

Zetetic scholar

An Independent Scientific Review of Claims of Anomalies and the Paranormal

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3 BIG DIALOGUES



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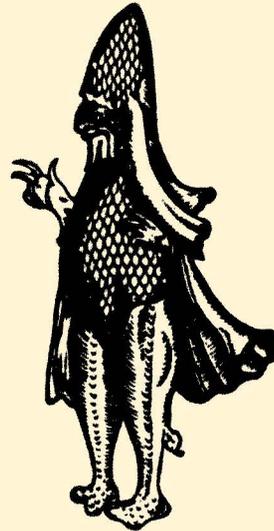
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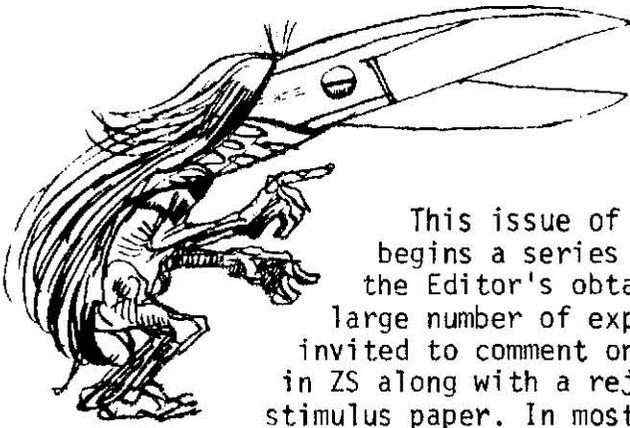
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CHRISTOPHER EVANS

We were greatly saddened to learn of the death of Dr. Christopher Evans in England on October 10, 1979. Chris Evans was an extraordinary person and will be deeply missed. In addition to his many contributions to psychology, especially dream research, he had a continuing and fair-minded approach to claims of the parnormal. He was a strong supporter of the goals of ZS (to which he was a Consulting Editor) and an astute and friendly critic of psi research, towards which he was a true skeptic, doubting but very much open to evidence and dialogue. His death leaves a vacuum in the hearts of many of us, and his kindnesses and intellectual contributions will be long remembered.

JAMES WEBB

As this issue of ZS goes to press, we have just been informed of the tragic and untimely death of James Webb. A further statement will appear in the next issue of ZS following our receipt of further information.



EDITORIAL

This issue of ZETETIC SCHOLAR is our largest so far and begins a series of ZS Dialogues. These are initiated by the Editor's obtaining a stimulating paper that is sent to a large number of experts on the area discussed. These experts are invited to comment on the paper, and these comments will be published in ZS along with a rejoinder to these comments from the author of the stimulus paper. In most cases, only about one-half of the persons invited to comment chose to do so (for a wide variety of reasons). And many experts are probably not invited at this first stage of the Dialogue. The invitations are extended by the Editor in conjunction with recommendations made by the author of the stimulus paper and from suggestions of the commentators who are invited. But this is merely the first round of the Dialogue. It is our hope that all ZS readers will then become involved in a continuing exchange of ideas and information. Hopefully, some of the experts invited to participate in the first round but who declined will choose to enter into later rounds. And, of course, commentators may also re-enter the Dialogue and remark on the papers of the other commentators. In short, everyone is invited to participate, and I hope this results in a fruitful and helpful parade of ideas and data.

The term "Dialogue" is carefully chosen to suggest a cooperative and courteous discussion among peers interested in shedding light on the issues. Unlike a debate, where someone is considered to be the winner and someone else the loser, a true dialogue benefits everyone concerned with a common interest in getting to the truth. Note, too, that I refer to the initial paper as a "stimulus" rather than a position paper. This first paper is not intended to represent a target for everyone to criticize but as an initiating set of conjectures that will launch a serious discussion of the topic involved.

In addition to these specific ZS Dialogues, all papers and information in ZS is open to your comments and rejoinders. I hope that ZS will become a major forum for the exchange of ideas that will encourage inquiry. That's basically what ZS is all about.

True zeteticism represents the suspension of judgement and the presence of doubt with the need for continuous inquiry. Thus, ZS represents true skepticism rather a posture of dogmatic denial. Our commitment is to the scientific method as the means of inquiry, and this does not necessitate any a priori assumptions about what is and is not impossible. As scientists we remain conservative and place the burden of proof on the claimant, but we recognize that science is ultimately descriptive and not prescriptive. This scientific middle-of-the-road posture is not a popular one. It is easy to obtain support for or against ideas; it is far harder to rally those interested in suspending judgements. As such, ZS remains a journal unlikely to ever get a large set of supporters. But we are after quality and not quantity in our readership, and I am pleased to say that so far that is being accomplished. ZS is being taken more seriously every issue, particularly by those scholars we wish to attract to our pages. Lets face it, most people interested in reading about the paranormal would not appreciate things like the bibliographies and other features of this journal that are essential if the level of discourse is to be raised. If you believe in ZS and what it is trying to do, please give us your support. ZS still runs at a deficit and that keeps our future problematic.

LETTERS

Science has established a good reputation for credibility largely by its use of outside reviewers. While on-one is truly and always objective, our referees come as close as is humanly possible. We do not load the dice either for or against an item.

It should be obvious that a clear-cut, unarguable demonstration of parapsychology would be a tremendous development. To be the publisher of such material would constitute a great coup. We would have much to gain. However, we have not received manuscripts that contained reproducible or statistically significant data. We await such material.

In my experience of more than 17 years, few authors have asked for special privilege with respect to reviewing. The major and striking exceptions have been the parapsychologists.

--Philip H. Abelson
Editor, Science

The above letter was Dr. Abelson's response to the allegation of a bias against publishing parapsychological research by advocates of psi while occasionally publishing anti-psi articles in Science, a question raised in the "Prologue" to the Kelly-Diaconis exchange on psi research in ZS #5.-- MT

Velikovsky suggests that around 1500 BC and 700 BC the Earth suffered near-collisions in which its rotation was affected and its axis was shifted.

If Velikovsky is right then the alignments of megalithic (3500-1500 BC) standing stones should show evidence of such changes. But they do not. They remain exactly (typically within 0.05 degree) on targets set by an undisturbed orbital geometry. [Books on megalithic alignments tend to be either specialized, superficial, sensational, or (in view of rapid progress) out of date. An excellent critical review is J.E.Wood's Sun, Moon and Standing Stones, Oxford University Press 1978.]

Of all the tests of Velikovsky's thesis this is the most direct, sensitive, and clear-cut. The thesis is not supported.

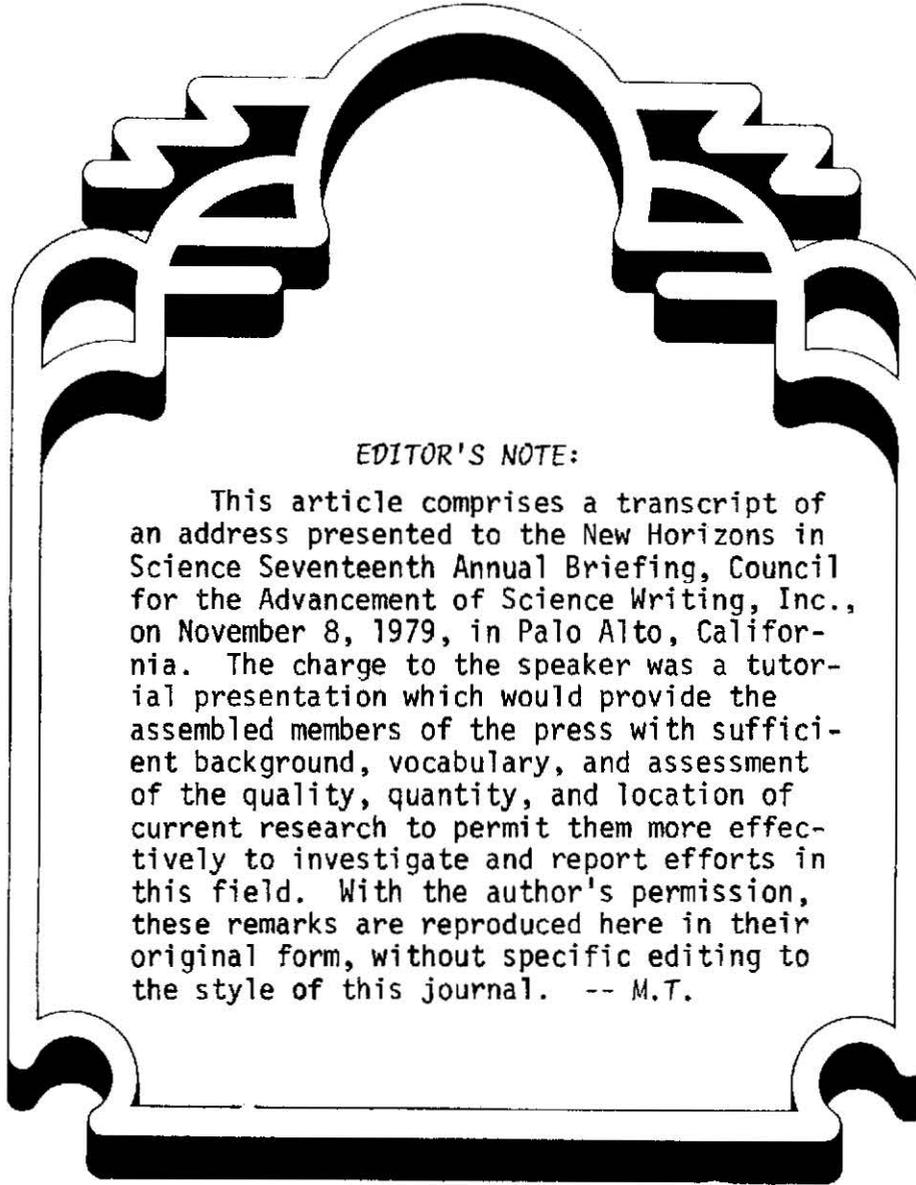
How does Dr. May explain this?

--Geoffrey Dean

Dr. May's reply will appear in the next issue of ZS. -- MT

PSYCHIC RESEARCH: NEW DIMENSIONS OR OLD DELUSIONS ?

ROBERT G. JAHN



EDITOR'S NOTE:

This article comprises a transcript of an address presented to the New Horizons in Science Seventeenth Annual Briefing, Council for the Advancement of Science Writing, Inc., on November 8, 1979, in Palo Alto, California. The charge to the speaker was a tutorial presentation which would provide the assembled members of the press with sufficient background, vocabulary, and assessment of the quality, quantity, and location of current research to permit them more effectively to investigate and report efforts in this field. With the author's permission, these remarks are reproduced here in their original form, without specific editing to the style of this journal. -- M.T.

INTRODUCTION

No field of scholarly endeavor has proven more frustrating, nor has been more abused and misunderstood, than the study of psychic phenomena. Dealing as it does as much with impressionistic and aesthetic evidence as with analytical substance, and carrying by its nature strongly personal and numenistic overtones, it has been incessantly prostituted by charlatans, lunatics, and sensationalists, categorically rejected by most of the scientific establishment, and widely misunderstood by the public at large. The purpose of this presentation is to review some of the history, nomenclature, and contemporary serious effort in this area; to discuss whether, once the overburden of illegitimate activity and irresponsible criticism is removed, there is sufficient residue of valid evidence to justify continued research; and, if so, to suggest how this research might best be styled, facilitated and evaluated.

At the risk of some immodesty, it is probably worthwhile to set my remarks in the context of a bit of the personal history which leads me to this task. My formal training is that of an engineer and applied physicist, and the bulk of my research has concerned a sequence of topics in the broad domain of the aerospace sciences: fluid mechanics, ionized gases, plasmadynamics, and most recently, electric propulsion. My appointments have been primarily in the academic sector, at Lehigh, Caltech, and Princeton, where for the past eight and one-half years I have been Dean of the School of Engineering and Applied Science. This school, which currently enrolls 850 of the university's 4400 undergraduates, features a substantial amount of independent work in its curriculum, and it was in that context three years ago that I was requested by one of our very best students to supervise a project in psychic phenomena. More specifically, this young lady proposed to bring her talents and background in electrical engineering and computer science to bear on a study of controlled, low-level psychokinesis. Although I had no previous experience, professional or personal, with such topics, for a variety of pedagogical reasons I agreed, and together we mapped a tentative scholarly path, involving a literature search, visits to appropriate laboratories and professional meetings, and the design, construction, and operation of simple experiments. My initial oversight role in this project led to a degree of personal involvement in it, and that to a growing intellectual bemusement, to the extent that by the time this student graduated, I was persuaded that this was a legitimate field for a high technologist to study. It is in that spirit I have continued to consider the problem and in that tone I speak to you today.

To proceed with the effort, I obtained the appropriate approval from my university, assembled a small staff, and secured the requisite funding from a few private sources. I should emphasize that my fractional involvement with this program is quite minor in comparison to my other responsibilities, and that the work is still very preliminary and tentative, but it provides the base of cognizance for my broader remarks on the field.

I confess that I shall make these remarks with some trepidation, borne of previous unpleasant experiences. For example, an earlier lighthearted article in the Princeton alumni magazine,¹ in which I attempted to share some of our experiences in this field with the university community at large, brought an intensity and breadth of reactions for which I was totally unprepared, ranging from irresponsible and categorical condemnation on one extreme, to equally irrational messianic accolades on the other. Rather than precipitating further such distracting outbursts, I have largely avoided opportunities for public presentation, a guideline I am setting aside on this occasion only because I believe that this audience can have an unusually significant effect on the development of the field by the manner in which it chooses to represent it to its respective constituencies. It is my request that you treat this as a tutorial presentation, hopefully contributing to your cognizance of, and attitude toward psychic research, rather than as any claim of specific individual achievement therein.*

*A request now extended also to the readership of the Zetetic Scholar--R.G. Jahn.

HISTORY

To get on with the matter in terms of a brief historical background, I would remind you that whereas human interest in psychic phenomena is at least as old as recorded history, clearly displayed in the cave drawings of ancient man, in varied activities of the early Greek, Roman, and Oriental civilizations, in the Bible, and in medieval and renaissance art and literature, systematic scholarly search for understanding of these phenomena is just one century old. It was in 1882 that the Society for Psychical Research was founded in London, providing the first professional forum for presentation of controlled experiments in telepathy and clairvoyance. The American counterpart, ASPR, was formed three years later, with William James as one of its leaders. The most familiar and substantial academic effort in this country was initiated at Duke University in the 1930's, when J.B. Rhine and Louisa Rhine established a parapsychology laboratory and began publication of the Journal of Parapsychology. A second professional organization called the Parapsychological Association was formed in this country in 1957, and in 1969 was recognized as an affiliate by the American Association for the Advancement of Science.

Over this first 100 years of scholarly effort, the field has attracted a significant number of eminent scholars from established fields. The SPR alone has numbered among its presidents three Nobel laureates, ten Fellows of the Royal Society, one Prime Minister, and a substantial list of physicists and philosophers, including Henry Sidgwick, William James, Frederic W.H. Myers, Lord Rayleigh, Edmund Gurney, Sir William Crookes, Sir William Barrett, Henri Bergson, Gardner Murphy, and G.N. M. Tyrell.

At the present time, there are seven English language publications covering this field,² supplemented by numerous less formal magazines and countless books of widely varying quality and relevance. Research activity is reported from over twenty U.S. universities and colleges and many foreign institutions, but in most cases it is of very small scale. There are very few credible academic programs of study, although some fifty M.A. and Ph.D. theses have been accepted on psychic topics at reputable universities over the past forty years. Some ten research institutes and private corporations in the United States have also authorized publications and reports in the field. The extent of Eastern Bloc effort and of classified research in this country are matters of considerable speculation on which I cannot comment with authority.

In many respects the present status and growth pattern of this field resemble those of the natural sciences in their earliest days, or perhaps even more those of the incubation of classical psychology, in terms of the absence of replicable basic experiments and useful theoretical models, the low level of financial support and internal professional coordination, and the low credibility in the academic establishment and public sectors.

NOMENCLATURE

For purposes of constructing a concise catalogue, I shall

define psychic phenomena to include all processes of information and/or energy exchange which involve animate consciousness in a manner not presently explicable in terms of known science. By psychic research, I shall imply any scholarly study of such phenomena employing scientific methodology, as opposed to any dogmatic, ritualistic, or theological approaches. With this definition, the field may be roughly divided into two major categories: (1) extrasensory perception (ESP), which includes such information transfer processes as telepathy (perception of another's thoughts or emotions), clairvoyance (perception of hidden objects or events), precognition (perception of future events), and various animal ESP indications (homing, trailing, group consciousness, etc.); and (2) psychokinesis, which subsumes a variety of effects wherein energy is transferred to a physical system, either in controlled or spontaneous effort, and over a wide range of energy levels. (A few types of psychic process, such as survival/reincarnation, psychic healing, out-of-body experiences, etc., while not fitting these categories quite so neatly, actually involve aspects of both at a more fundamental level.) Note that in this subdivision, the field conforms to the two major thrusts of present-day science and high technology, i.e., the extraction, processing, transmission, and storage of information, and of energy.

STATUS

In my opinion, a comprehensive, objective survey of the scholarly research into psychic phenomena over the past one hundred years would support the following conclusion: some of the results are suggestive, some even provocative; none is fully convincing in the traditional scientific sense. Obviously, more extreme opinions have been voiced on either side of this position. On the one side, there are zealots who claim the case has been made, by their own experiments or others; I cannot claim that for my own work, or for any other I have seen. On the other side, there are critics who reject the field categorically for an assortment of reasons, including instances of outright fraud; naiveté of method, such as sensory cueing of subjects, application of improper statistics, or other theoretical incompetence; failure of replicability, evasiveness of the phenomena under close scrutiny, and sensitivity of results to the observer, all of which violate the scientific method; and the vague conviction that since nothing totally convincing has been demonstrated during this century of study, the domain must be fundamentally invalid.

In my view, all of these criticisms are justified to some extent, but in some cases they have been overworked. This field, by the nature of its phenomena and its inherent numenistic overtones is immensely vulnerable to fraudulent exploitation and naive gullibility, and such have indeed occurred to a distressing degree. Yet, it seems shortsighted and irresponsible to tar all sincere and scholarly work with this brush. To my knowledge, there have been no totally replicable experiments yet performed; what yield there has been has been anecdotal, or at best, statistical. It should be noted, however, that numerous areas of modern science percolate contentedly on lower statistical yield than is offered by some of

the better psychic studies. The sensitivity of psychic experiments to the particular observer could indeed be indicative of fraud or delusion; but it could also be an important clue to the role of human consciousness in such processes. The frustrating evasiveness of the phenomena to more precise and refined experimental techniques is perhaps the most damning criticism from a scientific standpoint; yet even this may offer a legitimate indication of a basic characteristic of the processes: e.g., just as great art, great music, or great creative thought in general may be stifled or sterilized by excessive analysis or constraint, so psychic interactions may be intrinsically casual and free-flowing, rather than deliberate. Finally, the complaint that enough effort has already been squandered on this psychic goose-chase should be moderated by the recent availability of far more precise instrumentation, and far more powerful theoretical tools and data handling methods than have heretofore been deployed.

On balance, then, categorical rejection seems to me equally as untenable as blind acceptance. Rather, I prefer an attitude which first strips away the illegitimate and sloppy work, and then submits the remainder to close individual scrutiny. This done, one finds a few--to be sure, a very few--experiments which provide sufficiently provocative anecdotal evidence to justify further serious and systematic study. From my engineering point of view, I am most interested in three such categories of study: (1) the so-called "remote perception" work of a number of laboratories, notably SRI,³ Mundelein College,⁴ and our own, which has provided an adequately large data base, of sufficiently high yield, to allow quantification and parametric correlations of the inexplicable results; (2) certain experiments in controlled, low-level psychokinesis, such as performed by physicists at the Mind Science Foundation in San Antonio,⁵ at Birkbeck College, University of London,⁶ and in our laboratory, among others, which have the advantage of focusing on quantifiable physical systems, wherein the departures from classical behavior can be made more explicit; and (3) the rare and spectacular poltergeist events, or so-called recurrent spontaneous psychokinesis (RSPK), which in the magnitude of their effects, and in the demonstrated correlations with neurologically extraordinary adolescents,⁷ seem to offer rich, yet largely unutilized opportunities for insight.

This list is not exhaustive; other commentators might prefer the extensive series of "ganzfeld" or sensory deprivation studies of free response clairvoyance and telepathy as pioneered at Maimonides Hospital⁸ and replicated at other laboratories⁹; the systematic and conservative reincarnation studies at the University of Virginia¹⁰; the modern psycho-physiological studies at Duke University¹¹; the personality correlates originally studied at CUNY¹²; or a few other deliberate programs elsewhere.¹³

NEW EXPERIMENTS

Since even the best extant research has been tediously slow to yield convincing results, if a new round of experiments is to be considered it seems important to reexamine ab initio, the criteria,

topic selection, and philosophical attitude that should prevail. For example, given the preceding pattern of incomplete satisfaction of the normal requisites for scientific credibility, what is the healthiest attitude toward data collection and assessment in this field? Should one reject out-of-hand all results that do not rigidly conform to the normal tenets of replicability and insensitivity to an observer? Should one waive those requirements and attempt to theorize and deduce solely on the basis of anecdotal phenomenology? Or is there a useful intermediate position which, while retaining full rigor in the experimental design, controls, protocol, and analysis, and still striving for some degree of reproducibility in the observations, tolerates imperfect replicability as possibly indicative of as yet unidentified parameters, or of an intrinsically statistical nature of the phenomena, or even of a basic flaw or incompleteness in the established physical models?

Whatever the attitude, it seems clear at this primitive stage of understanding that the specific experiments selected should be as clearly posed and conceptually simple as possible, with a minimum of reasonable alternative interpretations of any positive results. In addition, they should lend themselves logistically to rigorous, tightly controlled experimentation, and demonstrate sufficient positive yield to permit accumulation of a significant data base and its subsequent correlation with variable experimental conditions. Given these attributes, it would also seem best to focus on those studies which seem to have the greatest significance in terms of basic understanding of the phenomenology, contradiction of established scientific models, and ultimate practical application.

In all of the zeal for scientific rigor, there may be some risk of over-sterilizing the experimentation. If the phenomena derive to any significant degree from conscious or subconscious processes of the human mind, it is important that such not be ignored or inhibited in the design and operation of the experiments. More specifically, it is probably essential to include the insights, interpretations, and intuitions of those who have dealt most effectively in such processes, including not only the academics and professional observers, but most particularly those who have demonstrated creative capabilities in the generation of the phenomena. It is quite possible that the difference between a sterile experiment and an effective one of equal rigor lies in the more aesthetic aspects of its ambience and feedback than in the elegance of its instrumentation, and the former need to be well-tuned to the human subjects who are asked to function as components of the experimental system.

On the basis of this sort of logic, our own program at Princeton has selected two classes of experiments for its principal foci.¹ The major portion of the program revolves about a number of table-top experiments in controlled, low-level psychokinesis, using relatively simple physical systems--mechanical, optical, thermal, electrical, atomic, etc.--each of which involves a specific element or process that is vulnerable to disturbance, and which signifies such disturbance by a relatively large change in some feedback display for the subject. So, for example, our interferometer can indicate a disturbance of one of its optical plates of less than one-millionth of a

centimeter by a perceptible change in an attractive pattern of luminous concentric circular fringes displayed before the subject. Using this display much like a biofeedback indicator, that subject can then experiment with his own conscious or subconscious strategies for achieving the desired disturbance of the system. Other experiments monitor the deviations in temperature of thermistors to one-thousandth of a degree with a progressive pattern of small colored lights; the variations in electrical noise from a solid-state diode interface with an illuminated digital display; or the development of the statistical deflections of 10,000 marbles cascading through an array of obstructing pins by direct visual and photographic observation.

It is far too early to claim any definitive results from these experiments. On numerous occasions we have seen effects that to the best of our understanding and control of the prevailing conditions are classically inexplicable, but these effects vary from experiment to experiment, from subject to subject, and alas, from day to day. Whether substantial increase in our data base will sort any of this out remains to be seen.

The minor portion of our program addresses certain aspects of the "remote perception" problem mentioned earlier. In this type of study, the subject attempts to perceive aspects of a randomly selected target scene in which a colleague in the experiment, termed the agent, is immersed at a given time. Typically the percipient records his impressions of the target on tape, by sketch, or by notes, which then are rank-ordered against a pool of alternative targets by independent judges. Our particular effort has been to replace the necessarily subjective human judging process which is inherently vulnerable to personal biases and interpretations, by a more analytical method for evaluation of the degree of information transfer in such perception efforts. Briefly, the strategy involves coding of both the targets and perceptions in terms of some thirty binary descriptors, e.g. outside/inside; dark/light; noisy/quiet; motion/none; water/none; etc., and allowing a variety of scoring and ranking algorithms to derive quantitative indices of the quality and quantity of the information transfer.

Again it is too early to make firm claims, but this method clearly holds promise of reducing the ambiguity in interpretation of this class of experiment, which has shown some of the highest yields of any controlled psychic studies. It should be added that our particular experiments have dealt primarily with the precognitive mode of remote perception, wherein the percipient completes his report substantially before the target site is selected. We find the yield to be at least as high as for similar experiments performed in "real time," and we prefer this mode for two reasons. First, the logical impossibility of the task is heightened, forcing the subjects to abandon various unproductive strategies for the perception and rely more completely on "paranormal" mental process. Second, the contradiction with established physical conception of space/time is more stark, hence potentially more generally illuminating.

THEORY

Next to the evasiveness of the phenomena under controlled experimentation, the second greatest frustration in the study of psychic processes has been the absence of viable theoretical models with which to begin the traditional dialogue between theory and experiment on which all scientific progress eventually depends. Early hypotheses tended to presume wavelike progradation of psychic effects, usually in the electromagnetic modes, but their logic was vague, and a number of experimental results have raised serious doubts that such models are tenable. Only quite recently, with the attention of a significant number of theoretical physicists drawn anew to this task, have broader and more elegant representations been attempted which hold higher hope of accommodating the diverse and bizarre phenomena characteristic of this field.

Attempts at theoretical explication can proceed from any of several levels of presumption as to the fundamentality of the effects observed. For example, one may presume that:

a) the effects observed are illusory (e.g. artifacts of poor experimentation);

b) the effects can be assigned to known physical processes, associated with, but not deliberately precipitated by the subject (e.g. heat transfer, vibration, aerochemistry, etc.);

c) the effects are precipitated by the subject, but involve only known physical and physiological processes (e.g. electromagnetic radiation from brain circuitry or intercardial potentials);

d) the effects are precipitated by the subject, but can be accommodated within existing physical formalism only after identification of heretofore unrecognized modes of energy/information transfer;

e) the effects cannot be handled within existing physical formalism (e.g. the fundamental laws need further generalization, perhaps similar to the evolution of classical mechanics into quantum mechanics, or into special and general relativity);

f) the effects, although observable under controlled conditions, cannot be handled within a scientific paradigm (e.g. they are intrinsically irreplicable).

Obviously the subtlety of the model required, and the breadth of its significance increase markedly as one proceeds through this hierarchy of possibilities.

Extant theories of psychic phenomena tend to cluster into four or five broad groups which can only be described here in the most superficial terms. As mentioned, the first serious models dealt in terms of electromagnetic waves, usually in the very low frequency bands,¹⁴ some versions of which proposed modulation of the ambient environment. Other types of geophysical waves have

since been considered, such as geoseismic waves, infrasonic waves, and barometric fluctuations,¹⁵ but all of these now seem to be fundamentally inadequate to deal with certain classes of psychic phenomena, most particularly precognition.

More recent efforts have involved applications of the formalisms of various other categories of physical mechanics, for example:

a) statistical mechanics and statistical thermodynamics, whereby the subtle interplay of the thermodynamic concept of entropy with information theory is allowed to take on a broader implication in terms of the role of human consciousness in influencing random processes¹⁶;

b) hyperspace theories, whereby the basic laws of physics are recast and re-solved in more than the four coordinates of normal experience (3 space, 1 time), and the consequent new terms are applied to the representation of paranormal effects¹⁷;

c) quantum mechanics, whose inherently probabilistic approach lends itself to representation of phenomena that depart significantly from strictly deterministic sequences of cause and effect, and wherein the interaction of the observer with the observed physical system is explicitly acknowledged¹⁸⁻²⁰;

d) holographic inversions, whereby all of reality is regarded as deployed in an infinite syllabus of amplitude/frequency information, and the brain is hypothesized to function as a Fourier transform device to provide the familiar space/time localized imagery.²¹

Although none of these has yet produced anything approaching a comprehensive theoretical model, each probably has something to contribute to our conceptualization of the phenomenological processes addressed. For example, their application in various forms to the anecdotal experimental evidence assembled to date leads me to speculate that the following rather unconventional possibilities may be worthy of more detailed examination:

1) The phenomena may be inherently statistical, rather than directly causal, and we may be observing them "on the margin," i.e. the observed phenomena may represent marginal changes from normal behavior, on a very grand scale, and with fluctuation times which tax human observational capability.

2) Human consciousness may have an information-ordering capability that can be projected into an external system as well as received from it.

3) Quantum mechanics may be more than a system of physical mechanics; it may be a fundamental statement about human consciousness and perception processes, and the empirical pillars of this formalism, such as the uncertainty principle, the exclusion principle, the indistinguishability principle, and the wave/particle dualities may be more laws of consciousness than laws of nature.

4) Psychic processes may be inherently holistic, and thus the ultimate model may need to integrate both the scientific and the aesthetic aspects in order to identify the sources of the phenomena. That is, psychic processes may be manifestations of the intersection of the analytical scientific world with the creative aesthetic, and thus, to represent them effectively, it may be necessary to balance insights of both perspectives, without sacrificing the integrity of either.

Clearly, any of these intuitions would have to be developed in far more philosophical and analytical detail before a trenchant theoretical model could emerge, but at this primitive stage it is probably healthy to consider a few such radical possibilities, along with more prosaic explications.

CONCLUSION

Let us close by returning to the basic questions which undeniably underlie our attention to this topic. Namely, are psychic phenomena real; and, if so, should they be studied? The latter question begs a sequence of others, i.e. can such phenomena be studied systematically; would the knowledge derived from such study be significant; would that knowledge be useful; etc.? The honest response to all of these is that at present we simply do not know. The jury is still out--or more accurately, it has not even left yet; adequate evidence has not yet been presented to it. At this phase, therefore, everyone should be entitled to his own informal and considered opinions on such questions, and should be equally entitled to the tolerance of others toward those opinions.

But to the much broader, and indeed even more significant question: "do we have the right to inquire?," as a scientist and an academic I must make much firmer response, and if need be, defense. The fundamental requisites of scientific methodology: dispassionate rigor; humility in the face of observations; limitation on extrapolation of results; and openness of mind apply to any sincere scholarly endeavor, including psychic research. When these criteria are met, the results should be heard openly and fairly.

And these criteria are equally appropriate to the process of criticism. When they are honored, that criticism can play a healthy and constructive role. But when the criticism lacks any of these; when it is tainted by categorical rejection, guilt by association, or sloppy logic, it should be at least as suspect as the object of its attack.

I have spoken to you thus not as an advocate of psychic phenomena as valid science, but as an advocate of the right--indeed the obligation--of science to inquire into this field, with the same diligence, patience, integrity, openness, and tolerance--in both its study and in its criticism--that have characterized its noblest achievements of the past.

As recorders, transmitters, and interpreters of such activities for the public benefit, you have the same right, and--if you will permit me--the same obligation, to maintain that same high tone.

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SCIENTISTS AND ANOMALOUS PHENOMENA:
PRELIMINARY RESULTS OF A SURVEY



J. RICHARD GREENWELL AND JAMES E. KING

Introduction

In the summer of 1978, we surveyed 300 professional scientists on two types of anomalous phenomena, Bigfoot and the Loch Ness Monster (hereinafter referred to as Nessie). Three kinds of scientists were involved: physical anthropologists, marine biologists, and physical chemists. All held Ph.D. degrees or the equivalent, and were professionally employed at universities, research institutions or federal agencies. Half of each group was sent a Bigfoot questionnaire; the other half of each group was sent a Nessie questionnaire. Thus, we can compare responses from both physical anthropologists and marine biologists on either Bigfoot or Nessie. The physical chemists served as a control group, since they were presumably not professionally concerned one way or the other by the question of the existence of either of these creatures. As it turned out, our choice of physical chemists for this control group was a good one.

The purpose of this article is not to present the statistically significant results from the survey. Statistical analyses are still underway, and results will be presented at some future time (King and Greenwell, in preparation). Rather, we wish here to discuss some of the more general results, and, in particular, the reactions and critical comments on the part of many of the respondents, some of which were striking and unexpected.

Some respondents limited themselves to criticizing what they believed to be the widespread public acceptance of Bigfoot and Nessie (although a 1978 Gallup Poll found that only 13% of the American public believe in the existence of these creatures, far less than the belief in ESP [51%], precognition [37%], or astrology [29%]). Others criticized any studies whatsoever related to these topics, and some questioned the very purpose of the survey we were conducting, our motivations, and even the design of our questionnaire (this even though the exact purpose of the study was not known to them). Some respondents did not seem able to distinguish between the study of Bigfoot or Nessie as possible biological animals, which certainly falls within their areas of speciality, and the study of attitudes toward the possible existence of such creatures, which is a legitimate pursuit within the framework of social psychology.

Justification for Study

As our motivations and purposes were sometimes questioned, we shall first discuss the reasoning behind, and justification for, our survey. In looking at the history of scientific progress, one finds more often than not heated controversy between the proponents of new theoretical frameworks (or even slight shifts in existing theoretical frameworks), and those who ardently maintain that the existing frameworks are correct. Much has been written on these topics, and we do not intend to review this literature here. Suffice it to say that what many have called "establishment science" has been constantly battered by the adherents of new theories, laws, or effects, and most often science has ignored or rejected these claims. As the overwhelming majority of the claims have been ill-founded for one reason or another, one could state that this has been a proper approach by the scientific establishment. One may also wonder, however, to what extent the attitude of scientists (in rejecting such claims) is related to psychological causes, such as social conformity, rather than to a critical examination and assessment of the data themselves.

If social factors are indeed involved, the implications for the future of scientific progress are important, and the study of this interesting social phenomenon falls within the province of psychology. We thus decided to attempt to measure such attitudes on two continuing and controversial areas, the question of the existence of Bigfoot and Nessie.

Method

The names and addresses of 300 professional scientists were used in the survey. One hundred physical anthropologists were identified from the Fifth International Directory of Anthropologists, published in 1975 by the University of Chicago Press. The Directory contains addresses and biographies on all Associates of the professional journal Current Anthropology. Care was taken to select only physical anthropologists with Ph.D. degrees, and affiliated with academic or research institutions in the U.S. and Canada. One hundred physical chemists were identified from the 1977 edition of the American Chemical Society's Directory of Graduate Research. The Directory contains descriptions of all major chemistry departments in the U.S. and Canada, including faculty biographies. Likewise, care was taken to select physical chemists with Ph.D. degrees, and affiliated with U.S. or Canadian institutions. One hundred biological limnologists and oceanographers (for simplicity here referred to as marine biologists) were identified from the 1976 Membership Directory of the American Society of Limnology and Oceanography. The Directory contains the addresses and specialities of all the Society's members. Again, care was taken to select only biological (rather than physical or chemical) limnologists and oceanographers with Ph.D. degrees, and affiliated with U.S. or Canadian academic or research institutions.

A Bigfoot questionnaire and a Nessie questionnaire were designed and mailed to the target individuals with a cover letter. The two questionnaires were very similar in format and the types of questions asked. The cover letter, from the Department of Psychology at The University of Arizona, stated that their views were specifically being sought as part of a larger, national study of controversial topics on the fringes of science (in order not to arouse suspicion as to real purpose of the study, we also mentioned acupuncture, ESP, and UFOs).

Half of each group of physical anthropologists, physical chemists, and marine biologists was sent a Bigfoot questionnaire (50 x 3 = 150); the other half of each group was sent a Nessie questionnaire (50 x 3 = 150). A stamped, self-addressed envelope was enclosed with each questionnaire. All the questionnaires were mailed on June 6, 1978. Sixty-four percent of the returned questionnaires were mailed back within two weeks of their estimated receipt. Almost 79% were mailed back within four weeks.

Preliminary Results

Questionnaires were mailed to 300 scientists; 181 responded, representing a response rate of 60%. Of these, two respondents merely sent satirical questionnaires of their own (which purported to solicit further information on the nature of our survey), and one wrote a letter but refused to complete a questionnaire. Thus, 178 questionnaires were returned, representing a (usable) response rate of 59%. (A few envelopes had been returned by the Postal Service as undeliverable; we replaced these with new mailings to new target individuals in order to ensure that exactly 300 questionnaires were received.)

Of the returned questionnaires, 53% were on Bigfoot (physical anthropologists 22%, physical chemists 13%, marine biologists 18%), and 47% were on Nessie (physical anthropologists 17%, physical chemists 13%, marine biologists 17%). Overall, then, the highest response rate was from physical anthropologists on Bigfoot. It is interesting to note that the control chemists responded equally on both topics.

Our first finding, which was not altogether unexpected, was that acceptance of Bigfoot (as a living species "still unknown to science") among all three groups was far lower than the acceptance of Nessie (as a living species "still unknown to science"), 10.6% and 31% respectively ($\chi^2 = 9.85$, $df = 1$, $P < .005$). Physical anthropologists and marine biologists accept Bigfoot at an equal rate, 12.8% and 12.5% respectively, while only 4.3% of the physical chemists do so; 23.3% of the physical anthropologists and 30.4% of the physical chemists accept Nessie, while 38.7% of the marine biologists do so.

Among all three groups, 40.4% believe that ordinary animals, such as bears, are involved in Bigfoot reports, and 34.5% believe that Nessie reports involve such misidentifications. However, 69.1% believe that Bigfoot reports involve hoaxes, imagination, and myths

(physical anthropologists 74.4%, marine biologists 78.1%), while only 47.6% believe the same for Nessie reports (physical anthropologists 56.7%, marine biologists 38.7%) ($\chi^2 = 8.09$, $df = 1$, $P < .005$).

For those who reject Bigfoot and Nessie as real biological creatures (89.4% and 69% respectively), it is interesting to learn of their reasons for doing so. They cite the lack of fossil evidence (51% for Bigfoot, but only 6% for Nessie), the lack of specimens, or parts thereof (83% for Bigfoot, but only 54% for Nessie), the lack of bones (70.2% for Bigfoot, but only 35.7% for Nessie), too large a size (4.3% for Bigfoot, 1.2% for Nessie), the lack of nutritional resources in the environments where they are reported (10.6% for Bigfoot, and 17.9% for Nessie) the unlikelihood of remaining so long "undetected by science" (42.6% for Bigfoot, 32.1% for Nessie), or that their existence was simply "too bizarre to consider" (2.1% for Bigfoot, 2.4% for Nessie). Fewer physical anthropologists than marine biologists (35.9% versus 46.9%) accept the rationalization that Bigfoot could not have remained so long "undetected by science," and, conversely, fewer marine biologists than physical anthropologists (25.8% versus 40%) accept the same rationalization for Nessie.

One of the most interesting results from our survey is the different perceptions of the impact that the discovery of such animals would have "on science." Only 3.3% of the physical anthropologists believe that the discovery of Nessie would have a "severe" impact, 36.7% believing that it would have a "moderate" impact, and 60% believing it would have only a "slight" impact. When it comes to Bigfoot, however, the reverse effect occurs: 51.3% of the physical anthropologists believe that its discovery would have a "severe" impact, 30.8% believe it would have a "moderate" impact, and only 7.7% believe it would have a "slight" impact. This consensus among physical anthropologists on Bigfoot is not shared by their scientific colleagues in physical chemistry and marine biology. Only 13% of the physical chemists believe Bigfoot's discovery would have a "severe" impact on science-60.9% a "moderate" impact, and 17.4% a "slight" impact. Among the marine biologists, 21.9% believe it would have a "severe" impact, 53.1% a "moderate" impact, and, again, 21.9% a "slight" impact.

Despite the relatively low proportion of scientists who accept the existence of Bigfoot or Nessie, the majority would, nevertheless, support research in these areas. Among all three groups, 56.4% favor Bigfoot research (physical anthropologists 61.5%, physical chemists 42.4%, marine biologists 64.5%). At the same time, however, most feel very strongly that such research should not involve federal funds: 51% are opposed to federal funding for Bigfoot (physical anthropologists 61.5%, physical chemists 39.1%, marine biologists 56.3%), versus 29% who do favor federal support (physical anthropologists 35.8%, physical chemists 30.4%, marine biologists 35%), and 20% who are uncertain. An even greater majority of 63.1% is against federal funding for Nessie research (physical anthropologists 63.4%, physical chemists 60.9%, marine biologists 64.5%) versus 25% who do favor such federal support

(physical anthropologists 30%, physical chemists 21.7%, marine biologists 22.6%) and 11.9% who are uncertain. Perhaps the greater likelihood of success in finding Nessie is offset by the fact that U.S. tax dollars would be spent on solving a "foreign" problem at a time when scientists have been finding it increasingly difficult to fund projects of local or national relevance.

Among physical anthropologists, 59% claim to have read scientific literature on Bigfoot (another 5.1% remember seeing but not reading scientific literature), and 77.4% of marine biologists claim to have read scientific literature on Nessie (another 9.7% remember seeing but not reading scientific literature). Also, 33.3% of physical anthropologists have actually read physical anthropologist John Napier's 1973 book on Bigfoot;¹ another 46.2% are aware of the book, but have not read it. Among marine biologists, only 9.7% have read biologist Roy Mackel's 1976 book on Nessie,² and only another 16.1% are aware of it. That is, 74.2% are unaware of Mackel's book. We also find that 30.8% of physical anthropologists claim to have met a Bigfoot witness, whereas only 12.9% of marine biologists claim to have met a Nessie witness.

Data on age groups, academic or professional ranks, and sex have also been obtained, but must await further analyses. Self-identification by the respondents was optional in both questionnaires; respondents who left the personal data box blank were therefore also providing some form of data. We find that 71.8% of the physical anthropologists identified themselves when responding on Nessie. Likewise, 51.6% of the marine biologists identified themselves when responding on Nessie, but only 37.5% did so when responding on Bigfoot. Our control group of physical chemists identified themselves equally (26.1%) on both.

Finally, the number of comments of criticisms is of interest, as are the comments themselves. In the Bigfoot category, 48.8% of physical anthropologists accepted our invitation to comment (although some comments began on page 1, presumably before they had read our invitation on page 3!), 30.8% being "informative" comments, 10.3% being "abusive" comments, and 7.7% being both. (By subjective analysis, we categorized as "abusive" those comments critical of our survey, our motivations or intentions, or our questionnaire design. If a questionnaire contained several abusive or several informative comments, which was often the case, we counted them as a single comment for each questionnaire.) Among the marine biologists, only 25% made comments, 21.9% being informative and 3.1% being both informative and abusive (no responses were abusive only).

¹ Bigfoot: The Yeti and Sasquatch in Myth and Reality.
New York: E.P. Dutton.

² The Monsters of Loch Ness. Chicago: Swallow Press.

In the Nessie category, 45.1% of marine biologists made comments, 38.7% being informative, 3.2% being abusive, and 3.2% being both. Among the physical anthropologists, only 16.6% made comments, 10% being informative, 3.3% being abusive, and 3.3% being both. In both the Bigfoot and Nessie instances, 8.6% of the physical chemists provided comments, and they were divided equally among the informative and abusive kind.

Respondents' Comments

There was nothing in our questionnaire or cover letter that hinted at anything but a sincere interest in obtaining their views on a controversial topic. Why, then, were we subjected to abusive comments? We can report, in this regard, that an informal Bigfoot survey conducted in 1974 by Joel Hurd among 500 anthropologists, biologists, and environmentalists, failed to elicit any abusive comments whatsoever (Hurd, King, and Greenwell, in preparation). Could it be that a questionnaire elicits more abusive comments than a personal letter? Or perhaps the fact that our survey was connected with an academic institution (unlike Hurd's) provided a license for criticism. Whatever the reasons, and in spite of the fact that we at no time indicated any belief in, or acceptance of, either Bigfoot or Nessie, there was a pervasive assumption that, because we were conducting a survey on them, we must necessarily be convinced of their existence. This assumption was not limited to the "abusive" respondents. One physical anthropologist, who clearly accepted the reality of Bigfoot, seemed delighted by our survey, and commented: "Bully for our side!"

Most of the comments make very interesting reading, and selected sets are reproduced below. Although 43% of all the respondents voluntarily identified themselves, we are keeping all identities confidential. We can state, however, that some of the respondents are leading authorities in their fields of speciality.

SELECTED COMMENTS ON BIGFOOT

Physical Anthropologists

(1) There is an absence of physical evidence to support the existence of this hypothetical creature, and quite significant theoretical basis for doubting its existence. I can't take all the time gratis to go into all the details now...but think that if you're really serious you might want to expend some of your own resources on an effort to explore the reasons why a number of open-minded scientists with experience in the field doubt that this is a fruitful subject for investigation.

(2) There are some supposed observations which are obvious confusions; there is some purposeful fakery. But you cannot deny the hundreds of observations by reliable individuals who have nothing to gain by making such observations public, and you cannot deny the miles of footprints -- often found in places where no one would be expected to make such footprints.

(3) Although I feel this research would probably be a waste of time, I would never presume to say that anyone "should not" do it -- everyone should have the freedom to make an ass of himself, if that is what he wants.

(4) I am doing it (Bigfoot research). I am a member of the National Academy of Sciences.

(5) Until and unless there is more tangible evidence... it would be absurd for a sensible scientist to undertake or for federal funds to be devoted to such research. It does make some sense to investigate why so many people believe things without acceptable evidence, and I suppose that is what you are up to...

(6) We lose 2/3 aircraft per year in the mountains here (Seattle). If we cannot find a large static object in forests, why is it not possible to have difficulty finding a moving, smaller object (especially if these are rare and attempting to avoid contact)?

(7) I am inclined to be skeptical about Bigfoot, but feel inclined to accept the existence of...ESP, UFOs the Loch Ness Monster...

(8) I...consider it quite possible that an unknown-to-science large hominid may be living in Asia (but probably not in America).

(9) Severe problem (in Bigfoot research) would be publicity. Would pose major logistic problems. Also, there are ethical problems. If sufficient safeguards can be worked out, the research should be done.

(10) The amateurs are doing too good a job to have this particular endeavor befuddled with federal controls. There should be some fun left for the truly imaginative, who have carried the investigation ball and deserve the rewards.

(11) In that a lack of something can't be proved in the strict logical sense, I tell my students that it is far wiser to claim to believe (in Bigfoot) because if they don't exist, who's to prove you wrong?

(12) There is some doubt in my mind whether this questionnaire will yield thoroughly valid and useful results.

(13) What are you trying to accomplish with this questionnaire?

(14) This is not a well-constructed questionnaire.

(15) This is a poorly-designed questionnaire.

