP effect, particularly since there were only five sidhas, who may not have been representative of the population from which they were drawn.

## PSI SCORING ON INDIVIDUAL AND GROUP TARGETS BEFORE AND AFTER MEDITATION

William G. Roll† and Robin Zill (Psychical Research Foundation) and William Hight and Sheri Prather (University of North Carolina, Chapel Hill)

At last year's Convention we reported a study designed to follow up on the group effect and at the same time to make use of the statistical advantages of the forced-choice design. The same design was used in the present study.

The procedure involved a balanced design with dual aspect targets where one aspect was a group target, which was the same for everyone in the session, while the other was individually randomized. One half of the responses was elicited before the meditation session and one half after.

The emphasis was on meditation rather than on ESP and no suggestions were given either explicitly or implicitly for high scores. The ESP aspect was toned down further by being integrated into the mood questionnaires.

The meditation procedure was changed every session to keep it as fresh as possible. Except for the first, where mental and bodily relaxation was emphasized, the sessions involved a wakeful, attentive mental state, sometimes with eyes open, sometimes with closed eyes. The meditation period lasted about 30 minutes and was silent except for the initial instructions and for suggestions towards the end of each session to enhance lowering of ego boundaries. Two sessions a week were conducted in March and April 1980, for a total of ten sessions.

The group consisted of seven persons, three women and four men, ages 22 to 55, and included the authors. Except for the first day, when one meditator was absent, all seven attended each session.

The participants completed two similar seven-item questionnaires, one before and one after the session (the 14-item post-session questionnaire which did not show any psi in our previous study was omitted from this one). Each question was answered by marking one of the nine response boxes spanning a continuum between extremes of moods. For instance, item four ranged from "I feel very free and at ease" to "I feel very anxious and upset."

The two questionnaires also served as ESP response sheets. For each item we divided the column of nine blocks into parallel columns (separated by the numbers 1-9). One ESP response was made by marking the left or right column and a second ESP response was made by entering a cross or checkmark. For example, 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 two 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 left-right 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. The meditators did not know if a particular trial was for a group or an individual target.

We hypothesized (1) that there would be more evidence of ESP after meditation than before for group targets as well as for individual targets, and (2) that there would be more evidence of ESP after meditation on group than on individual targets. In the previous study these hypotheses had been verified for the cross-check group targets by a significant positive deviation in the post session ($p < .05$) and by a significant difference between this and a slight negative score on the pre-session cross-check group targets ($p < .03$). We therefore also hypothesized (3) that the scoring in the present study would be concentrated on cross-check targets. Finally, we hypothesized (4) that positive moods would be associated with positive ESP deviations and negative moods with negative ESP.

## Results

For each subject there are four types of ESP targets before meditation and four after, namely individual cross-check and left-right targets and group cross-check and left-right targets.

To test Hypothesis 1, the ESP hits in the pre-sessions were compared with those in the post-sessions.

There were no differences for the group targets but the totals (cross-check and left-right) for the individual targets were significantly different ($t = 2.61$, 67 df, $p < .005$, one-tailed) with a negative deviation before meditation and a positive afterwards. These effects were due mainly to the differences for the cross-check condi-

tion ($t = 1.68$, 34 df, $p = .05$, one tailed). The scores on these targets were insignificantly negative before meditation, and significantly positive after ($Z = 2.04$, $p = .02$, one-tailed).

The totals reflected the scoring tendencies of the participants. Six of the seven subjects showed an increase from pre- to post-sessions on the cross-check targets. This became five to seven for the total of left-right plus cross-check scores.

Hypothesis 2 was not verified. There was no evidence for improvement in scoring from pre- to post-sessions with either cross-check or left-right group targets.

Hypothesis 3 was borne out by the study since the scoring (on individual targets) was concentrated on the cross-check targets.

There were generally mood increases from pre- to post-sessions, but no significant correlations with ESP.

## Conclusion

As in our previous study the ESP scoring focussed on the cross-check targets, but this time it shifted to the individually randomized targets, again showing an increase from negative to positive scoring between the pre- and post-sessions. Though this conformed to our hypotheses, it should be pointed out that the changes in sign of the ESP deviations were not associated with significant changes in the amount or degree of this deviation from chance expectancy.

The results, it seems, can be attributed to the preferential effect. It was probably known to all participants that the senior experimenter believes meditation may enhance ESP and they may have conformed to his expectancy by scoring negatively before meditation and positively after.

# SELF-ACTUALIZATION AND CLAIRVOYANT ABILITY

Mark G. Shafer† and Kathryn A. Angle (University of California, Irvine)

Maslow used the term self-actualization to refer to the emergence of full psychological health in a human being, and distinguished two degrees of self-actualization: transcendent and non-transcendent. Transcending self-actualizers were those motivated by a desire for spiritual or transpersonal fulfillment, as well as by a desire to actualize their personal human potentials. The hypothesis of the present research was that a positive correlation would exist between a bivariate measure of transcendent self-actualization and scores on an experimental clairvoyance task.

Fifty volunteer subjects participated in the study. The male-female ratio was approximately 2 to 3; mean age was 27. The authors served as experimenters, each running approximately half the subjects, none of whom was personally familiar to the experimenters.

Targets for the clairvoyance task consisted of photographic copies of pictures from National Geographic. Twenty target pools of four pictures each were chosen and a color slide was made of each target pool displaying the four pictures in a 2 x 2 arrangement. For the experiment, the picture designated as target was sealed inside a heavy book mailing envelope by an outside agent. A pseudo-random process based on digits from a random number table was used to determine targets, yielding counterbalanced use over the entire study of target pools, pictures within target pools, and position of target in displayed target pool.

Transcendent self-actualization was measured by equally weighting and summing scores from two questionnaires: the Northridge Developmental Scale (NDS), designed by Gowan to measure the self-actualized development of an individual; and Hood's Mysticism Scale (M-Scale), which measures the degree to which a respondent's past experiences have been mystical in nature. Subjects attended a preliminary session to fill out the two questionnaires.

The design was double blind; the experimenters were familiar with neither subjects' questionnaire responses nor the identity of targets and target pools that would be used in experimental sessions. Subjects were run individually in two experimental sessions each. These began after the subject was seated in a comfortable chair and had listened to and followed the instructions of a 20-minute edited version of a relaxation tape developed by William Braud. The tape consists of a progressive muscular relaxation exercise, autogenic suggestions for relaxation, and a brief meditation on the sensations of the breath. Following this, four clairvoyance trials were done. Each subject completed a total of eight trials over the two sessions.

Experimental results for the 48 subjects who generated usable data did not show evidence of ESP. The total number of trials on which subjects rated the correct target picture the highest--direct hits--was three less than expected by chance; the total number of trials on which the correct target was rated either highest or second highest--weak hits--was six less than expected by chance. Similarly, when only the trials from the first halves of each session were examined performance was still at chance: overall deviations were -4 for direct hits and -10 for weak hits. No correlation existed between the bivariate, transcendent self-actualization measure and psi-hitting (Direct hits: $t = .0059$, 46 df, $p$ = ns; Weak hits: $t = -.0476$, 46 df, $p$ = ns). Neither were the NDS self-actualization scores or M-Scale scores individually correlated with psi-hitting (NDS: $t = .0873$, 46 df, $p$ = ns; M-Scale: $t = -.1316$, 46 df, $p$ = ns, Direct hits). Thus, the hypothesis of the research that transcendent self-actualization scores would be positively correlated with psi-hitting was not supported.

The two factors of the transcendent self-actualization measure, the NDS self-actualization score and the M-Scale score were significantly and positively correlated with each other ($r_s$ = .5379, 46 df, $p < .001$, two-tailed). Furthermore, each of these factors was positively correlated with age (NDS: $r_s$ = .4276, 46 df, p = .002, two-tailed; M-Scale: $r_s$ = .3945, 46 df, p = .006, two-tailed). Thus those who were rated more self-actualized by the NDS were found to report having more mystical type experiences; and, both the NDS self-actualization scores and M-Scale scores were found to increase with age. These findings are consistent with what would be expected from the literature on self-actualization.

Post hoc analyses revealed a mild experimenter effect which seemed to mask a pertinent trend in the data. For M. S. 's subjects, the direct hit mean was 2. 23, where chance = 2. 00, as compared to 1. 72 for K. A. 's subjects. This difference approached significance (t = 1. 268, 46 df, p = .20 ns, two-tailed, corrected for inhomogeneity of variances). A similar but less marked difference existed between means for weak hits (t = .730, 46 df, p = ns, corrected for inhomogeneity of variances). A difference also existed in the variance of psi scores for M. S. 's subjects as compared to K. A. 's subjects, for direct hits; M. S. 's subjects showed a significantly greater variance (F = 2. 60, 21/24 df, $p < .05$, two-tailed). There also turned out to be a much wider age range and variance of ages for M. S. 's subjects (F = 4. 31, 21/24 df, $p < .001$, two-tailed). These results prompted analysis of M. S. 's subjects separately. From this data, a partial correlation of the NDS self-actualization scores with psi-hitting, controlling for the effects of age on the NDS score, approached significance (Direct hits: rij. k = .4112, 19 df, p = .064, two-tailed; Weak hits: rij. k = .3248, 19 df, p = .15 ns, two-tailed). For M. S. 's subjects, then, it appeared that when self-actualization not due to age and independent of mystical experience is considered, a relationship in the direction of the hypothesis of the research is suggested; namely, that self-actualization may somehow be positively correlated with psi ability. This result was independent of the transcendent dimension of self-actualization, however, which is contrary to the specific hypothesis considered by this research.

## ANXIETY AND ESP: ANATOMY OF A REVERSAL

John Palmer, † Chris Ader, and Martha Mikova (John F. Kennedy University)

Experimental clairvoyance research with forced-choice testing procedures has generally revealed a negative relationship between ESP scores and measures of trait anxiety or neuroticism when subjects are tested individually. I have tentatively interpreted this relationship as mediated by subjects' comfort in the test situation. In

1978, Haight, Kanthamani, and Kennedy published a significant reversal of this relationship. While such reversals can undermine confidence in the relationship, they also have the more positive potential of revealing its boundary conditions and thus contributing to its theoretical understanding.

What might such boundary conditions be? Haight et al. left their subjects alone to take the ESP test, and this may have increased the comfort of anxious subjects; conceivably, the relief even made them more at ease than the less anxious subjects. A second possibility is more difficult to define precisely but is based on Rao's differential effect. Haight's subjects each completed two separate psi tasks, one with immediate feedback of hits and the other without. Fifty per cent of the six studies of the anxiety-ESP relationship which reversed the predominant pattern used such within-subject designs, while only 22 per cent of the 18 confirmatory studies did so. Our facilities precluded us from manipulating presence of the experimenter without compromising essential safeguards, but we were in a position to explore the influence of the differential effect on the anxiety-ESP relationship.

## Experiment I

The subjects were 40 high school students from C. A. 's classes who volunteered to participate in an ESP experiment. In group sessions conducted approximately one week before the experiment, the subjects completed Cattell's High School Personality Questionnaire (HSPQ) and were assigned code numbers to preserve anonymity.

The subjects were ranked according to scores on the HSPQ anxiety scale (sten scores adjusted for sex) and members of each group of four were randomly assigned to one of four experimental groups. One group received immediate feedback of ESP hits on both four-run blocks (F/F); the second group received no feedback until the end of the blocks (N/N); the third group received immediate feedback on the first block only (F/N), and the fourth group on the last block only (N/F). Thus half the subjects received only one type of ESP test while the other half received two types.

The testing apparatus was an Aquarian four-choice ESP tester with an internal electronic REG. The bell which normally signals a hit was disconnected, and for non-feedback runs, pieces of cardboard were placed over the display panel and hits counter to eliminate visual feedback. Pasted on the cardboard were the numbers 1-4 to indicate the response alternatives corresponding to the buttons on the device.

Subjects were tested individually by C. A. following afternoon classes in a small room at the school. They had previously given color codes indicating their experimental conditions, and they revealed these, but not their number codes, to C. A. After a few practice trials on the Aquarian, they completed eight 25-trial runs,

with a rest period of approximately five minutes in between blocks. After each run, C. A. recorded the number of hits on a sheet before resetting the counters. (After the fifth subject, duplicate records were made). These records were shielded from the subjects in the N conditions.

Following the test and debriefing, C. A. left the room and asked the subjects to fill out an eight-item rating scale describing their reactions to the experiment and to C. A. as a teacher. Subjects were to record their number and color codes on this sheet and on one of the sheets containing the run scores (C. A. retained the other copy) and give them to a school secretary who in turn mailed them to J. P. for scoring. Five subjects forgot their code numbers but we were able to recover the information later.

A few days prior to the beginning of the experiment, the experimenters manually generated 1000 trials, pressing each button an equal number of times in deterministic sequences. Chi-square tests revealed no significant evidence of non-randomness at either the singlet or doublet levels.

The mean number of hits per run was 6.39, which did not differ significantly from the MCE of 6.25. The overall Pearson correlation between HSPQ anxiety scores and total ESP hits was +.17 (df = 38), which is consistent with the finding of Haight et al. If this reversal of the customary trend can be attributed to the dual-aspect nature of the ESP test, the correlation should have reversed in the N/N and F/F conditions. On the contrary, the correlation in these conditions was +.25 as compared to +.07 in the combined F/N and N/F conditions. There was a negative correlation of -.27 (df = 18) between the F and N scores in these latter conditions, which although non-significant, suggests the possibility of a differential effect being operative.

The ESP means in the four conditions were all close to MCE, as were the F and N means in the dual-aspect conditions. There were no significant mean differences.

Each item on the rating scale was scored on a 1-5 scale. As the items were moderately to highly intercorrelated, item scores were summed yielding a composite score with a range of 8-40, a high score indicating favorable or positive ratings. This scale correlated only +.11 (df = 38) with total ESP hits. However, the particular item which addressed most directly subjects' comfort in the test did predict ESP scores significantly. The 29 subjects who responded with a positive rating of four or five on this item had a mean ESP score of 6.61 as compared to a mean of 5.80 for the remaining subjects ($t(38) = 2.21$, $p < .05$).

## Experiment II

The above relationship is consistent with the hypothesis that

comfort in the test facilitates psi hitting, but it is not definitive because of its highly post hoc nature and because subjects knew their ESP scores when filling out the rating scale. A second experiment was conducted to explore this finding further. We also reasoned that the failure of the negative anxiety-ESP relationship to materialize might be due to the experimenter being the subjects' teacher. A strange experimenter might be more likely to engage anxiety in anxiety-prone subjects and produce the necessary variability for a relationship to emerge.

A new group of 40 volunteer high school students from C. A.'s classes served as subjects. They again completed the HSPQ before testing. Half were tested by C. A. and half by M. M., another school teacher unknown to the students. All subjects completed two four-run blocks on the Aquarian machine without immediate feedback, with a break of one to five minutes between blocks. They then completed a revised rating scale with the items about C. A. as a teacher eliminated. Half of the subjects tested by each experimenter filled out the rating scale prior to knowledge of their ESP scores, while the rest received their scores first. This resulted in a 2 x 2 factorial design with subjects again matched on HSPQ anxiety scores.

The overall mean ESP score was 6.23. There was a significant decline effect from the first (6.47) to the second (6.00) block ($t(39) = 2.42$, $p < .025$). There was no evidence of an experimenter effect. Randomicity tests prior to the experiment again revealed no evidence of non-randomness.

The anxiety-ESP correlation was a non-significant -.22 overall, but it did reach significance for the first block separately ($r(38) = -.30$, $p = .05$). It was slightly stronger overall for M. M. (-.28) than for C. A. (-.18). The composite score for the rating scale correlated significantly with ESP scores ($r(38) = +.38$, $p < .02$), and this correlation was independently significant for subjects who completed the rating scale before learning their ESP scores ($r(18) = +.50$, $p < .05$). However, the individual item that predicted ESP scoring in the first experiment was not a significant predictor in this study. While the general finding again supports the hypothesis of comfort in the test situation facilitating psi hitting, it is still conceivable that the experimenters' knowledge of the subjects' ESP scores after each run could have subtly influenced their behavior and hence subjects' ratings. The necessity of such feedback to experimenters will be eliminated in the next phase of the research.

Finally, it is noteworthy that in neither experiment was the HSPQ anxiety scale a strong predictor of rating scale responses. In the second experiment, the correlation was only -.29, which is non-significant and only negligibly stronger than the HSPQ-ESP correlation of -.22. In the first experiment, the HSPQ anxiety scale correlated positively (+.14) with those items on the rating scale concerning reaction to the test; i.e., the more anxious subjects reported less discomfort. These findings suggest that the failure of

personality tests to appropriately predict ESP scores in some studies might be due to their failure to predict the psychological orientations of subjects that are the more direct causes of high and low ESP scores.

## GANZFELD TECHNIQUES*

### GANZFELD PSI-OPTIMIZATION IN RELATION TO SESSION DURATION**

Carl L. Sargent, Trevor A. Harley, John Lane, and Keith Radcliffe
(University of Cambridge)

Forty subjects were randomly and equally assigned to the four conditions of a ganzfeld GESP free-response study: naive versus experienced subjects x 30 versus 15 minutes of ganzfeld isolation. Psychological data available for subjects were: Sixteen Personality Factor Questionnaire data, data from a four-item pre-session questionnaire (relaxation, mood, expectancy, motivation) administered immediately before psi testing, and a post-session questionnaire (17 items) administered immediately after ganzfeld isolation.

Subjects were tested individually and set up in an audio-visual ganzfeld. They were then randomly assigned to the 15- or 30-minute condition. In the 30-minute condition, a sender viewed a randomly selected target picture for 14 minutes during the session (12-26 within the 30); in the 15-minute condition he viewed it for seven minutes (6-13 within the 15). Since sending duration per se has not been found to influence receiver performance we equalized proportion of session allocated to sending rather than absolute time. During the session the receiver was located in a sound-attenuated studio and gave mentation reports to a monitoring experimenter. The sender, generally one of the four authors of this report, was located in a soundproofed room in another building.

After ganzfeld isolation the receiver was presented with four diverse pictures, one of which was a copy of the target. He ranked and rated (0-99 scale) the four pictures for their order and degree of correspondence with his mentation following an item-judging exercise during which each mentation item was separately scored against the pictures. Ranks and ratings were based on the outcome of this item analysis.

After completing the judging the sender was summoned and

*Chairperson: William Braud, Mind Science Foundation
**Delivered by John Beloff.

provided feedback. The following hypotheses were under test:

(1) Overall scoring should be significantly above MCE only for the 30-minute condition.

(2) Scoring should be significantly higher for later session utterances than earlier session utterances. This was tested by dichotomizing transcripts into two halves with an equal number of mentation items in each and computing signal and noise scores and an S/N difference score for each half as in previous research (Sargent, Exploring Psi in the Ganzfeld, a Parapsychological Monograph, expt. V). The difference between the S/N difference scores for the two halves was then examined. If there is no true difference the mean value for this parameter should be close to zero. The experimental hypothesis predicts that the S/N difference score will be greater for the later utterances than for the earlier ones.

(3) 16PF extraversion will correlate positively with rating-derived Z-scores (as computed by Palmer and associates). We expected such a trend to emerge only in the 30-minute condition and we did not predict this because we expected that the group sizes of only ten (ten experienced, ten naive) would be too small to show the effect significantly. Naive subjects had previously shown this correlation more reliably than experienced subjects.

(4) An auditory imagery factor from the factor analysis of questionnaire data will correlate with Z-scores, positively. Again this was expected only for the 30-minute condition data and was not specifically predicted. Such a correlation had emerged in the one previous ganzfeld study in which the same questionnaires were used as in the present study.

The results showed that overall scoring was at chance for both conditions and there was no significant difference between them. However an experimenter effect emerged. Three experimenters (C. L. S., J. L., K. R.) elicited eleven direct hits in 29 sessions whilst the fourth (T. A. H.) elicited none in eleven sessions. A rank-analysis indicated that T. A. H. elicited significant ($p = .014$, two-tailed) psi-missing as an experimenter which was significantly ($p = .014$, two-tailed) lower than the scoring he elicited as a sender. Which other experimenters/senders were working with him did not influence this effect. Moreover, the difference was also found to be significant in contemporaneous ganzfeld experiments of other kinds: T. A. H. elicited significantly ($p = .038$, two-tailed) lower scoring as an experimenter than as a sender. Altogether, in 16 sessions as an experimenter in the various projects he elicited no direct hits and only one second rank. Extensive analyses indicated no obvious basis for this effect in terms of personality differences in subjects tested, psychological differences in subject reactions to ganzfeld, judging bias or error, or PK effects on target selection.

