HAS SCIENCE DEVELOPED THE COMPETENCE TO
CONFRONT CLAIMS OF THE PARANORMAL?

Charles Honorton

Explaining their decision to publish a preliminary re-
port on "remote perception" experiments performed at Stan-
ford Research Institute in the fall of 1974, the editors of
Nature openly posed the issue whether science has yet de-
veloped the competence to confront claims of the paranormal.
The editors of Nature deserve a great deal of credit for hav-
ing the courage to raise this question, and in order to ex-
plore the issue further, I think it will be helpful to examine
both "establishment" science and "paranormal" science.

Establishment Science

It is appropriate to begin with establishment science,
the scientific orthodoxy, or "mainstream," since this is what
defines and limits the scope of factual knowledge. Through
its publication practices, establishment science controls the
dissemination of research findings. Through the top-level
policies of its executive councils, establishment science con-
trols the disposition of research funds. And through our edu-
cational institutions, establishment science teaches what is
currently known and/or believed about the nature of reality.
Has establishment science developed the competence to con-
front claims of the paranormal?

Before pursuing this question further, I think it is im-
portant to recognize that despite the impressive accomplish-
ments of modern science and its applied technologies, science
has yet to come to grips with some of the fundamental prob-
lems underlying much of what it currently regards as normal.
Among the still unanswered questions of normal science are
these: What is the source of power of the atomic nucleus?

*This is the Presidential Address, given August 21, 1975.
How is biochemistry translated into consciousness? Where is memory? These and similar questions tend to be ignored, but are kept alive by our truly great scientists, men like Eddington, Eccles, Wigner, and Wheeler.

Those of you who are familiar with the history of paranormal research will, I think, agree that establishment science competently confronted claims of the paranormal during the period we now call the "ESP Controversy" of the 1930's. During the five-year period following publication of J. B. Rhine's Extra-Sensory Perception in 1934, the scientific community responded as it should to any claim of new discovery, by disseminating both positive and negative research findings, by careful scrutiny of the experimental and evaluative techniques, and by encouraging fresh replication efforts.

During this period there were approximately 60 critical articles by 40 authors, published primarily in the American psychological literature. Fifty experimental studies were reported during this period, two-thirds of which represented independent replication efforts by other laboratories of the Duke University work. The critical issues raised during this period were, for the most part, legitimate ones, and the experimentalists were quick to modify their procedures to accommodate valid criticism.

By 1940 there was, if not a general consensus on the reality of ESP, at least a general consensus on what constituted a good ESP experiment. Yet despite the adequacy of many of the experimental studies, concede even by the leading critics of the period, and despite the continued accumulation of new experimental confirmations, the active confrontation between establishment science and claims of the paranormal went into hibernation for a decade and a half. Between 1945 and the mid-1950's, very little discussion of paranormal claims appeared outside the parapsychological specialty journals. Virtually no funding was available for research and graduate students who wanted their degrees were strongly discouraged from pursuing parapsychological topics. Aside from occasional textbook and lecture references to obsolete criticisms of the early Duke work, establishment science, during this period, sought to ignore rather than to confront claims of the paranormal.

In the 1950's, the two leading interdisciplinary journals of science published speculative attacks on paranormal research. Nature carried what in itself was a "paranormal" claim, in the form of Spencer Brown's attack on probability

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theory (Brown, 1953). Science gave special prominence to George Price's article on "Science and the Supernatural" (Price, 1955). Price's confrontation with the paranormal began with the assumption that ESP and other paranormal claims are impossible. While conceding the methodological and statistical adequacy of many of the experiments purporting to demonstrate ESP, Price argued that since ESP is impossible, experimental evidence that cannot otherwise be explained away should be regarded instead as evidence of experimenter incompetence or dishonesty. Seventeen years later, in a cryptic "Apology to Rhine and Soal," Price (1972) retracted these allegations.

Our current situation shows definite signs that establishment science is once again attempting to actively confront claims of the paranormal. There are numerous indications of this, but I will discuss just two. While federal grant support for parapsychological research is still virtually nonexistent in comparison with more conventional research on, say, military frisbee design, a beginning has been made in the support of a few projects. There is at least now some inclination on the part of funding agencies to consider individual proposals on the basis of merit, past accomplishment, and the likelihood of continued success.

Aside from providing financial support for new research, the greatest contribution establishment science can make toward the resolution of controversy over new claim, I believe, is to allow the dissemination of research findings, both positive and negative. In this regard the current confrontation between establishment science and claims of the paranormal must be considered to be highly ambivalent.