(16) Leading, loaded questions...

(17) Bully for our side!

Physical Chemists

(1) Most of the time the orthodox scientists are right. We should have an open mind for the heavier-than-air plane, the ignoble gases, and the coelacanth, but this does not suggest we should not keep our guard up against the Velikovskys, the Uri Gellers, and probably Bigfoot.

(2) We have questions of much higher priority...Surely you can find problems in our society which better merit your talents, my time, and Arizona's resources?

(3) When can we talk about UFOs?

Marine Biologists

(1) ...at this point, I believe it (Bigfoot) is extremely unlikely for strictly biological reasons: there simply is not space enough for a small group of hominids to live at the hunting-gathering stage of culture in north-west California without leaving much more evidence of their existence than the scattered sightings. Furthermore, most hominids are much too curious about their surroundings to stay hidden in such fashion.

(2) Anything that arouses curiosity due to observation or measurable data is worthy of scientific investigation. Otherwise, opinion based on belief will become the stronghold of bigots.

(3) With the number of hunters, etc. in that area for so many years and no material whatsoever to show, actual existence is highly unlikely.

- (4) ...I don't feel we should close the door to such supposed "nonsense" -- but prove it by private support.
- (5) I'm an agnostic with respect to Bigfoot. I'm willing to believe on the basis of concrete evidence.
- (6) I would approve of a limited program to specifically resolve the question of (Bigfoot's) existence or non-existence.
- (7) Proposals for such (Bigfoot) investigations should be considered and judged on their merits.
- (8) I feel that the quality of this particular questionnaire is lacking, and will be cited as a weakness, potentially detracting from any published results.
- (9) Obviously, I don't know much about monsters.
- (10) You may be right.

SELECTED COMMENTS ON THE LOCH NESS MONSTER

Physical Anthropologists

- (1) I have visited and camped on Loch Ness. Great story -- it keeps the tourists coming -- also a beautiful place. I don't believe in the Loch Ness Monster, but I am glad we have the myth.
- (2) I would give (Nessie research) money in pure science to competent and productive people, not for projects per se.
- (3) I spoke to some of them (tourists at Loch Ness) and recognized that they were there because of faith, not because of scientific training. It is all great fun to believe in monsters, and I'd never discount their existence to the true believers.
- (4) I certainly hope you are trying to get at something other than a survey of attitudes regarding the Loch Ness Monster with this questionnaire.
- (5) ...and the same goes for ESP, UFOs, Bigfoot, chiropractors, etc. Nonsense.
- (6) This arrived with 13¢ postage due.

Physical Chemists

- (1) Greatest area of uncertainty is regarding the value of such a survey. Is this a study of Nessie, or to see how many people will respond to such a survey? I hope federal funds are not being used for this!
- (2) Scientific method and ethics of science apply to this area and to others -- Bigfoot, ESP, etc.
- (3) Confirmation (of Nessie) would have mainly publicity appeal, but probably not all that much impact on science.

Marine Biologists

- (1) What are you going to do research on? What is the problem? What is its contribution to mankind? To find an explanation for every unknown problem? That science can always explain the unknown?! Mankind cannot tolerate nor afford such unlimited research outlooks, but must learn to live in harmony with the environment and limited resources first, or his superficial knowledge will get him nowhere but acceleration to extinction!
- (2) It is remarkable to me to view the large amount of bias and unscientific reaction of much of the scientific community to reports of the "Loch Ness Monster" and UFOs. In view of many reports from reliable observers of both these phenomena, scientists should be open-minded and apply the scientific method to these topics. Otherwise, we abandon the field to amateurs and/or mystics. We may be ignoring questions of tremendous significance to mankind.
- (3) The myth about the Loch Ness Monster appears to be the result of a combination of: sightings of fish, schools of fish, itinerant mammals (groups of otters, seals), honest misconceptions, and humbug. Research should be left to students, interested laymen, and retired scientists.
- (4) ...if there is a phenomenon in the Loch -- be it physical or biological -- it is the scientist who should be entrusted in doing his/her best to explain it.
- (5) I can think of dozens of projects that would be deemed by me to be more worthwhile in preference to a search for such animals.

(6) It is poor science to simply deny truth to observations such as Nessie, even if "truth" is not probable. I would not have given much credence to Nessie before the MIT team publicized its results...

(7) Should any firm evidence turn up, research should certainly be conducted and perhaps federally financed. Until that time comes, scientists' time will be more productive elsewhere. I personally would like to see it (Nessie) proved, but haven't much hope.

(8) Please recall that within a few years past living coelacanth fish have been found living in the waters off Madagascar. Until they were found living, they had always been presumed extinct for millions of years...If it has happened once, why might it not happen again? Pots of fish bait lowered to great depth off the California coast and time-lapse photographs have shown that there are very large unidentifiable animals which have visited the pots to feed. Might there not be other Nessies?

(9) I personally find it very difficult to accept a "Loch Ness Monster" or Ogoopogo in Okanagan Lake (British Columbia) because such a large creature should have been detected by now. Especially for Loch Ness with all the attempts to find the Monster, it seems unlikely that more firm evidence should be so obviously wanting. However, I do believe that such a "monster" could exist in the marine environment; i.e. "Cadborosaurus" from Victoria, B.C., could easily exist. The ocean is large enough for such a critter to have escaped notice or capture by scientists.

(10) I believe that there are still unknowns out there. Scientists would be conceited indeed to assume that knowledge is now complete.

(11) At the moment I have not seen any evidence except some rather fuzzy photos. As a scientist, I would need more evidence -- data -- before volunteering any opinion as to its (Nessie's) classification... ..let's spend U.S. money on basic research on basic problems.

(12) I'm rather suspicious of your motives, particularly the manner in which some questions are asked. There may be something to these stories -- undoubtedly hundreds of prehistoric genera exist in oceans and large lakes -- really a sampling problem.

(13) ...I have talked to people who claim to have talked to people who claim to have seen it (Nessie).

(14) Not U.S. dollars for Scotland expedition!

(15) We could use such a shaking.

(16) What the hell are you trying to find out?

Conclusions

Until statistically significant results are available, it would be premature to present definitive conclusions. What is apparent at this time is that there is considerably more skepticism among scientists about the existence of Bigfoot than there is about the existence of Nessie, although the existence of both is doubted by the majority of physical anthropologists and marine biologists surveyed. The consensus seems to be to attribute such reports to imagination, hoaxes, myths, and exaggerated tales, rather than to honest misidentifications of ordinary animals. The lack of specimens, parts of specimens, or even of bones, seems to be the principal reason for rejecting such reports, and many respondents believe that such animals could not remain so long "undetected by science." Nevertheless, most scientists seem to bend over backward when it comes to the question of research on these topics, and would support such research provided federal funds are not involved.

There is at least one important difference in the attitudes of the scientists surveyed which correlates with their diverse disciplinary backgrounds, and this difference may help in the understanding of the social factors involved in the acceptance or rejection of anomalous phenomena by scientists. Most physical anthropologists, unlike the marine biologists and physical chemists believe that the discovery of Bigfoot would have a "severe" impact "on science" (we were very careful to phrase the question in terms of "science" -- not "anthropology"). Does this imply that physical anthropologists have a different concept of what science is? Or, alternatively, are physical anthropologists conforming to what they perceive to be the "correct" attitude within their discipline (i.e., Bigfoot is so unlikely that its discovery would have a severe impact)?

The data also seem to indicate that physical anthropologists are more negative about Bigfoot than marine biologists are about Nessie, and perhaps this should not be surprising. Bigfoot, if it exists, is a terrestrial primate, and its discovery would fly in the face of the belief that all North American land mammals have been identified and studied. Nessie, on the other hand, supposedly exists in an underwater habitat, which, although smaller in area than Bigfoot's supposed habitat, is much harder to survey (some marine biologists even mentioned the great likelihood of such creatures surviving undetected in the oceans).

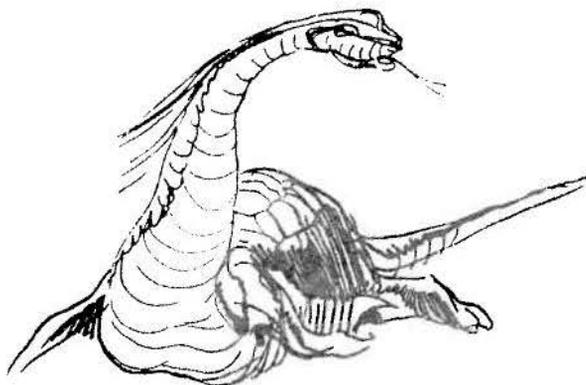
Also, being a primate and a hominoid, possibly even a hominid, Bigfoot would be a close genetic relative of man, perhaps too close for comfort, and the legal and moral implications involved could be substantial. The emotion aroused in both believers and disbelievers, some of which is captured in the above quotes, is perhaps a reflection of this awareness. Nessie, on the other hand, does not threaten man's elevated status in the animal kingdom.

We should also remember that the marine biology subjects of our survey were American and Canadian professionals, whereas Nessie supposedly inhabits a far-off and romantic spot of Europe. It would be interesting, in this regard, to compare our survey results with those from a similar survey of British (particularly Scottish) marine biologists.

The physical chemists played an important role in our survey. Although they generally doubted the existence of Bigfoot and Nessie (and cited the lack of physical evidence as reasons for doing so more than did the physical anthropologists and marine biologists), they tended to attribute Bigfoot reports more to honest misidentifications than to outright hoaxes, imaginations, or myths. They also tended to be more moderate on the question of financial support for Bigfoot or Nessie research.

If there is a "proper" scientific attitude, one would expect the responses of all three groups to be very much the same. As in many instances they were not, the respondents must have been influenced by other, non-empirical factors. We will not at this time propose what these other factors may be, other than to state that they necessarily must fall within the realm of psychology.

When further statistical analyses are completed, we may be able to shed more light on this interesting social phenomenon.



ANNOUNCEMENT

THE FORMATION OF PSI SOURCES INTERNATIONAL (PSI)

Readers of ZETETIC SCHOLAR may be interested in participation with a new organization now being initiated. (This is essentially independent of ZS, and readers should not infer any endorsement by the ZS editorial board.) PSI SOURCES INTERNATIONAL (PSI) is being formed to produce a large research pool of subjects who believe themselves to have psi abilities.

Many persons believe themselves to be psychically "gifted," and the literature available about them indicates that many of them believe their abilities constitute a psychological burden rather than an advantage. The parallels with those of high I.Q. are striking; and just as the organization MENSA was formed to bring together those with high intelligence quotients, so is PSI being formed for those with "high psi quotients." The literature also indicates that these persons are anxious to learn more about their abilities and wish to help scientists by acting as subjects for investigations.

PSI will centrally be concerned with generating research using PSI membership, but it will also make information and resources available to its members. PSI does not make any claims about the reality of psi but is anxious to promote objective inquiry. Persons seeking membership in PSI will be initially screened by means of a specially constructed test and questionnaire. Those accepted as members will be able to undergo additional testing towards validating and examining their abilities. This can lead to higher levels of membership as the validation measures are more impressive. Much of this will be handled through local chapters, sodalities.

Since PSI does not claim to "certify" anyone's psi abilities, skeptics and others are welcome to participate in our research efforts. Whether or not one believes in psi, many interesting questions can be explored including ascertaining what persons in PSI might have in common other than their belief in their own psi abilities.

Two things seem clear from the evidence we have so far: (1) Many persons believe they have psi abilities or have had psi experiences and want to know more about them. (2) Parapsychologists have repeatedly found better psi performance from those who believe in psi ("sheep") than from those who do not ("goats"). And many psi researchers have emphasized the necessity for getting larger and better samples of everyday subjects instead of "star" psychics with strong vested interests in the outcome of any tests of their abilities. PSI will take advantage of all these elements.

PSI is now undergoing organization and is seeking qualified Research Consultants who would like to be associated with developing PSI and having access to the research pool. Since many ZS readers are professional scientists, many of whom should be interested in the formation of PSI, I am announcing its organization here and invite ZS readers to comment, help, or make further inquiries. To know more about PSI, write to:

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PATHOLOGICAL SCIENCE: TOWARDS A PROPER
DIAGNOSIS AND REMEDY*



RAY HYMAN

How would you react to the following situation:

A competent and respected colleague reports to you that he held a seance in his own home. During the course of the seance, one of the sitters asked if the medium could materialize a sunflower. Following this request, a sunflower, six feet high, fell upon the table. Your colleague produces affidavits from witnesses, each of whom is a respected and honorable man. He insists that both the house and the medium were carefully examined prior to the seance and that all precautions were taken to prevent trickery. Furthermore, he concludes that the only explanation is that the medium somehow had access to a new force, one that he refers to as a "psychic force."

Take a moment to consider what your response might be. Remember that this colleague is one who has earned a reputation as a competent and successful scientist in his chosen field. He is still doing acceptable science within this field. But now he insists that as a result of the seance just described, as well as a number of others which he conducted under carefully controlled conditions, he has obtained many phenomena that cannot be accounted for by currently accepted scientific principles.

Such a situation actually occurred to scientists during the Victorian era in England. Alfred Russel Wallace, the cofounder of the theory of evolution by

*Paper to be presented at the Annual Meeting of the American Association for the Advancement of Science, San Francisco. Section 17: History and Philosophy of Science. Program: Science and Pseudoscience. San Francisco Hilton, Continental Ballroom 5, 9:00 a.m., January 4, 1980.

natural selection, shocked his scientific colleagues in 1869 when he made public his conversion to spiritualism. Up until that time, his scientific colleagues had taken it for granted that he shared the same materialistic and naturalistic outlook that they had. In addition to shocked disbelief, Wallace's colleagues responded in a number of confused ways--with embarrassment, with attempts to ignore it, with open hostility, with attacks on his character, with refusals to listen to his arguments or view his evidence, with misrepresentations of his claims, and with a variety of other reactions which could hardly be called rational, dispassionate or scientific.

1.0. Pathological Science

Wallace's bizarre claim and the confused reaction of his scientific colleagues is a good illustration of what I am calling "pathological science." I have borrowed this term from a talk that Irving Langmuir, late Nobel Laureate, gave at the General Electric Company back in December, 1953 (Langmuir, 1968). Langmuir defined "pathological science" as "the science of things that aren't so." His examples dealt mainly with claims of mysterious radiations or forces such as N-Rays, Mitogenetic Radiation and the like. I would add to his examples such cases as Martian Canals, the non-existing planet Vulcan, Gall's faculties, as well as Wallace's "psychic force." I would also broaden his definition to include cases in which scientists have wrongly insisted something wasn't so. For example, meteorites, the impossibility of heavier-than-air flying machines, Semmel-weiss's childbed fever, and many cases of missed discoveries. We should also include cases of data massage, unconscious plagiarism, deliberate cheating, and a variety of mixed cases.

The distinctive characteristics of these examples, as I see them, are the following:

- 1) a scientist of acknowledged competence and accomplishments
- 2) surprises his colleagues by claiming the existence of a phenomenon or relationship that is considered to be bizarre or even impossible by currently accepted principles
- 3) the scientific establishment either ignores or attacks with hostility this bizarre claim
- 4) the deviant scientist, along with a few deviant supporters, sticks resolutely to his guns in the face of attacks and indifference
- 5) the bizarre claim is considered to be discredited in the eyes of the scientific community
- 6) the claim is banished from further consideration in scientific literature, textbooks and education.

2.0. The Problem Posed by Pathological Science

Wallace's scientific colleagues, for the most part, could not believe that a six-foot sunflower could be materialized out of

thin air by a psychic force. I suspect that most of you cannot believe this either. But this poses a problem for Wallace's friends and colleagues who respect him as an honest and outstanding scientist. They could not accuse him of being an incompetent scientist; nor of being dishonest.

How should scientists respond to such a bizarre claim from one of their own trusted and distinguished members? Whatever the answer, one would like to say that the response should be consistent with rationality, objectivity, fair play, integrity--in short, with accepted scientific principles. Unfortunately, scientists are not trained nor given models about how to behave under such circumstances. The reactions, understandably if regretfully, are typically confused, ambivalent, erratic, and emotional. The reaction in these cases of pathological science appears to be more one of panic than of considered critical analysis.

If there is truly "pathology" in these cases, the pathology seems to be exhibited as much in the reaction of the scientific community as it is in the claims of the offending scientist. The gut reaction of the scientific orthodoxy is to discredit the offending claim by any means possible--ad hominem attacks, censorship, innuendo, misrepresentation, etc. This panic reaction usually does succeed in discrediting the bizarre claim. It becomes completely cut off from the main body of scientific lore and future generations of scientists have little opportunity to become exposed and possibly contaminated by it. But the manner of the discrediting and the results, I will argue, have consequences for the future of science that may not be worth the price.

4.0. The Reaction to Wallace's Claims

As I have already indicated, the response by the scientific community to Wallace's psychic claims was confused, erratic, inconsistent, and often emotional. Some, like Darwin, merely tried to avoid any public reference to the matter. Others dismissed it out of hand without actually trying to account for the specific evidence and arguments put forth by Wallace. The most dedicated critic--one who seemed to act as the "hit man" for the rest of the scientific community--was the physiologist William B. Carpenter. His basic position can be summarized by his own words:

I have no other 'theory' to support, than that of the constancy of the well-ascertained Laws of Nature; and my contention is, that where apparent departures from them take place through Human instrumentality, we are justified in assuming in the first instance either fraudulent description or unintentional self-deception, or both combined,--until the absence of either shall have been proved by every conceivable test that the sagacity of skeptical experts can devise. (Carpenter, 1877).

In addition to assuming fraud or self-deception, Carpenter also attacked claims on the grounds that the scientist making the claim was incompetent and had earned his reputation by being specialized just for one specific narrow field of science.

Carpenter saw the psychic and spiritualistic claims as part of an "epidemic delusion" and saw his mission in these terms:

I have no other motive than a desire to do what I can to save from this new form of Epidemic Delusion some who are in danger of being smitten by its poison, and to afford to such as desire to keep themselves clear from it, a justification for their 'common sense' rejection of testimony pressed upon them by friends whose honesty they would not for a moment call into question. (Carpenter, 1877).

Thus, Carpenter had no hopes of saving those such as Wallace and Crookes who were already "smitten by its poison." Instead, he was crusading to save those not yet contaminated. This might account for why Carpenter did not feel it necessary to look closely at the exact nature of the evidence and claims put forth by Wallace and Crookes. His task was to attack the epidemic by any means available. He was not concerned with the specific arguments and evidence being put forth. Rather, he wanted to make sure that those who might be tempted to listen, would not. His job was to frighten them away from temptation by any means possible.

And it is just in this sort of reaction that I see a serious problem for the continued viability of science.

5.0. The Negative Consequences of Inappropriate Reactions

It may both be understandable and almost inevitable that the reaction of the scientific critics to heretical hypotheses are emotional, irrational, and irrelevant to the specific arguments put forth. But whatever the reasons for such reactions, I believe that they have negative consequences for the conduct of science. The sorts of tactics employed by the establishment's "hit men" against the offending claims--blocking access to regular communication outlets, ad hominem attacks, misrepresentation of claims, dismissal on a priori grounds--do succeed in a way. They serve to "discredit" the deviant hypothesis. And once it is so tainted, then the establishment scientists feel relieved and ignore it.

But "discrediting" is not the same as disproving. As it turns out, often only through hindsight, most of discredited hypotheses deserved their fate. And perhaps the militant crusaders such as Carpenter can take comfort in the fact that their emotional and often irrational put downs of pathological sciences saved both contemporary and future generations of scientists from becoming smitten by the poison of wrongheaded heresies.

But the nature of the discrediting and some of its aftermaths may actually foster the very "evils" the crusaders were hoping to banish from science. Let me briefly elaborate upon this point.

First, let us consider the effect of the discrediting procedure upon the proponents of the "pathological" claim. Biographers and historians have written about how his defense of embarrassing causes harmed Wallace's subsequent fame. The main effect during his lifetime, however, was a forced compartmentalization of Wallace's orthodox biology and his unorthodox psychical inquiries into separate worlds. His scientific colleagues continued to accept and respect his orthodox contributions while they simultaneously tried to ignore his unorthodoxies. As a result, Wallace had to live in two separate worlds. When he did regular biology, he could talk, correspond, and publish within the world of established science. When he talked or wrote about his investigations of mediums, he could do so only in an entirely different world of individuals who were outcasts or non-entities with respect to the scientific establishments.

The same was even more strikingly so for Wallace's contemporary, William Crookes. Crookes tried to conduct laboratory research on psychic phenomena produced by mediums. He not only was bitterly attacked for this, but all his attempts to get his work read at scientific meetings or published in scientific journals were ruthlessly blocked. He finally gave up trying to get a hearing for these unorthodox views among his scientific colleagues and published his findings in spiritualist magazines. At the same time, however, he continued his purely orthodox chemical and physical experiments which were not only completely accepted by the scientific establishment, but which eventually won for him just about all the honors possible for a scientist of his period, including knighthood (Palfreman, 1976).

This enforced compartmentalization, if fact, seems to be true for most of the other cases of pathological science.

Consider what this compartmentalization accomplishes. It isolates the deviant scientist and his claims from further debate and interaction with orthodox science. He is restricted in his further consideration of his position to discussions and exchanges with individuals who already believe in and support his claim. In addition, most of these individuals have neither the training nor aptitude for rigorous scientific evaluation. Thus, the deviant scientist has no further incentive to refine, improve, or correct loopholes in his position. This further entrenches him in his belief that his initial claims were correct.

But even when the deviant scientist was in the process of being discredited by establishment spokesmen, the ineptness and irrelevancies of the criticism further strengthened him in his belief. For the proponent and his few supporters within the scientific establishment could see that the criticisms were based

on misrepresentations, irrelevancies, character assassination, plausibility arguments and failed to deal with the actual substance and specific arguments put forth. Such inept criticism, far from forcing the deviant scientist to re-examine his evidence and arguments, strengthened him in his belief that he was being treated unjustly and unscientifically. It further strengthened him in his conviction that he must be right.

But such inept discrediting procedures, in my opinion, have an even worse impact upon the conduct of science itself. By discrediting the offending hypothesis, the critics succeed in getting further consideration of it banned within the scientific community. This, indeed, might have the intended effect of saving the uncommitted scientists from becoming contaminated. But at what cost?

By banishing the failures of otherwise accomplished scientists, we prevent from consideration the learning of any lessons from them. Future scientists not only do not learn why and how such failures occurred, they do not even learn that they did occur. If they read about Wallace at all it is in connection with the theory of natural selection or Wallace's line. They do not read about his defense of psychic phenomena, phrenology, or his attacks on vaccination. If they come across discussions of Sir William Crookes, it is in connection with his discovery of thallium, his work with the cathode ray tube, and his invention of the radiometer. They are kept ignorant of his claims to have discovered a psychic force that enabled the medium Home to float or that allowed Florence Cook to materialize full-bodied spirits out of thin air. Similarly, they do not read about Newton's commitment to alchemical pursuits or Sir Oliver Lodge's studies of survival, etc.

Is it any wonder then, that several scientists today endorse as genuine psychic feats the conjuring antics of Uri Geller? Or that others claim scientific evidence for the ability of certain individuals to project thoughts upon camera film? Or that still other competent scientists argue that we can cure cancer with large doses of vitamins?

If we keep future generations of scientists ignorant of the follies of some of their most accomplished ancestors, how can we hope to prevent repetitions of these very same follies? How can we learn lessons from examples that are banished from further consideration?

When I talk about cases of pathological science such as that of Wallace, I often get two related responses from scientists in the audience. One is that Wallace's situation occurred 100 years ago. But it could not happen today. Because we know more today and science has become more sophisticated. The other response is to the effect that Wallace or whatever example I happen to be using is a special case and that most scientists could not get trapped into such an error. Both these responses seem to me to

beautifully illustrate what Zimbardo, and his co-authors call "the illusion of personal invulnerability" (Zimbardo, et al., 1977). They both are a way of saying it cannot happen to me.

In fact, it not only can happen today, but appears to be happening with increasing frequency. Paradoxically, this very attitude that it cannot happen to "us" or to "me," contributes to the vulnerability of scientists to such pathologies. And the fact that the discrediting procedure keeps scientists unaware of the many failures by otherwise recognized scientists further contributes to this illusion of scientific and personal invulnerability. The scientist, as part of his education, only hears about the successes of his great predecessors. The illusion is enhanced that science is much more a series of successes than is actually the case. By banishing the many and outstanding failures to a skeleton closet, both the scientists and the laymen become victims of the myth of scientific invulnerability and continue along a path of false security.

Another counterproductive aspect of this discrediting procedure is that sometimes otherwise promising students of science do go back and check upon the circumstances of the supposed debunking of a bizarre claim. They sometimes become anti-scientific or cynical about the objectivity and rationality of science when they see how the establishment has reacted in putting down a heresy.

I could list other counterproductive consequences of the typical reaction of the scientific establishment to suspected heresies among their ranks. But I probably have suggested enough to at least raise questions in your mind about the advisability of continuing to condone such tactics just because they seem to be effective in keeping us from having to cope with uncomfortable embarrassments.

6.8. The Benefits of a More Appropriate Response

At this point, let me recapitulate my main points and forestall any misunderstandings that might arise. Maybe I can do this best by putting my message into a more positive format.

First, I want to make it clear that I am not defending unorthodoxy as such. Nor do I believe that most claims of pathological science deserve serious consideration in themselves. I do not believe, for example, that Wallace's claims about a psychic force have any chance of being true.

Second, I fully appreciate that it is very difficult and demanding to develop an effective and rational response to such bizarre hypotheses. I myself know how demanding and difficult this can be from my experiences in trying to be a responsible critic of parapsychological research.

Nevertheless, I strongly urge the scientific community to bring the pathologies out of the closet and to openly work towards developing a more appropriate and rational response. The fact that such pathologies have occurred and continue to occur should be taken as a sign that all is not well. At the very least, there are germs that can potentially contaminate much that will be done under the name of science. The very fact that these pathologies are committed by otherwise competent scientists means that the response to them should be sober and scientific.

We need to keep the great failures as well as the great successes constantly before us. Not just as reminders of our own vulnerability, but as the first step towards comparing and contrasting them in the hopes of finding out just what it was, if anything, that led the very same scientist, such as Wallace, to recognized success in one instance and ignominious failure in another. At the very least, we can hope that such a step will prevent us from repeating identical mistakes in the future. At best, it might help us discover what it is about scientific thinking and procedures that leads to success and what it is that can produce failure. Or, as some historians and philosophers of science seem to be implying, it may teach us that any system for discovering truth has built-in limitations.

Furthermore, by insisting upon proper criticism and fair play in any critiques of heretical claims, we may move closer to a proper diagnosis of what actually went wrong. We focus on the claims and the evidence and this forces the deviant scientist to respond with further replications, refinements, controls, etc. At the same time, it forces the critic to not only show that the proponent is wrong, but in what ways he went wrong. This could lead to better understanding about how trained scientists can be trapped into defending false systems.

7.0. Conclusion

If "pathologies" do exist in the sense that some of our best scientists defend bizarre positions, then like all sicknesses, they are a symptom of something. Something is wrong and requires remedy. We cannot discover what is wrong by bad diagnoses--by failing to acknowledge the disease exists, by preventing others from learning about it, or by isolating the disease from the main body of science. Good science requires good and effective criticism. Bad and irrational criticism, even when the object is bizarre or outrageous, benefits no one. In the short run it "discredits" the object of the attack; in the long run, however, it "discredits" science itself.

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COMMENTARIES ON PROFESSOR HYMAN'S PAPER

COMMENTS BY JOSEPH AGASSI:

I find it hard to respond to Dr. Truzzi's kind invitation to comment on Ray Hyman's "pathological science" since I do not share so many of his background assumptions. I am in great sympathy with his proposal to keep an open mind about the paranormal, yet find his effort wasted on trite examples. But let me try.

It has been reported that a leading philosopher of science responded with alarm at the reported success of Rhine regarding parapsychology. If these reports were true--they are false, it turns out -- then, he feared, his whole philosophy would collapse. It is amazing to me that this should be possible. Certainly, if the Rhine experiments were judged bona fide, or if flying dragons were as real as elephants and castles, then our view would need adjustments, since today we exclude them. It is clear that if clairvoyance were bona fide, this would call for a very radical change of our opinions about time and causation, no less than if Fred Hoyle were successful at obtaining signals from some future inhabitants of our own planet. Yet, somehow, we have to agree, it is good to have an idea of what is empirically impossible and to be ready to change one's mind when scientifically observing the allegedly impossible.

Certainly Hyman is right in advocating this, and certainly there are examples where the allegedly impossible was first denounced as hoax and then admitted as scientifically attested fact; at least one example is the midnight sun, and another is mesmerism. No doubt hypnosis is mesmerism sans some claims about its causes, its magnetism or what-have-you. No doubt, Mesmer unknowingly meddled in hysteria without knowing what he was doing. Nevertheless, it may be convincingly argued, were Mesmer's contemporaries more open-minded, the recognition of hypnotism could have come sooner.

It is a misfortune, however, that Hyman confuses this with bona fide errors, whether a non-existent planet claimed by a scientist to exist, or a similar non-existent chemical element or radiation, etc.

These bona fide errors do not at all resemble factual reports which are presumably impossible: when two people's opinions as to what is possible or impossible clash, there may be room for honest debate. Yet factual claims are a major weapon in a debate, and if they are ineffective perhaps it is better to give up the debate and write the opponent off as a mere dogmatist. The question, then, is what are the conditions under which we must recognize and accept observation reports and thereby reject hypotheses which contradict them?

There is a traditional view of the matter, officially endorsed by the Royal Society of London from its very start and since then taken as a matter of course by the whole scientific community. It is first stated in Robert Boyle's Certain Physiological Essays of 1661, second essay, on the unsuccessful experiment. Boyle proposes this attitude and explains it at great length. He says, when an experiment is unrepeatable we need not call the one who has reported it a liar, but we should also not accept his report as true: we must suspend judgement until we can repeat the experiment. He gives two examples, one of a blind man who can sense color by his finger-tips and one about white gold from Madagascar. Though he is suspicious of both, especially since the blind man senses, it is reported, black more strongly than white, he suspends judgement. White gold, we know, does exist; it is doubtless platinum. The blind man was used by Jonathan Swift in his *Journey to Laputa*. Since then the story repeated itself a few times. I remember myself having read in a newspaper a few years ago that the blind man with the magic fingertips has reappeared in Russia this time.

Boyle's proposal is no clear solution to the problem. It is, no doubt, always possible that an experiment is repeated two or three times, yet turns out to be unrepeatable. And vice-versa. And there are examples for that from the annals of science. Yet, if an experiment was repeated, we do declare it repeatable. Are astronomical data repeatable and in what sense? Are psychoanalytic observations repeatable and in which sense? Surely phobia and conversion hysteria are, yet each case is observed as individual and as markedly different. Also, of course, many repeatable observations were mistakenly reported, including reports of discoveries of unknown elements in the sun (helium is the known example since it is, for all we know, the one truly observed in the solar spectra). Even when an experiment is repeatable, it is repeatable under specific conditions, some of which we know since we can vary them, some of which we do not know. This is a known fact, and Isaac Asimov based his novel The Currents of Space on it.

Are any paranormal experiments repeatable? Not to my knowledge. The fact that Rhine's experiments were faked is unimportant: were they repeatable his assistant's hand would not matter, and as they are not they do not matter either.

This raises the question, why do we insist on repeatability? The answer is given by Karl Popper: science equals testability equals repeatability. We want to be able to test the test, to refute the refuting observation. Suppose a refuting observation is repeatable. Do we have to endorse it and give up our theories? Karl Popper says, yes, or else the game is foul. I say, no, we may have a good excuse for rejecting an observation. But then the excuse has to be given and discussed. If it can not be given, at the very least we must recognize this as a defect and explain why we tolerate it.

Suppose some paranormal evidence were indeed repeatable. To take Hyman's example, suppose every medium in a trance could produce a sunflower out of thin air upon request. This would be an enormous boon for people who frequent mediums, since they could always verify that the medium is (deep enough) in a trance by requesting that a sunflower be produced out of thin air. Will this convince me? I honestly do not know. Perhaps, following Michael Faraday, I would inquire why no medium opens a sunflower seed oil factory. If the answer be interesting, I would pursue it and I can not now say with what impact. But I am exasperated by the fact that mediums claim to know such utter trivialities as details from my own past, rather than details of, say, Agamemnon's childhood, or of Elijah the prophet. My greatest complaint about all claims for the paranormal that I have ever met is that they are very boring.

It may be a matter of taste to decide what is boring. Some people find excitement in watching the wheel turn and decide whether someone standing by it be a millionaire or a pauper. For my part, I find it a bore even if I would be fascinated and hypnotized to watch the wheel as everyone else does. And perhaps fascination is many people's substitute for interests and perhaps this is why so many intelligent people are voluntarily wasting so much of their time on so boring sets of data. There is no reason to think this interesting or useful in any way.

All paranormal discussions leave open important questions. Astrologers seldom ask, how could my moment of birth be so crucial to my career, how could the distant stars effect it? They do not ask, is extrasensory perception the communication of information with no channel or with a hitherto unknown one (as radio waves were but a century ago)? Necromancy practitioners and ghost seekers have no idea how the dead behave, what mentality they possess, etc.? And, to my way of thinking, the study of the general psychology of ghosts is much more advisable a way to detect them than the study of rotating door-knobs and squeaky door hinges. The study of the paranormal is all too often an escape from thinking. Hence, the invitation to be open-minded about the paranormal often leads those who accept it to much frustration. Can this impasse be broken?

I do not mean my question rhetorically. The great W.B. Cannon, the inventor of homeostasis and author of The Wisdom of the Body, has claimed to have observed voodoo death, i.e., murder by a magic spell. His observation was ignored until it was explained by a body reaction to chemicals produced by fear and by despair and after these chemicals were reported to have killed a laboratory rat. This story shows how unwilling we are to accept reports of paranormal facts until their paranormality is explained away. For my part I find the dogmatic hostility to the paranormal no less boring than what thus far goes under the banner of the paranormal.

COMMENTS BY STEPHEN BRAUDE:

I am in substantial agreement with Hyman about how scientists ought to respond to the radical or unorthodox proposals and hypotheses of their colleagues. And I agree that scientists often disgrace themselves and damage their profession through the manner in which they attach apparently heretical claims. But ironically, Hyman's paper appears simply to be a non-hysterical example of the sort of practice it purports to condemn.

Hyman first describes proponents of radical or unorthodox hypotheses as "deviant" scientists whose claims are ineptly or irrelevantly attacked by the scientific establishment. And as he discusses the undesirable impact of such procedures upon the scientific community, the reader is led to believe that Hyman wants to be a spokesman for a rational and fair assessment of such unorthodox claims. But then a sudden and revealing shift occurs in Hyman's dialectic. He begins by referring to the class of unorthodox or radical proposals as "failures" and "follies," even though he acknowledged earlier that pathological science sometimes attacks hypotheses that are later vindicated and incorporated into the body of accepted scientific knowledge. Moreover, this choice of words is not merely an isolated verbal slip. The remainder of the paper strongly supports the conclusion that Hyman (despite his apparently self-serving protestations to the contrary) is really an ally of those whose critical practices he decries.

I'll return to this last point shortly. But first I must remark that there are no grounds, as far as I can see, for condemning the studies of D.D. Home as "failures," or Wallace's investigation of psychic forces as "ignominious failures." For example, Hyman's apparent assurance that the case is closed, so to speak, on Home flies in the face of the considered judgment of many competent people who have studied this material closely and thought about it (and associated issues concerning the acceptability of spontaneous case material in parapsychology) very carefully. It seems to me that, under the circumstances, a defender of non-pathological science ought to be more agnostic, or at least open about the fact that others in the scientific community do not regard the Home case as closed.

I have a similar reaction to Hyman's indictment and cavalier dismissal of all studies of psychic photography as 'follies.' I am confident that Hyman realizes that many people have studied this material carefully (I suspect more carefully than he), and do not regard the case as closed on psychic photography either. In fact, I have studied this portion of the parapsychological literature rather closely recently, and in my view the shabby treatment of Jule Eisenbud's studies of Ted Serios would make an ideal example of the dishonest and intellectually cowardly criticism that Hyman thinks can only harm the scientific community. Again, it seems to me that the position Hyman ought to take--the one consistent with his objection to pathological science--is to acknowledge that such cases are still controversial, no matter what his own intuitions about such alleged phenomena might be. If Hyman were not victim of

the sort of pathology he describes, I would think he would not select currently debatable cases as examples of failures and follies in science.

Anyway, returning to the subtleties of Hyman's dialectic later in the paper, consider the force of the analogy from medicine he uses in his final paragraph. Hyman refers to the scientific defense of bizarre positions as "sicknesses," something requiring remedy. Apparently, Hyman has forgotten that he earlier admitted that in the history of science, some radical proposals, no matter how maligned they may have been at one time, later became incorporated into the body of science. It would appear that Hyman regards the "objective" study of radical proposals as merely a way of cleaning the scientific house by a respectable method. But he sees it as housecleaning nevertheless. (Analogously, I suppose, one might argue that it is better to remove a derelict from one's doorstep by asking him nicely to leave, rather than by kicking him bodily into the street. And of course, construed this way, what is at issue is the best way to get rid of something undesirable.) Hyman apparently does not see the scientific enterprise as one whose method permits, not only the close scrutiny of radical proposals, but also their eventual acceptance if they pass the test of such scrutiny. A disease, after all, is something that must be destroyed. A radical proposal, however, may prove to be revolutionary and salutary.

Another telling feature of Hyman's discussion is his decision to call what should neutrally be designated as radical, alternative, or unorthodox positions as "bizarre." Some, of course, are. But to use this term throughout the paper to refer to the entire class of radical proposals is prejudicial already.

It seems to me, then, that Hyman does not really advocate the impartial, open-minded assessment of radical scientific claims, and that he is specifically unwilling to entertain seriously the radical proposals of parapsychology. His paper is only a plea to banish them in a way that preserves the surface integrity of the scientific community. To use the overworked terminology of T. Kuhn, Hyman's paper would seem to be a manifesto in defense of current normal science, and in fact appears to display a deep lack of confidence in the scientific method. And as a result, Hyman's description of pathological science turns out to be rather shallow, ignoring a very important kind of symptom of the pathology. It strikes me as significant and revealing that Hyman fails to observe (both in print and in practice) an important truth. The pathological response to radical scientific claims need not be manifest either in the shrill indictments or the supercilious disregard of those claims and their advocates. It may, instead, be expressed perniciously under the guise of objectivity and fair play. Like Brutus, perhaps, Hyman professes one set of attitudes and beliefs, and betrays another. One's dagger may be brandished openly or concealed under one's cloak. Real malevolence may be served either way.

COMMENTS BY HAROLD I. BROWN:

Thank you for the opportunity to comment on Professor Hyman's excellent paper, I have two comments to offer.

1. If the notion of "pathological science" is to be of any use it must be defined much more narrowly than "the science of things that aren't so," or the defense of "false systems." Hyman recognizes that we must include cases of mistaken denials by scientists under this rubric, and that there will turn out to be much more pathological science than is usually recognized, but he does not make clear just how much of science will be pathological on this definition. Strictly speaking, idealizations such as the ideal gas laws and approximations such as that objects near the surface of the earth fall with constant velocity, are false. Similarly, much of the classical mechanics that still forms the basis of our physics courses is, from the point of view of relativity and quantum mechanics, strictly false. In contemporary physics there are a number of unsettled debates: there are competing gravitational theories, there is new disagreement over the exact value of the Hubble constant and the processes by which the sun produces its energy, about the existence of quarks, about the distance to quasars and the source of their energy, etc.; in each of these cases only one of the extant views could turn out to be true and they may well all be false. The normal course of science requires the discussion and testing of many theses which are eventually rejected as false, and a definition which makes most science pathological is not very illuminating. If there is a useful distinction to be drawn between pathological and non-pathological science it must distinguish between those false views which are scientifically respectable and those which are not.

2. There is an assumption that many advocates of extra-sensory perception, psychic forces, etc. share with their most vehement critics, i.e., that if such phenomena exist, then, at the very least, there are phenomena that cannot be investigated in scientific terms, and perhaps the entire scientific approach to understanding the world around us is misconceived; there is no reason for accepting this assumption. Of course, if such phenomena do exist, then contemporary science will have to be supplemented, perhaps revised, but, as I have already indicated, this is happening regularly as a part of normal scientific research. Where the supposed challenge to science lies is in the claim that these phenomena are "supernatural" and thus not open to a scientific account. There is, however, no more basis for believing this to be true in the case of these putative phenomena than there was in the past for assuming that meteorites or dreams have a supernatural origin. Rather, if psychic phenomena and such do exist, then we have every reason to believe that they will be open to study by the usual processes of observation and theory construction.

COMMENTS BY MARIO BUNGE:

I suppose that the thrust of Professor Hyman's paper can be summarized thus. First, once in a while reputable scientists come up with unorthodox views that do not resist a careful rational or empirical analysis. Second, this suggests that there must be something wrong with the way we educate scientists. Third, the way such unorthodoxies are usually treated by the scientific community is often unscientific and always counterproductive.

I agree with all three theses but I disagree with Hyman's claim that there is such a thing as pathological science. The very notion of pathological science seems to me to be just as self-contradictory as the notions of atheistic religion (or religious atheism), irrational logic, and illiterate literature. The examples of "pathological science" mentioned by Hyman, such as the hypotheses of psychic forces and of Martian canals, suggest that he has fused two essentially different categories under the single name "pathological science," namely pseudoscience and false science, or pseudoscientific beliefs on the one hand and scientific statements that have been proved false on the other.

This distinction is necessary, because science does not have the monopoly of truth. (The telephone directory of New York and even Sears Catalogue contains more true statements than all of the social sciences put together.) While science pursues truth it is not characterized by truth, but rather by corrigibility. At any given time every science is full of falsities as well as of hypotheses that cannot be tested at the time. But science is self-correcting: far from regarding their hypotheses and data as dogmas, scientists put them to the test and, if the statements fail the tests, they attempt to replace them with truer hypotheses or data. Not so pseudoscience, which is a body of fixed beliefs rather than a field of research. What was "pathological," or rather contrary to the "spirit of science," was not Gall's hypothesis that mental functions are discharged by special subsystems of the brain, but the tenacity with which phrenologists clung to this hypothesis in the absence of experimental evidence. (Incidentally, contemporary physiological psychologists have independently revived Gall's general hypothesis; not however his particular cerebral map of the brain. Thus phrenology may be said to have had a grain of truth even though it was practised in a typically pseudoscientific fashion.)

In science dissent and controversy are normal and healthy. In pseudoscience they are punishable heresy. (Remember how Freud treated Jung, Adler and other dissenters?) In science controversy can be settled, at least in the long run, by observation, calculation or rational argument. In pseudoscience controversy is either hushed up or dissolved by resorting to authority or even force.

To be sure there is occasional data doctoring and deliberate cheating or stealing in the scientific community. But the former is eventually found out. (Remember the frauds committed by Sir Cyril Burt?) And malpractice can be exposed and punished. But such breaches of the ethos of science do not deserve to be put together with honest errors, such as errors of measurement, calculation or mistakes, or the proposing of erroneous hypotheses. Let us keep the

distinction between a mistake and a lie. There is nothing pathological about making mistakes; rather, what would be abnormal is hitting on the truth every time. Nor is it necessarily pathological to commit forgery under the guise of science: it is simply to indulge in a non-scientific activity.

I submit then that (a) no field of scientific research is free from error, but (b) in every such field one can discover errors and correct them; (c) dissent is of the essence of the scientific process, and the occasional pressure to suppress it in the name of the orthodoxy of the day is even more injurious to science than all the forms of pseudoscience put together; and (d) what is wrong with pseudoscience is not that it is wrong, but that it clings tenaciously to its tenets and does not make the slightest attempt to find any laws, or to harmonize its hypotheses with the bulk of science. In short, there is no pathological science: there are instead false scientific ideas, some dishonest researchers, and nonscientific belief systems parading as scientific. There is also, among scientists as well as laymen, a remarkable ignorance about the philosophical underpinnings of science and such general concepts as hypothesis, rule, theory, evidence, and inference: such ignorance is pathological.

So much for diagnosis. As for treatment, I would propose that every science student should be exposed to one course in each of these fields: logic, philosophy and methodology of science, and history of science (if possible his own). But not any logic: modern, i.e. mathematical logic rather than Aristotelian logic. And not any philosophy of science: only one done by someone who knows what he is talking about through having engaged himself in scientific research, rather than someone who has never come near it or has become disenchanted with it. And not any history of science either but one that, as Professor Hyman demands, tells us about "the great failures as well as the great successes": one teaching us that doing science is not hoarding truths but searching for more accurate and deeper truths as well as for more reliable research methods.

Of these three remedies the second is the most difficult to procure. Indeed most philosophers have had no experience of scientific research and they have been exposed mostly to nonscientific philosophies that place either authority or sophistry higher than agreement with mathematics and the science of the day. Therefore it may be best for scientists to try their hand at building a philosophy of science that at least agrees with their own research experience. Still, they should not ignore professional philosophers in the process: they ought to engage in dialogue with them. There are at least two ways students can derive benefit from such cross-disciplinary discussions. One is by inviting them to participate in group discussions. Another is by organizing faculty courses on the philosophy of science (or of psychology, or social science, or any other field of scientific research). Who knows: a new philosophy of science, one relevant to live science, may be born from some such attempts.

COMMENTS BY ROGER COOTER:

To accept Hyman's paper as merely a joke intended to solicit replies, is to miss the fact that it is a well-conceived caricature of uncritical academic thinking. As belied by the un-scholarly dogmatism of "a proper diagnosis and remedy," this surely is one of those clever end-of-the-session papers designed gently to mock the political and social naivety of science's practitioners, historians and philosophers. With a few good strokes and well-sustained tongue-in-cheek, Hyman has skillfully written himself into a Dickensian script: the mixture of Smilesean values, pneumatic pumps, phrenology and chemistry that the Uncommercial Traveller encountered in the Dullborough Mechanics' Institute in the early nineteenth century, is updated by Hyman to the American Association for the Advancement of Science at their annual meeting in, appropriately, San Francisco.

If Hyman did not intend his paper as a caricature then there would indeed be cause to worry about his critical poverty and about how a "learned" audience could have been duped. The possibility that a part of Hyman's audience might have missed the joke and taken this junk propaganda seriously is a frightening thought, and on that account his paper merits comment.

