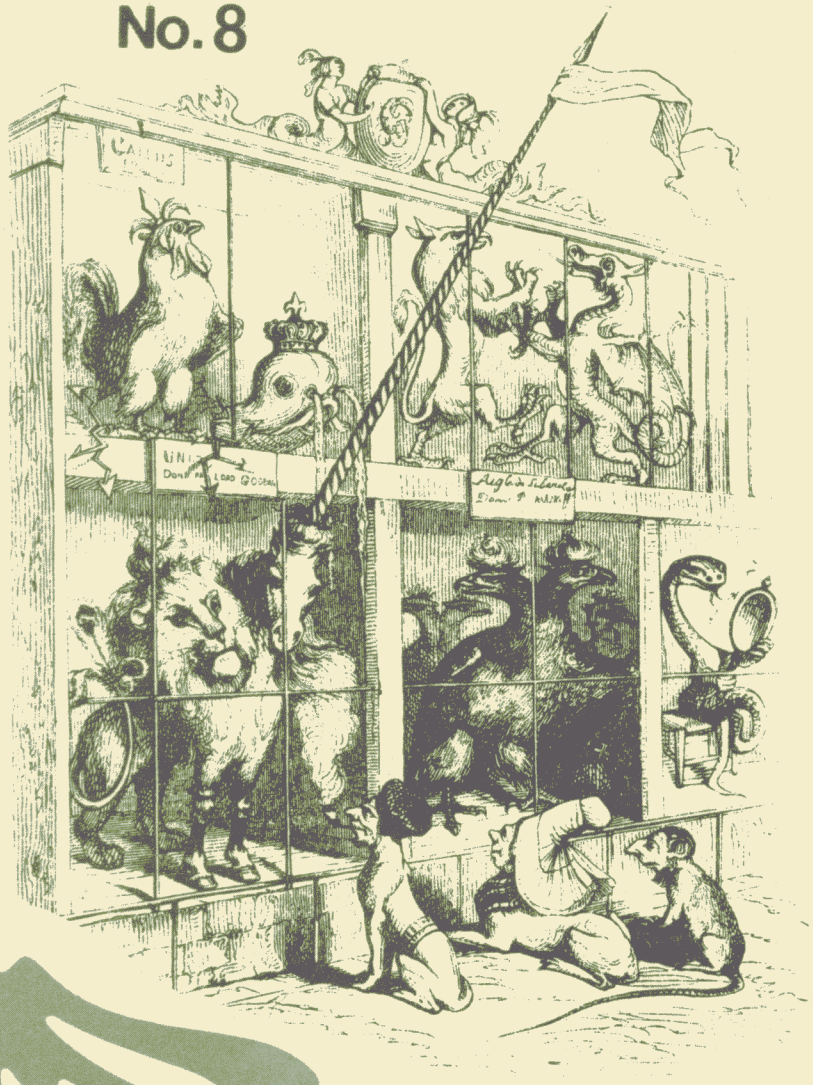
Zetetic scholar

An Independent Scientific Review of Claims of Anomalies and the Paranormal

No. 8



THE SIRIUS MYSTERY

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PARAPSYCHOLOGY

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UFO SIGHTINGS

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PSYCHIC SURGERY

& MORE!

1981

Zetetic Scholar

AN INDEPENDENT SCIENTIFIC REVIEW OF CLAIMS OF ANOMALIES AND THE PARANORMAL

THE JOURNAL OF THE CENTER FOR SCIENTIFIC ANOMALIES RESEARCH (CSAR)

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Zetetic Scholar

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CONTINUING ZS DIALOGUES

REGULAR FEATURES

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ERRATA

The following corrections should be made re I.J. Good's "Scientific Speculations on the Paranormal and the Parasciences" in ZS #7:

Page 11, line 9: For 0.00000000000000000, correct to p.000000000000000001.

Page 24, line 18 up: For $f(x_0,x_{t,1})$, correct to $f(x_0,y_1(y_0,x_{t,1}))$.

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In David W, Swift's "Comments" on J. Richard Greenwell's article, on page 99 of ZS #7, on line 6 up, the word "psychics" should be corrected to read "physics."

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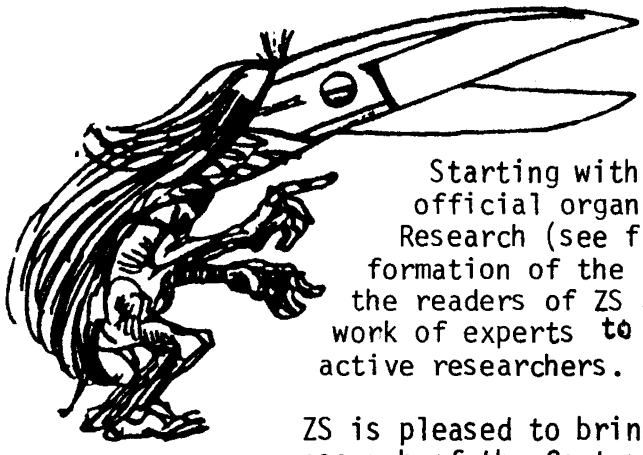
In the "Random Bibliography" in ZS #7, page 108, the reference to the article by Robert Jastrow given as being in Science News should ve corrected to Science Digest.

JAMES W. MOSELEY's name was misspelled as "Mosley." Mea culpa.



EDITORIAL

Starting with this issue, ZETETIC SCHOLAR becomes the official organ of the Center for Scientific Anomalies Research (see full announcement in this issue). The formation of the center is largely a response to many of the readers of ZS asking for us to institutionalize a network of experts to allow greater communication between active researchers.

ZS is pleased to bring you the first two reports of ongoing research of the Center in this issue. The first is from our large scale study of the use of alleged psychics by law enforcement agencies (the Psychic Sleuths Project), and the second is from an analysis of engineers' reports of UFO encounters (the Anomaly Project). Further reports will be issued from both these projects by the Center, and some of these will be published in future issues of ZS.

Involvement with the Psychic Sleuths Project and other involvements with what some have called applied parapsychology has made certain issues salient. Critics have decried the use of the term applied parapsychology on the grounds that you can not build up applications of a science until that science and its alleged variables have been established. You can not apply psi unless psi exists, and an applied pseudoscience would be worse than a plain pseudoscience since that would be like selling snake oil remedies. I think that most responsible parapsychologists would agree with such critics that an applied parapsychology would now be at least premature. Yet, there seems little doubt that the claims of those like dowsers, psychic detectives, psychic counselors, and psychic healers are frequently quite impressive (at least on the surface), and many people find these practices useful. In fact, such "applied" areas often generate a great deal more excitement in practical terms than do dull guessing experiments in laboratories.

I would suggest that we might wish to distinguish between Experimental and Clinical Parapsychology, much as we distinguish between Experimental and Clinical Psychology. The criteria for evaluating clinical efforts is far broader than the purely scientific criteria found in experimental methods. The criterion of effectiveness plays a major role in the evaluation of clinical methods. Thus, whether or not the theory is correct or not may be secondary to the pragmatic consideration of whether or not the patient/client is helped by the procedures. When dealing with clinical matters, an element of art as well as science is typically involved, and importance is often weighed not in scientific but in human terms. So, questions like "Was the patient's condition improved?" or "Did you find the missing object?" become more significant than the validity of the theory behind the method used to get the positive result.

In normal psychology, we are usually concerned with what statisticians term a Type I error. This error would consist of mistakenly thinking that something special was going on when it was not. But there is also a Type II error. This consists of mistakenly thinking that nothing

special is going on when actually something rare (a small signal amidst much noise) is happening. Since most psychologists are concerned not to think psi is operating when merely normal perceptual processes are producing the results, they usually concentrate (quite properly) on not making such a Type I error. But for many parapsychologists, the existence of psi is believed to be so important that they don't want to make the error of ignoring it if it really is present in the world. They, then, are particularly concerned about not making a Type II error. Concern with a Type II error is most common where extra-scientific factors make the existence of a rare variable important. In medical research, there is commonly interest in avoiding a Type II error because the outcome may be a matter of life and death, and we don't want to overlook something that may be hard to find but terribly important.

Other extra-scientific factors may make things important. For example, in the case of military interest in psi research, the existence of psi might have extremely important military-political consequences should an enemy be able to use it to break through national security defenses. Thus, even if the chances are small that psi really exists, it is quite rational to want to avoid a Type II error and investigate this area. It is simply too important to neglect. The same is true in cases where people have exhausted all normal remedies. If you desperately need water and the geologists tell you there is none on your land, or if the doctors tell you you are doomed, it is not irrational for you to pursue the use of a dowser or a form of alternative medicine on even the slim chance that something positive might come of it. If we recognize that the probability of success is very low but have no orthodox alternative, and if the costs in trying an unorthodox method are reasonable, I would suggest that it is only rational to give the unorthodox method a try if the need for success is great. But the degree of need and the concern with importance that determines our desire to avoid a Type II error is typically extra-scientific. It may be unscientific for law enforcement officers to try the use of an alleged psychic in solving a dead-end case, and it may be unscientific for the Pentagon to spend money on psi research (at least in terms of Type I considerations), but that does not make it irrational to do so. Quite the contrary. The old adage "any port in a storm" still makes sense. We must remember that even placebos work.

None of this is to say that we should not be cautious in our uses and evaluation of unorthodox methods. The world remains full of charlatans and frauds ready to con the unwary. And many claims of effectiveness may actually prove to be invalid. But we must discriminate purely scientific from the broader notion of rational pursuits. Thus, one can agree with the psychologists who consider the evidence for psi to be unconvincing but still support the use of public funds (at a moderate level) by the military to conduct research into psi and similar areas where even unlikely matters might have great potential consequences. It would be premature to routinely involve psychics in crime investigations, but it would also be premature to call those police who try them on long-shots irrational. The rational person who has been pronounced doomed by his doctors should not foreclose all unorthodox methods seeking to heal just because some would label such pursuits "magical" or without scientific credibility. In the meanwhile, we might learn much from looking at the evaluation procedures used in clinical psychology to measure effectiveness and apply these to clinical efforts in parapsychology. We may be surprised to find that fortune-tellers don't do that much worse than psychiatrists.

LETTERS

In Zetetic Scholar #7 Jon Beckjord claims that I have admitted in print, in Saucer News, July 1980, that I was able to see the supposed Sasquatch in his indistinct and ambiguous photos. Let me repeat exactly what I said there:

"While it is true that I did indeed see the Sasquatch in his interdimensional photograph, I was badly in error. I saw only one such critter. Then Jon informed me that it was not a single creature, as I envisioned in my myopia, but an entire family of Bigfeet: a Mama Bigfoot, a Papa Bigfoot, and two baby Bigfeet. Now, my eyesight is pretty good for a CSICOPPER, but nowhere near as good as his. Even after he had pointed out this family portrait to me, my closed mind was still unable to see more than black blob".

I should point out that despite my attempt at humor, the above statement is factually quite correct: Beckjord claims to see an entire family of Bigfeet, while I saw just a black blob. If he cites the above statement as an endorsement, I would hate to see some of the more negative comments he has received.

-- ROBERT SHEAFFER

******

Morris Goran's review of Ronald Story's Guardians of the Universe? (ZS#7) makes painfully clear why ufologists have long despaired of any constructive dialogue with hard-core debunkers. Trying to explain how a disbeliever in ancient astronauts like Story can take UFOs seriously, Goran asserts that ufologists do not seek "so much to expose a pseudoscience" as to "push aside a competitor." Using a common debunking practice, he damns ufologists if they do and damns them if they don't. If they accept Erich von Däniken's "theories" (as apparently Goran would prefer that they do), they are credulous fools; if they reject them, they are venal opportunists.

The simple fact of the matter is that ufologists have dismissed von Däniken's ideas not because they feel threatened by them but because the case for ancient astronauts is weak in ways that the case for UFOs is not. Even Goran's fellow skeptic Daniel Cohen recognized the difference. As Cohen writes in The World of UFOs (1978), "There is room for disagreement on the subject of UFOs. There is no room for disagreement of the subject of von Daniken's theories. Most of the evidence that he presents is either misinterpreted or just plain wrong."

The problem with Goran-style debunkers is that they continue to define issues as they wish they were rather than as they are. No wonder the debunking movement has failed so abjectly to rid the world of heresy.

-- JEROME CLARK

I was puzzled by Morris Goran's review of Ron Story's Guardians of the Universe? Over half of his review was dedicated to UFOs, represented by only a brief, 13-page chapter in Story's book. Dr. Goran apparently assigns equal probability to all sorts of claimed anomalies -- assuming at the same time that they are all equally invalid. This is a trap that all undiscriminating debunkers fall into (Ron Story being a "discriminating debunker").

Rather than accusing Story of wanting to "push aside" a competitor (ancient astronauts), Dr. Goran, as a scientist, should have thanked Mr. Story for being more discriminating and for evaluating each anomoly independently on its own merits. As the distinction between UFO reports and the concept -- as I prefer to call it -- of ancient astronauts appears to be problematical for Dr. Goran, perhaps it is time to address this question.

The ancient astronaut concept is based on particular interpretations of certain archaeological artifacts in various parts of the world (artifacts is here used in the broad sense, and includes large structures and monuments). Most of the examples used by ancient astronaut supporters have perfectly normal explanations. Some do not, but that is probably because of our knowledge gaps in the archaeological record. What the ancient astronaut supporters are doing is interpreting artifacts differently than the way they are being interpreted by professional archaeologists. The former are more likely to interpret artifacts out of context. The latter, acquainted better with cultural chronologies and artifact sequencing, are able to better place artifacts in their proper contexts.

It is important to note that there is no dispute over the existence of these artifacts. Rather, the dispute concerns the method and purpose behind their construction. With UFO reports we have a totally different situation. The dispute concerns whether there are, in fact, any "artifacts" at all (that is, any real unidentified -- or perhaps unidentifiable -- lights or structures). A UFO observer can report a direct visual sensation and perception of a supposed anomalous object. His report is derived from an immediate biophysical response to a stimulus -- it does not have to rely on nebulous interpretations of four or five thousand year old artifacts.

To lump the speculative concept of ancient astronauts with reports based on biophysical responses is like mixing apples and crocodile eggs. Surely Dr. Goran does not apply this methodology to physics problems in his laboratory. Why does he expect us to accept such a procedure in this case?

Before concluding, I wish to clarify one point: I do not necessarily mean to imply that the evidence for UFOs as extra-terrestrial spacecraft is conclusive -- or even compelling -- while the evidence for ancient astronauts is not. My point is that it is different, and this difference demands a different evaluative approach.

Incidentally, if Dr. Goran liked Guardians of the Universe?, he'll love UFOs and the Limits of Science, our latest work.

-- J. RICHARD GREENWELL

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My ideas have evolved since I commented on J. Richard Greenwell's article in Zetetic Scholar #7.

First, I take for granted that no information of any sort has been gathered about UFOs since people have tried to "study" them. Does one know anything about them? I think not.

Supposing that UFOs are intelligent, this complete lack of results is (I think) sufficient evidence that the phenomenon does not want to be known, and that it perfectly succeeds in achieving that. If it is not intelligent, that means that the whole collection of "good" cases is false and must be rejected.

Second, in regard to the Extra-Terrestrial Hypothesis (be it relevant or not), we have to face a new fact.

Within a short lapse of time (some dozen years), we Earthmen shall be able to send in to the heavens such signals that if they were now sent from space by someone else, we would already have detected them through interferometric techniques. In other words, the complete failure of SETI is very strong evidence that nobody in the heavens is sending us such signals as we firmly intend to send ourselves as soon as we can, that is, within a few dozen years.

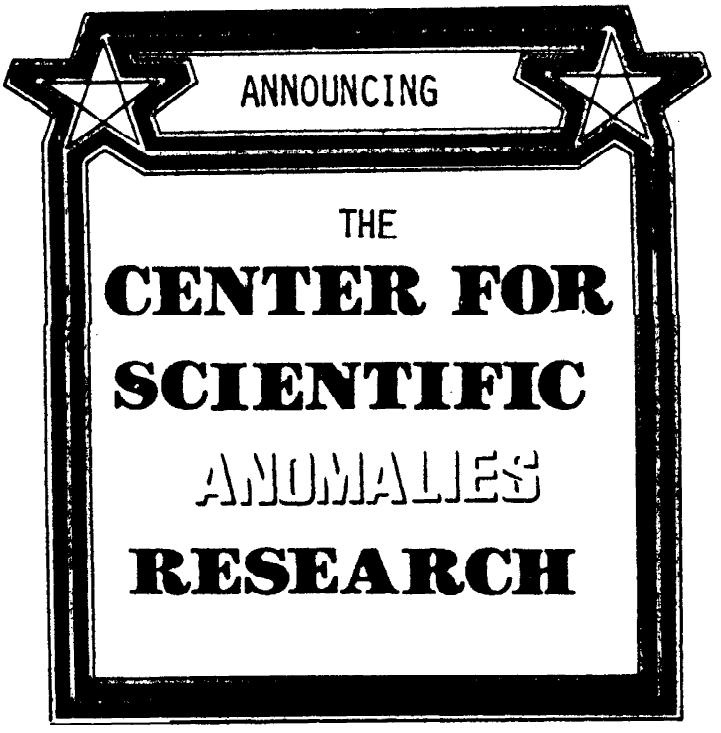
The fashionable hypothesis deduced by most astronomers, if not by all, from the failure of SETI, is that "we are alone in the heavens."

Since it is unlikely that life is a unique phenomenon in the universe, we are left with only very few explanations for the heavens being silent. In fact, I can see only one: Every civilization reaching the level of being able to send signals into the heavens (every one, without any exceptions), is prevented from doing so by something that for the present we cannot imagine.

Does every civilization reaching that level commit suicide (without any exceptions)? (I insist without any exceptions because if there is one, that would be sufficient for it to be some millions of years ahead of us in spreading through the galaxy so that the heavens should be full of signals, which is not the case.)

Or will the contact take place at that very moment when we send our signals, that is, within a few dozen years? Or does science at that same moment in every case discover new means of communication that we are not now aware of? In any these three cases, the next dozen years are of paramount importance.

-- AIME MICHEL



I am pleased to announce the formation of a new private Center for Scientific Anomalies Research (CSAR) which will bring together scholars and researchers concerned with furthering responsible scientific inquiry into and evaluation of claims of anomalies and the paranormal. The Center will:

* Advance the interdisciplinary scientific study of alleged and verified anomalies.
* Act as a clearinghouse for scientific anomaly research.
* Publish a journal (ZETETIC SCHOLAR), a newsletter (THE CSAR BULLETIN), research reports, and bibliographies.
* Promote dissemination of information about scientific anomaly research.
* Create a public network of experts on anomaly research through publication of a CSAR DIRECTORY OF CONSULTANTS.
* Sponsor conferences, lectures and symposia related to anomaly research.
* Promote improved communication between critics and proponents of scientific anomalies.

In addition to the Director of CSAR, Dr. Marcello Truzzi, and its Associate Director, Dr. Ronald Westrum, both sociologists at Eastern Michigan University, CSAR is sponsored by a group of distinguished scientists who have agreed to act as its Senior Consultants. These thus far include:

Prof. George O. Abell (*Dept. of Astronomy; University of California, Los Angeles*),
Dr. Theodore X. Barber (*Cushing Hospital; Massachusetts Dept. of Health*),
Prof. Daryl J. Bem (*Dept. of Psychology; Cornell University*),
Prof. Mario Bunge (*Foundations & Philosophy of Science; McGill University*),
Dr. Persi Diaconis (*Dept. of Statistics; Stanford University*)
Dr. Eric J. Dingwall (*East Sussex, England*),

Prof. Gerald L. Eberlein (*Instituts für Socialwissenschaften; Technischen Universität München*),
Prof. Hans J. Eysenck (*Institute of Psychiatry; University of London*),
Prof. Paul Feyerabend (*Dept. of Philosophy; University of California, Berkeley*),
Prof. I.J. Good (*Dept. of Statistics; Virginia Polytechnic Institute and State University*),
Prof. Morris Goran (*Dept. of Physical Science; Roosevelt University*),
Dr. Bernard Heuvelmans (*Centre de Cryptozoologie; Le Bugue, France*),
Prof. Ray Hyman (*Dept. of Psychology; University of Oregon*),
Prof. J. Allen Hynek (*Dept. of Astronomy; Northwestern University*),
Dean Robert G. Jahn (*School of Engineering/Applied Science; Princeton University*),
Dr. John Palmer (*Dept. of Parapsychology; John F. Kennedy University*),
Prof. Robert Rosenthal (*Dept. of Psychology & Social Relations; Harvard University*),
Prof. Thomas A. Sebeok (*Research Center for Language and Semiotic Studies; Indiana University*),
Prof. Peter A. Sturrock (*Institute for Plasma Research; Stanford University*), and
Prof. Roy Wallis (*Dept. of Social Studies; The Queen's University of Belfast*).

In addition to this board of Senior (Science) Consultants, CSAR is also sponsored by a board of Senior Resource Consultants consisting of persons recognized for their special knowledge and informational skills in relation to bibliographic and archival resources. Thus far, the Senior Resource Consultants include:

Mr. William R. Corliss (*The Sourcebook Project*),
Mr. Peter Haining (*author-editor*),
Mr. Michael Harrison (*author-editor*),
Mr. Robert Lund (*American Museum of Magic*),
Dr. J. Gordon Melton (*Institute for the Study of American Religion*),
Mr. Robert J.M. Rickard (*The Fortean Times*),
Mr. Leslie Shepard (*author-editor*), and
Ms. Rhea A. White (*Parapsychology Sources of Information Center*).

The primary focus of the Center will be on the study and evaluation of bodies of anomalous observations rather than upon esoteric theories seeking to explain already known phenomena. The orientation of the Center is exclusively scientific, places the burden of proof on the claimant, and recognizes the need for a degree of proof commensurate with the extraordinary character of the phenomenon claimed. But the Center also wishes to promote open and fair-minded inquiry that will be *constructively skeptical*. We recognize that scientific anomalies, where valid, may be instruments and driving forces for reconceptualization and growth in scientific theory. Critically and constructively approached, legitimate anomalies should be welcomed by science rather than perceived as ill-fitting nuisances. History clearly demonstrates that tomorrow's science is likely to contain surprises, and tomorrow's theories are likely to explain *some* of what are today viewed as controversial anomalies. Also,

tomorrow's explanatory theories may be in areas of science not now perceived as relevant to the anomalies being considered. Thus, "anomalistics" must necessarily be an interdisciplinary endeavor.

ON THE ORGANIZATION OF CSAR

CSAR is still in its formative stage. Those associated with CSAR may be Consultants and/or Members. Consultants will be persons with demonstrated expertise in some area of anomaly research. Consultants will vary widely in their opinions, and need not necessarily become Members of CSAR. Persons who apply for Consultant status and are approved for inclusion, will be listed in the CSAR DIRECTORY OF CONSULTANTS. The object is simply to produce a network of experts whom those interested in anomaly research might contact. Consultants will be of two types: Research Consultants and Resource Consultants. Research Consultants will normally be professional scientists with advanced training in research. Resource Consultants will include others with expertise about anomalies, such as philosophers, historians, science writers, conjurors, or even professional psychics. The key criterion for designating someone a Research Consultant will be evidence of expertise in research and methodology. The key criterion for designating someone a Resource Consultant will be evidence of special information or knowledge that might be useful to to others doing scientific research into anomalies. Since these Consultants may differ both among themselves and with the goals of CSAR, in no way does a recognition of such expertise constitute an endorsement of their views by CSAR.

Members will be persons who make up the financial support for the Center. Consultants may choose to become Members but need not do so. Membership is open to all who agree with the scientific goals of CSAR. Members will receive the journal and newsletter of CSAR and will have special priviliges including access to research reports and bibliographies of the Center. Non-members may subscribe to ZETETIC SCHOLAR but will not be able to subscribe to the newsletter. The policies and governance of CSAR are not under the control of its Members, but suggestions and criticism are always welcome by the governing board.

We are not yet calling for the enrollment of members. This should take place early in 1982 and will be announced in the next issue of ZETETIC SCHOLAR. Between now and then, CSAR is undertaking the formation of its initial network of Consultants. Those wishing to apply for Consultant status or who wish to contact CSAR with suggestions, questions or comments, should write to:

Dr. Marcello Truzzi, Director
CSAR
P.O. Box 1052
Ann Arbor, Michigan 48103

CSAR is currently sponsoring two major research projects --one on the use of alleged psychics by law enforcement agencies, the other on industry engineers' attitudes towards and experiences with unidentified flying objects. Two of the first reports emerging from these projects are being published in ZETETIC SCHOLAR #8, and further reports will follow in future issues. As with all such reports, CSAR sponsorship does not mean endorsement of the findings by CSAR or its Senior Consultants, The reports are the responsibility of their authors.

THE MYSTERY MEN FROM HOLLAND, I: PETER HURKOS' DUTCH CASES

PIET HEIN HOEBENS

The minuscule kingdom of the Netherlands has produced what seems a disproportionate number of occult detectives - individuals who profess to assist the police by paranormal means in locating missing persons and solving crimes.

Foreign newspaper reports have conveyed the impression that psychics are employed as a matter of course in Dutch police investigations. Some spectacular cases continue to be cited in American and English publications, usually accompanied by the assurance that the author has personally verified the evidence.

The purpose of the present article is not that of evaluating the whole problem of occult crime-busting, but that of critically examining a number of Dutch miracles as they have been reported in in the Anglo-Saxon world. I have selected those reports with the strongest claim to respectability, and compared them to whatever authentic sources I could trace in Holland.

PETER HURKOS' DUTCH YEARS

Of all Dutch "paragnosts" (as they are called in Holland), none has achieved more fame in the US than Pieter van der Hurk, alias Peter Hurkos. The Radar Brain Man was born in Dordrecht on May 21, 1911. He spent the major part of his professional life in the US, and is now an American citizen, but his relatively short "native" period is by no means unimportant. All his biographers, himself included, agree that Hurkos amazed the Dutch before he went on amazing the rest of the world.

The principle sources on Hurkos' Dutch years are his autobiography Psychic, the Story of Peter Hurkos (1); an authorized biography by Norma Lee Browning The Psychic World of Peter Hurkos (2); an interview in the book Psychics by the editors of Psychic Magazine (3) and a chapter in Fred Archer's Crime and the Psychic World (4).

Some of the claims made in these books are so extraordinarily vague that it is quite impossible even to attempt to check their accuracy. For example, on page 71 of Psychics Hurkos states: "It [helping the police solve crimes] started when I was in Holland. I was asked to help locate a little girl who was missing. It was sad: some woman had killed her and threw her in the water. I found the girl's body and helped solve the case." No names, dates or places are mentioned, except that the event occurred in the Netherlands, early in Hurkos' career. The case - his first success as a psychic sleuth - is unaccountably ignored both in the autobiography and in Mrs. Browning's book. Dutch files which I was able to consult do not contain the smallest hint as to what event the clairvoyant may be referring to.

Acknowlegement: I am grateful for the help of Dr. Brink on obtaining file material relating to the Nijmegen arson case.

Undated too is the case Hurkos relates on pp. 66-67 of his autobiography. "One day" he found the body of the son of one Captain Folken. The boy had drowned in Rotterdam harbour. "The police were as amazed as I was at the accuracy with which my extra-sensory perception had been employed in this case," Hurkos claims.

I am grateful to Mr. Bouwman of the Rotterdam Municipal Police for having attempted to verify this tantalizingly concise story. Mr. Bouwman looked through numerous volumes of police reports and contacted several retired policemen who might remember such an incident. In spite of his determined efforts, no trace was found of a document pertaining to a case resembling the one Hurkos reports. No member or former member of the corps remembered anything. The enthusiastic newspaper reports Hurkos mentions seem to have disappeared mysteriously from the files of the local papers. Mr. Henk Schröder, who kindly offered to search the press archives, drew a blank.

While this failure to unearth corroborating evidence does not actually disprove the claim it certainly cautions us against accepting it at face value.

If I do Mr. Hurkos an injustice by casting doubt on the veracity of his report, he himself is to blame. Fortunately, in relating some other cases Hurkos and his biographers have included enough details to allow the investigator to draw firmer conclusions.

THE PSYCHIC WAR HERO

As is well known, Hurkos acquired his uncanny gifts as the result of a fall from a ladder in the summer of 1941. At that time, Holland was an occupied country, and the Dutch police was controlled by the German invaders. In those circumstances it would have been unpatriotic to assist the authorities, so Peter Hurkos decided to use his ESP to further the aims of the resistance movement. In his autobiography, he portrays his own role in World War II in heroic colours. He states he had been a member of an underground group led by "a man named Hert Goozens, one of the bravest men in the entire system of secret fighters."

Mrs. Browning reports that after the occupation was over, Hurkos was received at the Royal Palace by Queen Wilhelmina and was presented by Her Majesty with a gold medal and a charter proclaiming his valorous deeds in the service of the Fatherland.

Some of Hurkos' reported activities as a war hero are for all intents and purposes in the psychic sleuths category and deserve to be mentioned here.

On pp. 17-20 of his autobiography, Hurkos relates how, when he was still in hospital recovering from his fall, he was visited by a mysterious stranger who was about to be released after an emergency appendectomy. As they shook hands, the newborn psychic "knew" that the other man was a British agent who was destined to be killed by the Germans on Kalver Street a few days later. In vain, he tried to

prevent the doomed stranger from leaving the hospital. "He will be killed on Kalver Street! Stop him! Stop him!" he cried. The doctor and the nurse thought he had a raging fever. Two days after his release, the British agent was in fact shot dead by the Gestapo on Kalver Street. Hurkos learned this while he was still in hospital. According to an official report cited by Mrs. Browning, the sensitive had been admitted to Zuidwal hospital on July 10, 1941, and had been released on August 5 of the same year. So the Briton must have met his dramatic death some time between these dates. Given the unusual circumstances (the Nazi's certainly did not make a habit of executing captured enemy agents on busy streets), I expected this incident to have been extensively documented.

If the event had really occurred, the State Institute for War Documentation (known as RIOD) in Amsterdam should certainly know about it. Its archives are by far the most complete of their kind. I made inquiries with the RIOD and a few months later received a letter, dated February 1981, from Drs. C.J.F. Stuldreher, whose help I gratefully acknowledge. Concerning the murdered Briton, Drs. Stuldreher writes:

"It is not known to us that in the summer of 1941 a 'British' agent (either of Dutch or British nationality) has been shot by the Gestapo in the Kalverstraat in Amsterdam or any other Dutch town. It is very improbable that this occurence really took place."

On pp. 40-43 Hurkos relates his most daring war time feat. A friend had been arrested by the Germans and taken to a camp in the town of Vught (which Hurkos erroneously thinks is only a few miles from The Hague - the actual distance is closer to 60 miles.) Peter got hold of a German officer's uniform, went to Vught, introduced himself (in "flawless German") as "Wehrmachtkapitan Robert Fischner" and told the camp commandant that the spy was needed at headquarters for questioning. The Germans readily believed him and took him to the barracks where his friend, Yap Mindemon, was held. The moment Hurkos entered he knew, by ESP, that Yap was going to spoil the entire plan. The prisoner would assume that Hurkos had gone over to the enemy side. "In a moment, I could feel, he would shout and denounce me to the Germans..." Hurkos had no choice. Cursing - in flawless German - he hit and kicked Yap until the poor fellow was knocked out. The soldiers carried the unconscious prisoner to the staff car that the camp commander had kindly put at "Robert Fischner's" disposal. "Arrogantly I slid behind the wheel and drove through the gates of the camp as fast as I could go," the psychic recalls. Thus, Yap Mindemon was rescued from the clutches of the Nazis.

The files of the Vught camp are kept at the RIOD, and Drs. Stuldreher kindly checked them for any evidence of this heroic deed. He found nothing whatsoever. His verdict: "The story seems to me a product of the imagination."

On pp. 126-130 we find what must have been one of the most dramatic cases in which Hurkos ever was involved. After having complained that "in a world of skeptics and fakers, it is not easy for a psychic to establish a reputation for truthfulness and accuracy..." Hurkos

recounts how, "one day," he was invited to give a seance in the house of Mr. R., "one of Holland's richest, most influential, and renowned patriots." The guests were impressed, but Mr. R. himself remained incredulous. Then came his turn to hand Hurkos an object to "psychometrize." He chose a cigarette case. The psychic touched it, and suddenly was hit by a terrible vision. "Sixteen Dutchmen--Sixteen Dutchmen!" he exclaimed. "What sort of man are you! Sixteen men--shot!"

Mr. R. "choked, and in a desperate voice he gasped, "you are insane! give - me - that..."

It was too late. The man collapsed on the floor and lay still. The guests sat frozen in their chairs. Hurkos could not restrain himself. "He is a traitor!" he cried. "He was honored by our country as a patriot, and he betrayed us - sixteen men - shot - sixteen Dutchmen shot - and all his fault! He made a deal with the Nazis; they ran his factories but he controlled them."

Mrs. R. then became hysterical. "He's dead, he's dead - and you killed him!," she screamed. "Liar! Liar!"

But Hurkos had told the truth. After "five long, lonely, haunted years" it was finally established that Mr. R., the honored patriot, had indeed been a collaborator with the Germans. He had betrayed sixteen members of the underground to the Gestapo. All had been shot.

Unfortunately, the enormous files of the RIOD do not contain the slightest indication that this drama, or anything like it, ever took place. Nothing is known there about "one of Holland's richest, most influential, and renowned patriots" who has posthumously been exposed as a traitor. Drs. Stuldreher has the impression that the story falls in the same category as the Vught case.

I do not know where Mrs. Browning checked her claim that Peter Hurkos after the war was decorated by Her Majesty. The RIOD "has no information on any underground activities of Mr. Hurkos, alias Peiter Cornelis van der Hurk," Drs. Stuldreher wrote me. Mr. Hert Goozens, "one of the bravest men in the entire system of secret fighters" must have been extraordinarily fond of secrecy, for even now nothing is known about him or his group.

PSYCHIC DETECTIVE

After the Liberation, Peter Hurkos put his uncanny gifts at the disposal of the Dutch authorities. An early success is related on pp. 64-65 of the autobiography and on page 181 of Archer. In the autumn of 1946 a young coal miner in the province of Limburg was murdered by his stepfather, Bernard van Tossings, who was known to have a jealous passion for his stepson's wife. The police were sure of his guilt, but they did not have conclusive legal evidence. For that, they needed to find the weapon with which the crime had been committed. Hurkos appeared on the scene, handled the victim's coat,

gave an accurate description of the suspect (moustache, spectacles, wooden leg) and urged the police to "take a look at the roof of the murdered man's house." There, a revolver was found. The stepfather's fingerprints were on the butt, assuring his conviction.

This claim received a certain amount of publicity in Holland in 1958, when word got around in the province of Limburg that Hurkos was planning to shoot a motion picture in the coal mine area, featuring the psychic himself solving local mysteries. From what they had heard of the cinematographic project, the Limburgers feared that they might be used as "witnesses" of dubious occult successes. The Amsterdam newspaper De Telegraaf then phoned Hurkos in the US and asked him what cases would be highlighted in the film. Hurkos mentioned the Van Tossings affair. The journalists checked with the Limburg authorities and learned that Hurkos had indeed made some statements concerning the murder of a young coalminer that had taken place in October 1946 in the municipality of Spekholzerheide. The suspect had been arrested immediately after the crime, as it was known that he had quarrelled with the victim. After having been handed a photograph, Hurkos had stated that the weapon would be found in a brooklet. The police dragged in vain. The revolver was found the next year, not in a brooklet, but on the leads of a house. The murder had not been one of the "crime passionelle" type, and the victim's wife had played no role whatsoever in the tragedy.

Hurkos had told De Telegraaf yet another story. In 1955, 43 coalminers had been trapped inside a Limburg coal mine, as a result of a failure in the lift system. After seventeen anxious hours they had been rescued. Investigation showed that the cause of the accident had been sabotage. The miners vowed to go on strike unless the culprit be found. At that moment, Peter Hurkos appeared on the scene. The chief of the mine police implored him to help. The clairvoyant obligingly psychometrized the wardrobe of all the personnel and picked out the working attire of an elderly employee. Guided by his paranormal intuition, he walked straight to the man's house. Inevitably, the culprit confessed.

The real facts, De Telegraaf learned from the authorities, had been slightly less dramatic. The failure had occurred in an unmanned lift, and the cause had been wear, not sabotage. No suspect was ever arrested for the simple reason that no crime had been committed (5).

To the north of Limburg lies the city of Nijmegen, the scene of what is reported as Hurkos' most impressive success in his native country. The relevant part of the autobiography (pp 89-93) can be summarized as follows.

In August 1951, an outbreak of arson occurred in the area around Nijmegen town. Farmers were terrorized. The damage amounted to hundreds of thousands of dollars. Two hundred men patrolled the countryside, but could not prevent the pyromaniac from striking time and again. At the request of an industrialist friend, Hurkos offered his assistance. At first, the police were reluctant, but they rapidly changed their minds after the psychic had demonstrated his

uncanny powers by accurately describing the contents of police chief Cammaert's pockets. Hurkos was taken on a tour of the burned-down farms. In the fourth ruin he visited he found a key. The moment he touched it he "saw" the arsonist. He told the police that the criminal was a boy of fifteen or sixteen years of age. The suspect was tall, had worked in a bakery but had been fired because he had tried to set fire to the place. The police told Hurkos they had pictures of all the town boys and asked him if he thought he could pick out the suspect. Hurkos was sure he could. In the police station, an officer "pulled out the highschool yearbooks of the schools for the past five years." The psychic began leafing through one of them and suddenly recognized the boy he had seen in his vision. The police were incredulous, as Hurkos had picked out Piet Vierboom, the seventeen year old son of a rich and respected Nijmegen family.

"I don't believe it," Captain Cammaert is quoted as saying, "It can't be. The family is one of the finest in Nijmegen." Shaking his head doubtfully, Captain Cammaert nevertheless fetched the boy for interrogation. Piet denied. Then Hurkos took over the questioning. Piet was lost. "I cannot lie to you. Yes, I did it," he exclaimed - so we are told in the autobiography.

This case has become something of a classic. It may have served as Hurkos' visiting card to the US, as it was featured prominently in the June 1956 issue of True that brought the sensitive to the attention of the American public. The story, titled "Man with the X-RAY MIND" and written by John Kobler(6),is basically identical to Hurkos' own version. In addition, it mentions the name of "Baron Speyart von Woeden," chief of police for the region, as a witness. The "amazing story of extrasensory perception" is accompanied by an endearing painting by William A. Smith. There, we see Hurkos in a prophetic posture confronting Piet Vierboom, looking for all the world like Huckleberry Finn. The lad, white as ash, is backing away, while the Nijmegen police, dressed as officers of the pre-war Royal Bulgarian Army, sternly look on.

The case is described by Archer and Browning too. Mrs. Browning's account is slightly different from that in the autobiography. For example, she states that the police asked for Hurkos' assistance, whereas Hurkos says he volunteered. In Browning, the clairvoyant is not leafing through highschool yearbooks (which, incidentally, did not exist in Holland at that time), but is concentrating on photographs spread upside down on a desk. Mrs. Browning compares this psychic achievement with Swedenborg's celebrated vision of the Stockholm fire.