On the positive side, symposia on parapsychological methods and research findings are becoming a regular feature on the annual programs of a number of scientific societies, including the American Psychological Association and the American Association for the Advancement of Science. The affiliation of the Parapsychological Association with the AAAS in 1969 has provided an important new forum for the dissemination and discussion of parapsychological findings, primarily through symposia at AAAS annual meetings.

Yet while the AAAS encourages us to sponsor symposia at its annual meetings, its journal, Science, continues to suppress the interdisciplinary dissemination of our research findings. Even since our admission into the AAAS, Science has
published only reports of negative findings. Since Science is a highly selective journal, accepting only about 20 percent of the reports submitted to it for consideration, the publication of negative findings indicates that the editors of Science regard the problem-area to be one of some importance. If this is so, why has Science consistently rejected competent experimental reports with positive findings?

There is no more effective means through which to answer Nature’s editorial challenge than to allow interdisciplinarily consideration of both positive and negative findings in this area. This would allow the interdisciplinary scientific community to know just exactly what is being claimed about the paranormal; to assess for itself the degree of competence and the level of development; and to stimulate new independent replication efforts.

Yet despite encouraging indications to the contrary, the current status of the situation is still, I think, subject to the constraints imposed, in a well-known quote from the physicist, Max Planck, by what I shall call “Planck’s Second Constant”: “A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it” (Planck, 1949). For the moment, I think we must conclude that while Nature has developed the competence to confront claims of the paranormal, Science hasn’t.

Paranormal Science

Has parapsychology developed the competence to confront claims of the paranormal? Not as much as we would all like, but much more than anyone has a right to expect given the level of support, the degree of irrational prejudice, and the small number of competent investigators who have been able to sustain themselves in this field. In fact, I suggest to you that some of our paranormal claims have much more support behind them than many of the more widely accepted claims of normal science.

Let us examine the replication status of findings in parapsychology and other areas of behavioral research. Almost all of the discussion of this important topic has occurred without the necessary bookkeeping. First, let us return to the card-guessing experiments of the 1930’s and examine the

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replicability of that work. The central claim upon which ESP was based during this period was formulated as follows: “Is it possible repeatedly to obtain results that are statistically significant when subjects are tested for knowledge of (or reaction to) external stimuli (unknown and uninterpretable to the subject) under conditions that safely exclude the recognized sensory processes?” (Rhine et al., 1940, p. 15).

Even among parapsychologists there is a rather widespread belief that most of the independent replications of the early Duke work were nonconfirmatory and I suspect this may be especially true among those of us who were not around in the 1930’s (which, incidentally, accounts for about three-fourths of the participants at this convention). In fact, I was surprised myself to find that this wasn’t so when I undertook a review of all of the English-language ESP experiments reported during the period between 1934 and 1939 (Honorton, 1975a). The results of this survey, in terms of replication, are shown in Table 1.

<table>
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<th>Duke Group</th>
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<td>All Other Laboratories</td>
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<td>20</td>
<td>61</td>
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<tr>
<td>Total</td>
<td>50</td>
<td>35</td>
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*Chi square (Duke vs. Other x Significant vs. Nonsignificant) = 1.70 (df = 1; nonsignificant).

This work involved a data base of approximately 3.3 million individual trials. As Table 1 indicates, 61 percent of the independent replications of the Duke work were statistically significant. This is 50 times the proportion of significant studies we would expect if the significant results were due to chance error. Of course, there is also experimental error and some of these studies left much to be desired in terms of methodology. Yet on the basis of my own study of this literature, I concluded that at least 33 of these 50 studies
were methodologically adequate on the basis of the experimental reports.

The next question, then, is this: Were all of these laboratories suppressing mountains of nonsignificant data? I think this is very unlikely, judging from the temper of the times, but it is not necessary to rest our case on this. It is legitimate to ask what volume of research data could realistically have been generated during this period. It is important to recall, in considering this question, that in the 1930's there were only two funded research laboratories in parapsychology, one at Duke University and the other at Stanford University. (I believe the Stanford endowment still exists, although the Stanford officials have consistently allowed it to be used for other purposes.) It is also important to bear in mind that the volume of reported work during this period was 3.3 million individual trials and that it takes time to shuffle ESP cards and to record subjects' guesses.

About six months ago my friend and colleague, Edwin May, opened a suitcase which he had carried all around India. Inside was an electronic random number generator. Just to illustrate what a little technology will do for an impoverished research area, this instrument is the third or fourth generation version of an instrument that was introduced in parapsychology only five years ago by Helmut Schmidt. It looked to me more like something one would find at Mission Control in Houston than in a parapsychological research laboratory in deepest Brooklyn. Ed explained the myriad of controls that decorate the front panel. "These knobs," he said, "allow us to ask 'physics-type' questions about PK. This one allows us to adjust the trial rate from one trial every ten seconds to a million trials in ten seconds ... in millisecond increments, of course," he added with a smile.