By "junk propaganda" I refer of course to the paper's obvious manipulative tactics rather than to its contents per se. To anyone who has studied science from a critical social perspective, the contents of Hyman's paper will not appear as problematic, for the issue of the "truth" or "falsehood" of the paranormal is irrelevant. After more than a decade of sociological inquiry into scientific knowledge and practice, no one doubts that "scientific truth" is, as Sir James Fraser said of truth, "merely the hypothesis which is found to work best" in any given socio-economic and cultural context. Thus from a social perspective what would be interesting about Hyman's paper (if it were straight) would not be the matter of the "scientific truth" that he hopes will be advanced through the creation of a scientific community receptive to the (ill-named) "pathological science," but rather, the way in which, by failing to question the cultural specificity of modern scientific truth, Hyman legitimates its superiority. In part this legitimation is overt through the lauding of orthodox science and its practitioners as rational, responsible and rigorous, dedicated to refining, improving and correcting through a process of debate and interaction out of which supposedly issues the wonderful one and only "objective reality." More interesting, however, is the way the wisdom of modern science is covertly celebrated by the diversionary tactic of appealing to the scientific community for open-mindedness: On the face of it Hyman seems to be suggesting the illegitimacy of the demarcation between science and "pseudoscience." But in fact nothing is further from his mind; he accepts that there is "true" and "false" science and requests only that orthodox science (which is otherwise irreproachable) give some consideration to "deviant" sciences before wholly or partially rejecting them. In retaining

the dichotomy between true and false knowledge, Hyman reveals himself as locked within what Harry Collins calls a "sociology of error" which claims to "explain" false science only by reference to orthodox science, and by that process reinforces belief in the latter's supposed value-neutral veracity. Hyman is also, of course, feigning ignorance of the fact that modern science mediates only a partial view of reality - one that treats the world as a set of interacting facts in which human essences are separated out in the dichotomies fact/value, science/scientism, science/ideology, science/pseudoscience, science/society - and is further pretending (?) to have no idea that this positivist "thingifying" view of reality was historically created (largely in the seventeenth century when constitutive with the "scientific revolution" was the reification of labour through the ascendance of a bourgeois capitalist class). The way in which this ignorance of the history and philosophy of science leads to the reproduction of uncritical support for the dominant ideological mode of scientific thought is nicely illustrated in Hyman's acceptance of Langmuir's term "pathological science," meaning "the science of things that aren't."

Mostly Hyman accomplishes his celebration of science's reifying (capitalist-constituted) ideology through an empty-headed (naive liberal) desire to have others share his open-mindedness. But in the failure to mention Wallace's commitment to socialism, must be seen a deliberate attempt to underwrite the orthodox scientific audience's unquestioned and self-righteous belief in the superiority of their way of viewing the world. Wallace's socialism is as well-known as his interests in phrenology, anti-vaccination, spiritualism and the transmutation of species, and the failure to mention it is as gross as the failure decried by Hyman, of considering Wallace's contribution to evolutionary theory without considering Wallace's phrenology and spiritualism. Anyone uncritically steeped in scientific rationalism attempting to treat Wallace's beliefs without treating the political and social outlook that gave those beliefs a delicate coherence, naturally comes to the mistaken conclusion that Wallace's mind was fragmented into orthodox and unorthodox scientific compartments. (Does Hyman interpret all sincerely religious scientists in a similarly unreal way?) Obviously, he has no idea of the glass house he lives in, nor has he ever been warned about the danger of throwing stones.

Hyman's pastiche makes crystal clear what is really vital in considering "pathological" in relation to "orthodox" science. By focusing on the bogus question of the "costs" of the orthodox scientific community's rejection of the "pathological" and thus revealing to us the sorts of metaphors that come easiest to the kind of mind being caricatured, Hyman succeeds in making us aware of the question begged: what are the human consequences of this point of view being taken seriously? The paper leaves little doubt that the answer must be the extension of orthodox science's philosophical hegemony - the "metrication," as it were, of

spiritualist and other deviant "extra-scientific" activities. Hyman nicely illustrates the classic liberal tactic of gelding what threatens to upset orthodoxy by enfranchising it into the dominant pre-ordained scheme of things. With paranormal deviancy already within the academy, Hyman offers the means by which the orthodox scientific community can both save face and further assert its authority and dominance: the orthodox scientific community has only to admit that it has let its "traditional" open-mindedness slip. Therewith can begin the historical re-run of the cultural imperialism of orthodox science. But there is an interesting historical difference: whereas (as Everett Mendelsohn has revealed) the second generation in the scientific revolution in the seventeenth century discarded alchemy, astrology and the like in order to erase awareness of the social and ideological construction of their natural knowledge and thus enhance their authority on the basis of an image of heavenly-descended pure truth; the modern inheritors of that subjective (so-called "objective") knowledge must now take back the deviant discards in order to retain the image of the objectivity in their pursuits.

The naivety that Hyman projects in his paper should also serve among spiritualists and other lay practitioners of "pathological" sciences to instill awareness of the social and ideological interests that lay behind what they might otherwise take simply as sincere scientific curiosity in their knowledge and activities. (Hyman, or Hyman's persona, they will see, is the "nice guy" in history to whom Mannheim alerted us: his goods are all the more worthy of critical inspection for their being offered innocently as face value commodities.) Spiritualists ought now to be conscious of what is so self-evident as to be often forgotten: that in attempting to recover the inner spirit of man they mediate and practice the antithesis of the worldly reifying materialism of modern rationalist science. It is no coincidence that the widespread interest in spiritualism of Wallace's mid- and late-Victorian generations occurred at that moment in history when the central contradiction of industrial capitalist "progress" was revealed most nakedly - i.e., man's dehumanized alienation from man as a result of the ruthless exploitation of labour conceived of as a mere commodity. Though spiritualism was for the most part an unconscious and individualistic response to people's soul destruction under advanced capitalism, in its attempt to restore to people something of their human essence, it can be seen as an untheoretical non-materialist complement to the socialist project. Although spiritualism has never been concerned with rearranging social structures and relations, it nevertheless offers, as socialism does, a means of fighting against the socio-economically-constituted corset mediated by the scientific productions of the Newtons, Carpenters and Darwins.

Insofar as Hyman's paper alerts us to all of this through its blatant disregard of history and unawareness of science as a social and cultural product, it performs a useful heuristic service. Much more could of course be said about the built in

absurdities of the attempt "to bring the pathologies out of the closet," but all else is largely contingent upon the two interconnected points raised here: (1) that the plea for orthodox science to be open-minded about the claims of "false science" deflects attention from the social and ideological constituents of modern science that are harboured in the imagined dichotomy and thus pari passu strengthens faith in the illusion of modern science's objectivity, and (2) that behind the plea for open-mindedness is the attempt to bring deviant scientific activity within the hegemony of orthodox science and hence extend that hegemony whilst castrating heterodox science of its actual and potential value to mediate alternative social and human interests and meanings.

For so cleverly caricaturizing all of this, Hyman deserves to be commended.

COMMENTS BY ALLEN G. DEBUS:

Ray Hyman is quite right in asserting that the history of science has traditionally perpetuated the myth of the constant advance of "real" science --- and that abnormal science (if judged from our present viewpoint) has not been examined with the care that it deserves. However, this situation has been changing in the past two decades. There has been an increasing interest in the alchemical studies of Isaac Newton, the mystical views of Johannes Kepler, and even the meaning of natural magic in the thought of Francis Bacon. And in addition to the mechanical philosophy, we now have some understanding of Hermetic and Paracelsian influences on the development of the Scientific Revolution. In short, there is a new picture of the rise of modern science --- one in which non-modern elements play an essential role.

However, one should contrast the free interplay of the "occult" and the "scientific" in the period of the Renaissance with the period after the mid-seventeenth century. The development of scientific societies after 1660 made possible the emergence of a scientific establishment having the power to admit to their ranks those with similar beliefs and to exclude those with whom they disagreed. And the simultaneous publication of the first scientific journals ensured the dominance of the mechanical philosophy. These developments have affected the scientific enterprise since that time. The views of Wallace and Crookes on spiritualism might well have been tolerated had they flourished in 1600. Such would not have been the case a century later.

There is little doubt that if the historian of science hopes to understand Wallace and Crookes he must study their thought in toto. That is, he must study both their "occult" and their "scientific" views. That is the only way in which we will be able to comprehend the key figures in the development of the sciences. To this extent I am in complete agreement with Professor Hyman. However, I do not think it is practical to extend this method to the present. Most of the proponents of "deviant" scientific views

today are to be found among the occultists. Perhaps we could begin by turning to some concepts held by otherwise renowned scientists that are condemned by their colleagues, but if we do, we would soon find ourselves deluged with a flood of cranks demanding that the same attention be paid to their own pet theories. One need only recall that Michael Faraday devoted himself to the exposure of spiritualism in the closing years of his life only to give up the task as hopeless. Today there are astronomers who regularly seek to expose astrology while the AAAS recently devoted one session to an examination of the claims of Velikovsky. Little is to be expected from such efforts since these people thrive on the attention paid to them --- even when such attention is negative. Commenting on the statement against astrology signed by 186 leading scientists several years ago Vivienne Killinsworth rightly noted that "the spectacle of the scientific Establishment bashing astrology is only likely to increase astrology's underground appeal. The comment of one of the prime movers' behind the [scientists'] statements, to the effect that he makes it a practice to denounce astrology every five years, can only give heart to his opponents."

I would conclude then that if we are interested in the development of modern science we must endeavor to place the work of earlier savants in their proper context. We must not define such work only in terms of the present. Professor Hyman is absolutely correct in calling for a study of the total work of Wallace and Crookes. But to recommend an examination of all "pathological" science today would open doors to occultists as it did in the nineteenth century. I doubt whether an open dialogue would develop --- only charges and countercharges by those who can not really understand each other.

COMMENTS BY GERALD L. EBERLEIN:

The "pathological science" situation is no peculiar situation, but rather a phase of a-paradigmatic science or anti-paradigmatic science. The scientist or the scientific community concerned with the controversial topic does not yet have a theory, or better a research programme (Lakatos), a paradigm, more precisely, a disciplinary matrix (Kuhn). In the language of the structuralist or non-statement view (Sneed, Stegmüller) no theory core exists, nor could the researcher/scientific community afford the intended applications of a theory core. As Kuhn, Stegmüller *et al.* have shown, this shortcoming has been blamed on the researchers as personal incompetence in the situation, "science in crisis." In my opinion this applies even moreso if the a-paradigmatic phase of the discipline under formation is evasive or even hostile toward the metaparadigm of pre-dominating normal science.

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COMMENTS BY PAUL FEYERABEND:

The first and to my mind most fitting reply to Ray Hyman's conundrum is: who cares? If scientists cannot get on with each other, then this is their problem. There is no reason why anyone else should get into the act, or be interested in its ramifications.

The reply assumes that science is an association of people sharing certain interests and beliefs, that it is a kind of club. A club has every right to insist on criteria for membership and to expel those who do not conform to the criteria. The work of the members occasionally pleases outsiders and may even be supported by them (use of clairvoyants by the police). This does not mean that the interests and the beliefs of the clubmembers are the only interests and beliefs that count and that internal conflicts about them concern everyone. It only means that some people sometimes like the products (the ideas, the procedures, the gadgets) the club has to offer.

Today only few people regard science as a club in this sense. The attitude is rather that scientific procedures and the results that emerge from them are parts of the Right Way. Science is therefore taught in schools as providing the only kind of knowledge that counts, the judgement of scientists and of quasi scientific organizations such as the AMA plays a major role in public affairs; scientific activities are supported by tax money just as the Roman Hierarchy was once supported by the tithe - as a matter of course. Science is a church - and salvation is not possible outside of it. Small wonder the domestic quarrels of scientists tend to become a public malaise.

Regarding science as a church has a variety of consequences, none of them desirable.

A person engaged in a difficult project has only finite resources (time, money, brains, patience) at his disposal. He cannot possibly consider and argue against every idea and procedure that differs from his own. Still, he has to make a choice. Naturally, he will choose ideas and procedures he likes, is familiar with, and can defend to a certain extent. If he belongs to a club, say the club of high energy physicists, then the shared assumptions of the club will provide further boundary conditions for his choice. Whatever the basis - the choice will contain a large subjective component which reflects his inability to examine and to close all alternative routes. Presenting his case in an honest and straightforward way the researcher will therefore say: "This is my research programme and this is the method I have chosen to check, defend and perhaps to improve it. I am aware that there are many alternatives; I have studied some of them, I have heard about others, I know there are many more I have never heard about and may never encounter. I can give you some reasons

for my choice. The reasons are neither conclusive nor complete. As a matter of fact, they hardly count in face of all the problems and objections that still remain. I can also tell you why I have not chosen the alternatives known to me - but even here you will notice that my reasons are quite thin in places; I just didn't have the time and patience to work out all the details. The rest I simply disregarded not because they are obviously wrong, or can be shown to be wrong, but because I had to make a choice, and this is the choice I and my friends at the club find most attractive. I hope you will like what we have come up with - but I quite understand if you disagree and prefer some other approach. After all, your ideas and presuppositions may be very different from mine and those of the club." It is clear that such an attitude cannot possibly lead to the problems raised by Hyman.

It is equally clear that most scientists and almost all philosophers of science not only do not practice this kind of honesty, but explicitly discourage it. Blinded by the myth of the Right Way they feel constrained to give 'objective' reasons for their choice: the excluded alternatives - all excluded alternatives - were not just omitted, they did not deserve being considered. Of course, the task cannot be carried out. Most alternatives are unknown to the scientists in the field in question and many are above the heads of the average practitioners (it would take a nuclear physicist years to understand even the elements of Aristotle; and the average training in acupuncture and the associated philosophy takes from 10 to 20 years). What scientists do in such circumstances is the following: they interpret the discomfort they feel in the presence of unusual ideas as a clear perception of the worthlessness of the ideas and so transform their own personal or group idiosyncracies into "objective" criteria of excellence. This "objectivization of the subjective" explains the simplistic and downright infantile character of arguments against "bizarre" deviations (example: the Humanist's encyclical against astrology; signed by almost 200 scientists, 18 Nobel Prize Winners among them. It is a monument of conceited illiteracy. And note how easily W. B. Carpenter whom Hyman quotes turns a questionable metaphysical principle into a condition of honesty and scientific good sense.) The similarity with the arguments some less gifted theologians raised against the motion of the earth is astounding, but not at all surprising: the predicament of the "objectivization of the subjective" is shared by all churches.

However there is no reason why the sciences should be dragged into this predicament. True - scientists now play a role that is in many respects similar to the role bishops and cardinals played not too long ago - but that cannot prevent us from cutting them down to size. True, science has been quite successful in some areas - but this does not mean that it should be imposed on everything and be regarded as the one and only measure of knowledge and even of humanity. Scientists invented an atom bomb, put a few wrapped-up bodies on a dried out stone, the moon, found the structure of DNA, improved chemical warfare - but this does not mean that

they and nobody else will be able to cure cancer, restore man's harmony with nature, make a contribution to our spiritual lives. Besides, it is difficult to judge success without the help of alternatives. Scientific medicine is often praised for the tremendous contributions it made to our lives - but how would it fare in a competition with traditional Chinese medicine that explores not only those few areas where Western medicine is obviously ahead (surgery, for example) but the entire domain of sickness and health? We do not know because we have no control groups (groups treated by Chinese medicine only). And we have no control groups because scientists have turned unchecked principles of their activity into conditions of sound medicine and so prevented the practice of alternative forms of medicine (cf. the remark on W.B. Carpenter made above): the church aspect of scientific enterprises very cleverly prevents us from checking their efficiency. We must also realise that success always means success with respect to certain values: for a mystic who can face God in all His Splendor the moonshots are a monumental exercise in futility. Result: there is not the slightest reason to turn the local ideas and principles of special enterprises such as scientific medicine or elementary particle physics into standards of rationality and there exist weighty objections against this procedure.

Now in a free society all values and all traditions have equal rights. Traditions different from science must be given equal access to tax money, public instruction, public decision making - no matter what other traditions think about them. If the Roman Church is separated from the State, then the Church of Science must also be separated from the State. If the Church of Science receives public funds, then the Roman Church must receive public funds, too. Giving all traditions equal rights in this sense also means an increased ability to judge the products of every one of them (cf. the brief remarks, made above, about the use of control groups in medicine): the church aspect of science conflicts both with the right of people to live as they see fit and with their right to judge the institutions they support and whose products they use. Science in a free society is therefore one church, one club, one private association among many, very much like an overgrown local theatre group, or a sock factory, or a flat-earth society. It will be encouraged, financed and its products will be used as long as it performs in accordance with wishes of the buyers. Financial support will be withdrawn as soon as the citizens prefer other products (support for scientific cancer research will be drastically reduced as soon as the citizens decide to give acupuncture a chance). Internal quarrels among scientists, however, are of no greater interest than the quarrels, within auto factories, about the shape of next year's cars. They are bothersome for practical reasons (delay in the production of new cars) - they have no deeper significance.

COMMENT BY ANTONY FLEW:

The clues we need are in Hume's Inquiry concerning Human Understanding. He begins Section X by distinguishing the merely marvellous from the authentically miraculous, notwithstanding that he can on his own principles have no means of so doing (Flew 1961). Ray Hyman by contrast starts from an imaginary example. A colleague reports that, in a scrupulously supervised seance in his own home, the medium materialized a sunflower. This sensational alleged performance is then described, very weakly, as "phenomena that cannot be accounted for by currently accepted principles." But, we should remind ourselves, phenomena satisfying that description come ten a penny: it must apply to everything in every new field still outwith the scope of present theories.

It is only with his second bite at the cherry, in treating the actual case of Alfred Russel Wallace on Spiritualism, that Hyman begins to speak of "a phenomenon or relationship that is considered to be bizarre or even impossible by currently accepted scientific principles." Nevertheless, although he is here holding the critical clue in his hands, Hyman does not recognize it as such. The crux is that the rejectors, whether rightly or wrongly, believe that they possess overwhelmingly good reason for dismissing the alleged phenomena. This they do: not merely as unaccountable by currently accepted scientific principles, or unmanageably bizarre and marvellous; but also, and much more reasonably, as just plumb impossible. They appeal, or at any rate they should appeal, to nomological propositions which are constantly tested and - it would appear - never found waiting in the daily living of scientists, technologists, and plain men. In particular perhaps, they appeal to some "basic limiting principles" seeming somehow prior to and more fundamental than even the oldest and best established of named laws of nature. (See on this, for instance, Broad.)

But, you may say, is it not always conceivable that, for once, and at last, the outsider is right; that this alleged phenomenon does genuinely occur; and that it is now high time for everyone to shake up their fossilized old notions of what is and is not, as a matter of contingent fact, impossible? Yes indeed it is conceivable; as the Humean ought to be the first to admit. Did not the good David himself labour to destroy "that implicit faith and security, which is the bane of all reasoning and free inquiry" (p. 26)? Yes, but; but, but. The heretical claims being considered here are all singular, and in the past tense: on such and such an occasion the medium said this, and then that sunflower materialized out of nothing; and so on, and so on and on. So, in assessing the evidence for the truth of every such singular historical claim, we have to appeal to all which we know about what is probable or improbable, possible or impossible. For how else can the critical historian proceed? Yet in that appeal to the

essential criteria of critical history the case for the miraculous collapses. At the very most, it can return a non-committal and typically Scottish verdict: 'Not proven'.

If we are really to be required to shake up our notions of what is possible and impossible, then we need to be confronted: not with a singular past tense proposition, which it is now too late to put to any direct test; but with an open general hypothetical, which can in principle be tested and retested at any time and in any place. We need, to put it less philosophically, a repeatable phenomenon. It would also help if the supposedly impossible could be naturalized by the excogitation of a fresh theory simultaneously explaining both these recalcitrant occurrences and a whole lot else of undisputed authenticity.

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COMMENTS BY J. N. HATTIANGADI:

Hyman's call for "fair criticism" is indeed a valuable reminder that even views we find outrageous deserve a fair hearing. There is evidence in the history of science, moreover, that "pathological" science had a great part to play in the development of what we today interpret as "healthy." Luigi Galvani, for instance, illustrated his idea of a special electricity of life by making a frog's leg twitch--after it had been amputated--by inserting lead and zinc pins into it. This is a science of "what aren't so." But Volta, who thought otherwise, argued that it is not the frog's leg, but the metals which give rise to electricity. He constructed a stack of alternating sheets of metal separated by a dilute acid. This is a great scientific discovery, even though Volta was doing "unhealthy" science, too! For we now know that the metal electrodes in a voltaic cell get their charge from the solution in the middle, not from the metal. Two "pathologies," it seems, led to a "healthy" result.

But this doesn't mean that every bizarre idea must be followed up, and fairly criticized. That advice is impossible to follow,

because we have limited time and resources to examine every wild idea that comes up before us. Sometimes what appears "pathological" is nevertheless valuable. A faithful history of science is a history of some "pathological" science that nevertheless had valuable consequences. But only some "pathologies" played a part, and most were useless, in the past and today. A scientist's choice of what to study, to attack, to defend or to research cannot depend on the sense of what is right and what is not. Science is always teaching us about new undreamt of possibilities, and so scientists must always be open-minded. Nevertheless, scientists must choose. How?

By-and-large, the question of what to consider for study, and what to ignore does not depend on whether a point of view is plausible or not. Very few great scientific theories have initial plausibility. They seem at first to be about things "that aren't so." They are nevertheless given serious **attention**, because they are solutions to difficult relevant problems of the science in question. Every science at any time has some central problems around which research is conducted. Some of these are technological problems, some problems of applied science, and some of a purely abstract variety. It is this focus of scientific interest which determines what to study and what to ignore.

What is scientific and what is not is therefore different from age to age--there is no eternal demarcation of scientifically "healthy" from "pathological" science. At any given time, however, there are irrelevant speculations, and relevant ones. Only the relevant ones deserve consideration. Relevance in science is relevance to its own set of important problems.

The general analysis of how ideas are relevant to the state of a science is a large question, beyond the scope of this comment.¹ It is clear, intuitively, however, that Wallace's "psychic force" would not solve any problem of nineteenth-century biology--or psychology.

There was a time in the history of science when it was believed that the Scientific Revolution (from Copernicus to Newton) produced the true and final laws of motion. Science, on this account, was the establishment once and for all, of the truth. The scientist could hope to do this only if he or she abstained from believing corrupt non-scientific speculation. When scientists held this view of science, they were always embarrassed by and hostile to other scientists who held peculiar views. These views would naturally seem to "discredit" science.

Today we learn from Darwin's revolution in biology, Einstein's in physics, and the ferment in new subjects like biophysics, sociology and economics, that though scientific laws are the very best, they are not necessarily the true and final ones. We do not need to feel embarrassed by a piece of "pathological" science. If it is relevant to our problems, we should give it the criticism it deserves--after all, it may lead to great results (pace Galvani and Volta). If it is irrelevant, we ignore it. There is no evidence that idiosyncratic views of any scientist have ever damaged the development of scientific knowledge. But the scientist cannot spend all his time attacking superstitions, or even criticizing them fairly.

NOTE:

¹Cf., J.N. Hattiangadi, "The Structure of Problems," Parts I & II Philosophy of the Social Sciences, Vol. 8, No. 4, December 1978 and Vol. 9, No. 1, March 1979.

COMMENTS BY SEYMOUR H. MAUSKOPF:

I experienced a strange oscillation of sentiment, from hearty agreement to intense objection, in reading Professor Hyman's paper. I think that my reaction was a reflection of a contradiction, or at least ambivalence, internal to the paper, which I would state as follows: Professor Hyman calls for a "more appropriate and rational" response from the scientific community to deviant or "pathological" scientific claims than the usual crude, ill-supported ad hominem accusations and innuendos. But he does so not in the interest of really open-minded discussion of unsettling assertions but rather the more effectively to lay them to rest: to disarm the recalcitrant deviant, to show him the error of his folly, and to admonish the naive who might be similarly tempted to go astray. Whatever else "more appropriate and rational" might mean, the phrase, as used in this context, clearly means "prejudged" response.

Professor Hyman would enlist the services of Clio in his proposed program and it is to his uses of the history of science that I shall devote the bulk of my remarks. His proposal is, prima facie, something with which I can only concur: that the total activities of past scientists be taken into account and not just the achievements which get recounted in the introductions to scientific textbooks. In particular, he would have brought to light those activities of some past scientists which have been forgotten or suppressed because of their disreputable aura: Newton's alchemy and A.R. Wallace's interest in spiritualism, to cite two of his examples. But as an historian of science, I take vigorous exception to his purpose in unearthing this material, which is to teach contemporary scientists lessons from "the follies of some of their most accomplished ancestors... to prevent repetitions of these same follies." His "pathological" metaphor is clearly a considered one here: like anaerobic bacteria in a dank sewer, the pseudo-scientific heresies of the past will continue to fester down the dark corridors of time unless brought into the light and air by diligent scholars.

My own aim as an historian of science is quite different even if at first glance it might seem to share similarities with Hyman's. In dealing with scientists (and science) of the past, I try to delineate the scientific outlooks and systems of belief as comprehensively as possible and to avoid, as far as possible, obtruding my own presuppositions into my analysis and understanding. If, for example, Aristotle believed in the perfection of circular motion (as did Copernicus and Galileo, for that matter), he must have had good reason within his system of scientific thought and it behooves me to try to understand this belief rather than to condemn it out-of-hand as wrong-headed

aberration. By extension, the same applies to Newton and alchemy, and to Wallace and spiritualism.

I give Hyman credit for calling for an end to achievement-oriented assessment of past science and scientists but it seems to me that his scheme leads to the same result; indeed would even reinforce it since positive achievements of scientists are now to be set in relief against their more "pathological" interests. ["We need to keep the great failures as well as the great successes constantly before us."] And how does one assess what was "pathological" if not in terms of presentist presuppositions? What other criteria, after all, could possibly define "the science of things that weren't so"? The game is given away by Hyman's list of scientific pathologies; why, for instance, denial of meteorites before 1800 should be considered "pathological" while mid-19th century denial of the existence of a planet within Mercury's orbit be deemed "scientific" utterly escapes me unless "scientific" is to be defined according to the criteria of what we happen to believe to be true today. But if this is how we are to judge the creditability of past scientific activity, then the history of science becomes a futile exercise indeed. More honest, in my opinion, simply to go on to chronicle past scientific achievements a la "textbook" history of science.

Moreover, Professor Hyman's dichotomy between healthy and pathological science leads him to distort somewhat the historical context of his principal exemplar, A.R. Wallace. First of all, Hyman seems to imply (or to assume?) that Wallace was (or must have been?) a bifurcated individual with two unrelated interests, one scientific and the other pathological. Even if it be granted that, concerning the reception of his ideas, there "was a forced compartmentalization of Wallace's orthodox biology and his unorthodox psychical inquiries into separate worlds," this compartmentalization by no means applied to Wallace's own scientific thought, as Malcolm Jay Kottler has recently shown.* Wallace's psychical beliefs have to be taken into account to understand fully his ideas on evolution. This is hardly what I expect Professor Hyman has in mind in having Wallace's spiritualist interests dredged up.

Secondly, Hyman's characterization of Wallace's psychical audience as a "world of individuals who were outcasts or non-entities with respect to the scientific establishment" shows ignorance of the intellectual distinction of the leadership of the Society for Psychical Research, for instance, which included both outstanding scientists and others like Henry Sidgwick who certainly held the esteem of the scientific and academic communi-

* Malcolm Jay Kottler, "Alfred Russell Wallace, the Origin of Man, and Spiritualism," Isis, 65 (1974), 145-192.

ties. This is not to deny that the spiritualist-psychical research movements encompassed (and still do) socially, professionally and intellectually diverse sets of people, but Hyman's oversimplification can only appear tenable to those who must assume what they wish to see proven.

Beyond my professional objections as an historian of science to Professor Hyman's scheme, I fail to see how it furthers the cause of science today. Professor Hyman seems to be most exercised over parapsychological claims (along with therapeutic claims for ascorbic acid). But what is so terrible about a scientifically trained person taking an interest in these claims? Why must he be insulated ideologically from them? And finally, to play devil's advocate: While I do applaud Hyman's call for a fuller and more equitable response to "pathologies," I would query why the scientific community should bother to invest more of its resources to answer pathological claims if it has indeed prejudged them as such to begin with? Such investment could only be worthwhile in the expectation that reasoned, careful consideration of such claims might yield concrete scientific advance. But I presume that Professor Hyman himself has no such expectation; otherwise he would not have placed these claims under the rubric of "pathologies."

If parapsychology indeed be a "pathology" (though how we are to know except in hindsight of future knowledge escapes me), then better to conserve scientific resources and continue the traditional indifference and inattention of the greater part of the scientific community. But, of course, if there should prove to be something to parapsychological claims, then Professor Hyman's scheme would be worse than useless; it would be pernicious. And he himself might well face the prospect of being dubbed "pathological" by a future policeman of scientific thought, much as he has stigmatized the opponents of meteors of two centuries ago.

COMMENTS BY ANDY PICKERING:

In his paper, Ray Hyman defends two propositions concerning anomalous observation reports in science.¹ The First is that "Good science requires good and effective criticism," and that therefore unusual claims should not be denied access to the scientific community simply because they are unusual. Rather, they should be as widely discussed as possible, in order to facilitate constructive criticism. This proposition I entirely agree with, and I will not discuss it further. However, there is a second proposition - or, better, presumption - which permeates Hyman's argument, and with which I wish to take issue. This presumption, which I believe to be false, is crystallized in the following quotation: "If 'pathologies' do exist in the sense that some of our best scientists defend bizarre positions, then like all sicknesses, they are a symptom of something. Something is wrong and requires remedy."

That this argument is false can be shown through counter-examples. Bizarre positions often prove to be justified. For instance, it is well-known that there were many reported sightings of stones falling from the sky - i.e. meteorites - before this bizarre idea was clasped to the bosom of scientific orthodoxy.² A second,

possibly less well-known, example, which I will discuss below, concerns the first observation of parity violation, which was made in the 1920s, although the existence of parity violation did not become established until the 1950s. One could extend the list of "prediscoveries" indefinitely, and, since the bizarre positions of the prediscoversers have since become part of present-day reality, one would surely not wish to say that their defence of those positions was a symptom of any sickness.

Now, how does the observation of stones falling from the sky differ from Alfred Russel Wallace's reports of the materialization of six-foot sunflowers and the like, and from Langmuir's "pathological science" in general? In nothing more, I would suggest, than that meteorites are now accepted belief while psychic phenomena, and so on, are not. It is noteworthy, in this respect, that throughout his paper Hyman repeatedly commits the very sin against which he is preaching, by denying the validity of claims such as Wallace's without a shred of argument. Thus, in order to decide what a reasonable response to bizarre claims should be, one has to ask whether, in general terms, one can understand (a) what enables one to perceive a claim as bizarre, and (b) how it is that bizarre claims sometimes achieve normality. Both of these questions, I suggest, are readily answered if one recognizes the role of scientific theory: it is in terms of some accepted theory that one distinguishes between bizarre and normal phenomena, and it is change in accepted theory which mediates the transformation between anomaly and orthodoxy.

To illustrate what I mean by this, let me refer very briefly to the history of parity violation.³ In the late 1920s a group of experimental physicists at New York University reported the existence of an asymmetry in the double-scattering of electrons. The precise meaning of these technical terms is not important here; what is important is that the asymmetry could find no interpretation in the physical theories of the day. Thus, in the specific theoretical context of the late 1920s, the report of the asymmetry was seen to be bizarre, and it was eventually dismissed, even by the experimenters themselves, as due to some unidentifiable defect in the experimental procedures. For almost thirty years this experiment suffered the fate of Wallace's sunflower. Then, in the 1950s, anomalous results started to appear in experimental elementary particle physics, and, at the brilliant suggestion of theorists T.D. Lee and C.N. Yang (for which they shared a Nobel Prize), these results were recognized as manifestations of parity violation. In the context of this dramatic theoretical development it became readily apparent that there was no reason to believe that the work of the New York experimenters was defective, and that, in fact, they had been observing a straightforward consequence of parity violation. The theoretical work of Lee and Yang had transformed their incongruous observations into a commonplace.

What, then, are we to learn from episodes such as this. Firstly, that what counts as an anomalous observation and what is run-of-the-mill is a function of the contingent theoretical context in which the observations are made. Secondly, that the transformation of

anomaly is also a contingent matter: not every anomaly can expect to find its Lee and Yang, and, there is no way to legislate for such transformations in advance. If at some particular time no such conciliatory hypothesis has been forthcoming in respect of a given anomaly, that cannot be construed as evidence that such an hypothesis will never appear. It is, of course, possible to recommend further experimental investigation of the anomaly itself and of the techniques involved in its initial production, but the idea that such investigations can be conclusive is never, in principle, true, and seldom if ever in practice.⁴ For just one illustration of this, let me return to my previous example. When physicists set out to explore the existence of the reported asymmetry in electron double-scattering, they noted that the original experiments had been done with a rather low-powered source of electrons, and, in order to see any novel effect more clearly, a more powerful source was used in subsequent experiments. No effect was found, making the original report appear to be highly implausible. The change of sources was unfortunate, one can say in retrospect, because the original experiments used electrons emitted in the weak decay of a radioactive substance, while the "replications" used a beam derived from thermionic emission from a hot wire. The improvement was actually a step backwards, since we now recognize that parity violation occurs only in the weak interactions which lead to the emission of electrons from radioactive substances, and not in processes such as thermionic emission. Since the weak interactions were not distinguished as an independent force at the time, the recognition of this would have required an even greater shift in the theoretical context than that of parity violation alone.

The conclusion of this discussion, then, is that anomaly is simply a function of the contingent theoretical milieu within which phenomena are reported. It is clear that scientists should not seek to obliterate anomalous reports from their consciousness, and that the constructive response to such reports is to investigate the possibility of mutual reconciliation through theory;⁵ but, beyond this, it seems misguided and misleading to ask, as Hyman does, for one specifically rational method for the diagnosis and remedy of pathology. Anomalies are constituted by their context, not by any intrinsic, peculiarly "pathological," attributes. A corollary which follows from this is that one of the most pathological features of the contemporary scene, which stands sorely in need of explanation, is the concept of "pathological science." But that is another problem.

NOTES:

¹R. Hyman, "Pathological Science: Towards a Proper Diagnosis and Remedy," Zetetic Scholar, this issue.

²See, Ron Westrum, "Science and Social Intelligence about Anomalies: The Case of Meteorites," Social Studies of Science, 8 (1978), 461-493.

³For good extended accounts of this episode see, "Discovery of Parity Violation in Weak Interactions," in B. Maglich, ed., Adventures in Experimental Physics (Princeton: World Science Education) 3 (1973), 93-162; and A. Franklin, "The Discovery and Nondiscovery of Parity Nonconservation," Studies in History and Philosophy of Science, 10 (1979), 201-257.

⁴For work which argues the point of principle and illustrates its working-out in practice, see, for example, my "The Hunting of the Quark: The Experimental Method in Science," Isis (forthcoming), and references therein.

⁵For case-studies and analysis of the relevant processes of transformation in scientific theory, see my op.cit., and "The Role of Interests in High-Energy Physics: The Choice Between Charm and Colour," to appear in R.D. Whitley (ed.), Sociology of the Sciences Yearbook, Vol. 5 (Dordrecht and Boston: Reidel, 1981).

COMMENTS BY THEODORE ROCKWELL:

Ray Hyman offers a fresh approach to the important question: How should scientists react to an outrageous scientific claim? I agree with nearly everything he says, except for his most basic, unstated premise. I think he reaches the right answer for the wrong reason.

He describes some of the greatest minds in the history of science--Newton, Lodge, Crookes, Wallace--devoting major efforts to investigations which lead them to believe in the possibility of psychic phenomena. He admits these conclusions have never been rationally examined by the scientific majority. (He does not note that their conclusions are remarkably consistent with each other.) And then he jumps to the unexamined position that these great men's findings were "follies." He gives no indication that they might have something.

He describes Lodge being listened to with respect concerning his discovery of thallium, his work with cathode ray tubes and his invention of the radiometer. Yet, he notes, this same great mind is ignored when he talks of his work on psychic phenomena. Having never even suggested--let alone demonstrated--any basis for concluding Lodge and the others were wrong about psychic phenomena, Hyman then chides present day scientists who are similarly intrigued by such phenomena, and argues that they must also be foolish. He has rationally analyzed every aspect except the crucial one: Might these men, and many others, be wholly or even partially right?

I have no quarrel with Hyman's recommendation. He asks scientists to act like scientists when faced with a scientific claim. He argues that to do otherwise damages science. I agree. But I disagree that the task is particularly "difficult and demanding." First, we have to clear away some distracting debris. We are talking about science, not horoscope columns, card tricks or fortune tellers. Some critics insist on mixing these, and this is inexcusable. Criticism is part of the scientific process and I agree with Hyman that such criticism is pseudoscience or pathological science. If some people prefer horoscopes to cross-word puzzles or fortune tellers to football games, it is no business of scientists to interfere.

But, we are talking here about experimental or theoretical papers in the classical mold, unorthodox only in the subject under investigation. The rules for handling such papers are well understood. First, they require peer review. This implies re-

view by persons personally competent in the field, although an occasional scientist from a related field may be used in addition. Second, it requires that the reviews and the responses to the authors be fully professional. No shortcomings in the paper under review can excuse non-professional or uncivil work by reviewers, editors or program sponsors. If papers do not meet well-understood scientific standards, it should be an easy matter to state specifically and clearly wherein this occurs.

I find it disgraceful that Science has never run a serious research paper on psychic phenomena, yet it devotes space to improperly reviewed and sweeping denunciations of the field, derogatory comments by itinerant magicians, and side-of-the-mouth editorial jibes.

So I agree with Hyman: Let's treat these claims in a straightforward, scientific manner. This includes the right of most scientists to remain indifferent. But the work should go through the regular process. Bad papers should be rejected for clearly stated and valid reasons. Good ones should be published. Criticism, evaluation and attempted replication should follow the usual course (which does not include public demonstrations before magicians). I expect this would lead to one or two major articles a year in Science, a couple of research notes, a news item or two, and an occasional letter and a book review. This would probably take less effort and no more space than Science's present anti-psychic campaign.

Evaluating such work under normal scientific procedure also includes sincere attempts to replicate. In normal science, one does not suppress the original research and then rush hasty failures-to-replicate into print. Failure to replicate is easy-- anyone can do it. Randi makes a living at it. All it means is that the original conditions have not been duplicated. The real scientist tries to understand what conditions are critical to achieving the reported phenomenon. To assume that every unusual event has been produced by fraud is a cop-out. The claim that psychical research is peculiarly vulnerable to fraud is over-worked. The record of fraud and failure to predict results is equally bad in many accepted fields of research, e.g. cancer and hypnosis.

So, let us proceed to use the scientific method. If Hyman is right, we may learn why so many brilliant scientists each made the same foolish mistake. But Nature has a way of surprising those who too confidently try to predict her ways, and we may learn something quite different, and considerably more important.

COMMENTS BY PAUL THAGARD:

Ray Hyman's discussion of pathological science is a valuable contribution to what might be called the "political philosophy of science". In judging how to deal with disciplines such as astrology, parapsychology, and Velikovskian cosmology we face both methodological questions of validation of scientific theories and political questions of appropriate practical reaction to those disciplines which are judged methodologically inadequate. Hyman argues that even obviously bizarre and outrageous science (or pseudoscience) should be subject to the best sort of scientific criticism. To exclude pathological science by means of sketchy, dogmatic dismissal is to lose the opportunity to have science learn from its mistakes.

I have much sympathy with Hyman's liberalism, and agree that such fields as parapsychology and astrology should receive less peremptory criticism than they have generally received. After all, they could turn out to have some validity. If we are not to violate C.S. Peirce's injunction not to block inquiry, then it seems necessary to give careful attention even to theories which, because they conflict with accepted theories or lack verification, are judged to be pathological.

However, Peirce's liberal principle of not blocking inquiry may, in the case of pathological science, come into conflict with another of Peirce's principles: the economy of research. Peirce stressed that a leading consideration in deciding what hypotheses to investigate is economy - of money, time, thought, and energy. Our intellectual resources are limited with respect to all these factors, so it becomes a legitimate methodological question whether the careful investigation of pathological science is worth the effort. No general answer is possible: we will have to decide in each case whether systematic investigation of a claim judged to be without scientific merit is worth the expenditure of time and energy subtracted from other pursuits. We have to at least allow the possibility that for reasons of economy a merely superficial dismissal of a discipline is in order.

There is another reason why in some cases Hyman's liberalism may be unwarranted. This is the social cost of rubbish. Fields like astrology have some fairly serious investigators, but, independently, they have a social impact which goes far beyond their minimal scientific content. For deplorable social reasons, people embrace astrology as a guide to life, with personally and socially undesirable results. It thus becomes a social question how best to combat irrationalism. Polemics may well be more socially effective than detailed criticism.

PROFESSOR RAY HYMAN WILL RESPOND TO HIS COMMENTATORS IN THE NEXT ISSUE.
OF ZETETIC SCHOLAR.

FAILURES TO REPLICATE REMOTE-VIEWING
USING PSYCHIC SUBJECTS*

EDWARD W. KARNES, ELLEN P. SUSMAN,
PATRICIA KLUSMAN, AND LAURIE TURCOTTE

The controversy concerning the existence of psychic or paranormal human abilities has been a subject of debate for many years. Lately, the controversy seems to have intensified as evidenced, on the one hand, by attempts to provide scientific support for psychic abilities (e.g., Goodman, 1977; Moss, Chang, and Levitt, 1970; Shealy, 1977; Targ and Puthoff, 1978; Valle, 1975; White and Kribbner, 1977), and on the other hand by the development of societies and journals concerned with critical evaluations of claims of the paranormal (e.g., The Committee for Scientific Investigation of Claims of the Paranormal, The Skeptical Inquirer, and the Zetetic Scholar.)

One phenomenon in particular, remote-viewing, has received wide publicity as "scientific proof" of the existence of psychic abilities. Simply defined, remote-viewing involves the ability of a person (a receiver) physically separated from another person (the sender) to describe the surroundings of the sender without prior knowledge of the sender's location.

The scientific respectability claimed for remote-viewing lies, in part, with the apparent rigor with which the experiments were conducted and, in part, on the scientific credentials of the two principal researchers, Russell Targ and Harold Puthoff, who are physicists and not parapsychologists. The original remote-viewing experiments were conducted by Targ and Puthoff at Stanford Research Institute (now SRI International) and were published in journals outside of the field of parapsychology, Nature (Targ and Puthoff, 1974) and Proceedings of the IEEE (Puthoff and Targ, 1976).

Replication of experimental results is a basic requirement for scientific acceptance of a phenomenon. Targ and Puthoff, (1978) have claimed numerous successful replications of remote-viewing using both experienced psychic subjects and persons with no previously known psychic abilities. From their results, the authors have concluded that extrasensory remote-viewing may be widespread in the general population.

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The replication requirement, however, involves not only repeatability of experimental results within a given laboratory, but also, replication of the findings by independent investigators. As noted by Moss and Butler (1978), the demand for replication is particularly crucial in an area where the findings appear to violate well-established physical laws. Replication by impartial or even nonsympathetic investigators is the only guard against results which may be contaminated by biases due to subjects, experimenters, or both.

It is in regard to replication by independent investigators that the scientific acceptance of remote-viewing becomes questionable. While successful demonstrations of remote-viewing by independent investigators have been reported (e.g., Bisaha and Dunne, 1977; Hastings and Hurt, 1976; Whitson, Bogart, Palmer and Tart, 1976; and Vallee, Hastings, and Askervold, 1976), failures to replicate the phenomenon have also been reported (e.g., Allen, Green, Rucker, Cohen, Goolsby, and Morris, 1976; and Rauscher, Weismann, Sarfatti, and Sirag, 1976). In some cases, specific information concerning the replication failures was not provided, e.g., Marks and Kammann (1978) in their evaluation of extraneous cueing in Puthoff's and Targ's original remote-viewing transcripts simply reported their inability to replicate the phenomenon. Robert Ornstein, in his review of Targ's and Puthoff's book Mind Reach published in the New York Times (March 13, 1977, p. 24), claimed that he was unable to repeat one of Puthoff's and Targ's experiments even though he had the same subjects they used and the full cooperation of the authors. Butler (Note 1) has also reported unpublished negative data on remote viewing.

In two previous experiments (Karnes and Susman, 1979, and Karnes, Ballou, Susman, and Swaroff, 1979), we too have been unable to obtain evidence for a remote-viewing ability in samples of selected college students. In the first study, we examined the reliability of remote-viewing by using a signal detection procedure to objectively measure the receiver's responses. The second experiment was an attempt to specifically replicate Puthoff's and Targ's results using their experimental procedures. In both experiments, control conditions were used to evaluate the guessing/frequencies (response biases) of potential targets. The results of both studies yielded absolutely no support for a remote-viewing hypothesis and were consistent in rejecting any possible psychic interpretation of the infrequent correct judgments by showing that successes could be accounted for by response bias factors (e.g., guessing).

The present experiment was designed to provide an additional test of remote-viewing by using a sample of professional and semi-professional psychic subjects and by providing multiple remote-viewing trials with feedback for sender-receiver pairs. It had been suggested that our previous failures to replicate remote-viewing may have been due, in part, to the limited number of trials (one in the first and two in the second experiment)

afforded the sender-receiver pairs. Since improvement in performance of any human ability should be expected as a function of practice with knowledge of results, reliable remote-viewing performance may require more than two trials. The present experiment was designed to afford sender-receiver pairs several remote-viewing trials.

It had also been suggested that samples of selected college students may not be entirely appropriate for demonstrating reliable remote-viewing. That suggestion has some merit since the psychic awareness of self-proclaimed psychics should be stronger than that of inexperienced subjects. It is reasonable to assume that reliable remote-viewing should be more easily demonstrated with experienced psychic subjects than with inexperienced subjects; therefore, the present experiment was conducted using self-proclaimed psychic subjects who claimed to have had repeated and successful experiences with paranormal communications.

The experiment was also designed to permit an accurate recording of the sending experience. In our previous studies of remote-viewing, it became apparent that a possible problem in obtaining positive evidence for remote-viewing concerned differences in the situation that existed at the target site when the sender was "sending" his/her impressions and the situation that existed at a much later time when judges visited the target site. The most efficient basis for a judge to establish a correspondence between a receiver's protocol and a target site would be not only by a visit to the target site but also a review of an accurate record of the sender's impressions obtained at the target site during the actual sending situation. Records of the sending experiences were obtained in the present experiment by requiring the senders to record their visual impressions (by using a motion picture camera) and their subjective impressions (by verbal recording using a tape cassette). Independent judges used the records as well as visits to the target sites to judge the accuracy of the remote-viewing.

Method

Design.

Eight self-proclaimed psychic subjects, self-selected into sender-receiver pairs based on their previous experience with psychic communications, participated in several remote-viewing trials. Two pairs of subjects were each given ten remote-viewing trials. The other two pairs of subjects were each given six remote-viewing trials. Sender-receiver pairs alternated roles as sender/receiver for the 6 trials.

The following procedures from previous successful demonstrations of remote-viewing were used: (1) An experimenter was closeted with the receiver during the sessions; (2) A double-blind procedure was used in that the experimenters and receivers had no knowledge concerning the number or identity of the target

possibilities; (3) Receivers recorded their impressions by offering narrative, stream of consciousness verbal reports and drew free-hand sketches of their visual impressions; (4) Feedback was provided by having the sender debrief the receiver at the target site immediately following each trial. The debriefing session was an open dialogue between the sender and receiver in which they compared the correspondence between their sending and receiving impressions.