Unambiguous contemporary accounts of Swedenborg's feat are sadly lacking, but in the Nijmegen case we are more fortunate. The outbreak of arson and Piet Vierboom's arrest were widely reported in the Dutch press, as was Hurkos' involvement in the case. In addiition to several newspaper reports, I have been able to consult a letter dated June 19, 1956, and signed by Baron Speijart van Woerden, at the appropriate time Public Prosecutor in the Arnheim district.

From these sources, the following facts can be established. The arsony started on August 12, 1951, in the Ooijpolder, an agricultural area near Nijmegen. Soon, the police began to suspect Piet Vierboom, the son of a Ooijpolder farmer. The boy, who was mentally deranged, had been employed at Van Mook's bakery in the village of Bemmel. On June 22, 1951, a mysterious fire had raged in the bakery. The local police thought Piet had been responsible, but the evidence was not strong enough to warrant an arrest. The authorities, however, remembered the incident and discovered that the Ooijpolder fires coincided with Piet's holiday, which he spent at his parental home. On August 14, at the site of one of the fires, candy wrappings were found. Investigation showed that Piet had recently bought a considerable quantity of this particular candy at the local sweet shop. The boy was quietly arrested on August 17. He was taken to Nijmegen and interrogated by Speijart van Woerden, who was soon convinced of his guilt.

At the same time, Hurkos happened to be in Nijmegen where he was to give a public seance. According to Speijart, this "stage telepathist" needed some publicity and offered his assistance to the police. In the afternoon of August 18, the day after Piet Vierboom had been arrested, the clairvoyant arrived at the police office. A policeman who had been present at the interrogation showed him a group picture of the Vierboom family. With an iron hook, Hurkos made "passes" over the photograph. At one moment, he pointed at one of the boys and stated that this was the pyromaniac. Those familiar with the elementary techniques of muscle-reading (in which Hurkos may be a professional expert) will not be surprised that the psychic picked out Piet (whose arrest had not yet been made public). "I cannot imagine that Peter Hurkos' performance had any scientific value whatsoever," Speijart concludes his letter. I can add that I have a photostat of a statement by the State Police of the Nijmegen district dated June 23, 1956, in which it is categorically denied that psychics were ever successfully employed in criminal investigations. The statement is signed by Mr. A. Cammaert...

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A Special CSAR Project Report

UFO SIGHTINGS AMONG ENGINEERS AND SCIENTISTS:

A REPORT ON THE ANOMALY PROJECT'S INDUSTRIAL RESEARCH & DEVELOPMENT UFO SIGHTING POLL

RON WESTRUM

Introduction

As a sociologist interested in the interpretation and explanation of UFO reports, I have frequently been frustrated by the inadequacies of the data with which I had to work. For instance, public opinion polls would collect data on persons who said they had "seen a UFO," but just what this meant was impossible to know. Having investigated UFO reports in the field, I knew that this did not necessarily mean that they had seen a "flying saucer"---a well-defined, disc-shaped object---but more often moving (or even stationary) lights in the night sky. How many of these "UFO sightings" involved distinct objects? What shapes were seen? What were the viewing conditions? And so forth. There was no way of telling. Until Dr. Peter Sturrock of Stanford did a study of astronomers' UFO sightings, there was no study which related "incidence" statistics and the actual events.

One of the frustrations in working with UFO literature regarding cases is that seldom are the same set of variables reported from one sighting to the next, so it is difficult to generate from the data at hand, for instance, a distribution of distance between observer and event, duration, viewing conditions, and many other matters which one would like to know. The manner in which data are collected and reported---in spite of strenuous efforts at standardization by UFO organizations---tends to work against data which are comparable. Another frustration is that the relation between reported and unreported cases is unknown, although one suspects that there is a bias in favor of better cases being reported.

While these problems were being considered, an opportunity arose by chance to collect some data of my own. In

February 1979, Industrial Research & Development conducted a reader-response card poll on the opinions of its readers about Unidentified Flying Objects. About 4000 persons responded to this poll, and the answers of 1200 of these 4000 were tallied and published in the July 1979 issue of Industrial Research and Development. Through the courtesy of a physicist interested in the subject, the Anomaly Project was able to obtain the original data cards.* One of the questions on the data cards was "Have you ever observed a UFO yourself?" The distribution of responses to this question was as follows:

yes	268	7%
perhaps	425	11%
no	3350	83%

N = 4043

To determine what was meant when respondents answered "yes" to this question, a standard questionnaire was designed regarding the details of the sighting, and this was sent originally to all persons who answered "yes." It was then noticed that persons with a "yes" response were approximately three times as likely as persons with a "perhaps" response to report that they knew someone else who had had a sighting. This suggested thay the "yes" and "perhaps" sightings represented essentially the same set of experiences, but which were more positively interpreted by persons who knew someone else with a sighting.** Hence, in addition to a second mailing endeavoring to get the non-respondents (to the first mailing) to send in a questionnaire, an effort which did elicit 31 more questionnaires, we also wrote to all the "perhaps" sighters. All the codable responses for the "yes" sighters (179) have now been tabulated. Twenty-eight other respondents indicated that they had not had a sighting; most of these had indicated on their original protocol that they had not had a sighting. The "perhaps" responses are still arriving and will be tabulated later; a superficial examination of these, however, suggests considerable overlap with the set of "yes" responses. The analysis below, then, will cover uniquely the "yes" sightings.

Through a mistake, questionnaires were sent to 30 persons whose protocol originally indicated that they considered themselves non-sighters; two of them, however, returned codable responses, and a third a marginally codable response. One wonders what would have happened if all the "no" respondents had been sent questionnaires!

The Context of the Sightings

Since some of the respondents returned more than one codable report, the number of sightings (186) was greater than the number of

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*This project would not have been possible without the assistance of Mr. Stanton T. Friedman, who assisted in arranging for us to get the data.

**The use of the term "sighting" is used throughout this document to describe the reports of the subjects' experiences. No prejudgment is intended as to any external validity of such reports.

respondents (179). Many of these, unfortunately, did not include the values for more than a few parameters, and this insufficiency is reflected in the analyses below. Also 30 respondents (17%) indicated that they had more than one sighting, and 10 of these (6%) that they had had four or more sightings!

1. When did the sightings take place? There are three sets of answers here: year, season, and time of day. (See Table I) Surprisingly, the sightings are distributed relatively evenly in time, without the large number of recent sightings that one would expect. Many of the sightings took place when the respondents were in their teens or even younger. In terms of season, it is not suprising to see that Summer predominated, followed by Fall and Spring, with Winter very low; this probably reflects the likelihood that people will be outside more often in warmer weather. The high percentage of sightings in the evening (61%) reflects a common pattern in UFO sightings.

2. The context in which the sightings took place is also interesting. (See Table II) For the great majority of respondents (80%) the sightings took place during leisure time activities, and while the witnesses were outside (53%) or in a car (30%). The great majority of sightings furthermore took place in suburban (38%) or rural (39%) areas, rather than urban ones(18%); this last finding probably reflects the fact that a far-off event is more readily witnessed when one has fewer obstructions intervening and clearer air to see through.

3. Most sightings had more than one witness. In only 30% of the sightings was the respondent the sole witness. In 19% of the sightings, there were 5 or more witnesses. Regarding education, of 271 persons (this includes non-respondents to our poll) who responded "yes" to the sighting question on the original IR&D poll, 13% had less than a B.S., 38% had a B.S. degree, 29% had a master's degree, and 20% had a Ph,D.; thus almost half the sample had a degree higher than the baccalaureate. This percentage is virtually identical to the corresponding figure for non-sighters. (It should be noted that the great majority of the sample are engineers and scientists.)

One interesting point is that there is a tendency for younger respondents to report "yes" or "perhaps" they had a UFO sighting more than older ones, just as there is for the general population. For the under 26 age group, the percent of "yes" or "perhaps" sighters is 21%; the percentage declines steadily so that for the over-55 group it is only 12%. It is to be noted that the overall percentage for the entire IR&D poll is 17% with a "yes" or "perhaps" sighting. Since this is higher than the sighting rate for the general population (about 11%), we can be reasonably sure that persons who had had a UFO sighting were more likely to respond to the original poll than persons who did not have one; in other words there are more sighters in the sample than we would expect by chance, and hence the sample is biased. This is to be expected in a reader-response poll, but since the primary purpose of this survey is to study sightings rather than to get a random smaple, this is fortunate for us.

Table I

When Did The Sighting Take Place?

Decade			Season			Time of Day		
1930's	1	0%	Winter	8	4%	Midn. to 6am	19	10%
1940's	4	2%						
1950's	45	24%	Spring	40	22%	6am to noon	13	7%
1960's	63	34%						
1970's	67	36%	Summer	78	42%	Noon to 6pm	35	19%
1980	1	0%						
NA	5	3%	Fall	44	24%	6pm to midn.	114	61%
			NA	16	9%	NA	5	3%

Table II

Context of the Sighting

Locale			Position			Activity		
Urban	34	18%	In building	12	6%	Leisure Time	148	80%
Suburban	71	38%	In open air	98	53%	Military Service	13	7%
Rural	73	39%	In a car	55	30%	Occupational Duties	10	5%
NA	8	4%	Other (plane, etc.)	16	9%	Other & NA	15	8%
			NA	5	3%			

Table III

Sighting Parameters

Duration			Estimated Distance		
0-10 seconds	45	24%	0-5 meters	1	1%
11-59 seconds	52	28%	5-20 meters	4	2%
1-5 minutes	52	28%	20-100 meters	8	4%
6-59 minutes	27	14%	100-500 meters	18	10%
1 hour or more	4	2%	.5 km-2 km	53	28%
NA	6	3%	2 km or more	76	41%
			NA	26	14%

Table IV

Distance Vs. Time of Day

Time of Day	Distance: less than 500 meters	.5 km-2 km	greater than 2 km	NA
Morning (6 am-12 am)	2 (1%)	6 (3%)	3 (2%)	2 (1%)
Afternoon (12 am-6pm)	4 (2%)	11 (6%)	16 (9%)	4 (2%)
Evening (6 pm-12 pm)	21 (11%)	31 (17%)	47 (25%)	15 (8%)
Night (12 pm - 6 am)	4 (2%)	5 (3%)	10 (5%)	0 (0%)

NA = 5

Table V

Perceived Shape Vs. Illumination (Time of Day)

	Time of Day: 6 am - 6 pm	6 pm - 6 am	
Torpedo (13) and Disc (47)	25 (56%)	35 (29%)	
Sphere/Ovoid	11 (24%)	31 (26%)	
Point Source of Light/Unable to Determine	9 (20%)	54 (45%)	
Total	45	120	NA = 21

Table VI

Distinctiveness of Shape Vs. Estimated Distance

	Distance: less than 500 meters	.5 km - 2 km	greater than 2 km	NA
Torpedo or Disc	13 (45%)	23 (47%)	18 (26%)	6 (33%)
Sphere/Ovid	6 (21%)	11 (22%)	21 (30%)	4 (22%)
Point Source/Unable to Determine	10 (34%)	15 (31%)	30 (43%)	8 (44%)
Total	29	49	69	18

NA = 21

What Did The Respondents See?

If one had to describe the "average" UFO sighting in the sample, it would be something like the following: a distant light moving erratically in the night sky, at least 500 meters distant, and seen for about a minute. As one can tell from Table III, only 31 persons said that the sighting took place at less than 500 meters, and only 13 of these at less than 100 meters. We might then say that only about 7% of the sample represented "close encounters." Duration was typically short, often less than a minute. We have already noted the propensity for sightings to take place in the evening. Only in a relative minority of the sightings, then, were the viewing conditions very good for critical observation.

One might well ask what it was about these "nocturnal light" events that led to them being categorized as UFOs. In the majority of cases it would be fair to say that what was seen was a luminous object which did not fit any natural or human phenomenon known to the respondent. In other words, they were "UFOs" because they were unidentified airborne objects, rather than fitting the stereotype of "flying saucer." They were UFOs because the observer could not fit them into any known category rather than because they matched a public image of a "UFO." A good example of these "nocturnal light" UFOs is in Case #4.

> R was star-gazing with his wife and 2 friends, when they observed a lighted object on the southern horizon. It traveled in a straight line until it was directly overhead and then traveled west for a distance and then north until it disappeared on the horizon. It occurred in under 10 seconds and made two essentially instantaneous right angle turns. It was at the same magnitude as a star or a distant satellite.

The motion of the "nocturnal lights" was apparently often the anomalous factor which led to the event being conceived as a "UFO sighting." Rapid shifts of direction without banking or any apparent deceleration, including right-angle turns (as in the above sighting) occurred in a significant minority of cases. Other respondents characterized the motion of the object as "erratic" without further elaboration. The "right-angle-turn" phenomenon is well known to UFO investigators, although seldom present in public stereotypes of UFO events.

Public conceptions of UFOs may have had some effect, however, as indicated by those sightings which the author, based on his knowledge of UFO reports, identified as probable meteors or advertising planes. Twenty reports were identified as probable meteor events and six as probable advertising plane sightings. Examples of each follow:

> Case 98. R and three others were squirrel hunting. Suddenly a long multicolored light appeared for about 10 seconds and suddenly disappeared. The object moved in a straight line. (probable meteor)
>
> Case 65. R and friend saw object hover noiselessly 600-700

yards away with sequential lights. The disc-shaped outline of the object was not indistinct or fuzzy. After hovering and slowly moving along the river bed the object accelerated very rapidly and moved off in a straight line until it was out of sight in approximately twelve seconds by R's watch. (possible advertising plane)

Had these events been reported to a UFO investigator, their identification would perhaps be most certain. They might be positively identified as meteors or advertising planes on one hand, or on the other, these potential identifications might be ruled out after checking.

One would expect that perceived shape is partly a function of the viewing conditions. That is, when the object is closer, its shape will be seen more definitely (and accurately); this will also be true when the illumination is better. The overall distribution of distance vs. hour of the day can be seen in Table IV. If one compares shape against the hour of the day, one will see (in Table V) that the objects seen after dark are more often described as a point source of light or "unable to determine." Similarly in Table VI we find that there is a tendency---less marked than I suspected, however---for the objects estimated to be further away to be seen less distinctly.

It is worth noting that ordinarily 90% of reported "UFO" events can be identified as known objects. With a more technically sophisticated sample such as the present one, one would expect this percentage to be considerably lower. In this regard, it is interesting to note the following distribution of respondents' conceptions of their sightings:

R has since been satisfied it was a normal event	7	(4%)
R is sure it has a normal explanation	31	(17%)
R feels event represents processes unknown to current scientific knowledge	118	(63%)
No Answer	30	(16%)
	186	(100%)

The high percentage of "unknown" responses here is partly due to poor question construction but also probably reflects the fact that those who felt their experience had a normal explanation were less likely to think of it as a "UFO" and report it in this way.

A number of sightings, however, would seem to qualify as "UFO sightings on a more unequivocal basis. For instance, if we consider only those that took place in daylight and at a distance of less than 500 meters (a total of six sightings) we find sightings like the following:

Case #139. R was taking a walk on the beach in the early afternoon, was looking over the waves and noticed an object hovering. It looked like an aluminum lens, and it tended to flutter like a falling leaf, but it did not descend. It appeared to be 10-20 meters in size, and 30-50 meters away. R watched it for 20-30 seconds and then it simply disappeared.

Case #36a. At age 12, R had seen a disc with a dome hovering above a ranch house for 10-15 minutes; there was no noise or disturbance. The object suddenly shot off toward Pittsburgh ("just a blur"). It was a clear and sunny day. There was a total of ten witnesses.

Or we might consider the following sighting, one of the rare "physical effects" cases:

Case #168. During geomagnetic survey of Barringer meteorite crater, an electronic flux-gate magnetometer responded to presence of two aerial objects over Canyon Diablo to the West. Magnetometer sounded over-current alarm, and thus drew attention to itself. The two skyborne objects were pointed out by an elderly couple, both tourists. After a brief stationary position above Canyon Diablo, both objects assumed a high-speed course toward Tucson and passed out of view.

Now of course no sighting is really "unexplainable." Some explanation can be brought forward even for the cases above. Nonetheless a large number of cases in the sample were extremely intriguing, to say the least. Details of the cases will be transmitted to the Center for UFO Studies for further analysis.

Many of the more exotic types of cases familiar to the UFOlogist were not reported in the survey. No one was zapped, no vehicles were stopped, no one was abducted (no one admitted even to having seen "occupants"); in no case were physical traces left behind by the object. It may be that the persons who had such experiences were among the respondents who indicated that they had had a UFO experience, but did not wish to discuss it; or it may be that such experiences do not exist among our sample.

What Did They Do About It?

Most of the respondents (88%) indicated that they discussed the sighting with family and friends, as we might expect. But only a fraction of the sample (22%) said that their sighting had been reported--most often to the military, the police, or the mass media. This percentage is considerably higher than the reporting rate for the general population (13%). 38% of the sample indicated that the sighting had increased their reading about UFOs. Although some of the respondents indicated that the experience had made a lasting impression on them, in no case did the respondent indicate that the experience had had any serious impact on their lives, unlike what is often reported to be the case for those involved in "close encounter" sightings.

Conclusion

The aim of the present study was not to reach any broad conclusions about the UFO phenomenon, but rather to explore the spectrum of UFO experiences among a group with relatively high technical experience. One often reads in the media statements like "11% report UFO experience," but the nature of this experience is seldom specified. This study has allowed exploring the nature of the experiences, although our resources do not permit an investigation of each case.

This study will continue with a consideration of the "perhaps" cases, and a comparison of these with the "yes" cases. Capsule descriptions of the cases in this sub-sample can be obtained from the author for $5.00 (make check payable to "Ron Westrum"). Subsequent reports will be published in ZETETIC SCHOLAR, official journal of the Center for Scientific Anomalies Research, in which the Anomaly Project is included.

Any comments or suggestions on this study will be welcomed by the author.

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The author gratefully acknowledges the assistance of Ranka Stajic-Mulkern, Stanton T. Friedman, Allen Hendry, and Ruth Hall in various phases of the Anomaly Project.

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APPENDIX: Some Additional Cases

Case 2. R and his wife were returning from a bridge tournament in Springfield, Mass. on a Sunday evening. Traffic was moderate on the heavily traveled I-91. Suddenly 45 degrees from the horizon there appeared a huge hovering object which R can best describe as a flying saucer. R wiped his eyes, unbelieving. R and his wife turned to each other simultaneously and both remarked "It's a (deleted) flying saucer!" What they saw was a disc with an appendage, larger than 50 meters, and between 100-500 meters away. Duration: 11-59 seconds.

Case 8. R was driving on a rural road in New Hampshire. As he approached Route 101, he observed what he at first took to be a large radar or microwave dish located on top of a hill at the intersection of the road and Route 101. Although there was no supporting structure visible, there was a huge round metallic disc motionless above the top of the hill. As he approached closer to the intersection his view of the top of the hill and object was cut off. He mentioned to his family that a structure had apparently been erected on the hill (they had not noticed it). Since R wanted a better look at it, he parked close to the base of the hill after going through the intersection. There was absolutely nothing to be seen where the object had been. The object had been in view approximately two minutes, and had remained perfectly motionless.

Case 10. At 9pm, R was resting after using his telescope when he saw a silent black triangle moving across the night sky. The corners of the triangle were illuminated with a kind of glow due to electric discharge. In the Niagara Falls Gazette the next day there was a small paragraph about a number of triangular objects seen over Spain. Duration less than 10 seconds.

Case 36b. On a spring day in 1976, approximately 11:00am, with slightly overcast sky conditions, R and another person saw a pink sphere about 1 meter in diameter float to the ground. R went out of the house to investigate. The object went back up in the air very fast (gone out of sight within 10-15 seconds).

Case 54. A small "ball of light" came from ahead, opposite R's trajectory (he was in a car) down a street of homes, and turned around (close range) to follow parallel to R for some seconds, then it assumed its original course. Duration less than 10 seconds. (potential ball lightning case)

Case 136. R was sitting in a pickup truck on a low hill overlooking the Rio Grande Valley and River from the west mesa, near Las Cruces, New Mexico. The distance from R to the Mesilla Dam was about 3 miles. He did not notice any interference on either his CB or AM radio channels; he was listening to the radio. While looking over the valley toward the Organ mountains he noticed a large shiny object on the ground, so bright that he described it as having to look around a shining reflecting mirror to see the scenery. It appeared to be a semi-circle with the missing half of the circle buried in the ground. The object appeared to be about 3/4 mile from R's location. After about 20 minutes the object appeared to move east along an arroyo about 1 foot off the ground. As it picked up speed, it climbed at a steeper angle such that one could see mesquite bush tops under it. At about 25 feet altitude, it started curving right for flat flight above the Rio Grande River. When it reached 50 feet altitude in level flight over the river headed south, it moved out of R's view behind some low hills on the west side of the river. R notes that there was "no comparison of this object to any local crop duster craft. This object was simply too big and did not move in the pattern that a crop duster uses to cover the fields thoroughly."

Further Reading

People often ask the author for a good introduction to UFO research. The following collection will provide a balanced guide to problems, issues, and controversies, with attention given to the entire range of cases likely to be met with by the investigator:

Richard F. Haines, Editor, UFO Phenomena and the Behavioral Scientist (Methuchen, NJ: Scarecrow Press, 1979). Eleven behavioral scientists explore various aspects of UFO experiences, reporting, and belief.

Richard F. Haines, Observing UFOs (Chicago: Nelson-Hall, 1980). An advanced treatment of physical and psycho-physical aspects of UFO observation by a psychologist.

Allen Hendry, The UFO Handbook: A Guide to Investigating, Evaluating and Reporting UFO Sightings (Garden City, NY: Doubleday, 1979). Probably the best single work on most aspects of UFO sightings. Indispensable for anyone with a serious interest in the subject.

Budd Hopkins, Missing Time: A Study of UFO Abductions (New York: Richard Marek, 1981). The best study of "abduction" cases to appear so far.

J. Allen Hynek, The UFO Experience: A Scientific Inquiry (Chicago: Henry Regnery, 1972). An older but still valuable book by the U.S. Air Force's former astronomical consultant on UFOs.

Philip J. Klass, UFOs Explained (New York: Random House, 1974). An important work by a major UFO critic.

Harley Rutledge, Project Identification (Englewood Cliffs, N.J.: Prentice-Hall, 1981). Narration of observations of aerial objects during a "flap" by a physicist who went to see (and photograph) for himself.

Frank B. Salisbury, The Utah UFO Display: A Biologist's Report (Old Greenwich, Connecticut: Devin-adair, 1974). First-hand investigation of a group of sightings in the Uintah basin of Utah. Also this is a good general discussion of some of the problems in evaluating reports.

Peter Sturrock, Report on a Survey of the Membership of the American Astronomical Society Concerning the UFO problem (Stanford, Calif.: Institute for Plasma Research of Stanford University, January 1977).

Ron Westrum, "Social Intelligence about Anomalies: The Case of UFOs," Social Studies of Science, Vol. 7 #3 (August 1977), pp. 271-302. A discussion of the social-science aspects of the UFO controversy.

ANNOUNCEMENTS

A group of philosophers have started "The Society for the Philosophical Study of the Paranormal." They hope to meet regularly in conjunction with regional meetings of the American Philosophical Association and perhaps at other times should demand warrant. Philosophers interested in being on the mailing list or wishing to contribute papers should contact the temporary convener, Professor Frank B. Dilley at the University of Deleware, Newark, Deleware 19711. The first mailing will be free.

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There will be a session on "The Occult and the Paranormal" at the 1982 Popular Culture Association's annual meeting to be held in Louisville, Kentucky. Persons interested in contributing papers should write to: Dr. David Stupple; Department of Sociology; Eastern Michigan University; Ypsilanti, MI 48197.

ON THE SIRIUS MYSTERY: AN OPEN LETTER TO CARL SAGAN

ROBERT K.G. TEMPLE

Dear Dr. Sagan:

I am highly flattered that you have wished to enter the debate about the astronomical knowledge of the Dogon tribe of Africa, first brought to public attention by my book The Sirius Mystery in 1976. Your contribution to this subject first appeared in the magazine Omni for August 1979, and was followed by assorted letters from readers in the issues of Omni for the following October, November, and December, and then this year (1981) by an article in Omni by myself. Your article also formed a chapter in your book Broca's Brain.

I try to reply to critics of The Sirius Mystery when there is an obvious forum in which to do so. It is not always easy to arrange this. A friend of yours, James Oberg, wrote a lengthy critique of The Sirius Mystery in Fate Magazine for November 1978, which I answered fully, completely refuting all points of criticism, in the issue of Fate for October 1980. Many months of illness prevented me earlier from making several replies such as I would have wished, or as early as I would have wished. A lengthy critique by A.C. Grayling in Issue Six of the British magazine Ad Astra was fully answered by me in Issue Eight of the same magazine. But I have experienced some difficulty in persuading certain magazines to grant me the right of reply. After a year of my insisting, Omni finally agreed to let me write an article about The Sirius Mystery as long as it did not consist of a reply to your points raised earlier in their pages. They repeatedly told me they did not wish to publish responses to your points because they did not wish to "offend" you. The Editor of Nature has also refused to allow me proper right of reply to a critic.

You can therefore appreciate some of the difficulties I have encountered in attempting to discuss these matters and respond to criticisms. Just for the record, you might be interested to know that I have yet to encounter a single criticism of The Sirius Mystery to which there was not a satisfactory reply. You can imagine, therefore, how grateful I am to the Zetetic Scholar for allowing me to respond to your own points in their pages, a discussion I feel sure you will not find in any way the slightest degree offensive, but will rather welcome in the true spirit of scientific enquiry.

There are various points you made in your article/chapter which I should like to correct, and which are of considerable importance. Let me first say that although you obviously thought I was British when we met in London, I am in fact an American. This is the least important of the 15 points I wish to make. But as you did describe me in your article as British, I thought it best to take this opportunity to correct you.

The Dogon tribe of Mali in Africa possess highly advanced astronomical information, much of it concerning the system of the star Sirius,

and this is what has come to be referred to as "the Sirius Mystery." You have raised the suggestion that this information came to the Dogon from modern Western sources. Dr. Germaine Dieterlen, the Secretary General of the Société des Africainistes at the Musée de l'Homme in Paris, is the anthropologist who together with the late Dr. Marcel Griaule first published an account of the Sirius traditions of the Dogon. As she has spent most of her life living with the Dogon and knows them and their traditions more intimately than anyone else alive, her opinion on a possible Western origin for the Sirius traditions of the Dogon is of the highest importance. She answers such suggestions with a single word: "Absurd!" The BBC-TV Horizon Program which was shown three times nationally as a Nova Program in America, "The Case for the Ancient Astronauts," featured a fascinating interview with Dr. Dieterlen in which she made this remark very strongly and held up in front of the cameras a Dogon artefact representing the three stars which the Dogon claim are at the Sirius system, and which she said was four hundred years old. I have this on video-tape at home, but was often puzzled why friends in America who saw the TV program never seemed to have "registered" Dr. Dieterlen's forceful remarks. Eventually I came to realize that this section of the program was apparently edited-out for American television! So that would explain why you also appear to be unaware of the opinion expressed on this subject by the world's leading authority.

My third point relates to Western missionaries. Long ago I wrote to the head of the White Fathers in Mali, Father Dubreul at Mopti, who replied saying that none of their missionaries had had any contact with the Dogon before 1949. But as you know, the information of the Dogon Sirius traditions had by that time already been gathered by the anthropologists. So therefore these missionaries are ruled out as a source. Please allow me to send you sometime a photocopy of this letter from Father Dubreul. I did send a copy of it to another person whom we both know, who shall be nameless, who went right ahead anyway and published a quite false and contrary story in a book in which he discussed the matter. The falsification to which I refer would probably have led you unwittingly into error on this subject.

My fourth point relates to a suggestion made by you, which originated with an earlier writer whom you consulted, that Dogon were conscripted by the French to fight in the trenches in World War I. These Dogon soldiers, it is claimed, would have had access to modern Western scientific traditions about Sirius. First of all, I do not believe it is true that any Dogon tribesmen fought in any trenches in Europe in the First World War. But let us assume that they did: it is physically impossible that this could explain the "Sirius Mystery" for the simple fact that the Dogon tradition of the superdensity of the star Sirus B could not have been obtained that early in the West. Eddington revealed the superdensity of Sirius B about 1926, long after the First World War had ended, and as you yourself mention, in 1928 he published this in his popular book The Nature of the Physical World, of which I have a first edition in my library which states that the work is a publication of his Gifford Lectures of 1927. You mention that this book was widely popular and translated into French. But alas, by 1931 the anthropologists were already with the the Dogon and would have known if some group of Western amateur astronomers had rushed out to the desolate hinterland of Mali to implant this knowledge in the presumably pliant minds of the Dogon priests

in that narrow period of two or three years before their own arrival. How all of this was then supposed to have filtered down through the entire Dogon and surrounding cultures of over two million people and been embodied in the hundreds or thousands of objects, symbols, woven blankets, carved statues, etc., etc., which exist in those cultures relating to the "Sirius Mystery" in only two or three years baffles me. And how these hundreds or thousands of objects are meant to have been expertly fabricated fakes purporting to be centuries old, fooling all dating experts, baffles me even more. It is considerations like these and many more (such as the tribal sacredness of the tradition making it unlikely that it could have come from Western intruders who would not have been highly regarded or in the confidence of the meticulous and traditional priests) which lead Dr. Dieterlen to reject the suggestion of Western origins as "absurd." Hence my fifth point: the 1928-1931 gap is too narrow and too late. (I also asked Father Dubreul whether any of the admittedly too late White Fathers were knowledgeable about astronomy and he said no.) And as regards Dr. Dieterlen, my sixth point is that you nowhere even mention her, despite her being the co-author with Dr. Griaule of the original anthropological report. And yet she is still very much alive and actually rejected your thesis on television years before your book advocating it was published.

My seventh and eighth points are highly important. You say "Temple says" the Dogon maintain the star Sirius B orbits around Sirius A in an ellipse, and you also say that "there is some evidence that the Dogon like to frame pictures with an ellipse, and that Temple may be mistaken about the claim that in Dogon mythology the planets and Sirius B move in elliptical orbits." First of all, it is not Temple who says this, but the Dogon themselves, and secondly, I am not mistaken about it because there is more explicit evidence than you noticed. If you look at p.45 of my book, where I publish an English translation (vetted for accuracy by Dr. Dieterlen) of the original anthropological report by Griaule and Dieterlen, it is not Temple who says anything at all! This part of my book is not written by me, but by the anthropologists. It is they who say explicitly that the system of the three stars at Sirius "is represented by a pattern...consisting of an oval [ellipsoid] in which one of the centers is Sirius." (italics mine). On p.40 of my book there is another diagram of the orbit to which you do not refer. There the anthropologists make quite explicit the elliptical nature of the orbit by reproducing the stationary position of Sirius A and the two extreme positions of Sirius B with the ellipse showing the movement between the two extremes and which they specifically say "gives an idea of this trajectory." On p.26 also you may see Figure 8, taken from the book Le Renard Pale by Griaule and Dieterlen. Here we see a specifically astronomical diagram: it shows the star Sirius C going around Sirius A in an elliptical orbit and a planet going around the star Sirius C also in a highly elongated elliptical orbit of its own, of a much smaller scale. You have neglected all of this evidence, I regret to say.

You briefly mention that in the Dogon mythology "twins play a central role," which is certainly true. But when you suggest that this might explain why the Dogon gave Sirius a companion star (an argument advocated at great length by some others), you have drawn a too hasty conclusion. For if looked at with sufficient attentiveness, the "twin" argument actually is seen to be evidence against, not for, the Dogon

Sirius traditions being as they are. The reason for this (which some others appear to have obscured on purpose because it did not suit their arguments) is that the Dogon insist that there are not two but three stars in the Sirius system. If they had fabricated a tradition of the stars to accord with the sacredness of twins, they would never have insisted on a third star's existence. Thus my tenth point: three stars do not make twins!

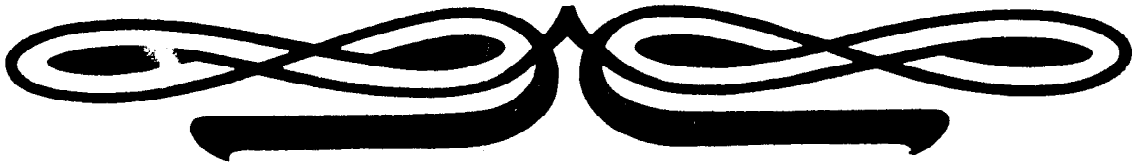
My next point is an astronomical one, something which evidently slipped your mind when discussing (before rightly rejecting) the idea that the tiny Sirius B, which evolved from an earlier massive star, might have been visible to the naked eye in the historical past. Although you reject this theory, you remark that if it were true, "The relative motions of the two stars about each other could be discerned with the eye." This might lead others who do not agree with you in rejecting the theory into error. For you neglected to realize that even at that earlier stage in its evolution millions of years ago when it was a large star, Sirius B, which is invisible now to the naked eye (one of the reasons for "the Sirius Mystery" being a mystery), would still have been indistinguishable from its companion star as a separate object because of the minute parallax. This effectively destroys all arguments for the visibility of Sirius B in the past without need for further discussion.

But my further points would be to agree with you that it is astrophysically impossible for Sirius B to have been such a large "red giant" star anyway within the past one or two million years at least, and also to comment on your interesting quotation from Horace about "The red dog star." There are a number of ancient quotations which either refer to Sirius as red, or are said to do so. "The redness of Sirius in antiquity" problem has been a major controversy in astronomy since the 18th century, in which leading figures such as Herschel, Schiaparelli, and Eddington have taken part. I have nearly finished the most complete historical review of this controversy ever undertaken, including translating the entire texts of the many German articles on the subject. It is my intention to publish the results and a full bibliography when time permits. I dare to hope that the two hundred-year controversy will then be settled in the negative: No, Sirius was not actually red in antiquity. It was red during the early Dog Days each year in the Mediterranean area because it was at the horizon and was reddened just as the rising and setting Sun is reddened. But some ancient Greek sources describing Sirius as poikilos are said to describe it as being red. Such reports are simple mistranslations and show only that astronomers are not always good classical scholars. The Greek word poikilos never at any time had the meaning "red" as some astronomers wrongly supposed. It meant "mottled," and referred to the high degree of scintillation for which Sirius is well noted. These few remarks, then, should go some way towards clarifying the information which you found in Horace. A full account of the matter will be published in my complete survey of that particular subject in the future.

I should like to remark in passing that, although you are correct in saying that the heliacal rising of Sirius was used in ancient Egypt to signal the inundation by the Nile, this was true only for a relatively brief period and was not the primary significance of the heliacal rising. The reason why this was a transitory aspect of the matter is that the day of the heliacal rising continually shifted, due to the precession of the equinoxes.

My fifteenth and final point is where I rush to the defence of the Dogon when you mention their tradition of the Creator and the plaited basket, which you find tempting to regard as an inferior or primitive myth. Actually, this basket may be a survival of the basket invariably carried by the Babylonian Oannes/Dagon, as clearly seen in Plates 6,7,8, and 9 in my book. And this may also be the source of the sacred basket of Demeter from the ancient Greek mysteries. In the Introduction to Le Renard Pale, Dr. Germaine Dieterlen speaks of the basket: "For the Dogon as well as for the other societies of West Africa, the smallest common object reveals by its shape and its decorations, the voluntary expression of a complicated cosmogony...Thus...a basket used for carrying represents, when it is upside down, the ark on which human beings descended from heaven to the earth, the square bottom represents space and the four cardinal points." Your account of this sacred basket is misleading. In fact, the tazi basket of which you speak represents the second ark of Ogo, and the third ark of Ogo is represented by another basket called nugoro, while the first is represented by the nukoro basket. The two latter ones are associated to rites related to the four major moons of Jupiter (which are also invisible to the naked eye). All three baskets are meant to portray what we Westerners would call space ships. It is to be regretted that you chose the example of the basket as an intended illustration of their backwardness. Space ships are really rather forward. And knowledge of the invisible moons of Jupiter is hardly primitive.

But there is more to the matter than that. I would hope that when one day you have the time, you might study the Dogon cosmogony more thoroughly. In my personal opinion, it is one of the richest, most profound systems of thought in the entire world. A superficial glance at it could offer any number of disconnected subjects of ridicule and derision, just as in the same way one could take the cross of Christianity, the candlestick of Judaism, the tetractys of the Pythagoreans, the rites of Shinto, the belly of the Buddha, or the Mohammedan's bowing towards Mecca and journeying there in order to walk in circles around a meteorite as examples - in the disjointed sense, out of context, - of barbaric primitivism amongst all those peoples, and prove them morons. And yet in those more familiar cases we know full well that the apparently ridiculous details form part of larger and deeply meaningful philosophies and religions. The same is true of the Dogon. I assure you, from the years I have studied them through reports, and the conversations I have had with the anthropologists who have lived with them over decades, the Dogon are, in the sense of the intrinsic value of their thought, one of the leading cultures of the world. They may be indifferent to modern Western technology, but their moral fibre equals or surpasses our own, their philosophy and religion are as a whole not a whit inferior to any other in existence on this planet; they are happy, contented, fulfilled people with rich and meaningful lives. And, frankly, if I were a Dogon I would be very proud of it and look at the materialistic West with some degree of pity. But they are too magnanimous to pity us. Their thoughts are, after all, often amongst the stars.



THE CASE OF PSILESS MARNER

MARVIN GARDENS[1]

Note: Any similarity between the characters in this report, including the author, and persons living, dead, or on The Other Side, is in all probability purely coincidental. And yet . . .

Once there was a sleight-of-mind expert called The Incredible Bambi. Motivated by an abiding concern for the welfare of humanity, distressed by the Rising Tide of belief in psychic phenomena, and considering that the magic business isn't what it used to be, he formed the Foundation for the Preservation of Sacred Ideas, otherwise known as the PSI Foundation.[2] Upon obtaining a tax-exempt status for the PSI Foundation, The Incredible Bambi secured a modest endowment from a well-to-do widower who wanted to be reassured that his wife did not survive bodily death.

As Research Director for the new foundation, The Incredible Bambi hired an experimental psychologist, Dr. Psiless Marner. Psiless knew everything there was to know about human behavior. In fact, Psiless Marner knew just about everything there was to know. He learned it in graduate school where he took copious notes and made straight A's in all of the multiple choice tests.

Like The Incredible Bambi, Psiless knew that psychic phenomena were impossible. This saved them both a lot of time that would otherwise have been wasted studying the parapsychological literature. Since ESP was impossible, there was really no need to do any further research. However, inasmuch as the PSI Foundation was endowed to do research, and inasmuch as Psiless Marner and The Incredible Bambi wanted to establish their objectivity and open-mindedness toward ESP, even though they knew it to be impossible, they decided to perform the definitive ESP experiment.

Unlike those credulous parapsychologists, Psiless Marner knew how to set up controls for a proper ESP experiment. Psiless always made his subjects undress so he could examine their orifices for hidden radios and magnets. He would then handcuff the naked subject to a chair which was securely bolted to the floor of the experimental chamber. The subject was then asked to guess the order of a sealed pack of playing cards, buried under the basement of The Incredible Bambi's house on Staten Island, many miles away. The Incredible Bambi had personally shuffled the cards and then buried them, securing the entire area with a five megaton atomic device which he had procured earlier under somewhat mysterious circumstances. (Magicians never reveal their secrets.)