Eager to play, we decided to run a control series of random checks. Several minutes and millions of trials later, I suddenly realized that we had just collected more trials in those few minutes than had been reported during the entire 50-year period between 1880 and 1939.

Even if we assume a position of extreme conservatism and allow for the possibility that for each published study between 1934 and 1939 there were five studies that were not reported and were nonconfirmatory (that is, 250 unpublished studies with 16.5 million nonsignificant trials), the rate of replication of the ESP hypothesis would still be highly significant.

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Replication in Parapsychology and Other Areas

It has become clear to me that the replication status of parapsychological findings must be viewed within the larger perspective of replication in other areas of research. Last year at Mlmalides we completed a two-year dream research project which was supported by the National Institute of Mental Health (Honorton & Ullman, 1975). This project was designed to replicate and extend our earlier telepathic dream work as well as to replicate certain nonparapsychological findings by another investigator. This investigator, while still analyzing data from a project begun six years earlier, had published several preliminary accounts of findings based on general impressions of his data. His tentative conclusions had gained fairly widespread acceptance, despite the exploratory character of his initial reports. To make a long story short, we not only failed to replicate our own telepathic dream findings, we also failed to replicate three of the four nonparanormal claims of this other investigator.

Recently, a number of us have begun exploring the possible role of mental imagery as a mediator of psi (i.e., paranormal information flow). Six reports have appeared thus far relating ESP performance to one very popular scale which purports to measure the vividness of imagery. While three of these studies showed some relationship between psi and imagery, as defined by this scale, the direction of the relationship has shown a disconcerting tendency to vary from study to study. We might be tempted to ascribe such lack of consistency to that perennial scapegoat, the "elusiveness of psi," were it not for the fact that similar reversals between this scale and a variety of nonparanormal performance measures seem to be more the rule than the exception (Honorton, 1975b).

I could go on, but I know that many of you can provide similar instances of how the findings of more established nonparanormal fields cannot be relied upon. My point is simply this: the problem of replication did not begin and does not end with parapsychology. In fact, parapsychology may actually be in a better position vis-à-vis replication than many other areas of behavioral research.

Let us take a brief look at the publication practices of the American Psychological Association, for example. A number of studies have been reported of APA publication practices. I will summarize just two. Sterling (1959)
sampled 362 research reports published in three of the leading APA journals. He found that while 97 percent of the studies basing conclusions on significance tests reported rejection of the null or specific hypothesis, none of the research reports was of a replication of previously reported findings. Similarly, Bozarth & Roberts (1972) sampled 1414 research reports published in four additional journals. They found that while 94 percent reported findings based on the rejection of the null or specific hypothesis, fewer than one percent were of replications of previous findings. I do not see how we can avoid the conclusion that it is impossible to assess the validity of findings and degree of replication in many areas of psychology.

Now let us return to our own situation and examine the replication status of research findings in contemporary parapsychology. Let us begin by examining what is being reported right here at this meeting and then step back and see how well or poorly some of our findings are holding up relative to similar earlier work.

Two dozen research laboratories are represented on the program of this year’s convention. There are 45 experimental reports with findings based at least in part on significance tests. Rejection of the null or specific hypothesis is reported in 63 percent of these studies and by 17 different laboratories. Moreover, 73 percent of the studies reported at this convention are of attempted replications of previously reported findings. Of these, 43 percent report significant confirmation of earlier findings.

I will now examine in somewhat greater depth two areas that have sustained systematic research efforts over a period of years. My selection is somewhat arbitrary and reflects my own research interests. There are other areas that could serve equally well.

Internal Attention States

The first area involves the use of internally-deployed attention states, brought about by a variety of means, to detect and register paranormal information flow. We can formulate the central claim or finding in this area as follows: psi information flow is more easily detected and recognized when the receiver is in a state of sensory relaxation and is minimally influenced by ordinary perception and proprioception.

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A good case can be made for this claim on analytical grounds alone and the empirical support for it pervades nearly every aspect of our literature. From our studies of cultural practices and spontaneous experiences to our clinical case histories and experimental reports.