Sixty four independent judges evaluated the accuracy of remote-viewing by comparing the receiver's protocols to the actual target sites and to records of the senders' experiences. Records of senders' experiences included a color movie of the sending situation and typed narrations of the senders' tape-recorded verbal impressions of their sending situations.

Subjects.

Four males and four female subjects having professional or semi-professional involvements in psychic matters volunteered to serve as senders and receivers in the study. Their mean age was 41 (range 30 to 65). They had a mean number of 15.88 years of formal schooling (2 Ph.D.'s, 1 ordained minister, 2 B.S.'s, 1 A.A., and 2 high school graduates). The mean number of years of professional or semi-professional involvement with psychic matters for the group was 8.38 (range 3 to 20).

The subjects were members of a group formed to explore paranormal experiences. The group was formed by the Whole Life Learning Center in Denver, Colorado and was associated with the Colorado Holistic Health Network. Members of the group met on a frequent basis (several times monthly) and participated in psychic development exercises including trance regressions, psychometry, psychic communications with one another, programmed dreaming, and psychokinesis. All of the subjects had either taught courses or had led groups in psychic development.

The subjects' interest in and their basis for volunteering for the present study resulted from a verbal presentation by the principal author on the remote-viewing research. All subjects felt confident about their abilities to successfully demonstrate remote-viewing based on their previous successful experiences with paranormal communications. The 8 subjects self-selected themselves into sender-receiver pairs based on their success rates in prior psychic communication exercises.

Target Sites.

Sixteen distinctively different target sites were used for the 16 remote-viewing trials. The 16 targets included 6 indoor and 10 outdoor sites. The 6 indoor sites were: a church interior; a large lobby; a classroom; an indoor swimming pool; a cafeteria; and a library room. The 10 outdoor sites included: a pedestrian overpass in downtown Denver; a Greek amphitheater; a city park with distinctive

sculpture and waterfalls; a set of tennis courts; the Colorado State Capitol entrance; a redeveloped set of historical houses; an enclosed pedestrian plaza; Larimer Square, a redeveloped area in downtown Denver; a Japanese plaza (Sukura Square); and a large parking lot.

Procedure.

The order of target sites to be used for the trials was determined by a table of random numbers. The principal author was the only person aware of the identity of the target sites and the order of use. Subjects were contacted by phone and were scheduled for the experimental sessions at their convenience. Assignment of target sites for each sender-receiver pair was determined by order of appearance.

Sender-receiver pairs met the principal author at his office. They were read a set of instructions which explained the procedures to be followed for the experimental sessions. Each sending-receiving session was 15 minutes in duration.

Receivers were isolated with an experimenter in a small quiet conference room. Receivers tape-recorded their impressions and drew free-hand sketches of their visual impressions. The experimenter's role was to remain essentially unobtrusive but to question the receivers if they were hesitant to offer comments or to ask for clarification concerning ambiguous or unclear comments made by the receivers. Receivers had the option of not having the experimenter in the room during the receiving sessions if they so desired.

Senders were escorted by the principal author in his auto, or by walking, to the target site. They were instructed to use a Polaroid Motion Camera to visually record their impressions of the target site and to verbally record their subjective impressions by using a portable cassette tape recorder. They were given the option of using the camera and recorder either during the 15 minute sending session or immediately after the session if they thought that the recording requirement would interfere with their sending concentration. All senders opted to use the recorders during the sending sessions. The sender was left alone during the sending session.

At the end of each sending session, the receiver was escorted to the target site by the principal author where he/she met the sender. The sender debriefed the receiver by explaining what he/she had been trying to convey. There were no restrictions placed on the dialogue between the sender and the receiver. Following the debriefing session the receiver returned to the conference room and the sender was escorted to another target site by the principal author for the next trial.

The sender-receiver pairs that participated in two remote-viewing trials had the same sender and receiver for both trials. One pair of subjects had both trials on one day. The other pair had seven days between the two trials. The two sender-receiver pairs that were given 6 remote-viewing trials alternated roles as senders and receivers. They exchanged roles after trials 1 and 2, i.e., on trials 3 and 4, and

alternated roles again on trial 5 and 6. All subjects were given two remote-viewing trials on a single day's session.

A professional transcriber typed the receivers' and the senders' narrations. The experimenter and the principal author checked the narrations for accuracy by comparing them to the tape-recorded records. Receiver protocols were prepared by including the free-hand sketches along with the typed narrations. Receiver protocols were identified by a randomly determined code letter (A-P). Sender protocols were identified by a randomly determined number (1-16). In preparing the receiver and sender protocols, all references to personal names, dates, times and gender references to the sender or receiver were carefully edited out to eliminate any extraneous clues that judges could use to evaluate the accuracy of the remote-viewing.

Judging

Sixty four independent judges evaluated the accuracy of the remote-viewing data. Judges were read a set of instructions which explained the purpose of the experiment and how they were to judge the accuracy of remote-viewing. Judges were run in groups of four or less. Each of the 16 target sites was evaluated against the entire set of 16 receiver protocols by four judges. Each judge was given one sender's description to read. After reading the narration, judges were shown the movie taken during the sending session followed by a visit to the actual target site. Judges were then given the entire set of 16 receivers' protocols and were required to separate the 8 that best matched the target from the 8 that least matched the target. Judges rank-ordered the 8 matches with 1 used for the best match through 8 for the least best match. Judges were allowed unlimited time including overnight to complete the judging.

Results

A hit will be defined as a judge's selection of the correct receiver's protocols for a target site. Since the judges were required to select the 8 best matches from among the set of 16 receivers' protocols, the chance proportion or hits for the 64 judges was .50. Evidence supporting remote-viewing would be obtained if the proportion of hits was reliably greater than chance expectations. The 64 judges obtained 25 hits. That proportion ($25/64 = 0.39$) was not significantly different from chance, $Z = -1.76$, $p > 0.08$.

Since the judges were required to rank-order the 8 receiver protocols that best matched each target site, a remote-viewing hypothesis would be supported if the mean rank-order value assigned to the 25 hits was reliably better than chance expectations. Chance expectation was computed as 4.5, the mean of the ranks 1 through 8. The mean rank-order value assigned to the hits was 4.36; that value was not reliably different from 4.50,

$t(24) = 0.48, p > 0.60.$

The effects of feedback on improving remote-viewing performance was evaluated by comparing the mean rank-order assigned to judgments of the correct receivers' protocols for the targets across the trials for the 4 sender-receiver pairs of subjects. Support for the hypothesis that feedback facilitates remote-viewing would be obtained if the judgment means decreased as a function of practice (trials). In the feedback analyses, the rank-order value of 12.5 (mean of the ranks 9 through 16) was assigned to misses (failures to select the correct receiver's protocol among the 8 best matches). Two sender-receiver pairs had only two remote-viewing trials. The mean rank-order values assigned to the correct receivers' protocols on the first and second trials was 10.39 and 11.29 respectively; that difference was not reliable, $t(14) = 0.70, p > 0.45.$

Two sender-receiver pairs had 6 remote-viewing trials. The mean rank-order values assigned to the six trials were 9.0, 8.5, 10.38, 9.89, 9.50, and 11.13 respectively; those differences were not significantly different, $F(5, 42) = 0.42, p > 0.50.$

While none of the previous analyses supported remote-viewing or feedback hypotheses, it is possible that one or more instances of accurate remote-viewing occurred in the experiment and that their occurrence may be obscured in the overall analyses. One sender-receiver pair, on their first remote-viewing trial, did appear to perform remarkably well in the judgments of the four independent judges who evaluated their data. The target was the pedestrian overpass in downtown Denver. The four judges of that target each scored hits (i.e., they selected the correct receiver's protocol from among the set of 16 receivers' protocols), and they selected the correct receivers protocol with a high degree of accuracy. The rank-order values assigned to the hits were 1, 1, 1, and 4.

Inspection of the entire data set revealed that the receiver's protocol that corresponded to the pedestrian overpass target also had a very high false-alarm rate, i.e., it was selected as a matching description by 62 of the 64 judges. To determine if that receiver's protocol had a higher selection rate than the other 15 receiver protocols, its proportion of selections ($62/64 = 0.97$) was compared to the combined proportion of selections for the remaining 15 receiver protocols ($450/960 = 0.47$). The difference was significant, $Z = 7.75, p < 0.001.$

The receiver's protocol for the pedestrian overpass target was also selected as a very accurate match to the other targets by most of the judges. The mean rank-order value assigned to its 62 selections was 2.54. The mean rank-order value assigned to the 450 selections of the other 15 receivers' protocols was 4.76. The difference was significant, $t(510) = 7.51, p < 0.001.$

Discussion

The results of the experiment offered no support for the existence of a remote-viewing paranormal perceptual capability in a group of experienced psychic subjects. Both the proportion of hits (0.39) obtained by the 64 independent judges and the rank-order values assigned to hits ($\bar{X} = 4.36$) were not significantly different from chance expectations. In fact, the obtained proportion of hits was in the opposite direction of that predicted by a remote-viewing hypothesis. The results also failed to provide any support for a hypothesis concerning the efficacy of feedback in remote-viewing procedures. Sender-receiver pairs failed to demonstrate improvement across the two or the six trial situations as evidenced by the mean rank-order values assigned to the correct receivers' protocols on each trial.

The failure to obtain positive evidence for a paranormal remote-viewing perceptual capability is totally consistent with previous investigations of the phenomenon in our laboratory. In fact, the data are extraordinarily consistent in that regard. Our first study of the phenomenon (Karnes and Susman, 1979) was designed to address what we considered to be methodological problems in the original experiments on remote-viewing. We modified the procedures used in Puthoff's and Targ's original experiments by requiring the receivers to identify the sender's location from among photographs of the potential target sites. That signal detection procedure permitted an objective measurement of the success rate in remote-viewing data. We also included a control condition designed to measure the guessing frequencies of potential target sites.

Puthoff (Note 2) in a subsequent review of our first study, claimed that the failure to obtain positive evidence for remote-viewing was attributable to the differences in procedures. He contended that: receivers are the poorest judges of the accuracy of their own remote-viewing performances; requiring them to select photographs would most certainly guarantee a null result because it would interfere with "analytical" functioning; the lack of feedback degraded remote-viewing; and finally, the control condition as designed did not permit an accurate measurement of the guessing frequencies of potential target sites.

Puthoff's review served as the basis for designing the second study (Karnes, Ballou, Susman, Swaroff, 1979). In that study we: (1) used the procedures employed in successful demonstrations of the phenomenon; (2) compared the abilities of receivers and independent judges to judge the accuracy of remote-viewing data; (3) designed and used another control condition to evaluate the guessing frequencies of potential targets. The result of the first and the second were remarkably consistent in failing to provide any hint of support for remote-viewing. In fact, the control conditions, while providing a basis for the evaluation of guessing frequencies, were unnecessary in arriving

at conclusions concerning the unreliability of remote-viewing. That is, we were unable to obtain any statistical support for remote-viewing using the same kind of data comparisons as performed by Puthoff and Targ (1976).

The results of the two experiments were also consistent in discrediting a psychic interpretation of the infrequent successes in judgments of remote-viewing by demonstrating that successes could be accounted for in terms of response bias differences among targets or individual judges, or both. That is, targets on which hits (successes) occurred had reliably higher guess rates (false alarms) than targets on which hits did not occur, and/or judges who scored hits were reliably more prolific guesses than judges who did not score hits.

If remote-viewing is a viable perceptual capability as claimed by Targ and Puthoff (1978), the results of the present experiment should certainly have provided some support to that hypothesis. Consider the following. First, the subjects claimed to have had many years of successful experiences with various sorts of paranormal phenomena. Second, the subjects self-selected themselves into sender-receiver pairs based on their previous successes in psychic communications. Third, the procedures used in successful demonstrations of the phenomenon were strictly followed including multiple trials with feedback. Fourth, records of the sending situation as well as visits to the target sites were provided for judging the accuracy of the remote-viewing performances.

We are at a total loss to explain the discrepancy between our results and the successful demonstrations of remote-viewing. The results of the present experiment and our previous investigations of the phenomenon are remarkably consistent and close to what would be predicted on the basis of chance. In our opinion the rejection of any conclusion supporting the viability of the phenomenon is inescapable.

There is, however, one very important possibility which may account for the difference in results between our study and successful demonstrations of the phenomenon. That possibility concerns extraneous cues that may exist in the remote-viewing transcripts. The possibility of extraneous cues has been raised by Marks and Kammann (1978) - but it has also been refuted by Puthoff and Targ in a reply submitted for publication to Nature. In our study, any possible extraneous cues that could be used by the judges to evaluate the accuracies of the remote-viewing transcripts were carefully edited-out. References to personal names, dates, times, and the gender identification of the senders and receivers were removed from the transcripts. In the absence of such cues, the independent judges' abilities to successfully match sender-receiver data in our experiment were remarkably close to chance expectations.

One final point should be mentioned. The dangers in selectively citing or ignoring portions of a set of data were especially evident in the results of the present experiment. One receiver's transcript did appear to be a remarkably close match to the target site in the judgments of the four independent judges who evaluated those data. The target was the pedestrian overpass and the judges all identified the correct receiver's protocol with a very high degree of accuracy - rank-order values of 1, 1, 1, 4. That finding taken outside the context of the entire set of data, could indicate highly successful remote-viewing.

When all of the data were reviewed, however, the apparently successful remote-viewing performance could be explained in terms of guessing - or response bias factors. The correct receiver's protocol for the pedestrian overpass target site was also selected as a correct match for the other 15 targets by 58 of 60 judges who evaluated those 15 targets. Its rate of selection as a correct match was reliably higher than the selection rate of the other 15 receivers' protocols. In addition, the mean rank-order value assigned to selections of that receiver's protocol was reliably lower than the mean rank-order value assigned to selections of the other 15 receivers' protocols. The success of the sender-receiver pair for the pedestrian overpass is certainly not surprising in light of these facts.

In conclusion, the results of the present study and their consistency to the results of our previous investigations of remote-viewing raise serious questions concerning the reliability of the phenomenon. We have attempted to replicate the phenomenon using: (1) procedural modifications as well as the procedures prescribed by Targ and Puthoff; (2) selected college student samples and a sample of experienced psychic subjects; (3) accurate records of the sending situation; and (4) very large groups of independent judges. In every instance, statistical support for the existence of a paranormal perceptual capability was totally nonexistent. We cannot account for the successes of others, but we are confident that no paranormal perceptual capabilities were demonstrable in our investigations.

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Notes

1. Butler, D. C. Personal Communication, September 12, 1979.
2. Puthoff, H. E. Personal Communications, March 28, 1978.

COMMENTS BY JAMES CALKINS:

I was very pleased to read the present Karnes, et. al., (1980) study of remote viewing. It has been a few years since I involved myself with the remote viewing studies published by Puthoff and Targ first in Nature (1974) and then in the Proceedings of the IEEE (1976a). The latter publication so offended my methodological sensitivities that I wrote a very critical letter (Calkins, 1976) outlining in some detail why control conditions of various kinds would be required in this type of study, the basic example being an empirical non-remote viewing condition to control for guessing, experimenter bias, cueing of various types, etc. The Karnes group obviously has experimentally taken Puthoff and Targ to task along these same lines. But what particularly exasperated me was the thought that Puthoff and Targ could feel they could use fewer and less rigorous controls in a psychology study than in their own physical science research, and at the same time do so in a situation which is proposing a revolutionary theory about the very essence of the relationship between the brain and perception. For example, I am sure they would find it nonsensical reading of my experimental determination that laser rays are really just electrons without any mass whatsoever, or that Ohm's Law really is $R=EI$, and, what is more, I made this discovery in one basic experiment. My point is that psychology has been researching the interrelationships that exist between perception and the brain for over 100 years now, and has built up an increasingly elaborate explanatory model of the role of sensory systems and the brain in the control and elaboration of perception, learning, memory, motivation and emotions. Even a casual reading of a basic undergraduate psychology text tells us that much. But here we have a theory of perception presented to us almost totally unrelated to our present general psychological models of the interrelationships between brain and behavior, all of which models require that information arriving in the cerebral cortex be first encoded into patterns of nerve impulses by the sensory organs, and that requirement certainly includes the occipital lobes' role in the production of complex visual perceptions of the sort studied in the Puthoff and Targ experiments. I can only say that, in this context of a revolutionary discovery that greatly changes our understanding of the nature of perception and its relationship to the nervous system, studies on remote viewing have got to be especially logic-tight and conservative in their experimental design so that any positive results are clearly and convincingly demonstrated. Instead, what we get from Puthoff and Targ are alleged studies that do not pass the requirements of routine experiments in undergraduate teaching laboratories in psychology, so that you have numerous confounding variables present that make it impossible to conclude that clairvoyance or any other psychic effect is present. I think they confuse demonstration with experiment, and control of conditions with a basic control condition.

More specifically, if we turn to either the original Nature article, or the somewhat more recent IEEE Proceedings article, we encounter essentially what psychologists would probably call the "one-shot case study," that is, an elaborate procedure that essentially constitutes a single condition of what otherwise would be an

experimental study--an "experimental" condition, as it were, without the corresponding basis for comparison we all know as the famous "control" condition concept (or control group, if an independent subjects' design is used).

I can tell you I continue to find it frustrating in the extreme to appear on radio and TV talk shows with parapsychologists and psychics who proceed to throw at me these Puthoff and Targ so-called "experiments" (and others, like the studies from the Maimonides Dream Lab) as evidence that there is indeed scientific proof that clairvoyance and telepathy exists! My protagonists are quick to point out the prestige of the journals in which the publications appear (no matter what non-sense might be printed), the scientific status of the researchers (no matter how irrelevant for psychological research), and the sophistication of the equipment used (even if it has little usefulness in the reported study).

For example, years ago a hypnotist and I were on a WCAU Philadelphia radio talk show, "Psychic World," moderated by Mr. Barry Magarick and produced by Ms. Susan McAninley, where the hypnotist claimed he routinely was able to hypnotize people remotely, at kilometer distances! Interestingly, this phenomenon has some classical basis in the history of 19th century psychology in the work of M. Pierre Janet. Apparently, Janet (1968; orig. 1885) considered this alleged effect a form of Somnambulism that could be induced hypnotically and at a great distance. It is not clear that he believed this remote telepathic induction was an established fact, since, he observed, he was not certain how many times each day the subject might become lathargic, and in effect hypnotized anyway! This is exactly the point I tried to make with our believing hypnotist, that we did not know how often his sending a signal, and her passing out was a non-causal coincidence! We mutually agreed to take the problem to the laboratory. There, we specifically set up a series of tests in which the hypnotist was not sending the signal (a control condition) as well as was sending the signal (an experimental condition). It turned out, as we expected, that the subject went into her trance the same number of times when he was both allegedly telepathically sending and not sending the trance-inducing signal.

This same comparison argument applies to the innumerable PK dice-throwing studies in which the psychic concentrates, say, on getting "5s" as the dice are thrown out by a tumbler. How often is the psychic's success based on evidence for the PK effect in which the number of "hits" is compared to chance expectation rather than an empirical control condition? For example, if the psychic were trying for the 5s, and got 500 of 600 die thrown out to be 5s, we would be mightily impressed because this would occur by chance extremely infrequently--unless, of course, something were very odd about the dice (loaded, for example), in which case the appropriate comparison is between an experimental PK condition in which the psychic tries for the 5s, and a control condition in which they are simply thrown out but he does not try for 5s. Obviously, if 500 or so die again turn up as 5s, our belief in the PK effect should be considered not supported! This is the logic of the experimental approach to these phenomena, and is particularly appropriate to help distinguish whatever is due to a possible connection between independent and dependent variables in these studies, from what may be purely coincidental.

And it is, I think, the psychology of coincidence with which we are dealing here, a psychology that produces steadfast belief in many allegedly paranormal phenomena. For example, I could hardly convince my hypnotist that we had only the coincidence of his sometimes sending the trance, and his young lady assistant passing out. He was sure it was his telepathic ability! After all, we could all plainly see her hypnotized directly in front of us whenever he sent that trance! So he knew he had that power! Unfortunately, he did not equally notice when she passed out when he was not sending the trance (selective perception). The same is the case when a dowsing says to dig in a certain place for water, and water is found! People do not pay comparable attention when water is not found!

The American psychologist, B. F. Skinner, explained it quite clearly. In operant conditioning terms, what we are dealing with here is a conditioned response (e.g., the hypnotist sending his trance) and the consequence of this--a positive reinforcement situation (when water is found). Since this occurs only once in a while, and is not consistently related to the dowsing activity, we have a random ratio reinforcement schedule that is extremely resistant to extinction.

This control for coincidence becomes a fundamental quality of the control condition concept: to help the research scientist avoid self-deception and making a fool of himself by enabling him to distinguish between causal and non-causal (coincidental) events. This is the problem in the remote-viewing studies where we have a dubious connection between sketches drawn in the lab by an allegedly remote viewing psychic and photos taken of objects allegedly being clairvoyantly viewed through the eyes of others. The control condition we desperately need here is basically a non-remote viewing condition, that is, a situation in which the essential condition for paranormal psychic remote viewing is not present (i.e., no on-site observer brain and visual perception available) so we can see how our DV measure works out when we do not have any psychic situation present. In this experiment, the dependent variable measure is the frequency with which each given target is correctly associated with a drawing made at the time of the remote viewing when the photo was taken in one case with actual observers present, in the other case with no one present (same photos reused).

Suppose we have just two objects to be remotely viewed. Let one object be, say, an automobile, and the other a fountain. Our remote viewing psychic makes two sketches, one while the on-site observers are actually observing the automobile, and the next when our observers are looking at the fountain. This constitutes the experimental condition (the only condition used by Targ and Puthoff) of our independent variable. We then ask our judges to match each sketch with one of the photographs (notice we have operationally defined the observer's on-site perception that is allegedly available to our psychic in terms of a photograph taken from the observer's vantage point--our dependent variable). A correct match is of course a "hit" in parapsychological jargon. But let our experimenters also randomly assign each of the photographs to one of the additional control sketches generated from our psychic under the guise of another additional remote viewing effort. These are also submitted to the judges. The appropriate statistical

test is then based on the comparison between the DIFFERENCE IN THE NUMBER OF HITS BETWEEN THE EXPERIMENTAL AND CONTROL CONDITIONS. If there is a psychic power operating here, there clearly should be a statistically significantly larger number of hits in this psychic clairvoyant condition than in the control condition. Now we can see if our "hit" rate is greater than the normal, background "coincidental" rate of hits that normally would operate in this rather bizzare situation.

In the letter I mentioned above that I wrote in 1976 to the Proceedings of the IEEE criticizing the Puthoff and Targ methodology, the authors had an opportunity to reply (Puthoff and Targ, 1976b). Their response made me realize I wrote too abstract an analysis of their alleged experiment and failed to provide a procedure to get at that "coincidence" base rate of hits--that is, to provide a control condition for remote viewing. I could tell this had been the mis-understanding because they had written that "...obviously there is no single research strategy or experimental design that can be applied in cook-book fashion to all research issues" (p. 1549), and this is indeed true. But, they were claiming to have done an experiment, which is the preferred method to establish causality and definitely represents one particular type of research strategy. This being the case, we can definitely point out that experiments require the presence of some very fundamental conditions, to wit, an independent variable consisting in at least two variates or values, not just one as in their work. This is what I had not made clear.

The situation seemed especially ironic to me since they had specifically claimed that "...we concentrated on what we considered to be our principle responsibility to resolve under unambiguous conditions the basic issue of whether or not this class of paranormal perception phenomenon exists," (1976a, 334-335) and proceeded to do so in a series of studies without independent variables and without control conditions, the upshot of which is that we have just the opposite effect of maximizing ambiguity since innumerable confounding variables may then be held responsible for whatever effects are obtained (confounding at least implies ambiguity)!

Let us hope experimenters like Karnes and his colleagues will continue to provide, as in the present and other studies, the unambiguous conditions necessary to reveal a genuine paranormal phenomenon of the remote-viewing type so that the critical body of science will have the appropriate basis to judge the reality of this until then alleged phenomenon! I can only hope that the continued negative results from these studies that I anticipate, probably directly related to the adequacy of the control conditions present, will remain sufficiently reinforcing to the Karnes group to continue this kind of work, and for the journals to publish it, so that ultimately less professional time will have to be taken up with such nonsense as this, and we can get on with the serious work of seeing to the survival of our civilization and our science!

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COMMENTS BY BRENDA J. DUNNE AND ROBERT G. JAHN:

The remote perception experiments reported by Karnes, et al, seem to us well-posed and well-conducted, and their conclusion of overall insignificance seems justified for their published data. Their findings are thus comparable with numerous other negative results obtained in similar experiments elsewhere, including some efforts at our own laboratories. They do not, however, serve to dispel, or unfortunately even to illuminate, an equally substantial data base of positive findings, derived under equally rigorous conditions, at many of the same laboratories, again including our own. Rather, they further emphasize the lack of full control, indeed of full cognizance, of all relevant parameters of experiments of this class, which continues to plague attempts to display and interpret such phenomena.

In an effort to remove the vagaries of subjective human judging from the evaluation of remote perception transcripts, we have recently developed an analytical scoring technique which is capable of assessing the degree of information acquisition within a given individual perception effort. It is not clear from Karnes' published data whether any of their perceptions would display significant information acquisition by our criteria, but we have found in certain instances that transcripts which are unpersuasive under subjective human evaluation do convey significant information when coded into binary algorithms appropriate to the target pool.

Even with this refinement, the series-to-series success rate of remote perception experiments is far too low to contend universal replicability, but it is also far too high to dismiss categorically on prosaic grounds. Proliferation of studies such as Karnes reports is thus desirable in the hope that an enlarged data base will reveal some factors which favor higher yields.

NOTE:

¹Dunne, B.J., and Bisaha, J.P., "Precognitive Remote Perception: A Critical Overview of the Experimental Program," Proc. Parapsychological Association, 22nd Annual Convention. Moraga: St. Mary's College, August 14-18, 1979.

COMMENTS BY ARTHUR HASTINGS:

After reading the very lucid and fair description of the experiment by Karnes, Susman, Klusman, and Turcotte, I would say that the reason they did not get positive results was that no remote viewing occurred. In my experience, remote viewing can function very strongly, but some people do not see a thing, and in some experiments, nothing happens. Sometimes it is a question of experimenter bias, faulty target selection, inadequate learning conditions, poor treatment of subjects, or unsatisfactory judging, but even the best of cooks can not make a rabbit stew without a rabbit, and you can not get statistically significant results in a remote viewing experiment if you do not have any remote viewing.

I note that there were no significant results in the two previous studies by Karnes and his colleagues, so it seems likely to me that their future studies will have the same outcome, unless they can get a handle on the process itself. At the initial stages I do not think the design is that important.

For persons interested in obtaining positive results, I would strongly recommend trying remote viewing on an informal basis with themselves, friends, and anyone until they begin to have a degree of success based on straightforward, open comparison of the target and their guesses. This can be done with individuals or with groups to find persons who do well. People differ in their ability to do remote viewing and to do remote viewing experiments, so you should try different roles. Then work with one or two persons to improve and stabilize their perception. Once there are some consistent (if not continual) positive results, formal experiments can be carried out to confirm that remote viewing is occurring and will function under test conditions. After that, you can try varying the judging, target selection, sender-receiver pairs, and other conditions. I think you should not do formal tests until you are getting results in nonexperimental situations.

Not only does the subject doing the remote viewing require selection and development, but the judging also requires attention. Based on the many sets of trials I have judged, for Targ and Puthoff and others, I would estimate about 6 to be the maximum number of transcripts that I can easily compare in relation to one target. Further, I may spend as much as two days on a set of six targets and transcripts, visiting sites more than once and taking quite a bit of time to make comparisons and evaluations. Judges need to be experienced; you can not take someone off the street and accept their matches as a valid appraisal of the information in the transcripts.

The best way to train judges is probably to give them one or two transcripts from a series that has good remote viewing and have them go to the sites with the correct transcripts to observe how the subject responded to the target, with the judge knowing which is the correct transcript. Then try them on blind matching with a series of trials, giving feedback to let them know if they matched correctly. Finally, select as judges the persons who succeed in this second stage of blind matching.

I believe that fewer, tested judges are better than many, inexperienced ones. Using 64 judges, as in this experiment, is an interesting technique to explore, but we do not have any independent measures of their judging ability; and even if there is remote viewing information in the transcripts, the matches of a few good judges could be wiped out statistically by other, less astute judges, so they would have to be looked at individually.

It is also useful where possible to work directly with people who are carrying out remote viewing research, to see how they do it, observe their results, and get the feel of how the situation is constructed. As with all psychological research, there may be subtleties that neither set of experimenters recognize, but which can be communicated through personal experience.

COMMENTS BY DAVID MARKS & RICHARD KAMMANN:

Karnes, Susman, Klusman and Turcotte must be applauded for their persistence in trying to meet Targ and Puthoff's requirements for replicating the Targ-Puthoff remote viewing effect. But since there are no lawful principles of the "psi process," we can predict that new ad hoc rationalizations will be offered to psi advocates to explain away Karnes' three failures to replicate under carefully controlled conditions.

The Karnes team note that details on the Marks-Kammann failure to replicate remote-viewing are missing from the original report in Nature. The fact is that Nature deleted the details of our nonreplication because of the high premium on space in that journal. Further details of our experiments are now available in our recent book, The Psychology of the Psychic (Marks and Kammann, 1980).

In all, we ran five subjects reporting to have psychic sensitivity for 9, 5, 9, 5 and 7 remote viewing targets respectively, and had the transcripts evaluated by 5, 5, 5, 5, and 1 blind judges, respectively. We followed the Targ-Puthoff methodology as closely as possible, except we did not leave cues in the receiver transcripts or provide the target list in the correct order to judges as Targ and Puthoff did, at least in their Price and first Hammid series. All of our results agreed beautifully with chance expectations indicating no evidence of remote viewing whatsoever.

In our book, we also describe the cues to the order of targets we found in the Price and Hammid transcripts used by Targ and Puthoff, and the disappearance of the good results for Price when blind judges were provided with cue-free transcripts.

There are many factors other than the presence of sensory cues which invalidate the Targ-Puthoff experiments. There is clear evidence of data selection in the Hammid series in which the experiments in the "official" published series appear to have been selected from a longer series. In addition we believe statistical errors were made in evaluating the results from the series of experiments involving more than one subject (the Elgin-Swann, Pease-Cole, visitor, and technology series). A further undesirable feature of the Targ-Puthoff research is that the nominal targets were specified for the judge post hoc following the collection of the subjects' descriptions. These and many other troublesome problems are outlined in some detail in The Psychology of the Psychic.

The Karnes team are therefore premature in concluding that Targ and Puthoff have "refuted" our evidence of cues and other methodological errors in the original remote viewing studies. A reply to the Targ-Puthoff rebuttal has been submitted to Nature, and is available in manuscript form on request. This information may help researchers to a better understanding of the methodological differences between successful and unsuccessful remote viewing experiments.

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COMMENTS BY JAMES RANDI:

This paper is remarkable indeed. The authors have managed to anticipate every probable rationalization that the parapsychologists might produce against its findings, an ability only learned from long and careful examination of the well-established procedures of the art of pseudo-science.

There is little that serious critics could add to their work except to applaud their efforts. It is of great interest to me that their "One final point" in closing reflects a similar observation made by Dr. Richard Kammann and David Marks in their account of similar remote-viewing replication attempts in New Zealand (Psychology of the Psychic, Prometheus Books, Buffalo, N.Y., 1980) namely, that it is not at all difficult to imagine close matches between targets and transcripts when failing to consider the entire set of data, rather than just the limited aspects.

Quite properly, the authors of the present paper have omitted to mention such facts as that which most annoyed and dismayed me personally about the Targ/Puthoff presentation in the IEEE journal. One such highly effective convincer was accomplished by publishing photos of the targets--taken well after the judging had been done--from angles that were designed to emphasize the similarities to drawings made by the subjects. An incautious reader might fail to note that these photographs were not part of the original protocol. Such techniques certainly helped to add strength to the Targ/Puthoff paper, even if only peripherally.

It will be fascinating to see what rationalizations the supporters of Astral Projection/Out-of-Body Experience/Remote Viewing will be able to summon to support their (I'm sure) continued belief in this highly doubtful phenomenon that has so charmed the paranormalists.

COMMENTS BY CHARLES T. TART:

In a paper in this issue of ZS, Karnes, Susman, Klusman and Turcotte (1980) report an experiment in which they were unable to obtain evidence for ESP of the sort described as "remote viewing." If they had obtained such positive evidence, however, their study would have had to be considered only suggestive, at best, because of a serious methodological flaw.

Karnes et al. state that "The order of target sites to be used for the trials was determined by a table of random numbers. The principal author was the only person aware of the identity of the target sites and the order of use. Subjects were contacted by phone and were scheduled for the experimental sessions at their convenience. Assignment of target sites for each sender-receiver pair was determined by order of appearance. Sender-receiver pairs met the principal author at this office. They were read a set of instructions"A fundamental rule of sound parapsychological research was violated. The principal author (Karnes), knowing which target site a particular sender was going to, nevertheless engaged in sensory interaction with the receiver. This created the possibility of subtle cueing of the receiver by the principal investigator as to qualities of the target site.

It has been a rule for decades in scientific parapsychological investigation that no person with knowledge of the targets should be permitted sensory contact with the subject, especially fairly prolonged and complex interaction such as seems to have occurred in the Karnes et al. study. From my reading, this apparent flaw also appears to have occurred in two earlier studies by Karnes and Susman (1979) and by Karnes, Ballou, Susman, and Swaroff (1979).

I would certainly be personally (but not professionally) sympathetic to Karnes if he felt that his interactions with his subjects were controlled by him so that he was subjectively certain that he did not give out any such cues, but the importance of eliminating any possibility of sensory cueing has been emphasized many times by both critics and parapsychologists. If Karnes and his colleagues do any further research in this area, I hope they will eliminate this methodological flaw.

There are also a number of important procedural differences between the present and earlier Karnes et al. studies and the prototype remote viewing procedure developed at Stanford Research Institute, but I shall delay commenting on these, pending a systematic study of differences in a number of variables across all remote viewing studies to date.

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EDWARD W. KARNES AND ELLEN P. SUSSMAN RESPOND TO THE COMMENTS:

We appreciate and read with interest the six reviews of our remote viewing experiment. The reviews by James Randi, James Calkins, and David Marks and Richard Kammann were favorable as well as informative about the problems involved in conducting remote viewing experiments. We are in essential agreement with the points raised in those reviews, and would only like to mention in response to Marks' and Kammann's review that we were indeed premature in concluding that Targ and Puthoff have "refuted" their evidence of cues and errors in the original remote viewing studies. We were simply careless in using the word refuted--mistakenly thinking that there were degrees of success in refutations. The proper word should have been disputed not refuted.

We had hoped that proponents of the remote viewing phenomenon would have delineated what they felt were the specific reasons for our failures to replicate successful remote viewing. Unfortunately, little was received in that regard. Dunne and Jahn acknowledge their successes and our failure to replicate successful demonstrations of remote viewing but provided little in the way of constructive criticisms.

Charles Tart cited what he considered to be a "serious methodological flaw"--the fact that the principal author (EK) having knowledge of the target sites engaged in "fairly prolonged and complex interactions" with the subjects. We view that criticism as irrelevant to the results and conclusions of our study since no evidence for successful remote viewing was obtained. If successful remote viewing had been found, then the possibility of cueing could be raised, but the possibility of cueing to account for negative results is untenable. We felt that the most adequate way to insure confidentiality concerning the identity and sequencing of the target sites during the sending/receiving trials was to use the procedure described in the report. Also, the principal author's only contact with the receiver prior to a series of trials was to read a set of instructions. We find it difficult to view the reading of a set of instructions as fairly prolonged and complex sensory interactions.

We certainly agree with Tart that cueing could be a distinct possibility in accounting for reported successes in remote viewing. In fact, Marks and Kammann (1980) have shown that when various cues were removed from Puthoff's and Targ's data and procedures, independent judges could not correctly match the transcripts to the target sites. Tart, Puthoff, and Targ (1980) have claimed that when they removed the existing cues from the same transcripts, a new independent judge was still able to obtain successful matchings. This discrepancy in judging results using the same remote viewing data raises the intriguing possibility that any "psychic" abilities may reside in the judges rather than the sender/receiver pairs per se. Of course, a more reasonable possibility may rest with differences in the degree to which the cues were successfully removed from the transcripts and/or how adequately the judges were blinded.

What would have been appreciated from Tart are suggestions as to why we obtained negative results. He tempts us in that regard by stating that there were important procedural differences between our study and the prototype remote viewing procedure developed at Stanford Research Institute (SRI). Since we tried to closely follow the SRI procedures, it is unfortunate that he is delaying commenting on these differences. However, thanks largely to Marks' and Kammann's (1980) evaluations of the SRI remote viewing data and procedures, we can identify procedures in our experiment that were very likely different from the prototype SRI procedures.

First, all possible cues were eliminated from the receivers' and senders' transcripts before submitting them for judging. Second, the judges were totally blind concerning the number, identity, and sequencing of the target sites since each judge matched the set of receiver transcripts to only one target. In addition, the experimenters who conducted the judging trials were not the same experimenters who participated in the remote viewing trials and were totally blind concerning the identities of the sender-receiver pairs, the sequence of target visits, and the time at which the remote viewing trials were conducted. Third, there was no selection of data to either support or not support remote viewing. We made no distinctions between "demonstrations" and "experiments" and all of the data were used in statistical analyses. Fourth, the ambiguities involved in deciding what specific aspects of a target site were relevant for judging were reduced since visual and written records of the sending situation as well as visits to the target sites were provided to the judges.

Arthur Hastings commented that we failed to obtain positive results because no remote viewing occurred. We definitely concur with that evaluation! If we correctly understand his comments, there are two possible reasons why we failed to obtain successful remote viewing: 1) our subjects were inadequate and 2) our judges lacked experience in judging. The first possibility, inadequacy of subjects, is especially troublesome. It's like a "Catch 22" in investigations of claims of the paranormal. If positive results are obtained, the subjects have the abilities. If negative results are obtained, the validity of the phenomenon remains unquestioned, and the negative results are attributed to the lack of proper abilities in the subjects. Obviously, such criticisms of negative results dampen the enthusiasm for impartial and/or critical investigations of claims of the paranormal.

We also think Dr. Hastings missed the point of the study. The subjects were self-proclaimed psychics; they claimed to have had successful experiences in various sorts of psychic communications exercises; they were confident of their abilities to successfully demonstrate remote viewing; and they self-selected themselves into sender/receiver pairs based on their previous successes in psychic communications. Even if our subjects weren't "proper," Dr. Hastings' contention is contrary to Puthoff's Targ's contention that remote-viewing is a human skill that can be successfully demonstrated in all subjects.

Dr. Hastings recommends that remote viewing should be tried on an informal basis and "experiments" should follow when there are some consistent positive results in the informal exercises. This is an especially interesting recommendation in light of the report by Marks and Kammann (1980) that some subjects in their unsuccessful attempts to replicate remote viewing thought that they were remarkably accurate in their remote viewing "perceptions." However, the independent judges totally failed to perceive these accuracies. We noted, but did not report, the same phenomenon in our experiment. In many cases, the sender/receiver pairs were quite confident that they had demonstrated successful remote viewing, but the judges were unable to verify these perceived accuracies.

We believe that the explanation for this apparent self-deception concerns the motivation to demonstrate remote viewing and the ambiguous natures of the receiver's "perceptions" and the sender's perceptions at the target site. To cite one example, the receiver had "perceived" trolley tracks (among other things). The sender had actually been at a large open Greek amphitheater; there were no trolley tracks at or even close to the site. During the debriefing period following the trial, the receiver and sender were confident that remote viewing had occurred due, in part, to the fact that the sender declared that he had been at one point concentrating on the seams in the cement floor of the amphitheater. The parallel lines were thus seen as being similar to trolley tracks. The judges, however, had absolutely no success in finding any correspondence between the sender's transcript and the target site. The point is that if Dr. Hastings' recommendation were followed, experimenters would very likely be deceived by the subjects' misperceptions of the "successes."

Concerning Dr. Hastings' second point (the need for experienced and trained judges), we are unaware of any specific references to this requirement in the published literature on remote viewing. Charles Tart mentioned, in the original manuscript for the Tart, Puthoff, and Targ (1980) Nature article, that judges need to have specialized intellectual "faculties" and that they need to be motivated to use these "faculties" effectively. He also attributed the negative results obtained by the two independent judges in Marks' and Kammann's evaluation of the SRI transcripts to inadequacies in the judges. After reading Marks' and Kamman's (1980) revelations of the judging procedures conducted at SRI, we wonder whether the "specialized intellectual faculties" might not somehow involve the degree to which the judges are truly blinded to extraneous cues. Hopefully, future research will address this adequacy-of-judges hypothesis.

We made no attempt to ascertain whether or not our judges had the "specialized faculties" referred to by Tart since we do not know what

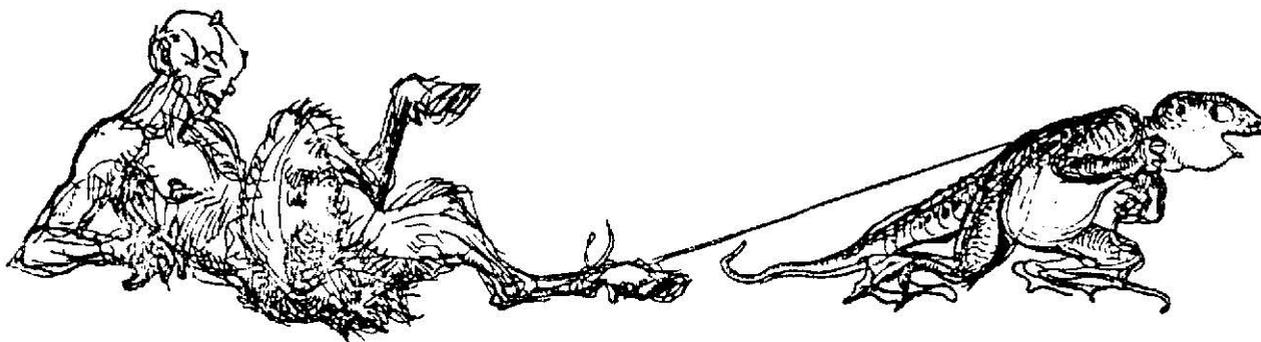
constitutes these "faculties." We are, however, confident that our judges were motivated to do an effective job in judging. Judges volunteered on the basis of having an interest in the experiment, and they were read a set of instructions explaining the phenomenon and their judging task. They were also given considerable time to complete the judging.

Dr. Hastings recommends that judges for remote viewing experiments should be trained. We believe that it would be difficult to follow Dr. Hastings' prescription for training judges since he recommends using as training aids transcripts from a series that has good remote viewing. First, we did not have "good" remote viewing transcripts. As we have consistently found in our studies on remote viewing, "hits" occur on receiver transcripts that are consistently guessed as being a correct match to many targets and/or judges scoring hits are more prolific guessers than judges who do not score hits. Second, how do we obtain good remote viewing transcripts if we do not use, at least initially, inexperienced judges? As mentioned previously, sender/receiver pairs cannot be trusted to judge the accuracy of their remote viewing experiences.

One final comment. Impartial investigations of remote viewing will become increasingly difficult given that supporters of the phenomenon can attribute negative results to: 1) inadequacies in subjects; 2) inadequacies in judges; and 3) deviations from prototype SRI procedures. The negative results obtained in remote viewing experiments should, however, at least convince supporters that the phenomenon is not nearly as robust and widespread as was initially proposed by Puthoff and Targ.

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ZS PARANORMAL CONTENTS BULLETIN

There are now literally dozens of publications dealing with claims of the paranormal, and scholars usually can not subscribe to them all. And, unfortunately, most libraries do not carry these journals. To help deal with this problem, we will--if initial interest warrants--begin publishing the ZS PARANORMAL CONTENTS BULLETIN. This will consist of a quarterly newsletter made up of xerox copies of the tables of contents of publications dealing with scientific anomalies: psi, UFOs, cryptozoology, etc. In addition, issue purchase and subscription information plus addresses will be included for the publications listed.

Permission to copy the contents pages of these publications is now being obtained, and it is hoped that all will cooperate and that we will have at least 30 journals included in each issue of ZS PARANORMAL CONTENTS BULLETIN.

Subscriptions to ZSPCB should run about \$10 per year. Each issue will be xeroxed off to order. Your suggestions and expressions of interest would be most welcome. Full information should be in the next issue of ZS later this year. --MT



SEVEN EVIDENTIAL EXPERIMENTS

JOHN BELOFF



NB. *These are listed below in order of date of execution.*

- (1) The Brugmans' experiment with the subject van Dam at the University of Groningen, May 1920.
- (2) The Blom & Pratt experiment with the subject Stepanek in Prague, November 1963.
- (3) The Musso & Granero experiment with the subject J.B. Muratti in Rosario, Argentina. 1967.
- (4) The Roll & Klein experiment with the subject Harribance at the P.R.F. Laboratory, August 1969.
- (5) The Kanthamani & Kelly experiments with the subject B.D. (Bill Delmore) at the FRNM Institute, February 1972 - April 1973.
- (6) Helmut Schmidt's experiments on PK in selected subjects using a binary random number generator at FRNM Institute, 1973.
- (7) Terry & Honorton's 'Ganzfeld' experiment with student volunteers at the Maimonides Laboratory, Brooklyn, New York, 1975.

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COMMENTARY

Criteria of selection. Although any list of this sort must, in the end, be a matter of personal judgment, the following criteria were applied in this instance: (a) the chief experimenter must be someone of good standing and long experience who is well known to the international parapsychological community; (b) the report of the experiment must have appeared in a reputable scientific journal; (c) the overall scores must reach a level where the odds against chance are so high that any suspicion of selective reporting or optional stopping can be discounted; (d) conditions must be such as to rule out effectively the possibility of sensory cueing or of cheating by the subject; (e) the scoring rate should, if possible, be at a level which would exclude any counter-explanation in terms of some subtle artefact; (f) other things being equal an experimental finding which has been confirmed many times is to be preferred to one which is unique or has seldom been replicated; (g) the list should represent a variety of methods and effects. This being said, it must be admitted that

there cannot, in the nature of the case, be any final guarantee against the kind of experimenter-fraud which vitiated the Soal experiments with Basil Shackleton in 1941.

Comments on the strength and weakness of the particular cases chosen.

(1) This was a one shot affair. Nothing further of parapsychological interest was forthcoming from this laboratory and nothing further was heard again of its protagonist, van Dam, who was, at the time a student of mathematics and physics at the University of Groningen. The subject's task, moreover, ascertaining the correct square on a chess-board type of target, was highly untypical if not unique as a psi test and yet the results are almost unbelievably good.* Nevertheless, the experiment has stood up remarkably well over the years and I know of no serious attempt to undermine it. On the contrary a thorough reexamination of it recently, see Schouten and Kelly (1978), has done much to vindicate it.