Psiless Marner had carefully worked out stringent criteria to exclude "unhealthy" persons as subjects in his experiment. Only those

who had trouble sleeping, never remembered their dreams, had never been in an altered state more severe than California, and who never experienced psychic or other hallucinatory phenomena, were eligible for participation. So Psiless recruited most of his subjects from the local medical school.

Since psychic researchers claimed that ESP was inhibited by unfriendliness, Psiless would always smile and say, "Now just relax," as he handcuffed a naked subject to the chair. He further reassured his subjects by telling them that they would only be required to undergo a lie detector test if their ESP scores were positive.

Everything was going very well, until, one day, midway through the experiment, an unqualified subject slipped through Psiless Marner's eligibility criteria. Her name was Virgo Risene and she neglected to tell Psiless that she had passed (with flying colors) ESP tests in a famous parapsychology lab. The doors to the experimental chamber locked and bolted, Psiless waited impatiently for Virgo to begin guessing the distant cards. The intercom was silent. "Guess the cards," demanded Psiless, fearing that he would be late for the monthly meeting of Spoonbenders Anonymous. "I see a mushroom cloud," Virgo responded dreamily, ". . . a very large . . . mushroom cloud."

"Forget that!" insisted Psiless Marner, "Guess those cards!"
"The mushroom cloud is . . . getting larger and larger," Virgo continued.

Suddenly, Psiless Marner's secretary burst into the monitoring room: "Dr. Marner," she said breathlessly, "Dr. Marner, there's been an accident. An atomic device has just been detonated on Staten Island!"

The doors to the experimental chamber were unlocked and the bolts were removed. "It's a displacement effect, you see," explained Virgo, "it happens all the time," she smiled.[3]

Miraculously, The Incredible Bambi escaped the blast unscathed. (Magicians never reveal their secrets.) The wealthy widower started receiving communications from his wife through a Venus Flytrap, and, upon her advice, withdrew his support of the PSI Foundation. The Incredible Bambi rebuilt his house on Montauk (Staten Island no longer existed) and began looking for a well-to-do widower with a brown thumb. Psiless Marner disappeared mysteriously, and neither he nor Virgo Risene were ever heard from again.

FOOTNOTES:

1. Marvin Gardens, well-known science writer, book binder, and amateur magician, was the author of The Annotated Helter Skelter, wrote a weekly column for the Scientific Lithuanian, and shortly before his tragic demise during a flight through the infamous Bermuda Triangle, he filed this report with his friend Charles Honorton. Mr. Honorton is Director of Research at Psychophysical Research Laboratories, Princeton, New Jersey.

2. Psi, the twenty-third letter of the Greek alphabet, is a term used by parapsychologists to denote psychic phenomena. Its principle advantage is that unlike terms such as psychic phenomena, ESP, etc., Psi does not presume any particular explanation. Since parapsychologists don't have any particular explanation for psychic phenomena, this seems like a good idea. However, in recent years, due to media sensationalism and commercial exploitation, the term Psi has been indiscriminately linked to everything from pyramids to UFOria. Therefore, many prominent parapsychologists have begun to look for a new noncommittal term. The term "Edgar" has gained currency among some workers in the field. Others who are perhaps less creative continue to search for an explanation of psychic phenomena so that noncommittal terms won't be necessary. The controversy goes on.

3. According to parapsychologists, "displacement" occurs when a subject picks up something psychically which is more interesting than the target that the parapsychologists want the subject to pick up. It is a frequent source of frustration and is known in the medical literature as Parapsychologists' Disease.

PSYCHIC SURGERY: HOAX OR HOPE?

DAVID HOY

The tension in the small, crude operating room is intense. A young Filipina Woman, in an apparent state of fear and semi-shock is helped onto a long wooden table where she lies on her back, darting glances at the "surgeon" who is about to "operate" on her to remove a growth that she fears is malignant. There are none of the trappings of a conventional operating room; no sterile gowns or masks no apparatus for administering an anesthetic, no array of scalpels, clamps, scissors. The "surgeon," in his regular clothes, watches as an assistant unzips the patient's slacks and rolls them down to reveal the girl's naked stomach. The "surgeon" himself rolls up the girl's blouse to extend the operating field, and it was at this point that I saw the rapid movement of his hands as he palmed an unidentifiable object and placed it up under the girl's blouse. To an expert sleight-of-hand artist, the palming technique is crude; and I wondered if this, my first close-up exposure to a Philippine "psychic surgeon," is typical of what I will see in the course of my investigation. My concern is genuine because my colleagues in this research project are all serious scientists, anxious to prove or disprove the claims made by the "psychic surgeons," and I feel a deep responsibility to keep an open mind about what I am observing.

It is difficult to detail all the steps that led to my presence there in the deep Philippine country side as I watch a "healer" named Brother Nemesio G. Taylo as he prepared to pluck a supposed malignant growth from the body of this obviously terrified peasant girl. To position myself, I was included with this group because my objectivity in psychic matters has been attested to by recognized authorities in the United States, Canada and Brazil. In addition, I have a reputation as a student and practitioner of stage magic. Was I then, I asked myself, to blow the whistle on Nemesio Taylo on the basis of a single crudely executed sleight-of-hand maneuver? I decided on the spot to continue my observations but to withhold comment until I had seen other "miracle workers" perform. Another consideration entered my mind: I determined that I would not criticise any action that I could not reproduce myself as a sleight-of-hand expert.

While those thoughts were flashing through my brain Senor Taylo massaged the girl's stomach with a kneading motion as though he were mixing up a batch of bread. An assistant to Taylo then placed a small plastic bowl, partially filled with what looked like water, on the operating table in plain sight of us attentive observers. With a typical magician's gesture, Taylo showed us that his hands were empty, then dipped them into the bowl several times as he continued the kneading motion until the indentation he had made in the patient's stomach was filled with

the liquid. At that moment, he deftly reached up under the blouse and retrieved whatever it was I had seen him hide as the "operating" began. Red liquid suddenly appeared on the stomach, and Taylo asked for absorbent cotton to soak it up from the operating area. This was apparently the "cure," for the patient was summarily dismissed and another took her place on the table. Yes, I was disappointed; there was nothing spectacular, nothing dramatic in Memesio Taylo's performance. We were simply asked to believe that the red substance which materilized from the clear liquid contained the cause of the girl's illness. I was to learn later that other "healers" do produce even more spectacular, effects.

"Psychic surgeon" Juan Blanche practices "miracle healing" in a small chapel in Pasig, Republic of The Philippines, only twelve miles from modern, thriving Manila, the capital city. The distance in miles is short; the distance from what we know as modern medical practice is mind-boggling.

Juan Blanche's church was small. Ten rows of crude benches face an altar and a pulpit. A garish (in an American's eyes) banner identified Blanche's operation as part of the Espiritista Church. The "healer" himself is middle-aged, close to fifty, is portly and, I'm sorry to say, a crude, primitive butcher in his practice of the healing arts. Yes, I am reluctant to make such a judgement, but I am forced to by what I saw in almost three hours at his "healing center." His treatments produced evident pain in each of the sick people he treated that day.

His operating theater, off the chapel area, is a small room. An operating table was covered with oilcloth and a small cabinet and contained only bottles of "healing oils," cotton balls, matches, candles; simple paraphernalia. Juan Blanche stood behind the table most of the time, but he moved around the room and even left it at times during the course of a treatment.

The first patient I saw that day was a young girl with a severely abscessed area in the lower right part of her mouth. Blanche's treatment was to cauterize the area with a match stick wrapped in cotton and set afire by an assistant. The girl writhed in pain each of the four times Blanche touched the flaming cotton wad to her gums. The odor produced and the sizzling sound of burning flesh were, I confess, rather sickening although I am not a squemish type. It was actually the obvious pain the girl experienced that sickened me. This was the complete treatment.

Next, a young Filipino took his place on a stool next to the table and bent his head downward to receive his treatment for an ailment causing him severe neck pains. At that point my attention was drawn to typical sleight-of-hand movement as Blanche swiftly reached into his side pants pocket. To one trained to catch such movements, this is a classic palming move and, in this case, was not particularly well-executed. In another, quite expert move, Blanche drew his thumb quickly over the side of the patient's neck and then grabbed my hand and pantomimed a movement as though I were making a surgical incision without benefit of a surgical instrument. Almost immediately blood oozed out of the

"incision." Swiftly, Blanche placed a coin over the cut and placed a small wad of cotton on the coin. The cotton wad was set afire and small glass tumbler up-ended over the burning cotton. As the oxygen trapped by the glass was consumed by the flame, blood flowed from the wound. "Thick blood," Blanche announced; "thick blood causes pain. If I don't get it out, his arteries will close and he will die," he intoned. Perhaps. The entire process is duplicable, in my experience. The "incision" could have been made with a small piece of razor blade hidden under the thumbnail. Remember, I had already consciously noted Blanche's palming moves just before the cut was made. Involving me by moving my hand over the incision area was classic misdirection, designed to distract me to the point of forgetting to watch the "healer's" own hands. The upturned glass over the burning cotton wad is schoolboy "magic," as the oxygen is consumed and suction draws the blood into the glass. Finally, Blanche's description of the blood as "thick" and therefore the cause of the patient's illness was simply a statement that I, and the other observers, were expected to accept without question; as was his pronouncement that the patient would have died without this procedure.

Another operation by Juan Blanche was somewhat more spectacular. The patient, an older Filipina was taken into the side room. After a few minutes behind closed doors, Blanche beckoned George Meek and me to enter. The patient was stretched out on the table, her right pants leg rolled up to reveal a growth about the size of a small grapefruit on the inside of the leg, almost up to the groin area. A professional hemostat, or clamp, was already in place, and Blanche simply hacked at the growth with a double-edge razor blade until it fell off into the table with a "plunk" sound. The profusely bleeding wound was then crudely cauterized with flaming cotton swabs and strips of adhesive tape were applied. The blood continued to ooze and the woman groaned in pain. She stopped when Blanche harshly spoke to her in Tagalog--and the "operation" was over. I was not impressed by this latest demonstration. What I had seen was crudity, in my opinion. There was no attempt to sterilize the "surgeon's" or assistant's hands, the clamp or the razor blade, no anesthesia; in fact, nothing but the willingness to hack away at a growth that might or might not have been malignant! To my mind and training this was just too much to accept on faith--given the obvious hand trickery I had observed.

So it was that my first exposure to the highly-touted "psychic surgeons of the Philippines" left me with even more doubt than I had had when I undertook the research project. But, I was still trying hard to be objective, and I could not accept that the entire practice of so-called psychic surgery was based on such blatant fakery. I felt then, and still do, that the phenomena that were being passed off as miracles have deeper implications to a student and researcher in parapsychology.

The "psychic surgeons of the Philippines" are descendants of a long line of witch doctors, medicine men, necromancers and soothsayers, a line that reaches back to the earliest days of recorded history. What they have shared in common from the dawn of history until the present is the fact that their methods and their "cures" defy cold, scientific explanation.

But, and I want to emphasize this next statement, such lack

of rational basis does not rule out the possibility that truly miraculous results can occur outside the boundaries of orthodox medical practice. This view is shared to one degree or another all over the civilized world by an ever-increasing number of respected scientists in many fields - orthodox medicine, psychology, anthropology and sociology. We simply don't know where the real truth lies; the mysteries and the capabilities of the human mind are apparently limitless.

It is probably significant at this point to mention that my own prime interest is in the field of extra-sensory perception and my inclusion with the group researching the "psychic surgeons" of the Philippines was probably based as much on this competence as on my reputation as master magician. Extra-sensory perception, or ESP, begins with the principle that we use only a small portion of our basic five senses in our physical and intellectual lives. We see, but don't perceive; we hear, but we listen poorly; we touch, but we don't feel - these statements are part of the concept underlying the study of ESP and its possible application to our daily existence. So that in attempting to uncover possible fakery among the "miracle healers" of the Philippines, I was also deeply interested in how they created a psychological climate that impressed the respectable scientists in our research project. And it's a deep study which I can only outline here.

The beginning point is acceptance of the fact that we don't want to die. No sane person does. Medical doctors and orthodox surgeons will, in moments of frankness, confess to total lack of understanding as to why the human spirit fights to live even when faced by the most terrible physical evidence that the end is inevitable. Terminal cancer patients see themselves shrink to almost half their normal size and weight, endure the ravages of bedsores and painful reactions to medication; in fact, are fully aware that there is no hope within the power of orthodox medicine. That's where the doomed man or woman makes the transference from orthodox to unorthodox medicine. And that's the role that "miracle workers" fill - to take over when the doctors, the researchers, the laboratory technicians that make up the world of regular medicine all say there is no hope. All civilized belief systems are suspended; all critical faculties are pushed aside and the doomed patient and his anguished relatives turn to the world of jungle medicine.

I must here state that I have no desire to dismiss unorthodox healing out of hand, as impossible, as totally dishonest, as criminal exploitation of the seriously ill or deformed. Rather, that the particular practitioners of "psychic surgery" that I observed in the Philippines did resort to deceptive practices as part of their ministration to their patients. Whether they do indeed have miraculous healing gifts remains to be proved or disproved by methods that are not now within our ability to construct.

One manifestation of the scope of the Philippines' "healing" industry is the lavish "medical center" and resort purportedly owned and operated by Antonio C. Agpao atop Domingo Hill in Bagio City. Agpao is, in material terms, the most successful psychic

surgeon among the thirty or forty healers who practice in the Philippines. His installation runs along the spine of the mountain and consists of perhaps ten buildings, all well-built and carefully tended. There is an elegant hotel, bars, fine dining rooms and a night club here and "Tony" Agpao's staff is in keeping with the operation of so grand a layout. Many of his assistants are "true believers," and they come to study under him with high humanitarian motivation from all over the world, many from America. However, in spite of the fact that our group was a serious one with respectable scientific credentials, "Tony" was not available to us. He has, however, been the subject of several books, none of which, to my knowledge addressed itself to the question to which I sought an answer -- is trickery, deception, sleight-of-hand, inherently a part of faith healing? I was not, as I said, able to observe Agpao at work-- to my regret. Completely subjectively, I can report that by the time our group reached Tony's complex, I had the feeling that my cover identity had been blown and that any future opportunity to watch him at work would meet with the same lack of cooperation. Call it a psychic impression if you want, but there it is!

During the remainder of my stay in the Philippines, I was able to observe about six "surgeons" at work in addition to the ones I have already mentioned by name. In the next section of this survey, I will recap these experiences and attempt to explain how a skilled sleight-of-hand artist might duplicate many, if not all, of the tricks used by the healers. In fact, I will recount how I was able to demonstrate some of them to my own colleagues, to their considerable amazement.

My first exposures to famed "healers" was disappointing, to say the least. I saw flagrant examples of trickery, deception, misdirection and downright fraudulent psychological manipulation of desperate patients. I saw sleight-of-hand techniques used so crudely that they would have put the average beginning amateur to shame. From my stance as an objective observer, I was forced into the position of being offended at such shoddy use of classic skills; skills that are used to entertain people in our culture, not to mislead them in such a grave area as personal health and safety. Certainly, some of the "healers" were skilled, some even dazzling in the grace and effectiveness of their manipulative skills. But the skillful ones were the exception, not the rule. Thus, I was forced into subjective rejection of entire sets of belief systems that were unshaken by such flagrant fakery.

This is a point at which I must, for the record, lay out the fact that I am a strong believer in psychic phemonena, a recognized contributor to research in extra-sensory perception and a successful lecturer on the subject throughout the United States and Canada. In fact, my total intellectual and physical energies are directed at understanding the unknown and harnessing the staggering potentials of the human mind for the common good. So, my reaction to the demonstrations of so-called "psychic" healing were to me unconscionable on two levels: one, the obvious deception I've already described; and, two, the perversion of this quest for understanding paranormal matters for the benefit of a few at the cost of possibly untold thousands of sick people.

Also for the record, it must be stated that among my group of observers I was the most outspoken in my rejection of the idea that "miracles" were taking place. For example, one of the "healers," called Placito, specializes in what he and his followers call "spiritual injections." His treatment consists of reciting biblical passages to the suffering patient while transfixing him with an hypnotic stare. Oh yes, the patient is partially hidden behind a sheet held up as a sort of make shift screen by the sorcerer's assistants. At a high-point in this psychological buildup, Placito points his finger and, at this moment, the patient is supposed to feel a "zap" of psychic energy which will bring about a cure. Well, Placito cheats. In the charged atmosphere of misdirected faith, Placito finds time to reach around in back of the patient and give him or her a nipping pinch -- Voila! a "spiritual injection!" What turned me off on this classic bit of misdirection was that, on the occasion of my visit, the patient was the horribly deformed, congenitally crippled four-year-old son of a young and pretty Australian woman. Placito's "zap" had no effect, of course, and even though Placito had tried previously and failed, the young mother was planning to go back home to Australia, work as a waitress again to earn enough money for air passage and Placito's fee and return to subject the child to another "zap" session. Such is the intensity of hope kindled by desperation. True, there are many who would argue that such "miracles" are possible -- witness Lourdes -- but my point is that the "spiritual injection" as administered by Placito via a fairly crude act of misdirection places him outside the unknown forces that might eventually prove the possibility of miracle healing. I put my own feelings regarding Placito to a personal test and, yes, I did feel a zap; but I was also alert enough to ascertain that it was a quick nip on the back by a very human Placito that I felt, not a heavenly manifestation.

This survey was not conceived as a model of reportage and does not pretend to be an in-depth study of faith healing in the Philippines; rather, it is an examination of but one aspect of the subject -- the use of classic sleight-of-hand techniques as part of the "treatment." It would serve no useful purpose to name the other half dozen "healers" I visited or detail their particular skills as illusionists. However, each and every one of them made such tricks as I've described a greater or lesser part of their performances, some skillfully, some incredibly awkwardly.

It is one thing to be made aware that trickery is taking place. It is another to reproduce the act. So, before parting company with my group of hard-boiled scientific colleagues, I set out to do just that - reproduce the more spectacular parts of the healers' acts. Remember, I was in Manila, far from home, knew no one who could supply me with standard ingredients or props used by stage magicians and was pressed for time to perfect an "act." Nevertheless, by impregnating plain drugstore absorbent cotton with Burnt Sienna #2 and Carmine (red) #2 water colors (purchased at a Manila art supply shop) I was able to bring forth "blood" when I mixed "holy water" from the hotel water tap with the wads of cotton I had secreted in various out-of-the-way parts of my body. There was even a plus product from the mixture: I could see that some of the stained cotton wads could be passed off as "tissue" from the body were they to be deftly palmed and

introduced at the right psychological moment in a "psychic surgery" performance.

Faking an incision was another matter, I admit. Even the most courageous researcher might be reluctant to experiment with sharp instruments on his own hide. But the challenge had been made - that I would attempt to reproduce the effects used by the psychic healers whose performances my group of scientists had observed. With a little experimentation, I found that part of a standard double edge razor blade could be so sheathed and hidden beneath my thumb nail that a light, ever so light, cut on the skin of my right leg produced an incision that barely penetrated the subcutaneous layer. Moreover, the incision did not show for a full ten seconds and then a fine pinstripe of blood appeared and the flow continued to increase until a swipe with an alcohol-soaked swab stopped it.

The pain, incidentally, was minimal - about what one feels when he scratches himself with a rough edge of his fingernail. Next, I placed a coin on one of the "incisions," then placed a burning piece of cotton on the coin and inverted a shot glass over the whole megillah. The vacuum created in the air space under the shot glass encouraged a more rapid flow of blood, and it soon soaked the cotton thoroughly and even overflowed rather dramatically. The effect was nothing short of spectacular! Given the opportunity to create an air of mystery, something any competent magician can do quite easily, I felt that I could reproduce many of the more gory effects used by the healers. Actually, my only concern was for sterilizing my self-inflicted "incisions" so as to avoid infection in the hot humid atmosphere of the Philippines.

What did my experiments prove? In my opinion and because I was a trained sleight-of-hand practitioner myself and because I had detected many classic misdirectional moves used by the "healers," I was forced to the conclusion that "cures" produced by these means were at least suspect. Further, that if there were indeed supernatural forces at the command of these people, they weakened their credibility by using the crude methods of deception that I have described and can reproduce. It's a challenge I expect I'll be called on to defend some day and will do so under the proper conditions of scientific method and evaluation.

As my stay in the Philippine Islands neared an end, I was driven to deep introspection. I had come to the Islands with an open mind and in the company of men who I expected would bring the same degree of open-mindedness to the project. My discovery of fraud and deception among the primitive healers forced me to re-examine my entire set of belief systems. As a student and researcher in psychic phenomena, I was prepared to accept -- or at least admit the possibility of -- the proposition that miracle cures could occur outside the rational and extroverted Western frames of reference. I looked for some degree of commitment, on the part of the healers whose treatments I had observed, to metaphysical principles. I had hoped that I would sense a meaningful psychic bridge between the "surgeons" and their wretched patients. But I was disappointed on both counts. Instead, these

men and women were simply creating magical effects to provide a level of hope to patients that in fact did not exist. And the hardest pill to swallow was that the techniques of sleight-of-hand, misdirection and palming were so crudely executed as to offend the professional standards of even a mediocre stage magician. Still, thousands of the desperately ill and diseased were literally placing their lives in the hands of these crude practitioners of the healing arts.

I realize that this is a harsh judgement, and I make it very reluctantly. But I am forced to report the evidence of my own eyes because I sincerely believe that healing of this kind is a clear and present danger. It involves not only the deluded people who submit to the faked ministrations of the kind of "psychic surgeons" I've described, but it threatens to totally undermine the serious attempts of thinking men and women to complete the bridge between orthodox medicine and the unknown potential for unorthodox methods of helping mankind to better physical and mental health.

Unfortunately, my estimate of the powers of the "psychic surgeons of the Philippines," was not warmly received by my presumably objective scientific colleagues in this expedition. This is not the place to air all the opposing points of view of these thoroughly reputable men, but there was a common thread running through their reactions to my observations. The common thread was that many, many scientific disciplines are bursting out of the boundaries that have been blindly accepted in the past; that nothing, almost no formerly valid set of beliefs can be regarded as the final word. This spirit of investigation, of stretching beyond the perimeters of man's present knowledge extends even into the hardest scientific disciplines. Our theories of the nature of the universe itself are being reexamined. Physicists and astronomers, anthropologists and geologists, mathematicians and biologists are all in a ferment of creative rethinking and challenge. My colleagues shared this spirit of inquiry, in the main, but it was this very willingness to suspend established belief systems that, in my opinion, made them relatively easy marks for the skillfully faked performances of the "healers" we visited.

Time after time, the statement was made that "the camera doesn't lie," in reference to the photographic "evidence" that several of my colleagues displayed. It was difficult, if not impossible, to explain that the camera's "eye" is actually only the eye of the person taking the picture and that the tricks of misdirection, sleight-of-hand and palming of objects that are the main stock in trade of the "healers" are in no magical way revealed by even the most expert photographic techniques. There were many other instances in which, according to my personal observation, my colleagues had been hoodwinked by the "healers" like a country boy at a carnival.

The net result of all this -- I hesitate to call it "conflict" -- was that my role became one of a dissenter, though my negative observations were included in the book edited by the Philippine expedition's leader. [George W. Meek, ed., Healers and the Healing Process, 1977.] I certainly can live with that but feel that I have a duty to further expose my own views to the general public. I feel this duty strongly because I fear that the influence of the "Psychic Healers of the Philippines," is being merchandised aggressively all over the world because it has proved profitable to many strictly commercial interests. Certain travel agencies right here in the United States and in Canada, in cooperation with airlines and hotels in the Philippines have created "package tours" and, I expect, will continue to do so. It is, in my view, rather grisly to wring profit from the misery of so many thousands of desperately ill, crippled and diseased people.

I cannot end this survey without a firm restatement of my feelings for and knowledge of unorthodox medicine. Yes, I do believe that we are on the edge of a rich and fruitful era of discovery in parapsychological studies. Yes, I do believe that man's potential to harness psychic forces for the good of humanity cannot even be conceived let alone described. Yes, I do believe that many serious researchers are making worthwhile contributions to our understanding and that each step forward promises untold benefits to us all. Yes, I am commited to extending the frontiers of human potentiality and, in fact, devote my entire time to the subject through my lectures and writings on extra-sensory perception. In total, it is these very commitments that have motivated me to expose the methods of the Philippine "healers" in these articles and in my public appearances and in voluminous personal correspondence.

Fortunately, I am not alone in my search for fuller understanding of the nature and possible practical uses of psychic phenomena. Many members of the medical profession are openly admitting the failures of conventional medicine in treating the whole person, not just the obvious textbook symptoms of illness and disease. Many of these men and women approached the subject as skeptics and became convinced that healing phenomena do indeed exist. Among these is William A. Nolen, M.D., a Minnesota surgeon who is also an articulate and balanced writer. His search for truly miraculous healing abilities and his disillusionment with such practitioners as Norbu Chen, Kathryn Kuhlman and several of the same Philippine healers I had myself observed, is detailed in his 1974 Random House book, Healing: a Doctor in Search of a Miracle. Another articulate and compassionate researcher into the occult and its implications to the healing arts is Dr. C. Norman Shealy, a physician and director of the famed Pain Rehabilitation Center in LaCrosse, Wisconsin. His most recent (and dreadfully titled) book is, Occult Medicine Can Save Your Life, published by Dial Press in

1975. There are so many other serious researchers and writers on unorthodox medicine that it would be impossible to list them here. But, the serious layman is urged to plunge into further study of the subject with an open but skeptical mind lest he be hoodwinked by the tricksters who abound in the psychic field.

THEORIES, HYPOTHESES, AND SPECULATIONS ON THE ORIGINS OF UFOS

PART II

Due to an oversight, Dr. Richard F. Haines's comments upon J. Richard Greenwell's article on the theories and origins of UFOs in the last issue of ZETETIC SCHOLAR (#7) were inadvertently left out of the issue where they were supposed to appear along with the other commentaries. I apologize to Dr. Haines for the earlier omission and publish his comments here now. Mr. Greenwell responds to Dr. Haines along with the other commentators in his reply which follows. -- M. Truzzi

COMMENTS BY RICHARD F. HAINES:

Even an abbreviated review of the numerous books, papers, and other material on the origin of unidentified flying object (UFO) eyewitness reports requires far more space than is available here (see Catoe, 1969). Writer Greenwell is to be congratulated for attempting to reduce the major theories and other musings on the origin of UFO to but two categories. Nevertheless, I do not feel that adoption of a "conventional" and "unconventional" category is the best way to categorize the theories that have been proposed. Indeed, what is conventional to one may be highly unconventional to another; the paper (as now written) asks for disagreement and controversy at the outset. Yet there seem to be other difficulties as well. Rather than discuss each of the points presented I would rather comment on the following general points: completeness, originality, and validity of deductions.

With regard to the matter of whether this paper presents all of the available theories of the origin of UFO reports (i.e., completeness) most serious students of the phenomenon would answer no. To illustrate this the reader is referred to Sprinkle (1979) for instance, where others are cited. Yet Greenwell does not claim to discuss all of the available theories and hypotheses but rather "numerous hypotheses." An article such as this should be as complete as possible for the sake of the reader who may probably learn most of everything he will ever learn about the subject from this article.

Regarding the second general point, the question is raised, should a review article like this one be original? I think it should. Here is an ideal opportunity to raise critical challenges to each of the theories and hypotheses in a creative fashion, in an original way. The present format used to discuss the "unconventional" theories seems to mix what UFO might be (i.e., their possible identity) with where UFO might come from (e.g., inside the earth, underwater), I would have rather seen a review of these and the other theories dimensioned along other continua such as the amount of kinetic and potential energy required to produce such phenomena or the socio-cultural antecedents which are already known and documented and which are the same as or similar to the various bizarre phenomena reported as UFO.

The degree to which one's conclusions follow from the data depends upon the completeness of the data one uses, its validity, the logic one employs, and the originality of one's thinking. Greenwell appears to

come to the conclusion that the "Space Animal Theory" is unlikely on the bases that "it is doubtful that biological forms could evolve in space or even in the upper regions of the atmosphere..." (pg. 4). This argument may or may not be valid depending upon the presentation of a great deal of related microbiological and other data (which is not presented). Yet a second argument against the theory is given, viz., "The absence of oxygen for carbon-based life would also rule out biological space animals..." (Ibid.). Unfortunately we do not yet know for sure that all life forms are based on the same bio-molecular structure as exists on earth, i.e., carbon-oxygen-hydrogen, which greatly weakens the tie from the alleged facts to the conclusion.

Author Greenwell should not be held responsible for the original theories. But if he is going to review the bases upon which they were originally proposed it is imperative for him to produce valid deductions which derive from their alleged supportive evidence.

As I have pointed out elsewhere (Haines, 1980), the core nature of the UFO phenomenon may be represented by the resultant of a number of factors: UFO = a : b : c : d : e in which case nothing less than a systematic comparison, correlation, and cataloguing of the factors "a" through "e" will be required to understand the "UFO" term. And should another expression be found to be true such as: UFO = a : b (d-e)/f:-g then we must also find out the relationships that exist between each of the factors. While it is an epistemological question whether the phenomenon is determined by the manner in which mankind thinks about it this writer believes that the continued application of sound scientific practices will eventually uncover both the factors involved and their mutual relationship(s) in the "UFO" equation. Perhaps the various theories and hypotheses reviewed by J. Richard Greenwell represent different ways of viewing the same fundamental phenomenon and, therefore, represent some of the relationships that tie various factors together. If true then there should be something in common to all of these theories, viz, the core identity of the "UFO." But if these theories and hypotheses represent views of different phenomena, then the solution of any "UFO" equation will be vastly more difficult, perhaps even impossible. Greenwell's contribution does seem to provide a general overview of previous speculations that have puzzled mankind for years.

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REPLIES TO HIS COMMENTATORS

J. RICHARD GREENWELL

Introductory Remarks

I thank the many knowledgeable persons who agreed to participate in this Dialogue on the UFO problem. The original article on UFO "theories" was specifically written in a condensed form for The Encyclopedia of UFOs (Doubleday, 1980), but the critiques and comments in the preceding pages now provide me with an opportunity to expand my own thoughts in some areas, which, however tempting, I restrained myself from doing in the original article.

Some reviewers have gone beyond an evaluation of the "theories" discussed, and have addressed the UFO problem in other--and often more important--terms, so I will feel free to do likewise.

Response to George O. Abell

Dr. Abell believes my list of eight "theories" is incomplete, and he presents eight additional "theories." While these are all very interesting, the first sentence of my article clearly stated that I was assessing "theories" which have been advanced to explain UFO reports"; I am not aware of Dr. Abell's "theories" being proposed previously. In other words, I was assessing the "theories" which persons supposedly knowledgeable in UFO matters have advocated at one time or another. If Dr. Abell does not like those "theories," I can sympathize with him, but I refuse to be held responsible for their existence.

I generally agree with Dr. Abell's subsequent four paragraphs, although I think he tends to place too much confidence in the reliability of scientists. Too often, scientists are trained to be good researchers and/or teachers within a given discipline, but they receive very little preparation in the foundations of scientific thought and methodology, as well as in the status of other sciences outside of their own fields. Dr. Abell hints at the latter when he states that "scientists themselves cannot usually understand the papers of other scientists outside their own narrow areas of expertise." I will go further: the relationship between academic training and rational thought is often very weak. While we expect scientists, by their training, to be more rational, a doctorate, for example, is in no way a passport to rationality, and scientists commonly provide "scientific opinions" which have less to do with science than with personal (i.e. subjective) attitudes. When a scientist issues an "opinion," therefore, we should be very careful in ascertaining which hat he is wearing: his "science hat" or his "person hat."

Lest there be misunderstandings, I wish to make it quite clear that I am not "attacking" such scientists in order to extol the virtues of what Dr. Abell calls "members of the public." What I am stating is that, in some respects, scientists are also "members of the public," and we should not regard them as infallible guardians

of the truths of nature. For a recent critique of the almost-mythical qualities commonly attributed tc scientists (objectivity, rationality, open-mindedness, superior intelligence, integrity, and communality), see Mahoney (1979).

Dr. Abell then narrows-in on the Extraterrestrial Hypothesis (ETH), and states that it is "the average believer who has the emotional commitment to the ETH" and "it is blind committed belief in something that is narrow minded." I fully agree, and so indicated in my ETH section, which Dr. Abell chose to ignore. At the same time, Dr. Abell states that I "err" and do scientists "a disservice by implying that they have strong emotional feelings against the ETH." However, it is Dr. Abell who does me a disservice. Apart from his erroneous implication that "believers" are emotional and scientists are not, I did not state what he claims I stated, as any reader can easily determine. What I stated was that many scientists have strong anti-ETH feelings. As to how many "many" is, I will qualify it by stating that, overall, it is a minority. Most scientists do not have strong anti-ETH feelings simply because they don't give a tinker's damn about UFOs or the ETH. The reason for this is probably a social one. Most scientists work in areas totally unrelated to even the implications of UFO reports. Astronomers and physicists actually represent a miniscule component of the scientific world, although there exists a public misconception that those fields are science (a misconception which astronomers and physicists do little to dispel). It should therefore not be surprising that "most" scientists are disinterested in UFOs. I hope Dr. Abell is satisfied with this qualification.

Dr. Abell then goes on to discuss the question of extraterrestrial intelligence, and its possible relationship to UFOs. My main argument concerning his comments on interstellar travel is that he unfortunately limits his analysis to a matter/anti-matter system--far beyond our current state-of-the-art. Rather than discuss alternatives here, I refer the reader to a review of other, more practical propulsion systems (Greenwell, 1980a).

What I take particular exception to, however, is his overly optimistic view concerning the number of advanced intelligences in the galaxy. Dr. Abell seems to have fallen into the same trap as many other astronomers.

One may notice that those who take UFO reports seriously are branded as "believers" by Dr. Abell and some of his astronomical colleagues (while scientists, he states, remain "skeptical"), but that, when it comes to extraterrestrial intelligence, it is often these same astronomers who may be called the believers. Dr. Abell talks of "only" a million civilizations in the galaxy--as if he were being conservative--and refers to "informed guesses." Actually, we should refer to such estimates as "uninformed guesses," because they are based only on the astronomer's knowledge of astrophysics, astrogeophysics, astrochemistry, and astrogeochemistry, but almost totally ignoring evolutionary biology and related ecological factors.

The magic formula which lends respectability to such beliefs

is the Drake/Sagan equation:

$$N = R_* f_p n_e f_l f_i f_c L$$

where R_* is the mean rate of galactic star formation; f_p the number of stars with planetary systems; n_e the number of planets which are chemically suited for life; f_l the number of planets on which life has actually evolved; f_i the number of planets harboring intelligent beings; f_c the number of planets harboring intelligent beings at the stage of electromagnetic communication; and L the mean lifetime of such technological civilizations.

If one examines carefully the numbers that some astronomers plug into this equation, one may notice that a miracle occurs between f_l and f_i. There seems to be a curious belief among some astronomers (similar, perhaps, to the UFOlogist's belief) that, once life begins on a given planetary abode, it will, given enough time, lead to intelligence. There is absolutely no evidence whatsoever to support such a belief. On the contrary, there are far more substantial evolutionary reasons for accepting the proposition that extraterrestrial intelligence, if it exists at all, is quite rare.

The human species, for example, is almost certainly the result of a long series of chance events, some geophysical and climatic, some ecological and biological, and some social, the similar replication of which is so remote as to practically invalidate most of the optimistic arguments which can be marshalled in its favor--whether on Earth or elsewhere. What many persons often seem to forget is that the human species belongs to a remote Family of an obscure Order (the primates) of a recent Class (the mammals), one of several different vertebrate Classes. It should be remembered also that only the mammals (or their warm-blooded equivalent on other planets), with their long infant dependency, could produce a truly intelligent species. Yet many millions of kinds of organisms came and went on this planet long before the chance events ultimately resulting in our mammalian Order ever occurred. The later branches leading to the primates and to humans were also a result of many chance events.

Despite these critical facts, the three principal technical volumes on the topic of extraterrestrial intelligence gloss over this problem. In the first (Sagan, 1973), the chapter entitled "The Evolution of Intelligence" was the responsibility of a physiologist (who admitted that he did "...not have the slightest idea what it is that has caused one group of animals to evolve along a direction leading to higher intelligence...") and a virologist. In the second (Ponnamperuma and Cameron, 1974), the relevant chapter was the responsibility of a computer and information science specialist. The third (Morrison, Billingham, and Wolfe 1977) does not even contain a relevant chapter, but conveniently skips from "Cosmic Evolution, to "Cultural Evolution." Indeed, the conservative but more realistic appraisals of one of the few evolutionary biologists who has given the topic some attention, paleontologist George Gaylord Simpson (1964; 1973), have generally been ignored. As early as 1964, Simpson pointed out "...it is odd that evolutionary biologists and systematists have rarely been consulted."

and most eloquent popularizer of the idea of extraterrestrial intelligence, and it is he who has widely disseminated the original Drake equation discussed above. In the 1960s, Dr. Sagan produced the following factors for the equation: $N = (10 \times 1 \times 1 \times 1 \times 10^{-1} \times 10^{-1})$ $L = (10^{-1})\ L$. (Shlovskii and Sagan, 1966). We see here that he adopted 10^{-1} for f_i, which means that one out of every ten planets on which life has evolved will eventually harbor intelligence. (He also adopted 10^{-1} for f_c, which means that one out of ten planets with intelligent beings will develop a communicative technology.)

A more realistic analysis might give a much lower figure for f_i, and Dr. Sagan had such an opportunity eight years later. However, he changed his values to: $N = (10 \times 1 \times 1 \times 1 \times 1 \times 10^{-2})\ L = (10^{-1})\ L$ (Sagan, 1974). Here we find that f_i, rather than decreasing, has increased further, to 1; that is, practically every life-bearing planet will produce intelligence, given enough time! The fact is that only R_*, f_p, and n_e have any sort of evidence at all to support the introduction of values. Introducing values into the other factors is like pulling numbers out of the air. Nevertheless, Dr. Sagan comes up with $N = 10^6$, or one million civilizations in the galaxy -- the figure Dr. Abell and many other astronomers quote.

Dr. Sagan first admits that "...the number of fortuitous accidents which had to occur at the right time for man to develop the way he has is truly astronomical," but later, on the very same page, he states: "I believe we should adopt a value of unity for f_i; this amounts to stating that intelligence is an inevitable consequence of biological evolution, given enough time" (Sagan, 1974). No evidence to support this astonishing belief is made available by biology or by the geological record, but he is able to reconcile the two statements by unabashedly declaring that, while the odds of producing the human species were astronomical, other intelligent extraterrestrial beings would not be human, but more "general intelligent beings"! He states that the "details of structure and evolutionary timescale [of such beings] will be determined by the environment in which the intelligent species develops," and we are thus left to assume that the environmental conditions prevailing on other planets were much more favorable for the evolution of intelligent species than they were on Earth.