Let us first examine some of the analytical considerations. For these we are indebted to the late Cambridge philosopher, C. D. Broad, whose theoretical contributions to parapsychology have not yet received the attention and recognition they clearly deserve. Broad was among the first to suggest that if paranormal phenomena occur at all, they are probably much more pervasive in our everyday lives than has traditionally been recognized. Drawing an analogy with the discovery of magnetic fields, for example, he wrote:

Had it not been for the two very contingent facts that there are lodestones and that the one element, iron, which is strongly susceptible to magnetic influences, is fairly common on earth, the existence of magnetism might have remained unsuspected to this day. Even so, it was regarded as ... [an] anomaly until its connection with electricity was discovered, and we gained the power to produce strong magnetic fields at will. Yet all this, while magnetic fields had existed and had been producing effects wherever and whenever electric currents were passing. Is it not possible that natural mediums might be comparable to lodestones, that paranormal influences are as pervasive as magnetism, and that we fail to recognize this only because our knowledge and control of them are at about the same level as were men’s knowledge and control of magnetism when Gilbert wrote his treatise on the magnet? [Broad, 1935].

Broad pointed out that psi interactions need not involve conscious experience at all, that those that do are merely the tip of an iceberg, and that below the surface, psi interactions may occur as totally unconscious influences on our moods, our dispositions, our thought processes, and so on. As we have already seen at this convention and in the literature over the last several years, experimental support for this far-out notion has begun to emerge through the work of Stanford and his associates (Stanford, 1974; Stanford et al., 1975).

As for the relative rarity of recognized psi interactions (i.e., psi experiences), Broad’s analysis of the conditions necessary for the recognition of paranormal input
makes it clear that these conditions are normally seldom fulfilled. This analysis also makes it clear why psi experiences are so often associated with internal attention states.

Suppose, recasting Broad’s analysis in the communications terminology proposed by Morris (1975), that the output of an information source served as an influence on a sensorially-remote receiver. In order for the receiver influence to be detected and identified with its source, all of the following conditions are necessary and must be fulfilled:

1. The receiver influence must be detected. With human receivers, this means that the influence must take the form of overt behavior or conscious experience which the receiver can and does attend to.

2. The experience must be sufficiently prominent, or carry sufficient impact, to allow the receiver to distinguish it from among the many other, nonparanormal inputs which are concurrently influencing him. In this context normal perceptual, somatic, and cognitive influences on the receiver may constitute sources of noise.

3. The experience must be stored and reported prior to receiver-source contact through normal channels, otherwise it cannot be considered evidential.

4. There must be subsequent confirmation of a meaningful correspondence between the source and the receiver. This correspondence must be sufficiently unusual or consistent over repeated efforts to eliminate chance coincidence as a reasonable explanation.

These detection criteria can account for some of the most prominent features of spontaneous paranormal experiences. The high incidence of spontaneous psi experiences occurring in dreams and other internal attention states would be expected, inasmuch as such states are associated with deafferentation—sensorimotor noise-reduction—and deployment of attention inward, toward mentation processes such as thoughts and images which may serve to carry psi information, thus increasing the likelihood of detection. The utilization of imagery and other forms of mentation in the processing of environmental information has been demonstrated in studies of subliminal stimulation—which, incidentally, is also facilitated by internal attention states (Dixon, 1971).

The high incidence of paranormal experiences occurring between friends and relatives and the low incidence of occurrence between remote acquaintances and strangers would also be expected, since there is naturally a greater likelihood of confirmation in the former case. Unless the source and receiver know each other and are in relatively frequent contact with one another, the likelihood of confirmation is very low. Furthermore, unless the relationship between the two permits some degree of intimacy, it is unlikely that either would be sufficiently uninhibited to share unusual personal experiences.

Similarly, the high incidence of crisis cases involving situations of sudden, unexpected emotional significance would be expected, since such experiences carry more impact and are more likely to be remembered and recalled than relatively trivial mundane experiences.

The association between paranormal information acquisition and internal attention states can be traced back at least as far as the Vedic period of India and the reports of Siddhis or paranormal powers manifest in yogic meditation. There are the claims of the powers of entranced medicine men and the shaman of the non-technological societies; the “higher phenomena of hypnotism” reported by the early Mesmerists; the phenomenological descriptions of cognitive strategies employed by gifted individuals, such as Mary Craig Sinclair; and finally, the cross-cultural validation of dreaming as the state-of-choice in spontaneous psi experiences and psychotherapeutic case reports.