(2) Stepanek, though now parapsychologically defunct, has gone down in parapsychological history as the most long-lived of all the special card-guessing subjects. For ten years he continued to produce non-random scoring for many different investigators, see Pratt (1973) and Pratt et al. (1968). He even earned a mention in the Guinness Book of Records! There were, however, certain definite weaknesses in his performance: (a) his repertoire was restricted to the one binary card-guessing task on which he had initially been trained to perform by his discoverer, M. Ryzl, and any departure from this task, other than the introduction of additional envelopes or covers, produced only random scores as I discovered to my cost (see Ryzl, M. & Beloff, J., "Loss of stability of ESP performance in a high-scoring subject," J. Parapsych., 29, 1965, 1-11); (b) his scoring rate during most of his career was rather mediocre, his investigators were thankful if he could manage 55% correct where 50% would represent chance and, most seriously, (c) his performance was erratic and gradually shifted from guessing correctly at the colour of cards to a pattern of calling that came to be known as the 'focusing effect' (see Keil (1977)). This made it very difficult to predict on any given occasion exactly what sort of results he would produce.

(3) The beauty of an experiment such as this which used the free-response drawing method is that many of the responses can be seen to be self-evidently correct. A matching technique using blind judges demonstrated that the overall results were highly significant statistically. Nothing has been heard since of the subject, a Dr. J.B. Muratti, but it is of interest to note that he was a professional psychiatrist.

(4) Harribance was not as limited in his repertoire as Stepanek; he had, in fact been a professional psychic and medium, but his useful career in experimental parapsychology was much briefer and his in-

*It is of some interest to compare this set-up with that used recently by Charles Tart, with notable success, at the University of California Davis. If today we had a van Dam at our disposal we would not have to use a peephole in the ceiling while watching and attempting to influence his performance, we would use closed-circuit television. And this is what Tart did with his 'Ten Choice Trainer' (see his monograph The Application of Learning Theory to ESP Performance. New York: Parapsychology Foundation Inc. 1975).

investigators fewer. On the one occasion when I had an opportunity to test him informally on some runs of ESP cards he scored exactly at chance level!

(5) Taken on their own I would regard this series of experiments as, perhaps, the most evidential in the entire parapsychological literature!

B.D. was a law student at Yale University where he was celebrated locally for his prowess at guessing playing cards. His reputation attracted the attention of Irvin Child, the well known social psychologist at Yale, who persuaded him to take time out to undergo a thorough testing at the FRNM Institute at Durham, N. Carolina. Although his preferred task remained that of guessing at playing cards, and this provided the basis of the main series of tests (see Kanthamani & Kelly 1974), his psi ability extended to a variety of other kinds of tests, including PK tests (see Kelly & Kanthamani 1972, esp. Table 2). Particularly impressive was B.D.'s high rate of scoring on his confidence calls. It is only a pity that this subject has done nothing further since then.

(6) It is not easy to find a suitable PK experiment that met all our criteria. The many experiments of W.E. Cox seldom attain a very high level of significance while the experiments of Forwald which are statistically impressive are disqualified by the fact that Forwald was working on his own with himself as sole subject under conditions that were unwitnessed. Schmidt's experiments, of which this is a prime example, represent a highly sophisticated experimental design and make use of both visual and auditory feedback. Although results are very significant statistically the individual scoring rate seldom exceeds about 51% as against the 50% chance baseline. It has not proved easy, unfortunately, to replicate this type of experiment in other laboratories; we have so far had no success at Edinburgh.

(7) The Ganzfeld technique now probably leads the field in terms of the number of successful replications it has engendered quite apart from the many striking qualitative correspondences it has produced that would be hard to attribute to mere chance. Carl Sargent claims that his laboratory at Cambridge is now the eighth independent laboratory to achieve success using the Ganzfeld set-up first introduced by Honorton (see Harley & Sargent 1979). The fact that much of this success has been achieved with unselected subjects is another cause for optimism.

Conclusions: It is not my contention that any of the foregoing experiments were perfect (whatever that might mean) or beyond criticism. In retrospect one can always think of some additional controls one could have introduced or something one would have done differently. Moreover, unless a much higher level of repeatability becomes possible the sceptical option, that the results can be attributed to carelessness or to conscious or unconscious cheating on the part of one or more of the experimenters, remains open and valid. Nevertheless, it is my personal opinion, that these seven different investigations represent an overwhelming case for accepting the reality of psi phenomena.

Following are the reactions to Dr. Beloff's presentation from among those invited by ZS to comment. Dr. Beloff's reply to these commentators follows their remarks. Readers of ZS are invited to continue this dialogue in future issues -- as with all ZS articles.

COMMENTARIES ON DR. BELOFF'S PAPER

COMMENTS BY JAMES ALCOCK:

I have had difficulty in commenting on Beloff's article (Seven evidential experiments) because of the glaring inconsistency in his closing paragraph. However, perhaps the following will be useful:

Beloff himself criticizes most of the experiments he has listed. Other criticisms are possible, as well. (For example, Persi Diaconis in his recent (1978) Science article reported that the studies of "B.D." were not so well-controlled as was reported. He himself detected cheating during one of B.D.'s demonstrations). However, Beloff himself has been quite straightforward in cautioning that "unless a much higher level of repeatability becomes possible the skeptical opinion, that the results can be attributed to carelessness or to conscious or unconscious cheating on the part of one or more of the experimenters remains open and valid". (Why he did not mention the possibility of conscious or unconscious cheating by the subjects is a bit odd). What is astonishing to me is that, even after making this statement, he concludes that these experiments taken together represent an "overwhelming" case for accepting the reality of psi.

Beloff sees the same problems that the skeptic sees (although detailed examination of each study may yield further criticisms. One also must keep in mind Diaconis' observation that it is very difficult to judge what really went on in an experiment from the written reports. If the experimenters believe that the conditions were well-controlled, they will report that. Their belief could be in error). Beloff's acceptance of these experiments as overwhelming evidence appears to be the result of one gigantic leap of faith.

COMMENTS BY IRVIN L. CHILD:

John Beloff's selection of impressive experiments seems sound to me. Since, as he says, any single experiment of any kind is obviously subject to possible doubt, I would myself be inclined to stress even more than he does, in his very compact presentation, the number and diversity of impressive experimental reports. But if more critics of parapsychology can be led to read and ponder carefully even a few of these, that is all to the good.

I may differ slightly from Beloff about what inference is to be drawn. He speaks of "an overwhelming case for accepting the reality of psi phenomena." If by "psi phenomena" he means only "apparent transmission of information or influence by channels not understood, suggesting severe gaps in our knowledge of natural processes," I agree. And that may be all he means. But the phrase, "reality of psi phenomena," suggests something more substantive.

some particular conception of what processes are responsible for psi phenomena. I do not consider that these or other studies point very strongly toward any particular solution of the scientific puzzle they pose. Agreement on evidence for psi phenomena would be only a first step toward expanding the quest for understanding the processes involved. Extreme attention to the question of "reality," at the same time that it promotes scientific rigor, tends to distract attention from questions of equal importance about the processes to be studied and the means to be used.

The importance of these studies, in my view, lies not only in the possibility that some scientists who read them all might begin to find here real problems for investigation. It lies also in the fact that these studies and others closely associated with them contain suggestions for the research of the future. In view of space limits, I will confine myself to a single point about each of the studies Beloff has selected.

(1) In Schouten and Kelly's modern analysis of the early Brugmans data, one striking finding is that correct responses were especially likely to be fairly (but not maximally) speedy responses. This points to largely neglected ways of planning experiments that might throw light on the particular processes involved in psi hitting.

(2) When Blom and Pratt's experiment with Pavel Stepanek is set in the context of other experiments with the same outstanding subject, an especially promising lead for future research, emerging rather indirectly, may be the one stressed by Keil: Psi hitting may be favored by first enabling the subject to achieve the desired outcome by ordinary sensori-motor means, and only later withdrawing access to these means. If so, parapsychological research is hampered when it is at all points insistent on controlling sensori-motor channels.

(3) Musso and Granero's experiment with J. B. Muratti, in finding that this outstanding subject (a psychiatrist) was mistaken about the general conditions that favored his psi hitting and also could not recognize the specific occasions of his hitting and missing, confirms a considerable body of evidence about the usual inaccessibility of psi processes to consciousness even in outstanding subjects. Much in modern cognitive and social psychology would reinforce the point that introspective report has very limited value in guiding research.

(4) The Roll and Klein experiments with Lalsingh Harribance illustrate that orienting research toward investigation of underlying processes is no guarantee of positive findings. Testing two notions about process, one drawn from earlier research with Harribance and one drawn from the research on Stepanek, Roll and Klein found neither of them sustained by their work, so that its principal positive outcome was just the striking demonstration of psi hitting that led to its inclusion in Beloff's list.

(5) The experiments by Kanthamani, Kelly et al. provide a counterbalance to that by Musso and Granero, in illustrating that introspective report is sometimes a useful guide to research on psi events, for the hypotheses studied here came primarily from the subject's descriptions of his experience. Confusion-matrix analysis, adapted from cognitive psychology, provided an objective confirmation of his impression that the processing of psi input was in him akin to that of visual input.

(6) Of the Schmidt articles on psychokinetic effects on random event generators, the one cited by Beloff is of special interest for the preliminary but impressive evidence that rate of event generation (varying from one per minute to 300 per second) affects the probability that attempts at psychokinetic influence will be successful.

(7) The Terry and Honorton study, and the series of which it is a seminal part, offer a technique especially well adapted to studying the role in psi processes of general state of alertness and attention, and provide evidence for reduced external attention as a factor favoring the occurrence of psi events.

Most of these studies (Terry and Honorton excepted) have depended upon one or a few outstanding subjects. What is learned from them may help in developing more dependable ways of obtaining psi events in unselected subjects, so that experimenters and critics alike can devote their attention more fully to the substantive issues of scientific inquiry in this field. Even the position of the extreme sceptic, postulating that all apparent psi events result from fraud or error--should it be correct--seems more likely to be established as the endpoint of large-scale earnest pursuit of scientific inquiry than by advance assertion, with semi-religious certainty, that it must be the correct position.

COMMENTS BY DANIEL COHEN:

I have little independent information on any of the seven evidential experiments cited by John Beloff. However, from what I know of Dr. Beloff's reputation I am quite willing to accept his description of the strengths and weaknesses of these experiments. Yet even at that I find his conclusion that these seven experiments represent "an overwhelming case for accepting the reality of psi phenomena" absolutely incredible.

Dr. Beloff is aware that there is no generally (or even remotely) acceptable theory that will reconcile psi (or any part of it) with other areas of scientific knowledge. To accept the reality of psi we need not merely evidence, but very good evidence. The evidence would have to be far better than that which is considered adequate in conventional psychology.

Dr. Beloff also knows that anomalous or unusual but not necessarily significant experimental results are found in all fields

of science, and this is particularly true once one leaves the realm of the physical sciences. It is not possible or necessary to find the fatal flaw in each anomalous experiment.

Dr. Beloff is only too aware of the long and sad history of fraud and error in psi research; fraud and error which has sometimes involved the most respected people in the field.

He admits that because of the low level of repeatability of psi experiments "the skeptical opinion. . . . remains open and valid." Yet from there, by some unexplained leap of faith he finds these seven investigations to be an "overwhelming case."

I would not disagree if Dr. Beloff said that such experiments were "suggestive" or "intriguing." I would not even object to the use of a word like "compelling," though I would not use such a description myself. But "overwhelming"--hardly! These experiments have surely not overwhelmed the critics of psi.

If Dr. Beloff's list represents the best justificational evidence for psi then the case remains just about where it has been for the past seventy five years--unproven.

Dr. Beloff's "personal opinion" represents the triumph of hope over experience and, alas over good sense as well.

COMMENTS BY H.M. COLLINS:

Marcello Truzzi has put me on his "pro-parapsychology" list of experts so I must start with a disclaimer. As far as possible I am neither pro, nor anti, parapsychology though I must admit that what I see as a neutral stance has frequently been interpreted as a pro stance, especially by critics of the field. It seems to me that many critics will not allow anything but full blooded condemnation to count as other than "pro."

In this comment, I do not intend to defend or criticize the list of experiments put forward by John Beloff as good evidence for the existence of paranormal phenomena, except to say - purely by way of a biographical anecdote - that it was Schmidt's experiments, as reported in the New Scientist that first led me to be interested in the field. When I read about these experiments, they seemed to me to be as convincing as any other experimental reports that I was likely to read in the New Scientist. However, I was not present when Schmidt's experiments were carried out, and so, like nearly every other scientist, my judgement was based on a published report. In the same way, though the commentaries solicited by the Editor of this journal might seem to be commentaries on experiments, in nearly every case they will be commentaries on reports of experiments, for, in nearly every case, the commentator will not have done the experiments himself, and his knowledge of them will be based on such information as is available to him. The question I will address myself to is the nature of the information on which these commentaries must be based.

It is not possible to describe completely the conditions under which any experiment is done. This can be understood from a number of points of view. The environment of any experiment includes the whole universe and the universe is continually changing, not least because of the infinite complexity and creativity of human action. Thus, not only is it impossible fully to describe an experiment, but also no two experiments are alike. Of course, in the normal course of events we do treat experimental protocols as though they can be completely specified, and we do treat experiments as though they can be repeated. But these descriptions, and this notion of repetition are based on the mutual acceptability of a huge ceteris-paribus clause. That is, the community of scientists who talk routinely of experimental protocols and repeatability assume that all can agree that nearly everything that goes on at the time of an experiment is irrelevant to the experiment. Thus in the conventional sense, to describe fully the protocol of an experiment in say physics, it is not necessary to consider the state of health of the experimenter or the length of the bus queues in Clapham, the temperature of any of Jupiter's moons or of the Yellow River. It might be said that scientific knowledge is constituted by the sharing of such tacit understandings within the scientific community. If every experiment had to be described, in each of its aspects, as the unique and original occurrence that it was, then science would cease. And if communication were not impossible, it would be communication of anecdote with never a generalization.

Thus the growth of science is co-extensive with the reduction of anecdotal material in the description of experiments. In reading an account of an experiment one normally takes the account at face value and assumes one shares the same conventions with **the author** regarding what it is appropriate to leave out of the account.

Now, experiments can be attacked by questioning the validity of the assumptions upon which the experimenter based his work. For example, a critic might say that the religious leanings of the experimenter and his assistants are a crucial variable in the experiment in question, whereas the experimenter had assumed a common convention that religious leanings were irrelevant. This type of criticism works in two ways. It works directly, in that the critic throws into question that particular assumption, and it works indirectly in that the critic "deconstructs" the scientific facticity of the phenomena under discussion by introducing anecdotal material. Supporters of a scientific field will wish to see accounts "packed down" into generalizations (perhaps requiring a specialized and common language) whereas those who believe the field is spurious will try to "unpack" the generalizations to re-establish the anecdotal quality of events (which they may say has been hidden behind "spurious jargon"). I believe that both of these processes can be seen operating in the area of parapsychology. For instance, the invocation of religious background, and the general unpacking process are well seen in the painstaking reconstruction of SRI experiments by skeptical commentators.

Having no particular axe to grind, I tend to read accounts like that of Helmut Schmidt, in rather the same way as I would read any other account of an experiment - in other words, I would take it at

face value, and assume that Schmidt had treated the same sort of things as irrelevant to his experiment as I would, were I doing an experiment in parapsychology. On the other hand, were I skeptical of parapsychology and had I the time on my hands, I am sure I could reduce the credibility of Schmidt's work by unpacking it into the messy, unglamorous and frail form in which it actually took place. For instance I could check on the names of Schmidt's research assistants, or the colleagues in his laboratory (was J. Levy among them?) and find out if Schmidt had checked them out thoroughly - he would not have done so of course. In this and other ways I could find loopholes in the work and by virtue of this very exploratory activity, I would have manoeuvred the work toward the status of anecdote. But then anyone could do this with any experiment in any subject.

Thus there does not seem to be a lot of point in commenting one way or another on the experiments put forward by John Beloff. Each could, with the application of sufficient time, be made to look unscientific. Whether parapsychology becomes accepted or not seems to me to be much more a matter of whether there is a general disposition to accept accounts of parapsychological experiments at face value or a general disposition to open them up into anecdotal form. I guess the outcome of the present exercise will be about 50/50.

COMMENTS BY ROBERT L. MORRIS:

I agree with Dr. Beloff's statement that "In retrospect one can always think of some additional controls one could have introduced or something one could have done differently." Thus I do not share his final opinion that these seven studies (or any other seven studies) represent an overwhelming case for accepting the reality of psi phenomena. My feeling is that the strongest evidence for psi phenomena comes from groups of conceptually and procedurally related studies which point toward functional relationships. Such studies are a better indicator of whether or not psi phenomena represent phenomena with which one can do business using the tools of science. My main concern as a researcher is whether or not psychic phenomena represent an area in which human knowledge can be meaningfully extended. Since I intend to expand my views for potential publication in ZS, I will not elaborate here.

COMMENTS BY J. RICARDO MUSSO AND MIRTA GRANERO:

1. Dr. Beloff (1980) has included our experiment (Musso and Granero, 1973) in a group of seven that he considers to "represent an overwhelming case for accepting the reality of psi phenomena." We would like to explain that the evidence provided by our experiment has been considerably strengthened recently, through the discovery in it of a very strong U-effect, which we had not detected in our first analysis of the data. Our final report on this new discovery will not be published for some time yet. Hence, for this discussion in Zetetic Scholar we think it is appropriate to anticipate it by describing some of the conclusions that result from our new analyses, and the circumstances that led us to carry them out. There were two stages.

(a) Stage of criticism and clarification (1978)

In responding to various criticisms of our experiment made by a colleague in the Instituto Argentino de Parapsicologia (Kreiman, 1978), we had to make some new analyses of the data. They showed that the criticisms were not valid (Musso and Granero, 1979). But the criticisms had important positive effects; they obliged us to rethink problems that in our first report had been left unsolved, and to produce ideas and evidence directed at their elucidation. For example: we clarified the presence of runs of high scores and of low scores. Our first analysis had demonstrated their presence, at a high level of statistical significance (1973, pp. 26-28). Now we found that the runs were not due, as we had originally supposed (1973, p. 30), to a possible effect, alternately strengthening and inhibiting ESP, of one of our experimental conditions. We found that they were due instead to the simple fact that the subject had some good experimental days and some bad days, some sessions on which his ESP functioned and others on which it did not (Musso and Granero, 1979, p. 13).

The analysis showed that the differences among sessions were highly significant ($p < .001$). It also showed that division of the sessions into two groups, G_1 and G_2 , according to whether the mean score for the session was above or below the overall mean, led to a singular fact, explicable only by the very high level of significance represented by the general mean in our experiment. The sessions included in G_1 had means that each individually departed from chance to a significant degree (p varying from .078 to .016 with the nonparametric U test, and from .04 to .0004 with a more sensitive CR test). The sessions included in G_2 , on the other hand, did not depart significantly from chance, neither individually nor as a group. To indicate that G_1 comprised sessions in which ESP functioned positively (psi-hitting), and G_2 comprised sessions where ESP was inactive (chance results), we called them, respectively, "good sessions" and "bad sessions."

(b) Discovery of the U-effect in the data (1979)

Each of our experimental sessions consisted of just six trials. For the single session to give significant results (as happened throughout G_1), ESP must be operating very consistently, perhaps even in all six trials. This was a very exciting thought, for we saw that by chance, so to speak, without having designed it that way, we had succeeded in bringing together in G_1 a set of conditions (consistent ESP, small n , psychological similarity across experimental variations in view of the subject's ignorance that variation occurred) considered favorable for the production of position effects. ("Position effect" is a standard term referring to systematic variation of scoring level with position of the trial within the session).

Rhine and many other parapsychologists consider that two position effects, "declines and U-curves,... are probably the most lawful indications of psi performance thus far available" (Rhine, 1969b, p. 136). A pure decline effect (that is, not followed by a later rise, making a U-curve) did not seem likely in our data,

as it is commonly associated with conditions not present in our experiment, such as lengthy runs and psi-missing (Rhine, 1969a, p. 6). Our conditions seemed, on the other hand, especially likely to lead to a U-curve; any tendency in that direction could here show itself uncontaminated by other effects that at times mask its presence, such as effects of random responding, of changes in conditions, or of psi-missing. A sketchy indication of the U-effect had already appeared in our first examination of the data, in the total of all sessions, but we had to attribute it to chance because the analysis we did fell short of statistical significance (1973, p. 22). Now, with the confidence justified by the conjunction of theoretically favorable circumstances, brought about by our division of the sessions into two groups, we predicted that if in reality the U-effect is a lawful indicator of psi, then it should be clearly present in our G₁ sessions (ESP active) and absent in G₂ (ESP inactive).

Our original data were in an ordinal scale (ranks), and we knew of no standard statistical procedures proper for testing the form of a sequence of ordinal data. We devised a procedure which seems valid: For a symmetrical U-curve of six points, the theoretical expectation of rank for the six points in order would be 5.5, 3.5, 1.5, 1.5, 3.5, 5.5. The ranks actually obtained could then be compared with these theoretical values by a rank-order correlation coefficient. In addition, however, we transformed the original rank data into normalized T-scores and analyzed them with parametric procedures, especially those for testing orthogonal components of trend (Winer, 1962, pp. 70-77). We identified the U-curve with the quadratic component and predicted that in G₁ this component would be significant and the other components (linear, cubic, quartic, and quintic) not significant; in G₂ we predicted that none of the five components would be significant.

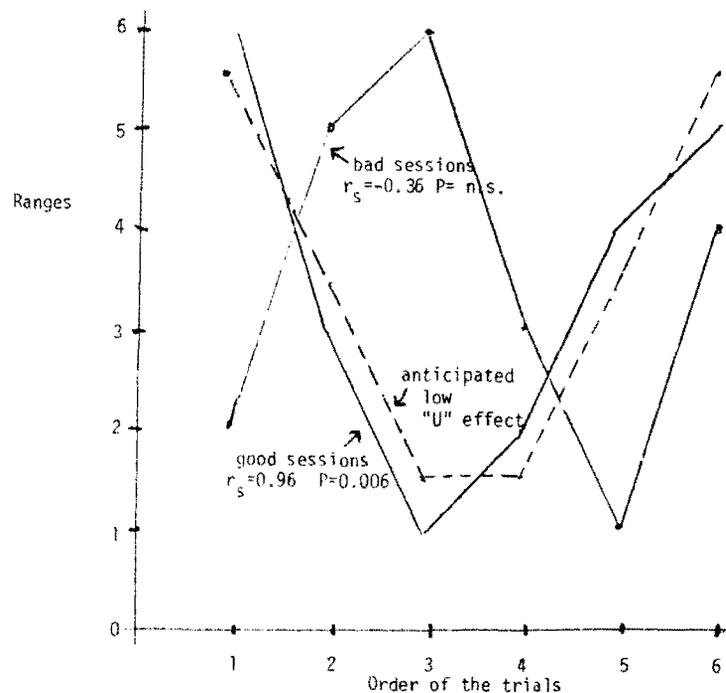


Figure 1. Distribution of the ranges observed in the good and bad sessions compared with the "U" hypothesis.

All these predictions were verified. In group G₁ (sessions 1, 2, 4, 6, 7, 9, 10, 11 and 13, in the analysis with non-parametric techniques), the U-effect appeared clearly ($p \approx .006$) in the distribution of ranks for the six positions (see Figure 1), and it appeared with high consistency within sessions ($p < .003$) and within the experimental condition C₁, C₂, and C₃ ($p < .0005$). In G₂ the U-effect was completely absent. Our assertions can be checked by repeating our analyses on the original data of the experiment, published in full in our 1973 article (pp. 18-19).

2. Dr. Beloff points out, correctly, that after our first report "Nothing has been heard since of the subject, a Dr. J. B. Muratti." This is because we were not able to perform new experiments with him. The success of the first experiment eliminated for him any doubt about his having, at times, notable ESP successes. From then on his interest shifted from the pure investigation of ESP to its practical use in games of chance. We **attempted** an applied experiment on precognition, whose design included use of the focusing method and the majority-vote technique. The practical aim was to use Dr. Muratti's ESP to predict successfully the results of thirteen soccer games that were to be played in the future. (A large pool, called Prode, is divided among the winners, and it is played for throughout the country). After analyzing responses accumulated through several months, we interrupted the experiment, because no significant deviations appeared that might suggest possible accuracy. Besides this unsuccessful attempt, on various occasions we had informal ESP sessions with Dr. Muratti, but his attitude toward the tests was clearly different from that which he had during the first experiment. The results were negative.

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COMMENTS BY J. FRASER NICOL:

If ten psychical researchers well versed in the literature of the last hundred years were asked to name the six most convincing experiments, it is safe to say that no two of them would completely agree. John Beloff's list seems judicious (though my own selection might be somewhat different), and I should guess that most psychical researchers would receive it with respect.

This business of choosing the "best" experiments is, however, highly controversial. In 1940 the Duke University Parapsychology Laboratory published a list of six researches which the authors judged to be beyond all reasonable criticism.¹ Some years later S.G. Soal² without specifically referring to the Duke list considered five of the researches. One he treated with respect, the others with varying degrees of doubt. It is an unhappy irony that almost all of Soal's own investigations, extending over a period of a third of a century, are now treated with considerable doubt. Betty Markwick has produced evidence that Soal faked some of the data in his experiments with the subject Basil Shackleton.

The conflict of opinion on the variable - and often controversial - quality of quantitative researches can be illustrated by the fate of Soal's last venture in card guessing. The experiments were with two adolescent cousins in Wales, one being the agent, the other percipient. The trials were so lacking in rigorous controls that there was hardly a session in which "normal causes" was not an adequate explanation of the stupendous scores. The experiments are now rarely mentioned in the literature of parapsychology. Yet, when Soal and a fellow experimenter wrote an enthusiastic book about them, J.B. Rhine and associates presented them with the William McDougall Award for their "outstanding" work. John Beloff wisely excludes the Welsh experiments from his list.

The contemporary situation in quantitative research can be studied in its historical context. Psychical research runs in fashions. At first in the 1880s and 90s the chief interest was spontaneous experiences, in which under the leadership of the great Edmund Gurney and others some very impressive evidence was obtained from rigorously investigated cases.³ For the next thirty years the main interest was in mental mediumship in which paranormal evidence was produced the truth of which was in some cases not known until the researchers made inquiries of people at a distance. I do not think the proved facts were evidence of post mortem survival but rather of mundane telepathy. Some observant and deep-thinking researchers studied the psychology of mental mediums, their trance states, and the processes of paranormal communication, shedding new light on the deeper aspects of the psychical process.

Behavioristic experiments, including card guessing, began in the 1880s. Edmund Gurney - doubtless the most creative experimenter our subject has ever known - was not content with visual targets, he also used methods involving the senses of taste, smell, touch and pressure. The modern vogue for card guessing was initiated by

G.N.M. Tyrrell in Britain, followed by Ina Jephson, Theodore Besterman, C.W. Olliver and others. In the 1930s J.B. Rhine put it into mass production. In the ensuing forty or fifty years in the United States and other countries millions of tests have been made at the cost of millions of money. Why then are the scientific establishments, especially in America, so hostile to our subject?

The answer is that parapsychologists after many years have failed to produce a repeatable experiment. That is to say an experiment that any competent person could perform with approximately similar significant results. In 1884 the physiologist Charles Richet and Edmund Gurney⁴ pointed to the need for such experiments. In 1891 Frederic Myers wrote of "experiments thus far made, although completely convincing to those who, like myself, have witnessed many of them, under very varied conditions, have nevertheless not yet passed into that desired stage at which one may be able to repeat them before any observer at any moment."⁵ Nearly ninety years after Myers the situation is unchanged. Occasional (and optimistic) claims to repeatability have passed into the limbo of forgotten things. Instead, we read of astronomical odds against chance, forgetting the warning of the greatest of modern statisticians Sir Ronald Fisher (an Honorary Member of the Society for Psychical Research): "Perhaps I may say, with respect to the use of statements of very long odds...that they are much less relevant to the establishment of the facts of nature than would be a demonstration of the reliable reproducibility of the phenomena."⁶

If in the next few years no truly repeatable experiment emerges we may find it chastening to consider the warning of the psychical researcher and psychologist Alan Gauld. Having pointed out that a repeatable experiment might well bring "rapid and widespread recognition" from the scientific world, Gauld added these words: "If, on the other hand, experimental findings continue indefinitely in their present somewhat unsatisfactory state, psychical research will not disappear, but it may well pass more and more into the hands of cranks as its present relatively respectable supporters drop away."⁷

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- ⁵Myers, F.W.H., "Harvest and Laborers in the Psychical Field," The Arena (Boston), September 1891.

⁶Fisher, R.A., Letter in "ESP Symposium at the A.P.A.," J. Parapsychol., 2 (1938), 267.

⁷Gauld, A., In Cavendish, R. (ed.) Man, Myth and Magic, London, 1971, Vol. 6, 2297.

COMMENTS BY JOHN PALMER:

Dr. Beloff has done an excellent job of comprising a list of experiments that exemplify if not constitute the best "justificational" evidence for psi. If I were making up such a list myself, I do not think it would differ substantially from his.

I was especially gratified to see the inclusion of the Musso and Granero experiment. I have always considered this to be one of the most elegant and evidential studies of the free-response type in the parapsychological literature, but it for the most part has been ignored by parapsychologists, perhaps because Musso is relatively isolated in Latin America.

If I were adding experiments to Dr. Beloff's list, my first choice would be to include some of the studies conducted by Dr. William Braud. His experiments in free-response ESP and PK are, in my view, among the most methodologically elegant in the field, and he has consistently obtained strong positive results in his work.

The one experiment in Dr. Beloff's list about which I have some reservations is the Brugmans experiment with Van Dam. While this was an excellent experiment for its time, the authors did not have the benefit of the kind of scientific give-and-take which caused experimental procedures in parapsychology to be progressively tightened during the 1930's and 1940's. Although Schouten and Kelly made a decisive case against there being any significant statistical artifacts in the analysis of this research, they were somewhat more circumspect in concluding that sensory cues had been eliminated (although they did state that they considered the sensory cue hypothesis to be very unlikely).

While Dr. Beloff has provided us with an excellent list, I must confess that I tend to shudder when anything like a list of "crucial" psi experiments is proposed. Fortunately, Dr. Beloff pointed out that any of these experiments may later prove to have been flawed, and he reminds us of the recent developments surrounding the work of S.G. Soal, which at one time looked unassailable. There is one sense, though, in which drawing too close an analogy between Soal's work and the experiments listed by Dr. Beloff would be hazardous. I vividly recall while reading Soal and Bateman's Modern Experiments in Telepathy¹ (before the scandal broke) how dramatically consistent and strong were the effects Soal reported, light years beyond anything I had seen elsewhere in parapsychology, including Rhine's work. I was particularly taken aback by reports that Shackleton's success completely evaporated when the researchers shifted from a telepathy to a clairvoyance procedure; most other research in the field suggests that this variable should

make little if any difference. The only other example I can think of in parapsychology where results have looked so clear for so long a period of time is the research of W.J. Levy, who of course admitted to fabricating at least some of his data. The results cited by Dr. Beloff, while still quite impressive, are much more within the mainstream.

I would like to close by pointing out that correlational data must also be taken into account in evaluating the case for psi. Statistically reliable relationships between psi scores and other variables are as good evidence for the reality of psi as are large excesses of hits. Moreover, to the extent that such relationships make theoretical or psychological sense - as they often do - the plausibility of the psi hypothesis is increased. I would refer the reader to my chapter in Krippner's Advances in Parapsychological Research² for a discussion of some of these trends.

NOTES

¹Soal, S.G., and Bateman, F. Modern Experiments in Telepathy. New Haven: Yale University, 1954.

²Palmer, J., "Extrasensory Perception: Research Findings," in Stanley Krippner (Ed.), Advances in Parapsychological Research, 2: Extrasensory Perception. New York: Plenum, 1978.

COMMENTS BY K. RAMAKRISHNA RAO:

The case for the existence of psi or for that matter any anomolous phenomenon does not rest with the results obtained from one or more experiments considered to be beyond all criticism. There could be no experiment that is infallible. Therefore, the call for a crucial experiment or the claim of proof based on a single experiment is unlikely to be anything but an exercise in futility.

Dr. Beloff's criteria for an acceptable "evidential" experiment are, by his own admission, insufficient to distinguish between the genuine and the spurious evidence. Soal's experiments are an excellent example that added safeguards do not necessarily guarantee genuine results. Even accepting Beloff's criteria, one may find his list incomplete and selective. For example, I do not see why Brugman's experiment should carry greater credibility than, let us say, Pearce-Pratt experiment (Rhine & Pratt, 1964). Lucien Warner's 1937 experiment is, from the point of safeguards, as good as any I know in this field.

The evidence for psi should be judged not piecemeal, but cummulatively. Recognizing that search for absolute certainty is a pursuit after a mirage, we should be content with assessing the probabilities and according relative values to the existence or otherwise of the phenomena in question. Since there could be no perfect experiment that would answer all criticisms for all the time to come, we need to set up criteria that would enable us to make a probabilistic estimate of the possibility of a phenomenon's existence. I think it is entirely feasible to make such an estimate concerning psi phenomena and I believe that it would be convincingly in favor of its existence.

For judging the credibility of an experiment in parapsychology we need to have the same criteria, no more and no less, as in other fields such as psychology. It is the cumulative effect of the credible experiments that would provide the overall evidence and the basis for belief or skepticism. In parapsychology there are several credible experiments including the seven in Beloff's list that would be regarded as unquestionably evidential if they were in any conventional field. The reason often given for raising additional questions of credibility in the case of psi experiments is the alleged anomalous and nonrepeatable nature of the phenomena. But neither of these two attributes is logically necessary for believing in the existence of a phenomenon. We cannot deny the existence of something simply because we do not know what it means. Nor are we justified in concluding that a phenomenon does not exist because it cannot be produced on demand.

Replication is not an all or none phenomenon. It admits of degrees. Psychological phenomena are not replicable to the same degree as physical phenomena are. Admittedly parapsychological phenomena are even less replicable than most psychological phenomena. This is not to say, however, that psi phenomena are not replicable. Honorton (1978) compiled a list of all the experiments known to have been carried out employing Schmidt's type of random event generator. Of the 54 experiments carried out in seven different laboratories, 35 reached a 5% level of significance. Studies employing meditation, hypnosis and ganzfeld stimulation as well as ESP and DMT studies gave comparable results with a replication rate above fifty percent (for a discussion of the role of replication in psi research, see Rao, 1979).

In cases where replication rate of a phenomenon is relatively low it is not absurd to raise nonconventional questions of credibility concerning evidence such as the honesty of the experimenter and/or his associates who have the primary responsibility for obtaining such evidence. If the rate of replication of a phenomenon is significantly lower than the frequency of fraud among scientists of comparable credentials, then those who argue in favor of the existence of that phenomenon have the responsibility to show that fraud is unlikely to account for the results. Conversely, if the observed frequency of replication is significantly higher than the known frequency of fraud, it is then not legitimate to raise the question of experimenter fraud without hard evidence.

My belief in the existence of psi is based on my judgment that (1) psi phenomena are replicated at a level significantly greater than manifest dishonesty or incompetence among scientists of similar stature, and (2) that there is indirect evidence that reasonably rules out conscious fraud on the part of the investigators. (A) Evidence favoring psi hypothesis was obtained by scientists who were initially skeptical of psi (e.g., Reiss, 1937; Carpenter and Phalen, 1937). (B) Significant ESP results were obtained by highly reputable scientists who published their results anonymously (e.g., Abbot). (C) Internal effects not ex-

pected by the original investigators were subsequently discovered in the data by later researchers (see Rao, 1977). The quarter declines in PK data, the salience and "U" curves observed in much of earlier card tests provide convincing evidence that fraud is an unlikely explanation for these significant results.

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COMMENTS BY JAMES RANDI:

It is significant to me that Professor Beloff cites four experiments that are at least ten years in the past and of such obscurity that only dedicated devotees of psi could know of them. I just cannot see the reward in looking up the details of experiments that are probably no better controlled and reported than those I am already familiar with. Their obscurity lends them no more validity than current work in the field. The remaining three tests, done more recently, are however more easily available for checking, and such investigation reveals that Beloff is indeed not too fussy about his standards.

The Kanthamani/Kelly tests of the mysterious "B.D." were properly exposed as simple card tricks recently by statistician/magician Persi Diaconis. This investigator found that the experimenters were, typically, allowing the subject to do things his way--a requisite of the conjuror--and being fooled by common "outs" and multiple end-point techniques, all part of the conjuring repertoire. The card tricks were recognizable as such even in the written reports, inaccurate and incomplete as they were.

Helmut Schmidt did indeed promise to show us some rather interesting results, but upon close investigation of his methods, it has been shown that (a) he has not been properly observed by an outside authority and (b) his tests have not been adequately replicated. The American Physical Society has taken tentative steps towards sending Dr. Ray Hyman of the University of Oregon at Eugene to observe Schmidt's experiments, and until a report is issued by such an observer, the Schmidt work remains classified as "well-intentioned but unproven."

To select out a set of Honorton's tests done at Maimonides is wishful in the highest degree. Failures there surrounding the successes add up to nothing statistically significant. One cannot select appropriate runs. Professor Beloff should not have to be reminded of this fact.

If these seven cases, or at least the three discussed above, are "evidential" material that Beloff sets before us as his offering, I suggest that he return to the mines in which he labors to seek richer ore. To attempt to establish a case for psi on these weak examples is hopeless. They are either obscure and distant in time and quality, or they have not stood close examination. Or, as in the case of "B.D." the conjuror, we know that once again the "experts" have been bamboozled.

COMMENTS BY CHRISTOPHER SCOTT:

Evidence for psi has two features which distinguish it from conventional scientific evidence. Firstly, it is negative evidence. The parapsychologist's aim is to get a result which cannot be explained. He then argues: I cannot think of any normal explanation so this must be a psi effect. The jump from "I cannot think of" to "there does not exist" constitutes the weakest link in the argument for psi, for people are not very good at thinking of plausible explanations of bizarre events - a fact well known to stage magicians and con-men. The mere assertion "I cannot think of an explanation" is hardly a solid foundation on which to base claims involving a radical reappraisal of existing knowledge.

Secondly, the evidence for psi is historical, not experimental. We believe that water is made of hydrogen and oxygen not because (to satirize Beloff) a certain Dr. Blogman was an experimenter of good standing well known to the chemical community who passed an electric current through water in The Hague in 1903 and got hydrogen and oxygen, but because you or I or anyone else can electrolyze water anywhere at any time. It is legitimate to say that the evidence on which our chemical knowledge is based consists of experiments; but the evidence for psi consists only of reports of experiments. The distinction is important because reports can contain errors. It may happen that the experiment as reported could not have a normal explanation but that the experiment as actually conducted could.

Thus, faced with a piece of evidence for psi, the task for the skeptic is to find a plausible explanation consistent with, not necessarily the report as published, but at least a plausible distortion of the report such as might represent the true events. This is a somewhat vague assignment. How "plausible" should one attempt to be? A successful psi experiment is a rare event, selected for its strangeness. A strange event should have a strange explanation. This seems to justify the skeptic in advancing an explanation based on at least moderately eccentric assumptions.

Turning to Beloff's "evidential experiments", no brief discussion can deal with all of them but let's look at the first as an example.

Brugmans et al. placed their subject, blindfolded and shielded, in front of a kind of chess board of 48 squares. Observed through a glass window in the ceiling, he attempted to indicate the square selected by the agent in the room above. In the original report (Beloff's first reference) 187 trials were reported, with a total of 60 hits. In a recently published re-analysis based on the original records (Beloff's second reference) Schouten and Kelly reported on the complete series of 589 trials, which yielded 118 hits.

The original report, dated 1922, was seriously deficient on crucial aspects. While the experiment was widely acclaimed, over several decades, as providing exceptionally strong evidence for psi, it ultimately came under attack on two grounds: failure to eliminate sensory leakage and inadequate target randomization. Schouter and Kelly (1978) addressed these two issues at length, using previously unavailable evidence, and largely succeeded in eliminating them as alternative explanations. Typically, having dealt with the critics they stopped there and concluded that the experiments were "fundamentally sound." They did not look for other loopholes. Beloff, similarly, reports that he "knows of no serious attempt to undermine" the experiment and shows no inclination to launch such an attempt himself. It is easy to argue "I cannot think of an explanation" if you don't try very hard to think of one.

Yet there is a glaring deficiency in the experiment which everyone seems to have overlooked. It is reported that the agent knew the correct target, but it is not reported who recorded the response. We know that two "observers" were present with the agent, but their role is not defined. If the agent himself was recorder, we have a design which would not be accepted in any modern parapsychology laboratory. In the present case we have some reasons for misgiving. The window through which the observations were made was small (32 x 52 cm.); it is known that the agent himself was required to watch the subject's movements intently and the question arises whether a second person would have found it convenient or practical to watch through the same window or whether it was left to the agent to report the results - either verbally to one of the observers or by making his own record. Even if the observers were the recorders (they did not know the true target), how can we be sure they were not influenced by the agent? (If they saw G3 and the agent cried out: "That's it: G4 - a hit!", can we really be sure they would record G3?) Much depends on the clarity of the response. Schouten and Kelly claim - though Brugmans says nothing of this in his original report - that the subject was required to tap twice with his finger on the chosen square "if he felt that he had reached the target area". Was this signal always clear? What happened if he never tapped? Did they ultimately give up waiting and accept the place where his finger was currently hovering? What happened if he responded too quickly, before the next target was ready? In the absence of any communication link between the rooms or a timing device to indicate when

a new trial was starting, there must surely have been many moments of confusion and uncertainty. Again, the number of trials was highly variable from one session to another: how was it decided to stop a session? All these uncertainties leave ample room for biased recording. At least in his original report, Brugmans gives no sign of having considered these issues.

Could recording errors really account for such a high rate of success (1 hit in 3 trials in the initial experiments)? It would be hard to believe this if the agent really was giving his two-taps signal. But suppose this was used only occasionally, or was only introduced towards the end of the experiments. Or suppose this method represents an intention which was never actually implemented. In other words, suppose the Schouten and Kelly report is slightly mistaken on this one issue. In these circumstances a naive and highly motivated enthusiast (and anyone who reads Brugmans' original article will surely accept this as a fair description of him) might perhaps achieve such a high rate of motivated error. We have to remember that this experiment has been selected as one of the seven best among tens of thousands which were either wholly negative or less rigorous in methodology. If you try long enough you can ultimately expect to run across some fairly eccentric behavior.

In the end we are left with the question whether telepathy is more likely than an unusually extreme degree of motivated recording error occurring just once in 10,000 or more experiments. Or, conceivably, there may be yet another explanation which no one has yet thought of. How can we be confident one way or the other? In the opinion of this author, no one need feel obliged to accept the overthrow of well established scientific principles in order to accommodate evidence whose implications are as uncertain as this.

COMMENTS ON CHRISTOPHER SCOTT'S REMARKS BY SYBO SCHOUTEN:

(Schouten's comments were received by ZS through Dr. Beloff in time for this issue so are published here--out of alphabetical order. Christopher Scott will reply to Schouten in a future issue of ZS. -- MT)

Dr. John Beloff drew my attention to Christopher Scott's suggestion that the results of the Heyman's experiment might be due to recording errors. The possibility of such errors which can take various forms is well known to parapsychologists. Whether recording errors have played a role in this experiment can be judged on two grounds. Did the procedure make it possible and can trends be found in the data suggestive of such errors?

As regards the procedure we know from various sources: that the onset of the trial was indicated by striking the floor with a hammer since verbal communication between the rooms was not possible (Carington, 1938); that when the subject felt he was 'on the spot' he signalled by tapping twice with his finger (Meededeelingen, p. 66, 68); that the observer was able to follow the movements of the subject's hand (*ibid.*, p. 69) and that the observer could check the target after the trial was completed by turning two stacks of cards of which the bottom cards indicated the

target (*ibid.*, p. 78). Hence it seems unlikely that recording errors due to uncertainty as regards the onset or termination of the trial or due to faulty recordings of the targets have played an important role. Since nothing is stated concerning the recording of the responses we cannot judge about the adequacy of the procedure in this respect. However, we do know that at every trial one or two observers were attending so that a certain amount of control must have existed.

As regards the data no trends were observed which could support a recording-error hypothesis. On the contrary, some findings rather contradict such a hypothesis. Firstly no significant difference was found in the subject's scoring when the different experimenters acted as agent. This implies that if recording errors were made all experimenters made such errors to the same (large) extent which seems unlikely. Secondly, assuming that the experimenters had a tendency to rate a near miss as a hit, by recording wrongly the response on an adjacent square as a response on the target square, we could expect, considering the excessive number of hits, a significant low frequency of responses on squares adjacent to the target square. However, the data yield a non-significant trend in the opposite direction. The analyses of misses show that in both sets of responses there is an excess of responses on a square adjacent to the target square (distance 1 in the distance analyses: MDR set $p(E)=.08$, $p(O)=.13$; LDR set $p(E)=.08$, $p(O)=.15$).

Whether recording errors offer a satisfactory explanation for the observed scoring is for everyone to decide for themselves. In my opinion neither the procedure nor the data support such an hypothesis.

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COMMENTS BY REX G. STANFORD:

There are definite differences of opinion among parapsychologists about what should be considered adequate evidence that we have discovered and are concerned with something meaningful, i.e., that parapsychology has a bona fide subject matter to study. Members of one camp--let us call them the "demonstrationists"--feel that real psi evidence comes from what might be called "crucial demonstration experiments." They tend to get excited about "dramatic demonstrations" of supposed psi. In the case of ESP such a demonstra-

tion often means that a single "star subject" produces some very improbable outcomes. (At another time the "star" may "fail" or "fall," and no one really knows why except for ad hoc explanations.) Such demonstrations show only that, at best, an anomaly has occurred. Beloff's contribution on which I am commenting seems to reflect largely this demonstrationist attitude.

A major opposing camp might be called the "constructivists." (This term refers to their emphasis on building a structure of systematic evidence which converges on a hypothesis concerning psi function and to their insistence that "ESP," for example, be developed into a hypothetical construct, not merely treated as a summary term for an anomaly.) Constructivists may well sympathize with the opinion of the late E.G. Boring that the anomalies observed in the demonstration experiments are simply "empty correlations" (e.g., between guesses and targets), that one can hardly claim to have "demonstrated a phenomenon" until one can show with reasonable clarity under what conditions one will or will not be able to observe the event in question. We who advocate this position worry about the testability of a "construct" which is not really a construct but involves only the labeling of a negatively-defined event (e.g., target-response correlations in the absence of sensory contact). Thus stated, ESP is not even a hypothesis in the usual sense of that term, for antecedent conditions are not specified. The ESP "hypothesis" of the demonstrationists tells us what to look for, but not when or where it will or will not be observed. ESP in this sense is not a testable proposition or construct, for it cannot be falsified. It is only a label for an unpredicted, anomalous observation. Constructivists, therefore, believe that our case for the reality of psi events must depend upon the degree to which our research outcomes truly converge upon some hypothesis or construct about the nature of those events. Of course, such research must involve careful methodology and some degree of replicability. Indeed, the advance of knowledge occasioned by such an approach should increase replicability. If after almost one hundred years of psi research we cannot do more than cry "anomaly," we should either proclaim the whole topic as scientifically intractable, throw in the towel, and stop wasting time, money, and effort; or we should call in a whole new team of more competent scientists. Fortunately, the situation is much brighter than that, but the demonstrationists have trouble realizing it.