The predicted within-session incline effect was not significant overall but it was highly significant ($p < .001$, one-tailed) for the 15-minute condition data. In the absence of C. L. S. the effect was significant overall ($p < .02$, one-tailed) and the C. L. S. depression effect was also significant ($p < .05$, two-tailed). In the 15-minute condition this experimenter effect was especially strong: in the absence of C. L. S. the effect was very significant ($p < .0007$, one-tailed) while in his presence it was null, the difference being again significant ($p < .01$, two-tailed).

Sixteen Personality Factor Questionnaire extraversion scores correlated with Z-scores in the 30-minute condition at $r_s = +.18$ for experienced subjects and +.67 for naive subjects. The latter correlation is significant at $p < .05$, two-tailed. This confirms our previous findings of a positive extraversion/ESP relationship in ganzfeld free-response testing which is more robust for naive than for experienced subjects.

Factor analysis of questionnaire data gave informative results. For the 30-minute condition data a pure auditory imagery factor emerged which correlated with Z-scores at $r_s = +.47$, $p < .05$, two-tailed, confirming previous results. For the 15-minute condition data, a six-factor solution was deemed more meaningful than the five-factor solution used for the 30-minute condition data and one factor correlated significantly ($r_s = +.54$, $p < .008$, two-tailed) with Z-scores. This factor had heavy loadings from pre-session variables of high expectancy and high motivation. It may be meaningful that pre-session variables are more important when ganzfeld isolation is of short duration than when it is of long duration. This requires further study.

The failure to observe any difference between the two time conditions on ESP scoring is hard to interpret because (i) the overall scoring was at MCE for both conditions, possibly because of the T. A. H. effect, and (ii) questionnaire data indicate no psychological differences for key variables (relaxation, imagery, rated success of ganzfeld in eliciting an altered state) across conditions. The predicted within-session inclines were present and significant only when C. L. S. was absent from any role in the experiment: the experimenter effect is very hard to interpret but the findings give some support to our previous findings and to Honorton's model of ganzfeld psi-optimization.

The personality and auditory imagery correlations are simple replications of previous findings and show the value of adequate psychological monitoring of what goes on, and who is involved, in ganzfeld experiments.

# PSI MISSING AGAIN IN GANZFELD SESSIONS?

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

In ganzfeld sessions conducted as a course requirement in 1978, we found evidence of psi-missing, as reported fully in JASPR, 1979, pp. 273-289 and 1980, pp. 171-182. Because we believed that a possible instance of psi-hitting would be more valuable educationally than a possible instance of psi-missing, when the course was next given in 1980 we tried to change the circumstances of the ganzfeld sessions so that psi-hitting might be more likely.

A survey of psi research done with the ganzfeld procedure, and of other instances of apparent psi-missing, had suggested that if psi-missing occurred in the 1978 sessions it might be attributed to the following factors: a negative attitude toward taking part in ganzfeld sessions due to their being a course requirement, and to their being conducted near the end of the semester when students were preoccupied with other activities; discomfort due to the instructor or teaching assistant remaining in the same room as the percipient in order to write down the imagery descriptions.

The 1980 sessions were therefore conducted with the following changes:

(1) Participation in ganzfeld sessions was made purely voluntary--an opportunity for those who were interested, not a requirement for anybody.

(2) The sessions were held in the early part of the semester.

(3) A one-way intercom system was arranged, so that the percipient could be in a room alone during the ganzfeld session, and yet his or her report of imagery could be written down verbatim as it was spoken.

(4) To reduce any feeling of regimentation that might remain, less emphasis was placed on getting two students to come at the same time. In four sessions, therefore, only one student participated--always as percipient--and one of the experimenters acted as agent.

(5) The experimenter in closest contact with the percipient was always A. L., rather than being equally often I. C.

(6) A film had been presented during the first class meeting, in which a strikingly successful ganzfeld session at Maimonides was shown. A possible by-product was that participants might come to the ganzfeld session with an increased expectation that psi-hitting would be a likely outcome.

(7) In the 1980 sessions we allowed students to participate more than once if they wished. Since there were 14 sessions in 1978, the comparison will be based on the first 14 sessions in 1980, but we will give some supplementary analyses that include three additional sessions that eventually took place.

With these exceptions, the 1980 ganzfeld sessions were conducted in the same general way we have already described for those of 1978.

Our attempt to obtain psi-hitting by introducing these changes was clearly a failure. The two sets of difference scores (1978 and 1980) are not significantly different from each other, and if we pool the two sets of data we still have highly significant evidence of negative results throughout the two sets.

If all 17 observations we were able to make in 1980 are considered, the conclusions are not substantially altered. Of the additional three sessions, one yielded a positive difference score (+38); the other two, negative difference scores (-3.75 and -43.25).

We have not, then, succeeded in effectively narrowing down the variables responsible for the apparent psi-missing. We have no strong evidence to suggest that the elements of compulsion and tension which we attempted to eliminate in 1980 had contributed to the psi-missing in 1978, though we cannot exclude that possibility.

Faced with this inconclusive outcome, we performed some post hoc analyses to see whether tentative suggestions might emerge. The 17 ganzfeld sessions we conducted in 1980 varied in several obvious ways. The only one suggestively related to outcome was whether agent or percipient or both had taken part in at least one previous session. Of the six "repeat" sessions, three yielded positive differences, and the average difference for all six was positive (+7.42) in contrast with the negative average for the other eleven sessions (-21.80). This difference has a t of 2.05 which even in testing an advance prediction would reach only the margin of significance ($p < .06$). There is just a slight suggestion, then, that the initial strangeness of the situation--related either to the social climate created by the particular experimenters or to the attempt to produce psi in the uncomfortable environment of a university psychology laboratory--may have been an important variable tending to produce psi-missing both years.

## A THREE SUBJECT STUDY OF PSI IN THE GANZFELD*

Deborah Delanoy, Adrian Parker, and Kathleen Wilson (University of Edinburgh)

There is now an impressive amount of evidence for the ganz-

*Delivered by John Beloff.

feld as a repeatable psi-conducive technique. Results to date appear to be independent of subject characteristics. However, certain authorities have stressed the importance of the experimenter's own prior personal experience with the technique. It was both with this objective and that of evaluating the efficacy of the technique, that the present study was undertaken.

The three subjects, D. D., A. P., and K. W., each underwent ten sessions of ganzfeld. As well as acting as a subject, each participant contributed ten sessions as an agent; five for each of the other two subjects. Each ganzfeld session lasted 30 minutes. Continuous mentation reports were given by the subjects during the session, and these mentations were recorded and simultaneously heard by the agent.

The target pools consisted of 30 sets of six pictures each, with each agent selecting ten sets according to his own preferences. Targets were selected using a random number generator on a p of 1/6 basis. The target pool consisted of 180 cards, with each individual set being used only once in the course of the experiment. It was anticipated that this aspect of the procedure would reduce possible displacement effects. Duplicate sets of each of the 30 target packs were provided for both subject and agent, to eliminate any possible cueing effects. The subject was in a cubicle, located at the far end of the parapsychology laboratory, and the agent was stationed in a classroom adjacent to the laboratory.

Each subject was fitted with the choice of either green or red translucent hemispheres taped over the eyes, followed by a set of earphones receiving white noise. The volume of the white noise and the position of the red light directed at the hemispheres were adjusted so that the subject felt comfortable. The subject, seated in a reclining chair, then received 30 minutes of ganzfeld stimulation, while the agent concentrated on the target picture.

The session's termination was signaled by the continuous ringing of a telephone until contact was broken by the subject. No communication was allowed between subject and agent until the subject had completed the ranking of the duplicate target set. Feedback was then limited to nonspecific comments until all 30 sessions had been completed. At the completion of the experiment, the subjects rated each of their target sets (from 1-10), according to their personal preference for the pictures.

## Results

It was hoped that, using the ganzfeld technique, subjects would produce overall psi-hitting. Using ranks one to three as hits, a total of 17 hits were recorded. The sum of ranks obtained was equal to 97 (MCE = 105, Z = .80 (corrected for continuity), p = .21, one-tailed). Hits were equally distributed between subjects.

Currently, the mentation tapes are being transcribed to allow for independent judging. A secondary analysis for sum of rank scores ($p \frac{1}{2}$) was also non-significant. An analysis for picture preference revealed that subjects gave a higher ranking to preferred pictures. The biasing or non-biasing of target packs in this manner did not, however, relate to positive scoring.

It is worth noting that all participants had a high expectancy relating to a positive outcome of the experiment, and the conditions were considered to be psi-conducive.

## MISCELLANEOUS TOPICS

### IMPLICATIONS OF INSULAR PACIFIC MAGICAL TRADITIONS FOR PARAPSYCHOLOGY

Gail C. Kawanami-Allen (University of California, Irvine)

Implications for parapsychological research in the ethnographic area, such as the Insular Pacific region, are intense and numerous. We can observe practical rituals in many traditional settings that do not dichotomize the natural and supernatural. Furthermore, such cultures seem to facilitate paranormal activities. Ritualistic procedures in these cultures, such as healing, sorcery, possession, and precognition, all point to a rich data-bank source that could offer practical dimensions for further examination of paranormal traditions.

The proposed approach gives us two main focuses of examination: (1) an evaluation of the concept of the paranormal in view of the ethnographic data in the traditional society, and (2) expansion of present parapsychological research paradigms to encompass insights gained from the documentation of the magical traditions and associated beliefs within these cultures.

### EFFECT OF SHORT-TERM STIMULUS UPON ESP FUNCTION

Soji Otani† and Hiroko Kurihara (The Japanese Society for Parapsychology)

In the P. A. Convention of 1978, S. O. reported that ESP scores showed fluctuation just after the application of a short-term sound stimulus to the subject during his ESP performance. We conducted a further analysis of the data, taking into consideration the quality of the stimulus and the change of the subjects' physiological conditions, which were measured during the experiment.

Eight male subjects were asked to attend four sessions of the experiment. In each session they completed two test sheets, each of which consisted of four ESP runs. A sound stimulus with a dura-

tion of two seconds was given to the subject twice each at a random place during the run. The subject was asked to make a call synchronized to a flickering light given every two seconds. The frequencies of the sound were 125, 250, 2000 and 4000 $H_Z$. These sounds were given to the subject at two different levels: loud and soft.

When the results were divided into two groups in accordance with the frequency of the inserted sound, a statistically significant positive deviation was found in the second trial, position (+2), after the application of the sound in the group of low frequencies (125 and 250 $H_Z$, CR = 2.937, $p < .003$). And when the data were divided into two groups on the basis of level of loudness of the inserted sound, a significant positive deviation was found at position (+2) in the case of soft sounds (CR = 2.937, $p < .003$). Galvanic skin response (GSR) was observed 137 out of 504 times of sound application. When we divide the data into two groups, as GSR and non-GSR, in the GSR group a significant positive ESP score was found at position (+2) (CR = 3.118, $p < .002$). The intervals of respiration and call intervals did not show any distinctive relationship to the ESP scores.

## EXPLORATION OF LONG-DISTANCE PK: A CONCEPTUAL REPLICATION OF THE INFLUENCE ON A BIOLOGICAL SYSTEM*

William H. Tedder and Melissa L. Monty (University of Tennessee)

Prior research on long-distance PK has produced varied results. The handful of studies completed have yielded a proportionate number of successes and failures in attempts to produce a psychokinetic effect on a target at distances of 100 feet to several hundred miles. The experiment reported here represents an effort by subjects to inhibit the growth of the fungus Rhizoctonia solani from distances of one mile to fifteen miles. The experiment was divided into: trials which represented each subject's attempt per week to influence the fungus, usually three trials per session, and three sessions for both series 1 and 2.

The second author, M. M., was solely responsible for the preparation and measurement of fungus cultures throughout the experiment. On each Monday, the first day of every session, sixty 90 mm. glass petri dishes with a 1.5 per cent water agar medium were inoculated with the fungus Rhizoctonia solani. After inoculation, two stacks of five cultures each were placed in each of six paper bags which were sealed and placed on the same shelf in an incubator

*Delivered by William Braud.

at 25° C. On the top of each paper bag a number was written that represented the five cultures located directly below it. Thus, one bag had the numbers 1 and 2, the next bag had the numbers 3 and 4, and so on. Variables such as humidity, light, and atmospheric pressure were not controlled, but both experimental and control cultures were exposed to identical conditions and were not disturbed for the duration of the session (approximately 72 hours).

The first author, W. T., was solely responsible for securing subjects for both groups. Group 1 consisted of W. T. and six others who knew and had frequently interacted with the first author for the last $1\frac{1}{2}$ years. Group 2 consisted of eight volunteer subjects who either did not know W. T. or who had infrequently interacted with him. In order to help subjects orient themselves they were all shown six pictures that pertained to the target area, such as: outside the Plant Sciences Building at the University of Tennessee, a view of the room and incubator that contained the targets, a view of the incubator door open five feet away from the six paper bags, etc. All targets were randomly generated from a PDP 11/04 computer by a third party not associated with the experiment and randomly distributed to the subjects.

Subjects were separated from the cultures by a range of 1-15 miles. They were allowed to concentrate from the location of their choice provided it was one mile or greater from the targets. They were also requested to concentrate for at least 15 minutes per day. Subjects were free to concentrate at any time during the day or night and could utilize any technique available to them to elicit results.

In analyzing the results, if the mean growth for the set of five experimental cultures was less than the mean growth of the five control cultures, the trial was considered a hit. Otherwise, the trial was recorded as a miss or as a tie if the two means were identical. In the first series, group 1 was successful in all seven trials that were completed ($p = .016$, two-tailed). Only seven trials were completed instead of the planned nine trials because two subjects withdrew from participation directly preceding initiation of sessions 1 and 3. Group 2 completed nine trials, with one of them being determined as a hit while six misses and two ties were also recorded ($p = .11$, two-tailed).

Analyses of the mean growth differential between experimental and control cultures were completed by the use of the Wilcoxon matched-pairs signed-ranks test. The four subjects in group 1 collectively produced a mean growth differential of -13.57 mm. per trial. This corresponded to a $t = 43$ and an $N = 32$, which was then converted to a $Z = 4.13$ ($p = .00004$, two-tailed). The five subjects in group 2 collectively displayed a suggestive but non-significant growth curve in the other direction. Their results yielded a $t = 286$ and an $N = 41$, which was converted to a $Z = -1.87$ ($p = .06$, two-tailed).

In series 2, nine trials completed by group 1 were analyzed

as being successful. There was a total of nine hits with no misses ($p = .004$, two-tailed). They produced a mean growth differential of -6.89 mm. per trial which gave a $t = 293$ and an $N = 42$. The results were converted to a $Z = 1.98$ ($p = .048$, two-tailed). For group 2, the four subjects did not succeed in obtaining significance with either comparison of trials or the mean growth differential.

In examining overall results, group 1 had 16 hits and no misses, producing a highly significant $p = .00003$, two-tailed. Collectively, the seven subjects produced a mean growth differential of -9.81 mm. per trial, or almost -2 mm. per dish over a total of 80 dishes. This yielded a $t = 639$ and an $N = 74$. This was converted to a highly significant $Z = 4.03$ ($p = .00006$, two-tailed). Group 2 finished with four hits, eleven misses, and three ties over the two series ($p = .08$, two-tailed) while the mean growth was non-significant.

Support for a hypothesis of long-distance PK is gained from two statistical treatments of the data. This is reflected in the success for group 1 in all 16 trials. Additionally, this confirmation is augmented by the significant growth differential curve established during series 1 and 2 and found in the pooled data. Thus, we were able ostensibly to demonstrate a psychokinetic influence on the targets from a distance of one to fifteen miles.

Examination of the mean growth differential for group 1 for both series (-1.96 mm. per trial) suggests the likelihood of both conscious and unconscious psychokinetic influences operating during the experiment. Overall, the mean growth rate for the 80 control cultures for group 1 was .65 mm. per hour. Subjects reported in a post-experimental questionnaire that they consciously concentrated from five to 15 minutes per day, amounting to a maximum of one hour per session. This suggests that they could not have retarded growth only during periods of conscious concentration. This would account for only one-third of the mean growth differential. A tangential issue is whether any production of unconscious PK was as selectively focused as we would expect while the subjects were consciously visualizing specific targets. Unconscious PK influence on targets spaced closely together would require a careful interpretation of individual contributions. The designated boundaries for target influence were arbitrarily established and any subject's lack of adherence to the "rules" of the experiment would conceivably require examination of a group PK effect.

An alternative explanation is that the fungus was appreciably "affected" during the periods of conscious concentration by an unknown psi-induced mechanism and required a latency period before resuming a normal growth rate. Alteration of growth rate might have occurred by: extreme temperature change, enzyme degradation, chitin breakdown, etc.

During any one session there was a minimum of eight individuals who through myriad psi interaction could have influenced

the outcome in any one direction. Experimenter influence might serve to help explain differences found between groups 1 and 2. On the one hand, parapsychological experimenter influence elicited by W. T. might have helped shape the divergent growth patterns. On the other hand, the established rapport between W. T. and the subjects in group 1 might have produced subsequently higher motivational levels and an expectancy effect not found in group 2. A post-experimental questionnaire indicated that members of group 1 had a greater acceptance or belief in four aspects of the experiment. The questions to which they responded more favorably were, "To what degree do you believe in your ability to: perform PK when close to a target, perform PK while at a distance of one mile or greater, inhibit fungus growth?" and "Did you feel successful at inhibiting fungus growth?"

By nature of the experimental design, there is one obvious reservation to the total acceptance of a hypothesis of long-distance PK. Co-experimenter M. M. could have served as an intermediary in gaining need-relevant information by extrasensory means and subsequently affected the outcome of the sessions. The probability of this, however, appears to be low when considering the complexity of the task. M. M. would have had to determine who the subjects were for each group and in which particular session they participated. Also, she would have had to determine the specific targets designated for each session and for each subject, and then influence the outcome by altering possible random results towards specified directions.

## THE EFFECTS OF RESPONSE BIAS ON INTENTIONAL AND NONINTENTIONAL ESP PERFORMANCE

Ephraim I. Schechter (University of Pittsburgh, Johnstown)

Three studies were done to evaluate the hypothesis that nonintentional psi testing produces higher hit rates than intentional psi testing when the responses involved are low-probability ones but not when high-probability responses are involved. An automated four-choice clairvoyance task was used, masked as a test of subliminal perception and ESP.

Experiment 1: Twenty undergraduate volunteers served as the percipients in this study. The percipients were told that a dim light would be briefly flashed behind one of the symbols. During the instructions, a white light behind one of the symbols was turned on and the percipients were told that the light used during the actual test would be much faster and dimmer than the one they were seeing. They were also told that half the trials would involve only "subliminal perception," while the other half of the trials would involve both subliminal perception and ESP. On the "ESP trials" the symbol that was the target for the trial would appear on a screen in the

control room. A colored light on the percipient's panel would indicate which test was being given at the moment.

These instructions were a cover story to permit both intentional and nonintentional psi tests. There were no "subliminal perception" trials; no lights were ever turned on behind the symbols during the actual tests. In addition, the colored light did not always indicate what was actually happening. On half the "ESP" trials the REG was selecting a target. These were intentional ESP trials. On the other half of the "ESP" trials the REG was turned off and no target was selected. This was a control for the effects of the percipients' belief that psi information was available.

Similarly, only half of the "subliminal perception" trials actually involved psi-mediated information. This was a test of the percipients' "normal" symbol-choice behavior. On the other half of the "subliminal perception" trials, however, the REG was turned on. Since the color of the cue-light should have led the percipients to believe that these were not ESP trials, this condition provided a test of nonintentional ESP. The four conditions were presented in a different random order to each percipient.