Space limitations prohibit even a light review of all the many evolutionary and ecological factors involved in the evolution of intelligent species on Earth, but we may touch on two of the more interesting ones. Perhaps the most critical event affecting our evolution was the large extinction of biota at the Cretaceous-Tertiary (Mesozoic-Cenozoic) boundary, approximately 65 million years ago. About 70% of all living species (terrestrial and aquatic suddenly disappeared. Numerous hypotheses have been advanced to explain this remarkable catastrophe; they have been recently reviewed by Russell (1979).

Three new hypotheses have since been proposed: 1) Arctic spillover, in which brackish Arctic water moved over the Earth's oceanic surface, resulting in the extinction of plankton, and a lowering of atmospheric temperature, which, in turn, resulted in large biota extinctions (Gartner and Keany, 1978; Gartner and McQuirk, 1979); 2) a greenhouse effect, in which marine algae were drastically reduced by nutrient depletion, caused by a regression of epeiric

ZS#8 ERRATUM: Top of page 52 should have the line:
Probably all can agree that Carl Sagan has been the principlal

seas, resulting in a large increase in atmospheric carbon dioxide, which, in turn, resulted in an increase of low atmosphere and terrestrial temperatures and large biota extinctions (McLean, 1978); 3) an asteroid impact, in which an Apollo object (+1km) hit the Earth, creating a large blanket of dust in the stratosphere for several years, which resulted in photosynthesis suppression, and, as a consequence, the collapse of food chains and large biota extinctions (Emiliani, 1980; Alvarez, et al, 1980; Ganapathy, 1980).

All of these hypotheses have problems, but the new 10 km. asteroid scenario seems to be the most adequate. Whatever the cause of the extinctions, the record indicates that no terrestrial animals heavier than 25 kl. survived, which means that the dinosaurs (whether endothermic or ectothermic) probably disappeared at that time. The importance of all this to our discussion is that, as many plants regenerated from seeds, spores, and roots, photosynthesis increased, and a vast number of new nitches suddenly became available to the survivors of the cataclysm. One of the small survivors, a relative newcomer, was the mammal, and his subsequent radiation, made possible by a freak geophysical event, set the stage for subsequent human evolution. One may ponder over what "primitive" biota would be dominating our planet today had the cataclysm not occurred. Certainly, there would not be an intelligent species.

If we look at the late Miocene, we will find another fortuitous event. Some of the small mammals had fortunately adapted to arboreal life, resulting in the grasping hand/opposable thumb, visual stereoscopy, and upright posture for brachiation. These primates were very well adapted to the trees, but most of the species were generalized enough to re-adapt to other habitats. The trick was to get them back down on the ground,

The fortuitous event was the thinning out of the tropical rain forests of what is now East Africa and Southwest Asia. Fossel flora indicate that lowland rain-forest and woodland were found throughout East Africa in the early Miocene. By the end of the Miocene, a number of geophysical events changed the East African landscape dramatically (Andrews and Van Couvering, 1975). Major rifting and sedimentation occurred, and a chain of large volcanoes rose up on the eastern side of the rift valleys. The ensuing climatic changes resulted in the splitting-up of the rain-forests (this is indicated by shared genera of birds, small mammals, and plants), the widespread propagation of grasses, and the establishment of arid-adapted plant communities (Van Couvering, 1980).

As the rain-forests began being replaced by arid woodland and bushland, primate species began finding themselves increasingly constrained. In a scenario I prefer (Greenwell, 1978), those which successfully retreated with the trees lived to tell the tale (represented today by numerous species of monkeys). Others were caught in diminishing arboreal pockets, and perished. But a few ape groups managed to cross short savannahs to other, still-existing pockets. Terrestrial locomotion thus began to have survival value, not for an end in itself, but merely as a vehicle for reaching new havens, where fierce competition for scarce arboreal resources

inevitably took place. These havens were dominated by the strong, the weaker species being forced to subsist in marginal woodlands or the arid plains themselves. The stronger species (represented today by the great apes) succeeded-- but became evolutionary dead ends. The weaker species -- the failures -- evolved into more intelligent savannah bipeds, and one of their branches eventually led to Homo sapiens.

Another important factor in this unlikely sequence of events has to do with timing. Had the dessication come earlier, before upright brachiation and bipedal potential had been well developed in our ancestors, quadrupedal locomotion would have been sufficiently adaptive, and "we" would now be something like baboons. Had it come later, after brachiation had been well developed (like with today's specialized gibbon apes), adaptation to the savannah would probably not have been possible -- and our ancestors would have perished. But they just happened to be at the "right" place at the "right" time when the "right" event occurred. The likelihood of similar events, in similar sequences, affecting similar organisms on similar planets is extremely remote. A slightly different event here, or a slightly different time sequence there, and this planet would certainly not be the abode of a technologically intelligent species today.

If some astronomers want to attribute "directed purposefulness" to organic evolution (i.e., that physiological complexity up the phylogenetic scale increases because of a controlling higher consciousness, rather than because of successful adaptation to new ecological nitches made available by geophysical events), they are free to do so within the framework of their personal, religious beliefs. However, they should not attempt to cloak such beliefs in scientific respectability, while, at the same time, deriding UFOlogists for their beliefs. In fact, although this may appear contradictory to Dr. Abell, one could go so far as to state that the evidence for UFOs (whatever UFOs are) is much, much stronger than the evidence for extraterrestrial intelligence. It is an irrefutable fact that many thousands of human beings have at least claimed to have seen UFOs (and that many thousands of UFO reports exist, some involving electromagnetic forms of evidence), while no astronomical claims or reports whatsoever exist to support the existence of a single extraterrestrial civilization, much less a million. Which belief of the two is more rational?

Dr. Abell concludes that we should "not pin our hopes on shoddy evidence, hearsay, and wishful thinking." This is stupendous advice, and I urge all UFOlogists and all astronomers to follow it.

Response to Jerome Clark

I cannot agree with Mr. Clark that UFOlogy "has yet to produce one [a "theory"] that is even interesting." Despite the difficulties involved, or maybe because of the difficulties involved, I find the ETH and the Time Travel Theory very interesting. If UFOs do represent extraterrestrial intelligence of some kind, it would demonstrate that the peculiar conditions necessary for both the evolution of life and the evolution of intelligence are not unique to Earth. This may seem

like a triviality in these days of Star Wars sagas, but the question is not only interesting but also an immensely important scientific and philosophical one. I tend to agree with Mr. Clark, however, when he postulates that, even if the ETH is correct, "they are not here in quite the same way" that many UFOlogists think, and that there has been a tendency to anthropomorphize these presumed extra-terrestrials.

It is apparent that Mr. Clark has now abandoned the Phychic Projection Theory, and bases his new position on the premise that UFOs do not touch "any psychological, mystical, religious, or social chord in human beings," and that they interest people simply because they happen to be novel and exotic. He goes on to discuss some reasons for this, and I certainly agree with many of his points. The cultural UFO myth must compete with a wide range of other stimuli constantly calling for our attention (professional, recreational, sexual, etc.), and the only reason the myth component of the UFO phenomenon has survived at all is probably because of continuing UFO reports.

Whether this "says something...about their existence as an objective, independent phenomenon" is another question. I am not sure that it does. Do UFO reports continue to come in because witnesses have been enculturated as to what to perceive in a "UFO" (domed disk, etc.), or do some observers see truly anomalous stimuli which, when translated into written reports, appear very similar to the myth-generated reports?

Response to Daniel Cohen

I thank Mr. Cohen for bringing to my attention the fact that the Hollow Earth Theory became popular in the early nineteenth century. I must admit that I was not aware of this critically important fact. Concerning Dr. Raymond Bernard, Mr. Cohen may suspect what he wishes, but I refer to works by published author's name, unless there is documentation to do otherwise.

I regret that I failed to include Mr. Cohen's favorite quote by the late naturalist Ivan T. Sanderson. In truth, Mr. Sanderson had so many delightful and quotable statements that it is hard to select just a few. Mr. Sanderson was a highly imaginative and colorful person, of whom I was personally very fond. I think the finest description of him in my files, by a biologist, is: "He never let a fact get in the way of a good sentence."

As for the "real point of debate" over the ETH, Mr. Cohen ignores the "human" factor when he states that the real debate is over the quality of the evidence. On the surface, this may appear to be so, but many in both camps (believers-proponents and debunkers-skeptics) bend and distort the evidence to fit their favored explanations. Sometimes, it matters little how good or how poor the evidence is for a particular case because psychological mechanisms will impede an unbiased assessment. My point was that the sheer number of UFO reports creates an atmosphere in which few scientists will take any UFO reports seriously. Believers/proponents, not realizing this, continue to publish voluminous amounts of reports, assuming that their mass will eventually be too overwhelming for scientists to ignore. This "debate"

is not at the visible (or conscious) level because, as I stated, those involved in it are generally not aware of this psychological system as it operates. Instead, they talk of the visible (or conscious) system, the so-called debate over the quality of the evidence which Mr. Cohen refers to.

The psychological factors involved in the UFO controversy are substantial, and I must agree with the position of astrophysicist Richard C. Henry, that "...the first problem presented by the UFO phenomenon is a human or psychological one, and one that has very little to do with what UFOs are or are not" (Henry, 1980). For further discussion of these topics, I refer the reader to other publications (Greenwell, 1980b; Greenwell, 1980c).

Except for Flatwoods, I know of no UFO reports involving "monsters" (whatever they are), or the Virgin Mary, unless one accepts Fatima. Bigfood has sometimes been linked to UFOs, but I know of no report where this linkage is reliably strong.

As for Mr. Cohen's main objection, that I touched too lightly on the shift by some UFO authorities from the ETH to the Ultraterrestrial Theory, I can only respond that this was not the purpose of my article. I covered eight "theories" in about 3,500 words, which allows an average of 440 words per "theory." The Ultraterrestrial Theory had about 410 words, so I will add about 30 more words here: I tend to agree with most of Mr. Cohen's statements (in his last three paragraphs). His approach is more historical, while mine relates more to the method of assessment; i.e., how can one evaluate the reasonableness of the Ultraterrestrial Theory, or of its various subcomponents? If this is Mr. Cohen's main objection to my piece, I suppose I must be flattered.

Incidentally, I am very pleased that I have "convinced" Mr. Cohen that there is no hole in the pole.

Response to William R. Corliss

I am quite sympathetic with Mr. Corliss' comments. First, I would like, if I may, to disassociate myself from those who are "clustering around" a major anomaly, and ignoring the "shaky structures" of others. It was not the purpose of my article to address other anomalies, although I have a continuing interest in the historical evolution and psychology involved in many of them, as well as in their interaction with the mainstream of science.

However, I am not sure that a "spectrum" of anomalies is an appropriate model. When Mr. Corliss criticizes anomaly students for treating their anomalies as "well-isolated," and when he refers to the UFO phenomenon as "part of an indivisible spectrum of other anomalies that differ only in degree, flavor, strangeness, and outrageousness," is he implying that all these anomalies are genetically related (that is, it is their nature or cause that are related, rather than the perception of their anomalousness by human beings)? If the answer to this question is "no," then I agree that such a spectrum can serve as a useful tool in looking at the whole waterfront of anomalies, but recognizing that some may be

biological in nature, others geophysical, others astronomical, and many, of course, purely psychological (although they may appear otherwise).

If, however, the answer to the question is "yes," then I have difficulty with the spectrum model. I think we can all agree that meteorites and the coelacanth had/have totally different causes/origins; yet, before their scientific acknowledgement, both had their places on the Corliss spectrum of anomalies. One was/is a physical phenomenon (as ball lightning may be), and one was/is purely a biological phenomenon (as the Loch Ness Monster may be). At the risk of being called an "anomaly snob," I will venture to regard these as "isolated phenomena."

In my judgment, the only way in which all anomalies on the spectrum could be genetically related would be if they all had psychological causes, and there are many scientists, I am sure, who would happily accept this proposition. I do not think, however, that this is what Mr. Corliss is proposing.

Response to John S. Derr

I agree with Dr. Derr concerning the importance of examining transient geophysical phenomena as an explanation for many UFO reports. In fact, Dr. Derr may recall that, about 10 years ago, I urged him to pursue analyses of earthquake light reports in order to determine their relationship, if any, to UFO reports.

The reason I did not include transient geophysical phenomena in the "unconventional" category (which were the only "theories" reviewed) is because of my criterion, since highly-criticized, that "conventional" "theories" do not involve purposeful intelligence, while "unconventional" "theories" do. Insect swarms and birds (either individually or in flocks) are "conventional," yet they have purposeful behavior, if not intelligence, perhaps akin to hypothetical space animals (which are "unconventional"); thus, my classification scheme has begun to crumble.

Dr. Derr is correct in pointing out that transient geophysical phenomena are unconventional from the perspective of our current knowledge of geophysics, and the same could be said for ball lightning. However, like meteorites, such phenomena could easily fall within the mainstream of "normal" science without too much trouble. That is, however unknown the physical mechanisms are at the present time, we "know" that the ultimate description of these mechanisms would result in new but relatively conventional sciences. At the same time, we "know" that with extraterrestrials, time travelers, etc. they would not. This was part of the reasoning behind my rather subjective division of "theories" into the "conventional" (no purposeful intelligence) and "unconventional" (purposeful intelligence) categories.

Response to Charles Fair

Mr. Fair's arguments concerning the different "theories" are, I

think, quite reasonable. Concerning the ETH, however, I would point out that we are probably not talking about extraterrestrials whose evolution "just happened to be synchronous with ours." On the contrary, if intelligent beings exist elsewhere, the statistical odds are that they would be enormously ahead of us -- so much so that it may be almost futile for us to attempt to rationalize what they would, or could, do (or, for that matter, what they would not, or could not, do). As to not detecting intelligent radio signals, it could be that electromagnetic forms of communication represent a stage which lasts for only a certain period, perhaps being displaced by other methods (a proposition once made by Carl Sagan himself). In other words, the SETI approach may, quite literally, be like whistling Dixie in the Cosmos.

Finally, why is it, indeed, that a large segment of the U.S. population (not to mention the rest of the world) is irrational? While I have some thoughts on this, which would digress too far from our topic, I would point out that we must be very careful not to confuse "intelligence" with rationality. Many "intelligent" persons (i.e., persons scoring high on I.Q. tests) can be quite irrational, and this can include university professors! Other individuals, on the other hand, can possess only a mediocre "intelligence" and be strikingly rational.

Response to Roberto Farabone

I thank Dr. Farabone for his comments. He makes the interesting statement that in "no other branches of knowledge have so many hypotheses been made 'a priori.'" One must first question whether UFOlogy is "a branch of knowledge." In fact, one could reverse Dr. Farabone's assumption, and state that the number of hypotheses (if we may momentarily call them that) should say something about whether UFOlogy is or is not a branch of knowledge. Later in the same paragraph, Dr. Farabone implies that UFOlogy is an "unknown" -- which can hardly be a "branch of knowledge."

While Dr. Farabone correctly states that we should not create pet theories, lest some are produced "which are not verifiable and which cannot be falsified," it is proper procedure in science to formulate some sort of working hypotheses which can be tested, even if such testing is beyond the current state-of-the-art. I think a few of the "hypotheses" I reviewed qualify in that respect. Others are more fanciful, and probably never will be falsified.

Response to Lucius Farish

I am in general agreement with Mr. Farish's comments, and as he thinks that I have "done a good job in summarizing the leading UFO theories," I had better not muddy the waters further. I will state, however, that, in connection with the ETH "Volume of Traffic," I do not attempt to judge which side is "right." I simply provide an explanation

of why there is a "debate." The fact is that scientists have not the fogiest idea of the level of technology possible extraterrestrial civilizations may have attained, or of their life-spans, their motivations, or their intentions. They cannot even be sure that extraterrestrial intelligences exist at all, much less how (or how often) they would visit Earth. While many of their arguments are reasonable, I think many astronomers are far too liberal in their estimates concerning the number of extraterrestrial intelligences in the galaxy (see my response to Dr. Abell).

Response to Stanton T. Friedman

Mr. Friedman gets to the point quickly by stating that the ETH is not an assumption, that "the very peculiar (UFO) flight behavior and appearance TOGETHER indicate they are manufactured elsewhere than on Earth." The word to note here is "indicate." They certainly do not demonstrate their extraterrestrial origin, so the "indication" has to be an assumption, or, to some, even a belief. Now, I am not stating that UFOs do not, or cannot have, an extraterrestrial origin; but it will take more than the existing reports of visual sightings, radar tracks, and even landings and "abductions," to make their extraterrestrial origin deducible.

I am acquainted with the reasoning behind Mr. Friedman's time-distance arguments, and have presented these sorts of data myself (Greenwell, 1980a). Concerning the "Volume of Traffic" question, Mr. Friedman has perhaps misunderstood my statements. I did not use the "Volume of Traffic" model to demonstrate the unlikelihood of UFOs being extraterrestrial vehicles. What I attempted to do, as a careful re-reading of that section will show, was provide a reason (among others, I am sure) why scientists do not take UFO reports seriously. Instead of responding as to whether or not he thinks the model is technically valid, he sould have responded as to whether or not he thinks the model has affected the attitude of scientists.

Mr. Friedman goes on to propose that the extraterrestrials are monitoring us, due to our warlike ways, prior to our own departure for the stars. That may be so, but why would they reveal themselves so openly at times? He also states that "...the vast amount of available evidence...indicate(s) 'beyond a reasonable doubt' that SOME flying saucers are ET (extraterrestrial) in origin." Again, we have the word "indicate." I do not deny that the available evidence indicates an extraterrestrial origin to many people--but that does not make it a fact. (Furthermore, how can any evidence "indicate beyond a reasonable doubt"? That is a contradiction in terms.)

Finally, Mr. Friedman quotes my statement: "No proof of ET visitation has been produced," and adds that "[that] may be true but is hardly relevant." My judgment is that it is very relevant.

Response to Richard F. Haines

I defer to Dr. Haines on the question of conventional and unconventional categories. The reasoning behind the construction of these categories was given in my response to Dr. Derr.

Regarding completeness, Dr. Haines is also correct in that the article does not represent all the UFO "theories." However, the article was "as complete as possible" for the medium for which it was intended (The Encyclopedia of UFOs). Even so, I think the major UFO "theories" are discussed. On originality, I am very sorry if my article was not "creative" enough, but, again, this is partly excusable by the medium for which it was intended. On the other hand, I am not sure that I have the capability to dimension the theories along a continuum reflecting the energy requirements or the socio-cultural antecedents involved. Perhaps Dr. Haines can meet the challenge, and undertake this ambitious study.

Dr. Haines' third point concerns validity of conclusions, and he questions my conclusion on the space animal theory because I did not present "a great deal of related microbiological and other data," and because "we do not yet know for sure that all life forms are based on the same bio-molecular structure as exists on Earth." First, if Dr. Haines wants to research this topic in more depth, he is free to do so. I do not have the time. As for the second statement, we do know that hydrogen is the most common element in the Universe, and I think my assumption that other life forms (if they even exist) are carbon-based is not unreasonable.

The main concern of Dr. Haines seems to be that I have arrived at certain conclusions without providing the documented basis for such conclusions. I can only answer that this was not a research paper, and that my review was based on the best information available to me within the existing time constraints. Furthermore, Dr. Haines may notice that I did not state space animals were impossible; I was careful to use the word "doubtful."

Response to Allan Hendry

Mr. Hendry has cleverly shown one of the sorts of problems one must deal with when examining the UFO question; namely, how ball lightning can be regarded as "unconventional" as the reported UFOs (which some people have tried to explain in terms of ball lightning). That is, it is easy to solve one problem by creating another. But is it good science? It is really a psychological problem. A UFO debunker may claim that one cannot compare the "unconventionality" of extraterrestrial flying saucers with the "unconventionality" of ball lightning: whereas the saucers would represent directed, purposeful intelligence, no matter how bizarre ball lightning may be, or however much they seem to violate the known laws of physics, at least we "know" it is a geophysical event, and not one directed by extraterrestrial intelligence (see my response to Dr. Derr).

It seems, however, that both hypotheses (extraterrestrial saucers and ball lightning) violate our known laws of physics, and that the selection of one over the other is therefore necessarily made on

psychological grounds. I fully concure with Mr. Hendry's last paragraph.

Response to Elaine Hendry

Dr. Hendry points out the importance of the repeatability of data in scientific analyses, and states that one option would be to abandon the methods used in science to accomodate the special nature of UFOlogy, a proposition she does not favor.

First, we should remember that there is repeatability in UFO reports, perhaps not so much in structural descriptions as in behavioral descriptions (i.e. falling leaf motion, 90^{o} angle turns, instant accelerations, soundless hovering, electromagnetic interferences, etc.). But now two qualifications are in order. First, the repeatability of an effect does not necessarily imply its "unconventionality"; if UFOs are the result of psychological and perceptual mechanisms, there is no reason to expect these mechanisms not to keep producing the same types of reports, thus providing some form of "repeatability." But the repeatability Dr. Hendry refers to relates more to predictable repeatability. As UFOs represent naturalistic phenomena (i.e., unscheduled field observations), they are not predictable, and I agree that science should not accomodate its methods to meet the special demands of one particular area of inquiry. That does not mean, however, that science (or some individuals "representing" science) cannot attempt to gain new UFO instrumented data within the framework of its current, highly-successful procedures.

I fully concur with Dr. Hendry concerning the status of UFOlogy, and how the lack of training on the part of UFOlogists can (and undoubtedly has) resulted in the erection of UFOlogical edifices without any solid foundations whatsoever, and, as pointed out by Dr. Hendry, this is particularly true in the realm of psychology. What I find of special interest is how both UFO proponents and skeptics have, over the years, resorted to pop-psychology "theories" to support their arguments, and this tactic is not restricted to non-scientists. In fact, a number of skeptical astronomers have resorted to providing both psychological and sociological explanations for UFO events, although they have had no training whatsoever in these disciplines.

I hope Dr. Hendry will forgive me for presenting and assessing these popular UFO "theories." I promise never to do it again! By the way, I have fallen in love with her term "reverse temporal provincialism."

Response to Richard C. Henry

I am intrigued by Dr. Henry's dividing up of the "unconventional," purposeful intelligence "theories" appearing in my article into three categories: 1) less intelligent; 2) equally intelligent; and 3) more intelligent. However, as indicated in my response to Dr. Derr, I should have given more thought to this classification scheme. Birds,

for example, have "purposeful behavior" (although I would not call them "intelligent"), and have often generated UFO reports; by my own definition, then, birds should be in the "unconventional" category, which would be absurd.

Dr. Henry goes on to raise some extremely important questions concerning the position of man in the Universe. I know from personal experience that he has done much thinking in this area, and it is my hope that he will, one day, present these thoughts in an expanded form. He asks just what can we not exclude when considering a billion-year-old civilization. It is a fascinating question. Would it still be a "civilization" in our sense of the word? One could speculate that information processing in their brains (if they still had "brains") would be very rapid, unlike our plodding pace; it takes me hours just to read the Zetetic Scholar. Perhaps the need for sleep, the aging process, even the need for biological bodies, would have ended long ago. It is all speculation, but these types of questions are also relevant to the future of Homo sapiens.

Response to J. Allen Hynek

Dr. Hynek's third paragraph is probably the most relevant single statement one can make concerning the UFO problem, particularly the question: "Did the reported UFO event-details actually happen as reported?" If we could have complete confidence in human observers over repeated instances, like we have in a barometer or a thermometer, the answer would have to be "yes." But, even though the human visual sensory system is far more sensitive than any man-made instrumentation, we cannot have such confidence, primarily because the human perceptual system is very much subject to socio-cultural influences -- including the cultural myth of flying saucers. This cultural myth certainly exists. The question is: is there a real physical phenomenon separate from the myth, but which can easily be interpreted as part of the myth.

I must bow to Dr. Hynek's expertise and long experience with the subject when he states that the probability is "high" that the answer to the fundamental UFO question is "yes." However, I myself prefer not to assign such probabilities until we better understand the world and Universe around us, including a whole myriad of physiological and psychological processes in the human species.

Response to John A. Keel

It seems that Mr. Keel is way ahead of all of us. I am sorry that he finds my article obsolete in both concept and content. Being a professional journalist, Mr. Keel must have seen an enormous amount of trivia in his time. In fact, one could reasonably assume that he is somewhat of an expert on the topic. I have just one question: what are "the enormous advances that have been made in the past decade"? I must admit that I am "completely unaware" of them. Mr. Keel cannot possibly hope to educate us by keeping us all ignorant of these advances.

Response to Bruce Maccabee

Dr. Maccabee's comments are fairly straightforward, and I have little to add. The problems with my classification scheme are discussed in my reponse to Dr. Derr, so I will not repeat them here.

The ETH is Dr. Maccabee's preferred hypothesis, but he leaves the door open to the possibility of another physical phenomenon, of an "unintelligent" nature, being responsible for UFO reports. His support for the ETH is based on "descriptions of objects which appear to be machines capable of traversing our atmosphere." The question which ultimately has to be resolved is whether or not witnesses descriptions of such "machines" are accurate, and not just resolved to the satisfaction of the believers-proponents or the debunkers-skeptics. We have to reach a situation one day in which all reasonable persons will agree on this point, even if that day is still another 30 or 50 years away. It is probably the most critical question in UFOlogy. Unless dramatic new evidence (i.e., physical evidence, multiple witness photography) is produced, human testimony will continue to be the main source of evidence, and we are going to have to apply much more expertise from the psychological sciences to the problem if the above question is to be answered.

Response to Paul McCarthy

I thank Dr. McCarthy for his kind comments, and agree, as I have already done, that the "conventional" and "unconventional" categories can be somewhat misleading because of the "intelligence" or "non-intelligence" criterion I utilized.

Concerning the definitional problems of IFOs (identified flying objects) and UFOs, I refer the reader to a previous discussion of this topic (Greenwell, 1980d). I tend to agree with most of Dr. McCarthy's other remarks, so I will not pursue them further.

Response to Aimé Michel

Some interesting questions are raised by Mr. Michel concerning the lack of visibility on the part of the supposed supercivilizations which "must have arisen in the galaxy." Some postulate that the lack of visibility rules out extraterrestrial intelligences altogether. Arguments for and against extraterrestrial intelligence have recently been advanced by Hart (1975), Jones (1976), Kuiper and Morris (1977), and Schwartzman (1977).

Mr. Michel postulates that the solution to the problem may be found in UFO reports. That is, the extraterrestrials are making themselves visible in ways we do not (or will not) understand. It certainly solves the riddle posed by the skeptics: "UFOs cannot represent extraterrestrial visitors because if extraterrestrials existed they would visit us"! This "UFO hypothesis" has also been advanced by Schwartman (1977).

Pierre Guerin, the French astrophysicist who directs that country's official UFO study, has made an interesting case for extraterrestrials directing the UFO phenomenon, although UFOs may not really be what they are made to appear to be, i.e., spaceships (Guerin, 1979). Dr. Guerin states: "An intelligence, which is not our own, directs the UFOs. Everything takes place as if this intelligence knows our own degree of scientific-technical evolution very well, and gives to UFOs forms which will appear plausible to the witnesses in part because they actualize, in each historical period, the human technological dream of the moment."

Response to James W. Moseley

My thanks to Mr. Mosely for his kind remarks. As usual, he proves himself to be one of the sagest observers of UFO matters.

Mr. Mosely also raises a very important question in his third paragraph concerning the UFO case, one case with all the correct components represented, rather than 100 cases sharing the necessary components between them. But no such case exists. Is there at least a consensus among UFOlogists on which is the best case? Surely, out of the thousands and thousands of case reports on file there must be one which all UFOlogists agree is the most outstanding. I can report that Ronald D. Story has worked on this problem in preparation for a forthcoming book (Story and Greenwell, in press). Story requested over 100 leading UFOlogists to identify what they considered to be the best cases on record. No consensus exists. The "winning" case (the report from Father William B. Gill, at Papua, New Guinea, 1959) received only seven endorsements; 36 cases were cited in total. Many individuals refused to answer because they considered it too risky to put all of their bets on one case. Nobody likes to be made a fool of, except a fool, and what if their selected case turned out to have a mundane explanation after all? This could also invalidate the thousands of other cases that may be authentic!

Mr. Moseley and Mr. Story have addressed one of the very fundamental problems of UFOlogy, but I am not sure that I have the answer.

Response to James E. Oberg

I agree with Mr. Oberg that "Hollow Earth" and "underwater" are postulated base locales, not necessarily "theories" of origin. However, in reviewing the literature on UFOs, one finds that they are, in effect, treated as "theories"; that is, there are UFO books written on Hollow Earth and Underwater Civilization "theories." There are no UFO books on Antarctica, etc.. As for the photos which show the supposed holes in the poles, I suppose (and hope) that Mr. Oberg is aware that those photos have a very mundane explanation.

Mr. Oberg's second paragraph inadvertantly touches upon another point of interest related to the UFO topic. Mr. Oberg admits that he is a "believer." His belief is that UFOs do not (or cannot) represent

extraterrestrial visitation. Is this belief necessary? The proper approach, from a scientific perspective, is to wait and see. There is no need to reject the ETH, nor should there be. Science is not in a position to issue verdicts on subjects outside of its domain; all science can do at this time is wait. Future evidence may bring the UFO phenomenon into its domain, or the phenomenon may eventually be explainable as a socio-psychological one. In the meantime, there is no need to reject the existing evidence. As for our beliefs, and we all have them (Greenwell, 1980b), we should at least try to keep them separate from correct procedure in science.

Concerning the inappropriate use of the word "theory," I made this clear in the third paragraph of my article. I also agree that "the conventional explanation...does in fact explain a larger fraction of reports than any of the other 'theories.' " The question arises, of course, concerning an explanation for the fraction which cannot, or have not been explained by conventional means. The word "cannot" may cause me trouble with Mr. Oberg because he may think that he or others can explain them all. But can they be explained 1) to the satisfaction of all reasonable observers (i.e., not just the debunkers); and 2) without altering the soft data provided by the witnesses? Reasonable observers will be willing to alter some of the data, because they are aware of the unreliability of human perception and memory; but where do we draw the line? At what point do reasonable observers say: "Hold it! If you change the data that much, you can explain anything away, and there is then no real purpose in analyzing the case in the first place." These are critical questions which all individuals involved in the UFO field should come to grips with in the future if a resolution is ultimately to occur.

Mr. Oberg seems distressed that I did not discuss how UFO reports are "distorted, exaggerated, falsified, and selectively edited by the UFO media," and that "that question is left for later discussion." This was not the purpose of my article; furthermore, the topic has been addressed previously, and not only by debunkers. Ron Westrum (1977) has described the social network UFO reports transverse, and I have described some of the tools and mechanisms employed by all parties when presenting their UFO information (Greenwell, 1980b).

Reponse to John Rimmer

Mr. Rimmer is correct in stating that I may have confused the issue by dividing UFO reports into conventional and unconventional categories. I have ellaborated on this above, and need not repeat it here.

He is also correct in his criticism of my review of the Psychic Projection Theory. As he himself stated, however, I was responding to the ideas of "Jungian UFOlogists," who may not have the same understanding of the topic as do professionals. Furthermore, I must admit that my training in psychology has been more oriented toward the experimental approach, which has left little room for pursuing Jungian theories, as interesting as they may be.

Toward the end of his critique, Mr. Rimmer states that I interpret the UFO phenomenon "mainly in physical terms" (Rather than related to "internalized psychologial factors," i.e. the Jungian approach). This is correct, but "physical" should not imply "extraterrestrial," or even "unconventional." I prefer looking at UFO phenomena from the stimulus-response approach of experimental psychology, rather than theorizing about obscure internal factors one cannot reproduce or measure. That is, a stimulus (in this case an airborne or apparently airborne object) causes a response in an organism (in this case a human sensor/perceiver). The stimulus usually has a physical property; that does not rule out a conventional origin or cause (as Mr. Rimmer seems to imply) which is simply misperceived by the observer.

So the question of interest to me is: "Are all such sightings due to misperceptions, or just the majority of them?" Even the debunkers would agree, however, that most stimuli resulting in UFO sightings (however misperceived) have physical properties.

Response to Michael K. Schutz

Dr. Schutz goes through a process of elimination to arrive at the ETH as the probable cause (together with conventional explanations) of UFO reports. I find interesting his comment that "cases come in, year after year, from any part of the globe, just as they should, if the phenomenon is really unconventional." The question we must immediately raise here is: "Would such cases keep coming in if the phenomenon has an entirely conventional cause?" The debunkers would probably answer in the affirmative. Whatever stimuli creates a UFO report in the U.S. could create a similar report in Finland, Bangladesh, or Bolivia, and, perhaps more importantly, the social myth of flying saucers has extended itself so pervasively across the world

that individuals of any country or culture are likely to interpret such unknown stimuli in terms of extraterrestrial visitations. Therefore, the existence of global UFO reports not only does not support an unconventional cause, but it actually demonstrates the opposite: that the UFO problem is caused by a global psycho-social phenomenon.

I am not trying to put words into the mouths of the debunkers (they seem to manage admirably without help from persons like me), but I think this argument (if they do indeed endorse it) is quite valid, up to a point. We must next ask by what means has the UFO myth been propagated globally. American movies and television shows, very popular everywhere, have played a major role, as have news-wire agencies and regional/local publications. However, all these organs of dissemination have had a social impact in the urban areas only, leaving most of the world's population, located in the rural areas, virtually unaffected. Not only unaffected, but I estimate that, of the current world population of 4.5 billion (1980 figure), about three billion have never heard of UFOs or flying saucers. These three billion people inhabit the vast rural areas of Asia, Africa, and Latin America.

My hypothesis predicts, then, that fewer UFO reports would come from these areas, and the data indicate that, except perhaps for Latin America, such is the case: there are very few UFO reports coming from rural areas of Asia and Africa. We should then ask whether this is due to 1) fewer stimuli (i.e., fewer man-made devices which could appear anomalous) in these rural areas; 2) less social influence from the global flying saucer myth; 3) poor communications between the urban and rural worlds, inhibiting our ever learning of such reports (we must remember that, if rural communities are so isolated that their acquisition of our flying saucer myth is inhibited, then they are also isolated to the point where we, in the industrialized world, would not become aware of their UFO sightings); for example, would the Papua, New Guinea, UFO case still be a classic if Father Gill (or a counterpart) had not been present? Were the natives influenced by Father Gill, or would they still have perceived the stimulus as anomalous in his absense? If so, would their observation have reached the outside world?; or 4) a combination of any of the above.

Dr. Schutz' comments on the "Volume of Traffic" problem are similar to Mr. Friedman's. His calling Earth an extraordinary planet is probably correct, but it may be far more extraordinary than even he imagines, so much so that there may not be anybody else out there to visit us! The reason for this is discussed in my response to Dr. Abell. Thus, I am not very happy with the concept of "Extraterrestrial Pluralism." I would be happy to settle for just one such intelligence playing games with us, but don't let's push our luck. The reason Dr. Schutz and I disagree on this point is that we are looking at different components of the problem. He is looking at the wide variety of shapes and sizes described in UFO reports, as well as their sheer number, while I am looking at the evidence for the evolution of intelligences elsewhere, using Earth as a living model.

I certainly agree with Dr. Schutz' last sentence: "Continued patience is required, as we continue to ask if there is truly an unconventional phenomenon here at all."

Response to Robert Sheaffer

I wish to thank Mr. Shaeffer for his kind remarks, and he may be gratified that I address Dr. Jacques Vallee's latest thoughts in my response to Dr. Swift. I am also pleased to see Mr. Sheaffer's admission that "the extraterrestrial hypothesis has been unfairly criticized by many scientists...," and I am very happy that I will now be able to so quote Mr. Sheaffer for many years to come!

His observation that one "interstellar UFO" could be responsible for numerous sightings in different geographical areas could be taken a step further: One could postulate what I call the aircraft-carrier hypothesis; that is, one giant interstellar mothership could remain in the Earth's environs, while many small craft designed for short-haul atmospheric flights could be dispatched. Thus, just one extraterrestrial visit could, over time, be responsible for hundreds or even thousands of UFO reports (and perhaps many more thousands of IFO reports generated by the myth created by the authentic sightings).

Mr. Shaeffer's comments on the possible uniqueness of man in the galaxy are quite appropriate; I have discussed this topic at length in my response to Dr. Abell. I also agree with him that I inadequately covered the "conventional" explanations, the reason being of course, that the article called for a review of "unconventional" "theories" for The Encyclopedia of UFOs. I should mention that I also discussed IFOs and other conventional explanations in a different Encyclopedia entry (Greenwell, 1980d), and I hope most readers of the Encyclopedia will find my other entries balanced and fair.

I agree also that the null hypothesis predominates, and it is up to the proponents to produce consistent evidence, over time, that there is a distinct phenomenon accurring.

Response to Peter A. Sturrock

Dr. Sturrock proposes that (UFO) hypotheses be formulated and compared to the data "according to the normal procedures of science." This seems a proper approach, and he further asks "...how one can best advance our understanding of the phenomenon in terms of a continuation of the present stream ...[of]...soft evidence." It is important to recognize that the consistency of data may indicate certain patterns, even with statistically significant support, which may result in the formulation of an "unconventional" hypothesis (for example, the ETH), when the data may actually be a result of socio-psychological causes; that is, socio-psychological mechanisms can also produce patterns, consistency, and provide statistically significant data, and can thus produce a false picture of the phenomenon being reported. The best way to "advance our understanding," therefore, is to both improve our ability to obtain instrumented data, and to increase our understanding of the human sensory modalities, and the mechanisms involved in perception and memory, in order to be able to formulate reliable parameters for human observers. Both are extremely

difficult tasks, and it is for these reasons, among others, that the UFO problem persists.

As an astrophysicist, Dr. Sturrock makes an interesting comparison between UFO research and astrophysics in that one cannot get one's hands on the data directly. Until very recently, marine geology was another good example (and still is, in some respects), and continental drift is a good example of one of Dr. Sturrock's "unlikely hypotheses" becoming "established conclusively." Whether or not one can also use the UFO analogy will depend on whether or not UFOs become "established conclusively," but it may be for future historians to make the analogy.

Also, it is interesting to ask: "To what discipline do UFOs belong during their current evolution (and 'most important' stage)?" All geologists admitted that plate tectonics, whether valid or not, related to marine geology. Astrophysicists agree that the supernovae-pulsar relationship, whether valid or not, is a problem for astrophysical science. But to what discipline do UFOs belong while they go through their stage? Geophysics? Astrophysics? Evolutionary Biology? Perceptual or Social Psychology? I don't see any discipline jumping up and claiming the UFO problem for itself (even physical anthropologists agree that Bigfoot, whether he exists or not, is a problem for their discipline), and that lack of a parent science is what makes UFOlogy a bastard -- truly an "illegitimate" science.

As far as I know, Dr. Sturrock is the first to apply Bayes' theorem to UFOlogy, although this journal has previously published a discussion of it in relation to the "paranormal" (Beauregard, 1978). However, I am not sure that my almost arbitrary listing of "hypotheses" --as Dr. Sturrock kindly calls them -- are adequate for the task, and I look forward to seeing his future reworking of the list.