Beloff's demonstrationist presentation is not impressive. Frankly, if I did not know what I know about the developments in psi research, I might conclude from his discussion that parapsychology has made hardly any advances, except technologically. Some of the studies he discusses have real problems, but I will not consider them here. What I want to focus on is that regardless of how hard we try, this demonstrationist emphasis can never do much to persuade those who feel this field is concerned, not with "extrasense," but nonsense. Those wishing to examine whether or in what degree psi research is converging on any hypotheses or constructs should, first, examine the useful reviews of particular, experimental areas which can be found in recent, edited volumes (Krippner, 1978; Wolman, 1977), examine some of the original work discussed there, and form their own opinions about the adequacy

of the methodology, the consistency of the findings, and the validity of the conclusions.

My personal observations suggest that whenever "skeptics" have begun to take parapsychology somewhat more seriously it has been precisely because of their awareness that a convergence of evidence is suggesting something meaningful, not because they have been knocked over by startlingly small probability values or demonstrations in the abstract.

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JOHN BELOFF REPLIES TO HIS COMMENTATORS:

At least two of my critics (Cohen and Scott) have rebuked me for claiming that my seven experiments together "represent an overwhelming case for accepting the reality of psi." Perhaps I was being needlessly provocative at that point, but I did advisedly add the qualifying clause: "it is my personal opinion." I was, of course, fully aware that, as this debate has so well shown, what may strike one person as overwhelmingly convincing may carry no weight at all with another. I must insist, however, that my opinion does not depend on any "leap of faith," as these critics aver, except, that is, insofar as any conjecture we may make involves taking some risk. That there are paranormal phenomena is no more than a conjecture, but it is in no way less rational than the conjecture that there are no paranormal phenomena. I am as much entitled to the one as my critics are to the other. Irvin Child objects to my phrase 'the reality of psi' on the grounds that it implies that we know what psi is whereas, on the only definition of psi on which there is any agreement, we know only what psi is not. I still find the phrase convenient and I shall continue to use it but, of course, Child is strictly correct so perhaps a more guarded statement of my present position should read somewhat as follows: "these seven experiments represent a strong case for rejecting, or, at any rate, questioning the sufficiency of the prevailing scientific world view if this is taken as defining the limits of what is possible."

I want now to deal with the criticism stated at length by Rex Stanford but implicitly endorsed by Bob Morris that this whole approach was misconceived. Parapsychology, they argue, cannot be vindicated by citing any set of isolated experiments, however worthy in themselves; its validity as a science must stand or fall on the basis of a general survey of the relevant literature. Let me say straight away that I am very far from being what Stanford chooses to call a 'demonstrationist.' I might go even further than he and say that the experimental evidence is not always the most convincing evidence. The records of spontaneous, real-life phenomena may be just as important for reaching a valid conclusion about the nature of psi as the reports of observations under controlled conditions. Scott has a point, surely, when he reminds us that, in the absence of any clearly repeatable experiments, parapsychology is bound to remain primarily an historical study. But, when all this has been said, the skeptic still has a perfect right to demand from us specimens of what we regard as good experimental evidence for psi. To refuse to commit ourselves would be not only a dereliction of duty but would lay us open to the charge of prevarication. Obviously, if there was even one reliable psi effect that we could point to we would not need to play this rather artificial game; we could simply tell our critics what to do in order to test it for themselves. But such a happy state of affairs is not yet within sight. In the meantime all we can offer are accounts of what particular researchers have found on particular occasions.

My own list of specimens was compiled in response to a direct challenge from Dr. Nils Wiklund, a Swedish psychologist and a friendly

critic of parapsychology. To have responded to Wiklund along the lines suggested by Stanford, i.e. to exhort him to scan the recent literature to see "whether or in what degree psi research is converging on any hypotheses or constructs" would be to evade his challenge, not to meet it. In Dr. Wiklund's case, moreover, it would be an impertinence since he is already thoroughly conversant with this literature.

If parapsychology were a regular science, rather than the extremely deviant science which it is, then there could be no question that the Stanford-Morris position would be the correct one; but, in the circumstances, it can only suggest an alarming lack of realism. Palmer, too, is no doubt correct in saying that: "statistically reliable relationships between psi scores and other variables are as good evidence for the reality of psi as are large excesses of hits"; but, again, it would require a great deal of special pleading to show that any such relationships are, in fact, reliable. Of course, one can always discover consistencies in the data if one looks for them but, equally, one can point to the most glaring inconsistencies. But even when consistent relationships are forthcoming these may turn out to be artefactual. One recalls sadly now the imposing constructions that were erected on the basis of the extensive work Soal did with his subjects, Basil Shackleton and Gloria Stewart, which looked meaningful enough at the time and yet now it looks as if the whole of this data-base may have to be discarded as invalid and fraudulent.¹ As for this talk about "converging constructs," I would have said that the one indisputable feature of the parapsychological scene is the almost total lack of consensus to be found on virtually every question even the most fundamental. Each of us has his own preferred way of conceptualizing the phenomena, and Stanford himself has made notable contributions on the theoretical side; but the models he has proposed at different times are themselves mutually incompatible in a way that suggests, not convergence, but the zig-zag maneuvers characteristic of a "preparadigmatic" science.

Stanford overlooks also the extent to which the pseudo-sciences can likewise produce "convergent constructs." One wonders whether, confronted by the supporters of some belief-system which he did not accept, let us say astrology, he would meekly adopt his own precepts and be content to immerse himself in its labyrinthine literature which is certainly not lacking its own internal consistencies. My guess is that he would, before wasting any time, demand of them that they show him some truly watertight piece of evidence for the validity of some specific astrological hypothesis (perhaps of the sort that Gauquelin might be ready to provide?). It is because Stanford takes for granted the existence of psi that he can treat the skeptic's legitimate requests so cavalierly. His whole argument, therefore, strikes me as a classic instance of mistaking the direction in which one wants to move for the place where one happens to be!

I come now to my particular choice of experiments. Fraser Nicol rightly reminds us how arbitrary any such selection must be so I was relieved and gratified to find that my judgment was for the most part supported by such authorities as John Palmer and Irvin Child who know the experimental literature much more thoroughly than I do. I was

especially delighted to read Child's constructive comments which draw attention to features or aspects of these experiments which I had ignored but which could have worthwhile implications for future research. I was also encouraged to read the outcome of fresh analyses which Musso and Granero have since performed on their data from their experiment. I suppose a critic could complain that such post-facto findings are of questionable value from a statistical point of view since there is no longer any possibility of following up such findings with further replications. However, given the excellence of the original experiment, their discovery that the "good" sessions reveal a salience effect that is absent from the "bad" sessions can only strengthen my conviction that their findings are genuine.

The only one of my seven experiments which was faulted in a way that I had not anticipated was No. 7, the Terry & Honorton study; and here the flaw was pointed out, not by any of the present commentators but by Nils Wiklund himself. It is true that it is only a minor methodological flaw, but it could theoretically have inflated the overall scoring. The point at issue, very briefly, is that in order to avoid using the same target when the same set had been selected (each set had four potential targets), the picture-reels were taken in rotation as targets instead of strictly at random as they should have been; and an astute subject, knowing this, could have taken advantage of the fact. I must point out, however, that No. 7 is the only experiment on my list which was not chosen as capable of standing on its own feet - the significances were anyway quite mediocre - but as the representative of a string of similar "Ganzfeld" experiments which, so far, have a better record for repeatability than any other type of experiment in recent years. The flaw that Wiklund pointed out is, I believe, peculiar to this experiment alone. I was keen to include at least one experiment which did not depend on the use of special subjects; but, had I been more alert, I would have chosen one that was methodologically more sound, perhaps, as Palmer suggests, from the work of William Braud.

Some doubts have been expressed concerning the earliest example on my list, the Brugmans experiment of 1920. It seems to be generally agreed that precautions against sensory leakage from the room above where the agent was situated were less than foolproof. Even so, it is hard to imagine how such massive and robust effects could have been due to inadvertent cueing from the agent. Christopher Scott has suggested misrecordings as a possible source of error and bias; but, again, misrecordings on such a scale would suggest deliberate falsification rather than carelessness, and this would be something else again. Fortunately for me Scott's hypothesis has already been dealt with by Sybo Schouten who is now the leading authority on this investigation. He found, for example, that there was no decrease in the number of "near-misses" as one would expect if near-misses were being carelessly recorded as hits. And Scott's hypothesis becomes even more tenuous when we discover that the roles of agent and recorder were duly rotated among all three experimenters, including the prestigious Professor Heymans himself, and that this made no difference to the pattern of scoring by the subject, van Dam.

The other experiment which aroused some controversy was my No. 5

with the subject B.D. (see Alcock and Randi). Here, too, my task has been eased by the fact that it has already been discussed at great length in the interchange between Kelly and Diaconis in the ZS issue No. 5, 1979. It is, I trust, clear to all by this time that Diaconis never attended any formal experiments with B.D., he merely witnessed the informal demonstration which B.D. gave at Harvard where he suspected (not, as Alcock would have it "detected") trickery. The formal investigation, to which my citation alludes, was conducted over many months at the FRNM Institute for parapsychology by Kanthamani and Kelly. They had full control over the conditions of testing and the set-up was so simple that one can say without fear of contradiction that there was just no conceivable way in which the subject could have cheated however skillful he may be at card-tricks. I would add that, although card guessing constituted the bulk of the testing, he also scored very significantly on a number of other tests including the automated 4-button Schmidt machine where, on one session alone while being watched by both experimenters he registered a C.R. score of over +10 on a series of 900 trials. If Randi would care to tell us how one can fake a score on a Schmidt machine while being observed, he would be saying something that would be worth listening to. Alcock expresses surprise that I never mentioned, as a possible pitfall, conscious or unconscious cheating by the subject. The straightforward answer to this is that an experiment in which it is possible for the subject to cheat in any way at all is, quite simply, an invalid experiment and no editor or referee who knew his business would allow such an experiment to be published. If I were asked to cite a single evidential investigation from the entire literature of experimental parapsychology as best demonstrating the reality of psi, then I think this is the one that I would choose; but, naturally, my judgment is influenced by the fact that I am personally acquainted with both the chief experimenters and have implicit confidence in their competence and integrity.

Randi's brief contribution nicely illustrates the danger of intervening in a controversy without first doing one's homework. Had he done so he would not have confused my No. 5 Experiment with the informal demonstration which Diaconis witnessed at Harvard on the basis of which no conclusions, positive or negative, could be drawn. Nor would he have accused me of selective reporting in citing the Terry and Honorton experiment without mentioning other unsuccessful experiments which have been carried out at the Maimonides Laboratory. Is Randi seriously suggesting that, for the purposes of evaluation, all experiments conducted under the same roof, even if they may have nothing else in common, must be pooled? There would certainly be a case for lumping together all experiments using the Ganzfeld set-up which Honorton devised. But that is precisely what Honorton himself has done in the reference I have listed (see his chapter of the Handbook of Parapsychology), and he found that even when all the known failures were taken into account the overall result was still highly significant.

I will now conclude by stating what I consider to be the important lessons which have emerged from this exchange of view which our editor has engineered, and I hope that what I have to say will command the widest consent whatever other differences may still

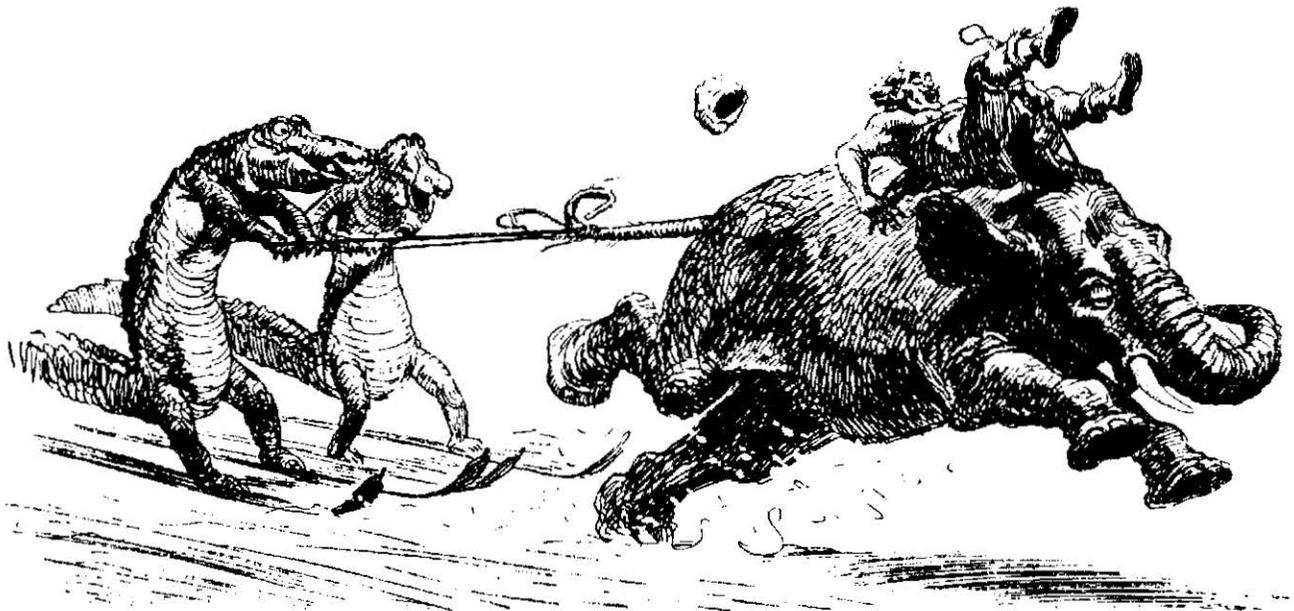
divide us:

(1) No list of experiments, however long the list and however impeccable the experiments, can suffice to establish a paranormal claim. For, as Collins points out, it is always possible in the last resort to make any experiment look unscientific if you are prepared to nibble at it long enough and rehash it in anecdotal form. The only way the argument ever could be resolved would be if it were possible to perform the experiment for oneself with a reasonable prospect of a successful outcome and whether or not that ever will be possible is itself an undecidable issue.

(2) Which side you take, therefore, or whether, like Collins himself, you prefer to remain on the fence, must depend on the pre-suppositions that you bring to bear on the evidence. Thus, if you start from the classic Humean view, the one I take to be shared by Cohen and by Scott, you know in advance that psi is impossible; and if, therefore, the evidence should suggest otherwise, you know that it is because you have not yet been able to think of an appropriate counter-explanation. This is a safe option, and it cannot be faulted; but it does of course commit you to an extremely conservative stance. If, on the contrary, you come to the evidence already predisposed to believe in psi (as I am), whether as a result of personal experience, hearsay, historical or background knowledge, philosophical intuition or any other reason, good or bad, then you are unlikely to be impressed with the kind of counter-explanations which Scott & Co. can offer; and it will seem far more natural and plausible to accept the evidence at face value.

NOTE:

¹ One reason, I feel sure, why the late J.G. Pratt was so reluctant to believe that Soal could have cheated, throughout all the controversy which surrounded his work, was the fact that his own reanalysis of the Stewart data had revealed consistent and meaningful effects in the scoring pattern which Soal, himself, had never noted. Yet, despite this, it now looks as if the Stewart data will have to be written off along with those of the Shackleton series.



ZS Dialogues

EDWARD F. KELLY RESPONDS TO PERSI DIACONIS'S REPLY (ZS #5) ON "STATISTICAL PROBLEMS IN PSI RESEARCH" :

The "Rejoinder" to me which Persi Diaconis has published in the previous issue of this journal is insufficient both in what it does say and in what it does not. It fails to address or even acknowledge many of the specific and correct criticisms I made of his Science article; and what it does say continues the basic pattern of misrepresentation, unsupported allegation, and unwillingness to examine experimental evidence that characterized the original article.

I strongly urge the reader who has followed our exchange thus far to go back and reread carefully what Diaconis wrote and what I wrote in rebuttal. Although that exercise should in itself go a long way toward justifying my introductory statement here, let me also reinforce it by replying more specifically to the main contents of the "Rejoinder," using its headings.

Point 1 - Feedback. In responding to my discussion of the section on feedback experiments in his Science article, Diaconis diverts attention not only from several minor points but from my main point. He does this (a) by shifting the weight of his remarks to a different class of cases than dominated his original discussion; and (b) by grossly misrepresenting what I said in my reply.

Diaconis attributes to me the position that "My results on feedback experiments are irrelevant to ESP research; and, anyway, the essence of my findings are (sic) well understood by parapsychologists (p. 29)." I did not say either of these things. Rather, it is precisely because they recognize the poorly-understood complications arising when trial-by-trial feedback is coupled with dependencies among successive targets that knowledgeable researchers have previously, in all but a few atypical instances, avoided the situations. I said therefore - correctly - that his remarks about feedback (among other subjects) are largely irrelevant to the existing literature. I meant specifically to refute the unmistakable and false implication, central to Diaconis' original discussion of feedback in his Science article (p. 134ff), that many researchers routinely carry out experiments in which trial-by-trial feedback of some type is given in deliberate conjunction with a closed-deck card-guessing procedure. I specifically acknowledged that his analytical results might prove useful for future research using such procedures. I also did not - and would not - claim that no examples could be found in the literature with possible problems associated with feedback. The examples he now dwells upon, however, are of quite a different sort, in which dependence is introduced inadvertently, through flaws in procedure or randomization, rather than by deliberate choice of manifestly correlated-trial designs. Such examples I agree are real, but they too are very uncommon in the literature. The particular studies Diaconis cites have also been appropriately and even severely criticized in the parapsychology literature itself for various weaknesses including those he points out (for example, Gatlin, 1979; Honorton, 1979; Kennedy, 1979 a,b; Stanford, 1977; Stokes, 1977).

Diaconis notwithstanding, I do understand the problems of feedback. I certainly did not claim, as he alleges (p.29) that anybody knows precisely how to adjust for the possible effects of partial information in every such case. At the same time I dispute the implication that the presence of such effects is automatically fatal for any study in which they could possibly have occurred. That needs to be investigated on a case-by-case basis. As I already indicated in my first reply (footnote 2), I do not believe that such factors can plausibly be regarded as eroding the available evidence in more than trivial degree, even in the bulk of those relatively few studies where they may be relevant.

Finally, I did not claim, as Diaconis implies (p. 30), that technically correct methods of analysis exist for every conceivable type of "modern complex" experiment, but only for certain classes of such experiments. These include the commonest type - free response to complex target material using an independent-trial testing/judging regime - and specifically exclude closed-deck procedures with trial-by-trial feedback, whether with forced-choice or free-response target material (Kelly, 1979, p. 22; see also Burdick and Kelly, 1977; Kennedy, 1979 a,b; Morris, 1972; Solfvin, Kelly, and Burdick, 1977). Diaconis' statement in the Science article (p. 134) that "the statistical tools for evaluating the outcome of more complex experiments are not available" is simply false, unless "more complex experiments" is arbitrarily restricted to a narrow class of cases which is almost entirely absent from the literature. If "dependence abounds" in some "complicated" experiments, it is not simply because they are "complicated" but because they are complicated in particular ways that most researchers normally can and do avoid. And if "many followup studies" (p. 29) are being carried out with defective protocols (which I doubt), it is because their authors have not paid sufficient attention to the abundant warnings in the mainstream parapsychology literature.

Point 2 - B.D. Diaconis is correct - I did not admit that B.D. used sleight-of-hand at the Harvard performance. Neither did I deny it however. What I did say - and say again - is that our opinions on that score are irrelevant. The relevant question is whether the conditions in force during the various formal experiments with B.D. were safe against subject cheating. Without once referring to the particulars of these experiments - which were performed as described and in sharp contrast to the entirely uncontrolled conditions at Harvard - Diaconis concludes without qualification that because no expert magician was on hand to observe the experiments, their results can be entirely dismissed, out of hand, as just "another amusing curiosity, not scientific evidence of any kind" (p.30).

Diaconis states simply that none of the reports mentions "any such explicit precautions" against cheating. The key word here is "such." As described very briefly in my first reply and at length in the published reports cited there, explicit precautions were taken; they were just not of the form Diaconis demands. The conditions of virtually all contemporary experiments are specified entirely by experimenters, not by their subjects, and are routinely designed to be safe not only against subject cheating but against all known forms of sensory leakage. Some of my own experiments with B.D. in fact fell short of the normal standards in respects which are clearly stated in the original articles. However, I see no concrete reason to suppose that any of these shortcomings invalidated either the data or the main statistical analyses performed.

Diaconis explicitly asserts (1978, p. 133) that the experimental conditions as described in the published reports were sufficiently similar to those of the Harvard demonstration to warrant discounting all of the associated results. However, the only concrete basis of similarity he has so far produced is the mere fact that B.D. participated in both sets of events. I do not believe it is fair or reasonable for any critic to make this sort of generalized suggestion of possible cheating without offering any specific hypotheses or evidence as to how it might have occurred. I would also point out that in a case such as Kelly, Kanthamani, Child, and Young (1975), a specific cheating hypothesis will have to explain a great deal more than the mere occurrence of excess direct hits. In particular it will have to account for a complex pattern of systematic errors that shifted systematically in relation to the overall scoring rate. Again, I urge readers to consult the original articles to form their own judgments about the quality of the conditions.

More generally, while agreeing fully that a magician's skills are highly relevant to self-staged and non-experimental performances, I dispute Diaconis' suggestion that the presence of a magician constitutes a necessary and sufficient condition for the integrity of experimental data. Indeed I think it is clear that this criterion fails in both directions. It is unnecessary because it is perfectly possible - indeed not all that difficult - to design experimental conditions which are impervious to cheating by any subject including a magician. And it is insufficient because if positive results were forthcoming, what would prevent a sufficiently resolute skeptic from claiming that the conditions were after all inadequate, and that the subject-magician was simply more skilled than the observer-magician? Like all other proposed suggestions for creating unassailable "demonstration" experiments - for example by multiplying independent experimenters, appointing blue-ribbon panels of observers and the like - this criterion will fail in the presence of sufficiently hardened skepticism. Along with most contemporary workers I believe the "ultimate" experiment - the perfect study that will convince everyone - is a chimera. The way we will convince people is by getting on with the regular business of experimental science conducted in its regular way, through systematic controlled experimentation that attempts to reveal the characteristics and conditions of occurrence of the phenomena at hand. That is the spirit in which the work with B.D. was conducted. I certainly do not claim that our experiments were absolutely flawless in every detail. Like virtually everyone who has ever published anything I can look back and see some things that I would like to have done differently, or additionally. Nevertheless, I myself think that these were good experiments which produced both valid and interesting results. I believe this is a reasonable position, and I will continue to hold it until someone comes forward with specific arguments and evidence to the contrary.

Diaconis' sweepingly negative attitude toward the experiments with B.D. therefore seems to me unsupported and unreasonable - and will I think also appear so to open-minded persons who study the original reports. Nevertheless, I honor albeit grudgingly his right to hold it, as a specific personal opinion about these specific experiments. What I object to is the fact that he has deliberately encouraged others to suppose (a) that our formal experiments with B.D. were just like the informal demonstration he witnessed at Harvard; and especially (b) that

the Harvard conditions were more generally typical of what passes for experimentation in psychical research. I am naturally displeased by the treatment accorded my own work. However, Diaconis' more general claim is a far more serious matter. There, his position is in my opinion unambiguously irresponsible. That is really the core of the matter, as I will now discuss.

Point 3 - not a survey. In the "Conclusions" of his Science paper, Diaconis wrote:

Poorly designed, badly run, and inappropriately analyzed experiments seem to be an even greater obstacle to progress in this field than subject cheating. This is not due to a lack of creative investigators who work hard but rather to the difficulty of finding an appropriate balance between study designs which both permit analysis and experimental results (sic). There always seem to be many loopholes and loose ends. The same mistakes are made again and again. The critiques and comments of Davey and Hall seem as relevant for modern studies as they did at the turn of the century.

Similarly, in his "Summary" he wrote:

[In search of repeatable ESP experiments, modern investigators are using more complex targets, richer and freer responses, feedback, and more naturalistic conditions.] This makes tractable statistical models less applicable. Moreover, controls are often so loose that no valid statistical analysis is possible. Some common problems are multiple end points, subject cheating, and unconscious sensory cueing.

All of the statements I have underlined are wildly inaccurate when applied, as Diaconis intends, to the main body of existing experimental literature in parapsychology. How did he arrive at these sweeping and erroneous generalizations? His position rests upon two complementary theses: (a) that his admittedly limited experience with the field does provide an adequate basis on which to appraise the state of contemporary research; and (b) that one cannot appraise this field in the normal scientific manner - i.e. through study of its professional literature, replication of published studies, etc. Both of these theses are false:

(a) Diaconis creates an appearance of candor by acknowledging that his experience of the field is limited and biased, but in fact he characterizes the nature of that bias in an extremely misleading way: "I tend to get called in on more sensational studies such as trials done by experienced researchers who have had an impressive success" (p. 30). Thus he attempts to justify generalizations intended to apply to the entire field by intimating that the specific situations he has witnessed and discussed in his article, while not a random sample of the research (as if anyone would seriously propose that as the way to find out what is known), nevertheless are faithful specimens of the best experimental work being done by the best people in the field. That is simply not true. What force Diaconis' observations and generalizations have - like those of Davey, Hall, etc. - they have primarily in relation to situations of precisely the type in which Diaconis himself has directly participated, situations involving star subjects operating under non-experimental conditions largely controlled by themselves, in which the investigator is reduced to the status of a participant observer. But this is completely uncharacteristic of the vast majority of modern investigations, and generalizations to the latter from his limited and unrepresentative

experience are correspondingly false and misleading. Had he looked at the literature even superficially he would have known that.

(b) The justification Diaconis gives for discounting the whole remainder of this experimental literature is likewise defective. He intimates that the literature is pervasively but secretly corrupted with problems of the sort that can arise in non-experimental situations such as the Harvard demonstration:

Regrettably, the problems are hard to recognize from published records of the experiment in which they occur; rather, these problems are often uncovered by reports of independent skilled observers who were present during the experiment (1978, p. 136, underlining mine).

He claims to have found "many examples" of this type, but where are they? I will again let Jule Eisenbud and the SRI people speak for themselves. In the case of B.D., Diaconis did not participate in the experiments and has not found any specific problems. He merely suspects that such problems exist. Moreover, what undisclosed suspicions he has, he specifically states are based on the published reports. The only experiments he ever discusses, and those only in the most cursory way - some early remote viewing work and Tart's first "learning" study - have in fact readily been identified as having possible problems, by critics and parapsychologists alike, on the basis of their published reports and through reanalysis of their data by other investigators. Furthermore, as I stated in my Reply (p. 22), the great bulk of modern experimental studies involves relatively unselected subjects working under strictly controlled conditions where the kinds of gross problems Diaconis describes simply do not arise. In short, no compelling reason has been or can be given to support the radical and repugnant thesis that the normal scientific mechanisms for conveying information and ferreting out error cease to operate where psychical research in general is concerned. Our people can and regularly do identify problems in each other's work, and they do it on the normal basis of literature review, reanalysis of data, and attempted replication based on published experimental reports. No responsible critic has the right to cast aside the normal procedures in favor of innuendo based almost entirely upon fringe literature, second- and third-hand hearsay testimony and speculation, and informal demonstrations.

Every fair-minded parapsychologist would admit that our field has an ample supply of real problems. It would be gross exaggeration to claim - as Diaconis has me say (p. 29) - that the literature contains a "huge volume of studies beyond reproach," but there are many hundreds of strictly experimental studies. Many are good, and some are very good. As in every other field, specific examples can readily be found in various kinds of faulty design and analysis. But to characterize this entire literature as corrupted throughout with crude and elementary methodological blunders of the sort Diaconis proclaims is simply erroneous and perverse. The plain fact is that Persi Diaconis - like many other self-constituted authorities and critics past and present - has manifestly not taken the trouble to become familiar with the actual experimental literature of the field. Not only that, but he displays little willingness to do so. Even after being clearly informed by myself and numerous others concerning the many appalling inaccuracies and irrelevancies of his Science article, Diaconis goes on broadcasting the same theses as though no shadow has been or could be cast upon them. Is this

conduct consistent with his repeated protestations that his attitude to the subject is one of open-minded scientific skepticism?

In my short time working in psychical research I have repeatedly been amazed by the willingness of otherwise good and even eminent scientists to form - and often to proclaim publicly - sweeping judgments pro or con on this subject when it is clear from what they say that they know practically nothing about it. Why is it that would-be critics of this particular field apparently do not feel themselves bound by normal standards of scholarly care? And why is it that even the most undisciplined attacks readily gain access to the pages of scientific publications that are virtually closed to responsible pieces of parapsychological research and criticism? Readers curious enough to want to see a variety of dissected specimens of irresponsible criticism, old and modern, should consult respectively the collections of Prince (1975) and Rao (1980).

For people who have the impression that psychical research is making little progress, that's something to think about. They might also consider the related fact that the entire professional research effort is being carried forward on a shoestring by a tiny number of workers. I estimate that 90% of the useful research is being carried out by perhaps fifty people worldwide, many working under material circumstances that could at best be described as disheartening. For example between 1973 and 1978, the most recent years for which figures are available, thirteen leading American centers for psychical research spent an aggregate total of \$552,000 per year, with a median individual budget of \$17,000 (Tart, 1978). Anyone who fancies these researchers to be cynical opportunists capitalizing on the ignorance of a credulous public is sadly misinformed.

Psychical research is here to stay. Whether we find the phenomena philosophically unpalatable or not, they are real, and scientific knowledge generated in the normal scientific way is slowly but surely being accumulated about them. Undisciplined critical attacks will not stop this process, but they can impede it by preventing the general scientific community from forming an adequate image of the field, one which reflects both its progress and its problems in an accurate, representative, and realistic way. The most reliable way to find out what is going on in this as in any other scientific field is to consult the professional literature (see for example Wolman (1977) and Krippner (1978) for recent surveys). If even a few genuinely open-minded persons are moved to do that by my efforts here, then those efforts will have been worthwhile.

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CHARLES T. TART COMMENTS ON PERSI DIACONIS'S REPLY (ZS #5)
ON "STATISTICAL PROBLEMS IN PSI RESEARCH":

I would like to correct some points made by Persi Diaconis in his rejoinder to Edward Kelly in the last issue of ZS.

First, Dr. Diaconis states that "Not long ago, Goldman, Stein, and Weiner showed that Tart had been using a faulty random number generator in a feedback experiment. Feedback introduces the possibility that subjects could notice the patterns in the guessing* sequence . . ." Lila Gatlin was actually the first person to call my attention to occasional small departures from an equiprobability and serial independence model in some target sequences of my First Training Study (Tart, 1976). I contacted Drs. Goldman, Stein, and Weiner, who are colleagues in the mathematics Department on the UC Davis campus, for advice on corrections for these small departures. Unfortunately I did not learn anything of consequence from this consultation that I had not already worked out. Indeed, I was disappointed in the absolutist way Drs. Stein and Weiner thought about randomness, and their lack of recognition of the fact that small departures from equiprobability and serial independence may often be trivial in terms of their usefulness for trying to predict the target sequence by some kind of mathematical inference strategy. In several very useful conversations with Dr. Diaconis about this problem, it has been my understanding that he has agreed with me that the departures from randomness in my First Training Study data are probably trivial in this respect. I trust he will correct me if we have not understood each other correctly. Questions about the importance of this bias have been discussed extensively in the literature (Gardner, 1977; Gatlin, 1978a; 1978b; 1979; Kennedy, in press; Tart, 1977a; 1978b; 1979a; 1979b; in press). I believe there is no convincing evidence to show that these biases were of real importance. The most extensive discussion is by myself and Eugene Dronek (Tart & Dronek, in press). There we also show that a powerful, computer-based mathematical estimation strategy cannot produce much hitting on my data in comparison with what the actual subjects did, nor does an inference strategy produce internal scoring patterns that at all resemble those of actual subjects, suggesting strongly that such an inference strategy was not used.

Second, Dr. Diaconis states that the above problem "... led Tart to redo the experiment with better random numbers, and the new results were non-significant." These two claims are incorrect. The Second Training Study (Tart, Palmer, & Redington, 1979) was undertaken as part of a planned research program on the effects of feedback on the potential for learning improved ESP performance, based on theoretical considerations (Tart, 1966; 1976; 1977b). It was underway independent of any questions about randomness in the target sequences of the First Training Study.

* I believe Dr. Diaconis meant the target sequences rather than the guessing/response sequences.

It is also incorrect to say that the results were insignificant. The Second Training Study, like the first, actually consisted of two experiments, one with a four-choice ESP feedback training machine, the other with a ten-choice ESP feedback training machine. Results on the four-choice machine were significant for the subjects as a group with $P = 4 \times 10^{-4}$, two-tailed, results quite similar to those obtained on this machine in the First Training Study. While the overall group results were not significant in the second study for the subjects who worked on the ten-choice trainer, two of the seven subjects showed individually significant results, which, because they deviated from chance in opposite directions, happened to cancel out an overall group effect. Additionally, the ten-choice subjects showed a significant negative correlation, as predicted in advance from the results of the First Training Study, between real time and +1 precognitive hitting, a result of considerable theoretical significance, discussed elsewhere (Tart, 1978a; 1979b). Further, the ten-choice results of the Second Training Study were predicted in advance to be of less significance than those of the First Training Study, because less-talented subjects were used, so this finding was consistent with the theory (Tart, 1966; 1976; 1977b) guiding the research.

Third, I am flattered when Dr. Diaconis lists me as one of the most skillful researchers in parapsychology. There is an implication, however, that I and (an unspecified number of) other researchers brought Dr. Diaconis into our laboratories because we perceived major problems in our research. Speaking for myself, this was not the case. I saw no major problems with my First Training Study at the time I initially contacted Dr. Diaconis. I approached him for his statistical expertise and its possible application in learning how to get more from the analyses than is ordinarily gotten, how to make them more sensitive. My discussions with him on this matter have been quite stimulating and valuable.

Fourth, Dr. Diaconis states that "My contact with these researchers has not brought to light any evidence of paranormal phenomena." I must admit surprise, and request clarification. During the course of our discussions I naturally asked him if he saw any basic statistical or procedural flaws in my First Training Study. I cannot recall him mentioning any such major flaws. Why then is he implying that my data is not evidential?

If Dr. Diaconis is simply reiterating a personal philosophical commitment that he will not accept results from any study that did not have magicians standing around, so be it: I don't argue with such philosophies, as I don't find them scientifically useful. Nor am I concerned about any standards of absolute perfection applied to a real experiment: useful hypotheses are determined by replications over studies, not single, absolutely foolproof experiments. But if Dr. Diaconis knows of some specific, major, demonstrable flaw in my First Training Study, I would like to know what it is. It is also a disservice to the scientific community to imply that such a specific flaw was detected if it was not.

I look forward to a clarification of Dr. Diaconis' views on the above specific issues. I must add that I have been quite disappointed with his response (Diaconis, 1978b) to my letter of criticism (Tart, 1978c) with respect to his Science article (Diaconis, 1978a), and with his response to Kelly's (1979) criticisms, as too many specific points were simply not dealt with.

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PERSI DIACONIS REPLIES TO EDWARD F. KELLY AND CHARLES T. TART:

RESPONSE TO KELLY:

Boy, some people really get angry when you criticize their research. Despite Kelly's anger, we are actually making progress.

On feedback experiments: I admit that card guessing experiments with deliberately given feedback are a small part of the reported ESP literature. Kelly admits that experienced, serious researchers have trouble analyzing their feedback designs. Since both of these points are deliberately made in my original article and in my previous response to Kelly, it is hard to see what more there is to discuss.

On B.D. At least one issue has come out into the clear: I don't think Kelly's experiments with B.D. are worth analyzing carefully unless he produces evidence that special precautions, far beyond ordinary protocol, were involved. Kelly thinks that the fact B.D. has been observed to use slight of hand in informal demonstrations it "is of very limited importance" to work with B.D.

One point concerning Kelly and B.D. is less clear. In my Science article, I reported observing B.D. perform slight of hand in a demonstration arranged by Kelly. In Kelly's own words "the central purpose of the 1972 Harvard demonstration was to attempt to secure funding for later formal experiments through the Hodgson fund,...." Kelly has had two opportunities to react as a responsible scientist in response to my exposure of B.D. He could acknowledge his own awareness of B.D.'s slight of hand abilities, or say he was not aware. He has chosen to do neither. Perhaps it is because I haven't asked directly. Let me do so now: In your next reply Dr. Kelly, please comment on your awareness of B.D.'s ability to manipulate cards.

Not a Survey. The only difference between this section of Kelly's reply and his former reply is his claim that I have not taken the trouble to read the ESP literature. If it helps, I have read the ESP literature for the past fifteen years. In preparation for my Science article, I read through the main ESP journals from start to present. The claims in my Science article are based on this, and the following experiences: In every case that I know of, when a skeptic has taken a closer look at a reported experiment, the "evidence" disappears. The difference between what was published and what the skeptic observed or found out was so striking

as to make further inquiry fruitless. These experiences could be due to skeptics imagining things, to a badly biased sample, or to a huge collection of sloppy experiments, badly reported.

I favor the third explanation. My opinion has been formed by reading, direct observation and the lack of a single repeatable **phenomenon** - in the sense that there is no experiment that a skeptic can be pointed to with assurance that the reported phenomena will appear when the skeptic tries the experiment.

I too hope the reader will look over my Science article and exchanges with Kelly. These document the many misrepresentations and mistakes that Kelly has made in dealing with me. I have no hope that his handling of other scientific matters is any more accurate or respectable.

RESPONSE TO TART:

On random numbers: Assessing the impact of Tart's faulty random number generator leads rapidly to a fundamental issue in assessing the evidence for any debatable phenomena; how much does a fly in the ointment count? For me, this varies with the circumstances. The same problem, a slightly faulty random number generator, appearing in a routine psychology randomization would bother me less than its appearance in Tart's experiments. My feeling in this case is that there was ample cause to redo the experiment with better random numbers. I am surprised at Tart for making the mistake of confusing the result of a data analysis with a test of statistical significance. His attempted replication failed completely to get statistically significant results on criteria specified before he looked at the data. Tart clearly admits this in the published version.

I hope that Professors Stein, Goldman, and Weiner will favor us with their version of their interaction with Tart. I hope too that Martin Gardner's detailed critique of Tart's replication sees the light of day.

A final, historical, response. I tried twice to suggest serious flaws or improvements to Tart. One suggestion involved the naive way that Tart was going about finding flaws in his random number generator (what he refers to as "the most extensive discussion..." above). A second area involved the techniques Tart has been using to analyze PSI data by looking at all possible ways a sequence could match (forward and backward in time). My impression was that Tart wanted to keep "fooling around" and was not interested in critical comments.

The above remarks are not intended as a criticism of Tart, so much as they are intended to point out how hard it is for people to have useful scientific interaction.

JON BECKJORD COMMENTS ON ROBIN RIDINGTON ON "THE SASQUATCH IMAGE" (ZS, #5):

Robin Ridington is a poet. He is a lyric anthropologist. He is a troubled man. He has good reason to be, for he is suffering from an advanced case of what I call "Napier's angst." He is in good company, for other scientists, such as biomechanical analyst Dr. D. W. Grieve, have had similar attacks. What causes this angst, this twisting on a stake, this writhing in intellectual agony, is well expressed by Grieve in Napier's Bigfoot: the yeti and the sasquatch in myth and reality (1972):

My subjective impressions have oscillated between total acceptance of the Sasquatch on the grounds that the film would be difficult to fake, to one of irrational rejection based on an emotional response to the possibility that the Sasquatch actually exists.

It is conflict between what the anthropologist (or M.D.) knows to exist in terms of catalogued type-specimens and what he sees in front of him on a screen as he watches the Roger Patterson Bigfoot Film (©1977 Mrs. R. C. Patterson).

I had the pleasure of watching this type of angst surface in Ridington and ten other scientists as I projected my enlarged copy of this film in the private rooms of Dr. Carleton S. Coon at the site of the UBC conference in 1978, Sasquatch and Other Phenomena. Due to politics, my much-enlarged copy of the film was not allowed to be seen in the auditorium, where instead another investigator projected his partially-enlarged copy on a screen dimly lit by an under-wattted projector. The packed room of scientists at Dr. Coon's however, had an excellent view, close to the fully-filled screen. They watched the sasquatch march back and forth, in reversed image and normal image, and in varying shades of printing density, which show details normally hidden in correctly-printed versions. I put on slides that effectively provided a freeze-frame technique. The film was also played at all speeds, including slow motion and the controversial 24 fps and 16 fps projection rates. Jaws dropped. Chatter ceased. Rampant skepticism in some seemed to almost visibly melt. Several viewers openly stated that their former doubts were greatly shaken. No one, to my knowledge, left with his skepticism intact. What impressed the majority was the naturalness of the motion, the sense of extreme bulk and weight, and the total lack of similarity to any contemporary monkey-suit performance they had ever seen.

Yet, the angst persists. It persists because of the lack of a body, despite hundreds of rounds of ammunition that have been allegedly pumped into innocent sasquatch bodies in dozens of states. To relieve this angst, writers like Ridington write about the sasquatch image growing "by leaps and bounds at the edges of our own culture's consciousness" and use other fine poetic phrases, in order to try to provide a proper place for the sasquatch, and to try to nullify or explain the power and effect of this famous film. I have respect for this problem, and such attempts to reconcile the film with the lack of a body by diving deep into cultural anthropology and psychology. Yet, I have an

answer for Ridington and all others saddled with "Napier's angst," and the answer is hinted at in Ridington's own article. He refers several times to "the natural fauna" and to "a natural animal." Herein lies the answer: the sasquatch exists, and even as a species, but not as a natural species and not as a "natural animal." It is not part of the "natural fauna" of this world. My thesis here, based on field experiences as a so-called "Bigfoot hunter," on my own extensive and exhaustive analysis of the enlargements of the Patterson Film, and on comparisons with other newer photos of other alleged sasquatches, is that the sasquatch fits neither the mold of the "real animal," nor that of the figment-of-our-imagination school of thought. The sasquatch is a visitor from Napier's "Goblin Universe." It is a visitor, perhaps a resident - perhaps a commuter, and it gives us finally the answer to the problem of giants, trolls, large gnomes, Bigfoot, the Yeti, and the strange assortment of creatures listed in Loren Coleman and Jerome Clark's Creatures of the Outer Edge.

I can sense perhaps a potential shutting of minds of readers at this point. It is time to get down to details. I put it to Ridington that his companion, James Butler, a biologist who assisted him in debating Krantz and Green on the topic "To Kill or Not to Kill," the debate that I suggested early to M. Halpin when she was setting up the U.B.C. Conference, was on the right track when he presented a paper on "higher sensory perception" in sasquatches at the Conference. Butler suggested that sasquatches have an extra set of senses that enable them to survive man's attempts to snare them. Butler is not only right, but he is just uncovering the "tip of the iceberg," in this case. The sasquatches have many other abilities and characteristics that are not normally found in natural fauna. The sasquatches are in fact paranormal in that their abilities are not yet explainable by science. I suggest this because:

1) Sasquatches possess weights that are incredible: in my recent paper, "A New Method For Calculating Sasquatch Weights," Pursuit (the journal of the Society for the Investigation of the Unexplained), forthcoming, I show how by using simple soil compression formulas that the indicated weight for the Patterson sasquatch, based on its footprint depth (eight times deeper than that of a man) is an incredible 2,448 pounds for an animal only 6 ft. 7 inches in height. The point I make in the paper is that we cannot assume to estimate sasquatch weights by what we think a large man, or a bull, or a gorilla would weigh, because while we have been able to determine the flesh density of these animals, we have never done so for the sasquatch.¹ We cannot even prove that a biologically normal and real sasquatch exists, thus we cannot simply make eyeball estimates of what we think it should weigh. We must instead take the only real and measureable piece of data available, footprint depth, **and proceed** as if we were making a soil test. No other method,

¹If anthropologist Grover Krantz is correct about his estimate of 8.44 cubic feet for the body volume of the Patterson sasquatch, then an incredible density of 296 pounds per cubic foot is indicated. Compare this figure to the density of human beings, 60 pounds/cu.ft.

save asking a sasquatch to walk with us to a freeway truck-weighing station, will do. Many other investigators have stated in the literature that they have encountered tracks so deep as to almost frighten them, while their own tracks barely penetrate the soil. I have encountered this phenomenon many times on the Lummi Indian Reservation and in the Cascades of Washington.

2) Patterson seems to have been one of the few to be able to take photos of a visible sasquatch. Why? Because for the last year, I have been enlarging some very controversial stills that show sasquatch-like animals, strange dogs, cat-like faces, and even gargoyle-mask-heads, mixed in together, that were not seen by the photographers. This is a very challenging statement, and I will make it even more controversial by stating that on a recent expedition to the Sierras to the best site for this type of unusual, perhaps paranormal, photography, I and another investigator spent five days looking as we took photos of the same sites and spots where other photos had shown such faces and bodies, and while our photos showed more such faces when processed, our eyes still saw not a trace of any animals, even through binoculars and telephoto lenses. Space does not permit a full discussion and report of the findings that resulted from these photos. (See Figure 1.) However, the stills taken at this pond site in the Sierras were analyzed by photo interpreters at the U.S. Army Intelligence Unit at Fort Lewis, Tacoma, Washington, and they were able to tell us that in their opinion as individuals, not speaking for the Army, we did have at least one "cat-like" face, and perhaps more (pending submission of our newer stereo photographs), peeking around a tree, that they fully agreed with us existed. This cat-like face is in my estimation more like a flat-headed baboon crossed with a wolf, and our measurements show it to be at least 14 inches wide, when compared with the tree it peeks around. Furthermore, this face, taken in a color slide and enlarged 40 times for analysis, seems to be of the same species as another animal face taken in five frames and three different positions (see drawing) by us on the August 1979 trip. This type of face with a very narrow muzzle, an underslung mouth, and a long nose ridge, with a "flattop" type of head-hair arrangement that leads to a slight peak of hair at the end/top of the long head, matches almost identically the head of the Patterson sasquatch. In 1978, photomicrography enabled us to discover that frame number 370 of the Patterson Film was perhaps the sharpest frame of the entire 951 frame film, and that by chance the sasquatch head is shown in a revealing quarter-profile that shows the chin up just enough to show far more detail of the muzzle and eyes than has ever been found or studied before. (See Figure 2.) Dr. Daris Swindler of the University of Washington anthropology department, although not a sasquatch believer, did point out to us that the curve of the muzzle and the strong brow-ridges, resembled greatly that of a chimpanzee, an established pongid. The side view of the Sierra sasquatch shows this same set of characteristics.