The instructions also attempted deliberately to manipulate the percipients' response biases. Each percipient was given a card indicating that one of the symbols would be the target on 50 per cent of the trials, a second symbol would be the target on 25 per cent of the trials, and the third and fourth symbols would be targets 18 per cent and 7 per cent of the time respectively. Each symbol was associated with each of the frequencies for one-quarter of the percipients. The targets which were selected on the psi trials were actually chosen at random (i. e., with equal probabilities of occurrence) by the REG.

The original plan called for several sets of 36-trial runs with each test condition, presented in a random sequence. However, time considerations forced us to limit the experiment to only 36 trials in each condition.

An analysis of the frequencies with which the percipients selected each symbol indicated that the response-bias instructions had produced strong effects. A factorial Analysis of Variance indicated that the number of responses dropped steadily from a mean of 13. 72 at the "50 per cent" bias-level to a mean of 6. 19 at the "7 per cent" bias-level ($F = 11.16$; 3 and 54 df; $p < .01$). There were no other significant effects, nor were there any significant interactions.

The hit-rates in the intentional and nonintentional psi conditions were analyzed by converting the absolute number of hits made to each target into a percentage of the total number of responses the percipient had made to that target. The proportions were subjected to an arcsin transform and analyzed by means of a factorial Analysis of Variance. The results appeared to fit the predictions made by the hypothesis, in that the proportions of hits increased steadily from

a mean of 21.56 per cent at the "50 per cent" bias-level to a mean of 29.96 per cent at the "7 per cent" bias-level in the nonintentional-psi condition, but showed no regular pattern in the intentional-psi condition. The interaction, however, was not statistically significant ($F = .66$, $df = 3, 54$; $p > .05$).

The data from this study show a weak trend in the direction predicted by the hypothesis. It is possible that, had more trials been run in each condition, the apparent trend might have been statistically significant. The second study in the series was a replication of Experiment 1 but with each condition tested four times, with 24 trials per run, for a total of 96 trials in each condition.

Experiment 2: Twenty additional volunteers served as the percipients in this study.

As in Experiment 1, the response-bias instructions had a pronounced effect on the percipients' choices. The mean number of responses dropped from 35.60 at the "50 per cent" bias-level to 15.19 at the "7 per cent" bias-level ($F = 16.67$; $df = 3, 54$; $p < .001$), with no significant effects of the other variables or interactions among variables.

The general pattern of the hit-rates resembled that seen in Experiment 1, with a fairly steady rise in the proportion of hits as the response bias increased in the nonintentional psi condition, from a mean of 26.66 per cent at the "50 per cent" bias-level to a mean of 28.46 per cent at the "7 per cent" level; there was no such pattern in the intentional psi condition. Again, however, the apparent difference in trends across the two conditions was not statistically significant ($F = .5273$, $df = 3, 54$; $p > .05$).

A post hoc examination of the overall tendencies to choose each symbol indicated that while the means showed a strong effect of the instructions, some individual percipients showed only weak biases. The percipients were divided into those whose call tendencies showed a progressive decrease as the instructions indicated a decreasing "frequency" ($N = 7$) and those whose call patterns deviated in any way from a progressive decrease ($N = 13$).

According to the hypothesis, the response bias instructions should have produced a progressive decrease in responses and a progressive increase in the proportion of hits in the nonintentional psi condition, and no particular change in the proportion of hits in the intentional psi condition. This means that "change scores" for responses and proportion of hits should have been negatively correlated for performance in the nonintentional psi condition and should have had no systematic relationship in the intentional psi condition. Pearson product-moment correlations suggest that these predictions were confirmed for the percipients whose call-patterns had been strongly affected by the instructions (intentional psi condition: $r = -.11$, $df = 6$, $p > .05$; nonintentional psi condition: $r = -.41$, $df = 6$, $p > .05$), but not for those whose patterns showed weak or

no effects of the instructions (intentional psi condition: $r = +.12$, $df = 12$, $p > .05$; nonintentional psi condition: $r = +.53$, $df = 12$, $p < .05$). Experiment 3, a direct replication of Experiment 2, was run to see if these relationships would recur.

Experiment 3: Twenty additional volunteers served as the percipients in Experiment 3.

As in Experiments 1 and 2, there was a statistically significant decrease in the number of calls as the "bias-level" decreased, from a mean of 34.48 at the "50 per cent" bias-level to a mean of 14.20 at the "7 per cent" level ($F = 42.68$; $df = 3, 54$; $p < .001$). Unlike the results in Experiments 1 and 2, however, there was no systematic increase in the proportion of hits in the nonintentional psi condition as "bias-level" decreased; none of the variations in performance was statistically significant. The post-hoc analysis revealed the same relationship that had been seen in Experiment 2. For percipients whose call-patterns showed a consistent decline in responses as "bias-level" decreased ($N = 14$), the correlation between the number of calls and the proportion of hits was +.12 in the intentional psi condition ($df = 13$; $p > .05$) and -.91 in the nonintentional psi condition ($df = 13$; $p < .001$). The correlations for those whose call-patterns suggested that they had not been strongly affected by the instructions ($N = 6$) were +.33 in the intentional psi condition ($df = 5$; $p > .05$) and +.30 in the nonintentional psi condition ($df = 5$; $p > .05$). In other words, the percipients who appeared to have been most strongly affected by the instructions showed the predicted relationship, while those who had not been strongly affected did not.

The results of these three experiments suggest that the hypothesized relationship between response-bias and whether or not the percipient knows that he or she is being tested for psi may exist, although it appears that it was masked by the inefficiency of the response-bias manipulations. The obvious next step is to design procedures that will maximize the effects of the response-bias instructions, to see if the relationship occurs more strongly.

## SUBLIMINAL PERCEPTION AND EXTRASENSORY PERCEPTION IN A LEARNING PARADIGM

R. Jeffrey Munson, James W. Davis, and K. Ramakrishna Rao†
(Institute for Parapsychology, FRNM)

Subliminal perception (SP) and extrasensory perception (ESP) are similar in that they are both unconscious processes which affect our conscious perceptions and behaviors, such as sensory experiences and scoring patterns in tests. Psychological factors such as anxiety, defensiveness, and belief modulate the effects of both kinds

of perceptions, as do various kinds of altered states of consciousness. SP and ESP may be related to primitive brain and/or psychic functions, or they may possibly represent perceptions at various levels of filtering. The difference between subliminal perception and ESP is in the nature of the stimulus. SP results from a chain of events which is, at base, sensory in character and conceptually easier to trace psychologically and physiologically. ESP, however, does not depend on the physical senses for reception by or emergence in the brain prior to its operation in or on the brain or psyche. Are ESP and SP parallel or compensatory functions? Where do their similarities cease?

The experiment presented here involved subjects participating in a visual subliminal perception task in which was embedded an unconscious clairvoyance task. Half of the subjects received trial-by-trial feedback while the other half did not. Thus, the test attempted to compare learning in ESP and SP without potential confounds from conscious awareness of the psi task. Specifically, the experimenters were interested in whether factors that aid SP (feedback) would help or hinder ESP. If feedback helped both, then their parallel nature would be revealed in a relative increase of both sets of scores over the span of the test. However, if the relationship between SP and ESP is a compensatory one, i.e., ESP "picks up" where SP "leaves off," then we would expect an increase in SP scores to be accompanied by a decline in ESP scores.

Subjects were presented with 80 slide projections and instructed to choose from among ten possibilities what they thought the target for each trial was. Though subjects believed they were doing just a subliminal perception task, they did not know that there were two projectors inside the protective enclosure. One had pictures of actual targets and one had slides of ambiguous drawings. The sequences of slides in both projectors were randomized by a white noise diode RNG, as was the order of projection by the machines. The projection of a "real" picture required (or allowed) the subject to use subliminal perception to choose the correct response successfully, while the projection of an "ambiguous" picture required the subject to use clairvoyance to succeed. Targets for ESP trials were "real" pictures in the "real" projector sitting in one-to-one correspondence with the presented "ambiguous" picture. The order of subjects given feedback was also randomized.

Feedback scores were separated from nonfeedback scores, ESP and SP scores were then separated, and, finally, scores in the two perceptual modes were divided into first- and second-half (of the test session) scores. A 2 x 2 ANOVA with one repeated measure was performed on scores of each perceptual mode, using feedback-nonfeedback and first half-second half as the levels of the factors. In addition, t-tests were performed where indicated.

A t-test comparing SP scores with chance expectation reveals that SP was clearly operative in the sensory condition ($t = 5.314$, 31 df, $p < .002$). The ANOVA shows that the difference

between feedback and nonfeedback scores in SP was borderline significant ($F = 3.83$, 1 df, $p = .057$). However, the significant difference between the first-half and the second-half of the test ($F = 13.012$, 1 df, $p = .001$) when feedback and nonfeedback scores are combined suggests that learning through feedback was less effective here than learning through attention to subliminal cues. The results of the ANOVA for the ESP data were nonsignificant. However, there was a suggestion of a relationship in the trends of the four factor-level means ($F = 1.937$, 1 df, $p = .17$); in the feedback condition, scores went from below chance to above chance, while the opposite occurred in the nonfeedback condition. If this trend had maintained itself for a larger population, it would have implied that feedback helped learning in ESP.

Changes of subjects' scores between session halves in each perceptual mode were computed in order to separate subjects into improvers and nonimprovers in each learning condition and perceptual mode. These different scores were then correlated with the difference scores of their opposite perceptual mode. Neither improvement nor nonimprovement in either SP or ESP scores significantly correlated with changes in ESP or SP scores, respectively, for both learning conditions.

## A LONG-DISTANCE, TIME-DISPLACED PSYCHOKINESIS EXPERIMENT WITH SELECTED SUBJECTS: A PRELIMINARY EXPERIMENT

William Braud (Mind Science Foundation)

If successful investigators are really "special subjects masquerading as experimenters," as Millar (EJP, 1979, 78-110) suggests, then intensive experimentation with these "modern mediums" would appear to be almost mandatory. The experiment reported in this brief was an attempt to determine the feasibility of studying the psychokinetic abilities of selected successful investigators, remotely and under quasi-optimal conditions. The task chosen for this initial attempt was one involving long-distance, time-displaced psychokinesis. The subjects were 23 members of the Parapsychological Association selected on the basis of their having published, rather consistently throughout the years, the results of experiments with "successful" outcomes.

During the evenings of April 7 and April 9 (1980) combined, a total of 23 blocks of runs was automatically generated by a microprocessor/random event generator system located in the Foundation laboratory. No one was present in the laboratory while these runs were in progress. Each block consisted of 40 runs, and each run consisted of 250 trials. For each of the 10,000 trials of an experimental block, one of five possible numbers was generated by the

computer's deterministic random generator; next, one of five possible numbers was generated by the radioactive decay-based, truly random event generator. A hit was defined as the coincidence of the two numbers generated on a given trial; thus the probability of a hit was 1/5. When the 40 runs of a block had been accomplished (a process requiring approximately 30 minutes for its completion), the computer provided a paper printout of the time the runs began and listed the number of hits for each run, as well as the mean number of hits for all 10,000 trials and the CR for these trials. Trial blocks commenced every hour on the hour throughout the night.

In the morning, W. B. removed the printouts from the printer without observing any of the printed numbers. When the printouts of 23 trial blocks had been accumulated, W. B. gave the printouts to an assistant, Gary Davis, who carefully photocopied each printout onto the first page of a form letter without observing the printed numbers. This first page of the letter instructed the reader to observe the CR, the mean number of hits, and the individual run scores, and indicated that these numbers were the reader's scores for an experiment in which the reader was then participating. Details of the experiment were provided on a second page of the letter which was stapled behind the first page. The third page of the letter, stapled behind the second, was a brief questionnaire which asked the reader to indicate the time and date at which the scores were observed, and (on ten-point scales) how "psi-favorable" was the reader's mood when he or she opened the letter and how much he or she believed in the reality of retro-PK. The reader was requested to return the questionnaire to W. B. in an enclosed stamped and addressed envelope. The envelopes which contained the letters to the investigators were inscribed, "Please open this letter only when you are in a psi-favorable mood."

W. B. 's observation of the original printouts and analysis of results occurred only after all questionnaires had been returned. Receipt of the questionnaires indicated that the scores had indeed been previously observed by the investigators. Eighteen of the 23 subjects returned their questionnaires before the pre-established June 1 deadline; therefore, PK data were analyzed for only these 18 subjects. A single mean t test indicated that the mean number of hits per run did not differ significantly from MCE = 50 ($\overline{X}$ = 50.00, t = .006, p = ns). The results of randomness tests (23 blocks of 10,000 trials each) did not differ significantly from MCE ($X^2$ = 49.90, t = -.53, p = ns).

It may be of interest that three of the 18 experimental CRs were independently significant ($p < .05$, two-tailed), and all of these were negative. None of the 23 control (randomness test) CRs was independently significant. (The CR was used in the feedback printouts because of its highly evocative and potentially rewarding power for para-psychologist subjects, and not as the preferred statistical test.) The questionnaire items regarding mood and belief in retro-PK were included simply for their information value; they cannot be used as predictors of PK performance since the subjects

had already seen their PK scores when they answered the questionnaire. (The author recognized this but deliberately designed the experiment in this fashion in order to maintain the "surprise" element which was felt to be important to PK success.) On scales of 1 to 10 (with 1 being the favorable extreme), the mean self-ratings of the 18 subjects for mood and belief in retro-PK were 3.83 and 4.28, respectively.

## LONG-DISTANCE, NOCTURNAL PSYCHOKINESIS

William Tedder (University of Tennessee) and William Braud† (Mind Science Foundation)

Two recent reviews of the experimental psychokinesis literature (Stanford, Wolman Handbook, 324-381; Braud, EJP, 1978, 137-162) indicate that PK effects may occur in the absence of immediate, trial-by-trial feedback and without definite knowledge of the specific "mechanics" of the target system. These findings suggest the feasibility of experiments in which subjects attempt to influence, at a distance, target systems, the specific nature and operating conditions of which they are unaware. The possibility of PK effects in the absence of immediate trial-by-trial feedback also suggests that PK experiments might be carried out while the subject attempting the PK influence is asleep. In this brief, we report the results of a preliminary test of this possibility.

In Experiment 1, a selected subject (W. T.) located in Knoxville, Tennessee, attempted to influence a remote random event generator located 1,100 miles away in San Antonio, Texas. Half of the trials occurred while W. T. was awake, half while he was asleep. The target system was a radioactive decay-based random event generator interfaced with a microcomputer. W. B. supervised the generation of target events which began at 8:00 p. m. and ended at 8:00 a. m. on each of 12 nights.

Each night, trial blocks began automatically every hour on the hour. For each trial, one of five possible numbers was generated by the computer's own deterministic random generator by means of an arithmetic algorithm. Immediately after the first number had been generated, one of five possible numbers was generated by the truly random (radio-active decay-based) generator. W. B. was unaware of which blocks were target blocks and which were control blocks. This determination had been made by W. T. before the start of the experiment and was not revealed to W. B. until the blocks in question had been completed. W. T. randomly selected for each of the 12 nights: an experimental block which he would attempt to influence while awake, a corresponding waking control block, an experimental block which he would attempt to influence while asleep, and a corresponding sleeping control block. Before sleeping, W. T.

gave himself the suggestion that he would attempt to influence the remote generator at the appropriate time; he would try to produce a large number of hits.

Each Monday morning, W. B. and W. T. mailed one another their photocopied records of the results and the target sequences, respectively, for the trials that had been generated during the previous week, assuring that the information crossed in the mail. At the end of the experiment, W. B. and W. T. independently analyzed the results of the experiment.

Single mean t tests were used to compare each of the four sets of 12 total hits scores with MCE = 2000 hits/block. The analyses yielded the following results: waking experimental--$\bar{X}$ = 1976.5, t = 2.39, p = .04; sleeping experimental--$\bar{X}$ = 1984.2, t = -1.33, p = .17; waking control--$\bar{X}$ = 1999.8, t = -.02, p = ns; and sleeping control--$\bar{X}$ = 1998.8, t = .11, p = ns (all ps are two-tailed). The combined 24 scores from the experimental blocks differed significantly from MCE = 2000 hits/block (t = -2.59, p = .02). Nocturnal sleeping was no more favorable to the PK effect than was waking.

In Experiment 2, an identical protocol was employed, with the exception that an auditory cue was used to signal the experimental epochs to W. T. This cue took the form of a 30-min. tape recording of soft but distinctive music with interspersed verbal suggestions by W. B. for PK success. This hopefully helped "focus" W. T.'s PK attempts. The auditory cue was switched on automatically by means of a timer.

The scores for the experimental epochs changed from a missing to hitting direction, but were not significantly above MCE for either the waking epochs (t = .56), the sleeping epochs (t = .25), or the combined epochs (t = .60); as before, the waking and sleeping epoch scores did not differ from each other or chance. Single mean ts for the waking, sleeping, and combined epochs were, respectively, -.007, -.27, and -.21.

The results of Experiment 1 suggest the feasibility of conducting long-distance, nocturnal PK experiments. The nocturnal aspect of the experiments permits the more efficient use of equipment which is otherwise idle, and the two-experimenter aspect should appeal to the critics of psi research who object to experimenters working alone. The results of Experiment 2, however, suggest that this particular experimental design is no more immune to the perennial decline effect than are more conventional designs.

# A MAJORITY VOTE TEST TO OPEN THE PRATT LOCK

Arthur S. Berger, Joyce Berger, Vicki Kalin Deriso and William G. Roll† (Psychical Research Foundation)

One of the major goals of parapsychology has been the development of methodology to make psi a dependable instrument for the acquisition of information. In this experiment the information to be acquired was the combination of a lock left by the late J. Gaither Pratt, who participated in Ian Stevenson's combination lock test for survival.

Sampling and majority vote techniques were used to enhance the psi of the five subjects who took part in the card guessing experiment of 90 runs each for a total of 450 runs. The pack consisted of 36 cards, half of which represented the index target (the combination, expressed in binary integers, of a lock identical to the Pratt lock) and half of which represented the actual target (the combination of the Pratt lock). The subjects were to guess whether each card represented the binary integer 0 or the binary integer 1. Neither experimenters nor subjects knew to which group a card belonged. The hitting rate on the index target was used to predict the hitting rate on the actual target.

Two alternative hypotheses were tested:

1. Positive scoring on the index target would mean positive scoring also on the actual target so that all calls would stand. Negative scoring on the index target would mean negative scoring on the actual target and all calls would be reversed.

2. The subjects were encouraged to try to open the Pratt lock, perhaps by acquiring the information from the surviving Pratt personality, rather than the control lock whose combination was in a co-experimenter's mind. If there was negative scoring on the index target, therefore, there would be positive scoring on the actual target and all calls would stand. If positive scoring, all calls would be reversed.

After the experiment the combination of the control lock was converted from three pairs of decimal numbers into 18 binary integers which were used to make a run-by-run scoring and sampling of the calls on the index target cards to determine if subjects were psi-hitting, psi-missing or scoring close to chance. After sampling techniques were applied run by run for 450 scores, a majority vote on each target card was taken as the single call for the card.

Under neither hypothesis did the subjects acquire the combination to the Pratt lock. Interestingly, the data with respect to the second hypothesis suggest that the subjects responded to the dual situation by a slight avoidance of the index target, which four of the five subjects missed more than they hit.

# AN ATTEMPT TO EMPLOY MENTAL PRACTICE TO FACILITATE PK*

Robert L. Morris (University of California, Irvine) and Judith Harnaday (Laguna Hills, California)

The present study is a continuation of the work presented at an earlier convention by Morris, Nanko, and Phillips (RIP 1978, pp. 146-150) on imagery strategies for influencing random event generators (REG's). The popular psychic development literature frequently contains the suggestion that one apply these strategies considerably in advance of the actual occurrence of the event, and that one experience, generally through visualization, the desired outcome at frequent intervals in advance of its anticipated occurrence. Our study applies this notion, the concept of "mental practice," using the same apparatus and PK task as employed in the earlier study.