In Bayes' theorem, an initial probability of zero will never change, regardless of subsequent evidence, and Dr. Sturrock states: "The initial probability which a scientist assigns to a hypothesis is likely to be of no interest to anyone except himself." That is the ideal we should strive for, but the "real world" is often different. Some scientists, for instance, have gained reputations in certain fields for fundamental areas of work, and their attitudes (or in this case the "initial probability they assign to a hypothesis") toward other topics can carry much weight among fellow scientists, even when they may know practically nothing about the topics in question, much less having critically analyzed the relevant evidence. Other scientists have been very successful at popularizing science, and have thus been given a disproportionate status by the public, the media, and even the federal government. As a consequence, their statements can carry enormous national weight, regardless of the topic they are addressing.

The same can be stated at the institutional level, as when a special panel of the National Academy of Sciences, charged to review the University of Colorado's 1969 UFO Project report, concluded that: "...the least likely explanation of UFOs is the hypothesis of extraterrestrial visitation by intelligent beings." Although nobody has been able to ascertain the criteria used for determining which was the

most "unlikely" hypothesis, the panel's statements closed the door on official UFO studies indefinitely.

Finally, Dr. Sturrock proposes a division of labor in UFOlogy into three groups: the case-study (or field) people, the statistics (or pattern) people, and the theorists. In some respects, this has been the case, although there has been considerable more overlap than Dr. Sturrock would like. Also, we find that, while many "theorists" have tended not to undertake case-studies, practically all the case-study people, usually non-scientists, have theorized -- and such "theories" are the meat of my original article.

One could now ask if Dr. Sturrock's approach is indeed the most appropriate one. There are so many complex and subtle aspects to the UFO problem, most of them socio-psychological, that it may be preferable to have the statistics and theory people experience field work first-hand (but not necessarily the reverse). This would give them a better "feel" for the nature of the perceptual and reporting mechanisms involved in the phenomenon, thus providing them with a subjective dimension which could be of some importance to their overall understanding of the problem.

Divisions of labor do occur in Dr. Sturrock's professional area (i.e., observational astronomy, theoretical astrophysics), and they no doubt serve their purpose. There are other sciences, however, where there are no such divisions, nor are they desired. A good example is archaeology, at least as practiced in the U.S. Basically, archaeologists operate at three levels. At the first level, they reconstruct cultural chronologies (the "when" and the "where" of a given culture); that is, they determine the culture's relative and absolute time and space parameters, primarily through site excavation and geological stratigraphy. At the second level, they reconstruct cultural "lifeways" (the "what" and the "how" of the culture); they do this by analyzing a vast amount of artifacts and ecofacts, including faunal, agricultural, and ceremonial remains, and computer programs are often necessary. Finally, archaeologists apply this knowledge to an overall interpretation of human cultural processes (the "why" of culture). This can also include comparative studies of present-day cultures, and tapping other disciplines, such as economics, sociology, or trans-cultural psychology. This third level is really the ultimate aim of all archaeology and cultural anthropology.

There are not, however (nor should there be), three types of archaeologists, the diggers, the analyzers, and the theoreticians. The archaeologist is expected to move along with the evolution of his research, experiencing first-hand all its different components, altering his approach according to the level it is at. And so it could be with UFOlogy, which, like archaeology (and unlike astrophysics), deals more often than not with social processes.

I am not necessarily advocating this integrated approach over Dr. Sturrock's divisional approach. Rather, I propose that the topic be discussed further by others in this journal and/or elsewhere, in order to assess the merits of the different approaches as they apply to the realm of UFOlogy.

Response to David W. Swift

I agree with Dr. Swift that there is no single "theory" that solves the UFO problem. All have flaws of one kind or another, and that is one reason the UFO problem persists. The one solution with no flaws would be to attribute every single reported UFO incident to a conventional cause. In other words, the percentage of explained cases would increase from about 95% to 100%. Besides the usual Venus, aircraft, and balloon explanations, we could envision all the "conventional" phenomena represented, including some phenomena that some consider "unconventional" -- ball lightning, earthquake lights, insect swarms. The only reason I do not immediately accept this solution is that, in some instances, the data have to be changed considerably in order to accommodate these explanations, and, despite the raising of eyebrows by my debunker friends, that is something I am not willing to do.

I will now address the "additional thoughts" of Dr. Jacques Vallee (whom I understand declined to participate in this dialogue), which Dr. Swift uses as an example of a "new theory." It is true that I mentioned Dr. Vallee's ultraterrestrial ideas only as they appeared in Passport to Magonia (Vallee, 1970). Since then, two more books have appeared, The Invisible College (Vallee, 1975), and Messengers of Deception (Vallee, 1979). Despite Dr. Swift's suggestion, I would find it very difficult to encompass all of Dr. Vallee's new thoughts into a ninth "theory." Regrettably, Dr. Vallee's thoughts are too inconsistent, poorly researched, and, at times, irrational.

One of the main problems with Dr. Vallee is that he delves into areas far outside his field of expertise (computer science and astronomy), and this gets him into serious trouble. It is not uncommon, of course, for physical scientists to confidentially become overnight experts in the social and behavioral sciences.

Dr. Vallee is against the ETH because it is too simplistic -- thus agreeing with the debunkers. But instead of dismissing the UFO evidence as the debunkers do, he constructs fantastic edifices on top of foundations of shifty information. In The Invisible College, he introduces the contactees and contactee-oriented groups into UFOlogy, a phase from the 1950s, which practically ended in the 1960s. (By "contactee" I mean a person who claims encounters with "space brothers" who impart cosmic messages of salvation -- not the "abductees" who claim to have been forcibly taken "on board" for examination.) Dr. Vallee would now have us believe that the contactees have held the key to the whole mystery all along.

He then goes on to propose seven categories of what he calls UFO "strangeness": 1) the flickering light; 2) the flaming mass; 3) the unknown craft; 4) the landing; 5) the observation of occupants; 6) the personal illumination; and 7) the reality gap. On page 113, he presents the Hilltop Curve, which attempts to demonstrate that the higher strangeness cases (5, 6, and 7) occur less often (and are not reported to official channels, or are not reported at all), and are now the subject of "special studies" by Dr. Vallee's "Invisible College" of scientists (who are never identified). He believes that

the kind of reports received by a UFO investigator "is a function of his image." Thus, scientists will only receive a certain kind of report, the military another kind, and so forth. The trick is to change one's perception of reality, and that is what Dr. Vallee, by his own admission, has done (p.112). In other words, if a scientist wishes to receive the real "far out" UFO information (Dr. Vallee claims to have even gone off the chart, beyond 7), then he will have to radically change his perceptions, abandon all the rational niceties of scientific methodology, and uncritically accept the belief systems of the contactees.

While many scientists will go to great lengths to obtain data (cultural anthropologists, for example, often spend years as participant observers in tribal villages to gather their information), the cost, in this case, would far outweigh the benefit. What would be the point of finally obtaining one's data if the price one had to pay to get it were to abandon one's world view and interpret the data uncritically?

Furthermore, Dr. Vallee assures us that the high strangeness cases (5, 6, and 7) are worth getting, as they are as reliable as the less strange reports. This is a very important point, but Dr. Vallee solves the problem quickly by further assuring us that Dr. J. Allen Hynek has "many reports in his files which are of both high reliability and high strangeness." No further information is provided.

The final chapter of The Invisible College is called "The Control System." In it, he proposes that "there is a control system for human consciousness." This is not an altogether unreasonable idea, particularly if one envisions an extraterrestrial intelligence far in advance of our own. But Dr. Vallee then attempts to couch his intriguing idea (actually borrowed from an ETH "sub-school") in terms of behavioral psychology, and discusses B.F. Skinner's work with behavior modification and schedules of reinforcement in stimulus-response research. Dr. Vallee, is able to reduce learning theory down to one paragraph, and then to propose that UFO activity is operating on some sort of reinforcement schedule so that humanity will acquire new knowledge.

He believes that some alien force (but probably not extraterrestrial) is applying behavioral psychology techniques to the modification of beliefs and attitudes of whole societies, pushing humanity toward a new cosmic order which remains undefined. What Dr. Vallee fails to recognize is that behavioral psychology is not concerned in any way whatsoever with beliefs, attitudes, or other "internal" forces, but only with direct, observable behavior. Behavior (defined here as an immediate, visible response to a stimulus) is the only concern of behaviorism, and supposed internal forces which cannot be observed or measured are considered totally irrelevant. In fact, behaviorism was born in the twentieth century partly as a response to the Vallees of Victorian Europe.

For the same reason, behaviorism cannot be applied to grandiose schemes, such as nothing less than the cultural evolution of man. John B. Watson, the founder of behaviorism, first attempted to apply it too

broadly, such as to World War I, but he later modified it, throwing out introspection, consciousness, and instinct in the process, and even attacking functionalism and structuralism. Watson's successors, Tolman, Hull, and Skinner, refined behaviorism even more, but now we find Dr. Vallee ignoring over 60 years of research in order to find quaint support for his ideas. He also fails to point out to his readers that the optimal time period between the stimulus and the response in classical conditioning (in both humans and animals) is half a second. Beyond one second, the association will not be made by the organism, and the conditioning will thus not occur. In other words, the "pairing" of S-R, for S to be associated later with R, has to be almost instantaneous. In instrumental conditioning, in which the response comes first, the reinforcer should also follow the response very closely (i.e., within seconds or minutes), or the association will be lost to the organism, and, again, the conditioning will not occur.

Dr. Vallee is way off the mark when he dabbles in psychology to support his esoteric ideas. The possible "conditioning" of human societies would seem to relate more to the discipline of sociology than psychology, and in his new book Messengers of Deception he suddenly becomes a sociologist.

The reasoning behind Messengers is even more confusing. Guided by a mysterious Major Murphy, a supposed retired intelligence officer interested in things psychic, Dr. Vallee has refined his "theory" down to three possible scenarios: 1) a highly sophisticated British intelligence deception group (the same people who gave us the Ultra Project), which is simulating extraterrestrial visitations to unify mankind -- a project which seems to be meeting with dismal failure; 2) an occult group which has discovered how to psychicly project images, has made contact with "other forms of consciousness," and has discovered the true nature of UFOs (this group is controlling the belief in UFOs, not the UFOs themselves); 3) the UFO "represents a manifestation of a reality that transcends our current understanding of physics," and that some humans have understood this reality and are manipulating the human belief in UFOs.

Dr. Vallee seems extremely preoccupied by the contactee movement, and the danger to our current science posed by such mystical irrationality. Two points could be made here. First, he fails to recognize that his propositions may seem as irrational to others as the forces he fears. Second, the contactee movement is not growing at a disproportionate rate, and, as professional sociologists well know, such cultural movements are common and maybe even necessary in social systems. Dr. Vallee has taken the social phenomenon of contactees totally out of context and attributed enormous importance to it to gain support for his ideas. Dr. Swift, who is a sociologist and who wrote the Epilogue to Messengers ("A Sociologist's Reaction"), is far more qualified to comment on this aspect. It has to be admitted, however, that the comments by Dr. Swift, both in the Epilogue and above, have been more supportive than critical.

Dr. Swift does propose (in Messengers) that we keep an open mind "weighing the evidence he [Vallee] presents." I suggest we do

just that. Those with further interest in this topic are referred to Greenwell (in press).

Thus, Greenwell's Gallery of Grotesque Guesses comes to an end.

Concluding Remarks

Some readers may now wonder: "Who won?" Well, the Zetetic Scholar won, because it has managed once again to bring together persons with diverse backgrounds and attitudes to debate a very controversial topic. I think we should extend our appreciation to editor Marcello Truzzi for pursuing this goal.

The Zetetic Scholar represents an interdisciplinary and objective approach often forgotten in modern scientific activities, which can be reduced to the following statement: "Let those who have something to say, say it, and then let the data speak for themselves."

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INTRODUCTION

Critics of claims of the paranormal often complain that it is difficult to publish articles debunking rather than promoting the paranormal. One would easily gather from these statements that the pro-paranormal publications mute their criticism and avoid publication of negative pieces. Yet, over the years, I have been struck by the fact that there is much jealousy and concern about priority within the frontier or borderland sciences, and much critical literature is to be found among the journals of the proponents. In fact, many would argue the most responsible and informed criticism may be found in these places since many outside critics from "normal" science sometimes write with little knowledge of the critical internal literature. In fact, within parapsychology, the most important debunkings (i.e., those of the work by Soal and Levy) have been by fellow parapsychologists. The same may be true for Ufology, where excellent critical work has been done by proponents like Allan Hendry.

Though the best such critical work may appear in the specialized journals which clearly seek scientific legitimacy (e.g., the Journal of the American Society for Psychical Reserach), even the most sensational publications (e.g., the National Enquirer) occasionally publish negative articles. To make my case more clear, I recently asked Jerome Clark, an editor at Fate magazine, to compile a list for ZS of all the Fate articles he would consider negative, debunking pieces from January 1975 to September 1981. He sent me the following list of 45 articles from over this nearly seven year period. This is an average of nearly 7 articles per year, or about one every other issue. Since Fate is basically an occult oriented publication and is even viewed negatively by many parapsychologists (especially for its advertising which is frequently outrageous in its claims), we should expect it to be one of the worst offenders. And yet its record is actually surprisingly good. These articles are often by critics rather than known proponents of the paranormal, and the quality of these pieces is excellent. It is clear, then, that the record for frequency in publication of critical or debunking articles for Fate may be second only to that of The Skeptical Inquirer. It is also clearly the case that Fate has frequently exposed fraud and error in media promoted pseudoscience first, before the regular critics seized on the news. But far more important, the debunkings in Fate get read by a large readership of proponents of and believers in the paranormal. The critical journals tend to be read only by those who are already critical. Fate may actually be changing some believers into disbelievers.

I am not arguing that the best critical work is done by the friends rather than the enemies of the paranormal; but I am arguing that good serious criticism gets done within and without the protosciences. And those of us wishing to be informed about these matters need to examine good research efforts wherever they are to be found. When dealing with what is largely still the unknown, we can use all the rational help we can get.

-- MARCELLO TRUZZI

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THE SCHISM WITHIN PARAPSYCHOLOGY

JEFFREY MISHLOVE

A major problem for parapsychology is that the general public seems prone to confuse the scientific study of psi phenomena with the most outrageous popular abuses of reason. This is partially understandable insofar as terms such as "parapsychology" and "ESP" have been appropriated by mystical and occult enthusiasts of many varieties. Consequently, many well-educated individuals would be hard-put to distinguish between the discipline of parapsychology and the rhetoric of pyramid power, radionics, spirit guides, aura reading, ouija boards, crystal balls, biorhythms, astrology, occult masters, ancient secrets, tarot numerology, psychic readings, and popular metaphysics.

Admittedly the problem is insidious, as this confusion is fostered not only by both overzealous believers and overzealous skeptics of parapsychology--but also by the theoretical controversies within parapsychology itself.

The issue of the overlap between parapsychology and various forms of occultism, mysticism, and plain old-fashioned religion has touched the raw nerves of many parapsychologists, inspiring in some a need to create as much distance as they can between the scientific and "popular" approaches to psi. Vessey (1969), in an article on parapsychology and the occult, put the matter simply: "There is no common ground between the psychical researcher and the occultist, for the simple reason that the former is attempting to pursue an exact science, whereas the latter is neither exact nor scientific." (p. 162) Vessey continued his analysis by dogmatically asserting that almost all interest in the occult stems from "obvious compensation for defects of personality." (p. 162) This position embodies, I believe, an insidious hubris which is actually damaging to parapsychology itself. In a similar vein, R. A. McConnell (1973) has stated:

> We are living in a crescendo of popular superstition whose only relation to parapsychology is through the substrate of weak, sporadic, natural phenomena to which both attempt to relate." (p. 227)

Parapsychologists, in general, have been primarily concerned with establishing a secure domain that can withstand the emotionally-charged attacks of critics and the dreaded "wild sea of superstition on which the small boat of scientific objectivity is trying to navigate." (McConnell and McConnell, 1971, p. 353).

While a desire for distance is understandable from a political and social perspective, this attitude, in my opinion, has ironically had a stifling impact on the development of ethnographic studies, participant-observer sociological studies and educational evaluations which must

necessarily recognize popular occultism as the legitimate object of investigation from a parapsychological perspective--and as a critical competitor to academic education in parapsychology.

Most parapsychologists actually have had some subjective, personal interest in psi. However, the controversies within and without parapsychology have, in my opinion, additionally stifled the proper development of the subjective disciplines within parapsychology. This is unfortunate, as many academic disciplines--such as physical education, music, drama, and psychotherapy--have flourished when both the subjective and objective aspects are cultivated.

This schism is reflected in an article by R. A. McConnell (1973) which described several areas in which "the demands of the populace endanger parapsychology" (p. 227)--superstitious teachings within educational systems, mind-control courses, and some psychic healing practices. He simultaneously urged fellow parapsychologists not engage in unprovoked public criticism of "those who most flagrantly misappropriate the name 'parapsychologist.'" (p. 241)

McConnell's article betrayed a hesitancy within parapsychology to provide ongoing critical examination and appraisal of the subjective occult disciplines--preferring to settle for a posture of aloof disdain and unexamined prejudice. In an odd twist, McConnell stated, "Our efforts must be creative rather than critical." (p. 241)

Ironically, McConnell also betrayed a surprising lack of self-awareness regarding the overlapping motivations between several occultists whom he cites and himself. For example, McConnell argued against religious interpretations of parapsychology and criticized a scholar, Professor Jack Holland (1971), who suggested that true parapsychology should attempt to get at the "great basic truth that lies behind the phenomena." (p. 12) Then he turned around and concluded that parapsychology may provide a means of answering questions such as the following:

> What am I? Am I autonomous to some degree, or totally a creature of destiny? How do I relate to my fellow humans? What freedoms can I rightfully claim and what must I sacrifice for the common good? (p. 243)

McConnell's stance has been accepted virtually without criticism in the parapsychology literature, until recently. Meanwhile, the hostile sceptics have continued to point both to the similarities of parapsychology and occultism and to the failure of parapsychologists to distinguish between the varying merits of the scientific approach to psi and other approaches.

The schism within parapsychology is highlighted in the writings of Rex Stanford. In his presidential address before the Parapsychological Association, Stanford (1974) argued from the radical position that investigators "should endeavor personally to experience as many psi phenomena

as they can," and should "study the practices and beliefs of magic, religion, and mysticism of diverse cultures and times as they relate to possible psi phenomena." (p. 158) Stanford's viewpoint was tied to the beliefs (1) that parapsychologists had to rethink their basic conceptual framework and (2) that more "process-oriented research" was necessary to determine which variables could be correlated with psi. Stanford was arguing for greater scientific rigor, both experimentally and theoretically. This process, he felt, could be encouraged by more "inputs" from personal experience and the thoughtful evaluation of mystical traditions.

However, more recently in a paper titled "Are We Shamans or Scientists?" Stanford (1980) reversed himself somewhat and argued that successful experimenters who have developed psi talents, are contaminating interpretation of their experimental data because of their abilities. Stanford acknowledged that there is theoretically "no way absolutely to rule out such shamanistic effects in our field." (p. 4) Nevertheless, he strongly urged the use of fixed lists of random numbers,as experimental targets, instead of electronically generated random events, in order to reduce the possibility of experimenter PK on the targets. Stanford believed that such a maneuver, although it could not rule out a psi influence directly on the subject, could result in less inter-experimenter variability. Even if fewer successful experiments were reported, results might be more meaningfully interpreted.

In my opinion, Stanford's newer position is based on wishful thinking. Stanford has not fully come to grips with the limitations imposed on objective experimental parapsychology by the experimenter effect. Process-oriented questions are important to ask; however, experimental tests of process-oriented hypotheses will always be confounded by the experimenter effect. More sophisticated experimental designs cannot avoid this methodological limitation. However, subjective disciplines and modes of inquiry may well provide the deeper answers which parapsychologists seek--answers that will ultimately yield greater magnitude and reliability of psi in the laboratory. The limitations of the experimenter effect can, thus, be turned to great advantage.

In originally urging that experimenters study magic, mysticism, religion and occultism, Stanford (1974) cautioned that researchers should not "naively adopt the beliefs in these areas for direct translation into hypotheses." (p. 158) He realized that many beliefs were intangible, metaphysical assertions (i.e. about deities), and that other beliefs would be highly implausible as hypotheses. He simply urged that such a study might "indirectly or directly yield important clues about psi processes, at the psychological level and perhap at more basic levels." (pp. 158-9)

Unfortunately, Stanford's original and, I believe, admirable position has been difficult for parapsychologists to live with--given the pressures from outside skeptics and internal critics who confuse the issues. Scholarly studies exploring subjective psi-oriented traditions have been

given barely discernable attention in the parapsychology literature. When attention is given, the parapsychology researcher is often confronted with the same painful existential/scholarly dilemma that responsible skeptics have when facing the experimental data of parapsychology.[1]

For example, David Read Barker (1979) reported that in the Tibetan culture there is "no word or concept which corresponds to the Western notion of the 'supernatural' or 'paranormal' . . . From the Tibetan's own viewpoint, there simply is no such thing as psi." (p. 52) Barker adds that the concept of PK has little meaning for Tibetans, because they regard the entire world to be "primarily psychokinetic, the creation of form by consciousness." (p. 53) In the following passage, Barker described an apparent incident of PK and its personal impact upon himself.

> Probably the most dramatic expression of apparent PK in Tibet is weather control. I witnessed what appeared to be a demonstration of this in Dharamsala, India, on March 10, 1973, when a revered shaman-priest named Gunsang Rinzing was employed by the Dalai Lama to stop a huge storm long enough to permit a festival of mourning for the collapse in 1959 of the ancient Tibetan state. . . . Everywhere else in the area it continued to pour, butthe crowd of several thousand refugees was never rained on during the six hours it assembled. At one point a huge hailstorm caused a tremendous clatter on the tin-roofed buildings adjoining the festival grounds, but only a few dozen hailstones fell on the crowd. The atmosphere of the grounds seemed to have an "airless" quality, and the whole experience produced in me a feeling of distress and disorientation which persisted for weeks. (pp. 53-54) (my italics)

Very few parapsychologist have been willing and able to venture into the depths of esoteric traditions and then report back to their colleagues about the experience. Some of the rare instances when this has occurred are reported in Psi-Development Systems: A Disciplinary Matrix for History, Theory, Evaluation and Design, my doctoral dissertation in parapsychology at the University of California, Berkeley which focused on methods of cultivating psi.

That genuine prejudices have been active within the parapsychology community is made more clear in an article by John Beloff (1978) in the European Journal of Parapsychology, titled "The Limits of Parapsychology." In this article, Beloff, one of the most respected and conservative members of the parapsychological community, acknowledged that a careful look at the data is forcing him to consider that such subjects as acupuncture, astrology, UFOs, and psychotronics should be considered within the legitimate domain of parapsychology.

Beloff described his previous view of astrology as "that archetypal pseudo-science which I had always treated with contempt." (p. 292) Then he went on to point out that the research of Michel Gauquelin (1977)

conducted under rigorous conditions and "independently corroborated by sceptical, not to say hostile, committees" (Beloff, 1978, p. 292) has forced him to reconsider the merits of at least some aspects of phenomena that are popularly labelled as "astrology."

Beloff described parapsychological interest in UFOs in a similar manner:

> Previously I was content to treat the UFO evidence in much the same dismissive way as the orthodox scientist treats the parapsychological evidence. (p. 294)

Beloff conceded that arguments of D. Scott Rogo (1977) have convinced him that UFO phenomena are, indeed, of interest to parapsychologists. Beloff concluded his article by cautioning his colleagues against treating occult material "as too many straight scientists still treat the parapsychological evidence, as beneath our notice." (p. 302)

John Palmer (1979), in his presidential address to the Parapsychological Association, took up Beloff's arguments and added that the "correspondence paradigm" of psi may define new topics of inquiry within parapsychology. Palmer stated the following:

> This implication ... will force us to reexamine some long-held prejudices: some of the synchronistic phenomena that will fall under our umbrella are now most commonly classified in that category we so disparagingly label "the occult".

Palmer also pointed out the danger in such a move that the critics of parapsychology will "seize upon what they see as a new opportunity to link parapsychology with popular occultism, i.e., the anti-rational and anti-scientific." Palmer urged parapsychologists to resist the simplistic maneuvers of critics and also to "resist the temptation to avoid certain concepts and lines of research because of their political risks."

One of the most forceful and articulate advocates of subjective discipline within parapsychology is Rhea White. Her dramatic presentation at the 1979 convention of the Parapsychological Association received an almost unseemly ovation. There she presented (White, 1979) for the first time ideas which she claimed to have held since 1955 when she began working at the Duke Parapsychology Laboratory--but withheld from her colleagues for fear of being considered "too eccentric." White's plea was simply that the most appropriate methodology for understanding what psi is and how it works is for researchers to learn to use psi themselves.

To do this, she suggested that experimenters stop being so concerned with the opinions of colleagues, friends, research directors, the experimental literature, and above all the skeptics. Instead, White suggested the following:

> I propose that we start from within and work outward to our experimental designs. If possible, the experimental question should have a bearing on whatever is of the greatest interest to the experimenter, no matter how silly or improbable it

might seem to be when judged by common sense. It should be related to the growing edge of the experimenter's personal concerns. I think that the answers we seek lie more in our feelings than in our heads. The ideas we already have fall short of where we want to go. So we must let go of what we presently have--of what we think and know--and dig deeper, reach higher, stretch further. (pp. 6-7)

Of course, White also recognized that such behavior on the part of the scientific parapsychological community might readily provide further bait for the skeptics and debunkers. However, she argued that parapsychology has suffered a malaise because its detractors "push us into a defensive corner by ourselves placing reason first, so we are not in a position to rest on the tides of faith, on confidence, and conviction which it seems are the prerequisites for results in parapsychology." (p. 11)

White concluded her paper with an impassioned plea for a new spirit in parapsychology. While clearly distinguishing her position from a defection to credulous occultism, White proposed that parapsychology stop being concerned with the irrational, but seemingly reasonable, concerns of skeptics:

> . . . we must stop being defensive and self-defeating about the questions we ask, the ways we conduct our research, and in proposing wild hypotheses about the nature of psi. If we are not accepted as a science, maybe the fault lies partly with ourselves. We behave as underlings. We defend ourselves as if indeed we were secretly guilty. If in fact we are a frontier science--and surely, if we aren't, who is?--then let us behave as frontier scientists, and let the devil take the skeptics! (pp. 12-13)

In 1972, I created an individual, interdisciplinary doctoral program in parapsychology from which I graduated in 1980.[2] The program was unique and somewhat controversial, insofar as it defined parapsychology as a discipline of both subjective and objective dimensions. This endeavor, built upon a century of parapsychological inquiry, owed a great deal to the two major methodological contributions of previous generations: the case history methodology of early psychical research and the experimental methodology of Rhinean parapsychology. In creating the program, it had been my intention to participate in the demarcation of a third synthesis with the following characteristics: (1) extending the self-conscious, critical history of parapsychology further into the prescientific cultural traditions of shamanism, yoga, Sufism, and kabbalah; (2) providing educational and consumer evaluation of the myriad of popular programs purporting to offer experiential psychic guidance; and (3) developing models for experiential ESP education and training which could be introduced into our mainstreaming educational systems.

In advocating the development of subjective disciplines within parapsychology, I am absolutely not encouraging researchers to violate an attitude of scientific agnosticism toward unproven or unprovable claims. Agnosticism, however, cannot mean disbelief--which is, in effect, another form of belief. Agnosticism, as I view it, implies the ability to consciously suspend both belief and disbelief, as well as hostility, prejudice, and contempt. On the other hand, agnosticism is entirely compatible with a respect for and an understanding of the traditions which share in common with parapsychology an interest in psi. The trick is to become immersed in the subjective exploration of psi, or social systems which attempt to relate to psi, without becoming submerged. The ideal researcher balances reverence and irreverence with good humor.

FOOTNOTES

1. It is often very painful for an outside scholar to honestly confront the data of parapsychology. The eminent statistician, and former President of the American Association for the Advancement of Science, Warren Weaver (1963) in his book, Lady Luck, reveals the critic's agony:

> The Rhine ESP results could be explained on the grounds of selection or falsification of data. Having complete confidence in the scientific competence and personal integrity of Professor Rhine, I find this explanation unacceptable to me. In any very long probability experiment there will occur highly remarkable runs of luck--as in the 28 recorded repetitions on one color at Monte Carlo, or the long runs of "passes" at craps. But I know of no analysis of Rhines data, based on such considerations, that makes it reasonable to believe that their success can be explained in this way . . . . . As I have said elsewhere, I find this a subject that is so intellectually uncomfortable as to be almost painful. I end by concluding that I cannot explain away Professor Rhine's evidence, and that I also cannot accept his interpretation. (pp. 360-361)

2. An article published in Psychology Today (October 1980), presenting an extremely critical perspective of my doctoral career at U. C., Berkeley contains over twenty-five factual errors and distortions. I have asked the magazine to publish a retraction. Interested scholars may obtain from me a list of the errors which occurred in this article.

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CRITICAL COMMENTARIES

COMMENTS BY JOHN BELOFF:

I am honoured that Dr. Mishlove should take note of my article in the European Journal of Parapsychology but I beg leave to clarify my own position lest his embrace become an embarrassment. He attributes to me the view that: "such subjects as acupuncture, astrology, UFOs and psychotronics should be considered within the legitimate domain of

parapsychology" (my underlining). This might suggest that I am in favour of, for example, allowing papers on such topics to be read at the annual convention of the Parapsychological Association. Quite the contrary. In my article I was making a much weaker plea, namely that parapsychologists have no right to turn a blind eye to the claims put forward by those working in neighbouring territories of the paranormal domain. If I may quote my actual words, I say in the concluding passage of that article:

"I am emphatically not making a plea to relax our hard-won standards of evidentiality. My purpose was to see whether, in the present state of knowledge, a line could be drawn between the legitimate province of parapsychology and the much wider field of the paranormal. In particular, I wished to alert some of my fellow parapsychologists to some of these more fantastic claims we have been hearing recently from this more extended domain which I, in company with so many other parapsychologists reared in the scientific tradition, have hitherto preferred to ignore. Not, let me add, with a view to accepting any of these claims but at least with a view to maintaining a watching-brief on such evidence as may appear rather than treating it, as too many straight scientists still treat the parapsychological evidence, as beneath our notice."

On the specific topics which Mishlove mentions, my current attitude is still best described as one of suspended judgment. Acupuncture, at least with regard to its analgesic properties, seems to me to be well on its way to integration with orthodox medicine thanks to the recent discovery of the endorphins. Gauquelin's astrological findings produce much the same effect on me as Rhine's findings did on Warren Weaver, that is to say they make me acutely uncomfortable because I cannot fit them into my conception of the cosmos. Gauquelin's own attempts to seek a physical explanation do not strike me as promising, and the only alternative that I can see is to acknowledge some kind of synchronicity. Ufology should certainly not be confounded with parapsychology even though the evidence suggests to me that we are here dealing with psychical rather than physical phenomena. Finally, if we take "psychotronics" to mean the idea that there are unknown forms of energy in nature that are involved in certain psi phenomena, I can only say that, so far as I am concerned, the case has still to be made.

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COMMENTS BY RICHARD KAMMANN: *The Parapsychologist at the Choicepoint*

The implications of Dr. Michlove's essay is to give parapsychologists a choice between scientific investigation based on empirical reality-testing and a religious stance based upon faith and subjective validation. I shall try to make clear why this is so.

Mishlove offers no new evidence here for the reality of psychic phenomena. I assume he agrees that the testimony of authority figures, be they Stanford, Barker, Beloff, Rogo, Rhea White, or any illustrious scientist or public figure, is not itself evidence for or against ESP. Thus, there is no new evidence.

It is only after concluding that ESP exists that one can debate second-order questions like the possibility of experimenters forcing positive results by their PK on random number generators, or the pos-

sibility of using their own psychic experiences as a guide to further research.

The first-order question is whether or not sufficient evidence has accumulated to justify belief in any paranormal phenomenon, including basic telepathy and clairvoyance, the two most plausible and testable psychic claims. Although positive results have been reported over the years there are good reasons for remaining skeptical. The first reason is the fizzle-effect, that is, the regular disappearance of a psychic effect when subsequent attempts are made to replicate the experiment. Examples are: the large set of non-replications of J. B. Rhine's early work (Hansel, 1980, pp. 99-100), the failure to get a reliable sheep-goats effect, the spotty replicability of the remote viewing paradigm, and the disappearance of Tart's results after he corrected a fault in his ESP training apparatus (Tart, Palmer and Redington, 1979). Related to this is the decline-effect in individual psychic subjects which is often attributed to the demotivating or response-extinguishing nature of the laboratory task, while it might at least as plausibly be attributed to progressive improvement of the experimental controls.

A second reason for skepticism is that psychic results reveal no lawful properties and do not accumulate toward a coherent model. For example, after 60 years of experimentation there seems to be no answer to the question of whether or not ESP is influenced by distance. Similarly, there seem to be no patterns in the data to explain how the psychic receiver can sort out the target message from all the other available messages buzzing around in psychic space.

Finally, it might be borne in mind that first-hand investigations of the rooms, the apparatus, or the raw data of certain classical ESP experiments have regularly uncovered signs of fraud, chicanery, or serious methodological loopholes. One thinks here of Hansel's (1980) analysis of the Pearce-Pratt and Pratt-Woodruff series at Duke University, of Markwick's (1978) demonstration of fraud in the Soal-Goldney series, and David Marks' discovery of significant cues in the Targ-Puthoff remote viewing transcripts (Marks and Kammann, 1980). But such firsthand investigations are time-consuming and difficult, may be blocked by the original investigator, and may not occur until many years after the fact when they occur at all. But in spite of these sporadic exposures, there is always a new list of best psychic experiments.

It is for all these reasons, but most basically the failure to get a one unambiguously repeatable effect, that I consider ESP to be unsubstantiated, and increasingly improbable, in spite of the mushrooming activity in the field of parapsychology, and its expansion into increasingly far-fetched arenas like astral travelling, poltergeists, plant communication, reincarnation and the like.

The residual question, which is relevant to Mishlove's thesis, is how so many people could believe in paranormal phenomena if they do not exist. The explanation draws from three psychological domains: motivation, cognition, and social influence, although in the end all three factors might be viewed as aspects of a cognitive model.

At the motivational level we might propose a human "drive" to eliminate uncertainty and ambiguity (Singer and Benassi, 1981), or to

remove the implications of death by demonstration of a detachable soul, or to gain symbolic control over factors that bring us unhappiness, or perhaps to preserve a sense of personal power against the frustrating authority of science itself. But while these are plausible motivators, I know of no significant evidence that relates them directly to paranormal belief, so I offer them only as conjectures.

At the social influence level, the massive attention given by the media to psychic and occult claims must strongly increase their plausibility in the minds of the public. This in turn sets up a social consensus that ESP has been validated, thus creating a tentative-belief norm in society that increases the audience for more media reports and the market for psychic and occult practitioners.

But it is a primarily cognitive model, focussing on the limitations of human perception, memory, and inference, that I present in Chapters 11 and 12 of The Psychology of the Psychic (Marks and Kammann, 1980) as an alternative explanation for the prevalence of psychic beliefs. While this is an informal model at this stage, it is in line with many recent findings on the cognitive psychology of social beliefs and judgments which have been ably summarized by Nisbet and Ross (1980).

The gist of my non-psychic model of psychic belief is first that human attention is captured by "oddmatches," by which I mean unexpected and unexplained pairings of events that are connected to each other by resemblance or similarity, as when a person dreams or imagines that a relative is in trouble or has died and this is confirmed soon after.

My second point is that the rapid flow of events in a person's life generate endless opportunities for simple coincidences, but these remain invisible to the observer because individual events are rapidly forgotten unless they produce an oddmatch. That is, without the aid of systematic recorded observations, the human observer is ill-equipped to recognize a simple coincidence when it occurs, a limitation which I have labelled "Koestler's fallacy".

In addition to simple coincidences, oddmatches may also result from unseen causes such as equipment malfunctions, deliberate deception, whimsical pranks, self-fulfilling prophecies, population stereotypes, and so on.

The more strongly that the paranormal hypothesis has become elaborated and available to consciousness in a person, the more readily it is evoked by oddmatch events. More importantly, there is also evidence that a strongly held belief or conceptual model selectively influences what is observed, what is remembered, or what is concluded about a situation, an effect that I call subjective validation and that Nisbet and Ross refer to as a theory-driven judgement, as opposed to a data-driven judgement. In such cases, which are by no means attributed uniquely to paranormal beliefs, the belief system becomes progressively immune from disconfirmation by data, and self-perpetuating.

To return now to Mishlove's essay, he might be saying that scientists should keep in personal contact with their experiments and data, and not turn them over to research assistants and computers. He might be advising that scientists must vary their experimental paradigms

to test the generality of results. Or he might be acknowledging that the choice of what experiment to perform next usually involves intuition or hunch, following a thoughtful review of all the available data, and that choosing the right hunch is the essence of scientific creativity.

But if Mishlove merely meant to give the above sorts of advice, his essay would be trivial. If I understand Mishlove correctly, he is advising parapsychologists to believe in, to take as valid, their subjective experiences which seem to them to be psychic. He is not very clear about what they should do with these experiences beyond believing them - perhaps he would like to establish a file of cases similar to that of the Religious Experiences Research Unit in England. Or should they be published as data in themselves?

The suggestion that parapsychologists should believe their personal experiences opens the door to more instances of Koestler's fallacy and subjective validation, and closes the door (already too narrowly open in parapsychology) to possible disconfirmation by objective data. This rejects Popper's insight that a scientific theory must be testable and falsifiable. That is why I said that Mishlove's thesis would move parapsychology away from science and toward religion, or one might even say, further toward religion, with apologies to Vessey, McConnell and all those parapsychologists who are strictly committed to scientific validation.

I don't know if Mishlove consciously intends to increase the religiosity in parapsychology, and I assume that Rhea White's cheerful affirmation that parapsychologists are also psychics was not intended to subvert scientific agnosticism, but I think these are the logical implications. My position is that the subjective experience of a parapsychologist is nothing more than a personal anecdote of the type that Koestler has used as evidence for psi, but which has no evidentiary value at all. But it will certainly help us part-time critics if parapsychologists would tell us which side of Mishlove's fence they are sitting on so that we can debate the scientists and leave the religionists alone.

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COMMENTS BY STANLEY KRIPPNER : *Confronting a Schismatic Parapsychology*

I have been aware of "schisms" within parapsychology ever since I became interested in professional psychical research in the 1950's. I was surprised to hear one group of East coast researchers refer to a recent book by J. B. Rhine (1947) as "The Retch of the Mind." Some of Rhine's associates, in turn, dismissed the rival group as "a cadre of spiritualists." I concluded that it was probably a healthy sign to see a controversial field strong enough to afford internal dissent.

In his article, Mishlove actually alludes to three "schisms" in parapsychology rather than one. First, he separates those who make philosophical/religious interpretations of psi data and those who do not. Secondly, he divides experimenters into those who are purely "objective" and those who are also "subjective." Finally, Mishlove separates parapsychologists who are studying psi within very narrowly constricted limits from those who admit they would also be open to investigating the relationship of psi to other anomalous data, e.g., UFOs, non-Western healing traditions, the Gauquelin (1973) data (which I, by the way, do not consider to support the tenets of traditional astrology).