In another photo, this time taken by a female tourist-camper from the SF Bay Area, a cluster of two adults holding two children in their arms may be seen at a distance of 126 feet, across a pond from the unsuspecting photographer. Height measurements at this spot showed them to have been ten feet tall. Computer enhancement done by Geo-Images

Figure 1. Three views of an alleged sasquatch head based on black and white photos taken 8/79 by Jon Beckjord of Project Bigfoot, Seattle, in the Sierras of California with a 35mm SLR type camera with a 50mm lens. Distance: 235 ft. Estimated width of head, based on tree measurements: 18-20 in.



Photo #3.



Photo #28.



Photo #27.

Figure 2. Roger Patterson's Sasquatch Female.

Pencil drawing based on frame #370 of the Roger Patterson Bigfoot Film, showing an ape-like face with a muzzle area that is prognathous. Teeth are visible, and the nose appears to be of a catarrhine nature, similar to Old World monkeys, chimps and gorillas, etc. (Release for non-profit scientific and research use only.)



A similar facial structure is also visible in other frames from this 1967 film clip, including the famous "Best Frame" which shows an almost front-on view, making detection of the muzzle difficult. Profile shots are more useful. The head itself is only 2/10 of one millimeter high in the 16mm movie frame, but photomicrography has enabled Project Bigfoot to enlarge the frames up to as much as 1000 power, thus bringing out these new details. With a hominid type of body structure and a pongid type of head, this primate is likely to be eventually classified as either a primitive but living hominid or a new genus and species in a new Family or primates intermediate between Hominids and Pongids.

technician Allen Gillespie,² Altadena, Ca., revealed that the largest of the two children being carried possessed a pattern of dark hair down the backbone, dark hair in the small of the back and a triangular patch of dark hair in back of the neck and across the shoulders, identical to the same hair pattern that can be seen on the Patterson sasquatch. Yet, Ms. Marion Schubert, taking photos through her Kodak Instamatic, saw no more than I and Ms. Judy Grant did, while watching for movement for five days at this pond over a year later. Perhaps somehow the sasquatches are able to manipulate the light spectrum so as to be visible only to camera film, but not to the human eye. All photos taken at this site that show any faces, show groups, even family clusters, but none of us have been able to see any trace of what later shows up in the prints. We have found deep 20 inch and 13 inch footprints there, but not nearly enough to explain the dozens of faces, and faces with bodies, that keep showing up in the photos.

3) Tracks that start - and stop - with no good reason: Ridington says that "No one has ever been able to follow the trail of a sasquatch as they would any other animal." He is part right and part wrong. Trackers have been able to follow sasquatches, and Glen Thomas, an Oregon logger, in particular has claimed to have followed sasquatch tracks for miles, and to have even caught up with them. However, most tracking stories end with the note that the tracker either got exhausted from climbing over obstacles that the sasquatch was able to clear with ease, or else that on seeing a glimpse of what was being tracked, the tracker lost his enthusiasm. Yet, many other stories abound of tracks that suddenly "end" or just as strangely, suddenly "start" with no place for the animal to have sprung to, or have jumped from. I have encountered such tracks in Puyallup,³ where, after meandering in half-loops and zig-zags, the tracks have suddenly quit, period, and in snow, with no trees or overhangs to provide a place to leap to. Our Bigfoot Hotline number in Seattle has received many reports from Washington and Oregon residents about hearing loud pounding footsteps happening right in front of them, yet seeing nothing. I have had such an experience while standing watch in the yard of one such person. I approached slowly a tree where the householder said he could see a sasquatch sitting. I was accompanied by a local state patrolman who has had other strange experiences in that area, Puyallup/Spanaway, and when we got close to the tree, in the dark, he said he saw something jump down over me, and land on the other side of a bush and run down a bank in big strides. I heard the thump and the running sound, but saw nothing but a ½ second subliminal flash of the outline of a large man-like thing, a sort of mental freeze-frame. Yet, it had jumped over me. We found by flash-light five 16-inch bare-foot tracks that were over six feet apart, which had literally sliced through

²Gillespie does technical work only, and makes no judgments as to the content of this work. He stands neutral.

³Located between Tacoma and Mt. Rainier. Unidentified screams and howls that we believe are not coyote, are heard in that area each year.

the grass cover into the dirt underneath. Yet, in daytime, these tracks were found to head right for a wire fence, on the other side of which was fifty feet of mud. Not a single track could be seen in the mud. The tracks seemed to end at the fence, as if the animal had "taken off" in a fifty-foot plus broad jump. However, no tracks were found on the other side of the mud-field either.

I do not want to get into a series of Bigfoot Reports, but I have offered these items as indicators that the answer to "Napier's angst" and perhaps Ridington's dilemma is that they are correct in that the Sasquatch is not "real" as we know "real" to be - yet, the sasquatches are able to behave and perform in a real manner, seemingly when they so choose, and then at other times, to "turn on" their abilities and outperform their pursuers and/or observers. Perhaps they have come from another space-time. Perhaps they are a small protruding corner of Napier's "Goblin Universe," acting as if another dimension existed from which they can enter and exit at will. Perhaps more prosaically, they are simply animals that have abilities that science has not yet found the right tools to measure. But whatever the right way of expressing the paranormality of the sasquatches may be, by studying and considering this new aspect of their behavior, perhaps anthropologists may find that trying to consign them to the back country of our minds no longer works, and a new time of stretching the bounds of traditional science is upon us. The sasquatches are performing on land in very much the strange way that UFO's are performing in the air. However, since they are land-bound, and do leave some physical traces, perhaps we ought to concentrate on the solution to the ways in which sasquatches bend the laws of physics as a first order of business on the road to investigating the fabric of space-time.

I will end with a quote from John Napier:

I am convinced that the Sasquatch exists, but whether it is all that it is cracked up to be is another matter altogether.

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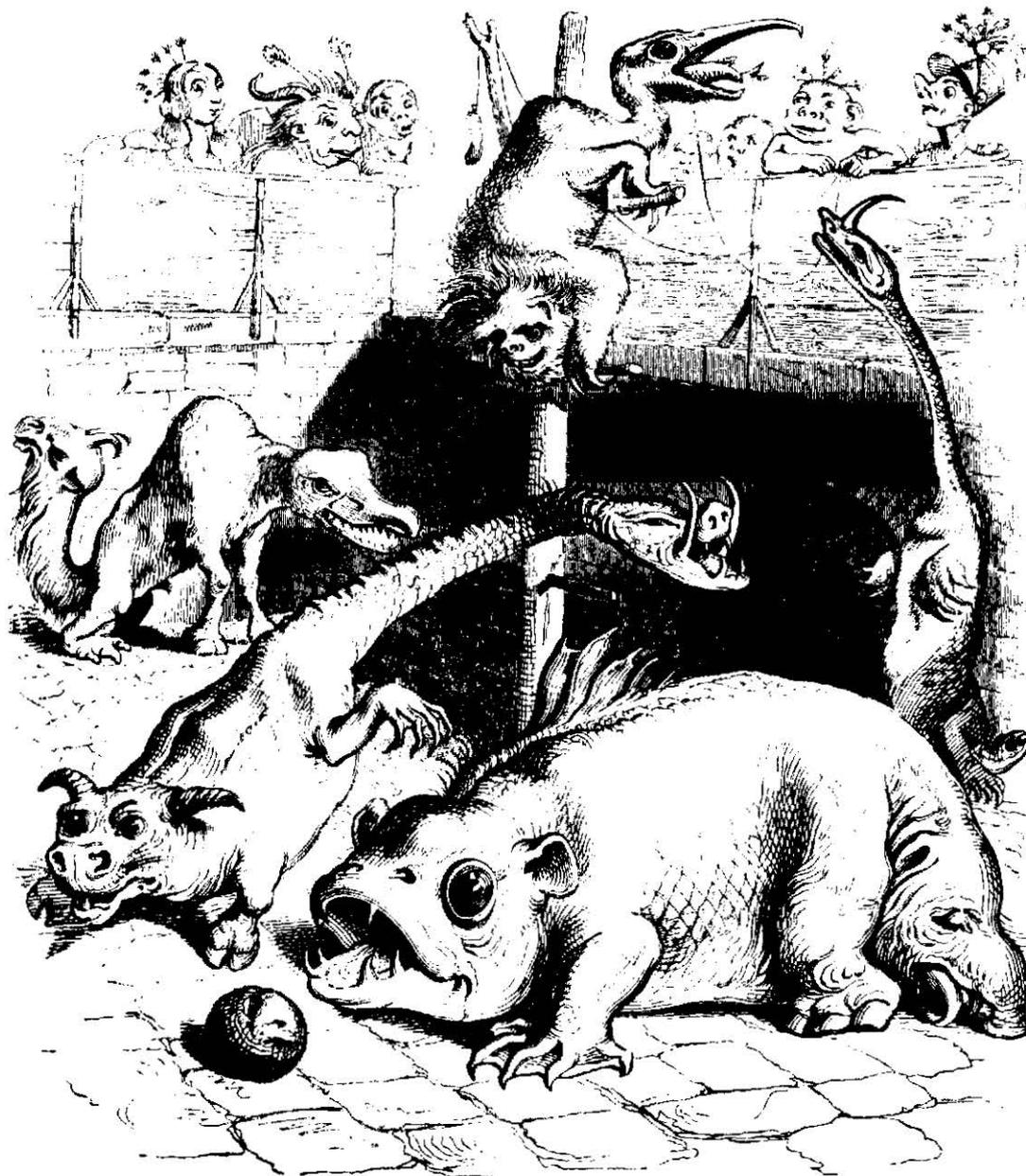
ROBIN RIDINGTON REPLIES TO BECKJORD:

Jon Beckjord is not a poet. He is an imaginative, energetic and creatively intelligent zealot. In addition to an uncanny ability to photograph sasquatch families that he was unable to see at the time his photographs were taken, Beckjord apparently has the ability to savour angst in an anthropologist (namely myself) who was also not present at the time of the reputed encounter. He reports sighting (could he mean citing?) me at a showing of an enlarged copy of a frame from Roger Patterson's sasquatch film, "in the private rooms of Dr. Carleton S. Coon at the site of the UBC conference in 1978." Although I did participate in the sasquatch conference and saw Patterson's film for the second time there, I was not present at the private showing Beckjord mentions. The angst he observed must have graced the countenance of some other anthropologist.

Beckjord's misidentification of me may be a simple error, but his use of innuendo based on similar situations reflects a zealot's pre-deliction for interpreting information according to preconception rather than allowing the information itself determine concept formation. Of course, a negative reading of the sasquatch evidence is conceptually less satisfying than a positive one, but Beckjord's interpretation of the sasquatch as some form of interdimensional time traveller, (Beckjord, personal communication), requires a set of entirely unsubstantiated assumptions about the world. His interpretation assumes that the sasquatch is physical, but because of the ephemeral nature of the physical evidence, he plunges to the conclusion that everything we know from other sources about the nature of physical reality must be questioned. The more economical if less exciting interpretation is that the sasquatch is meaningful as an image we hold of the limits of our knowledge. If physical science is to be fundamentally revised, I do not believe sasquatch sightings and circumstantial evidence are sufficiently persuasive and comprehensive to initiate such a revision. Beckjord's challenge to science is that of a Zealot. His photographs of alleged sasquatches that were not visible when the photos were taken convince me only of the mind's power to impress the form of its imagination upon the world. I do agree with Beckjord in his suggestion that sightings of sasquatches and UFOs may have a common source. Both are creatures at the margins of our discriminatory powers. In this they are alike. Both exist without the benefit of any solid scientific substantiation. They differ in that one exists with no culture whatsoever while the other exists through a super-culture. We humans are exactly in-between these two extremes. We have enough culture to blow ourselves up but not enough to assure our survival. I interpret the florescence of sasquatch and UFO interest as a reflection of our very real human predicament.

Jon Beckjord is to be congratulated for his diligence and ingenuity in pursuit of the sasquatch, but I find the testimony he cites unconvincing. His style of argument and case building may seem innocent enough because it is focused on something marginal to the mainstream of our society's concerns, but it is worth noting that a similar style of argumentation has been used by immensely powerful religious and political interests in the subjugation of opinion and evidence contrary to their

own orthodoxy. Arguments like those of Beckjord are being used to suppress the teaching of evolutionary biology or social theory that challenges a ruling system of thought. Images of the margins of knowledge are powerful and beneficial only to the extent that they define for us more clearly the epistemology of our central knowledge of the world. Only a zealot would bring the sasquatch image from its place in the shadows to a position of central concern.



C. J. RANSOM COMMENTS ON DAVID MORRISON'S COMMENTARY ON JOSEPH MAY'S "THE HERESY OF A NEW SYNTHESIS" (ZS #3/4):

Since Morrison's article in Scientists Confront Velikovsky was the only one that appeared to have been written with an attempt at ethical scientific investigation, it was surprising to see the type of comments he made in Zetetic Scholar. His first and last paragraphs were amazing from the standpoint that they would be more accurate if Velikovsky and Establishment Science were substituted for Scientists Confront Velikovsky and other nouns appropriately reversed. Actually, it is the Velikovsky supporters who read every detail written by his opponents and respond to the occasional logical arguments as well as the others.

An example is the comment about the comet-like Venus. This has been answered many times. The word comet is derived from a Greek word for "hair" and was applied to celestial objects that had tails giving the appearance of beards. The ancients described a time when Venus appeared to have hair and, hence, appeared comet-like (see details about properties in, for example, The Age of Velikovsky). It was magnanimous of Morrison to finally give permission for people to use the term comet in association with ancient Venus. Presumably, he will also allow Bridge, et. al. (Science 183, 1293, 1974), to refer to the "comet-like" tail of Venus.

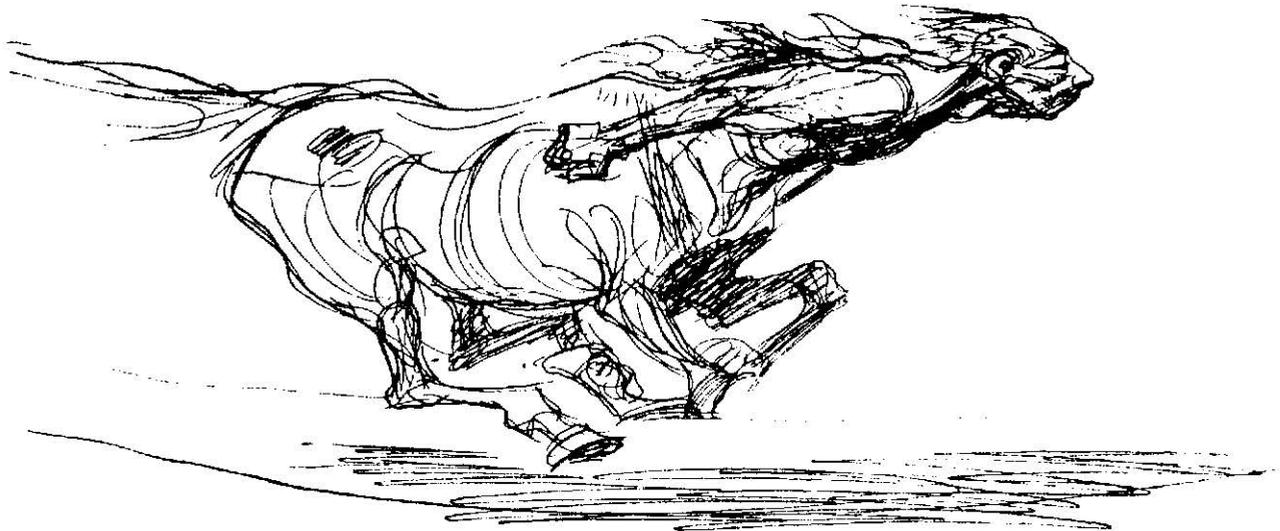
Morrison's statement that he "cannot imagine what evidence supports" the suggestion that some comets may originate from planets typifies the unscientific attitude depicted in many authoritative statements. The scientific approach is not to try to imagine what is in the literature, but to look at the literature. Perhaps he would find some of the articles written by S. K. Vsekhsvyatskii while he was head of the Kiev Observatory in Russia. He has for many years discussed the possibility that comets originate (ongoing process) from the giant planets or their satellites. The evidence considered by Vsekhsvyatskii led him to conclude that there would be a ring of debris around Jupiter and volcanoes would be found on some of the moons by Jupiter. When these items were discovered, they were called "surprises" and "completely unexpected." Morrison may prefer an alternate interpretation of the evidence, but he cannot "imagine" the evidence out of existence.

Vsekhsvyatskii has all the proper credentials and has the support of a number of other investigators in Russia, but some others still cannot imagine any evidence for his ideas. This makes it appear that Velikovsky's opponents' preoccupation with credentials is just a smokescreen.

It should be remembered that the comparison is not of a perfect theory, Uniformity, versus an imperfect one. Two imperfect theories are being compared. Both have unanswered questions. When one is found, this does not mean that the

entire theory must be immediately forgotten. If being wrong on only one point meant an entire theory was wrong, Uniformity would have been counted out years ago. It is often forgotten that most of the uniformitarian "facts" originally used as "proof" that Velikovsky was wrong, were not actually "facts." The observed data turned out to favor Velikovsky, and it was Uniformity that had to be readjusted to fit these facts.

The comments about Mays' article were generally of the nature of pronouncements that science has spoken once and need not be bothered again. While claiming that Velikovsky supporters do not read these pronouncements, the writers appeared to be unaware that rebuttals to Scientists Confront Velikovsky and other articles exist. This may be because ads for these rebuttals have been banned from Science News and are not accepted by some other periodicals. However, someone in the scientific community must know of the existence of the rebuttals to want to have them suppressed. As did Morrison, Huber and others, I would like to refer the reader to existing publications, but these were written after the ones they referenced. Although they are not easily advertised, the interested investigator can locate copies of the journal Kronos. These, of course, are not the last word either, but hopefully, the discussions can continue until facts instead of opinions become dominant.



ANDREAS N. MARIS VAN BLAADEREN COMMENTS ON JOSEPH AGASSI'S
"TOWARDS A RATIONAL THEORY OF SUPERSTITION" (ZS #3/4):

In five sections crammed with allusions Agassi has presented thoughts and comments purported to be basic in work "Toward a Rational Theory of Superstition": 1. The Current Theory of Superstition, 2. Impressions about the Volume at Hand, 3. The Theory of Theory-Assesment, 4. Irrationalism, and 5. Superstition, the Illusive. The origin of this review-article was an invitation to assess Recent Advances in Natal Astrology: A Critical Review 1900 - 1976, by G. Dean and A. Mather for the Zetetic Scholar (Nos. 3 & 4, pp 107-120).

Apart from ad hominem statements aimed at Feyerabend specifically and others in passing, in the core of his review-article Agassi poses a central problem of science, viz. the appraisal and reasonableness of probabilities as certainties. Put differently what are proper preconditions to be used in the acceptance and rejection (or temporary suspension of judgment) of findings not in the coda of current advanced thought?

Obviously Agassi uses reinterpretations of decisions on similar issues which he had taken in his earlier works. But this review-article is presented as standing on its own and has to be judged on its internal merits. Regretfully we can not follow Agassi's impish humor to "cast a horoscope for astrology" (119) for the role he thinks his review-article is to play in the future. Furthermore the applicability of his formulations of "progressive, regressive and degenerative" research programs might be more in debt to ideological conjunctions with a planet in the ascendant than Agassi would be willing to admit. However, lacking this ability to cast divinatory charts I would like to insert a slightly different and more mundane set of statements on astrology and Agassi's review-article from an admittedly different perspective.

Sociology and Astrology

The belief in astrology as a predictive tool for individual lifecourses distresses Agassi as much as me. By his oft expressed concern about this issue he no doubt shares with me also a concern with the certainty value ascribed to astrological predictions. If astrology guides social actions this means for those who use astrology as a guide that astrology has a certainty value in the same sense that for others certainty values exist regarding God or the ultimate coherence of the universe by which a few claim they can foresee the syntheses of history.

Furthermore some sociological studies over the last 100 years, with an intellectually respectable and scientifically recognized literature (too long to even start to enumerate), have concerned themselves with areas related to astrological approaches: the influence of birth (demography) and career opportunities as choices (the sociology of work). These studies were conducted by setting

up two sets of wards (1) to prevent the judgment of the observer to unduly affect the human subject, and (2) to be sure that no preconceived causalities (if such were indeed in existence) of the social world unduly influenced the conclusions reached. This array of intellectual tools (erroneously sometimes called methods) is employed to evaluate what has been used as acceptable knowledge.

When, as a sociologist, I turn for a moment to astrology and skip over details, I can summarize some sociological regularities which seem to obfuscate the claims of astrology. 1. Parturition and Conception. Variations in the instance in time of fertilization of the human egg are very seasonal (they have a differentially relative frequency over a normal social calendar year). Because the resultant births over several hundreds of years in different parts of the Western World shift in accordance with socially perceived seasons and not with the astrological "houses," even if we assume astrology to have an influence it is at best a predisposition rather than a determining general factor. 2. Birth Order and Work. Simple birth order combined with family of origin seems to be the most important factor in determining an individual's chance to enter social life through work to follow a career. Because of differential fertility, job opportunities can and do vary for jobs available. Again it seems that these are determined by other factors than those which seem to accrue to astrological interpretations. Finally 3. Sex and Mate Selection. At puberty the number in each sex in the age-cohorts is for all practical purposes equal. Under these conditions proximity of future partners and type of mate selection (open or arranged) determine the probable overall array of alternatives for possible partners regardless of astrological morphology. These general conditions imposed by numbers (of course to be observed again and again) can be raised as issues in astrology because they seem to impose socially determined pre-conditions which drastically limit astrological determination.

But we can take this one step further. Does astrology indeed operate to determine individual life course probabilities as certainties? I am here not talking about large numbers, but about an examination by astrologists of case-histories of which a goodly number are now available. We can then sketch the following possible scenario. A couple select each other on the basis of possible desirable astrological factors. They also select the moment of conception in accordance with the most likely astrological parturition potential for the to-be-born child. Apart from the inherent self fulfilling prophecy: does the result, the child, in his life course indeed confirm the astrological predictions? I wish to note that modern computerization can crank out such research and detailed customary implications by the gross lot and that features can be built in a program for immediate analysis. For the moment I will suspend judgment, although I am doubtful about the efficacy of astrology to provide some of the proofs implied.

In these examples I have so far skirted the issue of appeals to protosocial conditions. Astrology, as well as other closely related searchers for truth, wishes to relate its synthetic

conjunctions to psychic dispositions for individual life courses. While of course such approaches negate our whole conceptualization of human freedom, they also limit possible social relations later in life. Given the large sets of research data, from NORC to case-histories, the question is if astrological and other predictions are more effective (Occam's Razor) than those of the social sciences including sociology? For the remainder of this comment I assume that Agassi agrees that such minimal conditions have to be met in some form or another. But I am not sure that he and I agree on the particulars on which each of us would suspend the reasonable doubt we share on the efficacy of astrology and its apparatus of measurements and assumptions.

Ignorance and Social Life

If we eliminate from Agassi's review-article the direct and indirect references to astrology (taking care not to destroy the inbuilt stream of consciousness) we are left with contentions based on his interpretations of the goals and purposes of Western Thoughts. Appealing selectively to his current interpretations of thinkers from Ptolemy to Einstein and from Bacon to Evans-Pritchard (with a good number of Agassi's *bêtes noires* like Koestler and Ginsberg thrown in for good measure) Agassi presents his incisive judgments on the level and purpose of constructive thought. "The confusion of the irrationalists is illustrated by Evans-Pritchard's identification of the Zande magic system, the Communist dogma, Nuer religion, and even his own Catholicism, all as self-imposed intellectual systems," (*Italics added*, 119) is a good example of Agassi's style of presentation and intentions.

Be it far from us to accuse Agassi of rationalizations. Essentially Agassi stresses instead the fact that individual yearning for understanding and meaning is in direct opposition to "This inverted superstition.....logical positivism." (116-117). As a dialectician of great intellectual agility he reaches this statement by stacking synthesis on synthesis. The purpose of this approach is to refute the formation of any statements which might derive their certainty value from uniform or dualistic (or pluralistic) theoretical "pre"-assumptions. The emphasis is on the process thinking rather than necessarily the specific content or certainty of logical thought. The theory of theory-assessment particularly is not aimed at disparaging findings but rather at a way of thinking.

Agassi is thus not talking about the dialectic of the internal combustion engine, but about the use of an intellectual approach as process to distinguish certainties from other statements. He has cast his arguments in this manner because his concern is with ignorance by showing us another way of thinking, let the chips fall where they may.

Taking up the cudgels for knowledge to be attained through a dialectical process he points out that ignorance in general and "scientific" ignorance specifically is the result of an optical illusion. Ignorance is not lack of knowledge (about what we could know) but based on our inability to distinguish certainties from

other statements. Common sense knowledge as reality results in the "other" statements but certainties can only be reached through a process of (Socratic) dialogue. His article is one long castigation of those who do not follow his particular format of the Socratic dialogue on the road to understanding and meaning.

Mankind is not a bunch of stones according to Agassi. To come to an understanding of ourselves and others we cannot proceed as if we were, and the use of the methods so successful in physics leads to drastic separation between the searcher and his subject matter. More specifically the impossibility to interact socially or psychically with a stone becomes a pre-condition by which thinkers erroneously might start to assume that this impossibility is pari passu also a condition for our understanding of our fellow man. But two questions remain. First in how far his method is more successful to increase understanding and meaning, and I will pass on answering it because of the limitations of this comment. More to the point for the moment is the second question: Do we, in the case of Agassi's review-article, obtain good solid advice to avoid the ignorance which he implies as operating in assessment (in this case of astrology)?

It is thus not the purpose here to question his pronouncements but the degree to which in his approach, as a constructed certainty, ignorance can be avoided. It goes without saying that of course we are confronted with differentially ascribed meanings of ignorance. Not so obvious are the different philosophical approaches and related discipline-specific theories which can avoid ignorance. Even if we agree for this comment that "thinking" is a process and thus understanding and meaning are to be arrived at by a socratic dialogue we are dealing with those who believe (or not) in an ultimate purpose for it all, as opposed to those for whom knowledge is the "arrived-at-purpose." Of course we all think in analogies to systems of thought, but such "systems" (be they logically positivistic or not) all have criteria of relevance which are not prominently presented in Agassi's review-article.

His relevance can be best understood by applying Schutz's: "The man unable to take an interpretative decision based on the interpretatively relevant material at hand, takes his stick and hits the object."* This might mean that Agassi's appeals to an impressive pantheon of philosophical and scientific prominents is a smokescreen for his inability to review the book in clearly stated terms. If indeed Agassi continues to undertake no additional work to become closer acquainted with the issues raised in the Zetetic Scholar he will be very ineffective to get his point across. Starting from an interpretation he poses an approach which too easily might be mistaken for a series of superstitions, without clear counsel how we can avoid his definition of our ignorance.

A. Schutz, Reflections on the Problem of Relevance. New Haven, Conn.: Yale University Press, 1970, p. 45.

Professor Agassi will reply in the next issue of ZS. --MT



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WHAT'S NEW ON THE NEW RELIGIONS? A REVIEW OF RECENT BOOKS

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Were it not for Jonestown one would have said that popular interest in the new religions had passed its peak as the movements themselves for the most part went into decline. The Guyana events however, must inevitably sharpen the attention directed towards the latest dozen or so serious studies of one or another of the new religions. In terms of insight into the dynamics of movements which could eventuate in such extraordinary commitment, as indeed in terms of most other criteria of evaluation, these studies differ widely.

To provide some purchase upon them we can divide the approaches used by the authors concerned into two categories: externalist and internalist. The externalist account is essentially based on observation of the movement from outside, approaching the activity involved primarily in terms of a positivistic method which applies some standardised measuring device to each instance. The externalist thinks in terms of causal variables mechanically producing an effect upon susceptible minds. Persons are caused to perform in particular ways. Their thought and behaviour are seen as produced principally by processes other than rational evaluation. The internalist account on the other hand seeks an interpretive understanding of a movement through its own beliefs and a variably close acquaintance with the everyday life of the believers. It interprets behaviour as strategic responses to prevailing circumstances informed by particular - and perhaps even sometimes peculiar - beliefs, no matter how faulty the logic or unsound the premises upon which the actions are predicated.

These two approaches will be familiar enough to any sociologist. It would certainly be my view that those of the recent dozen or so books which can be classified as predominantly externalist in approach are inferior in character and achievement to those that adopt an internalist approach. This is not to say, however, that a close acquaintance with belief and practice, and an effort to understand a movement employing a little interpretive charity, are the royal road to success. Far from it; several of the internalist accounts are weak as intellectual products. But in my view the broad division remains and those accounts based on a desire not to become too contaminated by the object of study successfully, and to their considerable detriment, achieve that end.

Not all commentators would disapprove of this result. Many, particularly in the "anti-cult" movement, would applaud it. For the consequence of observing a new religious sect at a distance or largely through the reports of hostile defectors whose justifications for their former behaviour have become thoroughly systematised in a rhetoric of "mental kidnapping", "hypnosis", or "brainwashing", is to render the activity of the movement mechanical and sinister; to dehumanise the behaviour of its adherents; and to de-rationalise the beliefs - torn from context - to which they display such commitment. Such a result serves perfectly the interests of those hostile to the new religions, although not all externalists should be seen as opponents. The two dimensions are relatively independent:

Orientation to the movement

	Hostile		Non-hostile
Externalist	1		2
<u>Method</u>			
Internalist	3		4

This scheme gives us a way of dividing up the literature under examination. While several of the books treated here show signs of more than one type, they will be discussed in the context of their dominant orientation.

1. Hostile Externalist Accounts

Three of the more recent books seem to be informed by an antipathy to one or more new religious movements to such an extent that even direct contact with the movement and its adherents has been too disagreeable a research technique to contemplate. (Enroth, 1977; Horowitz, 1978; Conway and Siegelman, 1978). The volumes by Enroth and by Horowitz are clearly motivated by an ideological stance. Enroth is an evangelical christian, Horowitz an anti-authoritarian socialist. Conway and Siegelman seem to be informed by no more ideological animus than a conviction that moderation is desirable in all things, and a sympathy for the beliefs and actions of those engaged in the anti-cult crusade. Enroth not only did not engage in anything approaching participant observation in the groups on which he writes, but did not even meet some of the "cult" defectors he interviewed by long distance tape recording. At such a distance from the subject matter subtle and even some gross distinctions are blurred. It is thus no surprise at all for him to write: "In a real sense the familiar expression, Once you've seen one, you've seen them all, is applicable to current cult groups . . . The commonality of certain means to certain ends is so striking . . . that one is tempted to conclude that conspiratorial forces are at work." (Enroth, 1977:12) Enroth's accounts of seven new religious movements are based primarily upon a single informant in each case. In his commentary following the presentation of the seven cases he employs Lifton's "Thought Reform" model to explain the conversions - despite the entirely voluntary attachment of members and the socially deviant status of the ideology being transmitted, surely not insignificant differences from the situation prevailing in Chinese civilian prisons. Moreover, it fits very well with this approach that Enroth should employ Kanter's ideas about commitment in utopian communities. Kanter (1972) shows a correlation between various "mechanisms" or features of utopian life, and levels of commitment, with the implication that the more such mechanisms are operating the greater

the level of commitment. Such an approach encourages a thoroughly mechanistic appreciation of how commitment arises, and entirely disregards the possibility of an alternative direction of causality i.e. that greater prior commitment produced more of the alleged mechanisms. Hence Kanter and Enroth can on a superficial reading be seen to have shown that regardless of individual will and idiosyncrasy, commitment to a bizarre way of life can be generated almost mechanically by the right set of techniques. Such a conclusion is a most attractive one to those hostile to the new religions. It was formerly attractive in the guise of William Sargant's (1957) physiological psychology variant.

Horowitz's book is if anything a poorer example of the same genre. In this case, the investigator has scarcely been able to bring himself closer to the Unification Church - upon which he does not look at all favourably - than the extant newspaper and magazine reportage. The book is a composite of articles from popular magazines and sociological journals, extracts from the writings of Sun Myung Moon, trial documents and other unpublished sources, with a prologue and epilogue by Horowitz. The book makes a genuflection toward the notion of fair debate by including items of Moon's writings - although chosen by Horowitz for his own purposes and without the movement's approval or permission, and a sympathetic contribution from Frederick Sontag's book on the church (on which, see below).

Horowitz's book is mainly occasioned by Moon's reactionary politics, involving support for a vicious military regime in Korea, and support for Nixon in America. His dislike of such a political stance justifies Horowitz's reprinting of the circumstantial evidence and massive speculation concerning the relationship between Moon's movement and the KCIA. The book may be a significant document in the history of the controversy regarding the Unification Church, but it does little to further our understanding of the movement.

Flo Conway and Jim Siegelman however have probably already cornered a large slice of the anti-cult book-buying market with their curious volume Snapping. The title refers to the expression they allegedly found to be used by many former "cult" members: "Something snapped inside me" to describe their conversion. The book is a composite of well-systematised cult prejudice dressed neatly, and perhaps to the uninitiated, "scientifically", with a terminology drawn from information theory and the currently fashionable - although in this context quite vacuous - catastrophe theory.

Their methodology is epitomised by the statement that "we found very few people who got out of the Unification Church or any other cult on their own" (p.36). In the course of several years' research on new religious movements I must have met dozens of people who left such movements "on their own" so it is not much of a credit to Conway and Siegelman that they had so much difficulty. It is not, however, surprising since the anti-cult movement seems in this case too to have been their principal source of informants.

The main drift of this volume lies in the attempt to support the fatuous notion that it was only as a result of something "snapping" in their rational information-processing capacities that people join religious and non-religious cults. The information theory language in fact merely renders more contemporary the now hoary nonsense of William Sargant. The attachment to some alternative view of the world - consequent upon "snapping" - to that purveyed by Conway and Siegelman is labelled "information disease", betraying a somewhat excessive - and by no means clearly merited - complacency, if not conceit, concerning their own ability to process information.

"Snapping" and "information disease" are in sound Sargant fashion held to be the result of: poor diet; prolonged lack of sleep; and of "intense experiences that abuse an individual's natural capacities for thought and feeling". (p.114) No suggestion here that anyone might come to believe notions wildly at variance with the great western capitalist consensus on the basis of any rational reflection on the ideas proffered, experience of their mental or emotional consequences, or gut attraction to new friends or lifestyle. Conway and Siegelman have not only not troubled themselves to look to the many thousands of healthy voluntary and un-aided cult defectors (one could scarcely ask them to talk to the thousands of entirely rational and coherent cult members), but also they have not troubled to talk to, or read the reports of, the many impartial sociologists and anthropologists who have investigated the process of conversion and its consequences and found it to be neither ineluctable nor mechanical and certainly no more irrational than falling in love. (Perhaps we shall shortly be offered an account of falling in love that finds it a result of poor diet or prolonged lack of sleep.)

2. Non-Hostile Externalist Accounts

Less meretricious and intellectually impoverished than these are the accounts which do not define their orientation from the outset as inimical to that of the believers under investigation. The principal exemplar under consideration here is Nordquist (1978). Nordquist presents a history of the development of the Ananda Community in Nevada, a study of its world-view, and an analysis of its members' attitudes and background characteristics. Although Nordquist engaged in periods of observation in the community, he sought to persuade members to complete a battery of questionnaire and psychological test schedules which included such gems as:

18. What motivated your joining Ananda Cooperative Village? Visitors please continue to question 19

As an aid to answering this question (18), please continue your train of thought in answering the following questions and checking the listed categories:

- a. I was motivated mostly by: (check one)
- Social-political conditions _____
 - Psychological reasons (inner) _____
 - Both equally _____

If you checked mostly social-political reasons or both equally, please continue by checking one of the following:

- b. The social-political reasons were mostly related to:
- Immediate relationships with other people _____
 - Local social-political conditions or events _____
 - National social-political conditions or events _____
 - International social-political conditions or events _____
 - Other _____

If you checked mostly psychological reasons or both equally, please continue by checking one of the following:

- c. The psychological reasons were mostly related to:
- Self, identity (who am I?) _____
 - Personal relationships with close others _____
 - Role in society or personal worth ... _____
 - Altruistic reasons (universal love, brotherhood, relationship to God or the unknown, etc.) _____
 - Other (explain) _____

- d. Please elaborate upon the categories checked above. For example, if you checked 'national social-political conditions or events', please describe the event, when it occurred, your reaction and resultant change in values and/or behavior, which led to your joining Ananda.

19. How long do you expect to remain at Ananda Cooperative Village?

20. What keeps you here? Or, what is it about Ananda Cooperative Village that you consider of value as compared to opportunities in the larger community?
(pp 163-4)

Since the members of Ananda were primarily counter-cultural dropouts with a decidedly "laid-back" approach to life, it is not altogether surprising that only 28 out of a hundred or so completed the questionnaires and tests. In the event, moreover, lacking any meaningful comparative groups, Nordquist is able to do little more than report in table after table the results of this enterprise. Thus we learn of the 28 respondents who rated their social activity "a) before and after any radical change in your beliefs and values prior to any thought of joining Ananda Cooperative Village, and b) before and after joining Ananda Cooperative Village?" a) 25% (7) were "very active" before and 11% (3) after; 7% (2) rated themselves "once or twice" (sic!) before and 24% (8) after. (p.87). We learn that: "Joining Ananda and adopting the world view and yoga discipline taught by Swami Kriyananda was motivated by psychological reasons, usually interpreted as 'self-identity' and 'altruistic'." (p.102) It is not altogether clear whether these are primarily linguistic problems for Nordquist, whose mother tongue is Swedish, or whether he sees his respondents through a heavy fog of survey categories.

Where Nordquist does seek to compare his results in the attitudinal tests with those obtained on earlier trials by the instruments' inventors, the results are often trivial or incomprehensible. For example it is scarcely any surprise that the members of a community who have abandoned the urban industrial rat-race for spiritual development, craft work, a vegetarian diet, and an agrarian environment should display in one scale attitudes "reflecting a concern with run-away technology, a polluted and defiled environment, depersonalization in work, and the suppression of spontaneity in bureaucratic organizations and they very strongly favour mysticism and a simple life in a natural environment." (p.153) Had the scale not shown that he would surely have thrown away the scale. In short, this is a rather mechanical study of a potentially interesting topic. The historical and ethnographic accounts are ill-formed by any developed theoretical or analytical focus but a nebulous concern with the counter-culture, and fail to resonate as a closely empathic interpretation of this particular group of believers and their style of life. Attitudinal scales are a poor substitute for either conceptual sharpness or interpretive insight.

This is a lesson unfortunately not learned by Richardson, Stewart and Simmonds (1979) from whom Nordquist evidently gained much inspiration. Their study of a Jesus People movement which they call Christ Communal Organisation (CCO) promises a great deal. The three principals, supplemented at times by up to six others, spent periods of time over several years in locations of this movement, observing, interviewing, surveying and personality testing. They claim, credibly enough, that they have gathered vast quantities of data. Some of this provides the basis for interesting description of the origins and development of the movement, features of daily life within it, and background characteristics of members. But, unfortunately, most of the data were gathered to "test" various theories in a manner little short of a parody of positivism in sociology.

This involves here profoundly laboured commentary on prior theoretical work to arrive at a set of curious tests. For example, they take up Berger's notion of alienation as involving a situation in which people "forget" that their social world is created by them. Richardson et al translate this into "an 'alienation scale' that seemed to access (sic) the domain of substance addressed by Berger . . ." (p213) (The volume's literary qualities also leave something to be desired). This "alienation scale" comprises the following questions:

- "1. Do you think that God has a hand in the nomination and election of our country's leaders?
2. Do you think that the leaders of our country are guided by God in making decisions?
3. Do you think it is a sin to break a law of the land?
4. Do you think that the United States generally is an instrument of God in the area of world politics?" (p.214)

Affirmative answers to these questions (95% with q 1; 65% with q 2; 91% with q 3; 47% with q 4) lead Richardson et al to conclude that "alienated people (were) attracted to or formed by this group" (p. 216). The assumption here seems to be that the notion of a God who intervenes in social and political affairs entails that those committed to such a notion have forgotten that their social world is created by them. But believing it sinful to break the law is scarcely incompatible with the notion that the law is man-made. "Render unto Caesar" incorporates this well-known principle. Again, it is perfectly coherent to believe that God "has a hand" in choosing a nation's leaders as in everything else ultimately, while also believing that if you want to get good men elected you have to campaign like the devil. Moreover, even if we were to call these things indicators of alienation, how, without any comparative evidence, do we know alienated people were either attracted to or formed by this group? They might have been (i) more alienated before than they are now, but (ii) less alienated than everyone else. And surely it does make a difference whether alienated people were attracted to or formed by the group.

Richardson et al miss the chance to provide us with a close interpretive account of the life of CCO and to generate new theoretical insights from it because their preoccupation with being scientists leads to the erection of massive barriers between them and their subjects. These can henceforth only be viewed through some standardised instrument related in the most tenuous fashion to a body of thought, the abstractness and generality of which scarcely lent itself to such particularistic operationalisation in the first place, or through the categories of endless models and typologies which are "extended" and "applied" with no apparent regard for whether anything is thereby illuminated.

3. Hostile Internalist Accounts

Normally of rather greater utility to the sociologist of religion, because based on a closer acquaintance with the object

of study, are the internalist accounts. A number of these were written by sociologists but others have been produced by former members of the movements in question. It is from among the latter that hostile internalist accounts more frequently emerge; although a study like that presented in (Bugliosi, 1977) is derived from such close scrutiny and careful reconstruction of the life and beliefs of a particular movement as to attain a high level of descriptive authenticity and thereby of utility for the sociologist who would seek to derive from case studies a more general analytical or theoretical framework. Apart from slight differences in the reconstruction of the facts - inevitable in a context where Bugliosi was seeking to prove the guilt of his subjects of study - his account matches well those of Watson (1978) and Atkins (1978). Interestingly, the two ex-member accounts differ greatly in quality. Atkin's book provides little new information or insight into Manson's gory group, while Watson's book is a rich ethnographic document facilitating a greater understanding of the dynamics of the Manson Family and thus comparisons with similar totalitarian groups, e.g. these observations on sexual relations in the Family:

"The lack of sexual discrimination among hard-core Family members was not so much gross animalism as it was simply a physical parallel to the lack of emotional favoritism and attachment that Charlie taught and insisted on. As long as we loved any one person more than the others, we weren't truly dead and the Family wasn't one." (p.70)

This could scarcely have been stated more succinctly by Kanter (1972:86) who observes that

"Two-person intimacy poses a potential threat to group cohesiveness unless it is somehow controlled or regulated by the group. Groups with any degree of identity or stability face the issue of intimacy and exclusive attachment and set limits on how much and what kinds are permissible or desirable. Exclusive two-person bonds within a larger group, particularly sexual attachments, represent competition for members' emotional energy and loyalty. The cement of solidarity must extend throughout the group."

But relatively few first-rate hostile internalist accounts are available for the simple reason that antipathy felt for the movement is not conducive to viewing the thought and action of its members as rationally considered and strategically motivated in response to their circumstances. The tendency for the defector or the apostate is to dehumanise the movement's personnel, to justify his own lapse from conventional behaviour by representing all recruited to this alien world-view as victims of some routinely effective and ineluctable technology of mental control by which they are, and he was, tightly gripped.

Thus, the tendency is strong for defectors' tales* to drift readily into hostile externalist accounts, despite the author's close internal experience of the movement and his own recruitment to it. In the time between leaving the movement and writing about it the author has typically acquired a new, often very divergent view of the world and a dramatically changed perception of the movement, dramatically changed in many cases through the experience of "deprogramming".

Such accounts as these, then, have a moral purpose and must therefore be treated with reservations in some particulars. The story we are being offered has been reconstructed, in part at least, as a means of justifying past deviant behaviour or rehabilitating the formerly deviant author. This may lead to presentation of certain features of the narrative or relevant actors in it in a way that conforms to prevailing outsider stereotypes and thus symbolises to significant others that the author is at last truly seeing sense in these respects and is fully repentant of his earlier misdeeds.

An example in this genre is Christopher Edwards' book Crazy for God (Edwards, 1979). Edwards was for seven months a member of the Unification Church in Berkeley during 1975-76, from which he was kidnapped and "deprogrammed" by Ted Patrick. As the title vividly suggests, Edwards does not seek to display the rational character of life in this particular religious movement. His story stresses the infantile dependency which he believes to be generated by the recruitment process; the repression of criticism and progressive attenuation of critical evaluation; the subordination of individual autonomy to authoritarian leadership; the self-imposition of a rigorous and ascetic style of life; the zealous commitment to arduous and demeaning methods of fund raising; all in conformity to a set of beliefs which - on his account - appear bizarre and even delusional. Only "mind control", "thought reform", "brainwashing" could explain such behaviour, Edwards would have us conclude. Yet in what does this "technology of mind control" consist? It seems that Edwards can discover nothing more pernicious than an intensive round of activity, constant supervision and reinforcement by committed members, relatively little sleep, reiterated proclamations of love and displays of chaste affection by his companions, and appeals to his youthful idealism and belief in God. His efforts to exhibit himself as the victim of a cruel and mechanical subjection to a technology of mind control are self-serving and unconvincing. His wounds - if such they are - were self-inflicted.

*One of the best accounts by a former member of one of the new religious movements is Robert Kaufman's (1972) amusing, poignant and marvellously insightful account of his experience of Scientology.

But one cannot bring oneself too glibly to say the same of Jeannie Mills, her family and friends in the Peoples Temple. Mills (1979) does refer to herself and other members as "brain-washed" but she resorts less readily than Edwards to the clichés of "mind control" and "hypnosis". Indeed after reading her book one remains unclear as to her reasons for joining the Temple and feels that in this respect she remains as hazy as we do.

Jeannie and Al married in 1968 bringing five children from previous marriages with them, but lacking friends outside their family. Al had been dedicated to the Civil Rights cause since the time of Selma and one can see why the Peoples Temple of 1969 in Ukiah, California should be attractive to him in its attempt at racial integration and Jeannie as a reluctantly lapsed Seventh-day Adventist was perhaps in the market for a faith. They were both attracted by the warmth and friendship that they found there on their first and subsequent visits, and the apparent, mutual concern felt by members. They were awed by Jim Jones's confidence, by his "divine revelations", by his miraculous healings, and by his apparent discovery of a means of saving his followers in the event of the nuclear holocaust widely feared around that time. Yet surely it is more than merely the fact that European societies are less mobile than America, that people here are less ready to pull up stakes on a whim, that makes it difficult for me to comprehend how these could be such compelling reasons for selling a farm bought only the preceding year and abandoning their jobs to move the 120 miles with five children to become a part of Jones's church after a mere few months acquaintance. It only took that long because of the need to sell one home and buy another. Their decision to join had been made after only two visits.