The 31 subjects for the study (17 females and 14 males) were college-student volunteers and recruits from local and campus parapsychological circles. The main experimenter, J. H., was a volunteer from the community who has done parapsychological research as an undergraduate. R. M. served as subject recruiter and target preparer. The apparatus consists of a four-module multi-purpose testing system designed for a variety of studies (RIP 1976, pp. 39-40). It maintains an internally-generated source of random binary decisions by amplifying Zener diode noise, then converting the amplified noise to logic levels which are divided by two to insure that equal time is spent in the high and low states. The display to the subject consisted of a ring of 16 lights, seven cm. in diameter. The binary random decisions were employed to advance the illuminated light one step clockwise or counterclockwise, thus producing a "random walk" back and forth on the circle.

Before the study began, R. M. used a random number table to assign target directions (clockwise or counterclockwise) to each of 32 subjects, counterbalancing direction from the first sixteen to the second sixteen (unknown to E). Using a similar procedure, half of the subjects were assigned the same target direction for the second session and half were assigned the opposite direction.

Each S was greeted by E and led into a sound-proof 8' x 15' laboratory room. The subject was seated at a large round table, on which was placed a control console and a random walk circle-of-lights feedback apparatus. S was shown how to activate the apparatus, the procedure was explained, and S was given a questionnaire on visualization skills and familiarity with mental development procedures. E then left the room to operate the control console. S opened an envelope and read the target direction for the session,

*Delivered by Gail Kawanami-Allen.

then did four runs of 16 trials each. S then read instructions on mental practice and attempted to image being successful at influencing the lights, for approximately four minutes. Following this, S did four more runs of 16 trials each. At the end of the session E returned to S, provided general feedback, and provided S with sealed instructions for practice during the next week.

S's instructions for the week were to engage in a similar mental practice procedure once a day, bearing in mind a new target direction assignment also contained in the instruction envelope. At the start of Session II, S was told the format was identical to Session I, and shown one run of light movement to refresh memory. S was encouraged to take as much time as was comfortable preparing for and performing Session II. Upon completion of the entire session, S. was asked for the assigned target direction (later verified with R. M. 's records) and given general feedback. At session's end, S was asked to write out answers to the questions: How many sessions did you do, for how long, at what time of day, and how vivid was the experience each time?

Hits were determined by comparing the tally of clockwise steps taken for each run and the assigned target direction for that run. Since the tally was recorded before E knew the target direction, E was not in a position to make biased recording errors in copying the number from the console display. The overall results were positive but not significantly so (50. 50 per cent, Z = +. 89). There was no significant difference between first and second sessions or between halves of the sessions.

There was a general trend towards improvement from first session through the first half of the second session, but the second half showed negative scoring. Those given the same instructions from first to second session did insignificantly better the second session than those who were given opposite instructions. No meaningful correlations were found between psi performance and number of practice sessions, average duration of session, vividness of experience, or prior experience with a mental development technique.

## FEEDBACK EFFECTS ON CLAIRVOYANCE IN A SLEEP-DEPRIVED STATE

Douwe Bosga, Henk Smid, and Dick J. Bierman† (Research Institute for Psi Phenomena and Physics)

Two subjects did 16 standard forced-choice clairvoyance runs and 16 free-response clairvoyance trials at a rate of one each hour. Forced-choice runs were done on the even and the free-response trials were done on the odd hours. In half of the cases immediate feedback was given.

Data were split in two parts before analysis by different analyzers.

The results of the forced-choice runs were at chance level, as well in the first 16 hours as in the sleep-deprived state.

The average ranking of the free-response trials for the first part of the experiment was slightly better than the average over the sleep-deprived trials. However, for the trials on which feedback was given, the effect of sleep-deprivation was stronger, as was hypothesized on the basis of the Observational Theories. This was mainly caused by significant missing in the sleep-deprived state ($p < .02$, one-tailed). Feedback proved to be a relevant variable throughout the free-response part of the experiment, as the pooled first and second 16-hour trials showed a significant feedback effect ($p < .01$, two-tailed). No analyzer effects were found.

## A SURVEY OF OBEs

Susan J. Blackmore (University of Surrey)

Previous surveys of OBEs have given incidences ranging from eight per cent to 34 per cent. Some of these can be doubted, and most considered overestimates for a variety of reasons, including the ways the sample was selected and the wording of the question. Even in the best designed surveys it is possible that some respondents did not clearly understand the question and this is more likely to produce an overestimate.

A survey of OBEs was carried out using students who had attended parapsychology lectures, including one specifically on OBEs. They should have known precisely what the term meant. Two hundred and sixteen in four groups were asked the question "Have you ever had an out-of-the-body experience?" Answers to underline were: 1. No; 2. Once; 3. Occasionally; 4. Often; 5. Can have one at will. Thirteen per cent said "yes" (i. e., answers 2 to 5), which is less than in most previous surveys. Possible reasons for the difference are discussed. In addition, only 46 per cent of these claimed more than one OBE (i. e., answers 3 to 5), which compares with Palmer's figure of 82 per cent.

Some of the groups completed ESP tests, imagery tests (QMI) and a questionnaire on dream recall and vividness. The same students tended to report OBEs and lucid dreams ($X^2 = 6.72$, $p = .01$) but no relationship was found between reported OBEs and ESP scores, dream vividness or frequency of dream recall. Certain psychological theories of the OBE predict that OBEers should have more vivid imagery but no such relationship was found here.

A small group (33) of students with no knowledge of parapsychology was also tested and the results were very different. Thirty-three per cent claimed to have had an OBE and of these, most (82 per cent) claimed more than one. This is similar to the findings of previous surveys and further highlights the difference between the different types of respondent. Further comparisons of this kind are planned.

## ESP AND PERCEPTUAL DEFENSIVENESS: A REPLICATION

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

The Defense Mechanism Test (DMT) developed by Kragh in Sweden, which seems to have been reasonably well validated to measure psychological defensiveness in perception, has offered a rather unusual opportunity for testing the distortive-defensive concept of the bidirectionality of ESP. Detailed descriptions of the DMT have been given at earlier conventions and in the November 1979 issue of the EJP.

Several experiments have tested the relationship between subjects' ESP scores and characteristics in their DMT-protocols. In the majority of these experiments statistically significant relationships have been found. Subjects showing a high level of defensive organization in their DMT-protocols have tended as a group to obtain ESP scores at or below MCE, whereas a low level of certain types of "defensiveness" tends to go together with positive scoring.

### Procedure

Each subject participated in three sessions. Session I: 178 male students at various faculties at the University of Iceland completed an 18-item questionnaire dealing with dream recall and sleeping habits. At the end of the questionnaire was a 40-trial forced-choice precognition test ($p = 1/4$) with four different letters as targets, to be randomly selected for each subject by the university computer after all subjects had written their calls. From this pool of subjects, extreme groups were selected on the basis of high or low dream recall. In a study in 1970, Johnson had found a positive correlation between freedom from certain types of defensive structures in the DMT and ability to recall dreams. In another study he'd been able to show a positive correlation between ability to recall dreams and ESP-scoring. In this way, we hoped to obtain more extreme DMT-scorers than by the use of unselected subjects on the dream-recall variable.

Session II: 65 male subjects were administered the DMT.

Sixty provided usable protocols which M. J. scored during his stay in Iceland. Most of these subjects were drawn from the groups of high or low dream-recallers. Seven subjects were selected for DMT-testing based on extreme scores on the paper and pencil test; three based on low; four, on high scores.

Session III: A day or two after the DMT-testing, each subject participated in a 40-trial ($p = 1/5$) precognition computer game. Four psychology students served as experimenters, testing two subjects at one time. Continuous feedback of hits and misses was given. Emphasis was put on trying to create a playful but at the same time competitive spirit among the subjects. If a subject obtained a score of 12 or higher (MCE = 8), he or she received a book as a reward. Fifty-four subjects participated in this part of the experiment. Calls and targets and number of hits were automatically recorded by the computer. Tests of randomness of the RNG were carried out before and after each ESP computer game. The DMT-protocols were scored according to a stannine scale.

## Results

When the total ESP scores (from the precognition test of paper and pencil type, and from the computer game) were correlated with DMT-ratings, a marginally significant correlation of $r_s = .26$ ($p = .03$, one-tailed) was obtained. For the precognition test alone, the correlation is .21, and .14 for the computer game. Both are non-significant.

As in previous studies, experimenters conducting the ESP-test were kept ignorant of the outcome of the DMT-testing, which was in the hands of M. J. only. M. J., in turn, was not informed of the outcome of the ESP-test. The procedure as such was double-blind. However, toward the end of the experiment an accident occurred. A list of ten names of subjects who had scored with extreme ESP-scores in the paper and pencil precognition test had been left by one of the student-experimenters in a lab room where M. J. was working. He couldn't help noticing and identifying some of the names and the ESP-scores. This could have affected his scoring of the protocols. It is therefore imperative that these protocols be independently evaluated. The help of Dr. Kragh has been secured. His evaluation will be crucial for the assessment of the whole study.

## THE NEGLECTED SENDER: PRELIMINARY INDICATION THAT MULTIPLE SENDERS MAY ENHANCE PSI PERFORMANCE

Charles T. Tart,† Paul Chambers, and Melody Creel (University of California, Davis)

When "mental telepathy" was a dominant model for psi, the

role of the sender or agent seemed intuitively obvious: as well as the receiver being "receptive," someone needed to generate a strong psi signal and "send" it, if success was to be likely. When Rhine and his associates at Duke University found that clairvoyance experiments, with no real-time "sender" present, frequently seemed to work about as well as telepathy or GESP experiments, the role of the sender seemed less important, if indeed he really mattered at all. In parapsychological research today, the sender receives little, if any, attention. Is the sender really unimportant, or has he become unfortunately neglected for extraneous reasons? Personal observations by many experimenters in our laboratory, working with percipients on various versions of the ten-choice training devices, suggest that the sender is, at least occasionally, quite important, and that a group of senders (as when visitors are being shown the laboratory) may be especially effective. The following pilot study was carried out to see if these impressions would receive any objective support.

Data were collected on the Apple/ADEPT (A/ADEPT) ESP testing and feedback training device. Briefly, this consists of a circle of ten unlit lamps, numbered one to ten, with a push-button beside each. The percipient indicated her call by pushing the button beside the target she thought had been selected on each trial. The correct target lamp then came on for immediate feedback. A closed-circuit television camera displayed the percipient's hand movements over the response console on a TV screen mounted beside the experimenter/sender's console in another room. For group-sending sessions, the TV signal was also sent to either of two auditoriums in distant (more than one thousand feet) buildings on the UC Davis campus.

M. C. was the percipient in all eight sessions. She was chosen on the basis of availability and interest, and the fact that she had worked the previous fall with C. T. T. and P. C. as an experimenter in a small class dealing with psi experimentation. While it would have been desirable to have a percipient who had previously demonstrated psi abilities in this type of test, time limitations did not allow this. Thus, using M. C. as a percipient made the possible role of multiple sender even more important.

Four of the planned eight sessions were to involve only P. C. as the experimenter operating the equipment and the sole sender. The other four were to involve a group of students from C. T. T. 's ongoing class on Altered States of Consciousness acting as a group in one or the other of the distant auditoriums, as well as P. C. continuing to act as experimenter and sender. Time considerations determined that C. T. T. would lead the group-sending process on two occasions and his teaching assistant, Paul Hardy (P. H. ), would lead them on the other two. Attendance in each group session ranged from 12 to 31. The experimenter and percipient, P. C. and M. C. , were kept ignorant of which sessions were group and which were single-sender.

As the random scheduling turned out, C. T. T. led the first and third sending groups, P. H. the second and fourth. C. T. T. was the class instructor and thus much better known to the student senders, and C. T. T. is a known psi-favorable experimenter, while P. H., his teaching assistant, has no particular interest in psi: thus it seemed reasonable to look at C. T. T. 's and P. H. 's group-sending data separately.

Time constraints on running the experiment forced us to settle for a total of eight planned sessions, which is too small for a sensitive between-groups analysis, but could suggest differences if they were pronounced. Each session consisted of 22 trials. One single-sender session was aborted for extraneous reasons.

As a whole, the experiment produced 19 hits in 154 trials, a non-significant result. In analyzing the two multiple-sender conditions and the single-sender conditions, however, we find that C. T. T. 's group sessions yielded ten hits in 44 trials ($p = .01$, one-tailed, exact binomial), P. H. 's group sessions yielded an insignificant three hits in 44 trials, and the three single-sender sessions yielded an insignificant six hits in 66 trials. One of the group-sending sessions led by C. T. T. was also remarkable in having an apparently sustained period of psi functioning, consisting of a hit, followed by a miss, followed by three hits in a row, followed by a near hit (-1 spatial displacement).

The group-sending sessions led by C. T. T. attracted twice as many student senders as those led by P. H., and both of C. T. T. 's sessions were in mid-afternoon (3 p. m. ) as compared to the morning sessions (9 a. m. ) of P. H. Such possible confounding factors should be taken into account in future research.

This brief pilot experiment suggests that the neglect of the sender, common in modern psi research, may be deleterious. Perhaps multiple senders who can coordinate a simple burst of strong emotion in a tight time-slot create a psi signal that is easier for a percipient to respond to. A similar effect might hold for multiple agents in PK.

## PSI-INFLUENCED MOVEMENT OF CHICKS AND MICE ONTO A VISUAL CLIFF

Carroll B. Nash† and Catherine S. Nash (St. Joseph's University)

Nine chicks and ten mice were tested in 65 and 45 trials, respectively each by three agents to determine whether they could be paranormally influenced in a two-choice situation to move onto a visual cliff (a sheet of glass over an empty space) more frequently when they were willed to do so than when they were willed to move

onto a manifestly solid surface. The results were positive for the chicks with $p < .001$. For the mice, the results were positive with one agent with $p < .005$, negative with another agent with $p < .01$, and not significant with the third agent.

## A TEST OF THE PSI-STRUCTURE THEORY

Charles Knibbeler, Guus Boshouwers and Dick J. Bierman† (Research Institute for Psi Phenomena and Physics)

Simple psi structures were constructed, consisting of a single (target) person and a computer-generated drawing. Subjects were asked to guess which drawing had belonged to which person. The drawings which were part of the psi structures were mixed with similar drawings which were randomly assigned to the target persons but had never been in contact with anybody. The scores on the former drawings were compared with the scores on the latter (control) drawings. The difference evaluated by Wilcoxon matched pair ranking was non-significant. Furthermore, scoring on the control drawings was completely at chance level. However, scoring on the psi structure drawings was significantly below chance ($t = 13$; $N = 12$; $p = .04$, two-tailed).

## A TEST OF THE POTENTIAL OBSERVER THEORY USING GIFTED SUBJECTS

Dick J. Bierman (Research Institute for Psi Phenomena and Physics)

Two psychics completed two runs of PK on pre-recorded data sets. For the first data set the feedback probability of the result of each random event was 50 per cent. For the second data set this probability was reduced to 11.11 per cent. The data from these sets were displayed mixed in such a way that the subjects could not perceive any difference between the data from both sets. The Potential Observer Theory predicts that the data from the first data set (high probability feedback) would be influenced more strongly than the data from the second set. This would hold for the complete feedback as well as non-feedback data sets. The runs were divided in 208 segments. For each segment the difference between the non-feedback part of the first and the second data set scores was calculated. At this stage of analysis, destruction of 87.5 per cent of these differential scores took place to screen off PK influence of future observers. The remaining 26 scores showed pure chance results for the first run and marginal missing ($t = 1.76$; $df = 25$;

$p < .10$) for the second run. A secondary analysis which, within the framework of the model, cannot be reported here showed that the Potential Observer Theory has not been falsified by this experiment.

## THE PROBLEM OF "UNSOLVED" CASES OF THE REINCARNATION TYPE

Ian Stevenson, Emily F. Williams,† Satwant Pasricha, Godwin Samararatne, and U Win Maung (University of Virginia)

In many cases of the reincarnation type that we have studied, a person corresponding to the statements of the subject has been identified. Although observers may differ in the degree to which they are satisfied that the previous personality has been correctly identified, we have come to refer to such cases as "solved," and to those in which no adequately corresponding deceased person has been found as "unsolved." The designation of a case as "solved" does not imply that the case has paranormal features.

Unsolved cases are of particular interest to investigators, for two reasons. First, we may be able to solve some cases, and since we will have made a written record of everything the child said before the identity of the person corresponding to his statements was known, we will be better able to judge the paranormality of the child's knowledge than we can in many solved cases, in which normal and paranormal knowledge may be inextricably mixed by the time we reach the case. Second, a comparison of solved cases with unsolved ones may bring out important similarities and differences, and hence improve our understanding of the cases and the several interpretations of them.

Our study of unsolved cases has made us aware of several factors that may contribute to the failure to solve a case. The most obvious reason for failure is that the subject has not given enough information, or sufficiently specific information, to allow the positive identification of a corresponding deceased person. Shahida Ansari of India gave twenty separate details about a life she claimed to remember, but the only name she had was "Patan," where she said she had lived. Numerous villages and towns in India, however, have names ending in "Patan."

A second reason for failure is that the subject may have mispronounced or misremembered an important name. Husam Halibi of Lebanon said that he had lived in Mazraat el Chouf and had been killed in Mghaitheh. There is, however, a Maasser el Chouf, not far from Mazraat el Chouf, and there are several villages in Lebanon with names similar to Mghaitheh, such as Mghairiye and Mghaira. This case may remain unsolved, therefore, because a slight mis-

pronunciation or confusion in memory has led us to search in the wrong villages.

The subject or his parents may also make faulty inferences that mislead us. Anusha Senewardena of Sri Lanka never actually named her previous residence, but she did frequently point to Horaduwa, a nearby village, and say that she had lived there. Our search there did not turn up a person corresponding to the numerous other statements she made. Small villages in Sri Lanka are often similar in appearance. If Anusha was remembering a previous life in a village like Horaduwa, her parents or Anusha herself may have made the faulty inference that she had lived in Horaduwa itself.

Another factor contributing to the failure to solve a case could be that rebirth cases, both solved and unsolved, may consist of varying proportions of real previous life memories mixed with (normal) memories of this life and fantasies. Maung Soe Ya, a Burmese subject, gave numerous details about a previous life, including a previous name, Dr. Soe Paing, and an exact address in Mandalay where he said he had lived. He also mentioned that he had owned a Jeep, and in Burma this detail means that the person concerned must have died after 1946. Although two elderly persons in the presumed previous personality's area of residence vaguely recalled a Dr. Soe Paing, we were unable positively to verify his existence from other informants who should have known or heard about Dr. Soe Paing, if he had lived in Mandalay in their lifetimes. If, however, to real memories of a previous life, Maung Soe Ya had added the fantasy of owning a Jeep, our assumption about the probable time period for the life of Dr. Soe Paing would be erroneous.

Maung Soe Ya's case may exemplify another possible reason for our failure to identify previous personalities in some unsolved cases. Dr. Soe Paing may have lived so long ago that persons who would remember him are no longer alive or relevant documents are no longer available. Similarly, since some cases are solved when the subject or his parents unexpectedly meet someone or see a place connected with the previous life, a geographically remote previous life might be likewise difficult to trace.

Lack of interest in solving a case on the part of the subject's parents may also contribute to failure to solve it. Maung Soe Ya's parents made no attempt to identify the person about whom their son was talking, and their apparent lack of interest in the case is also indicated by their inability to recall several names he had mentioned when he was younger. The now irrevocable loss of these potentially important details may have lessened our chances of tracing Dr. Soe Paing, if he existed.

The suppression of verifying information by persons with knowledge of the correct previous personality may also be hampering our investigation of some cases. Some persons, such as those who do not believe in reincarnation or those who believe that the previous family would be upset by the claims of the subject, may not answer

our questions with complete honesty. For example, we have evidence of suppression in some cases concerned with Christians in Asia, and although Christians have on many occasions provided helpful information, the possibility of concealment in certain situations cannot be overlooked.

A final factor is simply that we may have given up the search too soon. For a case such as that of Husam Halibi, in which a slight mispronunciation may have led us to the wrong village, one of us should return to Lebanon and attempt to trace the deceased person's family in the villages with closely similar names.