Now let us look at the status of controlled laboratory experimentation in this area. I have been able to find 89 experimental studies spanning a 30-year period and involving controlled investigations of psi retrieval in internal attention states brought about by dreaming, guided imagery and hypnosis, meditation, perceptual isolation, and progressive muscular relaxation (Horton, in press). At least two different laboratories have reported work with each of these five procedures and this work was contributed by a total of 26 different laboratories or investigator-teams. For present purposes, I will examine these studies primarily from the standpoint of replicability. The question is this: How many of the studies with each procedure and overall have reported clear-cut evidence that paranormal processes were operating within the design of the experiment, and how does this number compare with what we would expect purely on the basis of chance error? For the purpose of this analysis, I am defining as “significant” only those studies in which, on the basis of overall psi scores or clearly-stated prediction, the investigators rejected the null or specific hypothesis at the .05 level or lower. These results are summarized in Table 2.
The replication rate with each of these five procedures is statistically highly significant ($P < 10^{-5}$). Overall, of the 88 experiments, 50 reported rejection of the null or specific hypothesis. This is 10 times the number we would expect on the basis of chance error. Significant findings were reported by 17 of the 26 laboratories; that is, by two-thirds of the laboratories involved.

Over 60 percent of these studies involved free-response ESP designs which, as you know, are much more time-consuming than shuffling ESP cards. Nevertheless, let us assume for the sake of extreme conservatism that for each published study there are five that were not reported and were not statistically significant. This would require 445 unreported, nonsignificant studies. I cannot imagine how anyone who is familiar with the amount of time these studies require, and with the level of support and research output of this field, could really believe that there are anywhere near this number of unreported studies; but let us assume that there are. The rate of replication in this area would still be highly significant.

Now let us compare these data with the new work being reported here at this conference. This year there are 11 experimental reports involving psi and internal attention states, contributed by five different laboratories. Six of these experiments report rejection of the null or specific hypothesis. This gives a replication rate of 55 percent compared to the 56 percent rate for the earlier work. These significant confirmations are reported by four of the five reporting laboratories and this year's crop is 10 times greater in proportion than we would expect on the basis of chance error. This harvest would still be significant if we were to posit five nonsignificant and unreported studies for each one reported at this conference.

I cannot take time now to describe some of the more interesting secondary findings related to this area. Elsewhere I have shown that the information rate for psi studies combining free-response designs with internal attention states is several orders of magnitude larger than for forced-choice guessing designs (Honorton, 1975c).

Nor have I discussed the quality of methodology in these studies. These experiments all utilized standard procedures for eliminating sensory contact between source and receiver, for randomizing targets, and so on. It is doubtful...
that any of them would live up to the reputation for super-
control enjoyed, for a time, by Soal’s work with Shackleton
and Stewart. But this is not necessary. Each of these
studies asked a question and provided a tentative yes-no an-
swer which, in turn, led to another study, in-house and by
another laboratory. This, after all, is how normal science
operates, at least in its better moments. The experimental
claim results in another experiment rather than in a post-
humous attack on the integrity of an individual investigator.
Soal’s honesty would not now be the topic of idle speculation
(Mundle, 1973; Scott & Haskell, 1973) if he had had a rep-lic-
able procedure rather than a gifted subject.

The existence of a new or hitherto unrecognized nat-
ural phenomenon cannot rest upon unrepeatable experiments
by isolated investigators working with unique practitioners.
Gifted subjects have much to contribute to our knowledge and
understanding of psi, but so far we have largely wasted their
talents.

Microdynamic Psychokinesis

The second area I have selected for special attention
involves psychokinesis on microphysical systems. The cen-
tral claim or finding in this area may be formulated as fol-
lowing: a human observer provided with sensory feedback to
an external source of randomness produced by a microphysi-
cal process can, by attending to the feedback signal, effect
an influence on that microphysical process with the result of
decreasing its randomness.

Order out of disorder, as a function of intention, per-
haps. The history of PK, even more than that of ESP, is
tainted with fraud, malobservation, and uncontrolled events
occurring in suspicious circumstances. Despite the apparently
good controls employed a century ago by Sir William Crookes
in his investigations of D. D. Home, or the Naples sittings
with Eusapia Palladino, or even the impressive credentials of
certain members of the “Metal Benders’ Guild” in contem-
porary Great Britain, such observations have never carried
great conviction outside the small circle of firsthand observers
present.

Moving from the macroscopic effects associated with poltergeists, hyperactive tables, and compass-needles, into
the arena of controlled experimentation, perhaps the most
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obvious trend has to do with the shrinking size of the PK tar-
get. When J. B. and Louisa Rhine salvaged PK from the
darkness of seance rooms and noisy tables, they introduced
light, dice, and quarter distributions of the half-set.