To my own way of thinking, the first two controversies have become less heated over the years. It is no longer reasonable to assert that science is "pure" and "value-free" or that scientists should not be held responsible for directing the uses to which their discoveries are put. As a result, scientists (including psi researchers) should be encouraged rather than discouraged to discuss the implications of their work on human affairs and to write about the exisential meaning surrounding their data. This has been the task of our Social Issues Working Group, a band of concerned PA members who have presented roundtables on the topic at the last two annual conventions. Furthermore, Jonas Salk (1973) observes that it is difficult for scientists to keep their minds on the subject matter of science alone:

> ...the human condition, altered by the evolution of science and scientists, has in turn so affected both that, perforce, their attention must turn increasingly to questions of general human concern. (p. x)

In addition, the division between "objectivity" and "subjectivity" has tended to evaporate if, indeed, it was ever actually present in the social and behavioral sciences. J. F. Rychlak (1977) states that "it is profitable to view two or more identities -- the experimenter's and the subject's -- in every experiment on human beings" (p. 493) and that "extraspectively framed methods are compatible with theories that are framed introspectively" (p. 493). Unlike many scientists who have been content merely to observe and bemoan this state of affairs, Rychlak has developed a "psychology of rigorous humanism" to deal with them. A rigorously humanistic parapsychology could logically follow his outline.

Mishlove's third "schism" represents a more serious issue. Surely it would be simpler to continue to restrain parapsychology from overstepping its bounds and to confine psi research to those laboratory studies in which any sensory cue (or for PK experiments, any motoric in-

fluence) can be eliminated. Further, this approach would minimize external criticism; it is outrageous enough to speak of "extra-sensory" perception and "psychokinetic" movement, but to add shamanism, "cosmic influences," and UFOs certainly increases the chance of ridicule. Perhaps it would be better to restrict psi research to a clearly delineated set of phenomena and procedures.

Alas, the phenomena do not appear to be that simple. In the first place, the major scandals in the field have centered around experiments which were single-minded in their design and straightforward in their execution. Secondly we might speculate that if there are connections among anomalous phenomena, these links might well hold the key to the explanation of the psi component.

The advent of sophisticated computer data analysis has made such investigations more feasible than ever before. In analyzing data patterns (with the IBM 360-40) from a variety of unusualy events (e.g., "ghost lights," infrequent astronomical events, "poltergeists," spontaneous human combustions, UFO sightings, unexplained disappearances), M. A. Persinger and G. F. Lafreniere (1977) have uncovered some evidence that these phenomena are symptoms of a natural organization more apparent at a global level than at an atomistic level. By ignoring a systems approach to psi (Ruttenberger,1979), parapsychologists may remain stuck at a lower level of the spectrum at which the unifying -- and untimately non-anomalous -- pattern can not be preceived.

There must always be room in the field for atomistic, single variable studies. However, the eagerly awaited explanations of psi phenomena may well emerge from approaches which rigorously investigate not only ESP and PK but their distant cousins, their neighbors, and also the strangers on the other side of the track.

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COMMENTS BY JOSEPH K. LONG:

As an anthropologist I find Mishlove's comments highly relevant. As one who has in the last decade frequently been involved with parapsychological data and parapsychologists and, from time to time, with their critics (or with them as critics of my own work) I find his most outrageous assertions entirely correct. White (1979:12-13) is right on target: "If we are not accepted as a science, maybe the fault lies partly with ourselves as if indeed we were secretly guilty." Anthropologically, the root of this evil rests with the idea some people -- parapsychologists among others -- have that the philosophical structure of science, together with the scientific method and results of our investigations, can somehow give us a total and absolute understanding of reality and a pure grasp of what is logically Truth in our universe. As Margaret Mead (1977) has shown to be the real case with science (with special reference to "paranormal" phenomena) nothing could be further from the truth and, in fact, our concerns about our personal beliefs regarding "psi" are quite beside the point (and the interjection of them into the discussion is antithetical to the very spirit of scientific inquiry).

When my interests in the paranormal began in 1970 I was working with three of the recognized leaders in modern anthropology. None had any particular belief (or disbelief) in the "reality" of psi, but all encouraged my research in and discoveries about psi in ethnographic context. By contrast, many parapsychologists, particularly some of the laboratory experiment-oriented ones, have been critical of my work and the fact that my observations have not been experimentally tested. And despite Stanford's (1974) comments for the Parapsychological Association (PA) as quoted above by Mishlove, requirements for membership in the organization are still such that I (and probably any other anthropologist) would never qualify for membership in it, since I am ethnologist and participant observer, not a laboratory experimenter. Hence it is understandable that some ill-informed members of the PA think they are doing anthropology when they are merely giving ESP tests to non-westernized peoples. It may also explain why no more than a handful of parapsychologists -- S. Krippner, J. Eisenbud, B.E. Schwarz, C. Tart, S. Rogo, and a (very) few others -- have done work of more than passing interest to anthropologists. Hence, while I disagree with some anthropologists' assertions (within the context of the Association for Transpersonal Anthropology's annual meetings) that "parapsychology is dead," I do regard parapsychologists' research orientations and extraction of psi from its cultural context as being severely limiting in terms of its anthropological relevance.

Nor can I be any more kind to most skeptics/debunkers (actually, some of the best debunking and discussion of verification problems has come from parapsychologists or from others writing in journals of parapsychology -- see Truzzi 1980). The Skeptical Inquirer displays all the most offensive atrocities. For example, Cole's (1978) short article contained numerous false statements on each page (Long n.d., 1978). Similarly, relative to Singer's

(1980) work on a psychic surgeon, Smiles (1980) and Kammann (n.d.) have "debunked" the "reality" of the work while remaining totally ignorant of what the work (which was anthropology, not parapsychology) was in process of studying.

To include and summarize as many thoughts as possible in such a brief "stimulus exchange comment," I would make these points: (1) While some parapsychologists have gone their independently productive ways, Mishlove seems correct in contending that defensiveness within parapsychology has led to a bizarre overemphasis on certain rather non-productive sorts of testing (and perhaps an over-production of statistical tests of all kinds). (2) On the whole, extreme skepticism, as best exemplified in The Skeptical Inquirer, has not seemed to enhance productive work (rather, parapsychologists have often been their own best monitors) but, in fact, has often seemed to produce more flagrant violations of honesty than parapsychologists ever have. (3) Anthropologists, as well as other scientists whose work is probably at least as questionable as that of parapsychologists, have demonstrated none of the defensiveness which Mishlove describes; yet, they are probably as well accepted within science as any other disciplines.

I regard this last point as being of great relevance for the "politics" of parapsychological anthropology. Hence, I have directed all of my own publishing energies towards anthropological publications, and others have done likewise. The effects of this have been encouraging. Over the past decade the interest in anthropology of the paranormal has vastly increased. With the advent of the Association for Transpersonal Anthropology (ATA, 164 Hawthorne Avenue, Palo Alto, CA 94301) and its journal, Phoenix: New Directions in the Study of man, and newsletter, Newsletter for the Anthropological Study of paranormal and Anomalistic Phenomena (which is to be renamed Newsletter of the Association for Transpersonal Anthropology), we are beginning to see the emergence of a genuine subdiscipline whose practitioners do not always have strong beliefs about the existence or non-existence of the paranormal but whose orientations have tended to be towards developing more genuine (at least, if one is an anthropologist) theoretical concerns than, e.g., the "truth" of reincarnation, telephathic communication, psychokinesis, or any such categories per se. (This is not to suggest that anthropologists do not recognize the need to carry out their own debunking of purportedly paranormal phenomena occasionally -- see de Mille 1980, Preuss 1978, and Strachan 1979.) The anthropological concerns presently focus on the adaptive nature of paranormal beliefs and practices and on the role of culture in determining these beliefs and practices.

Parenthetically, anthropological work on parapsychology probably benefits from one accidental variable. Anthropology departments are generally so small that few anthropologists could survive by "becoming parapsychologists." For example, I teach courses and publish research on ecology, physical anthropology, and deviance and attend more anthropology meetings than parapsychology ones. Hence, my anthropology colleagues know me first as an

anthropologists and aocept me on that basis; my parapsychological interests are then accepted in anthropology as natural outgrowths of other legitimate interests and not as ends in themselves.

In conclusion, I support Mishlove's views on the whole but also am inclined to think that his efforts might be more highly rewarded if he joined in support of the ATA rather than trying to reform conventional psychology.

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COMMENTS BY ANDREW NEHER:

The news, according to Jeffrey Mishlove, is that parapsychologists have forsaken "subjectivity." The reason: they have gone straight in their drive to be "scientific." The problem: subjectivity can be beneficial to science. Is he right? To answer this question, let's look at what Mishlove means by subjectivity. Although he does not clearly distinguish between them, he mentions several kinds (and neglects many more); we will consider them in roughly the order he does:

1. The application of findings from psi research to practical affairs. As with clinical psychology and medicine, applied psi would entail a large measure of subjective and even "artistic" judgment. Is this form of subjectivity valid? Well, it would be if psi existed, but as long as that issue is still unsettled, the verdict is "no."

2. The significance of psi findings to philosophical issues concerning the meaning of human existence. Again, this is a valid concern only if psi exists. Conclusion: this type of subjectivity is premature and thus invalid.

3. Personal experience as a source of hypotheses to be tested in controlled studies. No problem here; great advances in many fields happen this way (Kekulé's vision that revealed to him the structure of benzene is perhaps the most-cited).

4. Experimenter bias, in which the experimenter illegitimately influences the research outcome in a desired direction. Here we come to the most damaging form of subjectivity. Although Mishlove avoids discussing it, this problem has plagued parapsychology for many years, and many detractors cite it as the chief reason for their continued skepticism. Although it's unclear in your paper, Jeffrey, let's hope you're not advocating this type of subjectivity. It is true that, if psi exists, there would be no way to guard against experimenters psychically biasing the outcome of their studies. On the other hand, the fact that research results in the "hard" sciences are generally repeatable -- even by skeptics -- is an argument against the existence of a psi "experimenter effect."

5. Folk beliefs as a source of hypotheses for testing in controlled studies. No objection to this one. Certainly many folk beliefs and practices have led to scientific advances -- for example, many modern drugs are derived from plants originally used in folk cures.

6. Anthropological or sociological studies -- which of necessity are partly subjective -- of occult beliefs and practices. There is generally no objection to some subjectivity in such studies, but, in any case, this issue is irrelevant to the status of parapsychology.

7. Spontaneous experience as proof of psi. Although most parapsychologists realize that spontaneous psychic experiences do not allow the possibility of eliminating "normal" explanations, a few continue to indulge in this form of subjectivity. Mishlove's own book, The Roots of Consciousness, is a prime example. Louisa Rhine's Hidden Channels of the Mind is another. Please, Jeffrey, if this is what you mean by "subjectivity," we don't need it.

8. Subjective inspirations and motivations which direct researchers towards topics of interest to them. This kind of subjectivity not only is valid, but probably necessary to the scientific process.

So, what lesson can we draw from this consideration of subjectivity in parapsychology?

The lesson is that the issue isn't "more" or "less" subjectivity. We have seen that some kinds of subjectivity are useful to the scientific process--and objected to by none--while others are damaging. The real issue is, which kinds are characteristic of parapsychology and what have been the consequences? The answer is, parapsychology has through the years been guilty of several damaging "sins" of subjectivity. Of these, unquestionably the most serious is investigator bias (although Mishlove skirts this issue, a lengthy treatment

of the problem can be found in Hansel, ESP and Parapsychology; for a shorter treatment, see my book, The Psychology of Transcendence). The consequence is that parapsychology keeps coming up with "findings" that other investigators can't duplicate. That, Jeffrey Mishlove -- in spite of infighting among parapsychologists -- is the real issue. And, until it is resolved, that will keep the skeptics skeptical.

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COMMENTS BY JOHN PALMER:

Jeffrey Mishlove is correct in pointing out that the relationship between occultism and parapsychology has been an emotional and controversial issue, at least among members of the latter profession. As is often the case with emotional issues, the protagonists tend to see the controversy in black-and-white terms: in this case, occultism is all good or occultism is all bad.

Mishlove's article represents an admirable attempt to define a middle path. I agree that parapsychologists sometimes share with their own critics a tendency to throw out the baby with the bath water, but let us not forget in the process that the bath water can sometimes be pretty foul stuff.

More specifically, I think it is quite appropriate for parapsychologists to study the content of occult traditions as sources for research hypotheses. Parapsychology is still at such a primitive stage of development that it is simply foolhardy to shut off any potentially fruitful lines of inquiry. There is anecdotal evidence of powerful psi effects occurring in many occult traditions, and the lack of scientific verification of such effects does not necessarily make them invalid. Some of the prescriptions for facilitating psi that come from such traditions have received support in scientific parapsychological research, especially the induction of altered states of consciousness and belief in the phenomena in the context of their manifestation.

There certainly should be no objection to "the development of ethnographic studies, participant-observer sociological studies and educational evaluations" (p.2). In fact, I don't know of any parapsychologist who would object to these inquiries if conducted in a scientifically proper manner. I also don't know of any parapsychologist who would deny that personal experiences of psi can be a valuable source of insights to an investigator, provided he or she can then step outside of them for evaluation (admittedly a difficult maneuver that few are capable of). In short, I feel that Mishlove was sometimes beating down straw men in his paper.

What I find objectionable in most occult traditions is not their theories or knowledge claims per se, but their tendency to accept these theories or knowledge claims without adequate verification. Subjectivity and intuition are great sources of ideas, but objectivity is usually necessary to discover which ideas are valid or worthy of our acceptance. The absence of critical discrimination tends to cause profound insights and self-serving nonsense to coincide

in an undifferentiated blob the totality of which is then misperceived according to the a priori biases of the perceiver. It is this epistimological and methodological sloppiness, and only that, which the parapsychologist should reject out of hand.

Finally, I cannot share Mishlove's pessimism about the incapacity of the scientific method to deal with the experimenter effect in parapsychology. Social psychologists have been aware for years that experimenters can unintentionally influence the behavior of research subjects. Their solution was not to abandon science but simply to treat the experimenter as another variable or factor in the research design -- in other words, to look at the subjectivity objectively. Parapsychologists are quite capable of doing likewise, and this has been demonstrated in several recent psi experiments.

In any event, I fail to see how "subjective disciplines and modes of inquiry" can solve the problem posed by the experimenter effect, as Mishlove suggests it can. Pointing out possible limitations of the scientific method will have little impact, and deservedly so, until specific viable alternatives are forthcoming. We are still waiting.

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COMMENTS BY D. SCOTT ROGO:

In his paper "The Schism within Parapsychology" Dr. Mishlove raises several issues which deserve added comment. One theme which he brings forth is that parapsychologists seem to have developed a rather myopic attitude towards their own field--both as to its proper area of study and the role personal experience should play in the life of the parapsychologist. (Here I would like to point out that it was not Rex Stanford who first suggested that the researcher should seek to experience psi. It was actually the suggestion of C.J. Ducasse, the eminent philosopher and parapsychologist, who made the point some years ago in the International Journal of Parapsychology.) I share Mishlove's concern with these problems.

Researchers such as R.A. McConnell, the late J.B. Rhine, and other notable researchers have argued that parapsychology is rigidly defined as the study of ESP and PK to the exclusion of other areas of the "occult" or "borderline" sciences. However, this view cannot be supported if parapsychology is placed within an historical framework. Any student of parapsychological history will be able to point out that parapsychology has oftentimes had to redefine the boundaries of its study during the course of its short life. For instance, when the Society for Psychical Research was founded in 1882, one of its original committees was set up to explore the nature of hypnosis. Hypnosis and mesmerism were seen as legitimate parapsychological phenomena. Today the study of these areas of mental experience are explored in conventional psychology. They have been removed from the parapsychological scene. The early publications of the S.P.R. and the American S.P.R. were likewise filled with papers on a host of phenomena which the conventional psychological establishment of that day was wary of exploring, such as multiple personality, the nature of dissociation, trance, etc. Today we no longer study these phenomena in parapsychology, though the first psychical researchers were very concerned with them.

Remember, too, that many psychic investigators (such as Sir William Barrett and Lord Raleigh, who were both eminent scientists) were fascinated by the work and theories of the Baron von Reichenbach, a German scientist who believed in the existence of a Universal paraphysical energy to which the human organism was sensitive. A committee was even set up by the S.P.R. at its inception just to study these ideas and possible phenomena. Yet today, the parapsychological establishment would eschew any researcher who attempted to test Reichenbach's claims and theories--even though such a project would have been perfectly legitimate parapsychology in the 1880s.

In this respect, I would argue that a parapsychologist who wishes to study pyramid power, radionics, or Reich's "orgone," could justify his research on the grounds that such research falls under the purview of parapsychology as the science was originally conceived. (Parapsychologists love to point to their rich heritage, but only when it is convenient and flattering to do so.)

The point where McConnell, Rhine and others go far afield is when they refuse to realize that many "occult" or offbeat topics are experimentally testable. Parapsychology is, in the long run, the study of scientific wonders. What is considered, or not considered, the subject matter of the field is really more a matter of fad and fashion (as I showed above) than anything else. No researcher should be criticized for studying any offbeat phenomenon--such as radionics or pyramid power--so long as he does so experimentally and scientifically. If there really isn't anything to these fields (and I don't know if there is or isn't), good experimental explorations--not polemics about whether the subject is "scientific" or not--will resolve the issue. And if this research turns out positive...well, then parapsychologists will be morally obligated to confront a whole new range of mysteries.

Simply put, the subject matter of parapsychology has shifted so often during its history that it is myopic for anyone working in the field today to definitively state what parapsychologists should or should not be studying.

It is for this reason that, over the last few years, I have turned my attention to such areas of enquiry as UFO studies, "monster" reports, and religious miracles. These are topics with which established parapsychology does not usually concern itself.

But what if:

(1) A UFO witness begins having OBE-like experiences and ends up a poltergeist victim?

(2) A monster such as Bigfoot materialized in some forest area, breaks some twigs (to prove he's objectively there), but then dissolves right in front of a group of witnesses just like an apparition?

(3) A statue of Jesus or the Virgin Mary begins to bleed in a church, but only when a certain member of the congregation is physically present?

All of these wonders have been recorded in the literature devoted to these subjects. Being so, parapsychologists have a moral obligation to check out such stories, no matter how bizarre or outlandish they may seem. If they prove genuine, no one has the right to argue that these phenomena are not parapsychological in nature.

Always remember that, while parapsychology today is an experimental science, its roots lie within the realm of natural science--the science of observation. Today parapsychologists are once again becoming more and more interested in spontaneous case studies. And this point leads me to the second issue that Dr. Mishlove brings out in his paper. What role should personal experience play in the life of the parapsychologist?

Parapsychology is a richly introspective area of study. We all-so-often try to understand what is going on inside the mind of some one having a psychic experience. We do this so that we can isolate the requisite factors, reproduce them in our subjects, and--bingo!--get better test results. Most parapsychologists, while disdaining the idea that the researcher should make himself psychic, regularly run themselves in their own pilots to see how well they will do, and isolate any "bugs" in their protocols which might keep their subjects from psychically functioning. This habit is really tantamount to an admission by the psychologist that "being psychic" or "experiencing psi" is important for a researcher, since it helps him understand the area of his study. It is true that personal psychic experience has no great scientific value as such, but can be valuable in that it can be used by the researcher to generate testable ideas about the nature of psi.

What I guess I am saying in the long run is that parapsychology is actually a nebulous field of study. Being so, no parapsychologist has the right to look down or turn away from any area of "anomalistic science" that may eventually shed new light on the psi process. If parapsychologists wish to be considered scientists, perhaps they might begin by remembering what open-minded scientific inquiry is all about.

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COMMENTS BY GERTRUDE SCHMEIDLER:

This is odd. Every dilemma and gap and problem that Mishlove describes is real -- but I view them as showing basic unity behind diversity: methodological differences but no schisms.

Take his first example. He quotes a pleasant metaphor: that the parapsychologist and occultist have "no common ground" because "the former is attempting to pursue an exact science, whereas the latter is neither exact nor scientific." True, of course. We go by different routes; our paths do not now overlap. But we are both, I think, heading toward the same goal: to find how life and consciousness fit into the universe. Parapsychologists try to lay down a firm, straight roadbed and they make progress slowly; occultists travel easier routes which may lead to wild swings off the true course; but we can surely hope that the paths will eventually converge. There are differences in methods of inquiry; there is a big difference between the parapsychologist's delay in drawing any firm conclusion and the occultist's numerous certainties; parapsychologists deplore occultists' dicta

based on intuition; occultists deplore parapsychologists' limited findings; and yet there is an underlying similarity in the questions which both hope to solve.

Now one brief comment about a red herring. Mishlove compares parapsychology to "disciplines - such as physical education, music, drama, and psychotherapy." This is inappropriate. Parapsychology is a science, a particular type of discipline. Different rules apply here from those of art forms or of applied fields like education.

Mishlove next describes what he calls a schism within para-psychology: whether parapsychologists should or should not study topics called occult. But this is two separable issues. One is general and abstract; it concerns the proper function of parapsychology as a science. The other concerns each parapsychologist's individual decisions.

For parapsychology as a science, the directive seems to me to be unambiguous (and though I have not polled other parapsychologists, I fully expect them to concur). Parapsychology is limited to scientific methods. Scientific methods range from poorly controlled naturalistic observation to well controlled experimentation. All such methods are permissible. Poorly controlled methods, however, can at best lead to suggestions or speculations or hints toward a theory. What parapsychologists should do is to use any of these methods to study any topic they choose; but what they should not do is to draw a firm conclusion from their study if their research method was not well controlled.

For any particular parapsychologist, the directive depends upon further considerations. Perhaps the most important is whether the problem seems interesting, i.e., potentially important. If not, that person should not work on it. The next question is whether a good, well controlled method for studying it is available or can be devised. If the answer to this is affirmative, then it seems to me that the research should be performed whenever practical considerations permit, because it is the duty of a qualified person to make a contribution to knowledge.

But what if there is no rigorous way of investigating an interesting question? Here it is quite proper for individuals to respond differently, depending on their temperaments, their patience or impatience with the slow accumulation of information about ESP and PK, and their feelings about the value of speculations and suggestions. J. B. Rhine, for example, thought it not worthwhile to work on even the most interesting problems in the absence of rigorous methods; yet he encouraged Mrs. Rhine in her labor of classifying unauthenticated reports of spontaneous cases, which could lead her to only the most tentative of suggestions.

I see no schism here. I see only differences in personal preference, and a continuum of mutual tolerance mixed to varying degrees with mutual disapproval (the disapproval due to the expectation that the other person is wasting effort). Surely the history of science has taught us by now that an untraditional line of approach is sometimes fruitless but can sometimes be useful, and also that sometimes a painstaking, almost routine line of work which pushes our knowledge a decimal point nearer

accuracy is sometimes worthless or can be useful too. Our expectations about utility vary. One of us may think a particular project very, very likely to be useless; but it would be unjustifiably arrogant for anyone to state with certainty that it will be useless.

Unlike Mishlove, I see parapsychologists agreeing about the legitimacy of any well conducted inquiry; agreeing about the impropriety of firm conclusions where methods are weak; and differing only in their hunches about the future worth of any project.

This may seem like a naive statement of Utopian harmony, but in my opinion such harmony exists in the abstract, for general values. Decisions based on the values are another matter. The research worker whose time is limited, the administrator with limited research funds, or the editor with limited journal space must make yes-or-no, accept-or-reject decisions about one project after another. Even if all projects are valued affirmatively, some are valued more highly. Administrative and editorial decisions must set a cutoff point somewhere that is high up on the value scale. If Mishlove had written about the gap between accepted and unaccepted topics for a particular institution or a particular journal, he might have been able to show a clear separation, a true schism.

My argument is that such a schism in practical matters does not reflect a schism in evaluations about what parapsychologists should study or which of the scientific methods they should use. This repeats an argument of Gardner Murphy's, made in 1947 (Personality: A Biosocial Approach to Origins and Structure). He used as example a medical school admissions committee which was limited to some given number of acceptances. The difference between two applicants might be so small that the committee agrees it is negligible; but if only one place remains, one candidate will be accepted and the other rejected. Decisions about values create an apparent separation, even when the values themselves are seen to show only small differences along a continuous scale.

Mishlove brings up a third interesting issue: the gap between an emotional response to some datum and the same person's intellectual appraisal of it. Any experimenter must be well aware of this; I for one have often found my data bullying me to take a position that I feel is uncomfortable. It is part of the challenge of research in any field. We do research to learn something; we are not always happy with what we have learned. Eventually, of course, almost all of us resolve the dissonance either by conveniently forgetting or reinterpreting the uncomfortable finding or else by accepting it and becoming reconciled to it. Mishlove's point about an initial schism between cognition and affect in parapsychology is well taken, but the schism is not likely to be permanent and is not unique to parapsychology.

And neither is his major issue. Consider an analogy. One developmental psychologist follows a child through a day and tries to record all speech and behavior; these overwhelmingly rich observations under uncontrolled conditions lead only to tentative suggestions. A second developmental psychologist makes laboratory observations of random samples of children under two rigorously controlled conditions;

the narrow segment of behavior that is studied leads to a conclusion so specific that it carries little wisdom. A third overgeneralizes from past experience and tells us about THE four year old versus THE five year old. Do the three psychologists differ? Of course. Are the differences irreconcilable? They -- and we -- hope not. They follow different routes which have big methodological gaps between them, but they too expect their paths eventually to converge.

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COMMENTS BY ROGER W. WESCOTT:

One of the oldest jokes in Christian Tehological circles is that "mysticism begins in mist and ends in schism." Anyone interested in phychic phenomena can, I think, acknowledge the insight contained in this observation without necessarily subscribing to the implied scholastic animus against mystics.

The schism about which Dr. Mishlove writes so persuasively is the one between experimental parapsychologists, who insist on careful controls for all their studies, and fellow-traveling occultists, who will accept nearly any bizarre conclusion that excites their imaginations or satisfies their longings. At first, his reader is likely to infer that Mislove regards this schism as inevitable until such time as the occultists demonstrate willingness to accept the rigor and objectivity of positive science and abide by the results of impeccably controlled experiments. Increasingly, however, it becomes clear that he blames the schism at least as much on the narrow-mindedness of the scientists as on the gullibility of the laymen involved. And I am inclined to agree with him. For the investigator who repeatedly observes cryptesthesic behavior but never has cryptesthesic experience seems to me to be missing at least half of the parapsychological knowledge that is (at least potentially) available to him. Another way to put the matter is to note that, if only those events which can be precisely and continually replicated are scientifically reportable, then much of what occurs in our lives -- including what matters most to us -- becomes unreportable. And the version of reality which is then presented as science becomes, of necessity, a truncated version. Just as one who wants to know a house well must see it from inside as well as from outside, so must one who wants to gain perspective on his world interact with it subjectively as well as objectively. Intellectually, as otherwise, the goal is wholeness. A judicious blend of sceptical imagination and imaginative scepticism can, I believe, ultimately provide us with a picture of psychic phenomena and of other anomalies which is at once balanced and broad.

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COMMENTS BY MICHAEL WINKELMAN:

The schism which Mishlove addresses is not so much within parapsychology as it is between academic parapsychology and paranormal beliefs and practices, both historically and contemporarily. Mishlove's paper documents the growing recognition by parapsychologists of the need for a wider perspective and range of investigations within parapsychology.

The accomodation of parapsychology to the behavioristic stimulus-response attitude of mainstream psychology has acted as a blinder in the development of both methodology and content in parapsychology. Parapsychological research has been largely confined within the psychobiological paradigm, even though the assumptions upon which it is based were undermined early in Rhine's work. Parapsychologists are in the process of more clearly recognizing the fundamental conflicts of the findings and implications of parapsychological research with the traditional assumptions, values and metaphysic of mainstream western science.

The recognition of the ideologically incompatible relationship between parapsychology and mainstream science suggests the need to seek non-traditional methodology and content. Mishlove's suggestion that parapsychology become immersed (but not submerged) in the subjective explorations of psi or social systems which involve psi is particulary important for the development of parapsychology. In this century, systematic investigations of the paranormal have been largely confined to paradigmatically constrained laboratory investigations which ignore the larger reality of paranormal occurences and the traditional knowledge developed with respect to them. Parapsychology has "lost it roots". which it must recover in order to place its investigations in a context which will allow theoretical and methodological advance.

Parapsychologists face a problem in respect to psi and the paranormal which is very similar to the problem faced by academic psychologists in their invesigation of intelligence and cognitive processes. Like psychologists, parapsychologists have formulated their concepts and theories largely on the basis of individuals' performance in situations which are not represetative of the tasks and contexts which people routinely encounter and perform within everyday life. And like the psychologists who know little about how standardized test "intelligence" relates to success in life, parapsychologists know litte about how these statistically weak laboratory performances relate to psi abilities in everyday life. Progress in parapsychology requires that we base our investigations in phenomena which involve an adaptation to the principles of psi. The obvious candidates are the popular occult practices.

However, we do not need to characterize these new investigations as "subjective", especially in light of the pejorative connotation which such a designation carries in relation to "objective". We should begin, as Mishlove suggests with ethnographic investigations and participant observation studies within those systems which focus upon potential or notable psi processes. Admittedly, these kinds of studies may be more appropriately characterized as anthropological, and those who conceive of parapsychology as a laboratory science might be justified in seperating themselves from such investigations. Nonetheless, such studies are as integral to parapsychology and its development as ecology is to animal biology.

The development of field studies as a necessary part of understanding the social and cultural adaptation to the principles of psi phenomena brings parapsychologists face to face with the problem of how to deal with popularized versions of occult belief, as well as traditional cultures' paranormal beliefs. Obviously parapsychologists cannot adopt the position of arbitrators of what does or does not involve psi. Parapsychologists do not presently have the requisite knowledge to carry out meaningful and contextually relevant investigations of most assertions about psi processes in occult or traditional beliefs. We obviously cannot adopt an attitude of falsification in approaching the phenomena, since a single failure cannot suffice as evidence of the lack of the potential involvement of psi processes.

Any attempt to establish the possiblility of psi in occult or traditional practices requires an insider's understanding of the practice in order to establish our investigations in agreement with principles upon which the practice is based. In assessment of occult or traditional paranormal beliefs, we would be best advised to follow the example of the culturally sensitive crosscultural psychologists, and attempt to establish controlled situations in which the psi aspects of these practives are most likely to be manifested, rather than adopting an attitude or context directed at falsification.

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SCIENTIFIC LOGIC, IRRATIONALITY, AND SUBJECTIVITY IN PARAPSYCHOLOGY: RESPONSES TO COMMENTS ON MY ARTICLE

JEFFREY MISHLOVE

Primarily the sphere of objective law is the interplay of thoughts, emotions, memories and volitions in consciousness. In controlling volitions objective law controls also the correlations which are the physical counterparts of volitions.

--Sir Arthur Eddington, 1938

Of the comments which I have received thus far, those of Stanley Krippner, Joseph Long. D. Scott Rogo, Roger Wescott, and Michael Winkelman seem to be supportive of my major points. In this reply, I will address my thoughts to the comments of Richard Kammann, Andrew Neher, Gertrude Schmeidler, John Beloff and John Palmer.

The attitude of "skeptics" such as Kammann and Neher, as expressed in their comments, is that the only legitimate scientific approach in parapsychology must be limited to settling the debate as to whether psi exists. They are intolerant of my perspective and state that it will contaminate the scientific endeavor with the "'sins' of subjectivity" or with "a religious stance based upon faith and subjective validation." They are wrong, and I hope that they will see the error of their hasty conclusion. The stance which I advocate is legitimate in their own terms--since it is directed toward resolving the admittedly tricky and primary problems of repeatability and reliability of psi.

It is interesting to note that Dr. Kammann, who postures as a superpatriot of scientific rigor, evidences the most irrationality in his critique. Kammann's metaphor of a "choicepoint" between science and religion is unrelated to my essay and unsupported by his own arguments. I have never advocated abandoning empirical reality-testing, whereas I have distinctly advocated an agnostic (in which reverence and irreverence are balanced with good humor) and not a religious approach. I simply maintain that the experimental method can be fruitfully combined with other scientific, humanistic, and mystical disciplines--to achieve greater understanding of the psi-process and greater magnitudes of psi. As Helmut Schmidt (1979) has suggested such strategies can be accomplished without a loss of experimental rigor.

Kammann's irrational tendency to overgeneralize his criticisms of parapsychology have already been appropriately criticized by Robert Morris (1980, 1981). He, and other skeptics--other than Ray Hyman (Mishlove, 1980) who admits the insufficiencies in the contemporary skeptical position--have yet to honestly grapple with the best of parapsychology's old data (i.e., RNG studies, ganzfeld studies, etc.). Yet Kammann complains that I have presented no new data--as if I were obligated to do so. His spotty scholarship remains evident

in his suggestion that the decline-effect can "plausibly be attributed to progressive improvement of the experimental controls." To the contrary, the decline effect generally refers to a decline in scoring from the earlier trials to the later trials within a single experiment. Conditions from trial to trial are exactly identical. The decline effect stands in contradiction to Kammann's claim that "psychic results reveal no lawful properties."

Dr. Kammann is correct in noting that parapsychology has yet to achieve an unambiguously repeatable effect--if he means by this 100% repeatability. Yet, he commits an act of belief (in violation of scientific agnosticism) when he poses the problem, in the following paragraph, of "how so many people could believe in paranormal phenomena if they do not exist." He has made precisely the type of assumption about psi (albeit in the reverse direction) for which he would criticize me. And what is the result? Ironically, the outcome is some very interesting insights into the possible mechanisms of superstitious mentation (which Kammann would probably like to overgeneralize to include all psi-related beliefs). In spite of the religiosity of Dr. Kammann's skepticism, the outcome of scholarly interaction with him is not wholly negative.

Dr. Kammann thinks that I am advising parapsychologists "to believe in, to take as valid" psychic interpretations of their subjective experiences. This almost total misinterpretation of my position is clearly contradicted in the final paragraph of my essay. Disciplined personal exploration of ostensible psi (and its concomitants and ramifications), and disciplined objective exploration of the subjective dimensions of psi, and disciplined participant-observer explorations are, in fact, tangential to the belief systems of the investigator. Kammann's objection is a red herring--but not one without its unfortunate political impact. Disciplined subjective explorations have been stifled within parapsychology precisely because of the logical/emotional error which Kammann here evidences.

Philosophically, as well, Kammann is on shaky grounds. Popper's insight regarding falsifiability is very useful; however, it is not a totally adequate rule for demarcating between scientific and non-scientific propositions. The proposition that ESP does not exist, for example, is not technically falsifiable. I will always be able to find reasons for discounting possible falsifications as the result of fraud or delusion (that could eventually be detected if only skeptical scientists had the time and resources). Furthermore, the theory of falsifiability is itself unfalsifiable, and therefore unscientific by it own criteria (if rigidly applied).

I have been employing the term subjectivity in several different ways—and I appreciate Michael Winkleman's remark that it is often mistakenly given a pejorative connotation as implying "not objective." Perhaps, above all, I mean to introduce the forgotten Socratic dictum "Know Thyself" into the methodology of scientific parapsychology. Kammann's arrogance toward what he calls "religiosity" is understandable. His tendency to display the same "theory-driven judgements" that he is criticizing is both irritating and somewhat comical. This

type of projective behavior is a major source of unnecessary, superficial confusion in parapsychological debates. I think it amply illustrates the need for scholars to employ the subjective, humanistic disciplines of self-awareness--both intellectually and emotionally.

Andrew Neher's objections to my essay are similar to Dr. Kammann's although Neher is less religious in his objections to psi, and slightly more reasonable in his approach. Neher insists that as long as the issue of psi's existence is "unsettled" that the "application of findings from psi research to practical affairs" is not a valid area of exploration. There are two lines of rebuttal to this position.

First, Neher's logic assumes that psi's existence is somehow related to the debate over psi's existence. However, the debate is clearly colored by many philosophical, historical, sociological and emotional issues that are unrelated to the actual evidence (many hundred published experiments with statistically significant results). The debate is clearly conditioned by attitudes such as those of Martin Gardner (1957) who, in a moment of honest lucidity, admitted that he shared "an enormous, irrational prejudice...against even the possibility of extra-sensory mental powers" (p. 299). The debate highlights some of the subjective (and technical)issues that merit further exploration. But the fact that psi's existence is unsettled in the minds of skeptics cannot be used as an argument--as Neher consistently does--for stifling certain lines of inquiry.

If we wait for Andrew Neher to accept psi's existence, we may have to wait an unduly long time. Neher's The Psychology of Transcendence is one of the best skeptical texts available, and certainly good for beginning students. I especially noted his uncommon appreciation of the experiential dimensions of parapsychology, as it is popularly viewed--and would highly recommend the book for that reason. (Too many of my own students fail to distinguish between openness to personal experience, and uncritical acceptance of irrational belief systems.) Yet, by and large, his arguments against parapsychology were weak and typically failed to deal with the strongest cases. He recommended C.E.M. Hansel to his readers, for instance, and failed to mention the many serious critiques of Hansel's work. He suggested that the successful psi experiments were statistical flukes--ignoring the logistical impossibility of the millions of non-significant experiments that would be needed to counterbalance the significant studies published in refereed scientific journals. Additionally, Neher revealed little understanding of the probabilistic nature of psi. He cited Palmer's (1971) survey of sheep-goat experiment to support his contention of replication failures. Yet Neher failed to note that Palmer's survey supported statistical distributions comparable to those found by Schmeidler (i.e., six out of 17 experiments showed significant sheep-goat effects; in only four of seventeen studies did the goats obtain more positive ESP scores than the sheep, and none of these were statistically significant).

Secondly, skeptics must acknowledge the enormous variety of social activity which is predicated upon belief in psi. Much of this activity centers around popular claims regarding the application of psi for healing and diagnosis, crime detection, business forecasting,

agriculture, locating oil and minerals, archeology and military applications. Even if parapsychologists have been justified in not taking the lead in this area, what is the justification for failing to systematically monitor the progress of those who are moving in this direction--and to follow suit when the evidence warrants? There is a growing body of experimental literature and case studies which suggests that psi is being successfully applied (sometimes) in these areas. (My own files in applied parapsychology contain well over a hundred items.) Neher is, of course, correct in noting that the applied field does entail a large measure of subjective and even artistic judgement.

Neher states that enthnographic and participant-observer studies of occult practices are irrelevant to the status of parapsychology. This contention is short-sighted. I would rather offer as a model for future research a methodology--such as that currently being employed in Brazil by John F. Kennedy University student Patric Geisler--which combines empirical psi testing with the subjective ethnographic and participant-observer approaches. In fact, I would go so far as to state that every experiment in Parapsychology should simultaneously be treated as a participant-observer field study. In this way, perhaps we can get a handle on the unreported, subjective factors that may differentiate between successful and unsuccessful studies.

Dr. Gertrude Schmeidler is a creative researcher for whom I have great respect. In her comments, she expresses a vague "hope" that the paths of parapsychologists and occultists will eventually converge in the distant future. She also points out clear-cut distinctions she believes she has found between the methods of occultism and the idealized methods of science. Then, Dr. Schmeidler makes an even stronger claim--that the paths of parapsychology and occultism "do not now overlap"--with which I disagree. How ironic to maintain that parapsychologists delay in drawing any firm conclusions, while drawing a firm conclusion in the same paragraph that there are no overlapping areas between parapsychology and occultism. It seems that parapsychologists have "numerous certainties" of their own (while several schools of occult mysticism, i.e. sufis and Zen Buddhists, generally refrain from drawing any firm conclusions--preferring instead the irony of paradoxes).