The most obvious explanation for unsolved cases is that they derive from fantasies, and it is not our intention here to insist that they do not. Nevertheless, the study of solved cases has shown that they, like most other manifestations of paranormal phenomena, vary widely in the amount, specificity, and strength of detail included in the subject's statements; and our study of unsolved cases has suggested that factors other than fantasizing on the part of the subject may be preventing their solution. We feel justified, therefore, in supposing that unsolved cases may consist partly of fantasy, partly of errors on the part of the subject and his parents, and partly of real memories of a previous life. We hope that our study of unsolved cases will improve our ability to discriminate between these elements, to solve some presently unsolved cases, and to assess the possible explanations for cases suggestive of reincarnation.

## MURDEROUS ESP: A CASE OF STORY FABRICATION?

Rolf Ejvegaard and Martin Johnson† (University of Utrecht)

A case suggestive of an apparition of the dead was published in a Swedish magazine in 1978. The author gave specific details and assured the readers, by giving names of reliable witnesses, that it was a true story. If so, it would have been a remarkably well documented case of an apparition of the dead or of a dying person.

According to the story, a British couple was having tea in their home in a city in England. Suddenly the door bell rang, although it was late in the evening. The wife opened the door and to her surprise found her daughter standing on the doorstep. The young lady's clothes were torn. She said that she had been attacked by a man and had come home because she wanted to sleep in her own bed. Normally, she lived in Scotland.

The mother went upstairs and arranged the bedroom. Meanwhile, the daughter told her father in detail how she had been attacked and raped. She described the place and also gave a good description of the offender.

The parents discussed what they should do, and then the door bell rang again. The church curate had come to tell them that he had been asked by the police in Scotland to inform the parents of their daughter's death. The parents were confused. The mother went upstairs to check for her daughter but found the bed untouched. They then went immediately to the city police to report their experience. The father gave the description of the attacker. The city police passed the information to their colleagues in Scotland. The next morning the police in Scotland informed the father that the murderer had been arrested. The police were curious as to the father's ability to describe the murderer so accurately. The criminal went to court, pleaded guilty and was sentenced to 20 years' imprisonment.

The article in the weekly magazine ends with a quotation from a newspaper interview. The father said: "I can only give an account of what happened. I am convinced that our daughter, in the moments of anguish, desperately wanted to be with us in the security of our home.... For most people, this seems unlikely. But to all doubters, I'd like to add, how could my wife and I describe so accurately the murderer's appearance if our daughter had not told us?"

It was a tedious and frustrating task to check all the data with the most important sources: the police forces, the court, the newspapers, and the parents. If the story had been true, there would have been plenty of official documents backing it up. We found the names, addresses, dates, etc. were all correct. The murder case was a real one. A young, handicapped girl was brutally mistreated and raped. She suffocated by inhaling blood. She was the only child of a clergyman and his wife.

We now have sufficient documentation to properly analyze the case. We lean strongly towards the theory that the whole story, when it comes to its parapsychological parts, is a fraud, made up by an Irish news agency in order to make money. We have not been able to trace the story outside Sweden. It may have been published in other countries, but probably not in the British Isles.

We feel that articles suggestive of paranormal events and published in newspapers and magazines should, to a much greater extent than is customary, be checked as to their correctness. This should especially be observed when one is faced with stories seemingly supplying facts and supporting details which clearly indicate the author's desire to give the article an air of seriousness and credibility.

We wish to urge everyone in our field to try to isolate fabricated stories and to point out their writers. To what extent can we influence the flow of fraudulent parapsychological material in the commercial press? What strategy could parapsychologists take on this issue? Here may be an important task for members of local parapsychological societies.

## FURTHER STUDIES OF PSI AND VOLITIONAL ACTIVITY

K. Ramakrishna Rao and H. Kanthamani (Institute for Parapsychology, FRNM)

In two previous experimental studies by Rao and associates it was observed that when two subjects compete in an ESP task, their scores tend to differ significantly. This is called the volitional effect. When such a competitive set was substituted by a cooperation set in which the subjects hoped to work together to obtain a higher than chance expected score, no significant differences in their scores were obtained. Further, it was observed that in the first study this effect was obtained only in that part of the data where both the subjects were attempting to guess the same target sequences. In the second study, however, this was reversed and the effect was found only in the data where the subjects had different target sequences.

The present report deals with the third study which consisted of two series. The first series differed from the previous experiments in that K. R. R. (the first author) was not a subject. Also, the competing subjects worked at different times and were separated by long distances. In the second series K. R. R. again served as a subject, but he and the other competing subject were separated by long distances and did not work at the same time of the day. This kind of non-synchronous guessing was introduced into the design of the current experiment as a consequence of the unexpected finding that the volitional effect manifested itself in the different target data of the second study, which indicated that the two subjects need not focus on the same target at a given time to be able to produce the effect.

### Series 1

The subject, T. A., working from her home in Virginia, attempted to guess the targets prepared by and kept in the custody of H. K. at the Institute for Parapsychology in Durham. In each of the 20 sessions, T. A. made 125 calls (five standard runs) guessing ESP symbols. T. A. was told before the series began that she would be competing with a young man, A. H., of about her age, who at the time was living in Winston-Salem, and that whoever obtained better overall scores would receive a reward of parapsychological books. A. H. was similarly instructed. Both T. A. and A. H. had visited the Institute earlier and were familiar with the testing procedure employed in the series. T. A. and A. H. had never met each other and what they knew about one another was through K. R. R.

The targets for the series were generated using a standard computer program available at the Institute. The targets were generated with replacement and each sheet was given a unique number. H. K. was responsible for preparing the record sheets and safe-keeping them. She also prepared separate response sheets for the use of the two participating subjects, gave them consecutive

numbers and dates, and mailed these sheets to the subjects along with instructions.

As in the earlier study, half of the 20 sessions were intended to have identical target sequences for T. A. and A. H., and the other half different target sequences. The same and different target conditions were arranged in a random order, balancing the two conditions. The subjects were unaware of this.

The subjects were instructed to send their response sheets to K. R. R. T. A. sent them in irregular batches. A. H. sent the first sheet and never returned the others. All letters sent to him subsequently to return the other sheets were not answered. Therefore, it was decided by the two experimenters, before scoring any of the record sheets, to discard the single session record of A. H. and analyze only the results of T. A. It should be mentioned, however, that the experimenters, as well as T. A., were unaware of the fact that A. H. was not participating in the experiment until T. A. completed all of her guesses. Therefore, at the psychological level, the set of competition was very much in the mind of T. A.

## Results

It was decided, after it became clear that A. H. had dropped out of the experiment, to analyze T. A. 's results by computing the critical ratios for her session scores and to square them to obtain chi-squares which could be combined for each condition. The sum of chi-squares for the same target condition is 9. 05 (10 df), which is not significant. For the different target condition it is 18. 90 (10 df), $p < .05$. The F ratio between the two is 2. 09 (10, 10 df), which does not reach an acceptable level of significance. Thus, it appears that, even though T. A. 's performance did not differ significantly between the same and different target conditions, the volitional effect occurred in the different target condition alone. In this respect, the results of this series are like those in our second study, and unlike those of our first study.

## Series II

In this series the competing subjects were T. A. and K. R. R. While T. A. worked from her home in Virginia, K. R. R. worked from his office at the Institute. The ESP test was different from that of the first series. It was a language ESP test widely used by K. R. R., in which the targets were five words written in two languages, English and Telugu. This change was introduced largely for reasons of novelty and to keep up the motivation of T. A.

The target sheets were prepared and kept by S. K., a colleague at the Institute, using random numbers, without any attempt to balance the number of English and Telugu words. Each sheet consisted of 50 words. The subjects wrote their responses in Eng-

lish, and H. K. received their response sheets. At the end of the series, H. K. and S. K. exchanged duplicates of response and target sheets.

Before checking the data, K. R. R. reported to H. K. that his mental set during a greater part of the series was quite atypical, that he was tense and not at ease, and that he had to make a special effort to come to the Institute to make his responses during week-ends and holidays, which he did not particularly enjoy. He also stated that he might get psi-missing as a result of it. This was a time when K. R. R. was very busy finishing up several projects at the Institute and was planning a six-week trip to India.

## Results

The data in this series were analyzed to test: (a) whether the volitional effect would become manifest in a long distance non-synchronous competition test between K. R. R. and T. A.; (b) whether differential scoring between English and Telugu would occur under the conditions of this experiment; and (c) whether K. R. R. 's data would show any peculiarities consistent with his reported psychological state. Even though this series was planned to be a continuation of our study of the volitional effect, the atypical nature of K. R. R. 's state of mind at the time of doing this experiment cautioned us to regard this as an exploratory study independent of the previous ones.

Following the same procedure as in our previously reported studies, critical ratios for the differences between the scores K. R. R. and T. A. obtained in each session were computed. The $CR_d$'s were squared to obtain chi-squares. It may be mentioned here that the calls of the two subjects in each session where they had the same targets were compared, and the necessary corrections in computing the CR's were made to eliminate the effect of call patterns.

The sum of chi-squares in the same target condition is 19. 20 (10 df); $p < .05$. In the different target condition it is 14. 07 (10 df). The F ratio between the two is 1. 37 and is insignificant.

Neither T. A. 's scores nor K. R. R. 's provided any evidence for the differential effect between languages.

K. R. R. obtained overall 172 hits where MCE is 200. The negative deviation of -28 gives a CR of 2. 17 (corrected for continuity); $p < .05$. Much of this deviation is contributed by psi-missing on Telugu targets. K. R. R. obtained 77 hits in 479 trials. This score gives a deviation of -18. 8, CR = 2. 09 (corrected for continuity); $p < .05$. There was no such trend in T. A. 's data.

## Discussion

According to Rao's volitional model, when the intentions of

two subjects participating in a psi experiment are directed toward opposite outcomes, such as in a competitive situation in which one is attempting to obtain more hits than the other, a facilitatory-inhibitory function of psi becomes manifest, resulting in greater than chance expected differences in their scores. Such a difference, when significant, is called the volitional effect.

That we were able to obtain further evidence in support of the volitional effect under conditions so dissimilar to those in the previous experiments is both encouraging and frustrating. It is encouraging in that we may be dealing here with a fairly robust effect, but discouraging because the effect kept changing between the two target conditions and occurred even when only one subject was participating.

The results of Series I could also be interpreted independent of the volitional model. In reality there was no volitional conflict because the second subject of the pair did not participate. Therefore, the observed effect may be presumed to be merely a consequence of the competition set in the mind of the subject or the expectations of the experimenters. In any case, further research would seem to be in order to see whether the volitional effect is a kind of differential effect wherein the competing subjects tend to score in opposite direction in a given testing session, or whether their scores merely fluctuate independently of the other's scores.

It is of some interest to note that K. R. R. significantly psi-missed in the second series. This is what one would expect on the basis of K. R. R. 's subjective reports. There seems to be some value, therefore, to keeping records of the feelings and internal psychological states of the participants at the time they are involved in a psi experiment.

## ESP AND MEMORY: SUPPORT FOR KREIMAN'S SUMMARY HYPOTHESIS

Gertrude R. Schmeidler (City College of the City University of New York)

Many theorists consider it important to learn how ESP relates to memory or associations. In 1978 Kreiman proposed a new theory, tested it by an ingenious method on 16 students in his parapsychology course, and significantly confirmed its three interrelated hypotheses. Weiner and I independently ran replications, reported at the 1979 Parapsychological Association convention. Neither gave significant confirmation, nor (Kreiman wrote me) did his own replication. All, however, showed some supportive trends. This warranted further work. Here I report two additional series.

Kreiman's hypotheses were: 1) When subjects try to recall material they know well, they depend on memory and avoid ESP. They therefore psi-miss on ESP targets within that material. 2) When subjects grope to recall partially learned material, ESP targets within that material are facilitatory. Subjects therefore psi-hit on them. 3) Material hard to recall shows more psi-hitting than material easy to recall.

Kreiman's clever method was to ask subjects to learn 50 randomly selected words by rereading them repeatedly for five minutes, then recall them in any order, for 15 minutes. He defined material easy to recall as the first half of each recall list, and material hard to recall as the second half. His ESP task was precognitive; his inspiring preliminary discussion made his subjects eager for precognitive success.

I first tried to duplicate Kreiman's method, but precognitive targets seemed so remote to my subjects that most disregarded the ESP possibility and tried only for recall. Three later series used individual clairvoyant targets: lists (wrapped in aluminum foil, then sealed into manila envelopes) of 20 words randomly selected from the learning list. Subjects answered questionnaires. All three series pre-set experimental groups at 16 subjects with specified questionnaire responses.

Because most subjects wrote most of their recall words in an initial spurt, my second series (reported last year) defined words hard to recall as the last quartile of the recall list. Sheep who called themselves luckier tended to psi-hit on the last quartile.

The two current series used group testing and refined the scoring for the fourth quartile. The number of words already recalled was subtracted from 50; the number of ESP hits already made was subtracted from 20. The ratio of the obtained numbers specified mean chance expectation for ESP scores.

Series 3 tried for precision. I asked subjects (a) to draw a line under the last word readily recalled, and (b) to answer a detailed series of questions about luck in various situations. This was a disaster. Some subjects drew a line under each word; others drew no lines. Luck answers showed little relation to each other or to ESP scores. Trying to elicit precise responses produced chaotic data.

Series 4 went to the other extreme. Words hard to recall were again defined imprecisely as the last quartile of each recall list. Words easy to recall were defined as the first half of the list. The questionnaire asked the usual supersheep-sheep-goat question and two vague questions about luck. (I am grateful to Debra Weiner for suggesting appropriately vague phrasing for Question 2.) Probably anyone could answer either question affirmatively in one mood and negatively in another, because of selective memory about past experiences. Responses presumably, therefore, indicate mood: whether the subject feels lucky at the moment.

Experimental subjects were 16 sheep who answered both luck questions affirmatively. They recalled 287 words. Their ESP deviation was -7.9 for words easy to recall ($t = 2.12$, 15 df) and +8.95 for words hard to recall ($t = 1.71$, 15 df). The trends are consistent with Kreiman's first two hypotheses. The difference between deviations for words hard and easy to recall was significant ($t = 2.66$, 15 df, $p < .02$, two-tailed), supporting Kreiman's third hypothesis.

For completeness, I add that overall ESP deviations were below chance for each of the four goats and each of the three sheep who answered both luck questions negatively. The four goats and two of these sheep psi-missed on the last quartile. The five supersheep and 18 sheep answering only one luck question affirmatively had scores near chance.

Overall, then, the data indicate that when an experimenter succeeds in suggesting that ESP targets will aid recall, and when subjects are sheep in a mood conducive to ESP success, Kreiman's hypotheses are valid. With a different suggestion (e.g., that ESP lists will help initial learning) or subjects with different moods or attitudes, we should reserve judgment.

## EEG FREQUENCY MODULATION AS A DECODER OF UNKNOWN INFORMATION

G. L. Heseltine† and J. H. Kirk (Science Unlimited Research Foundation, San Antonio)

The methods by which information is handled in the central nervous system (CNS), and in particular how this handling is reflected in the ongoing EEG activity, remains a central question in neurobiology. Assuming the EEG reflects, at least in part, the highly integrated electromagnetic field associated with CNS functioning, three basic methods of information representation in the EEG are available. These are: 1) amplitude modulation, 2) frequency modulation and 3) phase modulation. The purpose of this exploratory study was to use a simple (and arbitrary) method of EEG frequency modulation to describe a series of unknown binary targets. In addition to the primary question, the efficacy of two areas of the EEG spectrum (theta, 4-7 Hz, and low alpha, 8-9 Hz) in describing the target was examined. This was done in a biofeedback paradigm.

A single subject was involved in two similar series of experiments. The EEG was recorded from the right hemisphere ($F_4$ -$P_4$). Prior to each session, a target sequence of 500 binary digits were generated based on the arrival time of particles at a radiation counter tube. Pairs of time intervals were taken and if $T_1 < T_2$, then a target 1 was generated, and if $T_1 > T_2$, a target 0 was gen-

erated. The EEG was demodulated in a similar manner based on the time between zero crossings of successive pairs of EEG waves. (The zero crossing detector put out a pulse every other time the EEG potential crossed zero, permitting measurement of the major period of the EEG wave.) In low alpha, if $T_1 > T_2$, then a 0 was the response. In theta, if $T_1 > T_2$, then a 1 was the response. Equal pairs were discarded. The target was complemented after each run.

The subject received auditory feedback indicating how well the EEG responses matched the target sequence. His goal was to maintain the low tone. Every 1.5 seconds the computer checked to see if there were more hits than misses, and if at least one pair of theta waves occurred. If this were true, then the appropriate tone was presented. An EEG response was generated only if both time intervals in the pair were in theta or both time intervals were in low alpha. Any other pair of time intervals did not constitute a valid response. Successive pairs of EEG time intervals were taken until 500 valid responses occurred.

Overall, the subject failed to match the target sequence at the $p \leq .002$ level. Examination of the target matching by EEG frequency showed chance scoring in theta (6,516 trials) and scoring at the $p \leq .001$ level in low alpha (53,484 trials). Low alpha was found to be associated with the greatest scoring in a previously reported EEG PK task.

## THE NONVERBAL COMMUNICATIONS OF PSI-CONDUCIVE AND PSI-INHIBITORY EXPERIMENTERS

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

Both laboratory lore and careful research indicate that some experimenters repeatedly obtain null psi results while others, whose procedures are equally careful, repeatedly obtain extra-chance psi results. By now it is not uncommon to designate the former as psi-inhibitory and the latter as psi-conducive.

To find if the experimenters' nonverbal communications relate to the data they obtain, we showed videotapes of both types of experimenters (with words inaudible) and asked subjects to judge their personalities. G.R.S. expected to find a meaningful relation between personality evaluations and the experimenters' data. M.M., who prepared the videotapes, felt uncommitted toward this hypothesis and did not expect subjects' judgments about personality of psi-conducive and psi-inhibitory experimenters to differ markedly.

The Council of the Parapsychological Association gave us per-

mission to videotape part of the program of the 1979 convention, on condition that experimenters who were taped consented. Tapes were made of 27; all graciously consented to use of the tapes for research or training.

We viewed the tapes, and selected all which showed (a) a speaker for at least four and half minutes; (b) some pause for questions and the speaker's response; (c) a person whose recent research permitted us to classify him or her as psi-conducive or psi-inhibitory.

We then matched the psi-conducive and psi-inhibitory experimenters on the basis of sex, approximate age, beardedness, and whether or not they had been reared in the United States. The matching left us with only five pairs of experimenters.

We used a random block design to determine the order of presentation within each pair, and determined the order of pairs randomly.

M. M. edited the ten selected experimenters' tapes so that each lasted approximately five minutes and included both prepared remarks and speaker-audience interaction. She transferred these segments to two fresh tapes and provided a short blank interval between speakers. One tape held six segments; the other held four.

G. R. S. 's undergraduate class in experimental psychology volunteered to act as judges. All were young adults or in early middle age. The tape with six segments was judged during a class period by 12 females and nine males. The other tape was judged by six females and eight males after a later class period. A small sum was paid for second judgings.

Each judge was given a response sheet for every experimenter to be judged. Instructions stated that the tape would show experimenters giving convention reports and responding to the audience, then asked judges to describe their impressions of the experimenter on the response sheet by (a) commenting spontaneously during or after the viewing; (b) scoring the adjective list; (c) responding to a final question. The 30 adjectives were listed alphabetically. The last question asked that the experimenter be put into one of two categories which represented, in shortened form, G. R. S. 's concepts of psi-conducive experimenters (those who bring out adaptive, lively, free responses) and psi-inhibitory experimenters (those who make subjects freeze up and give downbeat, defensive answers).

The room was darkened, and the videotape was shown with sound so low that words could not be recognized. When the segment ended, lights were turned on and judges made their responses. This continued until the tape was completed. Judges designated their sex.

The adjectives were scored on a five-point scale, from +2 for terms marked especially true for a person to -2 for terms marked definitely not true. Totals for the five experimenters in each

group showed higher totals for psi-conducive experimenters on 16 adjectives; higher for psi-inhibitory on 14. Adjectives are listed below in descending rank order; i. e., greatest difference first.