Yet despite the “overwhelming” evidence for PK pro-
vided by the quarter distribution data, I believe psychokinesis
might have died prematurely in the late 1940’s had it not been
for the work of W. E. Cox. Ed Cox nurtured PK research
for nearly two decades. He replaced the dice with, among
other things, BB’s running down paper straw pathways, and
his research provided some of the first really strong evidence,
not only for the occurrence of PK, but also for the hypothe-
sis that PK is goal-directed (Schmidt, 1974).

Helmut Schmidt has replaced Cox’s BB’s with electrons
and has substituted solid state data paths for Cox’s paper
straws. His electronic random generators show good random-
ness when run in unattended control checks and highly signifi-
cant departures from randomness when human subjects at-
tempts to exert feedback-guided influence. Schmidt has found
in a series of highly ingenious experiments that the internal
complexity of the instrument does not appear to affect the
degree of influence, and he too concludes that PK is goal-
directed (Schmidt, 1974).

Since Schmidt introduced this line of research five
years ago, 16 microdynamic PK experiments have been re-
ported, primarily by Schmidt and his collaborators. The re-
results are summarized in Table 3.

Thirteen of these 16 studies, over 80 percent, yielded
statistically significant outcomes. This is 16 times the num-
ber of significant studies we would expect on the basis of
chance error. However, since data collection is so much
faster in this area, let us assume for the sake of extreme
conservatism that for each of these reported studies there
are 10 which were not reported and which are not statisti-
cally significant. That is, let us posit 160 unreported, non-
confirmatory studies. Even if this were so, the replication
rate for microdynamic PK would still be highly significant.

Now let us compare this with the new work being re-
ported here at this conference. This year there are seven
experimental studies of microdynamic PK, contributed by four
different laboratories. Five of these seven studies report
significant confirmation of microdynamic PK influence and
Table 3. Survey of all microdynamic PK experiments, 1970-
1975.

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Total Significance: chi square = 202, df = 32; P < 10^{-10}

these positive replications are contributed by three of the four reporting laboratories. This is a 71 percent confirmation rate, is 14 times the number we would expect by chance error, and compares nicely to the 80 percent rate for the earlier studies.

Taking Parapsychology Seriously

It is time we take parapsychology and its phenomena seriously, that we stop being defensive with people who are skeptical but are unfamiliar with the serious literature of this field, its better research, the degree of control, and advances in methodology. I believe that it is time for us to go to the journals such as *Science* and to the funding agencies in Washington and demand that they at least examine our serious literature before they reject our papers and our grant proposals. We can present a strong case for our field and it is time that we do so.

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I really wanted to take this occasion to develop some speculative areas, but considering the slender basis of support which is currently sustaining this field, I decided upon the present approach. Most of the participants at this conference are here at great personal expense, do not know whether their research will be supported six months or a year from now (if it is supported now), and cannot publish their research in any journal but those that have a circulation of less than 2500.

We must not continue merely bemoaning the fact that parapsychology is not accepted by establishment science; we ought not to feel that, despite the fact that we know our literature and what we are doing in our laboratories, there must be something wrong with the work since it is not accepted; we should not continue to play the game that eventually, after all, science is objective and our findings will eventually become accepted on their merit. I do not believe this. We have been struggling against irrational prejudice for a long time. Patience goes only so far and I think that if the situation is going to change, we are going to have to change it. Our findings deserve better than they have received from the scientific establishment. If our work is faulty, it should be criticized, but the criticism must be substantive, not a priori. The scientific community has an obligation to assess, without prejudice, the serious research in this area. The only way in which this can be done is through dissemination of research reports to a wide scientific audience. As the *Nature* editors suggested, this would have the effect, not of providing an endorsement of any claims, but rather of stimulating critical discussion and further replication. I can see no basis of justification for the refusal of journals such as *Science* to accept research reports of good quality.

Subliminal Stimulation and Biofeedback

There are some very provocative parallels between two areas of nonparanormal research and the two areas of paranormal research I have focused on in this address. These areas of "normal" research are subliminal perception and biofeedback. I do not have time to explore either of these in great detail, but I would like at least to highlight a few points that I think are of special interest.