From the standpoint of educational evaluation both parapsychology and occultism can be viewed as "critical competitors" offering overlapping educational services to individuals seeking guidance about psi. In my article. "Psi-Development Systems: Structures and Strategies" (1981), I list thirty-five different parameters for comparing and evaluating systems for cultivating psi abilities. Inspection of many of these parameters indicate overlapping approaches between parapsychology and various forms of occultism--that merit greater exploration and clarification.

Several types of occult mystics--i.e., schools of yoga, vedanta, Buddhism, anthroposophy--consider their approaches to be highly disciplined, exact, logical and even empirical. We may be justified in arguing that these ideals are not maintained in practice (or in theory, by our standards). However, are we not also vulnerable to the same criticism ourselves?

Dr. Schmeidler's idealized image of the parapsychologist as an objective scientist receives its most severe challenge from neither the skeptics nor the occultists--but from the growing critical literature within the philosophy and sociology of science itself. For an overview, I would recommend Michael J. Mahoney's book, Scientist as Subject: The Psychological Imperative (1976). Dr. Mahoney persuasively argues that the "storybook image" of the scientist--to which most scientists apparently subscribe--is, in fact, continually contradicted by the empirical evidence. The actual behavior of scientists suggest an image that, in practice, overlaps much more with occultism--in both the positive and negative senses in which this might be taken. The eminent philosopher of science, Paul Feyerabend (1978) argues that major advances in science necessarily require the violation of normal scientific rules and standards.

These points are renewed again by Sigmund Koch (1981) in his presidential address to the Divisions of General Psychology and of Philosophical Psychology at the meeting of the American Psychological Association in September 1979. Koch provides a list of fourteen "cognitive pathologies" which are conspicuously common to scientific inquiry. Koch believes that these pathologies form a syndrome of ameaningful thinking. He states:

> A syndrome of "ameaningful thinking" is seen to underlie much of modern scholarship, especially in the inquiring practices of the psychological sciences. Ameaningful thought regards knowledge as an almost automatic result of a self-corrective rule structure, a fail-proof heuristic, a methodology--rather than a discovery. In consequence, much of psychological history can be seen as a form of scientistic role playing which, however sophisticated, entails the trivialization, and even evasion, of significant problems. (p. 257)

In contrast to ameaningful thinking, Dr. Koch describes meaningful thinking in terms which may seem more familiar to mystics, poets and occultists than to scientists:

> In meaningful thinking, the mind caresses, flows joyously into, over, around, the relational matrix defined by the problem, the object. There is a merging of person and object or problem. Only the problem or object, its terms and relations, exist. And these are real in the fullest, most vivid, electric, undeniable way. It is a fair descriptive generalization to say that meaningful thinking is ontologistic in some primitive, accepting, artless, unselfconscious sense. (p. 260)

Koch's article contains a lengthy list of many philosophical/psychological questions which are of deep human interest--and certainly very germaine to experimental parapsychology. However, he maintains that they are not, in principle, susceptible to experimental manipulation. Koch's concluding point is in rather close agreement with my own position--if one were simply to substitute the word "parapsychology" for psychology:

> Fields like sensory and biological psychology may certainly be regarded as solidly within the family of the biological and, in some reaches, natural sciences. But psychologists must finally accept the circumstance that extensive and important sectors of psychological study require modes of inquiry rather more like those of the humanities than the sciences. And among these I would include areas traditionally considered "fundamental"--like perception, cognition, motivation, and learning, as well as much more obviously rarified fields as social psychology, psychopathology, personality, aesthetics, and the analysis of "creativity," (p. 269)

It is in this essential common interest in the humanities, in philosophy, and in education--which necessarily leads parapsychology to rely on disciplines in addition to strict empirical science. I find Dr. Schmeidler's attempts to deny such a wider range of methodologies to be, itself, philosophically (and methodologically) unsound. Her paragraph about the "red herring," for example, in which she attempts to refute my comparison of parapsychology with other academic disciplines that have successfully integrated both scientific and subjective approaches, is simply circular and based on a limited conception of parapsychology as a science. Furthermore, I believe that Dr. Schmeidler's dogmatic assertion, that parapsychology must follow different rules than an applied field such as education, stands in contrast with the considered opinion of several parapsychologists including no less a distinguished scientist than Gardner Murphy. Murphy (1969) expressed "considerable guilt and considerable blindness" (p. 3) in not having explored the cultivation of psi abilities earlier in his career. Murphy's plan of action clearly called for the incorporation of the distinctly subjective aspects of training within the parapsychological endeavor--as indicated by the following quotation:

> It seems to me that the cultivation of the paranormal gift is not unlike the cultivation of almost any other kind of gift, whether profound or ridiculous--whether learning to play Bach fugues on the piano or learning to wiggle one's ears. It makes no difference so far as the psychology of learning is concerned: it takes motivation, persistence and a great deal of blind pushing when you just plain don't know how. (p. 10)

Murphy stressed that this unsystematic, untuitive approach could be integrated with systematic record-keeping procedures. Above all, he stressed the development of a <u>teamwork</u> approach, incorporating both subjective and objective methodologies.

I heartily agree with Dr. Schmeidler's dictum that parapsychologists should not draw firm conclusions from their studies if their research methods were not well controlled. However, it is clear from recent parapsychological literature that "well-controlled experimentation" in parapsychology is something of a myth. All studies testing process-oriented secondary hypotheses, for instance, are known to be inescapably confounded by the possibility of experimenter psi. Thus, no firm conclusions can be drawn in this area which currently constitutes perhaps 90% of research in the field. Thus, most of the conclusions

reached in experimental parapsychology, although bathed in the halo of science, are barely firmer than conclusions reached through "softer" methods. Almost as many alternative hypotheses could be brought to bear in each type of study. I strongly believe that rigorously controlled experiments are invaluable in establishing the existence of psi, and in demonstrating useful physical systems for measuring psi. However, beyond these limited (yet highly important) goals, it is unclear to me that the experimental method has proven itself more efficient or effective than other approaches.

Dr. Schmeidler and Dr. John Palmer maintain that there really are no schisms within parapsychology. Nevertheless, Schmeidler admits that if one were to evaluate this question using operational criteria (allocation of research time and funds, publication in parapsychology journals) one might be able to demonstrate such an apparent schism. It would merely be, she claims, more the result of practical expediency than actual prejudice. This point seems reasonable enough, and might be well taken, were it not contradicted by Dr. Schmeidler's own efforts to limit the very definition of parapsychology, so as to automatically exclude applied educational approaches.

Certainly, a unified synthesis of different approaches within parapsychology, that Drs. Schmeidler and Palmer seem to feel has already been attained, is a goal to which I subscribe. Undoubtedly, they have both found success in pursuing such a balance in their own respective carreers. Clearly, however, this is not always the case. If John Palmer really does not "know of any parapsychologist who would object to these inquiries if conducted in a scientifically proper manner," then why did he find it necessary, in his 1979 PA presidential address, to urge his colleagues to "resist the temptation to avoid certain concepts and lines of research because of their political risks"? If Palmer is correct, why did Richard Reichbart (1980)begin his paper, "Castaneda and Parapsychology," with the following thoughts:

> When I decided to examine the relationship between the works of Carlos Casteneda and the field of parapsychology, I did not anticipate finding my task so difficult. But it has been, for a number of reasons. First, I have been cautioned by two of my fellow parapsychologists about getting involved in the controversy at all. One of them thought the don Juan series was fiction, the other thought it was factual, but both of them thought nothing could be gained for parapsychology by pursuing the issue. (p. 218)

Reichbart's experience is, by no means, an isolated example. Dr. John Beloff, who has acknowledged the prejudices which parapsychologists have held toward other neighboring disciplines, seems quite firm in his response that he is not yet ready to allow papers on such overlapping topics to be presented at the annual P.A. convention. He makes no mention of the scientific rigor of such papers. Beloff believes that we should "maintain a watching-brief" regarding neighboring paranormal claims, Such a "watching-brief" would be more scientific, in my opinion, if it entailed systematic evaluations

and investigations. Yet I understand Beloff to be quite uncomfortable about having the results of such investigations, however well conducted, formally presented to the parapsychology community. I hope this is not really what he intended to mean.

Incidently, although Beloff has taken pains to avoid being embarassed by a viewpoint which he believes I attribute to him, a careful reading of my original paper shows that I carefully avoid attributing any viewpoint to Dr. Beloff. My statement (when one reads the entire sentence from which he quotes me) is that he is reconsidering his old attitudes, not that he has adopted any specific stance whatsoever.

Throughout my response, I have been repeatedly pointing out the logical inconsistencies in the criticisms of "The Schism Within Parapsychology." The pattern has been rather consistent and suggests to me an underlying emotional stratum to this discussion which deserves further exploration. Dr. Schmeidler seems to feel that the emotional dissonance associated with parapsychology is something "almost all of us resolve," and is "not unique to parapsychology" in any case. I think that there is more to be said on the topic. I think that we need to define more precisely the emotional concomitants which distinguish successful ESP studies from those which are not successful.

Perhaps psi is simply capricious in its very nature. Perhaps the skeptics are right and it does not even exist. Perhaps, however, there are "hidden variables" (or to use Sir Arthur Eddington's term from my introduction, an "objective law") which can account for the uncertainties we experience in attempting to measure psi reliably. I think that it is a viable research strategy to assume that there are, indeed, hidden variables: the hidden variables are us. If we cannot logically eliminate experimenter psi, then we must learn to improve it and use it. I think that the systematic specification of the variables necessary for reliable repetition of psi research may eventually entail standards (for experimenters and subjects) of self-awareness and self-discipline which have yet to be attained, but which may result from a fusion of the scientific, humanistic, and mystical traditions. Perhaps I am wrong, but in principle it seems to be a testable strategy.

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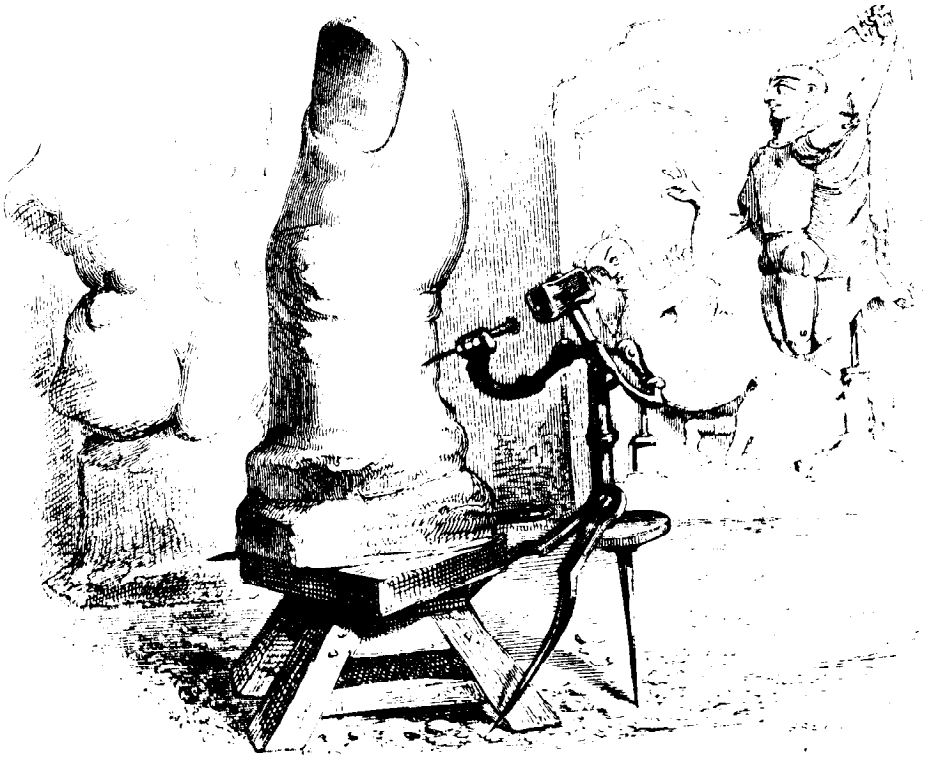

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ON CONDUCTING A ZETETIC DISCUSSION

LEONARD ZUSNE

I would like to comment on some fundamental issues involved in conducting a zetetic discussion, be it a dialogue or a debate. The Editor proposes to have dialogues, in the pages of Zetetic Scholar, among peers some of whom are proponents and some opponents of paranormal claims. The problem is that one's status as either a proponent or opponent cannot be completely determined in terms of the position taken on an issue. The proponent-opponent division intersects with and overlaps the believer-disbeliever division, and it is this latter dimension that determines whether a dialogue or a debate will ensue and whether there will be a hope for any sort of resolution of the issue involved.

The position taken by the Editor is one that tends to erase the distinction between discussants who might approach the same issue from two incompatible belief systems or conceptual frames of reference, especially when the influence of such systems is camouflaged by the use of the same language of science by both parties. It is stated that "science is essentially a method and not a specific body of empirical claims" (ZS, No. 5) and that, this being so, "any a priori assumptions about what is and is not impossible" are not required (ZS, No. 6). "Science," of course, is an abstraction, and as such can have no concrete body of knowledge, but each specific science does consist of an is characterized by the use of the scientific method and a systematic body of knowledge. This body of knowledge, incidently, contains statements of its own limits, that is, of what is and is not possible.

The use of the scientific method presents no special problem. Even in the most heated debate between parapsychologists and their opponents, the parapsychologists may admit, for instance, that certain controls were not instituted, or that their results might have also been produced by trickery. The discussion here is a dialogue in that both parties are operating within the same reference framework, abiding by the same rules. We can also speak of dialogues when two scientists, both of whom reject the concept of ESP, for instance, disagree either on some methodological point or point of theory and interpretation of the results. In the same fashion, parapsychologists have disagreed among themselves (and engaged in dialogues) over the

relative merits of statistical procedures and theoretical interpretations of the nature of psi. Dialogues are decidable, at least in principle, because the conceptual framework employed by the parties to dialogues is the same and the decision rules have been spelled out and accepted by them.

The situation becomes radically different when believers and disbelievers do not share such a reference framework. Dialogue becomes debate, and the debaters are indeed believers and disbelievers rather than proponents and opponents. They make statements that are based on incompatible conceptual frameworks or belief systems. The discussion is apt to center not on methodology but matters of substance, specifically the interpretation of what the results of certain observations, experiments, etc. mean. In spite of outward appearance of a dialogue between parties employing the same language, the discussion is really a debate that has little prospect of being settled because the real issue is not a matter of fact but of value, faith, and belief that existed long before the data or the phenomena that occasioned the discussion were observed. Each party may decide that he or she has won the debate – and it may be true – but only within his or her particular value system.

The parties to an undecidable debate are looking at the same thing from separate viewpoints. The distinct perspectives have even been given names: Pike (1967), for instance, has called them the emic and the etic viewpoints, that is, what things look like when seen by members of a group who share the same value system versus the viewpoint of the same phenomenon held by an outsider. If the belief system of the group calls for a belief in the power of their guru to levitate, then one sees levitation if one belongs to this group and shares their belief system. To an outsider, nothing extraordinary appears to be taking place. Or, what to parapsychologists may seem like massive and incontrovertible evidence of telepathy, may appear as pure coincidence to other scientists who acccept the existence of certain basic limiting principles of how nature works, among which is the impossibility of literally reading another person's mind. This is where the necessity of limiting discussion comes in. If the parties to a discussion are of the opposite persuasions in reference to the issue of paranormality, for instance, the discussion may never end, at least in theory, unless limits are set. When the Editor proposes not to set any limits to what is possible, combines it with a willingness to allow space for any discussion as long as a subject matter is seriously proposed, and as long as anyone is willing to answer the proposition seriously, some of the discussions would, in theory at least, never end. They would also serve no useful purpose.

Consider the Flat Earthers. Would the Editor permit space in the pages of ZS for a debate between Flat Earthers and whoever might want to refute them? Probably not, on the basis, I assume, that

such a debate would not be "scientific" enough, which is just the point. The flat Earth position is defended not so much because some literally believe that the Earth is flat on the basis of physical evidence for it, but because, for one reason or another, this particular form of nonconformity fits the belief system of a few people, and it is this belief system that is being defended (indirectly), and not the flat Earth proposition as such. But this is an extreme case. Would ZS sponsor a discussion between, say, the well-known psychologist-turned-parapsychologist Stanley Krippner and opponents of paranormal explanations? Probably yes. Would it make any difference if the Editor knew that Dr. Krippner has suggested that fairy-tale magic (human tissue, on being extracted from a patient by a psychic surgeon, turns into chicken tissue) should be given serious consideration as an alternative hypothesis to other, naturalistic hypotheses (Krippner & Villoldo, 1967)?

Debates involving participants operating within widely differing frames of reference have taken place in the past, of course. In psychology, for instance, we have the well known exchange between Carl Rogers and B. F. Skinner (in 1956) on the subject of freedom and control; between G. R. Price, J. B. Rhine, and others (in 1955-56) on the reality of ESP; and between S. Moss and D. C. Butler on one side and R. A. McConnell on the other (in 1978) on the same subject. The parties parted without having made the slightest dent in the armor of the opponent's convictions, and persuaded more than ever of the rightness of their respective positions.

There is the even older case of a disagreement on matters psychological between Sigmund Freud and Carl Gustav Jung. Both had been trained as psychiatrists, both had established practices, and initially both were in agreement on the main theses of psychoanalysis. Differences soon became apparent, however, so that, instead of becoming Freud's successor, as Jung was slated to, he left Freud's circle and started a psychoanalytical school of his own. Jung recognized, as Freud did not, that their disagreement had deeper roots than the matter of the relative emphasis on sexual factors in the etiology of neuroses. Freud was, in Jung's terms, an extrovert, while he, Jung, was an introvert. It was a matter of very fundamental differences in personality.

Human theories have always been a function of human personality and the particular world view that it gives rise to. All world views may be placed on a continuum, among others, whose ends are characterized by extreme subjectivity or objectivity. One view places an emphasis on and values the subjective side of life, inner experience, and thought, while the other stresses that which is tangible, objective, and deals with empirical facts. Rationalism and empiricism are two broad terms that describe these views or attitudes. William James described the rationalists as "tender-minded, intellectualistic,

idealistic, optimistic, religious, free-willed, and dogmatic," whereas the empiricists were "tough-minded, sensationalists, naturalistic, irreligious, fatalistic, and skeptical." J. F. Rychlak, a personality theorist, speaks of the "dialectic" and the "demonstrative" traditions. In the dialectic tradition one philosophizes, speculates, argues, believes in the power of the mind to arrive at the truth alone and unaided, using logic and introspection, and one holds to the principle that truth is demonstrated by self-consistency. In the demonstrative tradition the truth of an observation is validated by other observations, and these observations are not of oneself but of objects and events external to oneself. William James created the philosophical system of pragmatism in order to solve the dilemmas created by the perennial clash between these two world views.

I submit that the place where one stands on matters of anomalies, parapsychology, or the question of whether the subjective can autonomously transcend the objective has an intimate connection with one's world view. (There is probably another dimension involved — my colleague, Dr. Warren Jones, and I are currently working on research that involves these matters — but the subjective-objective dimension is the principal one). It is noteworthy that William James's attitude on psychical research was one that could have been predicted exactly from a consideration of his world view. From what the Editor has said in the pages of ZS and elsewhere, I gather that the Jamesian stance on matters anomalistic would be one that he finds to be a congenial one. James, we must note, however, considered not only the nature of belief but also the nature of the believer and the disbeliever. When it comes to anomalistic phenomena, to do otherwise is to deal with only half the story. For this reason anomalistic psychology, for instance, considers not only the psychology of extraordinary behaviors and experiences but also the psychology of those who tend to believe or disbelieve in them (Zusne & Jones, 1981).

Am I concluding from all of the above that discussions of the kind envisioned by Dr. Truzzi are useless? Of course not. They do serve a number of both latent and manifest functions. Among the latter are all those that are served by any scientific journal in any area — as long as the participants in the scientific exchange of information and opinion communicate within the same frame of reference. Changing the beliefs of those who have aligned themselves on the opposite sides of some emic-etic boundary is not such a function. Since their discussions are often clad in seemingly compatible linguistic trappings, it would behoove the Editor to identify such exchanges lest the canon of not prolonging debates beyond necessity be violated. How this can be done without also setting limits on what is possible I do not see.

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MARCELLO TRUZZI REPLIES:

Professor Zusne's points have been raised by other correspondents, so I am glad to take this opportunity to respond and perhaps to clarify the position of Zetetic Scholar on these matters.

Though I hope that authors of the dialogues in ZS will sometimes persuade one another to alter their views, the targets for these arguments are not the authors but are the general readership of ZS. By bringing together the best advocates in open discussions and cross-examinations, I hope that the rest of us, as part of the "jury" of science at large, can better understand the positions and make up our minds. If the author of a position presents poor argument or evidence, that will--hopefully--be ultimately displayed publicly for the rest of us to see. When a proponent or critic reveals him/herself using nonfalsifiable arguments or evading evidence put forward, I think the rest of us can go a long way towards determining whether that author is representing a legitimate scientific stance or merely engaged in pseudoscientific posturing. In general, acting as a kind of referee within ZS, I try to keep my own reactions to what I think are bad arguments out of ZS until the dialogue is completed by the advocates (though I will admit to intentionally picking some advocates because I think it likely that they will raise the critical points that I see needing to be raised). I have a great deal of faith in the scientific process of adjudication and feel confident that the truth will eventually emerge if we can get the real issues out into the open; and the cross-examination possible through dialogues seems an excellent way to get do that.

Professor Zusne assumes that I set no limits whatsoever. That is not the case. I try to restrict ZS space to discussion of protoscientific efforts, those I judge to be seeking to make their case within the ground rules set by science. I have rejected many metaphysical essays sent to ZS. In general, I try to err on the side of openness rather than closing doors to what might be good ideas. One ground rule I have used is whether or not the proponent is a solitary claimant or represents some organized protoscientific group. At present, the Flat Earthers do not represent such a group. It is conceivable that at some future time a protoscientific group could emerge that put forward an attemptedly scientific case for our earth being flat. Zusne is correct in saying that arguments for a flat earth are not "scientific enough" for ZS, but this is not merely because

the claim seems outrageous. It is because there is not (to my knowledge) any group of scientists (geologists, etc,) arguing a protoscientific case for this claim. (In fact, I am told that today the Flat Earth Society is largely made up of members who see the claim as a joke.)

Judging whether or not something represents a protoscientific effort or pseudoscience is a most complex matter. One indicator may be whether or not the research program being offered is only negative or whether it offers a constructive, positive program. Thus, in my view, groups that are interested in evolutionary anomalies and which seek to reconstruct evolutionary theory in some new direction are protoscientific. Even a group like the Ancient Astronaut Society which wishes to argue for extraterrestrial origins is potentially protoscientific (though I think much hampered by the apparent charlatanry in some of the arguments put forward by Erich von Däniken). It is this criterion that has generally resulted in ZS avoiding Creationism. It seems clear that Creationism couples anti-evolutionism with no positive scientific research program whatsoever. Instead, it seeks to use science to reject evolution and then asks us to turn through faith to biblical revelation. Unless the Creationists present a positive scientific program, which I do not think they yet have, their position is not protoscientific and actually represents a pseudoscientific (i.e., a theological but pretending to be scientifi) perspective. (ZS readers who disagree with my interpretation are certainly welcome to write and seek to convince me otherwise.)

I don't expect to see ZS Dialogues suddenly or magically transform matters. The zetetic posture demands that we show patience and tolerance and learn to tolerate doubt and the absence of certainty. But I do not share Zusne's pessemism about the final outcome. Individual proponents may come to the problems with incommensurable values that preclude their coming to a consensus, but I don't believe that is true for science as a whole. And I doubt that Professor Zusne believes in a relativistic view of science as mere incommensurable ideologies that make it all boil down to differences in emic viewpoint. Science is basically an etic enterprise, and to the degree that it uses the notion of emic perspective, that construct must itself be intersubjectively verifiable and falsifiable if it is to be incorporated into scientific discourse. That is, science must study subjectivity in an objective fashion if subjectivity is to enter into scientific examination.

Finally, a comment on Dr. Krippner's willingness to seriously consider the hypothesis that human tissue turned into the animal tissue later analyzed in the laboratory. There are degrees of seriousness. If we look at Krippner's actual reference to this "possibility," (p. 20), we see that Krippner is not seriously suggesting this alternative at all. He merely presents a discussion of the need to examine our underlying assumptions and proceeds to inventory a number of logically possible (though he makes no comment on their being empirically likely) alternatives. These alternatives are discussed in conjunction with a National Enquirer story purporting to debunk psychic surgery. Krippner's central argument is that the key fact needing to be established is whether or not any material object actually appeared paranormally during the "operation." There is no reason to conclude that Krippner takes seriously the actual likelihood that the human tissue turned into animal tissue, and my communications with Krippner about psychic surgery would actually indicate that he does not take such an alternative very seriously as an empirical matter. He only discusses the alternative "seriously" in this context as a logical matter. It may be exactly these kinds of matters that cross-examination in ZS Dialogues may be able to clear up.

ZS Dialogues

EVAN HARRIS WALKER REPLIES TO EDWARD W. KARNES'S REPLY (ZS #7) TO EVAN HARRIS WALKER'S COMMENTS ON EDWARD W. KARNES, ET AL., RE: REMOTE VIEWING (ZS #6):

Walker (1981) has given seven arguments against the acceptability of the Karnes et al's (1980) "Failure to Replicate Remote-Viewing Using Psychic Subjects." These are:

(1) Deviations from the protocol of the experiment being replicated. Karnes, et al made significant changes in the Remote Viewing experimental design (protocol) without giving arguments justifying the changes. The changes include modification of the kinds of records to be judged and

(2) The use of very large numbers of judges which hazzards "trivializing" the experiment.

(3) Sensory leakage between the experimenter who has manifested a bias and the judges who are in a position to impliment such a bias so as to alter the experimental results in favor of that bias.

(4) Use of a non-standard rank ordering procedure which involves the elimination of half of the material from the rank ordering statistical tests. The procedure used introduces additional degrees of freedom in the handling of the statistical data.

(5) Failure to establish the significance of a failure to replicate. A real phenomenon that yields a $p \leq 0.05$ significance does not automatically yield such a result each time the experiment is run. The significance of a failure to replicate, therefore, must itself be evaluated before conclusions can be drawn.

(6) Designation of subjects as psychic without testing subjects.

(7) Failure to include control tests to determine whether judges used actually had to distinguish correlations between transcripts and target site.

Karnes (1981) replies to argument #3 (also noted by Feldman (1981) and by Hoebens (1981) that sensory leakage is only to be criticized when positive results obtain. But bad experimental design speaks to the entire conduct of any experiment. It is good to see that Karnes, et al intend to repeat their experiment with provisions to be taken to avoid this problem of sensory leakage.

Karnes again replies to argument #3 by stating that "Walker suspects that our judges were biased against remote viewing because he suspects that the experimenters were so biased." Fortunately in science we can be more objective than this. It is not Walker's suspections that are of concern. It is that sensory leakage to the judges could occur in the experiment. Such leakage had the potential to produce an effect on the

outcome of the experiment, and the outcome of the experiment corresponded to the effect that this sensory leakage had the potential to produce. Therefore, the experiment was critically flawed.

Karnes has stated "I do share Walker's concern for bias possibilities in the judging procedure. In that regard, I suggest that he read Marks and Kammann's (1980) account of the multiple bias possibilities that existed in the judging procedures used in Puthoff and Targ's successful remote viewing experiments." It should be pointed out, however, that where the target has not been selected before the subject is isolated, and where time and ordering cues are removed from the transcript, bias on the part of the experimenter no longer plays a role in producing positive results. Experimenter bias, however, can still play a role in producing negative (null) results simply because the judge can match at random to void any chance of a positive outcome. Since Marks and Kammann failed to employ a control to validate their judges, their experiment involving the rejudging of a portion of the Puthoff and Targ transcripts is invalid.

Karnes also quotes Marks and Kammann's statement "We have, therefore, found evidence that Targ and Puthoff selected the nine experiments published in the Hammid Series from a larger set of experiments....obviously, if experimenters choose data they publish, their findings become totally meaningless." From my knowledge of the handling of the data and experiments, I can state that this accusation is in error. (The charge has been eliminated from Marks and Kammann's subsequent article submitted to Nature as a consequence of Puthoff and Targ having detailed the cause of Marks and Kammann's misunderstanding of the Puthoff and Targ report of the Hammid experiments.)

Karnes replies to argument #1 by stating that these changes in the protocol should not have had an adverse effect on the judge's ability to handle the material. The point, however, was that the protocol had been altered without giving justification.

But Karnes' reply highlights a most important deviation from the protocol of Puthoff and Targ. Each judge visited only one site against which all transcripts were judged. As a consequence, the judge does not know what features distinguish the target sites. Puthoff and Targ have stressed in their protocol that the target pool contains somewhat similar sites. The judge who does not know this may take as very significant a subject's statement that, say, the site has houses in the background. It is quite possible that all target sites contained houses in the background, and in fact that this was an irrelevant datum may have been obvious even to the subjects (for example if the tests were conducted in a large residential area). But the judge sees only one target site. The site would be paired to the transcript that mentions residential background even though this datum relative to the judging protocol of Puthoff and Targ is irrelevant. Again, why was the protocol altered?

Karnes answers argument #4 by stating that the statistical procedures were "quite optimum, standard, and few in number," Walker appears to be totally confused when he offers the criticism of our having 'sifted' or selected the data," and "Walker also appears to be confused about the

proper statistical analysis appropriate for handling the fact that we required the judges to identify the 8 protocols that best matched the target from the entire set of 16."

The problem with the Karnes, et al's statistics does not spring from a claim that Karnes, et al sifted their data. I did not make that charge. Instead I charged that Karnes, et al changed the statistical measures from the complete (meaning all data employed) rank ordering as used by Puthoff and Targ to an odd balled statistical analysis in which one first has the judges throw out 8 of 16 transcripts and only the remaining 8 are rank ordered. Then one applies a second analysis to see if the 8 thrown out is statistically significant vis a vis the target site. Why not rank order the entire set of date as is the correct application of the rank ordering statistic? That is the approach employed by Puthoff and Targ. Why change their rank ordering test procedure? Now, if this experiment had achieved positive rather than negative results, the argument could be made that the statistical significance is an artifact of the number of degrees of freedom permitted to the experimenter for analysing his data. It is clear that Karnes, et al's departure from the Puthoff and Targ protocol can be made in many ways and further that the particular handling of the statistics allows many variations as soon as one departs from the complete rank ordering of all data. Such handling of the rank ordering statistic would have been viewed with question and curiosity had Puthoff and Targ used it. I would have questioned their use of it had they so handled their data. I can not ask less of Karnes, et al. This is not a charge of sifting data. It is the requirement of complete rigor and unimpeachable procedure. Karnes, et al would require it of Puthoff and Targ; I would require it of anyone.

Karnes replies to argument #6 by quoting a statement by Puthoff and Targ (1978) that they have "carried out successful remote viewing experiments with about twenty participants, almost all of whom came to us without any prior experience....So far, we cannot identify a single individual who has not succeeded in a remote viewing task to his own satisfaction." Come now, Dr. Karnes. I quoted the statistics in my paper. Here you counter with the obviously qualitative "to his own satisfaction." Where unscreened subjects have been used by Puthoff and Targ as new and task naive subjects without prior test by other researchers, initial results have often been quite marginal. As Puthoff and Targ are experienced in their handling of subjects and experimental design, it would not be quite so surprising for them to have achieved statistically significant results for more marginal subjects. They have also reported results that failed to yield significance under such conditions. As I stated before, the record shows that one cannot depend on a subject's self-evaluation as a psychic as a basis for selecting subjects that have a potential for high schoring in Remote Viewing. The fact that task naive subjects do "frequently" or "occasionally" (depending on who is giving this qualitative judgement) give results that satisfy criteria for statistical significance does not permit the experimenter license in selecting subjects that he designates in scientific reports as psychic, regardless of his belief system, pro or con.

Karnes professes an ignorance of the relationship between probability measures and information measures. Information theory is sufficiently well represented in most technical libraries, so the theory need not be covered here. It is perhaps to be pointed out that any experiment yielding statistical significance does so because the experiment conveys information. In the case of the remote viewing experiments, the more information in the subject's

transcript that correlates to the target site, the more significant the statistical measure of results will be using robust statistical measures and competent judges. But information measures are obtained in exactly the same way and can be evaluated in terms of the Shannon Formula $I = \sum p_i \log_2 P_i$. Standard references explain the use of the formula. The point made in regard to the bits of information is that very few cues as to the correct matching of transcripts with their corresponding target sites are present in transcripts, even those that have the potential to yield statistical significance. As few as four or five bits of information may be present in the text being judged. Therefore, the judging task is far more critical than might at first be imagined.

Moreover, if Puthoff and Targ find significance at the $p \leq 0.05$ level using unselected subjects, as they have, and if this means the judges must discriminate the presence of as few cues as constitute four to five bits of information, then one cannot reasonably expect to find every experiment yielding significance. One must understand that the occurrance of these cues or bits of information are governed by the poisson distribution. Karnes has failed to understand this point.

Karnes has failed to reply adequately to this writer's criticisms. He completely failed to respond to those issues related to changing the Puthoff and Targ protocol without giving a rational for such changes. Thus Karnes, et al have indeed failed to replicate the remote viewing experiments. In his reply to this writer's criticisms, Karnes reveals further significant deviations from that protocol.

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EDWARD W. KARNES REPLIES TO EVAN HARRIS WALKER'S ABOVE COMMENTS:

Evan Harris Walker has reemphasized his previously stated conclusion (Walker, 1981) that our failure to successfully demonstrate remote viewing can be traced to procedural deviations from the protocols used by Puthoff and Targ. According to Walker's procedural-sensitivity hypothesis, remote viewing cannot be demonstrated unless an investigator uses proper subjects, proper judges, and the exact data-collection, judging, and statistical procedures used by Puthoff and Targ. I find it especially interesting that the remote viewing phenomenon that was widely proclaimed as being so robust now, at least in Walker's opinion, becomes so super-delicate. Also, it is of more than passing interest to note that the procedural sensitivity issue is being raised not by Puthoff and Targ but by reviewers who, to my knowledge, have not published or at least reported attempts to replicate remote viewing.

Walker discusses at length what he considers to be pitfalls that can occur in attempts to replicate paranormal phenomena. He identifies the judging procedures and statistical procedures as being especially critical. "Moreover, there is a clear hazzard (sic) that the use of 64 judges will trivialize (sic) the experiment...." (Walker, 1981, p. 133). "The statistical procedures are rendered invalid by a fault that has for quite some time become recognized by parapsychologists as a subtle but very serious hazzard (sic) in conducting parapsychological research. This hazzard (sic) enters whenever a less than optimum, other than standard, or a multiplicity of statistical analyses are carried out on a single set of data." (Walker, 1981, p. 134).

I commented on both of the above points previously (Karnes, 1981). Suffice it to say that Walker and I disagree. I think that the hazards that Walker belabors are not hazards at all, but rather post hoc reasons generated to account for the negative results that we obtained.

Walker's comments regarding subject adequacy are especially troublesome. Walker insists that replications require subjects who have been successfully tested for paranormal capabilities. This requirement for pre-testing is another example of Walker's post hoc rationalization for the negative results that we obtained. Surely, Walker is aware that there is considerable controversy regarding the validity of paranormal capabilities. Tests conducted by believers in psychic powers often find "gifted" subjects; tests run by skeptics usually fail to find such subjects. It is trite to say that hypothesized psychic capabilities are not like normal sensory capabilities. For example, if a blind man claims to have gained the capability to see, his possession of visual capabilities can be verified by those who do not believe as well as those who do believe his claim. That, however, is not the situation encountered when testing for psychic capabilities. In my experiments, I have run over 120 subjects

including samples of selected and unselected college students and self-proclaimed psychics in single and multiple remote viewing trials, and I have not yet found a "gifted" subject. Targ and Puthoff (1977) state, "In our experiments, we have never found anyone who could not learn to perceive scenes.....blocked from ordinary perception." (p.5). I have never found anyone who could learn to perceive scenes blocked from ordinary perception. I have, however, observed a few apparent "successes" in remote viewing. But, in all instances, the "successes" could be explained without resorting to paranormal interpretations. I think that the best definition of a psychic is the operational definition: a psychic is someone who claims to possess psychic abilities.

Walker discusses and attempts to clarify his earlier comments regarding data sifting and data selection. In response to his initial comments, I emphasized the difference between the experimenter's sifting and/or selection of data and the sifting of data by judges as a required procedure in the judging process. I cited, in way of clarification, the possibility of data selection in the Hammid series of Puthoff and Targ's experiments as discussed by Marks and Kammann (1980). It has been subsequently brought to my attention that the allegations of data selection in the Hammid series have been retracted by Marks and Kammann, and I regret using that possibility as an example. I did not, however, accuse Puthoff and Targ of data selection; I simply cited the allegation to clarify a point in regards to Walker's comments.

In his current comments, Walker again refers to the equation of levels of significance and information measures and again he is exceptionally vague on the basis of his reasoning. It is not the relationship between probability measures and information measures that is of concern. The equation of a probability of .05 and 4.32 bits of information is not a point of confusion. What is unclear is how an obtained significance level of .05 relates to 4.32 bits of information in the individual receiver's transcripts. More specifically, just how does the 4.32 bits relate to the number of relevant matching statements or aspects of the receiver's drawings? How does one compute the number or percentage of relevant matching statements in a transcript from the "bits of information" measure that Walker ties to the level of significance?

Finally, Walker read our original report in too hasty a manner. He believes that I revealed additional significant deviations from the Puthoff and Targ protocols in my response to his comments. He is mistaken in that belief. Specifically, Walker correctly notes that we required each judge to visit only one target site and to match all of the receivers' transcripts to that target site. That procedure was clearly identified in the report (Karnes, Susman, Klusman, and Turcotte, 1980). Walker is concerned that when the targets have similar features, the judges may be confused by relevant and/or irrelevant data in the transcripts. His reasoning in that regard is not convincing, but he should be informed that our targets were chosen for architectural/environmental distinctiveness. The similarities among our target sites were exceptionally minimal.

He also questions our reasons for using that judging procedure. It was used to lessen the time burden on individual judges and to hopefully enhance their dedication to the judging task. In other experiments, I have used the same judging procedure as that used by Puthoff and Targ, i.e., individual judges visited all target sites and evaluated all transcripts. No evidence for remote viewing was obtained when that procedure was followed, and I was concerned that the time burden may have had a detrimental influence on the judges' motivation and dedication to the judging task.