Psi-conducive experimenters were rated more flexible, free, friendly, relaxed, warm, enthusiastic, likable, playful; somewhat more active, poised, hesitant, confident; and perhaps slightly more tentative, earnest, encouraging, persuasive. Psi-inhibitory experimenters were rated more rigid, cold, overconfident, tense, irritable, egotistic, unfriendly; somewhat more dull, neurotic, forceful, introverted; and perhaps slightly more passive, nervous, extroverted. For 11 of the 30 adjectives, differences between the two groups were significant ($p < .05$, two-tailed).

For all large differences, the pattern is clear and consistent. Psi-conducers have higher totals for traits that center around being flexible and friendly; psi-inhibitors for traits that center around being rigid and cold.

But similarities between psi-conducive and psi-inhibitory experimenters were also clear. Both groups were rated confident, earnest, likable, active, poised and persuasive (though the psi-conducive were rated higher on each). Both were rated as not playful, overconfident, neurotic, unfriendly, irritable (though psi-inhibitors were rated even lower on playful, and psi-conducers lower on the other traits). For all 30 adjectives, the rank order correlation between the two groups was +. 34.

Individual judges differed markedly, and for most adjectives any experimenter was rated +1 or +2 by at least one judge and -1 or -2 by at least one other. To find if group ratings were stable even though individuals varied, totals for both groups and differences between the totals were computed separately for males and females. The rank order correlation between male and female judgments was +. 85, highly satisfactory for test-retest reliability of personality judgments.

Responses to the question about global ratings were uninteresting. Both groups of experimenters were rated likable and persuasive; almost all experimenters were categorized as psi-conducive.

Judges volunteered dozens of terms to characterize the experimenters. "Domineering" and "authoritative" seem the best candidates for a revised adjective list.

Adjective scores clearly confirm the expectation that G. R. S. stated before the data were collected (though the direct question did not). Psi-conducive experimenters were expected to elicit more adaptive, lively, free responses; and judges responded to viewing them by rating them more flexible, free, active, friendly, relaxed, warm, etc. than psi-inhibitory experimenters. Psi-inhibitory experimenters were expected to make subjects "freeze up"--and they were rated colder than the psi-conductors. They were also expected

to elicit more "downbeat, defensive answers," and confirmation is implied by the judges responding that they were more rigid, tense, irritable, egotistic and unfriendly than the psi-conducive group. Since the correlation between male and female judges was high, the difference between impressions created by the two groups seems stable.

This strong support for the expectations seemed especially meaningful because we felt we had used a rather weak method to test them. For one thing, we had sampled the experimenters' behavior in the role of a professional talking to peers; this might not adequately represent how the same person talks to subjects. For another, we did not utilize expert analysis of gestural patterns, although this might give more specific information than the judgments of an inexperienced group. Further, the ten persons on the videotape did not include some of the outstanding psi-conducive or psi-inhibitory experimenters. And finally, the available psi-inhibitory experimenters all seemed very likable to us (as indeed they seemed likable to the judges).

Since the data are strong in spite of all these points, we infer there are marked differences in body language, facial expression, or other components of normal nonverbal communication between experimenters whose subjects tend to give extra-chance psi data and those whose subjects tend to give chance data.

Many questions remain. Will comparable differences be found: With other experimenters and judges? With impressions from voice patterns taken alone (when the meaning of the speech is concealed)? With experts' analyses, presumably more cognitive and less affect-laden than these? Is there any relation between judges' impressions and experimenters' personalities for any of the traits? Will judges who report deviant personality impressions of an experimenter make similarly deviant psi scores when that experimenter tests them? Can experimenters modify their gestural patterns and thereby change the psi scores they elicit? Topics for further research are numerous.

## PSI PERFORMANCE AND AUTONOMIC NERVOUS SYSTEM ACTIVITY

William Braud† and Donna Masters (Mind Science Foundation)

This paper reviews published evidence for the role of autonomic activity in psi performance, and presents the results of two new experiments in which autonomic activity is directly assessed during the performance of receptive psi tasks.

Several investigators have suggested that there exist two major information processing systems having qualitatively different

operating characteristics and perhaps different mediating brain mechanisms: a primary system subserving selective attention and logical thought, and a secondary system subserving broadened attention and primary process ideation. Evidence indicates that these two systems appear to function optimally at different arousal levels: high arousal for the first system and low arousal for the second. It appears that psi functioning is more closely related to the functioning of the second system and would therefore be expected to be optimized under conditions of low arousal.

A number of psi-favorable conditions or altered internal states have been identified, including meditation, induced relaxation, ganzfeld stimulation, hypnosis, nocturnal dreaming, and the out-of-body experience. What are the autonomic concomitants of these various conditions? Peripheral autonomic recordings made during various meditation exercises have yielded a pattern of findings indicative of reduced autonomic arousal. Progressive muscular relaxation exercises have been shown to reduce sympathetic activation. The autonomic correlates of ganzfeld stimulation have not been studied systematically. The autonomic accompaniments of the hypnotic condition per se are unclear. However, behavioral observations of the typical hypnotized subject (in the absence of alerting suggestions) certainly suggest a condition of lowered arousal. During the out-of-body experience, it is clear that sympathetic arousal does not increase, and there are several indications of a decrease in autonomic activity. Rapid eye movement sleep (nocturnal dreaming) is characterized by increased autonomic variability, and its phasic autonomic reactions are sometimes correlated with eye movements and other phasic events.

In the parapsychological literature, we have been able to find ten relatively direct tests of the relationship between psi performance and sympathetic activation level. In seven of these ten studies, psi scores recorded during conditions of low autonomic arousal were significantly higher than those associated with high autonomic arousal. In the remaining three studies, significant relationships between psi scoring and degree of autonomic arousal were not found. In no study was heightened autonomic arousal found to facilitate psi scoring significantly.

## Experiment 1: Manipulating Autonomic Activity Through Autogenic Exercises

A group of fifteen volunteer subjects listened to and followed tape-recorded autogenic exercises designed to reduce sympathetic arousal. A second, independent, group of fifteen subjects practiced opposite autogenic exercises designed to increase sympathetic arousal. Autonomic arousal was assessed immediately before and immediately after the exercises by means of measurements of basal skin resistance level and frequency and cumulative peak amplitude of spontaneous galvanic skin reactions. Immediately following the second electrodermal activity measurement period, subjects attempted

to gain GESP impressions of a target picture being viewed by an agent (D. M. ) in a distant room. Specifically, electrodermal activity was measured during two-minute recording epochs immediately before and after an eleven-minute autogenic or opposite autogenic exercise period. Following the second recording epoch was a five-minute impression period during which the percipient attempted to become aware of the content of a target slide projected for viewing by the agent. The target slide was randomly selected from a pool of 100 slides which had been organized into 25 sets of four slides each. Basal skin resistance was recorded during one-minute recording periods at the very beginning and the very end of the session. Following the final basal skin resistance recording period, the percipient rank ordered the four slides of an appropriate duplicate set (used to eliminate the possibility of any agent-produced sensory handling cues) from 1 to 4 to indicate their likelihood of being the target; the percipient's ranking was based on his or her own written and drawn impressions. In addition to percipient rankings, two naive judges rated target-protocol correspondences according to the Z-score method described by Stanford (JASPR, 1979, 253-272). Electrodermal activity polygraph tracings were blind-scored, independently, by two assistants.

Independent t tests performed on the change scores for the three electrodermal measurements indicated that autonomic arousal was indeed lower for the subjects following autogenic instructions than for those following opposite autogenic instructions. The autogenic group's percipient ranking psi scores did not differ significantly from chance expectation; the opposite autogenic group's ranking scores evidenced significant psi-hitting ($t = 2.38$, 14 df, $p = .032$, two-tailed). Comparison of the two independent groups' scores yielded $t = 1.93$, 28 df, $p = .064$ (two-tailed). The judges' mean Z-score ratings did not differ from chance expectation, either for the autogenic or for the opposite autogenic groups; nor did the scores of these two groups differ significantly from each other. Since the scores of the two groups did not differ significantly, the scores were combined; a single mean t test comparing all 30 scores with chance expectation indicated significant psi-hitting ($t = 2.41$, 29 df, $p = .023$, two-tailed).

## Experiment 2: Autonomic Correlates of Motoric Clairvoyance

In this experiment, a somewhat less conscious motoric response was substituted for the more conscious imagery and verbal responses of Experiment 1 in order to minimize conscious monitoring of the psi-indicating response and to reduce egocentric striving or effort which might inhibit psi performance. Immediate trial-by-trial feedback was not provided for the same reasons. The psi task was a slightly modified version of the motoric task introduced by Rush (RIP 1978, pp. 88-89) and described in detail by Braud (JASPR, 1980, 297-318). This time, rotation of a movement transducer by the subject resulted in the generation of 100 possible voltage values, corresponding to 100 possible target voltages produced by a determin-

istic random program of a microprocessor. During two 8-1/3-minute clairvoyance periods, 100 targets were randomly generated at a rate of one every five seconds; three seconds following target generation, the computer detected and recorded the voltage being produced by the subject at that instant. Exact correspondence of the two voltages constituted a hit, with $p = 1/100$. Pearson product moment correlation coefficients were also computed for the 100 target-response sets of each clairvoyance period. The two clairvoyance periods were separated by an eight-minute period devoted to progressive muscular relaxation exercises and suggestions for success in the psi task. Spontaneous galvanic skin reactions were recorded continuously throughout each clairvoyance period; basal skin resistance measurements were made before the beginnings of each clairvoyance period. Electrodermal activity polygraph tracings were blind-scored by an assistant.

Analysis of the autonomic data indicated that the predicted increment in basal skin resistance (an index of reduced autonomic arousal) from first to second clairvoyance period did indeed occur ($t = 3.58$, 29 df, $p < .01$, two-tailed); however, galvanic skin response frequency and total amplitude did not change significantly. The number of direct hits did not differ significantly from chance expectation for either the first clairvoyance period, the second clairvoyance period, or both periods combined. Correlations between target and response voltages did not differ significantly from chance expectation for either the first, the second, or the combined clairvoyance periods. Shifts in number of direct hits and in target-response correlation from the first to the second clairvoyance periods were not significant. No significant correlations were found between any of the psi measure shift scores and any of the autonomic shift scores.

## Curvilinear Analyses

Since Otani (JP, 1955, 164-170) had observed a curvilinear relationship between psi score and change in basal skin resistance, a post hoc analysis of the relationship of basal skin resistance level shift and psi score was done for the scores of Experiment 1. It was found that the psi scores of subjects whose basal skin resistance level decreased during the experiment or whose level increased less than 10 K ohms were not significantly different from the psi scores of subjects whose level increased more than 80 K ohms, nor were the psi scores of either of these two groups significantly different from chance expectation. However, the psi scores of subjects whose basal skin resistance levels increments were of 10 K ohms to 80 K ohms in magnitude did significantly exceed chance expectation ($t = 2.57$, 12 df, $p = .02$, two-tailed) and were significantly higher than those of subjects whose basal skin resistance level increment fell outside of that range ($t = 2.76$, 28 df, $p = .01$, two-tailed). On the basis of the non-monotonic relationship observed in Experiment 1, it was predicted that a similar relationship would obtain in Experiment 2. In the latter case, the appropriate psi scores were the tar-

get number-response number correlations for the second clairvoyance run. The relationships found in Experiment 1 were replicated in Experiment 2. The psi scores of subjects whose basal skin resistance level increments fell within the 10 K ohm to 80 K ohm range significantly exceeded chance expectation ($t = 3.55$, 9 df, $p = .003$, one-tailed) and were significantly higher than those of subjects falling outside of that range ($t = 3.00$, 28 df, $p = .003$, one-tailed); psi scores of subjects falling, respectively, above or below that range did not differ significantly from either each other or from chance expectation.

# THE PRESIDENTIAL ADDRESS

Ian Stevenson

## Part 4: Presidential Address[1]

# CAN WE DESCRIBE THE MIND?*

Ian Stevenson

The founders of psychical research, predecessor of parapsychology, had as one of their principal motives the hope of obtaining support through scientific investigation for a view of man different from the materialist one that late-nineteenth century scientists developed rapidly and aggressively. The part of man previously identified as a soul had no place in the new understanding of man's nature that post-Darwinian biologists fashioned, usually with the open alliance of the fledgling branch of science known as psychology. Psychical researchers attended, participated, and met other psychologists on equal terms in the early congresses (now called conventions) of psychologists that were held at the end of the nineteenth century; but they were soon extruded from the company they were keeping when nearly all other psychologists closed ranks around the materialist outlook. Parapsychologists have persisted as a tiny, but fortunately hardy, minority convinced (for the most part) that man has an important component not adequately accounted for by present orthodox knowledge of his physical body, a component that we say has the capacity at times to communicate and obtain information by means other than the known sensory organs of our physical bodies. This belief is, I think, almost all that parapsychologists have in common, apart from their commitment to the scientific method for testing it; in other respects we have different allegiances, or at least different professional boundaries. Our shared belief in a nonphysical element in man gives us, despite our diversity, a unity that the awareness of Jewishness gave to the Jewish people before the founding of the political state of Israel. (We parapsychologists, however, are rather more like the Kurds, a people still without a recognized nation to call our own.)

If I am correct in saying that belief about the incompleteness of the prevailing scientific view of man unites parapsychologists, it is puzzling that we have not done more to describe or even conjecture the qualities of the component of ourselves to whose existence we have dedicated our professional lives. I must immediately add that a small number of philosophers and parapsychologists have written important papers on the nature of mind from the perspective of someone willing to take account of the data of parapsychology; and some

*Delivered August 14, 1980.

of these writers have even boldly speculated about the life we may live after death. My extensive obligations to these authors have deprived me of all claim to originality, except where I have made errors or deviations of which they would not approve.[2] Yet I think it fair to say that the majority of parapsychologists have concerned themselves little with the attributes of the mind that they believe, at least implicitly, may exist independently of the body, although associated with it during our familiar life. This failure may derive from intimidation by neurophysiologists, who, having made great advances in their field, sometimes proclaim that they will soon resolve all residues of difficulty concerning the relationship of mind and brain; and some of them claim to have done this already by asserting, dogmatically, the identity of brain and mind, which, if true, would make the mind-brain problem a pseudo-problem. Yet surrender to orthodoxy at this time seems particularly ill-advised, because several neuroscientists have recently declared bankruptcy, in effect, with regard to the ability of neurophysiology alone to solve the mind-brain problem.

I do not claim that I shall do what others have not accomplished. Instead, I shall leap over the mind-brain problem--deep chasm that it is--and attempt to describe a mind without regard to a brain. (But I shall return to brains later.) I cannot literally describe the mind, but I think I can point to certain properties and capacities that we can ascribe to minds, and perhaps I can obtain your agreement to my identification of them. What follows then are the speculations--not too far removed from data, I hope--of a radical interactionist.

You will naturally wish to know first how I define "mind" and "mental." By "mental" I mean all those processes and contents that I can attend to that are inherently private, that is, those that I and I alone can observe by introspection. (Here I must add the qualification--with regard to this feature of privateness--that it pertains only under some circumstances; a parapsychologist could not deny that minds sometimes invade each other's privacy.) By a "mind" I mean that part of a person in which the processes called "mental" occur. By a living "person," I mean a mind and its associated physical body. (For me the word "personality" has a more restricted definition than the word "person," but this need not detain us here.) By "physical," I mean whatever is not mental. Since I can conceive of minds having a structure, I must also suppose this structure to be of something, that is, of some "substance." This may be some type of material--a mental material, if you like--that our methods have not yet detected. And here I introduce another topic to which I shall return later.

I have already acknowledged--albeit quite inadequately--the debt I owe to philosophers and parapsychologists who have preceded me in considering these questions. Readers will also surely wish to know from what other sources of information I draw the data for my description of the mind. Consistent with the definition of "mental" that I have just given, much of that information comes from my ob-

servations of my own mental contents and processes. I have widened the range of my experiences, compared with that of most persons, by paying more than average attention to my dreams, by some practice of meditation, by five personal excursions with hallucinogenic drugs, and by observations of many other persons to whom I gave such drugs when this was still legal. I have also drawn data for my concepts from spontaneous cases in parapsychology. Spontaneous cases occur to people in abnormal states of mind. This is not to say that they occur to abnormal people in the psychopathological sense, but only that they show properties of minds, and relationships between minds, that are not usually discernible when people are in their normal states.

I must here acknowledge that I have not so far learned much about minds, or the processes of their paranormal communications, from parapsychological experiments. The common factor of successful experiments appears to be emotion, or what I prefer to call the intensity of an experience; but we can observe the effects of strong emotion much better in spontaneous cases than in laboratory experiments. The important events of life that generate strong emotions do not happen in laboratories, or not often. I am referring here to such events as serious illnesses, accidents, other stresses, and--above all--death. Almost any type of spontaneous case, from the faintest telepathic impression to the most tornado-like poltergeist, may tell us something about the mental processes that generate it. I think, however, that we learn more about minds from some types of spontaneous cases than from others. I refer to those in which we have reports of mental activity that seems to be slightly or not at all, associated with a physical body. I have therefore found it particularly helpful to study: apparitions; near-death experiences and other types of out-of-the-body experiences; and cases of the reincarnation type. Among the last group of cases, I have thought especially valuable a few in which the subject of the case had both verified memories of a previous terrestrial life and seeming memories of an existence between the death of the person whose life he claimed to remember and his birth. These memories of a life between death and presumed rebirth rarely contain anything verifiable. This is not surprising, because the events narrated are not always referable to the world of physical objects and living persons. Yet I think that if we accept a subject's claimed memories of the life of a deceased person as authentic and having a paranormal component, we should at least listen respectfully when he says that he also remembers events happening to him after the death of that person and before he was reincarnated, to state the matter as he sees it.

I shall next indicate six properties or capacities of a mind that I identify as important. My arbitrary order of listing does not mean that I consider one of these properties more important than another. I am indeed unable to think of a mind that does not have all the properties I attribute to minds; to subtract any one from the others would reduce and perhaps abolish a mind's "mindness."

First then, minds have images and image-making capacities.

Some images, as I am now using that term,[3] have spatial properties. These images--not all ones--are extended in a space, so that we can describe in spatial terms the relationships of the parts of one image to each other and of one image to another image. The mind must therefore have spatial properties; in short, be extended. This means that minds exist in a space that we can call mental space. All of a person's experiences occur in this mental space.

The physical bodies with which our minds are associated during our terrestrial life also exist in a space. This space, however, cannot be identical with the mental space of which I am speaking, if only because of the disparity between, on the one hand, the size and location of a physical object (in relation to our physical bodies) and, on the other hand, the images that we have of it when we are in its presence, or those that we may have of it later, when we are not. We may look, for example, at a skyscraper of 100 stories from a distance and then approach it and walk around it. We are then moving in a physical space shared with the skyscraper. Yet we do not suppose that our sense-data that derive from the skyscraper or our memories of the skyscraper (if we have any later) correspond in size or location with the skyscraper itself. The skyscraper and the sense-data or memories we have of it must exist in two different spaces. There is nothing illogical in this, because nothing requires us to think that there can be only one space. This raises the question of "where" the mental space that I am postulating exists. My answer is that mental space and physical space may share, to some degree, the same location, just as a sponge and the water it occupies have the same location, although the sponge and water have different properties. I think, however, that minds may have a larger location than physical bodies have, or at least a wider range of influence. Descartes was clearly wrong in saying that minds are unextended, since some aspects of minds, notably visual images, have spatial properties, which--as I have said--means extension. I am suggesting that minds are not only extended, but that they may have a much greater extension in mental space than our physical bodies have in physical space.

Minds have other contents qualitatively different from those occurring in a spatially extended form. These include the familiar sensations of sounds, tastes, and odors. (Some sensations related to our physical bodies, such as touch and pain, may also have spatial properties or may not.) The sources for the usual stimuli of all these experiences can be more or less localized.

Second, minds have feelings or emotions that are nonspatial and nonlocalizable. Every mental state has some accompaniment of feeling, whether pleasurable or unpleasurable. Sometimes the feeling component has such low intensity as to be almost observable, whereas at other times feelings of pleasure or displeasure increase to strong intensity. I emphasize that some emotion or feeling accompanies <u>every</u> image that we experience. When an image has a particularly intense emotion accompanying it, the associated emotion may persist after the image has been replaced by another. I con-

sider this feature of minds of great importance in psychopathology, but it is not relevant to my present theme.