I think anyone who is familiar with the internal states work in parapsychology and who has read Norman Dixon's
(1971) careful survey of the subliminal perception literature must be struck by the parallels. Both subliminal and psi influences are facilitated by internal attention states, both are subject to subtle experimenter effects and situational factors, and both involve the transformation and mediation of stimulus influence through ongoing mentation processes. Perhaps the similarity is no more than skin deep, but the point I wish to emphasize now is that we can begin to exploit these similarities to advantage in using subliminal versus psi comparisons. Those of us who have been involved in free-response psi research have not had any real basis of comparison in the terms of how strong our psi mentation-target correspondences are relative to strength and quality of correspondences in a weak sensory setting. A beginning has been made in this direction (Smith, Tremmel, & Honorton, 1976), and to the extent that the same kinds of stimulus distortions and transformations occur in both subliminal and psi tasks, we can have greater confidence in the informational characteristics of our psi findings. In fact, I suggest that this may be a useful prototype for a new methodology for studying psi processes, one that moves us away from a strictly theoretical baseline in assessing the significance of psi results.

The implications of biofeedback for parapsychology have usually been discussed in terms of developing biofeedback shortcuts to psi-conducive states. I agree that this is potentially very important, but I would also suggest that what has been referred to as the goal-directedness of psychokinesis is found also in biofeedback in the form of passive volition. Robert Thouless (1951) described the mind-set associated with his own success in PK dice experiments in the late 1940’s as “effortless intention to succeed.” This description would immediately be recognized by contemporary biofeedback researchers as passive volition. Elmer Green (1973-4) has described passive volition with an analogy to farming. He says, “A farmer (a) desires a crop, (b) plants the seed, (c) allows nature to take its course, and (d) reaps.”

We are puzzled by the fact that a subject can psychokinetically influence a process which he knows nothing about solely on the basis of intention to succeed and the guidance of a peripheral feedback display. Yet this is exactly what occurs in biofeedback. Let us take the example of single motor unit control. The motor unit is the functional unit of striated muscle. It consists of the nerve cell body, located in the spinal cord, its axon, terminal branches, and all the muscle fibers supplied by these branches. A single spinal

motor neuron may supply anywhere from a half-dozen to several hundred muscle fibers. It is now known, chiefly through the work of John Basmajian (1972), that a human observer can learn to isolate and control single motor units using sensory feedback techniques. This despite the fact that most subjects do not even know what cells look like or where they are located. Here is Basmajian’s summary of his results:

... normal human beings can quickly, in a matter of 15 or 20 minutes, be trained to isolate only one motor unit from the population of perhaps 100 or 200 which are within an area of pick up of an electrode pair. They can suppress all of the units, fire single units, they can manipulate those units, turn them on and off easily, they can suppress the one they started with, pick up another one, train it, suppress it, turn to a third and then, on command, they can respond with signals from the unit that you choose for them to respond with [Basmajian, 1989].

I think it is safe to say that ten years ago the voluntary control of a single cell would almost certainly have been regarded, along with psychokinesis, as a paranormal claim. And perhaps it is. Basmajian and a number of other biofeedback researchers have resurrected the concept of volition as a metaforce to account for this type of control. Is it possible that PK has always been closer to us than we’ve realized? This is, for the present, farfetched speculation. We now have the methodology and the beginnings of a technology, however, to begin asking such questions on an empirical basis.

Normalization of the Paranormal

Has science developed the competence to confront claims of the paranormal? I believe the most satisfactory answer to Nature’s question is this: we have begun to develop competent approaches to the paranormal. We have developed at least a few approaches that work well enough to allow us to build upon them. The job will not be done until we have succeeded in eliminating the “para” and have normalized these phenomena. This will require further articulation of the positive attributes and antecedent conditions of psi, as well as the determination of the role of psi in our normal life experience. We are beginning to make progress in this direction, on several fronts, and I have tried to indicate a few of these.
I believe we should give up our "paranormal" terms, even the one that serves to identify this Association. PSI phenomena are as relevant to physics and, I suspect, to neurophysiology, as they are to psychology. We are studying psychophysical interactions, and I would suggest that the term psychophysiology more properly identifies the range of activities we have called parapsychology.

My own inclination, which is provisional and subject to modification as a result of new data, is that we are, along with physicists and neurophysiologists, on the threshold of a new scientific enterprise, that we are beginning to do some serious psychophysical reality-testing, perhaps to erect a bridge connecting the perennial dualities of mind and matter, physics and psychology. We are, I believe, dealing on an empirical basis with what Eddington called "mind-stuff" and what we have found thus far supports the notion that mind is a real force in nature. I suggest that in the years ahead it will be useful for us to reconsider the type of theory proposed in various forms by Frederic Myers, Henri Bergson, and Sir John Eccles, that the brain is a transmitter of mind rather than its generator, and that mind manifests through the brain by psychokinetic influence on neural tissue.

**Observation and Participation in Science**

In closing, I would like to speak briefly about one other area of psi research, the role of the experimenter. Experimenter effects represent a sort of skeleton in our collective closet. This is a topic we discuss not infrequently in private, but seldom in public. We are concerned with the possibility that ourselves, as experimenters, may to some extent be the source of the psi influences we observe in our laboratories.