Walker's misunderstanding about the judging procedure also provides another example of an impressive ability to generate post hoc reasons for negative results. In his earlier comments, he objected to the burdensome nature of the judging task. "The embellishment of Puthoff and Targ's protocol in which the senders.....take movies of the target site and make tape recordings of their impressions, while seemingly more 'scientific' can easily adversely impact the ability of the judges to cope with the extensive data to be judged." (Walker, 1981, p. 132). Now that his misunderstanding about the burdensome nature of the judging task has been corrected, the fact that the procedure differed from Puthoff and Targ's procedure becomes a reason for the negative results.

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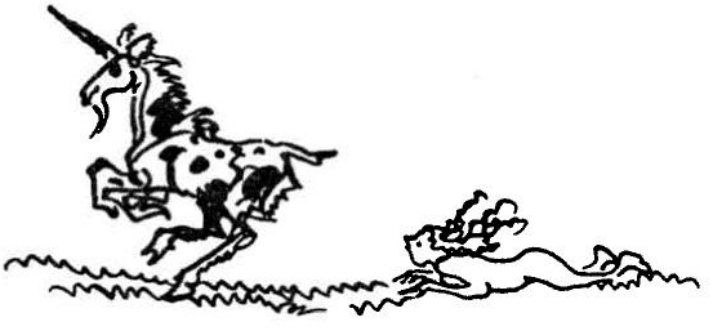

BOOK REVIEWS

Flim-Flam! : The Truth about Unicorns, Parapsychology, and Other Delusions. By James Randi. New York: Lippincott & Crowell, 1980. xii +340 pages. $12.95 Cloth. Illustrated, bibliography and index.

Reviewed by Milborne Christopher

Wham bam -- here's Randi's Flim-Flam: Dismissing unicorns with six lines -- "no reliable reports exist to verify the reality of this animal" -- the peppery Canadian magician, escapologist, and crusader against irrational thinking slashes out at fairy photographs, Bermuda Triangle myths, UFO sightings, Stanford Research Institute ventures in remote viewing, biorhythm theories, psychic surgery, and other subjects that intrigue readers of Fate, National Enquirer, The Star and the journals of the British and the American societies for psychical research.

The Committee for Scientific Investigation of Claims of the Paranormal "hitman," as Isaac Asimov terms him in the introduction, states unequivocally, "I will hit as hard as I can, as often as I can, and sometimes quite bluntly and even rudely." To him, scientists who insist that gifted children can bend metal without manual pressure are "nincompoops"; Maharishi Mahesh Yogi, introducer of a TM levitation that occurs only in the minds of the would-be body raisers, is "The Giggling Guru"; Russell Targ and Harold Puthoff, advocates of Uri Geller, Ingo Swann, and you-can-do-it-too astral projections, are "The Laurel and Hardy of Psi." Yet, though he chops with a saber, Randi scores almost as often as a less rambunctious wordsman would with a rapier.

In an earlier onslaught, The Magic of Uri Geller, Randi dissected the purported superman feats of this pretender; now he adds the confessions of Yasha Katz, Geller's former manager. Katz admitted that he gathered information for Uri, signalled him from the audience, and secretly opened a desk in a San Francisco television studio to sight a target design that was there in an envelope.

It is a pity that Randi was not invited to observe the SRI mind-reach experiments, to see the areas in the building complex where Geller was tested, or to view the films and tapes stored there. Even so, relying on published material and the statements of eye-witnesses about various feats, he makes a strong case against poor controls and faulty observations.

Probers of the alleged paranormal should study Randi's descriptions of his confrontations with "clairvoyant" card-manipulator Suzie Cottrell, metal-bender Jean-Pierre Girard, and magneto-therapist Sue Wallace. Note how the tapes were made and how they confirmed the conclusions of Randi and Martin Gardner in the Cottrell episode. Note the precautions taken in France during the Girard encounter. Note the details of the Wallace testing.

Remember the way it was made obvious at Camp Silver Bell in Ephrata, Pennsylvania, that a message reader was using the on-ahead system, not advice from a disembodied spirit. There are also lessons to be learned for investigators in Randi's tales of his sessions with Italian psychics. Giuseppe Festa, the food-mummifier, for instance, employed no trickery. He attempted to stop the decay of chopped beef, chicken breasts, and veal. Under test conditions, his anti-putrefaction influence just didn't work.

It is to be hoped that parapsychologist Thelma Moss reads the "Off the Deep End" chapter. In her book, The Probability of the Impossible, she cites the lifting of a person from a chair by the extended fingers of four volunteers as a mind-boggling feat of levitation, one that baffled science. It should be emphasized that Geller has presented this stunt as a demonstration of psychic ability. Actually, it is a centuries old parlor recreation. Randi tells how it is done, and explains why it works.

"Such former wonder-workers as Uri Geller and Jean-Pierre Girard no longer seem to attract the attention of the academic world, though they are still of some small interest to a shrinking public. This book may extinguish that last spark," Randi says. This is wishful thinking. The miracle mongers continue to appeal to the gullible. The Star rather than the National Enquirer is now the principal outlet for sensational psychic stories. Uri Geller, again up to his old tricks, has been featured in Star headlines and spreads. He has slimmed down and after a long absence has appeared on a syndicated talk program, "The Merv Griffin Show," causing a compass needle to move when he bent down so that his mouth approached it, and creating the illusion that he made a key warp with mystical power. Magicians, Geller said, could duplicate his feats with trickery, but he was "real." This time around, however, Griffin did not treat him with the deference of other days. When Uri announced that he was a father, Griffin's eyes widened and he said, "I didn't know that you were married." Geller replied that he was not; the mother was the young woman with whom he had lived. From this point on both Griffin's and the studio audience's interest faded.

It is likely that Geller has read Flim-Flim! He collects books in which he is mentioned. So few volumes critical of him, or phases of psi research, are printed that Randi's will be welcomed, if not by Uri, by those who seek for explanations of seemingly inexplicable marvels.

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Editor's Note: Since this review was received, much controversy has developed around this book's accuracy of detail. Interested readers are urged to begin perhaps with the article "James Randi, the 'Flim-Flam' Man," by D. Scott Rogo in the June 1981 issue of Fate magazine which is expected to carry Randi's rebuttal and further exchanges relevant to these issues. Cautious readers might also wish to examine "The Pseudoscience of Antiufology" (mimeo, 1981) by Stanton T. Friedman (and available from the author at 110 Kings College Road; Fredericton, New Brunswick E3B 2E7, Canada). --MT

The Astrology Game. By Malcolm Dean. Beaufort Books, New York, 1981. 360 pp. $15.95.

Reviewed by Don. H. Saklofske*

A number of recent surveys of astrology have dealt critically with its systems and validity from the perspective of the contemporary researcher.[1] Dean's book is an attempt to vindicate the "science" from such assaults on its veracity by his own interpretation of the present status of information and by primitive evaluation of selected research. His rejoinder to the scientific world fails to convince.

Throughout the book it is unclear to the reader to which astrology Dean attaches himself. At times he views astrology in a wide vacuous sense as the study of relationships between individuals and cosmic cycles (pp. 57, 66, 110, 151, 263). Later he informs us that the more responsible astrologers are humanistic astrologers, those who eschew any empirical approach to astrology and claim that one's future depends on oneself and not on the planets and stars (pp. 17, 280). In yet another section of the book he tells us that, unlike scientists who deal with statistical probabilities and causal mechanisms, astrologers rarely speak of probabilities in their predictions or character analyses, but, rather, they contend that the universe operates according to the acausal principle of synchronicity, so that chance does not play a role in the universe (p. 237). However, Dean most often argues for an astrology which ties human behavior to "planetary influences" (pp. vii, 99, 205, 270-1). This is the classic heads I win, tails you lose situation; if "planetary influences" (of any nature) are shown to affect terrestrial events, Dean can call it "astrology"; but, on the other hand, if such "influences" are eventually shown to be insignificant or nonexistent, he can opt for the synchronous universe or a form of Humanistic astrology.

Dean's acceptance of evidence is based more on his willingness to agree or disagree with the results than with an appraisal of the scientific methodologies and statistical analyses employed. The main scientific evidence cited by Dean to support astrology is the statistically significant but marginal relationships between personality variables and planetary positions discovered by the Gauquelins. Dean readily accepts the positive results of the Gauquelins but balks at accepting their data that is negative to astrology. He describes these negative results regarding zodiac signs as "a major puzzle" (p. 263) and gives us an aside that perhaps statistics is too young a science to deal with the subtle relationships between zodiac signs and human relationships (p. 231). He also refuses to accept their negative evidence of astrological 'houses' on the basis of the seeming argument that people's career choices are based on various cognitive factors such as the individual's beliefs; hence "it is possible for the traditional astrologer to argue that modern statistics are, in fact, confirming the essence of astrology, not disproving it"!! (p. 244)

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* *I would like to thank Ivan W. Kelly for his computations and help.*

In the final chapter of the book, "The Astrology of the Future," Dean informs us of "promising techniques" in astrology that are, in reality, based on a variety of methodological flaws. One technique supposedly involves the determination of relationships between planetary groupings and significant terrestrial events such as wars. Of this technique, one astrologer, Robert Hand, notes:

> that during the nineteenth century, the graph does not correspond well with events if Pluto is included. Without Pluto...there seems to exist clearer correspondences. The 'Pluto effect' on the graph only becomes apparent during the waxing Uranus-Neptune hemicycle, due to the position of Pluto either within or without the Uranus-Neptune arc. (in Dean, p. 359)

Also, on pp. 312-3 Dean summarizes the findings of astrologer Jeri Blake (author of When Presidents Die) to support Blake's claims that an analysis of planetary configurations of past U.S. presidential elections reveals that combinations of Saturn-Neptune and Jupiter-Uranus are present when presidents are assassinated. Since this combination is not present for the 1980 election, there will not be a presidential assassination but there is:

> an effective Saturn-Uranus (aspect) -- a pattern which we found in cycles covering two of the four presidential deaths from other causes (than assassination). A Mercury-Neptune combination, present in 1980, appeared in three of seven unsuccessful assassination attempts. (Blake, quoted in Dean, p. 313)

Dean and the astrologers he cites seem to be unaware that the mathematical possibilities of astrology give them endless opportunities to obtain celestial-terrestrial event matches which they can try out until a match is obtained and then report it in the astrological literature.

Dean argues that two astrologers pointed out the probable existance of Pluto's moon Chiron before its astronomical discovery in 1977 (p. 296). A close look at what the astrologer's actually said reduces our confidence in this statement. When one reads on, one finds that Dane Rudhyar was talking of a moon with an orbit entering the sun's photosphere and passing out to Saturn in five years. Rudhyar is obviously not talking about Chiron at all. The other astrologer, Charles Jayne, published an article in 1961 on hypothetical planets and predicted that a planet would be discovered in 1975 with an orbital cycle of 50 + 2 years which is very close to the cycle of Chiron. However, Jayne's other statements about "Chiron" leave much to be desired. In a later paper (not mentioned by Dean), "The Unknown Planets" (1974), Jayne tells us that six astrologers independently predicted the position of the planet Apollon which purportedly is 75% further from the sun than Pluto. If the astrologers have such remarkable abilities that they can detect the presence of planets at such great distances, how much easier it should be for them to describe very accurately the existence of so-far unnoticed bodies in our own solar system. But where were the astrologers'

predictions when Pluto was discovered in 1930 or the moons of Neptune and Uranus were discovered in the 1940's? And when did Jayne and the astrologers predict the three recently discovered moons of Saturn? Can astrology be taken seriously by scholars when the data are so arbitrarily manipulated, with the resulting devaluation of scientific criteria?

Dean accuses others of poor scholarship while indulging in it himself. For example, he claims that astronomers Culver and Ianna (authors of the critical work on astrology The Gemini Syndrome) have miscalculated every single date they list for the Jupiter-Saturn conjunctions of the so-called "twenty-year presidential death cycle" (p. 311). For the record, each of the dates listed in The Gemini Syndrome are taken from the American Ephemeris and Nautical Almanac and a recheck indicates that all of the dates they calculated, save one, are correct. (The incorrect one is listed as January 26, 1841, but should be January 25, 1842).[2] This may be a typographical error (not unlike Dean's reference to Dr. Roger Culver as "Dr. Robert Culver" on p.310!). One suspects that the discrepancies between Dean's dates and those of Culver and Ianna arise from the difference between the astronomical definition of conjunction (angular separation between the two objects is at a minimum value) and the astrological definition (same celestial longitude for both objects). In any event, the exact dates of these conjunctions are really not germane to the discussion of the Twenty Year Sequence. In fact, Dean's struggles to explain away the points Culver and Ianna raise provide a classic illustration of the astrological "empiricism" described in Chapter 7 of The Gemini Syndrome. Dean comments that "The truth of the matter is that the data is (sic) far too limited for any kind of acceptable scientific test" (p. 311), and yet continues merrily along with a discussion of the "Jupiter-Saturn cycle."

Dean offers several arguments for the validity of astrology, none of which are even marginally persuasive. He tells us that many psychologists, psychiatrists, astrologers and many critical clients claim that astrology "works" (p. 46). Apart from the vagueness of "many," and the vagaries of anecdotal evidence, even if astrology "works" it may not be for the reasons its proponents contend it does. Dean cavalierly rejects the psychological explanation for the success of horoscopes with clients (i.e., the use of Barnum statements, cold reading, etc.) but fails to provide evidence that the astrological account for the horoscope reading success is superior. A related argument is advanced later where Dean points out that many astrologers contend that one has to actually work with horoscopes before one can observe the powers of astrology (p. 235). The well of the critic has been effectively poisoned by the latter argument. Anything that the critic has to say is discredited before he ever begins. But, of course, the truth or falsity of astrology can in no way be inferred from the personal position of advocate or critic, it rests on the evidence brought to bear on the issue.

In a chapter on archaeoastrology, Dean points out that astrology played an important role in many early civilizations, a role that has been neglected by historians. Implicit in this chapter is the argument that since astrology could be found in so many early cultures

there must be "something" to it. This is a non sequitur. The fact that it is ancient and widely held does not make it valid as the commonly held but false geocentric hypothesis demonstrates.

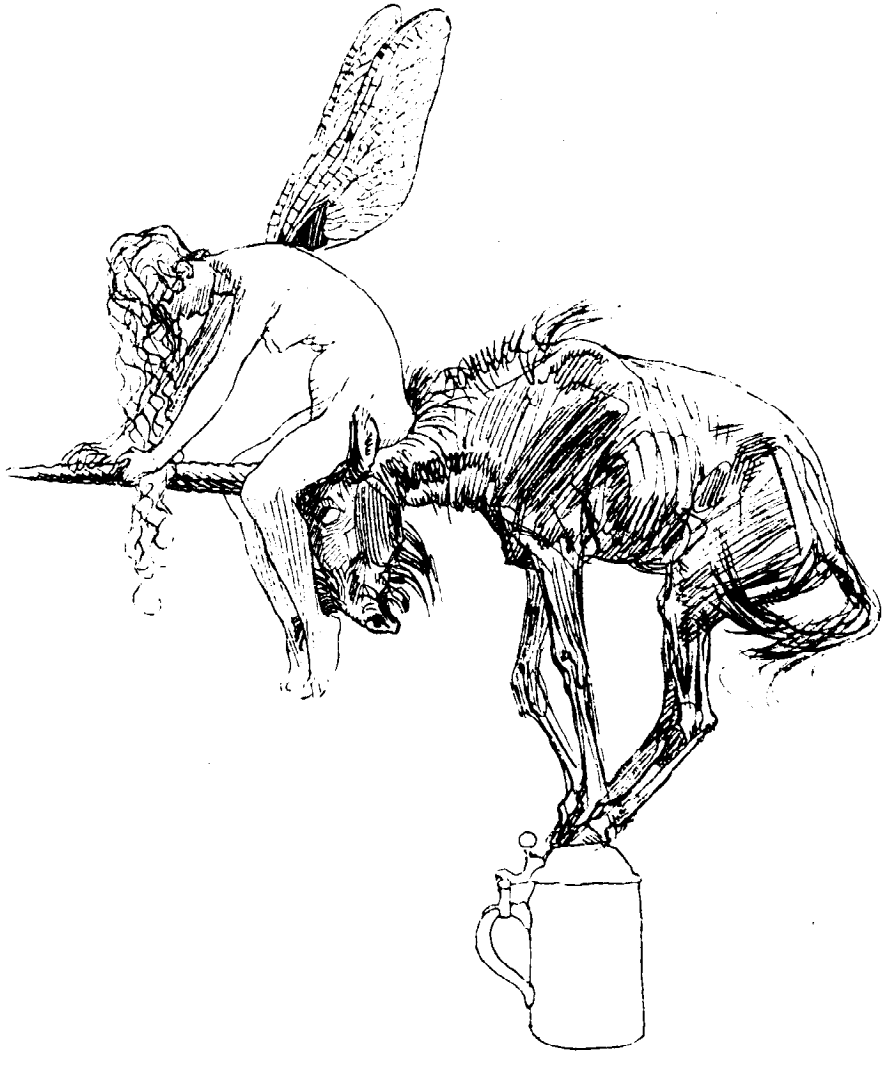
Dean tells us that we must prepare for an "astrological revolution" presently in the making (pp. 131, 154). However, after reading the evidence Dean provides for this, one strongly suspects that, like the reports of Mark Twain's death, the claim is greatly exaggerated. Finally, whatever the flaws in this book, one cannot argue against Mr. Dean's statement that "The two world views (of science and astrology) are light years apart" (p. 312).

1. G. Dean & A. Mather. Recent Advances in Natal Astrology. Para Research, 1977.

   I. Kelly. Astrology and Science. Psychological Reports, 44, 1979.

   R. Culver & P. Ianna. The Gemini Syndrome. Pachart, 1979.

2. I would like to thank Roger Culver and Phillip Ianna for their correspondence on this issue.



BOOKS BRIEFLY NOTED*

* *Listing here does not preclude later full review.*
* *Critical annotations are by Marcello Truzzi.*

Arnold, Larry E., The Parapsychological Impact of the Accident at Three Mile Island: A Forewarning of Disaster - The Message of TMI-2. Harrisburg, Pa.: Parascience International (1025 Miller Lane; Harrisburg, PA 17110), 1980. 106pp. $5.95 paperback. Report of an investigation into allegedly precognitive reactions to the nuclear reactor crisis. The 54 cases examined are provocative and the comparison with premonitions by passengers of the Titanic is interesting, but the study is methodologically very weak and insubstantial for its strong conclusions.

Bandler. Richard, and John Grinder, The Structure of Magic, I: A Book about Language and Therapy. Palo Alto, Cal.: Science and Behavior Books, 1975. 225+xv pp. $7.95. A linguistic analysis of counselling interaction by talented psychotherapists of divergent orientations seeking common structures. A remarkable book that has great relevance forquasi-therapy as well as orthodox practice.

Bartlett, Laile E., Psi Trek. New York: McGraw-Hill, 1981. 337+ ix pp. $12.95. A general and entertaining journalistic tour of current activities on the psi scene. A great deal of useful information and apparently sincerely done; but the author is remarkably uncritical towards the psychics she interviews,and the book should not be mistaken for a scientific work of ethnography despite the author's advertised credentials as a sociologist.

Barton, Robert, The Oceans. New York: Facts-on-File, 1980. 336pp. $19.95. A lavishly illustrated full-color survey on all aspects of the ocean and ocean life. Absence of cryptoichthyological topics like mermaids or sea serpents, but nice sections on Atlantis and on continental drift and a surprisingly uninformed section largely accepting the myth of the Bermuda triangle.

Behnke, Leo, editor, Impromptu Magic from the Magic Castle. Los Angeles, Cal.: J.P. Tarcher, 1980. 236pp. $11.95. A good but somewhat uneven middle-level conjuring collection for the amateur. Some good effects simulating paranormal abilities.

Bord, Janet and Colin, Alien Animals. Frogmore, St. Albans, Herts.: Granada, 1980. 258+iv pp. 7.95 pounds (U.K.). An excellent introduction to cryptozoology of the wilder kind, well written, heavily illustrated and well researched. Not a scientific work, but an excellent Fortean investigation deserving attention of anyone interested in tales of strange creatures not only for an excellent survey but for a provocative analysis.

Brau, Jean-Louis, Helen Weaver, and Allan Edmands, edited by Helen Weaver, Larousse Encyclopedia of Astrology. New York: McGraw-Hill, 1980. 308+xii pp. $17.95. A very useful book for its

wide coverage of topics and persons, including many photos and illustrations. Strictly an uncritical work but an excellent reference work on astrology and astrologers.

Briggs, Katherine M., Paul Hecate's Team: An Examination of the Beliefs on Witchcraft and Magic among Shakespeare's Contemporaries and His Immediate Successors. New York: Arno Press, 1977. 291+viii pp. $22.00. A reprint of the 1962 study by the late Dr. Briggs, probably best known for her many fine works on folk and fairy lore in the British Isles. A classic work and a necessity for anyone interested in the history of magic. Highly recommended,

Corliss, William R., compiler, Incredible Life: A Handbook of Biological Mysteries. Glen Arm, Md.: Sourcebook Project, 1981. 1018+vi pp. $22.50 Another in the superb series of sourcebooks put together by the indefatigable Mr. Corliss. Absolute necessities for any serious Fortean or anomaly collector, but also a wonderful book for browsing in and a highly recommended stimulus for any science fiction writer. Highly recommended.

Culver, R.B., and P.A. Janna, The Gemini Syndrome: Star Wars of the Oldest Kind. Tucson, Ariz.: Pachart Publishing House, 1979. 216+vii pp. $11.95. Two astronomers present a critical but well informed look at the claims of astrology. A responsible job that makes distinctions that most debunking books do not. Probably the best anti-astrology book for the critic thus far produced by astronomers. Recommended.

de Camp, L. Sprague, The Ragged Edge of Science. Philadelphia, Penn.: Owlswick Press, 1980. 244pp. $16.00. A collection of essays dealing with archaeological and anthropological mysteries, occultisms, and pseudoscience by the prolific writer of science fiction popular science. Though always an entertaining and usually well-informed writer, de Camp is sometimes more glib than responsible, as in his discussion of Velikovsky's work; but the collection is a welcome one and contains excellent debunking efforts which it is good to see reprinted here.

De Herrera, John, The Etherean Invasion. Los Alamitos, Cal.: Hweng Publishing Co., 1978. 157pp. $2.95 paperback. A rather strange consideration of an abductee narrative which superficially appears to be a proponent work but which ends up pretty well debunking the case. Some rather interesting material about the uses of hypnosis in UFO abductee cases, particularly as they are paralleled by the "stories" made up by non-abductees told to create abduction narratives.

Fitzsimons, Raymond, Death and the Magician: The Mystery of Houdini. New York: Atheneum, 1981. 195pp. $10.95. A new biography of the master magician concentrating on his activities with the spiritualists. Little new and inferior to Gresham's and Christopher's biographies, but well written and with an excellent annotated bibliography.

Fowler, Raymond E., Casebook of a UFO Investigator. Englewood Cliffs, N.J.: Prentice-Hall, 1981. 246pp. $10.95. A sensational and unconvincing but fascinating UFO book full of conjectures of

extra-terrestrial contacts and conspiracies of silence by our government. It will take a lot more than the evidence offered here to convince the hard-line critics, but much useful information presented even if not highly critically examined.

Franks, Felix, Polywater. Cambridge, Mass.: M.I.T. Press, 1981. 208 +x pp. $15.00. A very interesting history and analysis of the recent bizarre episode in "pathological science" that swept many scientists into now discredited research programs in both the Soviet Union and the West. A very revealing tale full of implications for the analysis of other deviant science programs including parapsychology. Highly recommended.

Frazier, Kendrick, editor, Paranormal Borderlands of Science. Buffalo, N.Y.: Prometheus, 1981. 469pp. $12.95 paperback. An uneven collection of reprinted articles from The Skeptical Inquirer. Some excellent pieces but generally a very one-sided anthology and unusually expensive for a collection of reprints.

Fuller, Uriah, Further Confessions of a Psychic. Teaneck, N.J.: Karl Fulves (Box 433; Teaneck, NJ 07666), 1980. 70pp. $6.00. The second volume of an exposé of the methods of pseudopsychics. A conjuring volume satirizing Uri Geller and associates which has been attributed to Martin Gardner but staunchly denied by Uriah Fuller. The introduction by Karl Fulves sets the harsh tone when he claims that the pseudopsychic is not a "streetwise magician" but is a "streetwise criminal." Though the book contains excellent material for the would-be pseudopsychic, the satire is so shrill that it may abuse where it intended to amuse.

Grinder, John, and Richard Bandler, The Structure of Magic, II: A Book about Communication and Change. Palo Alto, Cal.: Science and Behavior Books, 1976. 198pp. $7.95. The second volume in this set dealing with formal linguistic analysis of psychotherapy. This volume extends the analysis of verbal communication into the area of non-verbal interaction. A particularly valuable work for those interested in the process of "cold reading" by psychic as well as orthodox counselors.

Halpin, Marjorie M., and Michael M. Ames, editors, Manlike Monsters on Trial: Early Records and Modern Evidence. Vancouver: University of British Columbia Press, 1980. 336+xiv pp. $24.95 (Canadian). A serious, sympathetic look at Sasquatch and its hunters by scholars from a wide range of academic disciplines. An absolute necessity for anyone seriously interested in Bigfoot and his/her kin. Recommended.

Hanen, Marsha P., M.J. Osler, and R.G. Weyant, editors, Science, Pseudo-Science and Society. Waterloo, Ontario: Wilfrid Laurier University Press, 1980. 303+x pp. $7.50 paperback. A collection of papers from the 1979 conference sponsored by the Calgary Institute for the Humanities. I was particularly impressed by Marsha P. Hanen's article "Legal Science and Legal Justification" and by Cooter's paper "Deploying 'Pseudoscience': Then and Now," but the whole collection is a necessity for those interested in the demarcation problem between science and pseudoscience and the social factors involved. Highly recommended.

Hasted, John, The Metal Benders. Boston: Routledge & Kegan Paul, 1981. 279+xii pp. $25.00. An important new work on psychokinetic investigations conducted by a physicist. This work will be reviewed in detail in a forthcoming issue of ZS.

Hesse, Mary, Revolutions & Reconstructions in the Philosophy of Science. Bloomington: Indiana University Press, 1980. 271+xxvi pp. $22.50. A very important series of essays by a leading philosopher of science containing much of relevance for ZS readers concerned with the issue of social processes and their role in the development of science. Hesse's discussion of the social negotiation that is part of what was earlier seen as "objective" parts of scientific method brings her close to the extreme sociological position of those like Barry Barnes. Recommended.

Houdini. Harry, Miracle Mongers and Their Methods: A Complete Expose. Buffalo, N.Y.: Prometheus, 1981. 240+xv pp. $13.95. This is a welcome but rather over-priced reissue of the 1920 classic (a Canadian paperback edition put out by Coles for far less is available) with a Foreward by James Randi. As Randi acknowledges, many of the methods in the book really are not the actual ones (e.g., Houdini explains Thardo's immunity to rattlesnake bites as the result of her drinking a lot of milk!), and the book was almost certainly ghost-written for Houdini. A fascinating book, but as a true exposé of seemingly paranormal phenomena, the book is full of pseudo-explanations and frequent silliness. Unfortunately, the publisher presents it as a "skeptical" classic. If so, it is a terrible indictment of skepticism.

Hoyt, William Graves, Planet X and Pluto. Tucson: University of Arizona Press. 1980. 302+xiv pp. $9.50 paperback. An excellent history of planetary discovery, this work should be of special interest for many ZS readers for its relevance to such anomalies as the one-time claimed planet Vulcan.

Jordan, Peter A., Glimpses Through a Looking Glass: Four Psychics and Their Readings on the Subject of Unexplained Cattle Mutilations. Published by the author (217 Connecticut Rd.; Union, NJ 07083), 1980. 20pp. $3.00 paperbound. An interesting exploratory investigation in which four alleged psychics were asked to psychometrically respond to photographs of reputedly anomalously mutilated cattle and independently describe rather similar scenarios of paramilitary causes. Hardly scientific validation of anything, but a suggestive and curiosity-raising attempt that presents more methodological questions than answers to the mutilations mystery.

Krippner, Stanley, editor, Psychoenergetic Systems: The Interaction of Consciousness, Energy and Matter. New York: Gordon and Breach, 1979. 264+xxvi pp. $32.50. A collection of articles from the journal of the same name which emphasized a systemic and integrated approach in parapsychology with concentration on interdisciplinary, international, and bio-physical interpretation. Fascinating but sometimes quite wildly speculative stuff camaflouged by scientistic presentation. Particularly valuable for exposing Soviet efforts.

Krippner, Stanley, Human Possibilities: Mind Exploration in the USSR and Eastern Europe. Garden City, N.Y.: Doubleday Anchor Press, 1980. 349pp. $14.95.An excellent tour of the work on psychic healing, Kirlian photography and suggestology in the Soviet bloc countries. Not a sensational work but a sympathetic examination which will probably please neither the hard-line critics nor the miracle seekers. A great deal of information on some remarkable research being conducted. Krippner generally presents descriptions of the work without emphasizing either sharp criticism or extravagant praise. He is obviously excited about the work being done--as he should be--but the reader gets a moderate-conservative presentation that makes independent evaluation possible. A necessity for anyone interested in Soviet work on psi. Recommended.

Laidler, Keith, The Talking Ape. New York: Stein & Day, 1981. 181 pp. $11.95. A zoologist with a "Ph.D. in orangutans" tries using the techniques developed for dealing with autistic children to teach speech to an orangutan, Cody. A light presentation emphasizing the relationship between Laidler and Cody, the scientifically oriented will need to examine the author's technical works for serious analysis.

MacDougall, James, Folk Tales and Fairy Lore in Gaelic and English Collected from the Oral Tradition. New York: Arno Press, 1977. 328+xv pp. $23.00. A reprint of the 1910 classic full of fascinating stories for the specialist and those who simply love such tales. A welcome reissue.

Mauskopf, Seymour H., and Michael R. McVaugh, The Elusive Science: Origins of Experimental Psychical Research. Baltimore and London: Johns Hopkins University Press, 1981. 368+vi pp. $24.50. An extraordinary historical study of psychical research sympathetically but meticulously done. The best single book dealing with parapsychology that I have read over the last few years. I can not recommend this book too highly. Everyone seriously concerned with psychical research should read this book, for it clearly demonstrates how many old battles are simply being re-fought today out of ignorance of the earlier resolutions.(I found only two objections: (1) the index does not cover the copious and important footnotes at the rear of the book; and (2) the word "successfully" in footnote 36 on page 337, referring to Coover's private attempt to replicate Rhine's work, should be "unsuccessfully" -- an important typographical error.) I hope the authors will write a sequel bringing us up to date from 1940 where the book ends. If you read only one book on the paranormal this year, this should be it.

Nowotny, Helga, and Hilary Rose, editors, Counter-Movements in the Sciences: The Sociology of the Alternatives to Big Science. Boston: D. Reidel, 1979. 291pp. $35.25 clothbound, $15.80 paperbound. An excellent collection in the sociology of science. I was particularly impressed by Collins and Pinch's "Is Anti-science not Science? The Case of Parapsychology" and Grabner and Reiter's "Guardians at the Frontiers of Science," but all the papers are relevant for ZS readers. Highly recommended.

Popkin, Richard H., The History of Scepticism from Erasmus to Spinoza. Berkeley: University of California Press, 1979. 333+xxii pp. $18.95 clothbound, $5.95 paperbound. A revised and expanded edition of the classic 1960 work. Good coverage of Pyrrhonian zeteticism.

Pugh, Randall Jones, and F.W. Holliday, The Dyfed Enigma: Unidentified Flying Objects in West Wales. London: Faber & Faber, 1979. 186pp. 5.95 pounds (U.K.) Description of the bizarre events of 1974-77 flying saucer wave presented by two proponents who clearly state that theirs is not a scientific study. They believe in the narratives surveyed but recognize the lack of objective validation that would convince those skeptical, so the presentation is a fascinating one which does not overstep its limits.

Rhine, Louisa A., The Invisible Picture: A Study of Psychic Experiences. Jefferson, N.C.: McFarland, 1981. 267+xii pp. $15.95. A study of spontaneous psychic experiences. admittedly panoramic and exploratory and without scientific validation. A search through case studies for patterns related to the laboratory work believed by the author to conclusively demonstrate the existence of psi. An interesting exercise, and a good summing-up statement by the author, but its value -- as the author recognizes-- will probably have to await the final verdict on the laboratory evidence which here is a starting premise.

Ring, Kenneth, Life at Death: A Scientific Investigation of the Near-Death Experience. New York: Coward, McCann & Geoghegan, 1980. 310pp. $11.95. A very interesting study of over 100 subjects who came very close to death or expienced "clinical death." Ring presents evidence that the near-death experience is not affected by the individual's age, sex education, race or religion, and argues that there is a common "core experience." Critics would argue that this common experience does not validate any objective reality (i.e., survival) and other studies demonstrating contrary experiences need to be considered, but the book's argument is generally clear and moderate.

Robbins, Thomas, and Dick Anthony, editors, In Gods We Trust: New Patterns of Religious Pluralism in America. New Brunswick, N.J.: Transaction Books, 1981. 338pp. $ paperback. An excellent collection of sociological papers--mostly reprinted from the journal Society -- dealing with new religious orientations in America.

Romen, A.S. (translated and edited by A.J. Lewis and Valentina Forsky), Self-Suggestion and Its Influence on the Human Organism. Armonk N.Y.: M.E. Sharp, 1981. 222+xii pp. $22.50. A fascinating monograph by a leading Soviet researcher and physician into the uses of self-syggestion, especially in relation to preventative medicine. Should be particularly welcome in the holistic health community and should also be valuable to those currently interested in problems of coping with stress.

Sachs, Margaret, The UFO Encyclopedia. New York: G.P. Putnam's Sons, 1980. 408+ix pp. $16.95. A very useful compendium with remarkably small overlap with the Story Encyclopedia of UFOs, so both are well worth having. Sachs' work concentrates more heavily on the international UFO scene.

Schwarz, Berthold Eric, Psychic Nexus: Psychic Phenomena in Psychiatry and Everyday Life. New York: Van Nostrand Reinhold, 1980. 308+ xxxviii pp. $14.95. A fascinating work by a psychiatrist on psychical research, including ufology, which is remarkably uncritical and suggests great credultty by the author, exemplified by his description of Joseph Dunninger (a great mentalist and showman) as a genuine paragnost. Stimulating reading and interesting case studies, but misrepresented as a scientific work, which it may be if loosely defined, but which does little to gain the respect of critics for psychiatry and which ultimately probably undermines more responsible parapsychology. Critical works on the topics discussed are conspicuously absent from the author's bibliography.

Sheaffer, Robert, The UFO Verdict: Examining the Evidence. Buffalo. New York: Prometheus Books, 1981. 242 xi pp. $15.95. To be reviewed in detail in a future issue of ZS.

Sheehy, Jeanne, with photographs by George Mott, The Rediscovery of Ireland's Past: The Celtic Revival, 1830-1930. London: Thames and Hudson, 198 . 208pp. $19.95. A beautifully illustrated historical survey of the art characterizing Irish search for identity. Casts an excellent perspective on the occult interests of those like Yeats and others interested in the folklore rooted in Celtic antiquity. Not directly concerned with the paranormal elements in the revival but excellent background material for understanding the context.

Shupe. Anson D., Jr., and David G. Bromley, The New Vigilantes: Deprogrammers, Anti-Cultists, and the New Religions. Beverly Hills, Cal.: Sage, 1980. 267pp. $8.95 paperback. An important sociological study essential for anyone seriously interested in this topic. Recommended.

Spence. Clark C., The Rainmakers: American "Pluviculture" to World War II. Lincoln: University of Nebraska Press, 1980. 181+x pp. $15.95. A very welcome history of the attempts to produce rain surveying both the scientifically trained and the charlatans up until the first successful cloud-seeding experiments. Fascinating reading for anyone interested in the drought-busters of the golden age of quackery. Highly recommended.

St. Johns, Adela Rogers, No Good-byes: My Search into Life Beyond Death. A personal testimonial by a well-known writer, with anecdotal material on many personalities including Eileen Garrett, endorsin her belief in survival.

Story, Ronald D., with J. Richard Greenwell, UFOs and the Limits of Science. New York: William Morrow, 1981. 290pp. $12.95. An excellent survey of the "best" UFO cases and the arguments surrounding them. The authors conclude genuine anomalies seem present and notably deal with critic's attempts at debunkings, but the tone is moderate and cautious. Recommended,

von Däniken, Erich, translat-d by Michael Heron), Signs of the Gods. New York: G.P. Putnam's Sons, 1980. 252pp. $10.95. One keeps hoping von Daniken might lay to rest the criticism of his earlier works before going on to wildly and misleadingly conjecture further. He doesn't do so, and this book is just more of the same kind of "evidence" for his thesis of ancient astronauts Nonetheless, if read as pure fiction and for entertainment, the book offers much fun, and his section on the Ark of the Covenant actually being a mini-reactor sounds like something directly ouf of the recent hit adventure movie.

White. John, A Practical Guide to Death and Dying. Wheaton, Ill.: Theosophical Publishing House, 1980. 171pp. $5.25 paperback. A very biased work in relation to belief in survival, but probably very useful for those seeking to cope with anxiety and fear of death, and the book has much to commend it even for those who might disagree with its central beliefs.

Wylie, Kenneth, Bigfoot: A Personal Inquiry into a Phenomenon. New York: Viking Press, 1980. A welcome look by a most sympathetic skeptic,and one revealing much information about the internal machinations of the sasquatch seekers.The appendix on the Patterson-Gimlin film is especially valuable. Recommended.

Zacharias, Gerhard, translated by Christine Trollope, The Satanic Cult. London: George Allen & Unwin, 1980. 181+v pp. $44.50. A thoroughgoing, important historical work on the Satan-Cult and the Black Mass, originally published in Germany in 1964, with a most interesting thesis of its development as a compensation for the preeminently spritual tradition of Christianity and its exclusive emphasis on the good. Well documented and provocative. Recommended.

BACK ISSUES OF *Zetetic scholar*

The first four issues of ZS are no longer in stock. Reduced-xerox copies of #1 amd #2 are available for $8 each. A reduced-xerox copy of #3/4 is available for $12. Issue #5 is now in short supply and will soon only be available in reduced-xerox copies. Issues #5, 6, and 7 are available for $8 each. ZS is sent by 4th class book rate in the U.S. and Canada and by surface-mail abroad (recent postal increases make airmail abroad now impossible). If desired by airmail, please add $4 per issue (for North America and Europe and United Kingdom) and $8 per issue for Oceania and similar further points). Contents information about back issues available by writing to ZS. All funds should be sent by either international money order or checks on U.S. banks only. Those interested in back issues are urged to get them as soon as possible since issues are moving into short supply.

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COMING IN FUTURE ISSUES OF ZETETIC SCHOLAR

A major ZS Dialogue on the controversy surrounding the "Mars Effect" claims of M. Gauquelin and his critics as evaluated by Patrick Curry.
"Patterns of Belief in Religious, Psychic and Other Paranormal Phenomena," a close sociological look at the Gallup data.
Ray Hyman's new observations on cold reading and psychic counselling.
More CSAR Reports on psychic detectives.
New light on Edgar Cayce and his readings.
A bibliography on U.S. government and Soviet psi research available in English.
The role of conjurors in psychical research.
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