Third, minds have the property of memory, which is the ability to record information on one occasion and use it at another, later time. There appear to be two main types of memory: that of private images of past experiences that can sometimes be recalled at will and sometimes recur spontaneously; and that of learned actions, such as walking, shaking hands, or speaking a language. I call these latter memories "behavioral memories." These behavioral memories often become automatized, so that we can use them more or less when we wish without attention to details of execution.

A person's awareness on different occasions of having the same memories appears to be the single most important factor in his unique identity and also in conveying to him a sense of that identity or reassuring him about it, if he has any doubts. Similarly, the collection of behavioral memories that a person expresses, together with such of his imaged memories as he has communicated to other persons, provide for them indices of his identity by which they distinguish him from someone else. Two persons may look indistinguishably similar in physical appearance, as do some monozygotic twins; and yet they will have different identities, because each has had a separate stream of experiences and hence of imaged memories.

In the foregoing assertions I reject the bodily criterion of identity. I think memories endure longer than physical bodies. They may persist unchanged while bodies increase in size or, in old age, shrink. The attempt to discredit memories as the criterion of identity on the grounds that they have gaps and become transformed does not dissuade me from my adherence to them in this regard. If a senile man has forgotten some of his recent (or earlier) memories, he previously experienced a series of successive states in each of which he had memories of a still earlier state, and this linkage through a series of memories suffices to establish his identity. Other arguments have been advanced against the criterion of memories for identity. Some writers on this subject have, for example, said that because memories are private they are not verifiable. But they are publishable, and for the purposes of showing identity between the states of a person on different occasions we can properly use the claims of this person on these different occasions to have had identical (or resemblant) memories. It has been suggested that in order to examine such claims to remember, one must be able to recognize the person making them at different stages of his life, and that this recognition requires a bodily criterion of identity. It does not. Individual memories of different persons may resemble each other; but the patterns of all the memories--or even of portions of the memories--of different persons do not. No one could possibly have my set of memories, because no one else has occupied all the particular parts of (physical) space where the events remembered in my memories have occurred.[4]

I must next say a few words about the location of memories. Like all properties of the mind, memories seem to have a location in our brains, because we are ordinarily dependent upon the state of our brains for gaining access to our memories. When we become conscious of them, however, we do this in our mental space, not in the brain; and I suggest that a second depot of memories (the first being in the brain) exists in the mind. I find nothing incongruous in suggesting that memories may exist in more than one location; the process may eventually prove to be no more surprising than that by which we make a carbon copy of a typewritten letter. Perhaps, however, the mind alone holds our memories and the brain merely controls our access to them while we are alive. Yet some experimental data show that experiences that may be remembered may leave detectable changes in brains; such observations suggest that brains also have memories.

I am not satisfied that the word "identity" in what I have written above adequately expresses the meaning that I wish to convey under this heading. I certainly do not believe in a never-changing identity of personhood. Neither the mind nor the physical body of an infant are "identical" with the mind or the body of the old man that they become. It would be more precise to say that they have continuity rather than identity; but if anyone asks how we can know that a particular infant has become a particular old man, I reply that the evidence of their memories would influence my judgment in the matter more than would photographs of their physical appearances. [5]

Fourth, minds have purposes, and for the satisfaction of these purposes they usually act with other minds with which they form associations and attachments. Minds also make decisions between two or more choices, whether of belief or action; [6] and they implement their decisions with more or less capacity and persistence. I think it difficult to exaggerate the importance of the attachments that men make. I am not prepared to go so far as Buddhists do, and say that attachments cause all our suffering. Instead, I say that they enter into the processes of both our suffering and our happiness. The strength and quality of our attachments to other persons not only makes our ordinary happiness, or deprives us of it, but it also contributes--I would say necessarily--to the occurrence of paranormal communications.

Fifth, minds can communicate in two ways. In our waking state, they communicate chiefly by the use of physical signals--usually visual and auditory--emanating from a physical body that stimulates the sensory organs of the physical body of another person. Minds may also communicate directly without the physical signals and sensory organs of physical bodies. Such communication perhaps occurs continuously, with only a small portion of the communicated contents reaching awareness.

I believe that at some level all minds are united, just as all the islands and continents on our planet are attached to each other beneath the surface of the oceans. A few persons become aware of

this underlying unity at special moments or even for longer periods. We often refer to such experiences of the unity of all things as mystical, but this need not mean that they are unreal. Skeptics sometimes use the word "mystical" pejoratively in order to dismiss claims of glimpsing a truth that is hidden from persons unfortunate enough not to have such an experience.

The mystic enjoys a sense of general unity with everything else in the universe. Other persons who cannot have such a widely encompassing experience may nevertheless become aware of the underlying unity of minds in a more restricted fashion. I mean here persons who have what we call paranormal perceptions. Many of their experiences seem to be of unity, not with the universe, but with a particular person at a particular time. Typically, paranormal communications--at least those of which we become aware--occur between persons who have shared many intense experiences, such as members of the same family; and they occur especially at times when one concerned person is in distress and another is asleep, somnolent, or otherwise giving diminished attention to the events in his physical vicinity.

If all minds are united and form part of one whole mind, the union is either stronger or rendered more readily observable in certain groupings of persons than in others. I referred above to the occurrence of paranormal communications between members of the same family. Studies of spontaneous cases show that the effective link is not biological but emotional; paranormal communications occur as commonly between husbands and wives as between parents and their children. I think Whately Carington was right in assuming that all minds are one. But his application of the laws of association to paranormal communications omitted the important contribution that the intensity of feelings makes toward the strength of associational bondings; paranormal experiences occur mostly when the ties are strongest and when one person urgently needs another to whom he is already strongly attached.

We who claim that paranormal communications occur often should ask ourselves why they do not occur more often. One answer may be that although we have calamities enough, we do not love each other enough; and another may be that normal means of communication in the West make paranormal communication there less necessary than it was in earlier periods, and than it still is in some parts of the world today, where it may still be the only means of sending information over long distances.

I come finally to the sixth property of mind that I wish to mention. I refer to consciousness or awareness, including self-awareness. An important feature of a mind is that it has many more contents than those to which it can give attention simultaneously. Nearly all its processes and nearly all its contents lie outside its sphere of attention at any one time. Some of the contents that are ordinarily unconscious become more accessible under certain circumstances, such as when we dream, are under the influence of hal-

lucinogenic drugs, or come near death. In these conditions, memories may sometimes flow involuntarily into consciousness. But the mind has the capacity voluntarily to recall at least some images of which it is normally unconscious. This is such an important attribute that we may correctly define consciousness as that part of the mind that is under voluntary control. Voluntary actions, such as the practice of meditation, may influence subconscious levels of the mind, and ultimately behavior, but they do this gradually rather than immediately.

Now I shall turn back in the direction of the mind-brain relationship, not to show how brain and mind relate--I have already denied any capacity to describe this--but to offer some conjectures about the mind that reasonably derive, I believe, from the improbability of explaining it in purely neural terms. Up to the present, neuroanatomists have failed to account for the differences in sensory modalities by detecting differences in the structure or function of neurones in the different regions of the brain that are concerned with sensation. They may in the future find such differences, but this does not now seem likely. (It seems even less likely that neuroscientists will ever tell us, in their vocabulary, what it is like to experience orangeness, to say nothing of a sunset by Turner or a sunset in the sky.) We do, however, experience different sensory modalities, and different qualities within a particular modality (for example, we can discriminate between the sound of a rock falling on a tin roof and that of a chord played on a piano), and these different types of experience probably occur because the various parts of the mind respond differently to stimuli they receive from the brain. The mind must somehow fit the brain, perhaps somewhat in the way a football player's helmet fits his head. The activities of neurones in different parts of the brain may then cause different mental experiences by influencing the different parts of the mind with which they interact. The retina of the eye responds to light waves and passes signals to the visual sensory cortex and its association areas; but the retina does not respond to sound waves. I suppose, similarly, that the part of the mind interacting during life with the visual cortex and its association areas responds to changes in the neurones there, but not to those occurring in neurones elsewhere, such as in the auditory cortex. (Some exceptions apparently occur during the abnormal experiences of synesthesiae, which suggest a kind of reverberation within the mind from one sensory modality to another; I believe that next to parapsychology and the study of dreams, a better understanding of synesthesiae will contribute most to the coming anatomy of the mind.) Brain events, therefore, do not constitute mental events, but are one cause of them, although not the only one. Similarly, on the motor side, mental events may cause brain events and thus initiate and execute the various physical activities of which we are capable.

I conjecture further that the mind has a somewhat variable attachment to the brain, rather as the connection between the engine and wheels of an automobile may vary with changes in the clutch. In the conditions I mentioned earlier--in dreams, when we are under the

influence of hallucinogenic drugs, and as death approaches--the mind is more detached from the brain than it normally is. In these states, it becomes freer of the usual restrictions that its association with a physical body imposes on it during the life that we now have. In these freer conditions we notice a greater tendency for the presentation in consciousness of ordinarily unconscious memories, for synesthesiae to occur, and also for the occurrence of direct (non-sensory) communication between one person and another.

This type of communication--directly from mind to mind--provides perhaps the greatest single obstacle (and there are others) to our ever understanding mental processes exclusively in terms of brain states. It may be misleading and retarding of our progress to describe such communication as "extrasensory." Perhaps the process is not sensory at all. We usually refer to the person receiving the information as a percipient, but the more neutral word "experient" may prove more helpful when we try to describe a process that often suggests knowing more than perception; to be sure, the knower's new knowledge may be cast into the form of a perception that resembles his ordinary ones, but we cannot account for it by any of the physical stimuli that initiate normal perceptions.

Many persons have claimed that they could at certain times perceive their own physical bodies from a position other than that of their physical eyes. It is possible to explain away a substantial number of such claims on the grounds that the person had a hallucination of seeing his own body. This interpretation, however, becomes strained in certain cases, especially those in which the subject not only claimed to see his supine physical body, but also provided evidence of having had some paranormal cognition at the time he thought he was out of his physical body. It also fails to account adequately for cases in which the subject provided a stimulus for other persons to have had simultaneous paranormal experiences in which he figured, such as his being seen as an apparition at a distant place. If cases of this type--few though they be--are accepted as authentic, as I think some of them should be, they may throw light on all processes of perception. It is a feature of such experiences that the subject, when he is out of his body, goes on "seeing" the people around him, such as, for example, a medical staff struggling to revive his body, just as he sees them (although from a different position, that of his eyes) after he returns to his normal state of consciousness.[7] The small number of subjects who claim to remember events that they observed during an intermediate existence between death and presumed rebirth report similar visual observations. In short, persons of both the groups I have just mentioned claim to have had visual experiences (or vision-like experiences) without the use of their physical eyes and other parts of their body's neural equipment. This raises the question of whether all vision is not in one sense eyeless, that is, clairvoyant. (I am not the first person to suggest that all our physical apparatus of vision from the cornea back to the visual cortex may merely canalize most of our visual experiences without being necessary for them.)

I have already said that minds have spatial properties and a structure that one can, in principle, anatomize; and this structure must be of something. I have also emphasized the persistence of our memories during years when our physical bodies change. What persists, I maintain, are the patterns of our imaged and behavioral memories, not the unstable physical body with which they are associated during this life. I believe that our mental patterns--not just of our memories, but of our purposes also--will persist after our deaths. Then, however, we shall have no physical body, so the patterns must also exist in something else. I call this a mental "substance" without being able to define it further, except negatively by saying that it must differ markedly from the known ingredients of our physical bodies. I am aware of the hazards of using a word like "substance," and concerned about the rashness of conjecturing the existence of something I cannot describe.[8] Yet I do not see any way of avoiding such a further step after reaching the point at which I have arrived.

The special conditions--dreams, intoxication with hallucinogenic drugs, and the approach of death--to which I referred above provide, I believe, some preview of our probable condition after death. If I am correct in believing that they show what happens when the mind becomes partly loosened from its physical body, studying them may help us to prepare ourselves for what we shall experience when the separation becomes complete--when we shall see face to face and no longer through a glass darkly. This reasoning emboldens me to offer next some sketches of what life may be like immediately after death.

At death we shall not enter mental space, because, according to my view, we already live there. I should say more precisely that we live now in a part of mental space. After death we shall be able to explore more fully our own area of mental space, but perhaps with results that we shall not always find congenial, as we may expect from our experiences with unpleasant dreams. Memories will become more accessible to consciousness, and so will their accompanying feelings of pleasure and pain. The sudden flooding of consciousness with memories that occurs in some persons who seem about to die, or are afraid they are about to die, may occur to everyone when they die. The great Indian sage Patanjali said that the almost universal human fear of death derives from dim, subconscious memories of an unpleasant review of one's conduct in a previous life; and a dread of having another similar experience, even though the earlier one was not consciously remembered, made men afraid of dying. Perhaps Socrates was hinting at this "life review" when he said that a good man need not fear death.

I said earlier that our mental contents and processes (by the definition I gave) are private and, except for occasional incursions and excursions in paranormal experiences with other persons, they remain unknown to other persons. Since the states that I believe result from partial separation of mind and brain often include paranormal experiences, the communications that we now call paranormal

may be much more frequent after we die; they may even be the only ones we have.

Because communication after death may depend less on words, it may also be less precise. Feelings may spread from one person to another more easily and without censorship. A decline of verbal communication and a corresponding increase in the sharing of feelings may reduce the hypocrisy that language can hide. Perhaps also it will increase the spread of joy that we too often fail to share with each other now.

To be incarnated means that one is restricted in moving from (physical) place to place by the limitations of one's physical body and the physical vehicles that we can devise and afford. It means also that the ranges of the voice and eyes restrict our ability to communicate. Telephone, radio, and television increase the reach of our communications over distance, but do not alter their quality. In the discarnate state, on the other hand, movement may be instantaneous. As one survivor of a near-death experience put it: "I was free in a time dimension of space, wherein 'now' was in some way equivalent to 'here' in the ordinary three-dimensional space of everyday life." In that condition, to think of someone means to be with him instantly, again as in many dreams and paranormal experiences when we are alive.

The discarnate state may have the disadvantage of being one of contemplation with merely passive enjoyment or suffering, as the case may be. We may have less influence on our condition in the discarnate state than we have on it now, little as that often seems. We may undergo the sort of helpless drift that occurs in most of the dreams in our present existence. We may digest the thoughts that we have made during our incarnate lives, but have little power to introduce new ones. Perhaps it is true--as Buddhism teaches--that the discarnate state permits no progress along the evolutionary path that Buddhism conceives we tread. Yet reflecting on our past errors may prepare us for an improved performance in a later incarnation, if we have one.

This concludes my speculations about the nature of the mind. It remains only for me to make a few remarks to justify my offering these rash conjectures to you. I believe in the value of the scientific method, and some readers may wonder why I have not framed my speculations in the form of testable hypotheses. They may even reproach me with leaving science and wandering in the realm of metaphysics. This objection would both mistake my intention and contradict my view of how science proceeds. I think that many advances in science come from an intuition about a truth that precedes the obtaining of evidence for it. Yet I believe also that we should keep speculation only slightly ahead of data. This introduces the subject of the nature of evidence and its variable acceptance by different persons. With the advantage of working in the medical school of a university, I have never been as isolated from scientists in other

disciplines as some other parapsychologists--through no fault of theirs--have been. I may therefore appreciate better than they can that facts established for me are not so for many other parapsychologists, and are so for an even smaller proportion of scientists outside our field. Perhaps those who do not think my remarks adequately supported by sufficiently acknowledged facts may nevertheless accept them as expressions of intuitions in search of facts.

## Notes

1. Thanks are due for the support of the Division of Parapsychology to the James S. McDonnell Foundation, the Bernstein Brothers Foundation, and the John E. Fetzer Foundation; and to Miss Emily F. Williams, who made helpful suggestions for the improvement of this address.

2. I have written this address without giving references either to support my assertions or acknowledge my debts. The latter are, however, so great and so obvious that readers will easily recognize my creditors.

   In order to avoid excessive length I have left the address dense, although I realize that some matters to which I have alluded in a sentence--or even a phrase--deserve paragraphs, and perhaps chapters, for their proper development.

3. For brevity, I am using the word "images" to refer to those aspects of our experience that Hume calls "impressions" (and sometimes "perceptions") and Russell (and most modern philosophers) call "sense-data," which are stimulated by an event outside our minds, and usually outside our physical bodies; and also to those experiences, such as fantasies and dreams (of day and night), for which we can usually identify little or no external stimulus. Although in practice we usually distinguish sense-data from fantasies, they occur in a continuous range of experience that illusions and hallucinations fill in. This justifies me in using the word "images" for such a wide variety of mental ingredients.

4. Careful readers will have noticed that I have adopted "memories," not "memory," as my criterion of identity. The use of the plural emphasizes that a person's uniqueness derives from his having a group of memories that no one else shares. Although other persons may have been present with him at the time of some events he remembers and may therefore have memories of such events that at least resemble his, no other person has all, or anything like all, the memories that he has.

   The tests of recognition that are often given to subjects of reincarnation type cases recognize this principle. The child subject of such a case is asked to pick out from a heterogeneous pile of clothes and other objects those owned by the person whose life he claims to remember. I cannot defend many such tests because of their procedural weaknesses; but the underlying prin-

ciple--that the child, if he remembers anything of a previous life, would have memories that match the expected memories of one other person and no one else--seems sound to me.

5. The contemporaries of the Buddha debated the question of what it is that is reborn, and persons around him seem often to have asked the Buddha to make a statement about it. He always replied with remarks that appear deliberately gnomic, such as that what is reborn is both the same and not the same as what dies. The Buddha appears to have evaded the question at least partly because he considered it otiose and distracting from proper awareness of the changefulness of minds. Indeed, he was so impressed by the constant flux of mental processes that he regarded a physical body as a better criterion of identity than its associated mind. In some passages the Buddha appears to have expressed the view that the problem of the identity (and continuity) between the successive persons in a series of rebirths was ineluctably beyond human understanding.

6. Some readers may find it puzzling that I associate decisions with purposes, especially when I include the decisions that we call rational, that is, those based on logic or evidence. Yet even our most rational decisions, starting with the decision to make rational decisions, derive from our purposes.

7. Some of these experients later claim to have seen much more than their physical bodies and the people around them while they were seemingly dead or nearly so. They sometimes claim to have traveled deeply into mental space and met there deceased relatives and saintly guides. However, I wish here to emphasize only the essential sameness--as it seems to these persons--of their visual experiences when viewed from within the physical body, so to speak, and from without it.

8. In using the word "substance" I feel the reproach of David Hume, who (in the Appendix to A Treatise of Human Nature) wrote: "When we talk of self or substance, we must have an idea annexed to these terms, otherwise they are altogether unintelligible."

Part 5: Invited Address (The J. B. Rhine Lecture)

# POWER CORRUPTS: SKEPTICISM CORRODES*

Brian Inglis

I have to admit that it was with some surprise, as well as with a great deal of pleasure, that I received your invitation to speak here tonight. I am, in a sense, an outsider in this Association. My academic training was not in the sciences; not even in philosophy, with which parapsychology has had such valuable links--chiefly, I suppose because psychology and philosophy counted as "moral science," as a century ago. For some reason academic historians, a species of which I was once briefly and marginally a member, have not featured prominently, if at all, in your field. There have been members of our societies for psychical research who've done valuable historical research into the history of psi, such as Podmore and Dr. Dingwall; as well as a few who, I am sorry to say, have also done valuable research but have prostituted it to the requirements of their prejudices. But academic historians have tended to shy away from the subject, or to treat the evidence with levity.

I had another reason for surprise at the invitation. It was intimated to me that it arose out of what I had said at the SPR's dinner in the 1979 Conference in Edinburgh. I did not think what I had to say then was going to be welcome; nor was it, for some of the diners. But this encourages me to take up one of the themes I touched on then: the corrosive influence within the society of skepticism. And if those who were present then will forgive me, I would like to lead off with the same anecdote, as it happens to be relevant.