Of course there are different kinds of experimenter effects: from experimenter fraud on one end of the scale (as we learned last summer) to experimenter psi on the other end of the scale. I think it is interesting to note that many of the successful experimenters in psi research have also been successful subjects. Both J. B. Rhine and Robert Thouless were able to demonstrate PK effects with dice. The French pioneer in the cognitive study of psi, René Warcollier, was successful as a percipient in telepathy-oriented drawing experiments, and so on. Among our currently active experimenters, William Braud was one of the most successful receivers in the Braud's work on psi and progressive relaxation. Rex

Science Confronts the Paranormal

Stanford was successful in an EEG/telepathy-oriented study with Ian Stevenson. Robert Morris showed that it is possible to gain nonrandom entry points in tables of random numbers using a complex psi procedure. (Morris's studies on cognitively complex psi provide further evidence for the goal-directedness of psi.)

My own accomplishments as a subject mainly involve a series of PK experiments that I reported several years ago (Honorton & Barksdale, 1972). Helmut Schmidt lent me a manual random number generator and since I was teaching at the time, I decided to do a group experiment with my students. I formulated the hypothesis, based on Ted Serios, Kulagina, and other macro PKers, that unlike ESP, PK may require a high level of arousal and activation. I had my students alternate PK runs in which they tensed and relaxed. The results were very nice: the muscle tension runs were associated with significant psi-hitting. The overall results were significant and this was due to the contribution of the muscle tension condition. I returned to the laboratory very pleased with myself for having confirmed my hypothesis. I then set Warren Barksdale to the task of replicating this experiment with 20 individual subjects, following the same procedure for relaxation and tension. His experiment yielded only chance results. We then decided to run a third experiment in which I would be the solo subject and Warren would be the experimenter. I tensed and relaxed, and we alternated which of the two display lights was target for an equal number of runs, and so on. The muscle tension runs were about 3.5 standard deviations above chance and the relaxation runs were about 3.5 standard deviations below chance. Again, my hypothesis had been confirmed. The question that now arose in my mind, however, was, Who were, or was, the subject in that first successful experiment?

Recently, my very good friend John Stump really threw me a curve. When my study dealing with another type of experimenter effect, involving the demeanor of the experimenter as an influence on subjects' psi performance, appeared (Honorton, Ramsey, & Cabibbo, 1975), John examined the control data. In this study, there were two samples of "control random checks." These were run by me, manually depressing the random generator response buttons and attempting not to exert a psi influence on the instrument. Sample A was collected before the experimental data and Sample B was collected after the experimental data. Taken separately, each sample provided good randomness. What John Stump found, however,
was that looking at the difference between the two samples, there was a significant difference for each of the four analyses. Since this was a two-choice generator, we looked for sequence effects: how many times did "red" follow "red"; how many times did "red" follow "green"; "green" follow "green"; "green" follow "red"? In each of these four cases, a nonsignificant positive or negative deviation in Sample A was followed by a nonsignificant deviation in the opposite direction in Sample B. The overall result of this was associated with a probability of 10^{-5}. The experimental hypothesis itself was only confirmed at the .001 level.

John Stump went a step further. He used this post hoc analysis as a pilot study. His confirmation consisted of going back to an earlier study which I had reported with Malcolm Bessent as subject (Honorton, 1971). Again there were pre- and postexperimental random checks; and again the randomness was good within each sample, but with deviations in opposite directions, so that the difference between pre- and posttest deviations was again significant (P = .002).

It appears that John Stump has confirmed Jule Eisenbud's hypothesis about what he has colorfully termed the "individual mind-prints" of the experimenter. I frankly do not know what to do about this problem and I am sure that most of you don't either. But we had better start giving serious consideration to these experimenter psi effects and, to show at least that we are not alone, I will close now with a comment from the distinguished Princeton physicist, John Archibald Wheeler, which I think is pertinent to this:

Insular as we've learned to understand the quantum principle, it's the small tip of an iceberg that tells us that the momentum and the position of an electron are not qualities that exist independently of us, but depend upon our consciously making a decision to measure the position and the momentum in order to bring these features into evidence. I think that through our own act of consciously choosing and posing questions about the universe we bring about in some measure what we see taking place before us. Therefore, I think the word 'observing' is inadequate. A better word is 'participation.' We are going to come to appreciate that the universe itself in some strange way depends upon our being here for its properties [Wheeler, 1973, p. 32].


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