Such sudden conversions are not without precedent of course. The early disciples of Jesus abandoned home, family and work to follow their saviour and yet this is only to dignify such whims, not to explain them.

It seems equally hard to render rational and reasonable their remaining attached for six years to a leader so clearly in retrospect manipulative, mendacious, cruel, rapacious and paranoid but, of course, that is part of the problem of being a defector recollecting in tranquility. With hindsight Mills now sees Jones in that light as do most of us, but in the living of it those attributions were less obviously applicable, flickering briefly into consciousness only to be brushed aside as disloyal, as needing to be balanced by his great purpose and goals, as only possible from ignorance of his full plan, or as based upon inadequate data or comprehension. They could be rationalised away until the stage when they could no longer be denied, and from then silence was maintained from fear rather than self-doubt.

Hindsight does not entirely destroy the sense of lived experience and it is in the areas of the manufacture of charisma and the maintenance of attachment that Mills's account is most valuable. Jones produced an aura of power through his healings, prophecies, the striking down of the doubter or dissident, and the raising of the dead. The healing of cancer was a clever subterfuge in which assistants inserted chicken liver into the sufferer's mouth under cover of a cloth, causing him or her to gag and spit out this offal which seemed to come from their own

body. The prophecies and revelations were normally produced by the activities of assistants who explored the contents of the church member's house or garbage can while they were out, or by telephoning someone from whom relevant information could be obtained representing the enquiries as part of a social survey or the like. The striking down of doubters and dissidents seems to have been effected by the surreptitious administration of poison, and raising the dead by calculating relatively closely the duration of its effects. The powers manifested by Jones plus his avowed utopian aims and sense of purpose with which he endowed the lives of his followers, the welcoming community around him and his promises of greater things to come all provided grounds for excusing his bouts of temper and cruelty, his growing preoccupation with sex, the increasing frequency of his humiliation and punishment of members, the rising demands for their personal and financial dependence. But of course, beyond that members like Jeannie and Al had entered into numerous commitments. They had moved into the area, were fostering numerous Temple children whom they loved and whose future they cared about. They began working for the Temple becoming dependent upon it for income. They signed over their property to be managed by the Temple, and wrote letters describing crimes and immoralities they had not committed as a sign of loyalty and faith in Jim. They began to fear the possibility of public humiliation and physical punishment, that the children they loved would be taken from them, that their lives might be in danger. For fear of the doubts and concerns being reported to Jones each member would stifle what could be forced out of consciousness and keep to himself what could not. Pluralistic ignorance heightened the sense that doubts were a personal failing rather than a common condition. Even before being isolated in Guyana then there were powerful forces to maintain the attachment of members even to a leader who was, it is now clear for some years before the awful massacre a dangerous paranoid.

But what is obvious now clearly was not obvious then and for all the accuracy of the detail and wealth of human insight that Mills provides it can hardly be otherwise than that her account of those six years with Jones is coloured by the moral purpose of making sense of an association now too incredible and horrendous to bear.

4. Non-Hostile Internalist Accounts

However it is not merely hostile internalist accounts that can be adapted to a prior normative position. Sontag's (1977) study of the Unification Church clearly aims to fulfil quite the opposite purpose. Out of apparently almost unlimited access to persons and documents, including a nine hour interview with Moon himself, emerges an analytically quite trivial work designed it would seem to achieve no other end than to improve the public relations of the Unification Church. Sontag seems cautiously to have confined his questions and his probing to topics approved by the church's leaders and information sources controlled by them. He makes no effort to compare information gained by such means with information from other sources, such

as defectors, nor to explore other pressing questions about Moon and his movement beyond whether or not it may be divinely inspired (and it is no surprise when he answers that question affirmatively). It is a feeble intellectual product, adding nothing to our empirical or analytical grasp of this new religion.

Equally weak but for quite different reasons, is Damrell's (1977) study of a Vedanta church. Damrell engaged in participant observation in the Church Universal, an organizational form of Vedanta in a particular Californian city. Damrell appears to be engaged in an exercise in existential phenomenology. This seems to mean that the book is almost entirely about Damrell's subjective experience of engagement in the Church. He eschews a structural or interactional account on the ground that his approach is somehow purer and more valid. The problem for the reader, however is to discern any sociological relevance in Damrell's experiential narrative. Are we to assume that Damrell's experiences somehow exhibit features typical of members of the Church? Damrell provides no reason for believing that this is so. Moreover, even if it were there seems little virtue in presenting the experience of members of a group unless this leads to the uncovering of some relationship between the experience and more general properties of social relations. But there is little of intrinsic interest in Damrell's idiosyncratic experiences in a movement which appears otherwise sociologically rather dull on Damrell's account.

Even a hostile internalist account such as that of Watson (1978) is of more use in this regard, since in an untutored and unselfconscious way, Watson tells us biographical details of members of the group as well as its structural characteristics, and relates these to the group's development or to the experiences of participants. Understanding an alien or deviant world-view as a participant is only the first step in doing sociology. Damrell's book (1978) on a youth culture religious group, which he calls the Church of Cosmic Liberty, while published after the Vedanta book appears to have been written before, since it is drawn from Damrell's doctoral thesis. Sociologically it is a better study, being more conventional in approach, perhaps because of the constraints of its production, involving an element of distance from rather than complete absorption into, the world-view under observation. Moreover this study does seek to draw some broader sociological points from the case study, relating to the social location of contemporary American youth, and the character of the adolescent identity search. These points definitely merit publication but it is not immediately obvious that his whole book was necessary to achieve that end. My main reason for saying this rests on the essential triviality and banality of the lives of the members of the Cosmic Libertarian Church. Most of them appear to be inarticulate, shiftless, parasitic, irresponsible, and aimless. Their religion was invented as a self-serving legitimation for continuing drug use. It remains as a self-righteous justification for an otherwise apparently ill-deserved sense of ethical or spiritual superiority. But quite such detailed descriptions of the daily life, beliefs

and practices of such a tiny, insignificant, and - apart from the observations Damrell makes on youth culture - theoretically uninteresting group of people, seem unnecessary in the circumstances.

A considerable improvement on this is Downton's (1979) study of the Divine Light Mission. Downton's book is based on intermittent observations and lengthy interviews with 18 premises in Boulder, Colorado. His efforts to determine the validity of his findings from such a small sample of informants by gathering questionnaire data are not terribly convincing since they draw upon only a further 23 followers of the Mission. Four of the interviews are presented in edited version for the first quarter of the book and others are reported, on the basis of locating apt illustrative interview material, as support for the limited range of analytical and theoretical points Downton then makes through the rest of the book. The analysis is of a fairly low order and Downton does not, I think, offer any significant theoretical advance on the basis of this study. Moreover his account of the Mission organizationally is rather scant. The nature of the movement's structure can be gleaned and some elements of its developmental history are presented, but there is little effort to utilise this information to any theoretical end. Indeed, theoretically Downton's ideas are rather dated - Starbuck and Salzman on conversion, for example - and he has no great familiarity with the literature on sectarianism or new religious movements which might have focussed his theoretical thinking to more purpose, but the subject matter of the book is of considerable intrinsic interest to those working in these fields and the information he has gathered is extremely useful on a movement concerning which little reliable information is yet available. While Downton's conceptualization of the phenomena under investigation is not a major contribution, his approach is basically sympathetic to his subjects and the resultant materials are thus not forced into a form suitable to a set of prior antipathetic assumptions.

By far the best of the studies considered here however is Bainbridge's major work on The Process, which he calls "The Power" in his book Satan's Power (Bainbridge, 1978). Bainbridge cultivated an acquaintance with this movement over several years, participating in its activities, befriending its members and leaders, studying its writings. The book is a sympathetic work but loses nothing of its objectivity or analytical clarity for that. Bainbridge evokes a sense of the tangibility and personality of the people involved without having to rely upon their own, voluminously transcribed, words to do it. He conveys an understanding of the people involved, and their style of organisation without constantly dusting off someone else's model or typology, or in laboured and pedestrian fashion operationalizing some set of contrived and inconsequential "hypotheses". Bainbridge almost errs too much the other way. He is rather cavalier about referring to any comparative studies or applicable analytical or theoretical ideas and is sometimes less than explicit about the analytical implications of his descriptive material. His work is in the tradition of Lofland's Doomsday Cult, and permits the enthusiasm with ethnography to overwhelm the aim of sociology to provide some (limited) generalisations and to develop conceptualizations applicable beyond the single case. Fortunately,

Bainbridge does not entirely eschew this task and it is the combination of first-rate ethnography with some important and suggestive ideas concerning the formation of cults of this kind that make it far the most valuable of the studies considered in this review.

Conclusion

The lessons to be gathered are evident enough. New religious movements must be carefully understood before they are explained. Understanding them is likely to involve (but need not entail) having some sympathy with them. Explanation and analysis of any enduringly worthwhile kind are likely to emerge from close and direct observation, particularly when this is subjected to frequent comparative reference, rather than from the mechanical application of standardized "instruments". The latter style of exercise is likely to appear most fatuous when it fails even in its own terms, by glossing over or ignoring the need for adequate control groups subjected to the same standardized measures. Understanding even such alien groups as these must begin from a position of interpretive charity in conceding the reasoned - if not always "reasonable" - behaviour of those involved, and in exploring that behaviour as the willed and motivated acts of the persons concerned. Explanations which begin from the premise that the bizarre and deviant behaviour which abounds in this field is a purely reactive response to mechanical compulsions, social or psychological, is likely to secure so little purchase upon the phenomena as to be worthless.

WORKS REVIEWED:

- Atkins, Susan (with Bob Slosser)
1978 Child of Satan, Child of God, Hodder and Stoughton, London.
- Bainbridge, William Sims
1978 Satan's Power: A Deviant Psychotherapy Cult, University of California Press, Berkeley
- Bugliosi, Vincent (with Curt Gentry)
1977 Helter Skelter, Penguin, Harmondsworth.
- Conway, Flo & Jim Siegelman
1978 Snapping: America's Epidemic of Sudden Personality Change, Lippincott, Philadelphia.
- Damrell, Joseph
1977 Seeking Spiritual Meaning: The World of Vedanta, Sage, Beverly Hills.
- 1978 Search for Identity: Youth, Religion and Culture, Sage, Beverly Hills.

- Downton, James V.
1979 Sacred Journeys: The Conversion of Young Americans to Divine Light Mission, Columbia University Press, New York.
- Edwards, Christopher
1979 Crazy for God, Prentice-Hall, Englewood Cliffs, NJ.
- Enroth, Ronald
1977 Youth, Brainwashing and the Extremist Cults, Zondervan, Grand Rapids and Paternoster Press, Exeter.
- Horowitz, Irving L. (ed)
1978 Science, Sin and Scholarship: The Politics of Reverend Moon and the Unification Church, MIT Press, Cambridge, Mass.
- Mills, Jeannie
1979 Six Years With God: Life Inside Rev. Jim Jones's Peoples Temple. A & W Publishers, New York.
- Nordquist, Ted A.
1978 Ananda Cooperative Village, Uppsala University, Uppsala, Sweden
- Richardson, James T, Mary W. Stewart
and Robert B. Simmonds
1979 Organized Miracles: A Study of a Contemporary, Youth, Communal, Fundamentalist Organisation, Transaction Books, New Brunswick.
- Sontag, Frederick
1977 Sun Myung Moon and the Unification Church, Abingdon, Nashville.
- Watson, Tex (as told to Chaplain Ray)
1978 Will You Die for Me? Fleming H. Revell Co., Old Tappan, New Jersey.

Other Books Cited

- Kanter, Rosabeth M.
1972 Commitment and Community Harvard University Press, Cambridge, Mass.
- Kaufman, Robert
1972 Inside Scientology: How I Joined Scientology and Became Superhuman. Olympia Press, New York.
- Sargant, William
1957 Battle for the Mind, Heinemann, London.

BOOK REVIEWS

Search For the Soul. By Milbourne Christopher. New York, N.Y.:
T. Y. Crowell, 1979. 206 pages. \$9.95.

Reviewed by Martin Ebon

Milbourne Christopher leads a dual existence. He is an outstanding stage magician and chairman of the Occult Investigation Committee of the Society of American Magician; he is also a historian of the psychic and occult. His writings in this field include ESP, Seers and Psychics, and Mediums, Mystics and the Occult. Christopher, being a conjurer by profession, tends to spice his accounts of historical and contemporary psychic happenings with a generous dose of doubt. He is forever on the lookout for trickery, as are, of course, all serious and sophisticated parapsychologists.

Mr. Christopher's latest work, Search for the Soul, views recent fascinations with "life after life" against a background of century-old efforts to trace the human soul beyond bodily death. The author defines his task as "an account of the efforts that have been made to see, isolate, and analyze the soul," including "strange experiments in seance rooms and laboratories more than a hundred years ago and equally intriguing tests utilizing scientific equipment in modern times."

Milbourne Christopher, with somewhat sad and ironic detachment, describes earlier efforts to photograph the soul, to weigh it, and to define its shape in cloud chambers. He brings his readers up to date in a description of Dr. Raymond Moody's case histories, and cites their antecedents among nineteenth century spiritualists. Christopher finds fault, although in gentle terms, with techniques used in out-of-the-body (OBE) experiments conducted by Dr. Karlis Osis of the American Society for Psychological Research and Dr. Charles Tart, University of California at Davis. He concludes that "the paramount issue -- does the soul leave the body at death? -- remained unresolved."

Christopher's benign manner dissolves briefly when he speaks of Osis's voyage to India, where he observed seeming miracles by Sai Baba that "have been exhibited by other 'holy men' in India -- and by conjurers!" He disdainfully cites Dr. Osis as writing, "At no time did we see anything that suggested trickery," He concludes this chapter with the ASPR's report that "it did not accomplish its goal of proving the existence of the human soul."

There is virtue in brevity, but I found Christopher's treatments of Drs. Moody and Elisabeth Kubler-Ross truncated. It is to his credit that he deals with Kubler-Ross in a sympathetic and respectful way, considering her recent controversial entrapment by dubious spiritualistic practices. A chapter on reincarnation completes the book. The author ends on a note of poetic whimsy, echoing those who feel that the human soul continues to be "as invisible as the aroma of a flower and as elusive as the wind."

By covering as much ground as he has, Milbourne Christopher did not exhaust any particular aspect of man's long "search for the soul." One hopes that his well-mannered critiques will, in the future, zero-in on more specific aspects of psychic studies, so that he can bring his historical perspectives and research skills more fully and vigorously to bear.

Divided Consciousness: Multiple Controls in Human Thought and Action. By E. R. Hilgard. John Wiley & Sons, New York, 1977. 300 + xv pages. \$16.95.

Reviewed by Ivan W. Kelly

This is a remarkable and highly significant book.

For the last two decades Hilgard has been researching the topic of hypnosis as an altered state of awareness. As a result of this rigorous experimental research, Hilgard has been established as an acknowledged authority in this area.

Hilgard's explorations in the area have led him to investigate, using naturalistic techniques, phenomena such as multiple personality, automatic writing, fugues, "age regression," amnesia, and "possession" states.

The first seven chapters of the book give a clear and concise summary of the various kinds of dissociation. Three sets of dissociative phenomena are examined: (1) spontaneous nonhypnotic dissociations such as fugues, multiple personalities, and states of "possession," (2) hypnotic dissociations such as those found with the "hidden observer," and (3) everyday dissociations such as divisions of attention like doing two things at once. This section provides an excellent introduction to phenomena that will be of interest to Zetetic Scholar readers.

Hilgard cautions us against accepting paranormal explanations for events until we have exhausted all normal explanations. He presents us with an enlightening case from his own laboratory:

A bright male college student had submitted to hypnosis in a social gathering and had found himself thoroughly at home in England in the mid-nineteenth century, speaking in contemporary terms about the Royal family, and knowing the names and ages of Victoria's children when they were young. He came to the laboratory convinced that this was a genuine reincarnation experience, but was willing to have it subjected to criticism. When carefully interviewed about events that might have provided memories that could have been evoked under hypnosis, it turned out that, many years earlier, he had made an intensive study of the British Royal family, although, with a subsequent change of interest

from literary to scientific pursuits, he had forgotten all about it until the biographical search brought them to light. Although the evidence is against the reincarnation interpretation, it is interesting in its own right because it shows that memories may be re-captured without identification (as in source amnesia) and woven into a realistic story that is believed by the inventor of the story. [p. 51]

Hilgard is skeptical of paranormal explanations of these phenomena and, in the second half of the book, outlines his own theory to account for them. The framework within which these phenomena are examined is a neodissociative approach that proposes that an individual has a hierarchy of multiple control systems that can be reordered by hypnotic procedures. Hilgard's particular theory of neodissociation was formulated in large part to account for what he refers to, metaphorically, as the "hidden observer" which occurs with many highly susceptible subjects. He found that information can be processed at two different levels. While the individual is under hypnotic analgesia, a part of the person remains fully aware of, and can report, the pain that the hypnotized part is not aware of.

The book will probably fail to satisfy the reader who is primarily concerned with theory, for the treatment of some theories (e.g. information processing theories) is rather sketchy, and Hilgard does not present an attempt to synthesize the different theories into one overall approach to the study of divided attention and consciousness. These are the problems to which we hope Professor Hilgard will turn his attention in the future. Few people are better equipped to tackle them. In spite of this defect, Professor Hilgard's book can be warmly recommended to those who wish for a fascinating introduction to this field of inquiry.



BOOKS BRIEFLY NOTED

Listing here does not preclude later full review.

- Baker, Margaret, The Folklore of the Sea. North Pomfret, Vt.: David and Charles, 1979. 192pp. \$14.00. An excellent introduction including many paranormal claims from phantom ships and sailors to sea serpents. Recommended.
- Balfour, Michael, Stonehenge and Its Mysteries. N.Y.: Charles Scribner's Sons, 1979. 192pp. \$14.95. A lavishly illustrated and very well documented addition to the growing literature on Stonehenge and its related prehistoric sites. Possibly the best introduction and survey now available for the general reader.
- Banarjee, H.N., The Once and Future Life: An Astonishing Twenty-Five-Year Study on Reincarnation. N.Y.: Dell Publishing Co., 1979. 139pp. \$1.95 paperback. Mainly a series of case studies, generally poorly documented and of little scientific value. For the already convinced.
- Begg, Paul, Into Thin Air: People Who Disappear. North Pomfret, Vt.: David and Charles, 1979. 184pp. \$16.95. A generally well documented and well done survey of disappearances on land, at sea, and from the air. ZS readers will especially enjoy "They Vanished in Front of Witnesses" and the chapters dealing with the Bermuda Triangle.
- Blanpied, Pamela Wharton, Dragons: An Introduction to the Modern Infestation. N.Y.: Warner Books, 1980. 194pp. \$9.95. A scholarly dissertation on dragons in the world today, complete with charts, graphs, expedition photographs, and a lengthy bibliography of the zoological and related writings on these extraordinary beasts. For those who take their dragons very seriously. A much needed antidote to the recent books seeking to debunk claims of these much maligned beasts by suggesting they are merely mythological.
- Braude, Stephen E., ESP and Psychokinesis: A Philosophical Examination. Philadelphia, Pa.: Temple University Press, 1979. 283+xv pp. \$19.50. A major study in the Philosophical Monographs series. A very sympathetic but informative and careful study of parapsychological claims, and one which hopefully will stimulate further philosophical dialogue among philosophers of science. Though Braude's discussion of mind may be too much dependent upon his views about the reality of psi, the sections on synchronicity and on the definition of "paranormal" are particularly valuable even to those of us who might disagree with him on the issue of psi. Recommended.
- Brecher, Kenneth and Michael Feirtag, eds., Astronomy of the Ancients. Cambridge, Mass.: MIT Press, 1979. 206pp. \$12.50. An excellent series of articles which first appeared in Technology Review. Brecher's "Sirius Enigmas" should be of special interest to ZS readers.
- Bromley, David G., and Anson D. hupe, Jr., Moonies in America: Cult, Church, and Crusade. Beverly Hills, Ca.: Sage Publications, 1979. 269pp. \$18.00 hardcover, \$8.95 paperback. A major sociological examination of the Unification Church and indispensable to anyone seriously interested in the organization. Recommended.

- Brown, Barbara B. Supermind: The Ultimate Energy. N.Y.: Harper & Row, 1980. 286pp. \$11.95. A general survey arguing for extraordinary human capabilities. Poorly documented and scientifically dubious presentation obviously intended for a popular audience. Humanistic psychology meets pop-dualism.
- Burks, Arthur W., Chance, Cause, Reason: An Inquiry into the Nature of Scientific Evidence. Chicago: University of Chicago Press, 1977. 694pp. \$12.50 paperback. A very sophisticated and formal study of the logic of empiricism by an eminent philosopher and computer scientist. The author develops a combined and unified theory of induction, causality, and three kinds of probability (inductive, empirical, and frequency).
- Burl, Aubrey, Prehistoric Avebury. New Haven, Conn.: Yale University Press, 1979. 275pp. \$19.95. A definitive study of the standing stones at Avebury, fully illustrated and highly readable. A fine addition to the literature and the author's earlier The Stone Circles of the British Isles.
- Clarke, I.F., The Pattern of Expectation 1644-2001. N.Y.: Basic Books, 1979. 344pp. \$16.50. A fine survey of the history of prophecy of technology in the literature of the 19th and 20th century. From the utopian predictions to the modern futopian doomsters. Excellent.
- Cunliffe, Barry, The Celtic World. N.Y.: McGraw-Hill, 1979. 224pp. \$39.95. A magnificent and lavishly illustrated survey of Celtic culture. A fine introductory work and a beautiful "coffee-table" book.
- Doran, Frances C., Words from Tikal. N.Y.: Manor Books, 1978. 273pp. \$1.50 paperback. Purportedly a book of Mayan divination, this compendium of fortune-telling statements is an excellent source for stock replies for cold readings. Scientifically nonsense, but a good example of divinatory rhetoric which mentalists should find useful.
- Duncan, R., and M. Weston-Smith, eds., Lying Truths: A Critical Scrutiny of Current Beliefs and Conventions. Elmsford, N.Y.: Pergamon Press, 1979. 242pp. \$16.50. An interesting but rather uneven collection of original essays debunking common contemporary beliefs such as "reality exists outside us" and "you can prove anything with statistics." Some fine writers, including Colin Wilson, Arthur Koestler, and Antony Flew, but overall disappointing after the editors earlier Encyclopedia of Ignorance, to which this is a sort of sequel.
- Epstein, Perle, Monsters: Their Histories, Homes and Habits. Garden City, N.Y.: Doubleday, 1973. 123pp. \$6.95. A pleasant little volume for the young reader, entertaining and well done.
- Farson, Daniel, and Angus Hall, Mysterious Monsters. N.Y.: Mayflower Books, 1978. 256pp. No price indicated. An enlarged edition of the combined two earlier books Vampires, Zombies and Monster Men and Monsters and Mythic Beasts. Issued by London's Aldus Books in 1975. Generally an excellent collection, especially the illustrations. A real bargain at many "remainder" and discount book chains, and one of the best "coffee-table" books of its kind around with some excellent photos and color illustrations.

- Fell, Barry, Saga America. N.Y.: Times Books, 1980. 425pp. \$15.00. Dr. Fell continues his controversial presentation of the case for trans-Atlantic Old World incursions by Libyans, Carthaginians, Celts, Greeks, Romans and Vikings into North America, started in his book America, B.C. Fascinating but likely to be greeted as unconvincing by orthodox scholars.
- Ferguson, Marilyn, The Aquarian Conspiracy: Personal and Social Transformations in the 1980s. Los Angeles, Cal.: J.P. Tarcher, 1980. 448pp. \$15.00. A remarkable survey of the many strands of the consciousness movement by the editor of Brain/Mind Bulletin. Lots of information but uncritically and somewhat indiscriminately presented. Probably the best introduction to this vast area and well worth reading for the information contained aside from questions of assessment.
- Firsoff, V.A., At the Crossroads of Knowledge. Hornchurch, Essex: Ian Henry (U.S. distribution through Ross-Erikson Publishers), 1977. 139pp. \$8.95. Astronomer Firsoff's interdisciplinary consideration of the possibilities of alternative biochemistries for life elsewhere in the universe. Fascinating and anchored in research.
- Fisher, John, Body Magic. N.Y.: Stein and Day, 1979. 158pp. \$10.00. Not just another magic book but a series of enlightening experiments about our own bodies nicely scientifically explained. Excellent.
- Fitzgerald, Randall, The Complete Book of Extraterrestrial Encounters. N.Y.: Collier Books, 1979. 200pp. \$5.95 paperback. A very useful summary statement of almost all the leading ideas about UFOs, from those of Billy Graham to von Däniken. Mostly two page outlines of the "great books" of ufology including the leading skeptical book. Recommended.
- Frisby, John P., Seeing: Illusion, Brain and Mind. N.Y.: Oxford University Press, 1980. 160pp. \$16.95. A copiously illustrated, scientifically expert, and lucidly written book for the general public on visual perception including much material on illusions. Includes special glasses for viewing the specially printed color effects. Informative and fun.
- Fuller, Curtis, G., M.M. Fuller, J. Clark, and B.L. White, eds., Proceedings of the First International UFO Congress. N.Y.: Warner Books, 1980. 440 pp. \$2.75 paperback. A major collection of pieces dealing with most facets of ufology including most of the major pro-UFO writers. Hard-line critics are not included (though their position in ufology is scathingly but honestly examined by David Jacobs in his essay on the dubunkers), but it should become obvious to readers of this conference sponsored by Fate magazine that ufology contains much internal skepticism of a constructive rather than destructive kind. Recommended highly and a great bargain at today's book prices.
- Gauld, Alan, and A.D. Cornell, Poltergeists. Boston: Routledge & Kegan Paul, 1979. \$20.00. A major survey indispensable for any serious scholar interested in the topic of these mischievous spirits. Includes a computer analysis of 500 cases. The chapter on "explaining away the poltergeist" should be required readings for skeptics who too frequently regard poltergeist investigators of such phenomena as gullible and uncritical.

- Gillespie, L. Kay, Cancer Quackery: The Label of Quack and Its Relationship to Deviant Behavior. Palo Alto, Cal.: R&E Research Associates (936 Industrial Ave.; Palo Alto, CA 94303), 1979. 130pp. \$9.00 paperback. Originally a doctoral dissertation, this is one of the few works to consider the labelling perspective in modern sociological deviance theory in relation to medical "quackery." A very useful work and highly recommended.
- Goran, Morris, Fact, Fraud, and Fantasy: The Occult and Pseudosciences. South Brunswick and N.Y.: A.S. Barnes, 1979. 189 pp. \$8.95. An author of many physical science texts turns to consideration of the esoteric "sciences." Generally debunking but better informed and more responsible than many such works. Recommended.
- Goran, Morris, The Modern Myth: Ancient Astronauts and UFOs. South Brunswick and N.Y.: A.S. Barnes and Co., 1978. 192pp. \$8.95. One of the better debunking books, better for its first section on ancient astronaut claims than in the second UFO section. Goran has done his homework better than most critics, and the book is an important critical work that deserves more attention than it has thus far received.
- Gowan, John Curtis, Operations of Increasing Order, and Other Essays on Exotic Factors of Intellect, Unusual Powers & Abilities, Etc. (as Found in Psychic Science). Privately printed by the author (1426 Southwind Circle; Westlake Village, CA 91361), 1980. \$5.00 paperback. A well documented scholarly treatise which may ignore the critical literature too frequently but draws upon many excellent and often obscure sources worth knowing about. A valuable attempt at synthesis deserving of attention.
- Haines, Richard F., Observing UFOs. Chicago: Nelson-Hall, 1980. 300pp. \$21.50. An important new guidebook for the scientific observation of UFOs by a leading psychologist in ufology. Not centrally concerned with the nature of UFOs so much as with obtaining reliable information about them from witness interrogation procedures, etc. A must for any serious UFO investigator.
- Hanna, David, Cults in America. N.Y.: Belmont Tower Books, 1979. 283pp. \$2.25 paperback. A popular and rather sensationalist survey of contemporary "cultic" activity from Jonestown to Hollywood. Unsuccessfully tries to tie together everything from sado-masochist clubs and devil worship to the Moonies and deprogramming.
- Hansel, C.E.M., ESP and Parapsychology: A Critical Re-Evaluation. Buffalo, N.Y.: Prometheus Books, 1980. 325pp. \$15.95. Essentially a new edition of Hansel's earlier study with some new material including critical chapters on remote viewing and the experiments of Helmut Schmidt. Unfortunately, the new edition suffers from the same problems as the old one with Hansel taking little note of the many criticisms made. The book still contains the virtues of the older edition, but the new material is somewhat disappointing and open to counter-criticism. Still an important book but not what many responsible skeptics might have hoped for.

- Hunt, Inez, and Wanetta W. Draper, Lightning in His Hand: The Life Story of Nikola Tesla. Hawthorne, Cal.: Omni Publications, 1977. 269pp. \$8.95. A good biography and introduction to the life and work of Tesla who has recently been emerging as a major 'cultic' figure among those interested in psychotronics.
- Jones, Russell A., Self Fulfilling Prophecies: Social, Psychological and Physiological Effects of Expectancies. Hillsdale, N.J.: Lawrence Erlbaum Associates, 1977. 275pp. \$15.95. A thorough review of the literature dealing with self-altering prophecies. Excellent and highly recommended since the implications of such effects are to be found throughout the area of paranormal investigations, from Clever Hans to UFOs.
- Joy, W. Brugh, Joy's Way: A Map for the Transformational Journey, An Introduction to the Potentials for Healing with Body Energies. Los Angeles, Cal.: J.P. Tarcher, 1979. 290pp. \$6.96 paperback. A remarkable volume by a one-time orthodox physician who has been deeply influenced by Eastern and holistic thought and now believes in the healing potential of "body energies."
- Knight, David, UFOs: A Pictorial History from Antiquity to the Present. N.Y.: McGraw-Hill, 1979. 192pp. \$12.95. A large compendium of black-and-white photographs and illustrations of UFOs, their purported inhabitants and human contacts, and some ufologists.
- Leeds, Morton, and Gardner Murphy, The Paranormal and the Normal: A Historical, Philosophical and Theoretical Perspective. Metuchen, N.J.: Scarecrow Press, 1980. 265pp. \$13.50. An important new integrative monograph. Particularly significant since it represents the late Gardner Murphy's last work. Highly recommended.
- Lenz, Frederick, Life-Times: True Accounts of Reincarnation. Indianapolis, Ind.: Bobbs-Merrill, 1979. 205pp. \$10.00. A series of non-hypnotic case studies remarkably lacking in external validation. Interesting but scientifically useless for the skeptic.
- Lombard, Eric, ed., By Lust Possessed. N.Y.: New American Library, 1980. 218pp. \$1.95 paperback. A fascinating collection of case histories of alleged interactions with demon lovers. For the sexy psychic looking for chills as well as thrills.
- Maffei, Paolo, Monsters in the Sky. Cambridge, Mass.: MIT Press, 1980. 342pp. \$15.00. A very lucid presentation on the accepted anomalies in astronomy, from comets to black holes. UFOs are excluded on the grounds that they really are, by comparison, not monstrous enough!
- Mehta, Gita, Karma Cola: Marketing the Mystic East. N.Y.: Simon and Schuster, 1979. 201pp. \$8.95. A sobering and funny account of the flocking of Westerners into India seeking "karma" and how they looked to the natives there who knew better.

- Marks, David, and Richard Kammann, The Psychology of the Psychic. Buffalo, N.Y.: Prometheus Books, 1980. 232pp. \$15.95. Two psychologists examine the evidence for the psychic abilities of Kreskin, Uri Geller, and those tested for remote viewing at Stanford Research Institute. Much of value but the section on Kreskin who only the general public might mistake for psychic is a bit like debunking Houdini for those who insist he really dematerialized in his escapes. Valuable new material on Geller and on SRI work, but remarkably unacquainted with the general literature and history of psi research and the debunking commentary and title of the book are grandly overgeneralized.
- Melton, J. Gordon, The Encyclopedia of American Religions. 2 Volumes. Wilmington, N.C.: McGrath Publishing Co., 1978. 608+xxxvi and 595+xxvi pp. \$87.50. An extraordinary compendium of information about nearly all the religious groups in the United States including most cults and exotic varieties. A major contribution that no library should be without and strongly recommended for personal libraries of the serious researcher despite the cost of the volumes. Dr. Melton has received much notice and praise for this masterful project and he well deserves it. Much amazing material even for those of us who might have thought we were pretty well familiar with the spectrum of thought that has been institutionalized. Everything you probably wanted to know except the addresses, and those can be found in Melton's A Dictionary of Religious Bodies in the United States.
- Morely, Peter, and Roy Wallis, eds., Culture and Curing: Anthropological Perspectives on Traditional Medical Beliefs and Practices. Philadelphia, Pa.: University of Pittsburgh Press, 1978. 190pp. \$14.95. A very nice collection of papers covering topics from healing in Mexico by spiritualists to honey-and-vinegar cures in Vermont. Recommended.
- Moss, Thelma, The Body Electric: A Personal Journey into the Mysteries of Parapsychological Research, Bioenergy, and Kirlian Photography. Los Angeles, Cal.: J.P. Tarcher, 1980. 223pp. \$11.95. Autobiographical account of Dr. Moss's adventures in psychical research. Well written but also scientifically very unconvincing and suggestive of great credulity and lack of rigor in investigation. An interesting historical document but of little evidential value.
- Rigby, Andrew, Alternative Realities: A Study of Communes and Their Members. Boston: Routledge & Kegan Paul, 1974. 341pp. \$18.95. An important sociological study of British communes, particularly concerned with the viability of alternative institutions to the nuclear family. Some materials on mysticism of interest to ZS readers.
- Rogo, D. Scott, and Jerome Clark, Earth's Secret Inhabitants. N.Y.: Grosset & Dunlap Tempo Books, 1979. 213pp. \$1.95 paperback. A wild collection of Fortean goodies from the Mad Gasser of Mattoon, UFOs, Sasquatches and the better known anomalies to sighted Bat-Men, Kangaroos and Trolls. Hardly a scientific work but great fun and generally well documented.
- Rosenthal, Robert, et al., Sensitivity to Nonverbal Communication. The PONS Test. Baltimore, Md.: Johns Hopkins University Press, 1979. 407+xxi pp.

- \$20.00. A description of the Profile of Nonverbal Sensitivity Test developed by Rosenthal and his associates and the research using it. Very significant work of relevance to ascertaining the mechanisms involved in experimenter effects as well as better understanding non-verbal communication.
- Ross, Helen, Behavior and Perception in Strange Environments. N.Y.: Basic Books, 1975. 171pp. \$12.95. A pioneering book in environmental psychology that deals with error in visual and auditory space perception and of great value to anyone interesting in understanding reports of anomalies. Highly recommended.
- Rovin, Jeff, The Fantasy Almanac. N.Y.: E.P. Dutton, 1979. 313pp. \$9.95. An encyclopedic dictionary of fantasy and science-fiction people and creations, from Abrasax to Zothique. Very useful and well done with many folklore items included.
- Russell, Jeffrey B., A History of Witchcraft: Sorcerers, Heretics, and Pagans. N.Y.: Thames and Hudson, 1980. 192pp. \$15.95. A lavishly illustrated and generally well done popular introduction with some particularly good material on contemporary witchcraft. Recommended.
- Scheff, Thomas J., Catharsis in Healing, Ritual and Drama. Berkeley, Cal.: University of California Press, 1980. 262pp. \$12.95 clothbound; \$3.95 paperback. A phenomenological analysis of catharsis suggesting the role of distancing in catharsis. Relevant to issues of unorthodox healing and possibly even to out-of-body experiences.
- Schindler, George, Ventriloquism: Magic with Your Voice. N.Y.: David McKay, 1979. 149pp. \$8.95. An excellent instruction manual written mainly for the young reader. Ventriloquism has been suggested as an alternative explanation for so-called voice phenomena by mediums and is also an important means of communicating whispers while seeming to remain silent. As with conjuring methods, psychic investigators should be more familiar with ventriloquist methods than most are.
- Schwarz, Jack, Human Energy Systems. N.Y.: E.P. Dutton, 1980. 176pp. \$6.95. Metaphysical approach by a Dutch "sensitive" giving lessons and exercises for spiritual development and good health. Better than average packaging of guru-wisdom (bridging things like biofeedback with herbs and the tarot) but of scientific disinterest.
- Scott, A.F., Witch, Spirit, Devil. London: White Lion Publishers, 1974. 189pp. 3.75 pounds. An interesting little anthology of various odds and ends about witchcraft yesterday and today. Numerous out-of-the-way good items with generally good scholarship and high readability. One of the better popular books on witchcraft.
- Scott, Gini Graham, Cult and Countercult: A Study of a Spiritual Growth Group and a Witchcraft Order. Westport, Conn.: Greenwood Press, 1980. \$19.95. A fine ethnographic report comparing two very different contemporary occult groups. Informative, analytical, highly readable, and recommended.

- Sebeok, Thomas A., and Jean Umiker-Sebeok, eds., Speaking of Aoes: A Critical Anthology of Two-Way Communication with Man. N.Y.: Plenum Press, 1980. 480pp. \$37.50. An exception, though over-priced, anthology of special interest to ZS readers. The Sebeoks are strong skeptics as regards the use of true language by apes, but this collection presents a good cross-section of the work in this area, and should prove indispensable to any interested in these issues. Unfortunately, little attention is given to the critics of Pfungst and the other debunkers of so-called "talking" animals, particularly as documented in the literature of psychic research (e.g., the Elberfeld horses, Rolf of Mannheim, etc.). The arguments found in this volume sharply parallel those between critics and advocates of paranormal communication in psi research. Highly recommended.
- Simon, ed., The Necronmicon. N.Y.: Avon, 1980. 220pp. \$2.75 paperback. A rather silly book, largely derivative of Crowley, purporting to be the great lost book of magic created by H.P. Lovecraft. Far less sophisticated than the edition put out by George Hay for Neville Spearman publishers in 1978, and seemingly meant not as a joke but a trick on the gullible market.
- Smullyan, Raymond, This Book Needs No Title: A Budget of Living Paradoxes. Englewood Cliffs, N.J.: Prentice-Hall, 1980. 185pp. \$9.95. Described as a collection of "mind-stretching paradoxes, logical labyrinths and intriguing conundrums," this sequel to philosopher Smullyan's What Is the Name of This Book? presents approximately 80 paradoxical parables, some of which live up to the billing but many of which I found rather flat and more cute than profound. Fun to read but not recommended as highly as the earlier collection of logical puzzlers.
- Spiedlman, Ed., The Mighty Atom: The Life and Times of Joseph L. Greenstein. N.Y.: Viking Press, 1979. 237pp. \$10.95. A remarkable biography of a "superhuman," a 5 ft. 4 in. man who, pound for pound, was probably the strongest human being who ever lived. Vividly demonstrates the capabilities of the human body and will that borders on the paranormal since many of Greenstein's feats seem scientifically quite impossible. Highly recommended.
- St. Clair, David, David St. Clair's Lessons in Instant ESP. Englewood Cliffs, N.J.: Prentice-Hall, 1978. 198pp. \$7.95. Another of the many how-to-develop-your-psychic-powers books for what seems to be an insatiable market. Scientifically worthless.
- Stroman, Duane F., The Quick Knife: Unnecessary Surgery U.S.A. Port Washington, N.Y.: Kennikat Press, 1979. 192pp. \$12.50. An important critical study in the sociology of medicine. Should be read by all those who are quick to criticize unorthodox medicine as potentially harmful. Highly recommended.
- Sullivan, Walter, Black Holes: The Edge of Space, The Edge of Time. Garden City, N.Y.: Doubleday Anchor Press, 1979. 304pp. \$17.95. Everything you always wanted to know about black holes but were afraid to ask. Lucidly written and well illustrated.
- Tart, Charles T., Harold E. Puthoff and Russell Targ, eds., Mind at Large:

Institute of Electrical and Electronic Engineers Symposia on the Nature of Extrasensory Perception. N.Y.: Praeger, 1979. \$21.95. The important sequel to Targ and Puthoff's Mind Reach. Much valuable material including replications, analysis and information on Soviet researches. Though very much a one-sided presentation that will leave critics dissatisfied, the papers here are actually great improvements over the popular presentation in Mind Reach and deserve serious attention.

Taylor, John, Science and the Supernatural. N.Y.: E.P. Dutton, 1980. 180pp. \$10.95. Dr. Taylor's account of how he changed his mind about the reality of paranormal phenomena and returned to being a skeptic after his much publicized work described in his book Superminds. Unfortunately, his reasons for conversion both times seem based on rather poor reasons. Whereas he earlier accepted the reality of psi forces on the grounds of bad evidence that was congruent with his abstract theories, he now rejects that evidence because he has rejected those theories. We may be more hospitable to claimed anomaly X if it is supportive of our theory Q, but abandoning theory Q does not really change the status of the evidence for X.

van Gelder, Dora, The Real World of Fairies. Wheaton, Ill.: Theosophical Publishing House, 1977. 120pp. \$3.25 paperback. A reprint of the 1904 classic narrative, probably best read as an enchanting tale though presented as non-fiction. A delightful work of imagination valuable as fiction if not fact.

Vaughn, Alan, Incredible Coincidence: The Baffling World of Synchronicity. N.Y.: J.B. Lippincott, 1979. 256pp. \$10.00. Mainly 152 case studies of remarkable coincidence. Vaughn adds little to our philosophical understanding of the concept of synchronicity, but the cases are fascinating and highly suggestive.

Walker, Benjamin, The Encyclopedia of the Occult, the Esoteric and the Supernatural. N.Y.: Stein & Day, 1980. 343pp. \$7.95 paperback. Published in hardcover as Man and the Beasts Within in 1977, the paperback title is rather misleading since the book remains a compendium about unorthodox beliefs about the human body with sections under such titles as "buttocks," "nose," and "sweat" rather than what most readers would expect from the book's label. Lots of interesting content, but mostly irrelevant to matters paranormal despite some fascinating sections on such matters as "ideodynamics," the "perineum," and "xenophobia."

Webb, James, The Harmonious Circle: The Lives and Work of G.I. Gurdjieff, P.D. Ouspensky and Their Followers. N.Y.: G.P. Putnam's Sons, 1980. 608pp. \$19.95. An extraordinary analytical and biographical work full of insight and criticism of a movement that has largely been shrouded in mystery. A very welcome book that is bound to be greeted with some outrage by Gurdjieffians. Highly recommended.

Wentz, W.Y. Evans, The Fairy Faith in Celtic Countries. Atlantic Highlands, N.J.: Humanities Press, 1978. 524pp. \$11.25 paperback. A most welcome reprinting of the 1911 study with a new introduction by Kathleen Raine. Recommended,

Wolff, Keith M., ed., Regulation of Scientific Inquiry: Societal Concern with Research. Boulder, Col.: Westview Press, 1979. 222pp. \$17.00. AAAS Selected Symposium #37. Though not concerned with matters paranormal, this series of essays concerns the social control of science and is therefore relevant to ZS concerns. Many excellent papers dealing with the sociology of science, ethics in science, and presumptions and secrecy in science.

Yates, Frances A., The Occult Philosophy in the Elizabethan Age. Boston: Routledge & Kegan Paul, 1979. 217pp. \$17.50. Typical of Dame Yates fine historical work, this volume centers on the history of the "Christian Cabala" as reflected not only in the magical writings of those like Gornelius Agrippa and John Dee but in the writings of those like Marlowe, Spencer and Shakespeare. An excellent addition to the scholarly literature building upon Dr. Yate's earlier studies.

Zipes, Jack, Breaking the Magic Spell: Radical Theories of Folk & Fairy Tales. London: Heinemann, 1979. 201pp. 3.5 pounds. Six essays on the evolution of fairy tales, their uses in literature and popular culture and the politics behind them, by a professor of German and Comparative Literature. Fascinating analysis and a particularly interesting attack on Bettelheim's Freudian analysis.

-- M.T.



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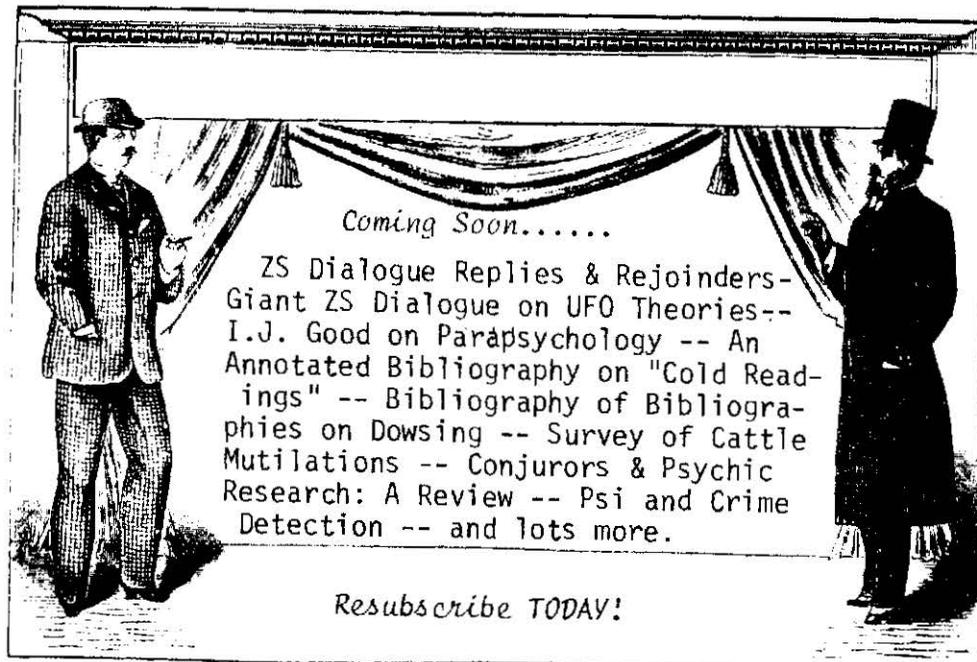
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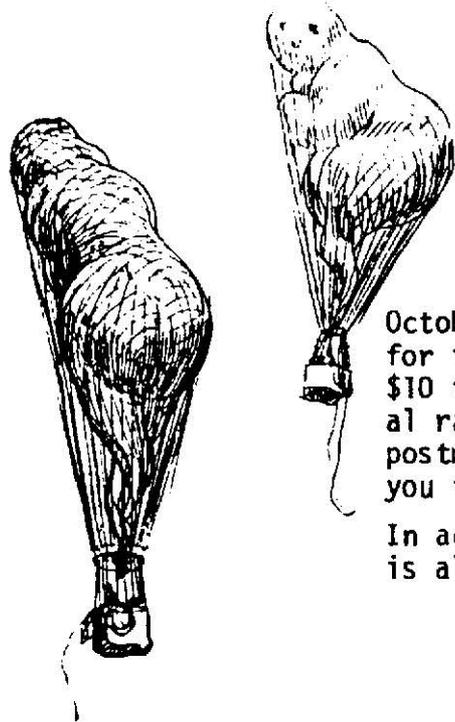
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