Periodically I have an experience which I am sure many of you have, too. The telephone rings and a voice asks whether I am the Brian Inglis who has written or broadcast about the paranormal. When I say yes, the voice continues: "You don't know me, but I know you'll be interested in an experience I have just had...."

The heart sinks, and a drowsy numbness pains the senses. I know what is coming: a precognitive dream often of the winner of a horse race, usually the Grand National. Of course he has only told the girl friend or the boys in the office. He did not have it

*Delivered August 15, 1980.

formally witnessed. Even if he had, I tell him, I do not think it would merit the attention of the Society.

I am very well aware, therefore, that to tell such a tale in company such as this would be shocking. Yet never again, I thought in Edinburgh, would I have such an audience--almost captive, even if only out of politeness. The temptation proved too strong. I, too, I told them, had once dreamed the winner of the Grand National--the Irish Grand National, always run at the agreeably named race course, "Fairyhouse." I, too, had informed the girl friend, and the boys in the office, but had not thought to get them to witness it.

Still, I will not bore you with the dream, apart from one feature of it which relates to what happened afterwards. The name of the horse was wrong. It was the number which was right. I mentioned this a few years later in a column I was writing for an Irish newspaper, and, as a result, had a letter from an Austrian living in a small village in County Carlow. He was a keen amateur hypnotist, he told me, and as his wife had turned out to be a good subject, he put her into a deep trance before that year's Grand National--the English one--to find if she could tell him which horse had won. In her trance, she "saw" what happened as if she had been in one of the stands. But she did not catch the name of the winner. All she could say was that it carried the number ten. Knowing nothing about races, they put money on the tenth horse in the newspaper list, and were disappointed. Having read my article, they were now wondering whether the winner had in fact been number ten. I rang up one of the sporting papers and asked. The winner, Quare Times, had indeed been number ten.

A few years later I happened to be having dinner with some friends just before that year's Grand National, and I told them the story. One of them was a well-known Dublin solicitor; he said he'd like to try hypnosis. Another was a clinical psychologist, and she offered to hypnotize him. It was agreed that the experiment should be conducted on the morning of the race. My friend rang me up in high glee, after it, to say he had "seen" the race, as if from behind. Coming up to the last fence there were only three horses in it, and one was so far ahead, and going so well, that he knew it must win if it did not fall. It did not fall, and surged further ahead up the finishing straight.

What number was it? I asked. That was the trouble. As my friend was behind the leading horses, he couldn't see it. But, he claimed triumphantly, he had seen the colors of the jockey's jersey, and sure enough, there was a jockey who would be riding in the colors. I would have put my shirt on the horse, but for the fact that in the office--by this time I was in London, working for the Spectator--there was, as there always is in such offices, one knowledgeable fellow who pointed out that when an owner has two horses in the race, the jockeys would wear the same colors; the only difference would be in the colors of the cap. There were two jockeys in the colors; and my friend could not recall the colors of the cap. So I

put relatively modest sums on both, and sat down to listen to the running commentary. Some of you may already have guessed what happened. The colors were those of the Queen Mother. Her horse, Devon Loch--ridden by Dick Francis, who has since won fame as an author of best-sellers--performed just as my friend had "seen." It was almost at the winning post, far ahead of the field, when it did the splits, for no accountable reason, and collapsed.

What, you may wonder, has this story got to do with skepticism and its corrosive effects? Well, as a result of my dream I was asked in 1950 to review a book which contains some of the most convincing evidence for the reality of precognition that we have had in our lifetimes. I shall be mildly surprised though, if more than a handful of you have heard of it, let alone read it. It is Tell Me the Next One, by John Godley, who is now Lord Kilbracken.

As an undergraduate at Oxford after the last war, Godley began to dream the winners of horse races; and he had the sense to have them attested. On one occasion he even rang up the Sunday Mirror, and told them of a "double" he had dreamt (when it came off the Daily Mirror offered him a job, which launched him on a successful career in journalism, until he went back a few years ago to look after the family estate). At this stage the story was looked into by an SPR member, and an account appeared in the SPR Journal, admitting that the evidence was "particularly well substantiated." But there was also by this time familiar hedging; although Godley claimed he did not know the horses, the writer suggested, he might have known them subconsciously. And the hedging was followed by ditching; Godley's book, when it appeared later with its complete record of the dream sequence, was not even noticed in the SPR Journal. The book has never, to my mind, had the attention it merits. It is full of interesting little sidelights, some after the manner of Dunne's Experiment with Time, like the names of the horses always being just a little wrong. Even his two failures were of interest in themselves: the first was a horse he knew about and had already backed before he dreamed that it won; the second was the only one on which he stood to win a packet--notoriously a psi-inhibitor.

Why, then, does the sequence not feature more prominently in the Precognition Stakes? The reason, I am sure, is that parapsychologists, absorbed in their search for ways to convince skeptics of the reality of psi phenomena, find that precognitive dreams, other than those dreamt in prescribed laboratory conditions, do not serve their purpose. And dreams of winners of horse races are positively counter-productive, because they are calculated to make the skeptic smile derisively. What has happened is that you have allowed skepticism to influence research policy: almost to dictate it.

Power corrupts; skepticism corrodes.... The tag "power corrupts" is commonly attributed to Acton, who did indeed write: "Power tends to corrupt. Absolute power corrupts absolutely." But I have come across an earlier version, put in quotes, as if it were

a known saying. "It is the dangerous quality of power, in a greater or lesser degree, to corrupt those who possess it," the Irish journalist William Paulet Carey wrote nearly two hundred years ago, adding that this applied regardless of the views of the possessors. My thesis is that "it is the dangerous quality of skepticism to corrode not only those who possess it, but those who are seeking to convert them."

We tend to think of skeptics as the opponents of parapsychology, and of course some of them are. There are the likes of Professor Hansel, who does not even pretend to have an open mind on the subject. As he recently admitted to an interviewer, he has irrevocably rejected psi. At least this is honest, compared to the pretence of objectivity characteristic of most members of the Committee for the Scientific Investigation of Claims of the Paranormal. There are also the naggers: those who accept the possibility of psi, but concentrate their entire energy upon exposing the weaknesses in the investigations of others. I was reminded of them, the other day, when I came across a comment Harold Laski wrote in a letter to Judge Oliver Wendell Holmes: "the academic mind lives profoundly in that state of resentful coma we call research."

But there are also the concealed skeptics: parapsychologists with powerful biases towards skepticism of which they are often unconscious. This was very strong in some of the early members of the SPR, usually taking the form of simply refusing to credit certain types of phenomena: levitation, for example, and even, for a time, clairvoyance. The Sidgwicks, Hodgson and Podmore were all corroded by this type of skepticism. And I have often encountered it among parapsychologists today. For example, a friend of mine asked me the other day whether I had even known X, a member for many years of the Council of the SPR, who had been his next door neighbor. I said I had not known X personally, but he had a very good reputation as a fair-minded, thorough investigator. How strange, my friend said, that a man like X, fascinated by the paranormal and in theory very ready to accept it, had admitted that he had never once, among all the psychics he had investigated, found one whom he had been prepared to accept as genuine. I had not known this. But my friend assured me it was true. X, he thought, had become constitutionally incapable of sticking his neck out for fear that it would be chopped off.

Why? What gets into such people?

The reason only dawned on me when I came across Festinger's theory of "cognitive dissonance." This is commonly defined today as the impulse which prompts people who have to make a difficult decision into justifying it, later, with all manner of queasy rationalizations. But in this psi context, there is an added emotional element. The researcher may want to obtain clear-cut evidence for psi; but he also wants at all costs to avoid being conned. Uri Geller, in particular, was responsible for a positive epidemic of the cognitive dissonances. The symptoms ran in three stages:

first, "I have seen Geller, and I cannot deny it; the fork did bend"; second, "I have seen the Egregious Randi, and he can do it too"; third, with a sigh of heartfelt relief, "Geller is a fraud."

You may remember how Richet put it:

> At the moment when these facts take place, they seem to us certain, and we are willing to proclaim them openly; but when we return to ourselves, when we feel the irresistible influence of our environment, when our friends all laugh at our credulity--then we are almost disarmed, and begin to doubt, May it not all have been an illusion? May I not have been grossly deceived? ... and we end by letting ourselves be persuaded that we have been the victims of a trick.

The really dangerous skeptics are those who believe themselves to be open-minded, but whose ingrained cognitive dissonance disqualifies them--or should--from acting as investigators. And unluckily, this type of skepticism has been powerfully reinforced by "protocolitis."

"Perhaps our most urgent need," Eisenbud wrote recently in this context, "is to try to understand ourselves and (even if this sounds a bit too dramatic) the enemy within. It is conceivable that we have exerted ourselves to follow in the way of a science not just because scientific method, is after all, a successful means of sifting information and error in many areas, but because we are prey to the very anxieties about ourselves and our nature out of which science, as a technique of coping, may in large part have developed." I am sure that when Rhine set out to demonstrate the reality of ESP by methods acceptable to orthodox science, it was right and proper that that experiment should be tried; and it has taught us a great deal about psi. I should add here, how much I personally was indebted to Rhine; first as one of the two people to stimulate my interest in the subject, J. W. Dunne being the other; and later as a benevolent mentor. But the indications now are that the scientific method is offering diminishing returns, and one reason, surely, is that you have allowed the skeptics whom you have challenged to choose not merely the ground and the weapons, but even your seconds--the statisticians.

There are two main reasons why protocolitis is a wasting disease, and they were put by Gaither Pratt eighteen months ago in his paper, "Parapsychology, Science and Paradigm Change." "I do not think that normal science will ever be able to incorporate psi without having to make drastic changes in the currently held view of the universe," he wrote; and he suggested, "we need to stop talking as if good evidence per se is simply passé and dull." I never met Gaither. I wrote him a fan letter about his Parapsychology, and subsequently we corresponded from time to time over the years. I can think of few people for whose writings--whether letters or articles or reviews--I had more respect. They were always of such sound sense, coupled with an innate decency of mind.

Take first his point about science's view of the universe. Parapsychology cannot be fitted into it, Procrustes-fashion; if for no other reason, because of psi experimenter-effect. In retrospect I realize that I had often come across it in the course of my research; but I did not read the report of the crucial Fisk/West experiment until a couple of years ago. Having read it, I rang Rosalind Heywood--always, for me, a source of refreshingly sensible advice (delightful Rosalind, she invariably contrived to leave me with the feeling I had been helping her). Why, I asked her, had not more been made of that remarkable result? Everybody, she said, had been too embarrassed.

There, surely, you have a classic example of the way in which protocolitis enfeebles the system. What would the scientists say? Let's sweep it under the carpet! Even before the Fisk/West experiment, Gardner Murphy commented that the lack of interest in experimenter-effect was "perhaps the most striking example of blind spots among parapsychologists"; it is only in the last few years that--thanks largely to Rhea White--it has achieved a measure of recognition. Yet a great deal of research continues as if it did not exist: I suspect for two reasons.

One was referred to in a very different context by Maynard Keynes in his memoir Dr. Melchior, in the passage where Keynes speculated about why the Allies had not lifted the blockade of Germany after the armistice at the end of the first world war. This was not, he decided, punitive in intent: "the blockade had become by that time a very perfect instrument ... its authors had grown to love it for its own sake; it included some recent improvements, which would be wasted if it came to an end; it was very complicated; and a vast organization had established a vested interest." A curiously accurate representation of the huge research machine, into which parapsychologists have been meshing! Some of you have come to love protocol for its own sake.

The other reason for the protocolitis epidemic is fear of the consequences of breaking orthodoxy's rules. If we accept the implications of psi experimenter-effect, one of the speakers at last year's SPR Conference at Edinburgh argued, parapsychology will be "doomed to be a pretty toy which we can't do anything with." Precisely the reverse! Psi experimenter-effect, if it can be demonstrated, will be no toy. It will hand you that sharp instrument, Occam's razor--for so long used against you, but henceforth a weapon in your own hands, with which the skeptics can be effectively castrated.

If psi experimenter-effect can influence experiments in parapsychology, it presumably can influence scientific experiments of any kind; and my impression is that its influence is far more pervasive than has been recognized. Most of you will be familiar with Rosenthal's experiment on the experimenters; but there is an even more striking example of psi experimenter-effect in research with laboratory animals.

Early in the 1950s Professor Neal Miller of the Rockefeller University in New York decided he would like to see whether rats could learn to control certain autonomic functions; it was an idea so contrary to established orthodoxy that for a while we could find no research assistant willing to work with him. When eventually one joined him, they found that rats could learn to alter their heartbeat, their blood pressure, their visceral responses, even the temperature of their ears, so that one ear would "blush"--become warmer than the other. Grotesquely improbable though this sounded, other researchers obtained the same results, using the same technique; and some of the findings were duly published in 1959.

Four years later I happened to visit Miller, to interview him about his work on the use of biofeedback to control blood pressure, and he showed me an article he had just written as a follow-up to the early experiments. In spite of the fact that the results had "appeared to be robust, and were secured by six different experiments," he had written, "recently it has been impossible to repeat the experiments." Looking back over the series showed a picture of "progressive decline." To him, "the unexplained difference between earlier repeated success and present repeated failure is an extraordinary perplexing dilemma."

Decline effect and unrepeatability, then, are not problems for parapsychology alone. Doubtless Miller would have appeared to be discredited, as parapsychologists have so often been, had it not happened that by the 1960s, ways had been found to enable humans, too, to control heart beat, temperature, blood pressure and the rest, sparing him from being denounced as a dupe or a charlatan. He would probably resent almost as much being called a psychic; but it looks as if he, or somebody who worked with him, must have been.

There are many other examples of meticulously protocoled and controlled trials whose results have contradicted each other so strikingly that psi experimenter-effect must be considered a possibility. Sabin, for example, following his triumph with the polio vaccine, produced what appeared to be conclusive evidence that the herpes virus is a cause of many human cancers. After a while, he had to admit that he could no longer obtain the same results. A few days ago I happened to meet Ted Steele, who has just created a sensation by publishing the results of experiments which appear to show that acquired characteristics--certain forms of immunity--can be inherited. He was awaiting the results of trials by other immunologists, he said, and it would not surprise him if some of them were negative; the same thing, after all, had happened to Temin, who had been in the wilderness for a time, but whose work had eventually been recognized and won him a Nobel prize. Pharmaceutical research, too, has sometimes exhibited decline effect, in trials of new drugs.

After last year's SPR Conference I wrote in the New Scientist that if psi experimenter-effect is established, parapsychologists will "at long last be able to tell orthodox psychologists to set their own

glass house in order, before hurling any more stones." Nevertheless, the abandonment of the scientific model, on the ground that it is unscientific because it does not take account of psi, is obviously going to present you with grave problems. As you know, it is not only experimenter-effect that you will have to worry about. There is also witness-effect. I once saw a man demonstrating, for purposes of a television documentary, that he could swing a compass needle encased in a glass jar, either clockwise or anti-clockwise, by mental action. When the late Chris Evans came into the room, and stood watching, the needle stopped. There is also the possibility of poltergeist-effect. With the most powerful psychics--D. D. Home, Palladino, Geller, Matthew Manning--some of the most striking manifestations have been, as it were, spin-offs; notably in the case of Geller's broadcasts, when spoons bent and broken clocks started up in homes all over Britain. If Arthur Guirdham is to be believed, you will also need to take account of past lives-effect. And, of course, notoriously, there is location-effect: the inability of psychics to perform in laboratories, or away from their own homes.

This brings me to Gaither Pratt's second point: that we must stop thinking of good evidence as in itself "passé and dull." The kind of striking evidence that can occasionally be obtained from, say, spiritualist mediums is despised because they cannot reproduce it to order.

Last winter I was asked to be one of a panel in a TV program featuring the celebrated medium Doris Stokes. If you have never heard of her, I can give you some idea how celebrated she is: recently she filled the Sydney Opera House for three nights running and, when she agreed to appear on television, "Starsky and Hutch" had to be moved to another slot to accommodate her. For the program I was on, Professor Hansel had also been invited, along with another colleague who is even more skeptical (if you can believe it) and Carl Sargent. Doris's performance, which consisted of picking out individual members of the audience and passing them messages from the departed, was not successful. At the end, Hansel and the other skeptic were predictably critical; so was Carl. Still, a couple of times Doris had got it right, but a little off beam. Once, a member of the studio audience, and another time, some viewer calling from home on the telephone, had claimed that the message was clearly from one of their deceased relatives, whom they had recognized from her description.

Of course, this would have been very easy to fake. But as even the skeptics conceded, Doris doesn't look or sound like a faker: and more to the point, anybody who was going to cheat would surely have made it more convincing. Anyway, shortly afterwards I was telephoned by one of the London's glossy monthly magazines to ask whether I would accompany its Features Editor to see Doris, as they wanted a story about her. Now, I feel about visiting a medium much as, I imagine, a conscientious sociologist feels about going to a blue movie; it may be justified as part of his academic work, and it could

turn out to be titillating, but he may not care to be seen going in. Still, I had liked Doris, and anyway I was going for a cup of coffee. I would leave before the seance, if that was what it turned out to be, began.

While we were still having coffee, however, messages began coming through to Doris (she is clairaudient). "I'm getting a name," she said: "Clive." The Features Editor shook her head; she did not know any Clive, let alone one who had recently "passed on." The message, Doris continued, was for Clive's friend, Tracy; but again, the Features Editor could think of nobody she knew of that name. At this point the photographer, whose only contribution had been to indicate whether or not he wanted sugar in his coffee, spoke up. "I feel I have to interrupt," he said. "Clive was my friend. He died at three o'clock this morning. Tracy was his fiancée."

Dramatic though this naturally seemed to the camera man, to the Features Editor and to me, it clearly was commonplace for Doris Stokes. Obviously, investigation of her and other mediums could potentially be a useful exercise. But the great majority of you, I imagine, feel as little enthusiasm for the prospect as I do. And too often, investigations of mediums, where they have been carried out, have been undertaken by members of our societies who regard themselves as part of a fraud squad, whose primary duty to their society and to themselves is to detect deception.

Suppose that you had been investigating Doris Stokes on that occasion: would you not have insisted upon the photographer being excluded from the sitting? In that case, her hit on the target, because it was an Outer rather than a Bullseye, would not have been recorded. Similarly, the experience which first gave me first-hand conviction of Geller's capabilities was when a respectable citizen standing next to me in a group, watching Geller trying to bend a latch-key, suddenly clapped his hand to his trouser pocket and pulled out his own latch-key, which he had actually felt bending on its ring. An accomplice? By no means. I have never seen a man more astonished and, indeed, embarrassed than he was. He did not even, I found later, tell his wife what had happened, until she saw the bent key some days afterward.

I nearly forgot: there is one other effect which parapsychologists need to take into consideration. Arthur Koestler has dubbed it the "ink fish effect." The ink fish, when threatened, escapes under cover of the marine equivalent of a smoke screen. Most of you, probably, have experienced it; some small protocol deficiency--a videotape running out just as something begins to happen; a sound tape "wiped" or even dematerializing. Yet it is just possible that if you concentrated on bringing out the evidence--identifying with psi, even cosseting it, rather than simply trying to monitor it--even this could change. We shall see.

# ERRATUM

## Research in Parapsychology 1980

In the course of some checking on the nature of the random number generator circuitry of the APPLE computer, I discovered that an apparatus repair, made part way through the experiment reported under the title "Are Prepared Random Sequences and Real Time Random Generators Interchangeable?" (pp. 43-45) in this volume, and thought by me and my technical associate to be of no consequence at the time, was quite serious. The experimental comparison was of performance differences between targets drawn from a stored random number table (and thus all existing in present time) versus those generated de novo on each trial. The change resulted in a seed for determining the targets for the next 20 trials of a particular run being generated at the beginning of each run. While this is not quite the same as the targets already having been generated and stored, it is also not the same as de novo generation, as we thought they were. This confuses the design for part of the experiment: the results should be taken lightly until I can locate the date of the change and reanalyze the data. I wanted to withdraw the paper until such reanalysis, but it was already set in unchangeable page proofs.

Charles T. Tart

NAME INDEX

SUBJECT INDEX